The Circulation of Science and Technology

Proceedings of the 4th International Conference of the European Society for the History of Science

BARCELONA, 18-20 November 2010

Hosted by
Societat Catalana d'Història de la Ciència i de la Tècnica

European Society for the History of Science
ESHS

SOCIETAT CATALANA D'HISTÒRIA DE LA CIÈNCIA I DE LA TÈCNICA

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FOREWORD

It gives me great pleasure to present the proceedings of the 4th International Conference of the European Society for the History of Science (ESHS), which was held in Barcelona between 18 and 20 November 2010. The ESHS, founded in 2003 in Paris, has become one of the most important reference points for our subject not only in Europe but also in the world[1]. The large number of papers presented for the Barcelona Conference, and the wide range of subjects bear witness to this fact.

The Barcelona Conference was organized jointly with the Catalan Society for the History of Science and Technology (Societat Catalana d'Història de la Ciència i de la Tècnica, SCHCT), a society affiliated to the Institute for Catalan Studies (Institut d'Estudis Catalans, IEC). More than a century after its creation, the IEC has become the main academic institution for higher culture and research in the Catalan speaking regions. The SCHCT was founded in 1991 and its function is to promote history of science and technology through publications, regular sessions and general meetings. The 4th Conference coincided with the XI Trobada of the SCHCT. On the 21st November, additional sessions took place, including a plenary lecture and the annual general assembly.

The aim of the Barcelona Conference was to facilitate the presentation of a variety of research topics and perspectives. Our logo represents: a) the medieval shipyards of the city (Drassanes), b) the Industrial School of Barcelona, c) the Hospital of la Santa Creu i Sant Pau, d) the Fabra Observatory, and e) the chimneys of the industrialized city. All this sums up the history of Barcelona, with its roots in medieval times, and its strong tradition of scientific, technical and medical activities.

I wish to express my sincere gratitude to all the persons and institutions for making this meeting possible. We obtained support from several universities (UPC, UAB), but very special thanks are due to the University of Barcelona (UB) for allowing us to use many of its facilities. The Institut d'Estudis Catalans (Secció de Ciències i Tecnologia, Secció de Ciències Biològiques) provided us with funds and many services. The following institutions made their lecture halls available to us: the Biblioteca de Catalunya, the Reial Acadèmia de Ciències i Arts de Barcelona, the Reial Acadèmia de Farmàcia de Catalunya, the Reial Acadèmia de Medicina de Catalunya, and the Residència d'Investigadors-Consell Superior d'Investigacions Científiques. We obtained funds from the Generalitat de Catalunya and from the Spanish Ministerio de Ciencia e Innovación. We should also like to acknowledge Gas Natural Fenosa for its generous help. Some editorial companies sent their publications for exhibition during the Conference: Taylor & Francis, the Royal Society Publishing, and Birkhaüser-Springer Basel. A stand of the official journal of the ESHS, Centaurus, was also present.

However, we owe a special debt of gratitude to all the delegates without whose participation the Conference could not have taken place. There was an excellent response through papers and we are indebted to the many researchers and research groups that had chosen this conference to develop their activities. This justifies the existence of our societies, whose objective is to promote history of science, technology, and medicine.

The 4th Conference was devoted to the circulation of science and technology. Circulation is one of the most distinctive features of human activity, and continues to be a challenge for humanity. The study and analysis of the circulation of science and technology would result in a better understanding of the role of science and technology in our society. I should like to mention especially the papers presented as plenary lectures by Julio Samsó (Universitat de Barcelona),

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[1] The previous conferences of the ESHS, which took place in Maastricht (2004), Krakow (2006), and Vienna (2008), offered excellent opportunities for the development of our discipline.

Efthymios Nicolaidis (National Hellenic Research Foundation, Greece), and Sona Strbanova (Academy of Sciences, Prague, Czech Republic). The excellent quality of their papers contributed to the success of the Conference.

As general coordinator of the Barcelona Conference, I should like to thank the programme committee for evaluating the proposals of sessions and of papers, and the local committee for taking care of the preparation and the development of the Conference. The present volume of Proceedings does not record exhaustively the development of the Barcelona Conference. Some participants informed us that their papers had already been selected for books or special issues of international journals. This may be verified by comparing the list of papers that appear in the general programme with recent publications in our field. In conclusion, it is a source of great satisfaction to have contributed in some way to the dynamic life of history of science, technology, and medicine.

Antoni Roca-Rosell

Universitat Politècnica de Catalunya

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SCIENCE AND THE EASTERN ORTHODOX CHURCH DURING THE 17TH-19TH CENTURIES

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Preface

The following paper aims to give an overview of a "marginal" subject in the historiography of science: the relations science-Orthodox church during the period when new scientific ideas developed in Europe.[1] Indeed, the great majority of works on the history of the relations between science and religion follow the history of Western Christianity. This just follows the prevailing historiography about the Christian world. In his preface at *The Cambridge History of* Christianity, the Archbishop of Canterbury describes this prevailing historiography as follows: "the Church gradually builds up a centralized system of authority, filling the vacuum left by the fall of the Roman Empire; its ideological monopoly is challenged at the Reformation, and the map of the Christian world is reconfigured… Even in some good and sophisticated surveys of the world Christianity published in recent years, this remains the dominant picture. But Christianity is more various than this begins to suggest."[2]

The existing literature on science and religion contains virtually nothing that pertains to the vast part of Eastern Europe and Western Asia that constituted the Eastern Roman Empire, the Byzantine Empire. Contrary to Western Europe, here there was no political and institutional void; on the contrary, we observe an astonishing continuity, damaged but not abolished in 1204, date of the conquest of Constantinople during the Crusades. This continuity also involved the perpetuation of an educational system put in place at the end of Antiquity that was associated with both secular and ecclesiastical power, the latter placed under the authority of the former. In the sciences, this translated into the perpetuation of a tradition of teaching ancient Greek science in the Greek language, sometimes as reviewed and modified by the Greek Fathers of the School of Alexandria, such as Saint Basil. Hence conflicts –when they existed– were not "science versus Christianity", but rather ecclesiastical conflicts that traversed the whole society and consequently also concerned the sciences by always coming back to the question of the importance of secular knowledge and the possibility (or not) for humankind to conceive of Creation through science. For the medieval Oriental Empire, it was not a matter, as in the West, of rediscovering the Ancients, because they simply were always taught. During all this period, the Orthodox Church searched equilibrium between secular knowledge and revelation, hence between science and Christianity. The Hesychastic debate

[1] This overview is based on the book: E. Nicolaidis (transl. by S. Emanuel) *Science and Eastern Orthodoxy. From the Greek Fathers to the Age of Globalization*, Baltimore: Johns Hopkins U.P., 2011.

[2] Williams, Roman, Archbishop of Canterbury, "Foreword", Anglod, Michael, ed.*, The Cambridge History of Christianity.* Vol. 5, *Eastern Christianity*. Cambridge: Cambridge University Press, 2006 p. XVI.

(14th c.) that initially expressed the hostility of monks and lower clergy toward the secularization of the high clergy and especially its distance from the ascetic tradition, is an example. Hesychasts believed than man, through prayer and ascetics could have a vision of God and thus true knowledge comes from this effort and not from secular knowledge. But this belief did not mean that they totally rejected science. The ideological father of this movement, Gregory Palamas (1296-1359), in one of his religious texts posed the problem of the location of the centers of the spheres of the two elements, earth and water, and based himself on the science of Aristotle and the geometry of Euclid to answer the problem. In effect, the Hesychastic leader did not deny the utility of the sciences; he was more distrustful of the place granted to them by Byzantine power, seeing it as one of the causes of the secularization of high clergy.

After the conquest of Constantinople by Mehmed II the Conqueror who established the town as the capital of his vast Empire, the Byzantine tradition was kept by the Greek Patriarchates of Constantinople, Alexandria and Jerusalem. The Patriarch of Constantinople became the leader of the Orthodox communities of the Ottoman Empire, assuming the role of the control of education, hence of the relations between science and the Church. Indeed, after its definite victory on the Byzantine Empire, the Ottoman Empire based its domination on the organization of *millet*s, that is to say, a system for controlling non-Muslim populations that was in large part delegated to their religious leaders as appointed by the sultan. The first *millet* to be created was that of the Orthodox Christian Church, followed by the Armenian *millet* and the Jewish *millet*. The *millets* had their own laws (for example, when a member of a *millet* committed a crime, the law applicable was that of the *millet* of the person harmed, but when a Muslim was involved, *sharia* trumped everything). They collected their own taxes in compensation for their loyalty to the empire, and they managed their own educational system, which largely freed Orthodox communities from Islamic influences.

Mehmed II, subtle politician that he was, sought to alienate the Greeks from the Westerners, in order to reduce the chance of a crusade by Christian countries against the Turks. For this purpose he supported the enemies of the union of Orthodox and Catholic Churches. Mehmed, learning that the anti-unionist leader Scholarios had been made prisoner during the sack of Constantinople, liberated him and placed him at the head of the prime *millet* of the Ottoman empire and after raising him rapidly through all the ecclesiastical grades (he was not in a monastic order), he appointed him Patriarch on 6 January 1454.

Scholarios, in the spirit of “the Turks are better than the Latins”, maintained good relations with the Sultan and inaugurated the Ottoman period of the Orthodox Church. Having control over the Orthodox Christian *millet*, he tried to purge it of nefarious influences, whether Latin or Ancient Greek. Thus the first Patriarch of the Ottoman period took a rather uncommon decision for the Eastern Church: to burn publicly a book as Hellenizing, idolatrous and satanic. This was the *Book of Laws* of a late Byzantine scholar and astronomer, Pletho, who made an attempt to return to a pre-Christian religion.

This aversion towards the Catholic West in a period of a vivid revival of sciences in this part of the world, and towards the Ancient Greek tradition which was synonymous to secular knowledge and science, lead to the fact that the sciences did not figure among the preoccupations of the Orthodox Church of the Ottoman Empire. Indeed, for a century and a half after the Ottoman conquest, the patriarchate of Constantinople did not have a policy of teaching anything except what was useful for the renewal of the ecclesiastical hierarchy. The only organized Orthodox school in the Ottoman Empire was the patriarchal school of Constantinople, refounded in 1454. With regard to curriculum, it had nothing in common with its predecessor in the days of Byzantium, when mathematics and astronomy were taught at the highest level. And until the start of the 17th century, this was the sole organized school for the Orthodox population in the Ottoman Empire. The other schools for Orthodox pupils were to be found in Italy or in the Venetian possessions such as Crete.

The “Orthodox humanism” and science

Thus, after the Ottoman conquest, the Orthodox Church followed a reactionary policy towards secular knowledge and in particular science. The Byzantine renaissance of the 15th century which brought up the development of science ended abruptly. East Orthodox did not participate to the birth of the new European science of the 16th and early 17th centuries. Nevertheless due to the flourishing Greek Orthodox communities of Italy, the Orthodox world came quickly into contact with new European scientific ideas. The problem of the spread of new science to the East Orthodox world has thus been set.

This problem did not oppose clergy to secular scholars but the debates traversed the whole body of the Orthodox Church. In fact, the scholars of the orthodox world who propagated this new science were in large part ecclesiastics, sometimes occupying high positions in the Church hierarchy. It will be the rise of nationalities at the end of the eighteenth century and the effort of modernization of the nineteenth that will first forge in the Orthodox world the image of a man of science who is not linked to the Church.

As mentioned above, the leader of the Christian Orthodox *millet* was the patriarch of Constantinople. Thus the power of the Church with respect to education and science increased, compared to what it had had in the Byzantine era, when it shared this domain with the Byzantine emperor. As was the case since antiquity, the scholarly language of Orthodoxy was Greek, and so education was dispensed in Orthodox schools in Greek. Thus concerning science specifically, the policy adopted by the Church involved all orthodox peoples (whether or not they were native Greek speakers) of the vast Ottoman Empire, which at its apogee extended over three continents: Europe, Asia, and Africa.

A discussion on the validity of secular learning that expressed Italian humanist views had already taken place in the 16th century, among Greeks who were in contact with Italian culture.[3] But, as until the start of the 17th century, only a tiny number of Christian Orthodox thinkers –not more than fifty during the 16th century– were involved with the sciences this discussion remained marginal. The creation of a scientific community worthy of the name among Orthodox Christians of the Ottoman Empire would be a long and slow process; it would not be fully achieved until the century of the Enlightenment. Yet such a community began to make its presence felt in the17th century, with a movement that modern historians call "religious (orthodox) humanism."

Cyril Lucaris, the leading humanist in the Orthodox world, was born in 1572 in Crete, then under Venetian domination. He had been educated by a tutor in Heraklion and continued his studies in Venice and then at the University of Padua. In 1593, he found himself in Alexandria where his uncle, Meletios Pegas, was patriarch. He went into religious orders and was sent to Poland in 1594-1596, and then again in 1601-1602 to fight against the influence of the Catholic Church, which was converting Orthodox believers to Catholicism. During his stay in Poland, Lucaris realized the importance of Jesuit colleges, and analogously he organized the Orthodox Academy of Vilnius. At the death of his uncle in 1601, he became patriarch of Alexandria. In 1612, Lucaris was named throne steward of the patriarchate of Constantinople, and in 1620 he was appointed by the sultan patriarch of Constantinople. In a rare display of cooperation, Catholics and conservative Orthodox believers united to convince Sultan Murat IV to remove him in 1623. Restored and removed by the sultan no less than four times, Lucaris was finally strangled, on the orders of the Grand Vizier in 1638.[4]

Lucaris brought a small revolution to Orthodoxy, supporting the Protestant spirit in the midst of the Thirty Year's War. Lucaris was the prime source of Orthodox religious humanism, aiming to re-establish the teaching of letters and sciences, especially ancient Greek learning, among the Greeks of the Ottoman Empire. This was one of the reasons why his enemies accused him of preparing an insurrection of Greeks in the empire. With Lucaris's encouragement, his friend Theophilus Korydaleus (c. 1570-1646) applied the ideas of religious humanism to scientific teaching. Born in Athens, this scholar had followed the characteristic educational path of wealthy families of that era: primary education in his native town, then at the Greek College of Rome, and finally theology, philosophy and medicine at the University of Padua. There his professor of natural philosophy was Cesare Cremonini, the well-known Aristotelian adversary of Galileo.[5]

In 1622, in the reforming spirit in education (which in this context consisted of introducing into the curriculum something other than grammar and theology), Cyril Lucaris called upon Korydaleus to teach at the Patriarchal School of Constantinople, and a year later he became its director. Korydaleus introduced instruction in natural philosophy, geography, and astronomy. It was the first time since Byzantium that such education was dispensed at the very heart of the patriarchate of Constantinople. Obviously this innovation would not leave conservative circles of the Orthodox Church indifferent. The adversaries of his friend Lucaris accused Korydaleus of Calvinism, and his career experienced highs and lows, more or less following those of Lucaris's influence. In 1640, two years after Lucaris was assassinated, Korydaleus retired definitively to his native city Athens, to teach philosophy. His teaching of Aristotelian natural philosophy and logics would mark the whole Balkan Peninsula until the end of the 18th century.

The most novel features of Korydaleus's thought, were that, contrary to Orthodox scholars of the 16th century, philosophy served knowledge of both divine and profane things and that the happiness of man in his bodily consisted of studying. Not only did philosophy *not* depend on theology, but its methodology was valid for theological matters. This position inaugurated a new debate in the Orthodox world, which in certain circumstances would last until our own day.

This new ideology advocated by Korydaleus coincided with the birth of a scientific community in the Greek Orthodox world that might be called neo-Aristotelian. This nascent community was closely linked to the University of Padua, that favorite site of Greek university study. Some Greeks even made careers in Italy, as George Koressios "Lettore della Lingua Greca nello studio di Pisa", who wrote a dissertation in Italian against Galileo's mechanics of liquids or Ioannis Kotounios, who studied at the Greek College of Rome and at the university of Padua, and succeeded Cremonini in 1632 in the chair of Aristotle. But the great majority of them would teach Orthodox Greeks and write their books in Greek in order to spread scientific ideas in the Orthodox world.

It is very important to note that Korydaleus revival of the sciences, although influenced by Western Europe, had nothing to do with the "scientific revolution" that was simultaneously unfolding in Europe. Korydaleus, like all the Greek scholars influenced by religious humanism, sought ancient Greek science. He wanted to return to sources unrelated to the

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[3] Petsios, Kostas, *Η περί φύσεως συζήτηση στη νεοελληνική σκέψη* [The discussion on nature in Modern Greek thought], Jannina, 2003., p. 65.

[4] On Cyril Lucaris, see Hering, Gunnar, *Das ökumenische Patriarchat und europäische Politik 1620-1638,* Wiesbaden: Franz Steiner 1968.

[5] On Korydaleus see Tsourkas, Cléobule, *Les débuts de l'enseignement philosophique et de la libre pensée dans les Balkans. La vie et l'œuvre de Théophile Corydalée (1570-1646)*, Bucarest, 1948; 2nd ed., Thessaloniki: Institute for Balkan Studies, 1967.

Christian belief in the ancestral wisdom offered to humans by God. Like the scholars of late Byzantium, religious humanists argued that Orthodox believers were the natural heirs of Ancient Greek learning. Korydaleus studied Aristotelianism in Padua with Cesare Cremonini. Nevertheless, given the sentiment that Greeks were the legitimate heirs of Aristotle, the neo-Aristotelianism propagated by Korydaleus in the orthodox world was not that of his Italian professor, but a "direct line" Aristotelianism that drew on the tradition in the Greek language, starting with Alexander Aphrodisieus. In fact, Korydaleus was opposed to the theological interpretation of scholastic Aristotelians, insisting on the autonomy of philosophy from theology.

However, Korydaleus did have to respond to the questions that Christianity posed to Aristotelianism, essentially about the genesis of the world. For him the scholar's primary duty is to make a distinction between theology and philosophy, keeping the two intact and separate. This position is illustrated by the following short anonymous passage, written in 1669 at the latest: "What is true for theology is also true for philosophy, and what is false for philosophy is also so for theology. For theology is based on the light of faith, and philosophy on the light of nature."[6]

Thus natural philosophy is independent of theology, and so Korydaleus can allow himself to teach the former without any reference to the latter. He presents reasons for the utility of this physical science, which are both practical (for example, applications to agriculture), and theoretical, when it comes to the nature of the universe or of light. In the latter case, utility consists of the acquisition of wisdom, which is pleasing to God.

Speaking of the world, of matter, of genesis and decay, without presenting the Christian theory of nature as it had been formulated by the *hexaemerons* of the Church Fathers, was something new in the Orthodox world of the Ottoman Empire. Moreover, Korydaleus' written works were designed for education in the very orthodox Greek colleges. Korydaleus was not just anybody, he was close to the highest spheres of the Church during the reforming patriarchate of Lucaris, he had taught at the very heart of Orthodoxy, at the Patriarchal School. Given the stakes, the reaction in orthodox circles to Korydaleus' kind of teaching was rather moderate. And starting at the end of the 17th century, Korydaleus would be accepted as a great scholar by the orthodox world; his name became a reference in the domain of the sciences.

This rapprochement was facilitated by the thesis of the "double truth" (philosophical learning is independent of theology) he had advocated, even if his theologian contemporaries thought he expressed it in an extreme manner. Little by little, the hierarchy of the Orthodox Church came around to the idea of teaching ancient natural philosophy independently of the teaching of creation. This acceptance was prepared by the idea –increasingly widespread in the 17th century– that the Orthodox were the heirs of Greek splendor and learning. This idea was a comfort to the Orthodox of the Ottoman Empire, who felt subjugated to the Muslim state and at the same time threatened by the specter of Uniates, Orthodox believers who had converted to Catholicism. Without political power, wedged between Islam and Catholicism, the Orthodox Church sought support. And the Greek heritage was such a support, and Greek philosophy could therefore gradually assume its place in the education controlled by the Orthodox Church.

The first references to the new European scientific ideas

The introduction of a scientific curriculum in the Orthodox colleges did not bring any revolutionary scientific learning. But it did increase the interest in the education and it increased contacts with Italian scholarship. Between about 1630 and the end of the century, at least a dozen Greek colleges were founded or fundamentally renovated in the lands controlled by the Ottoman Empire. This education network would constitute the means by which the new science would later spread to Southeast Europe but this new science would not become central to the preoccupations of Greek scholars before the middle of the 18th century. Nevertheless, bits of this science were introduced at the second half of the 17th c. thanks to the circle of Greek Aristotelians. Alexander Mavrocordatos (1641-1709), a student at Padua who wrote his dissertation on the circulation of the blood, taught the new ideas of Harvey at the Patriarchal School between 1665 and 1672.[7] Nikolaos Koursoulas (1602-1652), in his commentary on Aristotle's *De coelo,* briefly presents the systems of Copernicus and Tycho accompanied by drawings. Ioannis Skylitzes (born in 1630) in his *Introduction to Cosmographical Sciences and Arts,* written before 1680, also presents the Copernican system but condemns it because it does not offer a greater simplicity in the explanation of phenomena.[8]

These few timid references to the ideas of the scientific revolution of the 17th century presented no danger whatever to the Orthodox Church. There was almost no reaction and certainly not a debate worthy of the name. The big majority of the clergy ignored all about the new system. The few scattered reactions such as this of Nektarios, Patriarch of Jerusalem

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6 Petsios, *op. cit.,* p. 178.

7 Alexandro Mavrocordato, *Pneumaticum instrumentum circulandi sanguinis sive de motum et usu pulmonum*, Bononiae, 1664, reedited 1665, 1682 and 1870.

8 Karas, Yannis, *Οι επιστήμες στην Τουρκοκρατία. Χειρόγραφα και έντυπα* [Sciences under Ottoman domination: Printed books and manuscripts]. vol. 2, *Natural Sciences*; Athens: Estia, 1992, p. 211-215 and 282-286.

(1602-1676), were the exception. Responding to the accusations of the French Protestant Jean Claude (1619-1687) that the Greeks were illiterate and superstitious, Nektarios wrote that they did not need professors like Copernicus or Galileo since it was their fashion to overthrow sacred texts by sophistry.[9] But the debate did not go farther.

As already mentioned, until the 19th century there was little to distinguish between the positions of church and secular scholars. Debates and discussions traversed Orthodoxy itself: every tendency was present among the clergy. The first book to present the heliocentric system in detail was written by Michael Mitros, called "the geographer" (1661-1714), future metropolitan of Athens under the name Meletios. About 1690 Mitros wrote a huge manuscript in which he describes the Copernican system and presents all the new discoveries of Galileo which came in favor of it –sunspots, the moons of Jupiter, comets– and discusses the existence of heavenly spheres.[10] As a man of the Church, Mitros could not be a partisan of the Copernican system, but as a cultivated man, he realized that the new phenomena observed by Galileo could not be explained by the geocentric system. Therefore he followed the position adopted by the Jesuit astronomers and supported the geo-heliocentric system of Tycho Brahe which was in accordance with the new observations and with the Bible.

The main characteristic of the reforming orthodox current of 17th century is a "return to sources," but these sources are not the Church Fathers and ecclesiastical Byzantines, but the ancient sources of secular knowledge. After two centuries of uncontested Ottoman domination, the orthodox communities of the empire were becoming more powerful by controlling essential economic activities. This would give them the desire for an independence that would only become concrete after the middle of the 18th century, with the advent of new national ideas. These impulses toward independence assumed ideological expression, such as the affiliation of Orthodoxy with Greek culture in the form of religious humanism. Orthodox believers would thus feel strengthened in the face of both the Ottomans, who had the political power but whose science left much to be desired, having neglected their Arab scientific heritage, and the West, who were developing a new science that would put them at the forefront of world scientific and technological achievement.

This return to Hellenic sources, which in science was translated into the introduction of natural philosophy and mathematics into orthodox schools, had a major theological impact: relaunching discussion of the validity of science in relation to theology. But this renewed discussion took place in a new context. Now it was a matter of affirming the glorious past of a people who felt themselves "in decadence," subjugated by a non-Christian state. Until then the only reference of the Orthodox had been Byzantium. But now European humanism revalorized ancient Greece and offered a frame of reference that had been neglected after the victory of the anti-unionists.

A scientific Patriarch: Chrysanthos Notaras

In 1697, the Orthodox monk Chrysanthos Notaras was sent by his uncle Dositheos, Patriarch of Jerusalem, to Padua in order to complete his education. The curriculum of the University of Padua included theology courses that were also followed by Orthodox students. This might appear to contradict the hatred of Catholics felt by a majority of the Orthodox clergy, but the clerical aristocracy needed men who understood well the dogma of the "Latins" and a certain portion of the clerical aristocracy maintained good relations with the Catholic Church. Chrysanthos's professor at Padua was the Greek Catholic Nicolas Komninos. His attachment to Catholicism did not prevent him from considering Italy as a sinful land, and he maintained friendly relations with Dositheos, to whom he wrote that his nephew was leaving Padua more knowledgeable but without having been perverted by Italian morals. In this period the teaching at the faculty of arts was Aristotelian. A curious mind that knew that a new science had existed for a long time, Chrysanthos was not going to be content with Aristotelian study. In 1700, at the end of his study in Padua, he went to France, another sinful land according to the Komninos.[11]

In Paris, Chrysanthos made the acquaintance of liberal theologians such as Louis Ellies Du Pin, Alexandre Noël, and Michel Le Quien. Still more important for him was his contact with men of science He asked to visit the Paris Observatory, and he was welcomed and lodged there for a week by John Dominique Cassini, first director of this modern establishment. As he himself wrote, "there we observed with him [Cassini] with the aid of the largest telescopes the Moon, Jupiter and its so-called satellite-stars, the galaxy and other things. He then told us that by using several methods and observations, he discovered that one minute of the earth's circumference –that is to say, an Italian Mile– is five thousand, seven hundred and six feet of Paris, or five thousand eight hundred and eighteen geometric feet."[12] Apart from astronomy, Chrysanthos expressed interest in methods of measuring the earth, that is, in surveying and the determination of coordinates and had the

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[9] Petsios, *op. cit.,* p. 203.

[10] Karas, *op. cit*., p. 243-251. The title of the manuscript is: *Astronomical book presenting and explaining the discoveries of Ancients and Moderns, from Adam to Ptolemy and Copernicus…*

[11] On Chrysanthos see Stathi, Pinelopi, *Χρύσανθος Νοταράς, Πατριάρχης Ιεροσολύμων, πρόδρομος του Νεοελληνικού Διαφωτισμού* [Chrysanthos Notaras, Patriarch of Jerusalem, precursor of the Greek Enlightenment], Athens: Syndesmos ton en Athinais Megaloscholiton, 1999.

[12] Chrysanthos Notaras, *Εισαγωγή εις τα γεωγραφικά και σφαιρικά* [Introduction to geography and the sphere], Paris, 1716, p. 92.

occasion to learn about Jean Picard's work, which was the state-of-the-art science on these subjects. He even constructed a sort of astrolabe according to Cassini's instructions and had engraved on it the following inscription: "This instrument has been fabricated by the monk Chrysanthos according to instructions from Cassini for his brothers in Jerusalem, so that they may adore God through his works." [13]

To adore God by science, that means understand the Creation by secular knowledge, and was not a new idea for the Orthodox; we have encountered it several times during the Byzantine period and was promoted by Korydaleus. What is new is that Chrysanthos, who on the death of his uncle Dositheos (1707) became Patriarch of Jerusalem, departed from the scientific tradition of Orthodox humanism that accepted only Hellenic science as valid. As a precursor of the Enlightenment he was able to present to the Orthodox world, with his immense moral authority, the new European scientific ideas.

Returning from Paris, Chrysanthos wrote a book that reflected what he saw and learned in this capital of science, the *Introduction to geography and to the sphere.* This was the first printed book to present the new science to the Greek world. Because of his position as patriarch, the prudent Chrysanthos presented this new science in such a way as not to raise theological problems. In the astronomical part, he presented the Copernican system while prudently explaining that for practical astronomy (determination of the positions of heavenly bodies) both systems −heliocentric and geocentric− were valid. Why then, he concluded, not lean to the latter as closer to Scripture? Just as cautiously, he publicized the heliocentric system through the engravings in his book and not in the text. Out of nine engravings presenting the subject, only one offers the "system of the world according to Ptolemy", one according to Tycho Brahe, and other seven the heliocentric system in detail, of which two are the "system of the world according to René Descartes", which includes the Cartesian whirlpools explaining the functioning of the world.

The presentation of the heliocentric system was totally symbolic in Chrysanthos' book. Although an able mathematician himself, he did not present any mathematical aspect of the Copernican system, remaining at the level of qualitative description. Chrysanthos choose a domain other than astronomy, that of geodesy, to present the new learning in more detail. In geodesy, Chrysanthos, aware of Picard's work, presented the rigorous surveying methods, as well as the instruments of triangulation and gave details of their fabrication. He would make his own measurements and tried to re-determine the coordinates of various cities in the Balkan Peninsula.

The very Orthodox Patriarch Chrysanthos considered "Latin" science the flower of civilization: "It was a common saying among the Greeks that what was not Hellenic was barbarous. In those days the barbarian nations were Germany, France, Holland, and others. But when they had received Hellenic wisdom, when they had established academies, gymnasia, and other schools, it was the barbarians who became Hellenes and the Hellenes became barbarians by losing all these things".[14] His project was to create schools in the Orthodox world. He believed that princes should contribute financially to the foundation of what would be the "best of the commonwealth," for in spending for this purpose "sacred revenues do not diminish but increase; whereas once they nourished laziness and do-nothings, now they are being encouraged to nourish and sustain wisdom, knowledge of God, the sciences, and all psychic and physical needs."

A condemnation of the new philosophy by the Church

Methodius Anthrakites (circa 1660 – circa 1736) has as a common Greek student, studied in Padua. He was author of an enormous textbook in three volumes, the *Mathematics Course,* which would shape mathematical education in the Greek Orthodox world during the first half of the 18th century.[15] This book provides a complete course, detailed and rigorous, of the "mathematical sciences" as they were taught in Padua at the start of the 18th century, plus some Byzantine texts.

Therefore, there was nothing revolutionary in the Aristotelian teaching of this monk. However, Anthrakites seems also to have taught –or at least presented– the philosophical ideas of the French philosophers Nicolas Malebranche and René Descartes. No doubt this novelty, and the fact that he clearly stressed the teaching of science and not philological subjects, led Orthodox fundamentalists to accuse him before the Holy Synod of being a heretic. In 1723 he was summoned to Constantinople to refute these accusations. Condemned, he was excommunicated and his educational books banned. After confessing his Orthodox faith and ceremoniously burning some of his own manuscripts, the Church lifted his excommunication and authorized him to teach again, on condition that he followed the course of Korydaleus. In his novel defense, Anthrakites claimed that the Church was condemning him for his philosophical ideas and not for having departed from Orthodox dogma. Thus he re-opened the debate within the Church on the separation between philosophy and theology. His stance succeeded only in sharpening the anger of the Holy Synod, which reaffirmed its position that only the Peripatetic

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13 Arvanitakis, G. L., «Notes astronomiques», *Le Messager d'Athènes*, Février 1939, No 5217.

14 Stathi, *op. cit.*,p. 110.

15 Anthrakites, Metodius, *Οδός μαθηματικής*, Venice, 1749; second edition 1775.

philosophy of Aristotle should be taught.[16] This controversy bore on the principle and not on the subjects actually taught in natural philosophy. In fact, Anthrakites always remained an Aristotelian, and into the 18th century he perpetuated the Orthodox humanist ideas of the previous century, which featured the renaissance of Greek science.

Clerics promoting new science during the Greek Enlightenmen

The introduction of new science in the Orthodox world follows the teaching of this science at the University of Padua. Indeed, this university was the main destination for Orthodox students until the middle 18th century, because the Greek community of Venice was until then the most important Greek community in the Western Europe. There, it was not until 1739 that this new physics was fully taught by the chair of *ad mathesim et ad philosophiam experimentalem*, (mathematics and experimental philosophy) occupied by Giovanni Poleni (1683-1761), who created his famous *Teatro di filosofia sperimentale* (theater of experimental philosophy), equipped with 400 instruments to teach experimental physics. The first reactions by Orthodox students to these reforms were negative. If they went to Italy, it was usually to study Greek sciences, which were taught at a rather elementary level in the Greek schools of the Ottoman Empire. But gradually a new discourse arose that aimed to reconcile the existence of the new science with the idea of a renaissance of ancient Greek science. This new discourse appeared in the prologues of Greek science books around the middle of the century, some fifteen years after the establishment of the chair of experimental physics in Padua. Until that time, Greek scholars conceived of science as solely about Greek and early Byzantine science. Henceforth, the new European science would be integrated as the brilliant heir of ancient science.

Eugenios Voulgaris (1716-1806), the most influential Greek scholar and cleric in Southeastern Europe in the second half of the 18th century, was born in Corfu, a Venetian dominion. He studied at Padua, where he followed the first courses in experimental physics from Poleni, and in 1742 became director of a School in Jannina. There he taught Leibniz, Locke, and Voltaire, which exasperated the Aristotelians who forced him to go teach these insanities elsewhere. In 1753, the enlightened patriarch Cyril V created a school on Mount Athos and appointed him to direct it. At the heart of mystical Orthodoxy, then, Voulgaris taught, according to the terminology of the scholars of his time, the new science and the new philosophy, that means the ideas of the European "scientific revolution" developed after the 16th century. This innovative teaching drew him many students as well as many troubles, which obliged him to quit the Academy in 1758. In 1759, he was called upon to head the Patriarchal School in Constantinople by Seraphim II, a patriarch who supported the Greek revolt against the Turks provoked by Russia; Seraphim also tried to establish innovative education in the capital of Orthodoxy. The experiment lasted two years, until Seraphim's fall. The new patriarch and the majority of bishops were hostile to this intrusion of Western civilization into Orthodoxy. Voulgaris would leave again, and after a stay in Romania (then administered by Greek princes named by the sultan), he arrived in Leipzig with the goal of publishing teaching manuals like his *Elements of mathematics,* based on a book by Andreas Segner. There he met the Russian Marshal Theodore Orlov who introduced him to Catherine the Great. Voulgaris, disappointed at résistance to new ideas, and the intrigues in the circles around the patriarchate of Constantinople, finally settled in Russia, where he was named archbishop of Slavonia and Chersonesos. He had been named in 1776 a member emeritus of the Science Academy of St. Petersburg.[17]

Voulgaris is as good example of the relations between enlightened Orthodox clerics and the Enlightenment. On the one hand, he considered himself as heir of the Ancient Greeks and was proud of it; on the other hand, he thought that the Greece of his day –confused with the *millet* of the Ottoman Empire– was totally decadent and would owe its salvation to the teaching of the new European science. For him, the new science consisted of the development of the science of the Ancients. "[Diophantus] is considered the sovereign of all arithmetical thinking. But his marvelous invention, the art called algebra, was developed and perfected by François Viète, René Descartes, and others," he wrote in his prologue to Tacquet's *Elements of geometry*.[18] This idea would be developed by other Greek partisans of the Enlightenment who considered the great European savants as the children of the ancient Greek savants.

Voulgaris' books were soon eclipsed by those of his junior Nikephoros Theotokis (1731-1800), who followed a similar career. Also born in Corfu, he, too, had studied experimental physics with Poleni and then mathematical sciences with Eustachio Zannoti, director of the Bologna Observatory. After his studies in Italy, he returned to Corfu, took his monastic vows, and created a school where the new science was taught. In the 1760s he was called by the patriarch to Constantinople where he was appointed Preacher of the Grand Church. His teaching of the new science and philosophy shocked conservative circles of the Church and Phanar; so Theotokis went to Jassy in Romania, where the ambiance

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[16] Aggelou, Alkis. "Η δίκη του Μεθοδίου Ανθρακίτη" [The trial of Methodius Anthrakites]. In *Των φώτων. Όψεις του νεοελληνικού διαφωτισμού* [Lights: Aspects of Modern Greek Enlightenment], pp. 23-37. Athens: Hermes, 1988.

[17] On Voulgaris see Batalden, Stephen K., *Catherine II's Greek prelate : Eugenios Voulgaris in Russia, 1771-1806* New York : Boulder, East European Monographs ; Distributed by Columbia University Press, 1982.

[18] Voulgaris, Eugenios, *A. Τακουετίου, Στοιχεία Γεωμετρίας*, Vienna, 1805, p. XI-XII.

seemed more liberal than in the capital of the Ottoman Empire. He soon left for Leipzig, where (like Voulgaris) he went to publish his manual *Elements of physics*.[19] In 1773 he returned to Romania, where he directed the Academy of Jassy. Facing résistance to his teaching, and considered a revolutionary by both clerical and secular conservatives, in 1776 he accepted an invitation from Voulgaris to join him in Russia, where three years later he succeeded him as archbishop of Slavonia and Chersonesos. In 1782 he became archbishop of Astrakhan and Stavropol.[20]

Theotokis' *Elements of physics* became the book of reference for the diffusion of new science in the Orthodox world for at least two reasons. The book was not content with presenting "novelties"; it taught physics in a rigorous manner that required solid mathematical notions that would be supplied in another manual by Theotokis, the *Elements of Mathematics Compiled from the Ancients and the Moderns*. Thanks to this manual and the one on mathematics, Theotokis was the first to present differential calculus to Greek schools in a rigorously didactic way. The *Elements of Mathematics* devoted eighty pages to it, using Leibnitz's terminology.

The late reaction of the patriarchate against science

Adherence to the new sciences, which were accompanied by the philosophical and political ideas carried by the Enlightenment, was strongly shaken by the French Revolution, when a portion of the clergy as well as conservative circles of the Phanar (the Constantinople's quarter of Greek aristocracy) close to the Sublime Porte, feared republican impact on Greek partisans of the Enlightenment and atheistic fallout from the Revolution. As in Russia, even enlightened people in favor of new ideas made retractions. In 1805, almost ninety-year-old Voulgaris, then in Russia, gave his accord for the publication of his old manuscripts, a translation he had done almost fifty years earlier of the fourth part (titled "De systemate Universi") of a book by Fortunatus a Brixia (1701-1754), *Philosophia sensiuum mechanica methodice tractata*, originally published in 1747. This book presented cosmological systems from Plato to Newton, via the Church Fathers, Kepler, Gassendi, Descartes, etc., and it took the part of the Tychonian system that saved phenomena while remaining faithful to sacred texts.

The reaction of the supreme leader of the Orthodox *millet*, the patriarch of Constantinople, against the diffusion of new science culminated before the unleashing of the Greek national revolution and was triggered by the wind of revolt that blew through the Balkans after the Serbian uprising. At the same time, Orthodox reaction was favored by the counterrevolutionary ambiance created by the Holy Alliance of European powers, as well as by the weakening, after the Napoleonic wars, of the merchant caste, which had been the principal pillar of support for innovative scholars. This time around, reactionary forces were able to impose their will. Between 1819 and 1821, the principal Greek schools of the Ottoman Empire (in which the new program was taught) were either closed or had to change their orientation. The subject most affected by these measures was science.

The more the head of the Church hardened his position, the more aggressive the partisans of Enlightenment became. Nicolas Piccolo (1792-1865), a philosopher and physician of Bulgarian origin, published in 1820 a poem, barely allegorical, against the obscurantism of the Church, superstitions, and ignorance, and in favor of Western education. Piccolo denounced the "disgusting mob" that was tearing Greece apart, and despaired that a "band of monsters" had thrown itself on Smyrna (an allusion to the closing of the School of Smyrna by reactionary forces led by the Church). This diatribe provoked an immediate attack from the patriarchate of Constantinople, represented by the hegoumenos Ilariôn, the abbot responsible for educational matters and for the new Greek printing press of Constantinople. Ilariôn imposed censorship not only on what was published by the Greek printer, but on all books sold in Constantinople.[21]

The Church's opposition to teaching the new science took an institutional form with a patriarchal encyclical written in March 1821 that dealt with education:

> Everywhere there reigns a disdain for matters of grammar, and the arts of logic and rhetoric and the teaching of the elevated theology are completely ignored. This disdain and ignorance come from the exclusive love of students and professors for mathematics and science, and a cooling off for our faith […] For the Nation, the teaching of grammatical classes is more beneficial and necessary than mathematical or scientific classes […] for what is the advantage for the students who follow these courses to learn figures and algebra, cubes and cubocubes, and triangles and trigonosquares, and logarithms, and symbolic calculations, and projected ellipses, and atoms and voids and whirlpools,

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[19] Theotokis, Nikephoros. *Στοιχεία φυσικής εκ των νεωτέρων συνερανισθέντα* [Elements of physics compiled from the Moderns]. 2 vols. Leipzig, 1766 and 1767.

[20] For an overview of the life and work of Theotokis see Vlahakis, George. "Nikiphoros Theotokis, Scientist and Theologian." In Graham Speake, ed., *Encyclopedia of Greece and the Hellenic Tradition*, 2, London: Fitzroy Dearborn, 2000, p. 1673-74.

[21] For the N. Piccolo see Iliou, Philippos, *Τύφλωσον κύριε τον λαόν σου: Οι προεπαναστατικές κρίσεις και ο Νικόλαος Πίκκολος* [God, blind your people: Prerevolutionary crisis and Nicolas Piccolo], Athens: Poreia, 1988.

> and forces and attractions and weight, and qualities of light, and polar auroras, and optics, and acoustics, and thousands of similar and monstrous things, in order to count the sand on the shore and the drops of dew and move the earth if support is offered via Archimedes, yet they are barbarous in their speech and poor in their writing, ignorant in their religion, perverse and corrupt, noxious to politics, these obscure patriots, unworthy of the hereditary call.[22]

It was not rare in these tumultuous years for scholars to be denounced by the Church to the Turkish authorities as revolutionaries advocating the overthrow of the sultan. The patriarch Gregory V and the metropolitan of Chios, Plato, used this tactic against the director of the Chios Gymnasium Neophyte Vamvas, as did the metropolitan of Smyrna against Constantine Economos, director of the Smyrna Gymnasium. The struggle against science reached its paroxysm in March 1821 when the Holy Synod was convened in Constantinople in order to put a stop to "philosophical" classes. The exact date was March 23rd, and already Christians would be arrested and executed after the rebellion of Prince Ypsilanti in Romania, but the news of the Greek national uprising in Peloponnese had not yet reached the capital. Shortly afterward, on April 10th, the same Gregory V who condemned scholars as subversive elements would himself be hanged, on the order of the sultan, because he had not been able to contain the rebellion. The victory of this rebellion seven years later will dramatically change the geography of the Orthodoxy by dismantling the unifying orthodox *millet* in several independent states, with their own Orthodox Church and educational and scientific culture. These States, seeking for modernity, will found new educational structures comparable with those of Western Europe. Science will then be taught in a standard curriculum, following this of secondary and university level European education. From then on, science-religion debates will be comparable with those of Western Europe, and will focus on Darwin's ideas and materialism.

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[22] *Ibidem*, p. 47-48.

THE DIFFERENT HISTORIOGRAPHIES OF SCIENCE. THEIR ADVANTAGES AND SHORTCOMINGS

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 27.

THE DIFFERENT STRATEGIES IN HISTORIOGRAPHY OF SCIENCE. TENSIONS BETWEEN PROFESSIONAL RESEARCH AND POSTMODERN IGNORANCE

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Abstract

History of science, as a branch of knowledge, is a discipline of two countenances. On the one hand, as a kind of history, it is one of the humanistic disciplines. As this type of discipline it interacts with other humanistic disciplines, such as general history, methodology of history, philosophy (including epistemology, methodology, rhetorics, etc.), sociology, theory of literature, theory of cognition, etc. Moreover, history of science is one of the branches of knowledge called science studies or science of science, which also include philosophy of science (with epistemology, methodology, rhetorics, ontology, ethics, theory of values), sociology of scientific knowledge, and politics of science. Hence, history of science develops in strict relationships with the branches mentioned.
On the other hand, as a kind of reflection on science, history of science interacts with science itself (or more precisely, with particular sciences themselves).
Such a dual nature of history of science lays the groundwork for various approaches to this branch of knowledge, different methods of researching history of science and writing about it, based on different combinations of the so-called internal and external factors of science development. As a consequence, we observe the existence of a wide spectrum of different interpretations of the history of science. The mentioned spectrum extends from a detailed, professional case study approach, to the ignorant postmodern approach.
In this paper I would like to develop the issues sketched.

Let us introduce two terms at the very beginning: “the historiography of science” and “ a historiography of science”.

By the term “the historiography of science” I mean “the study of the way history has been and is [(added by M.K.:) analysed and] written — the history of historical writing” (Furay, Salevouris (1988) p. 223).

From the history of this branch of knowledge we know that there were and there are many different approaches to study the history of science. In consequence, we can show *many distinct historiographies of science*, that is, different ways, styles of studying and writing on the history of science.

Thus, we may conclude that *the historiography of science* is composed, among others, of *distinct, different historiographies of science*. Let us list them explicitly:

1. historiography of science analysed and written from the point of view of positivistic philosophy, that is positivistic historiography of science, which we may define as Montuclian, Whewellian, Tannerian, Machian, Sartonian, Duhemian, Conantian, Crombian, Neugebauerian, ...,
2. historiography of science analysed and written from the point of view of history of ideas, which we may define as Lovejoyan, Koyrian, ...,
3. historiography of science analysed and written from the point of view of contemporary philosophy of science, which we may define as Fleckian, Bachelardian, Popperian, Kuhnian, Lakatosian, Feyerabendian, Hackingian (historical epistemology), ...
4. external, social, sociological or socio-political historiography of science, which we may define as Marxian (Hessen's, Bernal's), Bloorian – Barnesian – Shapinian (*The Strong Programme in Sociology of Scientific Knowledge*), Collinian (*The Empirical Programme of Relativism*), Latourian – Woolgarian – Knorr-Cettinanian (*Ethno-methodological Approach*), Callonian – Latourian (*Actor-Network Theory*),
5. historiography of science analysed and written from the point of view of cultural studies of science), that is, cultural historiography of science or socio-cultural historiography of science (Dear (1995),
6. postmodernist historiography of science (propagated by "Postmodernists", but severely criticised by the "Friends of science" in the 1990s quarrel of the so-called "Science wars") – see: Sokal, Bricmont (1997), Sokal (2007),
7. rhetorical historiography of science, which we may define as Prellian, Grossian, Dearian, Woolgarian, and Mossian – see: Haris (ed.) (1997), Fahnestock (2008),

Then, using terminology assumed by Russell (1984), Harrison (1987) and Hughes (1997), among the above mentioned kinds of historiographies of science, we may differentiate between two groups:

1. *Whig historiographies of science*
2. and *prig historiographies of science*.

Or, using the terminology assumed by Brush (2007):

1. *Modernist historiographies of science*
2. and *contextual historiographies of science.*

Furthermore, it is also worth applying in this context the terminology applied by Daston (1989), Hacking (1992), (2002), Sturm, Feest (2008), Daston, Peter Galison (2007), Kusch (2009), Pisano, Gaudielo (2009), in their analyses of historical epistemology and to distinguish three kinds of historiography of science:

1. The objective historiography of science (recorded in text-books as a list of data along with the corresponding mathematical laws, and taught through its techniques and objective concepts).
2. The subjective historiography of science (that is, the history of thoughts and experiences of scientists).
3. The effective historiography of science (that is, historiography (a) of epistemic concepts, (b) of the objects of scientific inquiry, and (c) of the dynamics of scientific developments, as they can be extracted from an analysis of scientific texts or practices).

Then, irrespective of the differences between distinct historiographies of science, we can show a simple schema for all such historiographies. Namely, every historiography of science is always written from a specific point of view determined by a certain choice of a set of more or less clear assumptions, concepts and ideas. Let us call this point of view, *the interpretative core* of a historiography of science.[1]

We can explain what the role of the *interpretative core* of a historiography of science is using the following comparison taken from the theory and practice of photography, spectroscopy and spectrometry. Namely, the interpretative

[1] I go in this point on footnotes of several scholars such as Józef Tischner, Imre Lakatos, and Gerald Holton. See Kokowski [1997].

core is a kind of a colour filter that transmits a certain limited range of frequency of light. Hence, like a photograph can take different photos of the same objects using different colour filters, we can provide different pictures of the history of science using different *interpretative cores*, that is, using *the different historiographies of science*.

The interpretative core a historiography of science is composed of two strata:

- the purely declarative stratum (it is based on rational argumentations)
- and the non-declarative stratum of a specific climate of thoughts, ethos, spirit or vision (it permeates and supplements the argumentation).

Let us note that the diversity of historiographies of science is caused by two reasons:

- In each historical epoch, there is a variety of possibilities of writing on history of science. This variety of possibilities is caused by the fact that we want to analyse the different combinations of internal and external factors of development in science. To achieve this task, we need to pick out concepts, ideas, etc., stemming from, among others, related branches of history of science, such as philosophy of science, sociology of scientific knowledge, etc. In other words, we can choose different possible interpretative cores.
- Science itself changes its meaning along history (this is reflected, among others, in the different classification of science from ancient times to nowadays).

Furthermore, the interpretative core of each historiography of science is always determined by a certain understanding of science assumed by its authors. Namely, it expresses the answers for such fundamental questions as follows:

- What is science?
- Whether does science differ in something from other branches of culture or not?
- Whether can one identify science with its results only?
- Whether can one identify science with methods and results?
- Whether can one identify science with a certain social institution?
- Whether can one identify science with a certain kind of argumentation and rhetoric?

Hence, each interpreter of the history of science always assumes more or less consciously a certain philosophy of science and ... a certain sociology of scientific knowledge.

Let us notice that according to the statement of Imre Lakatos:

> "Philosophy of science without history of science is empty; history of science without philosophy of science is blind" (Lakatos (1971), p. 91).[2]

Then, according to a degree of the granularity or the level of detail of the interpretative core, we can say on:

- macro-historiographies of science (when we are able to describe science in general aspects)
- and micro-historiographies of science (when we are able to describe science in details).

To better understand the problem of variety of historiographies of science, I postulate to introduce another term – "The Perfect Historiography of Science". It is an abstract, ideal notion. "The Perfect Historiography of Science" is written by a perfect subject that has a perfect, absolute knowledge on the history of science. (In the margin, let us notice, that this subject may be called "God").

However, we all know people, not being perfect beings, have limited cognition of the world. This simple remark regards also to the results of our ways of interpretation of the history of science. Hence, we always create limited models of the history of science. In other words, every historiography of science is always only one of many possible cuts of *The*

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[2] Lakatos Imre (1971) *History of Science and Its Rational Reconstructions*, [in:] P. S. A. 1970 , R. C. Buck, R. S. Cohen (red.), Reidel, p. 91-135.

Perfect Historiography of Science. We can illustrate it, by saying that *The Perfect Historiography of Science* can be represented by a sphere, and *a certain historiography of science* – by a circle, one of its cuts.

In this context, the fundamental questions arise:

- Whether are we allowed everything (Fayerabend's "anything goes") in making our historiographies of science?
- Whether are we entirely free in creating the interpretative core of a historiography of science?
- Whether all possible interpretations are equally good?

I have the same answer for these three questions – "Unequivocally No!".

My further *thesis is: Every historiography of science has its advantages and shortcomings.* The great diversity of approaches to the study of the history of science brings about both positive and negative consequences to research in this branch of knowledge – both profits and losses, so to speak. On the one hand, plurality of attitudes can create new promising perspectives in research, if, for example, such approaches are complementary to each other. On the other hand, however, it can be very destructive to research, because it allows for instances of relativism, overspecialization, or for ignorance by authors and propagators of falsely based assumptions (that, nevertheless, have great influence on the broad public).

One problem noticed Edward Harrison in his article *Whigs, prigs and historians of science*, published in "Nature" vol. 329, p. 213–214 (17 September 1987).

> "The whig interpretation of the history of science, practised by most scientists according to historians, commits the crime of reconstructing past science in the context of today's science. The prig interpretation, practised by many historians, adopts a superior attitude to historical work by scientist, and from fear of being unhistorical commits what it supposes to be the lesser crime of being unscientific" (quoted from Hughes [1997] p. 21).
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> "By communicating the history of science only to historians, and deigning not to communicate it to scientists, the historian lives neither in the past nor the present, in a never-never land where ignorance is bliss, unable to evaluate (and necessary to discount) the effect of modern science on modern styles of thought" (quoted from Hughes [1997] p. 22).

In contrast to Harrison (1987), I would like to emphasize that the problem mentioned is much more complicated.

1. We can cultivate the history of science in two fundamental different ways. On the one hand, the history of a particular branch of knowledge can be researched as a part of this branch of knowledge. For example, the history of physics can be researched as a part physics. On the other hand, the history of a particular branch of knowledge can be researched as a part of general history. These are two distinct research perspectives that show different pictures of science.
   In the margin, it is worth noticing that Sarton's problem of two cultures –Snow (that is, the culture of Science and the culture of Humanities), and Sarton's third culture –Snow (that is New Humanism) manifest here.
2. The historian of science has every right to write on the history of science in a different manner than the scientist, if the former writes on science in which held true the other classification of science than modern one's.
3. Writing on the history of science, the interpreter (either scientist or historian of science, philosopher of science, sociologist of scientific knowledge, ...) are not allowed to be an ignorant in this branch of knowledge and related branches of knowledge.

Furthermore, we can classify all works on history of science according to the following schema. There are:

- very weak works (treating the subject on a very low level of competence; such works have often a great "impact factor"[3]),
- mediocre works (treating the subject on a weak level of competence – the phase of "normal science"),
- ordinary works (treating the subject in a normal level of competence – the phase of "normal science"),

[3] Nevertheless, I am very skeptical in regards to the application of the idea of "impact factor" and its use to evaluate the real importance and quality of these works. I agree in this point with the stance of the scientific editors, presented in two texts: The European Association of Science Editors (2008) and Andersen (*et al.*) (2009).

- mature works (developing the accepted axioms and treating the subject in a very competitive way),
- premature works (treating the subject in a fresh way, however with insufficient justification of the proclaimed theses),
- innovative works with a low "impact factor" (treating the subject in a very fresh and exciting way, however without a greater impact),
- innovative works with a high "impact factor" (treating the subject in a very fresh and exciting way, with a greater impact – the phase of "revolutionary science").

Then, on the hand, it is easy to show many examples of great competence in the field of history of science.

Among the mature works, we can mention, for example, the works of Jean-Étienne Montuc – – влович Юшкевич (1906–1993) on history of mathematics, the works of Alistair Cameron Crombie (1915–1996) on history of science, etc. Let us notice that these works represent the positivistic historiography of science.

Among the innovative works with a low Impact Factor, we can mention, for example, The *Genesis and Development of a Scientific Fact* by Ludwik Fleck published in 1935, that became popular among philosophers only when *The Structure of Scientific Revolution* by Thomas S. Kuhn was published in 1962.

The latter mentioned work is a good example of the innovative works with a high Impact Factor.

On the other hand, unfortunately, it is not difficult to show many examples of real ignorance in the field of history of science. And both scientists and historians of science, philosophers of science, sociologists of scientific knowledge, etc. are the authors of such wrong works. I give here only two extreme examples.

- The most common mistake proclaimed by scientists is the anachronistic myth that mature exact sciences were born only in modern times.
- One of many important errors made by humanists (mainly experts of literature theory) in the interpretation of the history of science is the creation of the so-called postmodernist historiography of science.

References

ANDERSEN, Hanne (et al.). (2009). *Journals under threat: a joint response from history of science, technology and medicine editors*, "Medizinhistorisches Journal", vol.44(1), p. 1–5.

BOWLER, Peter J. and Iwan Rhys MORUS. (2005). *Making Modern Science: A Historical Survey* (Chicago: Univ. of Chicago Pr.; http://books.google.pl/books?id=LEl3s-wYg10C.

BRUSH Steven S. (1974). "Should the history of science be rated X?" *Science*, vol. 183, 1164−1172.

BRUSH Steven S. (2007). "Suggestions for the Study of Science", in: Jürgen Renn, Kōstas Gavroglou, *Positioning the history of science* (*Boston Studies in the Philosophy of Science* vol. 248), Springer, p. 13–26.

BUTTERFIELD Herbert. (1931). "The Whig Interpretation of History" (New York: W. W. Norton & Company, 1931, 1965; London: Bell & Sons, 1968); Html edition for ©Eliohs by Guido Abbattista – February 2002; http://www.eliohs.unifi.it/testi/900/butterfield/.

DASTON, Lorraine. (1989). *Historical epistemology*. (In J. Chandler, A. Davidson, H. D. Harootunian (Eds.), *Questions of evidence: proof, practice, and persuasion across the disciplines* (pp. 282–289). Chicago & London: University of Chicago Press.

DASTON, Lorraine and Peter GALISON. (2007). *Objectivity*. New York: Zone Books.

DEAR, Peter. (1995). *Cultural History of Science. An Overview with Reflections*, "Science, Technology & Human Values", Vol. 20, No. 2, 150–170.

The European Association of Science Editors, *EASE statement on impact factors.* (2008); http://www.ease.org.uk/artman2/uploads/1/EASE_statement_IFs_final.pdf.

FAHNESTOCK, Jeanne. (2008). "The Rhetoric of Natural Sciences", in: Andrea A. Lunsford, Kirt H. Wilson, Rosa A. Eberly (eds.), *The SAGE Handbook of Rhetorical Studies* (SAGE Publications Inc), p. 175–195; http://books.google.pl/books?id=RYRf2JACLGkC

FLECK, Ludwik. ([1935] 1979). *Entstehung und Entwicklung einer wissenschaftlichen Tatsache. Einfü-hrung in die Lehre vom Denkstil und Denkkollektiv. Basel: Schwabe und Co., Verlags-buchhandlung; English translation: The Genesis and Development of a Scientific Fact.* Edited by T.J. Trenn and R.K. Merton, foreword by Thomas Kuhn. (Chicago: University of Chicago Press).

FULLER, Steve. (1997). "Who Afraid of Contemporary History of Science", in: Thomas Söderqvist (ed.), *The historiography of contemporary science and technology* (Harwood Academic Publishers); *http://books.google.pl/books?id=9YojdH5nawsC*; p. 245 – 260.

FURAY, Conal and Michael J. SALEVOURIS. (1988). *The Methods and Skills of History: A Practical Guide* (Wheeling, Illinois USA: Harlan Davidson).

GOLINSKI, Jan. (1998, 2005). *Making natural knowledge: constructivism and the history of science* (Chicago, London: University of Chicago Press; 2nd ed. with a new Preface. Princeton: University Press); *http://books.google.pl/books?id=SZcCElvmF7sC*.

HACKING, Ian. (1992). *" 'Style' for Historians and Philosophers." Studies in History and Philosophy* 23: 1–20; *http://www.hku.hk/philodep/courses/SeniorSeminar/ Hacking%20Styles%20for%20H&P.htm*.

HACKING, Ian. (2002). *Historical Ontology*. Cambridge, MA: Harvard UP.

HARRIS Randy Allen (ed.). (1997). *Landmark essays on rhetoric of science: case studies* (Hillsdale, NJ: Lawrence Erlbaum Associates); *http://books.google.pl/books?id=CGvK8pBsiSAC*

HARRISON Edward. (1987). *Whigs, prigs and historians of science*, "Nature" vol. 329, p. 213–214 (17 September 1987).

HUGHES Jeff. (1997). "Whigs, Prigs and Politics: Problems in the Contemporary Historiography of Science", in: Thomas Söderqvist (ed.), *The historiography of contemporary science and technology* (Harwood Academic Publishers,); *http://books.google.pl/books?id=9YojdH5nawsC*; p. 19–37.

HULL D.L. (1979). *In defense of presentism*, "History and Theory" Vol. 18, p. 1−15; reprinted in: David L. Hull, *The metaphysics of evolution* (Albany: State University of New York Press, 1989); Internet version: *http://books.google.pl/books?id=68KgxwN8W8cC*; p. 205–220.

JARDINE, Nick. (2003). "Whigs and stories: Herbert Butterfield and the historiography of science", *History of Science*, vol. 41, p. 125–140; *http://www.shpltd.co.uk/jardine-whigs.pdf*.

KOKOWSKI, Michał. (1993). *Próba uniknięcia podstawowego błędu filozofii fizyki Kuhna*, ,,Zagadnienia filozoficzne w nauce", XV / 1993, s. 77–98.

KRAGH, Helge. (1987, 2003). *An introduction to the historiography of science* (Cambridge University Press, 1987; first paperback edition 1989, reprinted 1991, 1994; transferred to digital printing 2003); *http://books.google.pl/books?id=XOpELRq2GuUC*.

KUHN, Thomas S. (1962). *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press).

KUHN, Thomas S. (1968). "The history of science", in: In*ternational Encyclopedia of the Social Sciences* Vol.14, 74−83 (Macmillan, New York).

KUSCH, Martin (2009). *Reflexivity, Relativism, Microhistory: Three Desiderata for Historical Epistemologies*. Forthcoming in a special issue of "Erkenntnis", edited by Uljana Feest and Thomas Sturm.

MCMULLIN, Ernan (ed.). (1992). The *Social Dimension of Science* (Notre Dame, Ind).

PATTERSON Annabel. (2003). *Nobody's Perfect: A New Whig Interpretation of History* (New Haven, Conn.: Yale University Press). Pp. viii, 288. $27.50. ISBN 0–300–09288–1.

PISANO Raffaelle, Gaudielo ILARIA. (2009). "Continuity and discontinuity. Epistemological inquiry trough categories in history of science", *Organon*, vol. 41, p. 245–265.

RENN Jürgen, GAVROGLOU Kōstas. (2007). *Positioning the history of science* (*Boston Studies in the Philosophy of Science* vol. 248) (Springer).

RUSSELL Colin. (1984). "Whigs and professionals", *Nature* vol. 308, 777–778 (26 April 1984).

SHAPIRO Alan E. (1998), "Historians of Science Must Again Master Scientific Substance", *The Chronicle of Higher Education* (*http://chronicle.com*), XLIV, 21 (February 20, 1998), B4–B5; *http://eh.net/pipermail/hes/1998-February/004122.html*.

SOKAL Alan D. (1996). "Transgressing the Boundaries: Toward a Transformative Hermeneutics of Quantum Gravity", 'Social Text Affair' No. 46/47, pp. 217–252 (Spring/Summer 1996); *http://www.physics.nyu.edu/faculty/sokal/transgress_v2_noafterword.pdf*.

SOKAL Alan D. (2009). *Articles on the 'Social Text Affair'*; *http://www.physics.nyu.edu/faculty/sokal/* (last modified 20 March 2009).

SOKAL Alan D., BRICMONT Jean. (1997). *Impostures Intellectuelles*. (Editions Odile Jacob); eng. translation: *Fashionable Nonsense: Postmodern Intellectuals' Abuse of Science* (New York: Picador USA, 1998).

STURM, Thomas and Uljana FEEST. (2008). *What (Good) is Historical Epistemology?* Berlin: Max Plack Institute for the History of Science, preprint No. 386.

TOSH Nick. (2003). "Anachronism and retrospective explanation: in defence of a present-centred history of science", *Studies In History and Philosophy of Science Part A*, Vol. 34, Issue 3, September 2003, p. 647–659.

"Whig history"$_1$; *http://dic.academic.ru/dic.nsf/enwiki/179093*.

"Whig history"$_2$; *http://en.wikipedia.org/wiki/Whig_history*.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 34.

EPISTEMOLOGICAL ASPECTS OF HISTORIOGRAPHY OF SCIENCE IN GREECE

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Abstract

This paper examines the epistemological aspects of the course of development of the discipline of history of science in Greece focusing on three controversial themes: i) "Continuity" with the ancients, ii) reception of modern science in the Greek speaking communities in the 18th- early 19th centuries and finally iii) the issue of teaching the history of science in the science classroom.
Starting with the work of the first Professor of history of science, Michael Stefanidis, this paper examines the transition from a discipline of history of science dominated by the attempt to establish the continuity of Greek Science from the ancients onwards up to the last decades of the 20th century when a new generation of historians of science, which appears on the scene in the late 1970s, prevailed in the cultural life of Greece.
This new generation considered the argument for "continuity" as an ideological construction, and turned its attention in the study of the post-Byzantine era, in a period that has been called "Neo-Hellenic Enlightenment", and focused on the study of reception and assimilation of the ideas of the 17th century scientific revolution in Greek speaking communities.
Finally, this work examines the newly established relations between the historians of science and the historians of science education considering the study of the two fields tightly connected in the Greek framework and discusses the epistemological problems arisen from proposals and attempts to incorporate history of science in the teaching of science in line with the developments in the international community.

Michael Stephanides, the epigones and the issue of "continuity"

Michael Stephanides (1868-1957) is considered by many to be the first Greek historian of science. Stephanides studied philology and science at the Faculty of Philosophy of the University of Athens. The science chair at that time belonged to the Faculty of Philosophy. This unusual combination in this epoch ascribed to him the nickname "physico-philologist" by his fellow students. Being a scientist, philologist and historian, he had the ideal profile for addressing the emerging field that history of science was at that time.

Stephanides in 1896, earned his living as director of the Ottoman "Chemistry Civil Service of the Aegean Sea"[1] and at the same time he was professor of physics and history at the Gymnasium of Mytilene (the capital of the island of Lesbos).

During his service in secondary education, he published the *Mineralogy of Théophraste*, in which he compares the old and modern taxonomies of chemical elements. This is the first Greek book of professional history of science escaping the encyclopedic approach of previous works, written mostly by scholars trying to present developments in their field as scientific. *Mineralogy* is also the first Greek book of History of Science with an impact in the international community of history of science. In the following period of his life, Stephanides pursued a research career. After writing an article (1904) about Benjamin of Lesbos, his compatriot scholar of the 18th-19th century, he devoted himself to the study of Alchemy, publishing in 1909 his most famous scientific work: *The Art of Psammurgique and Chemistry*. In this book, he studied "psammurgique" vis-à-vis the Arab alchemy and Western "χρυσοποιι′α" (gold making) arguing that the Greek, Alexandrian and Byzantine alchemical schools are very distinct from the corresponding Arab and Western.

Stephanides was deeply influenced by the historiographical ideology of the modern Greek State, which advocated the "continuity" of Greek history and consequently continuity of science from antiquity to the 19th century, distinguishing three historical stages: ancient Greece, Byzantium and Modern Greece (which begins in 1453). Under this scheme, Stephanides published several studies on the history of Sciences in Byzantium and also at the time of Ottoman rule. Stephanides promoted a schema of continuity of the Byzantine sciences with the Hellenistic Alexandrian heritage. He was also the first to claim that during the period of Greece under Ottoman rule there was continuity in science education, which reinforces his thesis of continuity of ancient Greece until the late 19th century. This thesis, which implies that the modern Greeks are heirs of "Hellas" has been deeply influenced by the national state ideology and has been used extensively by state intellectuals to render the newly born modern Greek state a cultural coherence.

In every article Stephanides was writing, he tried to demonstrate the contribution of Ancient Greeks and Byzantines in all the areas it addresses. Nevertheless, his works are of value because they study a field largely unexplored: the sciences in Byzantium.

Chronologically, Stephanides is placed between Paul Tannery and Joseph Mogenet, the first having studied the field of mathematics in Byzantium, the second that of astronomy, while Stephanides has mostly dealt with that of alchemy (Nicolaidis 2010).

In 1924, while he was assistant professor of history at the University of Athens, he was successful in founding a chair of history of science at the Faculty of Science, which he held until his retirement in 1939. This is one of the first chairs of history Science in the world academia. Two factors played a significant role for the foundation of this Chair: i) the importance of the antiquity (including Hellenic science) for the formation and the constitution of the ideological apparatuses of the Greek state and ii) the personality of Stephanides himself, who was recognized as a prominent member of the international community of history of science. Indeed, he maintained a correspondence with George Sarton who encouraged him in his research on the sciences in Byzantium. He also was a member of the International Academy of History of Science publishing widely in various international journals and participating in international conferences. It has to be noted at this point that Stephanides was present in the Second International Conference of History of Science in London in 1931. This Conference acquired importance due to the presence of the Soviet delegation under Bukharin and the subsequent initiation of the field of Social History of Science. This remained largely unnoticed by Stephanides who wrote in his report that the Soviet papers given in the conference were not all related to history of science and makes no reference at all to the paper by Boris Hessen.

Stephanides was elected member of the Academy of Athens in 1938. In the same year, he published his textbook *Introduction to the History of the Sciences* which is a tribute to ancient Greek Science. Indeed, 240 of the 290-pages of the book are devoted to Greek and Byzantine Science and moreover, he argues that the method of the Ancients do not differ to that of the science of his time.

The personal success of Stephanides did not have any benefit for the scientific community of Greece. We do not see at this time the birth of a community of historians of science in Greece. Stephanides's successor to the Chair of History of Science, Christos Papanastassiou has left no work.

Nevertheless, during the decades that followed the retirement of Stephanides, history of science has a presence, although weak, in Greek publishing.

In 1940, Konstantinos Mermigas published his *History of sciences* which no longer insists on the greatness of the antiquity but on the achievements of modern times. Namely, only 350 out of the 500 pages of his book are about classical science.

Fifteen years later, another book was written by K. Logothetis[2]. This book was a series of biographies of men of science and philosophers, in chronological order and without any connection between them. The interesting fact is that this

[1] The Ottoman Empire controlled at that time several islands, including Lesbos Island the birthplace of M. Stephanides.

book was published by the 'state organization of school textbooks', which opened the door of secondary education for the distribution of the book as a valuable reading for teachers.

Neither Mermigas nor Logothetis were historians of science and their books were merely historiographical compilations. At about the same period Evangelos Stamatis edited and published brilliant works of Archimedes, Apollonius and Euclid. Stamatis was a secondary school teacher and had studied Physics in the 1930s in Germany. Very reactionary in his political beliefs, an admirer of the ancients, he published in the 1970's excellent editions of these three scholars of Antiquity. His works enjoyed the support of the 'state organization of school textbooks' and of the Chamber of Technology of Greece (Chamber of Engineers). With Stamatis a whole era is coming to an end. Stamatis is the last of a generation of historians of science who worked most of the time in isolation from the international community of historians of science.

The notion of "neo-Hellenic Enlightenment" and the study of "reception"

In the late 1970s after the fall of the dictatorial regime (1967-1974), an innovative spirit arose on the scientific community of Greece, with the arrival from abroad, where they were in voluntary political exile, of scholars who had established a career in European Universities.

Among those the most prominent figures were Yannis Karas, Nicolas Svoronos, Kostas Krimbas and Eftychis Bitsakis. With their arrival, we see in Greece the birth of a community of historians of science that will quickly develop in the next three decades. The common characteristic of this generation is its affiliation to the left which at that time, in response to nationalist ideas of the military junta, wanted to cease its links with the "glorious past" of antiquity and Byzantium and promoted the study of modern and contemporary issues. Moreover, it was this leftist political affiliation, which in the context of the critical science of the 1970s, motivated these scholars to be interested in the philosophy of science as well as its history.

The decade of the 1980's, gave birth for the first time to a scientific community of historians of science in Greece that was incorporated in the corresponding international community and which followed international research trends in the field.

This new generation considered the argument for the continuity as an ideological construction and turns its attention to the study of the period that has been called "Neo-Hellenic Enlightenment" and focuses on the study of the reception and assimilation of the ideas of the 17th century scientific revolution in the Hellenic communities.

Yannis Karas initiated the aforementioned field studying the development of science in Greece during the time of the Ottoman domination. He was employed in the Centre of Neo-Hellenic Research of the National Hellenic Research Foundation where he established the Research Program "Influences of the European Scientific Thought in the wider Greek and Balkan region". Karas turned his attention in what has been called "Neo-Hellenic Enlightenment" (the reception and assimilation of the ideas of the 17th century scientific revolution in the Hellenic communities). His work incorporates research on primary sources, connections of the history of sciences with the political, economic and ecclesiastical history, connections with the philosophical currents and the international political developments.

Indeed, Mathematics, Physics, Biology, Geology, Geography, Astronomy and Medicine have been studied by the Greek scholars of the 18th century, at a level that cannot be considered as elementary. Recent research has shown that the Greek scholars of the 18th century worked on an ideological "project" in order to enlighten the Greek speaking communities of the Balkan region and to guide them to a national revival, which was also scientific, social and political (Nicolaidis 2010).

Greek scholars of the Enlightenment considered that the Greek communities of the Ottoman Empire (belonging to the scientific periphery of Europe) should prove that they were a component of the western civilization. These scholars tried to transfuse in these communities the knowledge they acquired during their studies in European universities.

The scholars of the periphery are not passive agents whose only function was to distribute locally the knowledge delivered to them from the centres of Europe, but rather as active subjects who received no particularly clear directions on how to dispose of them locally.

When we examine issues of reception it is required to discuss the ways in which ideas that originate in a specific cultural and historical context are introduced into a different one with its own intellectual traditions as well as political and educational institutions.

The issue of reception of western science in the Greek speaking communities of the Ottoman Empire is part of a discourse that has developed internationally during the last decades, concerning the ways of diffusion of the dominant western science and technology. There is also a growing interest on comparative studies of the different scientific and technological traditions of the peoples of the world. The approach of these themes is not limited only in recognizing the

[2] "The Philosophy of Renaissance and the Foundations of Modern Physics" (1955)

cultural "specificities", but it goes further on to examine and reveal the relations of power and dependence, as well as the uses of science by the dominant social classes (Blay and Nicolaidis 2001).

Whether this project will be successful remains to be seen, given the controversial nature of factors involved, whither institutional, political, ideological and/or scientific. What is beyond any doubt however is the will of researchers involved to carry out the project whose importance transcends national borders and can be considered as a model case for other countries of the European scientific periphery.

Karas and his research associates in NHRF have organized a series of Conferences in the 90's focusing on the reception of the ideas of scientific revolution in Greece. When Karas retired his work has been continued by Efthymios Nicolaidis as the Head of the Program of "History and Philosophy of Science" in NHRF.

In the beginning of the 1990s, the growth of Greek Universities, allowed members of the community to create new institutions, apart from the already existing National Hellenic Research Foundation and the National Technical University of Athens. The culmination of these developments was the foundation of the Department of History and Philosophy of Science in the University of Athens and the publication of the journal *Neusis*.

During the 1990s also, the Department of History and Philosophy of Science in the University of Athens, initiated the project *"Hellinomnimon"* under the supervision of Prof. C. Gavroglou. This project, which has assisted greatly in the research on the reception of scientific theories in Greece, consists of a collection, in digital form, of all the writings of the Greek scholars of the 16^{th} - 18^{th} century and has been constantly enriched. The same group also participates in STEP (Science and Technology in the European Periphery) an international research group focused on the study of processes and models of circulation of scientific and technological knowledge between European centres and peripheries from the 16^{th} to 20^{th} century[3].

The issue of "teaching"

After 2000, a new group research group appears in the Greek academic scene called "History, Philosophy and Didactics of Science and Technology" – HPDST. HPDST originates from two complementary groups, the History and Philosophy of Science and Technology group of the National Hellenic Research Foundation and the group for the Didactics of Science of the Department of Education of the University of Athens. The merge was brought through the acknowledgment that there is a strong need for science to be re-established in society and engage its stakeholders. This goal is to be accomplished through state of the art research using an interdisciplinary approach. Since 2000, HPDST has developed into one of the most promising research teams in South-eastern Europe.

The epistemological issue arising is whether these two Disciplines i.e. History of Science and Didactics of Science can complement each other. The main question one may ask is: Whether Teaching of the History of Science in the science classroom is legitimate.

The first attempt to fuse these two research traditions mentioned above following developments in Greek Science Education was made by Andreas Kassetas, a secondary school physicist.

The second attempt was made through the organization of a series of Conferences in Thessaloniki (2001), Athens (2003), Athens (2005) and Patras (2007). These biennial conferences have been complemented by a series of other initiatives such as the formalization of the "Hellenic Society of History, Philosophy and Didactics of Science" following the Meeting organized in Athens in September 2007.

In the autumn of 1980, during the 2nd National Conference of the Hellenic Physical Society, Andreas Kassetas made the first reference for the need to incorporate elements from the history of physics in physics teaching in Greece. Which were the theoretical milestones of this proposal? According to A. Kassetas' own writings his theoretical sources included: The antipositivism of T. S. Kuhn's Structure of Scientific Revolutions, Arthur Koesler's Sleepwalkers, Yannis Karas' History of Greek Science, John Bernal's Science in History, Rene´ Taton's Histoire General des Sciences and above all the Harvard Project Physics Course.

A few years later, the Hellenic Physical Society appointed A. Kassetas and his team (his teacher in Athens University S. Mourikis and his friend and colleague N. Dapontes) to write a Physics textbook for the 10th and 11^{th} grade students (1st and 2nd grade of the Greek Lyceum). The textbook was written following the general line of the Harvard Project with certain variations and differences though, which reflect the originality of the authors' contribution in Physics Teaching. The textbook was endorsed by the Ministry of Education and was printed and distributed to "polykladika" Lycea. In the Greek educational system of those days there were two types of Lyceum: the "polykladikon" and the general. In the "polykladikon" Lyceum a

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[3] Science & Education 15, Nos 7-8, 2006, Special Issue: Textbooks in the Scientific Periphery

student could follow specialized and more intense Physics courses while the General Lyceum was oriented more on general education.

There were fourteen "Polykladika" Lycea in Greece with nearly 10,000 students. Kassetas' textbook has been taught for 12 years (1984–1996) while the revised edition of the book was taught for another 4 years (1996–2000) in all the Lycea of the country.

Unfortunately, there was no official state committee to evaluate the outcome of this attempt. We have in our hands only one evaluation report from 6 Lycea in the Athens region based on a questionnaire distributed among the physics teachers of the corresponding schools. The structure of the questionnaire is fragmentary, reflects only the personal opinion of the teachers and there are no any quantitative data to support solid conclusions. However, the general feeling is that the majority of Greek Physics teachers were not adequately prepared to teach the historical material contained in the book, since they did not have any formal education in the history of science, or even any training in the logic of the Harvard Project before the official introduction of the book. Their usual tactic was to omit the historical information or the paragraphs dealing with epistemological matters.

Kassetas estimates that only about 30–40% of teachers felt comfortable with the book and embraced it wholeheartedly in its first phase. The growing opposition to the book led to its revision in 1996 when a lot of the historical material was left out and finally to its replacement in 2000 by another textbook (Skordoulis & Halkia 2005).

It is interesting to note though that the main argument against Kassetas' book was based on a naive empiricism advocating "the introduction of experiment" as the solution to any problem that Greek physics teaching faced.

In this context, it is not surprising that the introduction of a History of Science and Technology course, on a compulsory basis, in the Greek Lyceum in 1999, supported by a textbook written by staff of the Department of History and Philosophy of Science (Gavroglou et al.), faced harsh opposition from both teachers and students and it was made elective the following year.

Following these events, it became a common belief among scholars in the Departments of Education of the Universities of Thessaloniki and Athens that only a process of systematic teacher education and training on both undergraduate and postgraduate level in History, Epistemology and Didactics of Science linked with research work on the conditions that shape the educational environment from the 19th century onwards and in constant dialogue with the international community holding similar views can create a fertile ground for the success of HPST in Greece (Skordoulis 2009).

A vast literature already exists trying to answer this legitimate question. Summarizing, teaching history of science assists in Abolishing the "Two Cultures" Dichotomy, to show the students that science is part of human culture and that science is a human activity, to improve the image of scientists in Society, to change students' attitude towards science and also attract highly qualified students to science courses.

All the above may sound highly utilitarian. But does the teaching of the history of science in the science classroom bring an improvement in students understanding of the scientific concepts? Do students that have been taught history of science have a better understanding of basic scientific concepts than those who have not? Do they perform better in science courses?

This is hard to answer emphatically. What has been proved by research in the relevant field is that Teaching History of Science improves critical thinking and problem solving skills and students acquire a better understanding of the Nature of Science (NOS). The objections of colleagues criticizing the teaching of history of science in the science classroom are based on the ideas developed by Thomas Kuhn in his "Essential Tension" (1977).

Kuhn's main argument is that Teaching History of Science in the Science Classroom undermines the dedication of students to Normal Science, to the dominant 'Paradigm', and that history of science in the science textbooks is a pseudo-history i.e. a history of science that has undergone a Didactical Transposition. The notion of 'Didactic transposition' was firstly introduced by the French sociologist Verret (1975) and was used extensively in the field of Mathematics Education by Chevallard (1985). According to this classical approach, scientific knowledge is transformed to school science after specific educational system (institutional) actors have functioned upon it. Some years later, Martinand (1986) showed that it is not only the formal (institutional) educational system but also the social practices that play a crucial role in the didactic transposition of a scientific concept. More recently, Clement (1998) has added the value system as a determining factor in the didactic transposition.

It is a fact that after having gone through its phases of infancy i.e. admiration of the ancient science, history of science in Greece runs now a mature period where current research trends are integrated with those of the international community of Historians of Science and also of science educators. The HPDST group has developed partnerships with European research groups and built a network of historians of science and technology in South-Eastern Europe. To support these goals, the group publishes a bilingual journal (*Kritiki:* Critical Science and Education), an international journal (*Almagest*, International Journal for The History of Scientific Ideas) and the *Newsletter for The History of Science in*

Southeastern Europe. The role of this group in SE Europe has been further strengthened by support from the European Union for its future development (program Hephaestus, REGPOT 1-2008).

References

BLAY M., NICOLAÏDIS E., (2001), *L'Europe des sciences : constitution d'un espace scientifique*, ed. Seuil, Paris

CHEVALLARD, Y., (1985), La transposition didactique, du savoir savant au savoir enseigné, La pensée sauvage, Grenoble, p39

CLEMENT, P., (1998), La biologie et sa didactique. Dix ans de recherches, *Aster* (INRP, Paris), 27, pp.57-96

KUHN T. S. (1977), 'Concepts of Cause in the Development of Physics' in his *The Essential Tension*, University of Chicago Press, pp. 21-30

MARTINAND, J.L., (1986), *Connaître et transformer la matière,* Peter Lang, Berne.

NICOLAIDIS, E. (2010), L' Histoire Des Sciences En Grèce: La Naissance D' une Communauté Scientifique, *Archives Internationales D'Histoire Des Sciences,* 60, 5-11

SKORDOULIS C. D. & HALKIA L. (2005), "Introduction: Notes on the History, Philosophy and Science Teaching in Greece", *Science & Education* Vol.14, No. 7-8, 601-605

SKORDOULIS C.D. (2009), "Hellenic Studies in History, Philosophy of Science and Science Teaching: New Perspectives", *Science & Education*, vol. 18, 1193-1197.

VERRET, M., (1975), *Le temps des études*, Librairie Honoré Champion, Paris.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 40.

ON THE HISTORIOGRAPHY OF MATHEMATICS IN ITALY

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Abstract

The historiography of mathematics in Italy has a long tradition dating back to the Renaissance (Bernardino Baldi, 201 biographies in his Vite de' matematici*). Moreover, Italian scholars contributed to the transmission and history of science with translations, commentaries and editions of ancient classics, and chronologies were included in encyclopedic treatises in the sixteenth century.*
In a modern sense, however, the historiography of mathematics begins in the eighteenth century, when the history of mathematics was considered an area of the history of human thinking. In Italy it was initially developed as a part of Italian literature and inserted into general works (Giovanni Andres, Girolamo Tiraboschi). Critical analysis of mathematical theories and their historical foundations can be found in works of mathematicians like Joseph-Louis Lagrange, Gregorio Fontana, and Pietro Cossali. Giambattista Guglielmini, at beginning of the nineteenth century, provided, with his eulogy of Leonardo Pisano, a widely followed model in the historiography of the century: expositive text and extensive bibliographic notes. An important work, which combined general overview, technical and archival investigation came out later: the famous Histoire des sciences mathématiques en Italie *by Guglielmo Libri (4 volumes 1838-41). The historiography of mathematics developed in the second half of the century, after the political unification of Italy, with Baldassarre Boncompagni's foundation of the* Bullettino di bibliografia e di storia delle scienze matematiche e fisiche *(20 volumes), which supplied both an international diffusion and primary sources. Antonio Favaro was the editor of Galileo's collected works, and Pietro Riccardi published a bibliographic work still of great importance, the* Biblioteca matematica italiana*. In the first half of the twentieth century, two mathematicians became the main historians of mathematics: Gino Loria and Ettore Bortolotti. The historiography of mathematics in Italy has increased with the publication, from 1981 on, of the* Bollettino di storia delle scienze matematiche *(29 volumes).*

The historiography of mathematics in Italy has a long tradition which dates back to the Renaissance. Inaugural orations of the new academic year at universities cite lists of illustrious mathematicians and astronomers like that of Luca Gaurico in Ferrara in 1507. The sixteenth century has left us a wide biographical repertoire by Benardino Baldi on the lives of mathematicians. In the 17th century it was encyclopaedic works on mathematics which contained biographies and chronologies of mathematics, like those of Biancani, Riccioli, and Milliet Deschales. In the 18th century, the history of mathematics developed, as part of a more general history of culture, in the general works of history of literature like those of Giovanni Andres or Girolamo Tiraboschi. This century, however, saw the beginning of historical investigations of mathematical theories, and, at the end of the century, we find the extensive work by Pietro Cossali on the transmission of algebra to the western world (Parma 1797-98). The history of mathematics plays a role in the mathematical works of Lagrange, where long introductions and historical notes were often inserted to create a link with the research of the past, and present modern theories as their final evolution.

In the Napoleonic period research into the history of mathematics underwent several developments in the surge of a national feeling and the vindication of Italy's glorious past discoveries. Giambattista Venturi proposed the scientific work of Leonardo da Vinci, and a history of optics, Mariano Fontana the arithmetic of Maurolico, Giambattista Guglielmini made in-depth research into the figure of Leonardo Pisano and mathematics of the Middle Ages (Bologna, 1813). This line of research also continues for the Restoration period through the works by Pietro Franchini, and the passionate in-depth study of the history of mathematics in Italy from the Renaissance to the 17th century (1838-41) by Guglielmo Libri.

Political unification injected more vigour and encouraged the extension of studies of the history of mathematics. Three figures emerged whose works also provided the indispensible tools for the development of research into the history of mathematics: Baldassarre Boncompagni, Antonio Favaro and Pietro Riccardi, the tools being: a specialised journal (the *Bullettino di bibliografia e di storia delle scienze matematiche e fisiche*), publications of rare or unedited mathematical works (the National Edition of Galileo's collected works), a bibliography of every publication concerning the history of mathematics in Italy (*Biblioteca Matematica Italiana*).

At the same time the history of mathematics was further improved by some of the most illustrious scientists: Luigi Cremona, Eugenio Beltrami, and Felice Casorati, whose aim it was to retrieve the true origins of the theories they were developing in the works of mathematicians of the past.

Specific studies at the turn of the century included: the edition, by Federigo Enriques, in 1900, of a collection of mathematical theories introduced and developed historically, fundamental to the formation of teachers of mathematics (*Questioni riguardanti le matematiche elementari*); the first critical history of non Euclidean geometries by Roberto Bonola, contributions to the history of logic by Giovanni Vailati with his essay on Saccheri's work, and the first historical papers on ancient Chinese mathematics by Giovanni Vacca.

As for critical essays and books of the early nineteenth century, first of all mention must be made of Gino Loria, a mathematician by formation, considered the most notable historian of mathematics of his day. Continuing in Boncompagni's footsteps, he, in his turn, founded the *Bollettino di bibliografia e storia delle science matematiche* in 1898 and published texts which became classics like the history of descriptive geometry, the history of mathematics and exact sciences in ancient Greece, the theory of algebraic and transcendent plane curves. Ettore Bortolotti also had a mathematical formation and his major contribution was to the history of Renaissance and seventeenth-century mathematics, by publishing Ruffini's complete works and reconstructing the most successful discovery of Renaissance mathematics: the solution of cubic equations and the origin of complex numbers.

The last contributions of the first half of the 20th century were given by Ugo Cassina, on the history of Peano's logic, Ettore Carruccio, Attilio Frajese (the editions of Euclid's and Archimedes' work in Italian) and Ludovio Geymonat on history and philosophy of infinitesimal analysis.

In the research of these Italian historians of mathematics one common feature emerges: namely, that of underlining elements of continuity in the history of ideas, as if mathematics in Italy had developed along a continuous line with successive contributions of various authors. Their contribution to historiography of mathematics has already been remembered elsewhere, so I will concentrate on more recent research in this field in Italy.[1]

After the Second World War

With the reconstruction of the university system following the second world war, the study of the history of mathematics was once again taken up, which, like mathematical studies, had to get back into contact with the international output that had been interrupted by the war and the last decades of the fascist period. This break was less serious for the history of mathematics which, by tradition, concentrated studies on more antique periods and celebration of moments of national glory.

Studies of the history of mathematics were mainly developed in Rome, where the school of Federigo Enriques proceeded through his pupils: Attilio Frajese, above all, took over teaching of the history of mathematics when Enriques had been removed from his post as a result of the racial laws. Frajese's work as a historian of mathematics, consolidated in over sixty articles and various books, mainly dealt with the mathematics of Ancient Greece, and he was considered Italy's top

[1] For more details, see: F. Barbieri, F. Cattelani Degani (eds.), *Pietro Riccardi (1828-1898) e la storiografia delle matematiche in Italia*, Bologna, Tecnoprint, 1989; M.T. Borgato, 'On the History of Mathematics in Italy before Political Unification', *Archives Internationales d'Histoire des Sciences,* 42 (1992), 121-136; U. Bottazzini, 'Italy', in: *Writing the History of Mathematics: Its Historical Development*, ed. By J.W. auben, C.J. Scriba, Birkhäuser, 2002, 61-95.

expert in the field owing to his knowledge of ancient languages which allowed him to read texts in the original languages (Greek, Latin and Arabic). His profound knowledge of Greek mathematics of the Classic period led him to translate, in collaboration with Lamberto Maccioni, Euclid's *Elements* and the works of Archimedes. Linked to the Frajese school was the research of Silvio Maracchia, one of his pupils, who took up his position in the "Sapienza" in Rome.

In Turin, the tradition of the history of mathematics was carried on by Tullio Viola, above all in the field of mathematics in pre-history and the mathematics of ancient civilisations (Babylonia, Egypt etc.). Viola had been an assistant to Enriques in Rome, as well as Ettore Carruccio, who also belonged to the school of Federigo Enriques, taught history of mathematics at the University of Turin from 1955 to 1975, and then in Bologna. The main focus of his works on the history of mathematics was the history of mathematical logic.

Lucio Lombardo Radice is, perhaps, the greatest exponent of another way of dealing with the history of mathematics: a politician and mathematician involved in mathematics education, he wrote general histories of mathematics which were strongly influenced by his philosophical line of thinking (*La matematica da Pitagora a Newton*, 1971) as well as important translations (Nikolaj Ivanovič Lobačevskij, *Nuovi principi della geometria*, 1974).

In Lucca, Gino Arrighi, a scholar of mathematical formation and remarkable erudition, carried out research into a field which, largely unexplored at that time, later became very productive for the reconstruction of the mathematics of the Middle Ages and Renaissance period: the manuscript texts of the school of arithmetic which flourished in Tuscany, above all in Siena and Florence from the 13th to the 16th centuries. Arrighi published about twenty treatises and two hundred essays. Successive research works took up this line of study like those of other historians of algebra such as Laura Toti Rigatelli and Raffaella Franci, who, in 1980, founded the Centro Studi della Matematica Medioevale in Siena.

Another learned scholar, Angiolo Procissi, has left works on both the history of algebra and the theory of numbers, as well as bibliographical works; in particular we may remember the bibliography of the Galilean manuscripts preserved in Florence.

The number of universities teaching the history of mathematics, always as part of the degree course in mathematics, was very scanty in Italy. The history of mathematics was, moreover, considered as a complementary discipline to related subjects in the formation of secondary school teachers (foundations of mathematics, didactics of mathematics, complementary mathematics, elementary mathematics from a higher point of view), and generally as a less important discipline compared to pure and applied mathematics. The first university chair for the history of mathematics was given to Ettore Carruccio in Bologna in 1975, but it was left empty shortly afterwards due to Carruccio's having reached the age limit for such a post. Umberto Bottazzini was then awarded the teaching post but it was it was only several years later that a national competition for a chair in the history of mathematics was established.

Even at the beginning of the 1970s the number of scholars in the field was fairly restricted and output was limited to themes initiated by Enriques and a few strictly scholarly works. At the end of the 1970s, however, new contributions to the discipline were made by various scholars, often of strictly mathematical formation, who carried forward the history of mathematics autonomously. Reflection and evaluation of the output in this field was, therefore, imperative.

The historiography of mathematics, when it is not just a question of biography, chronology or bibliography, requires a specific formation; in Italian universities, since historians of mathematics mostly have a mathematical formation and teach the history of their own discipline, they often specialise in related sectors, such as, history of algebra, history of mathematical analysis, mechanics etc. If, on the one hand, this fact allows us a greater insight into mathematical principles on the basis of the different theories, on the other, the risk is a levelling out of the historical interpretation based on modern ideas and language and a disproportionate importance being awarded to the results of one or another mathematical school. Historiographic problems of this type were discussed in preliminary and general terms, when the community of mathematical historians in Italy got together to organise the creation of a national network of scientific relations and co-ordination.

A first step forward took place on the occasion of the conference held in Ferrara in 1981 on Gianfrancesco Malfatti, an Italian mathematician of the second half of the eighteenth century.[2] The initiative to promote these studies, by bringing together the various scholars who have made individual contributions to them, originated from two professors of mathematical analysis: Enrico Giusti and Luigi Pepe.

A second meeting on the history of mathematics in Italy followed in Cagliari in 1982, where an overview of the many different fields developed by Italian researchers and new themes, or new points of view, were presented.[3] A third meeting on critical editions took place in Trento in 1985.[4] This meeting highlighted the basic features essential to critical works: philological accuracy, with no concessions to modernizations and no direct intervention on the text, a body of essential

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[2] *Gianfrancesco Malfatti nella cultura del suo tempo*, Ferrara, Università degli Studi, 1982.

[3] *La storia delle Matematiche in Italia. Atti del Convegno*, Bologna, Monograf, 1984.

[4] *Edizioni critiche e storia della matematica*. Atti del convegno CIRM, Trento, 1986.

critical notes pointing out any possible errors without, however, free interpretation, painstaking description of the document reproduced, a sober introduction of the contents combined with a historical collocation of the work. While the aim of a critical edition of a text is to give scholars access to unedited documents or works that are difficult to retrieve, it would be more appropriate to discuss interpretation and speculation in essay form or apart in separate volumes. In this way the edition may maintain its validity for longer periods of time, even generations, whereas a historiographical analysis, even of high level, being linked to the author's personal view, requires continuous revision.

The debate continued in 1987, when a conference on the Italian historiography of mathematics took place in Modena, in which historians tried to define the long tradition of mathematical historiography in Italy, and reflect on the various types of historiography, their limitations and pertinence.[5] The main aim was to avoid the risks of an "ideological" history, examples of which are abundant in the recent international and national literature, when, in order to achieve originality at all costs, a thesis is established a priori and a secondary element assumes the value of a principle. A further goal was to avoid the limitations of parading one's learning or flattening history through the exaltation of the discoveries of Italian scientists, often resolved in the research of 'precursors' (this, too, is abundant in the contemporary historiography of various countries).

As may be seen, reflection on present and past production has led historians of mathematics to apply to their historical research, besides the principles of rigour and accuracy inherent to mathematics, those principles that guide general historical research, namely, accurate citations, adherence to the historical period in the interpretation of a work, collocating it in a wider context which includes the author's biography, his/her formation, the historical and social period and so forth. All these elements should be borne in mind, even if there are historiographic works with different aims; so far production includes: critical editions of works or correspondences, interpretations in essay form on works or authors, analyses of entire theories seen over the centuries, general histories covering all the mathematical disciplines centred on a particular historical period, histories of mathematics aimed at dissemination to a wider public, scholastic texts in which mathematics is used to introduce mathematical concepts in the classroom.

The successive period saw the output and study of the history of mathematics develop so much that Italy became known as one of the main centres for this type of studies; this development was consolidated in the organisation of several international congresses throughout the country and, above all, in 1981 with the foundation of a new international journal devoted entirely to history of mathematics: the *Bollettino di Storia delle Scienze Matematiche*. The journal, published by the *Unione Matematica Italiana* until 2000, was then taken over by "Il Giardino di Archimede", a museum of mathematics, at the time when the *Unione Matematica Italiana* became a member of the museum. The new series is published by Fabrizio Serra, Pisa-Roma, with Enrico Giusti as its director and Luigi Pepe, vice-director. The *Bollettino* publishes research on the history of mathematical sciences in a wide sense, to include the history of mechanics, theoretical astronomy, natural philosophy and other disciplines traditionally placed within the exact sciences. The *Bollettino* publishes essays and articles in Italian, English, French, German or Latin. In line with a traditional development in Italian historiography of mathematics, particular attention is given to the publication of unedited texts of particular importance to the history of mathematics, for instance, correspondence, bibliographical essays and archive research, and in general to the tools for historical research. The journal publishes two issues a year and, so far, 29 volumes have come out.

Italy publishes international journals on the history of science like *Physis*, founded in 1959 and published by "Domus Galilaeana" in Pisa, open to all possible aspects of the history of science and technology. *Nuncius* is the journal of the *Istituto e Museo di Storia della Scienza* in Florence, founded in 1976 under the name *Annali di Storia della Scienza*, which since 2011 has changed publishing company and editorial profile adding the subtitle: *Journal of the Material and Visual History of Science* in order to highlight the fact that along with the investigation and enhancement of primary sources the journal is devoted to the "history of instrument making, scientific heritage and more generally to the relations between arts and science". Other journals, like the *Rivista di storia della scienza*, published by the Faculty of Sciences of "La Sapienza", University in Rome, devoted to the history of mathematics and natural sciences (1984-1996), have ceased to be.

In 2001 the *Società Italiana di Storia delle Matematiche* (SISM: Italian Society of the History of mathematics) was formed; it is a free association whose aim is to stimulate research and study in the field of history of mathematical sciences by favouring meetings for its members, publications and the promotion and dissemination of the history of mathematics. Among its main activities is the organisation of a congress for its members to be held every year in a different Italian city; the official journal of the Society is the *Bollettino di Storia delle Scienze Matematiche*. The first President was Clara Silvia Roero and now is Luigi Pepe. At present there are about one hundred and eighty members (http://www.dm.unito.it/sism/).

Main areas of research

I shall try to give a description of the various groups and current lines of thought which present a wide range of interests, and I apologise to the authors cited for the simplification of my outline given the restriction of space available to me.

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[5] *Pietro Ricardi e la storiografia cit.*

In Florence there is Enrico Giusti, whose main contributions have been to the analysis of the works of Galileo and the Galilean School: the doctrine of motion, the theory of proportions, Cavalieri's theory of indivisibles, as well as to the historical foundations of infinitesimal calculus. Enrico Giusti also deserves special mention for planning and setting up "Il Giardino di Archimede", a museum of mathematics aimed at arousing young people's interest in mathematics and the history of mathematics. He is assisted in the running of the museum by Raffaella Petti. The research group also includes Sandra Giuntini, whose main contribution is the edition of correspondences and documents of mathematicians of the seventeenth and eighteenth centuries, Elisabetta Ulivi, who, after her first studies on the Galilean School, devoted her attention to abacus mathematics and the abacus schools in Tuscany in the 14th-16th centuries, expanding the research carried out by Arrighi, Procissi and then by Laura Toti Rigatelli and Raffaella Franci. The research group collaborates with Pier Daniele Napolitani in Pisa, who investigated the transmission and reconstruction of science of the Greek Hellenistic Age by sixteenth century mathematicians like Luca Valerio, Federico Commandino and Francesco Maurolico; he is now supervising the national edition of Maurolico's works. Veronica Gavagna, after an initial research on mathematical correspondences of the early twentieth century, besides taking part in the scientific publications of the Maurolico Project, is now focussing on the mathematics of the Renaissance and post Galilean periods.

In Turin, Silvia Clara Roero and Livia Giacardi, two pupils of Tullio Viola, have devoted their research mainly to Giuseppe Peano and Corrado Segre, as well as to reconstructing the mathematical milieu of Turin through its main protagonists and the history of its university. Silvia Clara Roero, has, moreover, been working with Silvia Mazzone from Rome on the origins of infinitesimal calculus in Italy; Livia Giacardi on the history of non Euclidean geometry. Sandro Caparrini and Erika Luciano are also part of the Turin group.

In Rome, Giorgio Israel co-ordinates a group, of which Ana Millan Gasca and Luca Dell'Aglio are part, that has developed research on the history of mathematics applied to economics and social sciences, probabilities and statistics. Israel has, moreover, been working on the reconstruction of the mathematical milieu of the Fascist period.

Mathematics in Italy between the two world wars is also the subject of research by Angelo Guerraggio from the *Bocconi* University of Milan. He founded a journal of mathematics and history of mathematics for circulation among enthusiasts and teachers of mathematics called *Lettera Matematica Pristem* (in collaboration with Simonetta di Sieno). Massimo Galuzzi, working in the State University of Milan, has been researching the history of the solution of algebraic equations (Galois' theory), the School of Bourbaki and the historiography of mathematics.

In Milan, there are also Paola Gario and Umberto Bottazzini, the latter a historian of international renown who has focussed his research mainly on mathematics in nineteenth century Italy, and its relationship with the French and German schools. One of his pupils, Rossana Tazzioli, has devoted herself to the study of the history of non Euclidean geometries of that period and the first models of hyperbolic geometry. Paola Gario has studied the Italian school of geometry, with particular regard to Enriques and Castelnuovo, as well as the history of the teaching of mathematics and the formation of teachers of mathematics.

In Bergamo, Pier Luigi Pizzamiglio has devoted his studies to the bibliography and publication of classics, the history of scientific instruments, and the history of mathematics in relation to the teaching of mathematics.

The nineteenth century is also the focus of Paolo Freguglia's research at the University of Aquila, regarding the history of algebra and geometry in the 16th-17th centuries, the foundations of mathematics and logic in the 19th-20th centuries, the vector calculus and correlated theories (quaternions). Instead, the history of absolute differential calculus and mathematical physics of the late nineteenth and early twentieth centuries, and the foundations of the probability theory constitute the subjects of research by Luca Dell'Aglio at the University of Calabria.

At the University of Calabria the leading historian of mathematics is Luigi Maierù, whose investigations are above all linked to sixteenth century mathematics such as the criticism of Euclid's Fifth Postulate, the angle of contact and the theory of conics.

The Ferrara group is composed of Luigi Pepe, Maria Teresa Borgato and Alessandra Fiocca, whose main focus has been the mathematics of the eighteenth and early nineteenth centuries through the publication of numerous unedited works above all of J.L. Lagrange. Luigi Pepe has carried out research into mathematics in eighteenth century Italy and the Napoleonic period with particular regard to Gaspard Monge's missions in Italy, the dissemination of infinitesimal calculus in Italy, the history of the teaching of mathematics and the scientific institutions. Maria Teresa Borgato's interest has focussed on the Jesuit scientists of the seventeenth century, Lagrange's biography, eighteenth century actuarial mathematics, and mechanics and hydraulics of the Napoleonic period. Alessandra Fiocca has studied the science of waters in the Renaissance and seventeenth–eighteenth centuries, the history of the university and Libri's historical works.

Francesco Barbieri and Franca Cattelani Degani, in Modena, have contributed with archival research and the publication of correspondences to the biographies of mathematicians and historians of mathematics of that city (Pietro Riccardi, and Paolo Ruffini).

In Genoa, we have the group co-ordinated by Antonio Carlo Garibaldi, which includes Giuseppina Fenaroli and Michela Malpangotto: their research works have investigated the history of the probability theory, the history of mechanics and the publication of various correspondences between mathematicians.

In Naples (and then in Salerno) Franco Palladino has been studying the mathematics correspondences in the nineteenth century and the history of mathematical models and instruments. Romano Gatto has done research into the mechanics of Galileo and the Neapolitan tradition. Giovanni Ferraro, at the University of Molise, has investigated the theory of series, the integration theory and the theory of functions in the eighteenth century.

In Palermo, Aldo Brigaglia is the co-ordinator of a group, which includes Cinzia Cerroni, whose focus is the history of algebraic geometry as well as the *Circolo matematico di Palermo*, a famous journal that collected important contributions from European mathematicians. Pietro Nastasi also works in Palermo studying the history of the Institute for the Applications of Calculus as well as the history of institutions during the Fascist period.

Various Italian scholars have carried out their studies mostly in other countries: Niccolò Guicciardini in Great Britain, where he worked on Newton's theory of fluxions and the development of calculus in that nation; Marco Panza in France where he carried out research mainly on the eighteenth century; Rossana Tazzioli, presently in Lille, has studied the relationship between Vito Volterra and French mathematicians.

CROSS-NATIONAL AND COMPARATIVE HISTORY OF SCIENCE EDUCATION

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TWO WORLDS APART –COMPARING GREEK AND AMERICAN 19TH CENTURY SCIENCE EDUCATION

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Abstract

In this paper, I intend to describe an example where the study of science education in two specific national contexts can illuminate the formation and character of the respective scientific communities. Thus, I will try to compare the educational structures and institutions created in nineteenth century US and Greece and show how they interacted with scientific practice in each case. At first glance, these two social and political formations had almost nothing in common. USA was a huge but fragmented state, with abundant natural resources and clear ties with a European superpower. On the other hand, Greece was a devastated small country, emerging after four hundred years of Ottoman rule and struggling to modernize under heavy European influence and interference. Despite their many differences, I aim to show how both American and Greek science education drew aspirations from the same European pedagogical models and how this process contributed to the character of scientific practice in each country. The tentative proposal of this work is that the history of science education in these cases should be seen as the history of discipline construction and not just as the last and least function of a mature scientific community.

Educating future scientists in nineteenth-century USA

National histories of education are notoriously hard to produce and thus approached with reluctance. Even if we try to examine only how scientists were trained, a description of the American system appears as especially hard because the different strata of American education did not have parallel historical evolutions, firm boundaries or autonomous functions. In any case, after the American Revolution and by the time of Jefferson's death in 1826, American education would revolve around free press, popular schooling, patriotism and the mobilization of a host of voluntary associations, such as political parties and clerical institutions (Chemlin 1980, p. 105-112). By 1850, different educational theories started to be incorporated in secondary and higher US education. The travels of educationalists such as John Griscon and William Woodbridge had brought Pestallozian pedagogy in the American classroom, especially in the academies, which were at the peak at the time (DeBoer 1991, p. 18-24). Johan Heinrich Pestallozzi had developed a system of teaching that drew heavily on J. J. Rousseau's 'Emile', emphasizing among others sense impression, experimentation and reasoning over memorization. These new methods were much more favorable to the new ethics of science, as they were being articulated at the time by speakers such as Huxley and Spencer, who, it must be noted, had also drawn explicitly on Pestallozzi 's work. Finally, Pestallozzian education was in turn eclipsed by the advent of Herbartian pedagogy, which by the 1900 was the most influential theory of education in continental Europe and beyond.

Prospective scientists were to be found in all educational institutions. The English School and the urban primary school formed the most basic education (Rury 2005, p. 70). Attendance of a public school of that level usually meant textbook driven teaching, terms defined by the demands of the local agrarian economy, amateur and transitory teaching staff and a fuzzy curriculum that could change in accordance with the idiosyncrasies of the teacher (Joncich 1966, p. 671). Subsequently, in the academy and the American high school the textbooks, which students had to learn by heart, dominated the classroom (Herbst 1996, p. 38-51). However, secondary education also

displayed a high proportion of female students and the sciences were much more easily found in female schooling than in male. The motto 'Classics for gentlemen, science for ladies' describes the situation accurately (Tolley 2003, pp.34-53).

Thus, scientific subjects were considered as having an inferior status in the context of the gentlemanly education. However, it is also a fact that the college curriculum had early on incorporated the natural sciences. Antebellum colleges actually employed a proportionally large number of men of science and taught natural sciences under the rubric of Natural Philosophy, by means of lectures dispersed with laboratory demonstrations (Guralnick 1974, p. 352-354)[1]. College admission did not require an examination of scientific subjects, but rather a firm grounding in the classics. These led future scientist of the nineteenth century to have a solid knowledge of the subject, many of whom saw it as an invaluable asset for their later career (Jonicich 1966, p. 681). However, physics, astronomy and a smattering of chemistry and geology was taught at junior and senior years. Recitation of the canonical textbook was how students were both taught and examined (Cremin 1980, p. 404-407). After 1850, the introduction of mathematics in the textbooks, the use of laboratories for teaching and the introduction of sciences in the sophomore year gradually became the norm, even as the colleges of old were transformed into universities. While the textbook was still the undisputed king of the classroom, new ideas would slowly be implemented that would eventually lead to the Research University of the twentieth century (Guralnick 1975, p. 60-77). But even then, the classics were still considered superior. Both trends can be seen in the following recollection (Millican 1950, p.14):

> "At the close of my sophomore year in Oberlin, my Greek professor, John F. Peck, asked me to teach the course in elementary physics in the preparatory department during the next year. To my reply, that I did not know any physics at all, his answer was "Anyone who can do well in my Greek class can teach physics.
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> …I at once purchased an Avery Elements of Physics, and spent the greater part of my summer vacation of 1889 at home in Wichita, Kansas, whither Father had recently moved, trying to master the subject."

The American scientific community and education

We are now in the position to document the ways the evolution of the American scientific community followed the educational patterns of its time. First, we must start by noting that throughout the nineteenth century, the prime occupation of the American scientists was teaching. At least one third of American physicists, for example, were retained as teachers in primary or secondary education in one point or another of their career (Joncich 1966, p. 675). The teaching aspect of being a man of science was even more prominent in higher education. Antebellum colleges and later on academies offered courses on natural philosophy, chemistry and physics. These were usually taught by the same person, who stereotypically did not have specialized education in science[2] (Guralnick 1975). In the later part of the century, and despite the fact that science held a strong appeal to the 'common people' (Goldstein 2008), scientists active in research circa 1880 were less than 500 in a group of 3300 (Kevles et al. 1980). The rest mostly taught, an activity which also took the majority of the time of even prominent researchers. Teaching was not a peripheral activity for any scientists, and many did measure their scientific competence by their performance in the classroom (Joncich 1966, p. 683)

In the same vain, scientists for a long period doubled in both the role of the researcher and the popularizer, as the case of Benjamin Silliman shows. Silliman's popularizing lecture fees almost matched his Yale salary in the early decades of the century. His demonstrations requited visual aids and equipment, a fact that made him a firm supporter of the need of American instrument makers. This trend facilitated and in many ways led to the creation of a native instrument making industry (Warner 1988), which in turn enabled the subsequent emergence of the school and college laboratory. Object

[1] We must also note that chemistry was also taught in medical schools and high level physics was taught in West Point and engineering schools.

[2] Nor could he have. Graduate training, PhD degrees in sciences and specialized courses were to appear after the 1860 mostly in Germany and then mimicked across the world.

teaching, as advocated by Pestallozzian and later Herbartian pedagogy, was, after all, in full accordance with the demonstrative experiment.

A parallel process can be seen with science textbooks. If we bear in mind the predominance of the textbook as the main teaching device, it is important to note how American scientists progressed from textbook translators to textbook editors to textbook writers. Apparently, more than half of the men that taught mathematics and physics in antebellum colleges wrote or translated a textbook (Warner, p. 394). What the textbook contained determined significantly the curriculum itself. Thus, for example, when Silliman and later Denison Olmsted included experiments in their textbooks they in effect introduced them in the school curriculum. Again, textbook production was recognized by contemporaries as an important indicator of the strength of the scientific community (Millican 1950, p.14):

> "In the sophomore year I registered for a single twelve-weeks course in physics, using as a text a new American one by Anthony and Brackett, which we all thought both unintelligible and dull, greatly inferior to Ganot Physics, which had theretofore been used. For me the course was a complete loss.
>
> An indication of the state of advancement of American science at that time is found in the fact that so far as I know the only two physics texts then used at all extensively in American colleges were both translations from the French, namely, Ganot Physics and Dechanelles' Physics."

Finally, there is also a correlation to be made between the character of American science and the way it was taught. The period after Civil saw contemporary scientists recognizing a clear divide between 'pure' and 'practical' knowledge. 'Practical knowledge' and experiment was the field where most American scientists pursued internationally acclaimed research. Even in physics and chemistry, US scientists usually acquired renown for their observational and experimental skills (Kevles 1981). This could be in fact far from a contingent matter. On the one hand, scientists, as we saw, were mostly a product of higher education. American college curriculum was shaped by a strong emphasis on the classics, as well as a pedagogy which insisted on the formation of concepts as the main focus of teaching. Moreover, sciences such as physics were seen and taught as a finalized discipline, with only measurements remaining to be complete. It was thus a very appropriate subject to be presented through a textbook. As a result, scientists involved in 'pure' science were inclined to develop the mentality of a classicist, whose subject matter is not expected to evolve beyond some parameters. On the other hand, the emphasis placed on object teaching and observation would naturally lead to experimentation as a suitable research avenue. The result would be a scientific community predisposed towards observation but also conservative in its applications and reluctant to stray in speculative theorizing. And in fact, this is what we mostly see in the late nineteenth century. As an endnote, we could also point out how the well established rapid entry of women in science during the post-Civil War period could be partially correlated with the relatively high proportion of science subjects in their secondary education. (Tolley 2003, pp.34-53). Thus, the way scientists were educated comes across as especially concurrent with their research practices. Science education in the USA, far from being an auxiliary activity, comes across as a major formative and normative dimension of disciplinary emergence.

Sciences and the Greek educational system

In many ways, education in the modern Greek State followed a symmetrically different timeline than in the USA. From the start, it was centralized and under state control. The pedagogy instituted by the first Governor Ioannis Kapodistrias in 1828 was explicitly Lancasterian, through its French reformulation by Louis-Charles Sarazin (Papadaki 1992). The net result of his efforts was the multiplication of the number of primary schools in both mainland Greece and in the islands. The curriculum, under the influence of a Sarazin inspired manual, included mainly grammar, reading and writing and religion. The main focus was on the creation of a widespread public primary education system, with only a few higher vocational schools, such as the Military Academy and the Κεντρικό Σχολείο (Central School) of Aegina (Kyprianos 2004, pp 75-81). Thus, the textbook was the central teaching tool.

Following the assassination of Kapodistrias, Greece was placed under Bavarian Regency in 1833, which imposed the system of public education that was to remain in effect for the next ninety years. The three-tier structure which came into effect consisted of a primary school, the Hellenic School and the Gymnasium as secondary schools and finally the University and the Polytechnic School, as higher education institutions. However, educational matters, despite the fierce debates they provoked, were in reality not treated as central. Generally, the policy pursued was tacit rather than explicit, mostly consisted of the uncritical adoption of European laws and an insistence on a strong public education, following the examples of Prussia and France. (Mylonas 2000). Primary education was not significantly altered and thus sciences never got a stronger hold on the curriculum until the end of the nineteenth century. It was in the newly founded secondary education that innovations appeared. In the Royal Degree of 1837[3] the curriculum described for the Hellenic School and the Gymnasium allocates two or, in some classes, three hours per week to Natural History, Chemistry and Physics. However, no further explanations or guidelines are given, nor is a specific textbook mentioned. As a result, these lessons faded into obscurity, until in 1855, another Royal Degree changed the curriculum, allocating two hours per week to Natural History in the first classes of Hellenic School and another two hours weekly to Experimental Physics in the Gymnasium. This was an effort to create a more plausible educational scenario, as for example, many successful Greeks of the Diaspora left large grants for the establishment of laboratories and institutions, but the lack of approved textbooks and qualified teachers did not enable their full use. It would take another twenty years for a more analytic science curriculum to appear and specific textbooks to be approved. In any case, natural sciences in the Greek curriculum never occupied more than two or three hours per week.

In 1862, King Otto I was evicted from his throne and one year later the Danish Prince George succeeded him. He was to remain in his throne until his assassination in 1913. During his period, Greek intellectuals more and more turned to German institutions for inspiration. Primary education once again became the focus of all educational reforms, where the monitoring school of Sarazin was replaced by the German pedagogy of Johann Friedrich Herbart. However, the adoration of all things Western of the past was no more. Actions taken by both France and Britain during the Crimean War had made the Greek population very suspicious of everything European. It was around this period, namely in the decades from 1865 to 1895, that specialized natural scientists start graduating from the Faculty of Philosophy of the University of Athens. It would however require another twenty years for them to be recognized as the only qualified to teach natural sciences in secondary education.

It was thus only in higher education, in the University of Athens and in the Military School, that sciences had a specified, albeit minor, role. The Physical and Mathematical Faculty in the University of Athens operated under the aegis of the Philosophy School. For the whole of the nineteenth century, there was only one Chair of Physics, while most of the subjects taught were heavily focused on Natural History (Stefanides 1948, Vol A, pp. 4-22). In any case, there were never more than ten professors of natural sciences in the teaching staff of the university.

Turning now to the Greek scientific community, it is immediately noticeable that most of the Professors of the University of Athens had studied in prestigious European universities, such as those of Paris, Munich and Berlin. They also remained in contact with their peers in Europe and took part in international conferences. Their main occupation, however, was teaching. Not only do they appear quite vocal when their teaching fees are threatened, but they also frequently write on the need to attract more students in their respective disciplines. In return, the public sees them as esteemed teachers and their role in establishing 'Greek science' is always related to their teaching achievements[4]. Furthermore, despite some initial ambivalence towards secondary education during the Bavarian rule, the second generation of professors active from 1860 onwards are quite firm in their insistence that science needs to be part of the curriculum and that only trained scientists should teach science courses. Once again, legitimization and autonomy for the scientist is reflected in their role in public education.

This process is nothing if not explicit. Scientists early on take on the mantle of the textbook writer, first for higher education and later on, for primary and secondary education also (Tampakis &Skordoulis 2010). The textbook becomes an invaluable symbol of authority on the subject and a valuable way to augment one's income. The importance placed by the scientists themselves on textbook writing can be seen by their very public debates when one of their textbooks did not make the official list of approved school books[5]. Moreover, it was not uncommon for scientists to get attacked by rivals not on the basis of their research, but on the flaws of the textbooks they had written (Kritikos 1995).

Finally, in the case of Greece, the reciprocal relation between the creation of a public science education and the emergence of a scientific community becomes forcefully apparent when the demographics of science are considered. It is no contingent fact that, from 1838 to 1865, no university graduate had specialized in natural sciences. On the one hand, the agrarian and mercantile nature of Greek economy did not readily create a social demand for science. Physics, Chemistry and Natural Science remained for the most part of the century a rather esoteric corpus of knowledge, warily accepted as necessary by the cultured elite for nebulous reasons having to do with modernity and progress. On the other, secondary education offered science courses only in name, while the centralized and hierarchical structure of Greek education did not allow for different avenues to higher studies. In a very real way, the lack of science education prohibited the emergence of Greek scientific practice. And indeed, the scientists of the time were either coming from abroad or had a direct relation with a scientist of the older generation. However, with the institution of an actual secondary science education in 1865 and with the

[3] All Royal Degrees mentioned in our discussion, as well as a comprehensive statistical analysis, are to be found in the first volume of Antoniou 1989.

[4] The reference to the teaching ability of a Professor as a measure of his worth is a recurring theme in the honorary speeches given on the occasion of his retirement. We find in the commemorative volume in honor of Prof. Mitsopoulos in 1901, of Prof. Christomanos in 1906, of Prof. Damvergis in 1917 and in the article written to honor Prof. Stroumpos by his successor T. Argiropoulos in 1890.

[5] As was the case, for example, with Prof. K. Mitsopoulos' textbook on Geography in 1894 and with Sp. Papanikolaou 's textbook on Physics in 1906. In both cases, furious public debates with leaflets ensued between the two contestants.

appearance of state approved textbooks, the University produced more than 30 science specialists within the next years. Science education in Greece not only went hand in hand but actually facilitated the creation of the Greek scientific community.

Conclusions

In this paper, I tried to show how scientific practice and education interacted in two different national contexts, that of nineteenth century USA and Greece. My ultimate goal is to suggest an alternative to the standard view that science education comes after the formation of the scientific communities or to put it bluntly, science education is what scientists do when they are away from their laboratory or conferences. In two specific cases, the picture that emerges is rather one of parallels, dichotomies and mutual construction. USA and Greece had a radically different social, economical and political timeline, which affected decisively the constitution of their educational and scientific space. But even while nineteenth century Greece and USA appear as two worlds apart, some striking similarities and parallels can also be noticed that suggest a larger picture. In both countries, men of science mostly taught, in higher, secondary, even primary education. The way they saw themselves and they way the public perceived them was tied to their educational activity, be it formal or popularizing. Thus, despite the historiographical blind spot that makes histories focus on pioneering researchers, most scientists of the era were engaged more with the classroom much more than with the laboratory or the field.

Furthermore, scientists were significantly more inclined to write textbooks than research papers. The development of native textbooks followed the development of the scientific community much more accurately than research publications. The strength of the scientific community was implicitly if not explicitly judged by the merits of its textbooks and scientists themselves were quite aware of the serious role that textbook writing had to play in the formation of their respective disciplines. Furthermore, especially in the US, something similar happened with the scientific instruments: the need for exhibition equipment made scientists into supporters of local craftsmen, which in turned helped scientific instruments to appear initially in the classroom and then in the research laboratories. In Greece, where instruments were not manufactured locally and scientists disassociated their role inside Greek society with their role as active researchers, textbook production acquired a highly visible role, with scientists being judged publicly by the merits of the school books they wrote.

Finally, the character of the ‘national’ science education appears to have had a formative role in the character of ‘national’ scientific practice. American scientists come across as very much influenced by their classical training, by the role that prevalent pedagogy gave to observation and object teaching and by the dominant role of the textbook within their classes. In Greece, the formation of the scientific community actually followed by almost twenty years the establishment of science in the curriculum. Even if it is a case of correlation rather than causation, the fact remains that it was only after public education expanded significantly enough for science to reach students that scientists started to appear in significant numbers. And when they did, the one common theme they had was their insistence on exclusive teaching rights.

References

ANTONIOU D., (1989), *Τα προγράμματα της Μέσης Εκπαίδευσης (1833-1929)*, Three volumes, CNR/ HNRF

CREMIN, L., (1980), *American Education: The National Experience (1783-1876)*, Harper &Row Publishers

CREMIN, L., (1988): *American Education: The Metropolitan Experience (1876-1980)*, Harper &Row Publishers

DEBOER, G., (1991*), A history of ideas in Science Education: Implications for practice*, Teachers College Press

JONCICH, G., (1966), Scientists and the Schools of the Nineteenth Century: The Case of American Physicists, *American Quarterly* 18(46), pp. 667-685

GOLDSTEIN, D., (2008), Outposts of Science- The Knowledge Trade and the expansion of Scientific Community in Post–Civil War, *Isis* 99 (3) , pp. 519–546

GURALNICK, S., (1974), Sources of Misconception on the Role of Science in the Nineteenth-Century American College, *Isis* 65(3), pp. 352-366

GURALNICK, S., (1975), *Science and the Ante-Bellum American College*, Memoirs of the American Philosophical Society 109

HERBST, J., (1996) *The once and future school: Three hundred and fifty years of American Secondary Education*, Routledge

KAISER, D., ed., (2005). *Pedagogy and the practice of science - Historical and contemporary perspectives*, MIT Press

KEVLES, D., STURCHIO, J., CARROLL, P., (1980), *The sciences in America, circa 1880*, Humanities Working paper, California Institute of Technology

KEVLES, D., (1981), *The professions of science in America: Their ambivalent history*, Notre Dame Lecture Series on the Professions in American History

KRITIKOS, Th., (1995), *Η πρόσληψη της επιστημονικής σκέψης στην Ελλάδα- Η φυσική μέσα από πρόσωπα, πράγματα και ιδέες (1900-1930)*, Papazisi

KYPRIANOS, P., (2004), *Συγκριτική Ιστορία της Εκπαίδευσης*, Vivliorama

MILLIKAN, R., (1950). *The Autobiography of Robert A. Millikan*, Prentice Hall,

MYLONAS, Th. (2000), The strong tendency to transfer legislation from France to Greece in the first half of the nineteenth century and Maourer' s organic law of Greek primary education (1834), in Terzis, N. P. (ed), *Education in the Balkans. From the Enlightenment to the Founding of the Nation-States*, Kyriakides Brothers

PAPADAKI, L., (1992), *Η αλληλοδιδακτική μέθοδος διδασκαλίας στην Ελλάδα του 19ου αιώνα*, Dodoni

RURY, J., (ed), (2005), *Urban education in the US: A historical reader*, Palgrave Macmillan

STEFANIDIS, M., (1948), *Ιστορία της Φυσικομαθηματικής σχολής,* Two volumes*,* National Typography

TAMPAKIS, K., (2010), The many faces of textbooks: Science, education and science education in the early Greek state (1838 – 1931), *Archives Internationales D'Histoire Des Sciences* 60(163), pp.93-116

TOLLEY, K., (2003), *The Science Education of American Girls: A Historical Perspective*, Routledge Falmer

WARNER, D., (1988), Commodities for the classroom: Apparatus for science and education in Antebellum America, *Annals of Science* 45(4), pp. 387-397

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 53.

KNOWLEDGE DIFFUSION AND THE LEARNING OF PRACTICAL KNOWLEDGE: CASE STUDIES OF EARLY MODERN JAPAN AND EUROPE

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Abstract

The paper concerns comparison of the roles played by texts and objects in higher education in Western Europe, China and Japan in early modern period. As is well known in Europe the interest for the direct study of nature and properties of material objects grew during the sixteenth and seventeenth centuries. Although the use of these resources in practical teaching was limited, this type of sensorial teaching method increasingly gained its importance in educational theories. Texts and objects also shed a light upon the understanding of teaching traditions in contemporary Tokugawa Japan. While reading and interpretation of Confucian texts dominated the core part in the traditional curriculum in the Japanese elite schools, Western learning particularly medicine and military techniques were increasingly incorporated into practical teaching in higher education from the late eighteenth century. The paper also examines the contrasts and interactions between this new type of teaching with traditional Chinese teaching, that is, reading and interpretation of classical Confucian texts in the sixteenth and seventeenth centuries.

Introduction

In his book *Matters of Exchange*, Harold Cook writes: 'The powers and pleasures of accurate descriptive knowledge were not only praised by the chief advocates of the new philosophy' but were 'also noted as the chief concern of European science by others such as Japanese scholars who interested themselves in such matters'.[1] Cook illustrates this point with reference to the intellectual exchanges between a Dutch East India Company surgeon and the Japanese scholars in the second half of the eighteenth century. During the Tokugawa period (1603-1868), the Tokugawa government relatively effectively controlled its nationals' direct contacts with Europeans between the 1630s and 1858 under the semi-seclusion

[1] Cook (2007), p. 410.

policy. By granting the official trade monopoly to the Dutch, the government kept away the Catholic Christian missionary influence from Japan. Officially no Japanese people were allowed to travel abroad and the imports of foreign publications were under strict censorship of authority.

However, through Nagasaki, where the Dutch East India Company's factory was located, European culture especially its scientific and technological knowledge diffused in the Tokugawa period and it later played an important role in Japan's rapid assimilation of Western modern technologies in the subsequent industrialisation in the nineteenth century. The Dutch contact gave Japan the advantage of acquiring not only the Dutch origin culture but also more widely the latest scientific and technological knowledge. The Dutch Republic as a political power played a leading role in European cultural life and - with extensive activities oversee - stood at the centre of a worldwide network for commerce and the exchange of information. For example, Swedish Carl Thumberg, who arrived in Nagasaki in 1775 as the Dutch Company's surgeon, studied botany under Linnaeus at Upsala University before joining the Dutch East India Company. His time in Japan was spent collecting data of Japan's natural history and receiving frequent visits from Japanese naturalists and scholars who came to converse about European books and particularly European medical knowledge.[2] The paper will argue that the development of indigenous practical and utilitarian science in Japan played an important role in the intellectual interaction with the Europeans. Between the 17th and the 18th century, both in Japan and Europe we see developments in the attitudes towards nature, and to certain extent educational tradition that are highly comparable. The paper will analyse these phenomena focusing on their intellectual and social origins in both regions.

Useful knowledge: Japan, the Dutch and the Chinese tradition

An orientation towards empirical knowledge of natural world was evident in the knowledge circulation in the Tokugawa period. The *Genroku* era (1688-1703) witnessed significant increases both in agricultural and industrial production and the arable margin was extended significantly by irrigation. Productivity increased through more intensive application of fertiliser and human labour. The rise of commercial economy promoted the institutionalisation of artisanal skills. Agronomists often under the patronage of local fiefdom lords published agricultural treatises, *l*ocal almanacs and technical manuals.[3] The late seventeenth century agronomist Miyazaki Yasusada (宮崎安貞) spent thirty years travelling to collect and record agricultural techniques across western Japan and used this knowledge in experiments on his own land. His enterprise culminated in the agronomical treatise called *Nogyo Zensho* (農業全書) in 1697. Unlike the Chinese model agronomical treatise *Nose Zensho* (農政全書) (1639), written by the Ming court scholar徐光啓 and based on bibliographical records, Miyazaki's treatise drew upon his observations and experiments which included techniques with Chinese origins used by Japanese farmers. Miyazaki's treatise was printed and widely circulated across the country and promoted the accumulation of knowledge on agricultural production[4].

It is our thesis that Western sciences were so appealing to the Japanese because the two scholarly communities, different as they were, did share common tendencies and interests, namely in a new development of science that would be more practical oriented and would more closely analyse and take as a starting point for enquiry the natural and material world.

It is well known that in Europe from the sixteenth century a more practically oriented kind of scholarship, that will culminate in the so-called scientific revolution, started to take foot. This change of focus –compared with the medieval tradition– happened gradually and involved many different spheres. A part from the well known developments in philosophical and "scientific" circles, part of the discourse in fact took place amongst educational reformers. John Amos Comenius (1592 –1670), one of the most famous European pedagogists of the time, urged –for example- a change in the subjects being taught. He reflected upon the necessity to confront students of all grades with the natural world, material objects and practical problems. He underlined this when describing the ideal curriculum of the Latin school, the school that would prepare for University. "The pupils should learn four languages and acquire an encyclopaedic knowledge of the … seven liberal arts, a knowledge of which is demanded from a doctor of philosophy. But our pupils must aim higher than this, and in addition must be Physicists, who know the composition of the earth, the force of the elements, the different species of animals, the powers of plants and of minerals, and the structure of the human body, and who, beside knowing these things, can apply them to the various uses of life. Under this head is thus comprised a part of medicine, of agriculture, and of other

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[2] Screech (2005), p.79. Thumberg on his return to Upsala succeeded Linnaeus as professor in botany.

[3] Sugimoto (1967), pp. 290-95.

[4] Kandatsu (2005), Chapter 6.

mechanical arts"[5]. The great importance given to the new sciences and the study of nature can be seen also in the fact that Comenius dedicated a whole chapter to "The method of the sciences, specifically", in which he stressed the importance of direct observation and of experiencing with the senses the materiality of things.[6]

Comenius was a protestant Czech teacher, and later became a bishop. As a consequence of the religious wars he was forced to live and work in many different countries. In the long run he was very influential for educational theory and practice in Europe and beyond although his principles were scarcely put into practice during his life time. His work however shows that the inclination towards the study of the material world through observation (put forward in the same period by figures such as Bacon and Descartes) was a feature that did not attain only the most advanced natural scientist and philosophers but was felt more broadly and started to percolate, even if very slowly, towards a wider audience.

Books: from people and objects to texts

Japan's encounter with Western knowledge was established both through books and through direct contact with practitioners coming to Japan as VOC officials. While the VOC had earlier employed mere 'barber surgeons' (with ambition to achieve a quick fortune from smuggling in East Asian trade), later the company recognised the value of both practical and academic training offered by Dutch Universities and selected young physicians with university qualifications. These men turned out to be highly appreciated by their Japanese interlocutors. The VOC even received an official request to bring to Japan someone who would have a more grounded understanding of medical matters and was not a mere practitioner[7].

Precisely speaking, Chinese translation of western texts was the initial and main source of western scientific knowledge. Despite the strict censorship of the Bakufu's Nagasaki Magistrate, some foreign books kept coming in. Order was made almost annually throughout the seventeenth century to Chinese merchants and the VOC to supply with books on practical subjects such as medicine, history and world geography. For example the Neo Confucian scholar Arai Hakuseki 新井白石 (1657-1725) studied world geography using many Chinese translated Jesuits texts.

Before the 1730s, most of the imported Dutch language books seemed to have often gone into the shogun's court library without being used. Occasional references by curious shoguns and their court members were recorded: particularly favoured were herbal and zoological texts such as Dodonaeus' *Cuijdeboeck* (1554) (presented to the Shogun Iesada in 1618) and Jonston's zoological treatise (Jonston 1660 presented in 1663). The shogun and domainal lords sometimes requested books on medicine and botany "with many illustrations" and "preferably in Portuguese."[8] The supply of books came from Amsterdam, the most active book trading centre in Europe, via the Company's Factory in Batavia.

The imports of foreign books increased from the relaxation of the rule in 1726 when the eighth shogun Tokugawa Yoshimune ordered over three hundred Chinese and Dutch texts on western science and practical subjects. Although Chinese texts remained the main source of Western knowledge, the mastering of Dutch language was encouraged. Yoshimune appointed the herbal scientist Aoki Konnyo 青木昆陽 (1698 -1764), who was trained in the Jitsugaku tradition at the Kodigo academy in Kyoto, and ordered him to learn the Dutch language to study its texts. By mid eighteenth century, the usefulness of European science became recognised not only by its advocating scholars but also by Tokugawa politicians. The distinguished government councillor Matsudaira Sadanobu 松平定信(1759-1829), wrote: "In the barbarian countries they know much about principle: especially in astronomy and geography and armament, or internal and external cures there is much one can profit from."[9]

Ordering nature for a purpose: Chinese, Japanese and European taxonomies compared

The following case study relates not simply to an increased production of compendia of useful and practical knowledge in the period, it represents the transition from Chinese originated practical manuals to Japanese vernacular publications. In the medieval period, when encyclopaedias (its East Asian term *Ruisho* 類書) came from China, Japanese versions were published following the Chinese model. These volumes were primarily hand-copied for the personal libraries of court aristocrats and used mainly for reference in reading Chinese poetry and Confucian texts. Ten collections of such reference works appeared between the late tenth and the sixteenth centuries but seldom contained information relevant for material production. In contrast, the early and mid Tokugawa period (1610-1739) saw the publication of some fifteen printed

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[5] Comenius ([1631 1967], pp. 374-75.

[6] Ibid, pp. 183-84.

[7] Cook (2007), p.275.

[8] Boot (2009), p.51.

[9] Ibid.

collections of encyclopaedias and compendia of herbal and geographical data.[10] Advanced printing techniques, the expansion of a reading public through basic schooling were behind this movement, but of foremost importance was the desire to make knowledge accessible.[11]

Of particular interest is *Yamato Honzo* (大和本草) (1708) by the Confucian scholar and herbalist Kaibara Ekken (貝原益軒) (1630-1714) which was designed for the utilitarian ordering and understanding of botany and the properties of herbal medicines. It was in fact a Japanese version of a Ming model *Honzo Komoku* (本草綱目) (1590) but Kaibara clearly intended to make his version more relevant to production. For example, he paid much attention to agricultural products as well as herbs and flora of China and Japan. Learning from the Ming text, Kaibara modified the Ming classification into a more suitable one for Japanese use. He made a more precise classification of the items according to their locations – mountains, rivers, seas and land.[12] Kaibara served as Confucian official scholar to a local fiefdom lord in Kyushu. He frequently travelled on official duty to other fiefs. He held a position among the cultural and intellectual circle of Kyoto scholars, with useful connections to Kyoto publishers of collections of "Ekken books" drawn from Kaibara's practical knowledge and observation.[13] Furthermore Kaibara wrote most of his books in simplified Japanese letters – as opposed to philological Chinese contained in many texts circulating in Tokugawa Japan. He ordered their contents alphabetically. His clear intention was to diffuse useful knowledge, in an accessible form, to Japan's medical, technical and manufacturing professions.

The publication of these 'practical' compendia can be connected to an array of governmental initiatives put into place as a reaction to large trade deficits Tokugawa Japan was suffering in relation to China and Korea. Growing consumption amongst urban residents for imported goods such as silk and medicinal herbs had led to the drain of silver from Japan. As a result the Bakufu shifted its economic policy in a mercantilist direction. The need to increase the domestic production was identified and policies of import substitution were initiated. The government's establishment of the Koishikawa pharmaceutical research institute in Edo needs to be seen in this context: scholars were sent across the country to collect unknown plants and send them back to the institute to be studied.[14]

In this initiative different systems of knowledge meet and intertwine. The herbalists involved in this project were, in fact, well acquainted with traditional Chinese herbal knowledge and botany. The plants and minerals they collected from across the country were tested and identified against Yamato Hozou, its Chinese original Honzo Komoku and other Chinese herbal texts. At the same time the shogun Yoshimune (1716-1745), himself a keen herbalist, eager to acquire the Western knowledge, ordered the herbalists Noro Genjo and Aoki Konyo to translate Dodonaeus' *Cruijdeboeck* which, as we have seen previously, had been presented to an earlier shogun in 1618 by the VOC yet stored in the court library unused. The accurate reproduction of plants by Dodonaeus through numerous illustrations had caught Yoshimue's attention.

A similar development of scholarly enquiry that produced encyclopaedic texts in response to economic needs can be observed at the same time also in Europe. Dodoneus' work, for example, had probably been chosen as a present for the Shogun because of the great success it had enjoyed all over Europe (it was translated into French in 1557, into English in 1578 and into Latin in 1583). This success originated not only in its intellectual and scientific interest but also in its practical usefulness. Similarly to what we have been describing for Japan, also in Europe there was an underlying economic preoccupation that fostered botanical works. In the case of Dodoneus we know that before finishing his famous *Cruijdeboeck* he published part of it in *De frugum historia*.[15] This was to comply with pressures made by many international friends interested in improving agricultural output through the selection of better varieties of grains alternated with the cultivation of soil enriching plants, apt to animal consumption.[16] A process that will lead to the agricultural revolution of the 18th century. Both in Japan and Europe we therefore see a rise of applied knowledge, particularly in the botanic field, driven by intellectual interests but also by economic preoccupations.

Botany is also an important discipline for the development of systems for ordering knowledge and differences between Japan and Europe need to be underlined. Between the 16th and the 18th centuries in Europe there is a large debate about plant taxonomy that will culminate in the establishment of the Linnaean system. From an utilitarian or an alphabetical

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[10] Sugimoto (1967), pp.341-45.

[11] On literacy rate and reading public in Tokugawa Japan, see R. Rubinger, *Popular literacy in Early modern Japan* (Hawaii, 2007) and *Shikiji to dokusyo*, ed. S. Matsuoka, T. Yakuwa (Kyoto, 2010). For English sources on schooling in Tokugawa Japan, see R. Dore, *Education in Tokugawa Japan* (Michigan, 1965), 252-70. Tokugawa schooling for commoners has been studied extensively by historians but the teaching of practical knowledge in these schools remains to be explored.

[12] In the preface to *Yamato Honzo*, Kaibara stated a number of criticisms on the classification of *Honzo Komoku*. One of his doubts was that the Ming text did not distinguish seaweeds from acuatic plants.

[13] Yokota (2007), chapter 11.

[14] Kasaya (2001), pp.180-190.

[15] Dodonaeus (1552).

[16] Ambrosoli (2001), p.62.

way of ordering plants slowly botanist moved towards a 'natural' division according to species and 'families', a division that was trying to reflect nature's order.[17]

This new generation of botanical studies developed as a consequence of the fact that scholars, who until the 15th c. had been using reference text from antiquity for their studies, noticed discrepancies between the plants described in those books and the ones they could see around them. This stimulated a new trend towards direct observation of the natural world that harmonised well with the general developments of natural philosophy from the late 16th and early 17th centuries. Alongside direct observation a further important point became the retrieval of local knowledge through consultation with peasants, whose nomenclature became used also by the intellectuals. The choice to use the vernacular had different backgrounds. It was related to the fact -just mentioned- that the knowledge displayed in the books was local knowledge compiled *in primis* for a local audience.[18] Secondly it often reflected the intent by Protestant scholars to push the use of the vernacular as opposed to Latin.

This trend away from alphabetical ordering in Europe is peculiar to botany. General encyclopaedias until the Middle Ages were organised thematically and the alphabetic ordering starts to take over towards the 17th century. This development is linked to the use of the printing press and the deriving new understanding of letters as 'bricks' building a word.[19] Moreover while early encyclopaedias tried in their structure to reflect the world and the way it had been conceived, by the 17th century it was felt that organising knowledge in an 'organic' system was beyond reach. The alphabetical ordering seemed, than, a feasible alternative. As observed for Japan, also in Early modern Europe technical reference books proliferate. From the 17th century so called Hard-word lexicons (which only explain unusual terms, often addressing technical and scientific matters) appeared, than dictionaries of specific disciplines became more common and finally the dictionaries of arts and sciences were developed.[20]

Conclusion

This paper has analysed how different cultural tradition meet in Japan in the 17th and 18th centuries and exchange ideas and practices about observing and classifying the world. Stress was placed on the understanding and describing of plants and the organization of knowledge about natural resources. More practical oriented, utilitarian taxonomical systems merged with cosmological traditions with a growing interest for details which took different forms in different cultures. Notions about what is useful became entangled with ideas about how to organize knowledge and to make it accessible to different users: from government officials to common citizens. In this context intellectual trends are strongly connected with the makeup of educational systems that, as we have discussed, developed between the 17th and 18th centuries, in different forms, both in Europe and in Japan.

Interesting similarities between the Japanese and the European case can be found: as scholars in Japan start 'adapting' traditional Chinese knowledge to the local situation, Europeans are adapting and appropriating knowledge from antiquity. In this processes of acquisition in both cases we observe a reverence towards the classical canon and, at the same time, an effort towards making that knowledge more 'fitting' and useful to the own place and time. Related to the attempt to produce more useful and widely usable knowledge we observe in both cases the rise of a more broadly understandable language (the vernacular in Europe and simplified characters in Japan). It seems that the success of these enterprises aiming at collecting and classifying knowledge leys not only in their intellectual value, but also in the fact that they produced instruments that can be used to solve economic preoccupations.

The similarities and differences in institutional terms remain to be analysed. An in depth analysis of the changing role of different actors such as the Shogun, the domain Lords and private academies in Japan as well as individuals, private enterprises, scholarly societies and the state in Europe will allow a better understanding of the dynamics underpinning these phenomena and their different outputs.

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17 This process was started by the German school in the 16th century (Visser 2001).

18 Many small format compendia of local flora were published in the second half of the 16th century (Ambrosoli 2001: 64).

19 McArthur (1986), p.77.

20 Yeo (2001), p.20. Thomas Blount introduced the term 'hard-word dictionary' in his *Glossographia* (1656).

References

AMBROSOLI, M. (2001). 'Botanical Practice and agricultural demands in early modern Europe: the contribution of Rembertus Dodonaeus' in: *Dodonaeus in Japan*, pp. 60-75.

BLOUNT, T. (1656). *Glossographia; or, a dictionary interpreting the hard words of whatsoever language, now used in our refined English tongue*, London.

BOOT, W. (2009). 'The Transfer of learning: the import of Chinese and Dutch books in Tokugawa Japan', in: *Canton and Nagasaki compared 1730-1830. Dutch, Chinese, Japanese Relations, Transactions,* edited by Evert Groenendijk, Cinthya Viallé, Leonard Blussé, Intercontinenta 26, Institute for the History of European Expansion, Leiden.

Johann Amos COMENIUS ([1631] 1967). *The Great Didactic of John Amos Comenius*, translated into English and edited by M. W. Keating, New York.

COOK, H. (2007). *Matters of Exchange,* New Haven.

DODONAEUS, R. (1552). *De frugum historia*, Antwerp.

DODONAEUS, R. (1554). *Cruijdeboeck*, Antwerp.

DORE, R.P. (1965). *Education in Tokugawa Japan*.

FURETIÈRE, A. (1960). *Dictionnaire universel*, Rotterdam.

HARRIS, J. (1704). *Lexicon Technicum: Or, An Universal English Dictionary of Arts and Sciences: Explaining not only the Terms of Art, but the Arts Themselves*, London.

KANDATSU, H. (2005). *Kinse ichi nosyo no seiritsu*, Tokyo.

KASAYA, K. (2001). 'The Tokugawa Bakufu's policies for the national production of medicines and Dodonaeus' Cruijdeboeck' in: *Dodonaeus in Japan*.

JOHNSTON, J. (1660). *Naeukeurige Beschryving van de Natuur dre Vier-voetige Dieren, Vissen en Bloedlooze Water-Dieren, Vogelen, Kronkel-Dieren, Slangen en Draken*, Amsterdam: J. J. Schipper.

MCARTHUR, T. (1986). *Worlds of Reference. Lexicography, learning and language from the clay tablet to the computer*, CUP, Cambridge et alia.

NAJITA, T. (1987). *Visions of virtue in Tokugawa Japan: the Kaitokudo merchant academy of Osaka*, Chicago.

RUBINGER, R. (1982). *Private academies of Tokugawa Japan*, Princeton.

SCREECH, T. (2005) *Japan extolled and decried: Carl Peter Thunberg and the Shogun's realm 1775-1796*, London.

SUGIMOTO, I. (1967). Kinsei jitsugakushi no kenkyu: Edo jidai chuki niokeru gijutsu gaku no seisei, Tokyo.

SUGIMOTO M. and SWAINE, D. L. (1978). *Science and culture in traditional Japan A.D. 600-1854*, Cambridge: Mass.

TUCKER, J. A. (1998). *Ito Jinsa's Gomo Jigi and the philosophical definition of early modern Japan*, Brill, Leiden.

VANDE WALLE, W. F. and KASAYA, K. (eds.) (2001). *Dodonaeus in Japan. Translation and the Scientific Mind in the Tokugawa Period*, Leuven University Press, Leuven.

VISSER, R. (2001). "Dodonaeus and the herbal tradition", in: *Dodonaeus in Japan*, pp. 45-56.

YEO, R. (2001). *Encyclopaedic Vision. Scientific Dictionaries and Enlightenment Cultures*, CUP, Cambridge.

YOKOTA, F. (2007). 'Ekken bon no dokusha' in: T. Yokota ed. *Kaibara Ekken: Taihei waraku no bunmei gaku*, Tokyo.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 59.

KNOWLEDGE TRANSFER AND THE JESUITS: COMPARATIVE CASE STUDIES OF EARLY MODERN JAPAN, CHINA, AND INDIA

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Abstract

This paper concerns the comparative historical research of the cultural regimes of knowledge transfer in the early modern period. It compares the cultural regimes in China, India and Japan, which patronised or restrained the diffusion of the western scientific orientation introduced by the Jesuits missionaries in the sixteenth and seventeenth centuries. The periodisation accords with the late Ming (1368-1644) and the early Qing (1644-1911) dynasties in China, the early Tokugawa period (1603-1868) in Japan, and the Mughal Empire and southern princely states in India.

Introduction

The comparative approach of this paper accords with the recent development in theories of knowledge economy and innovation in historical analysis of the European path. In the past few centuries the Western Europe seemed to have benefitted from its distinctive cultural regime favoured to foster technological advancement.[1] Interesting questions then should be asked about the Eastern regimes. In the early modern China, India and Japan, when encountering the European scientific orientations, how did their regimes interact with it? Are there to be observed differences and similarities amongst them? If so, what light can they shed upon our understanding of the wider question? In this preliminary research, the paper will focus on the case studies of the knowledge transfer patterns of the Jesuits science and its outcomes.

India

In the case of South Asia, it has been argued that the making of knowledge was shaped by 'material, economic, and symbolic transactions between indigenes and Europeans'.[2] The indigenous, it seems, are often just limited to Brahmin

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[1] Jacob (1997); Mokyr(2002).

[2] Raj (2007).

scribes. Although there was certainly a role played by merchants, peasants, consumers and artisans in the interaction, this is something that still needs to be researched.[3] On the European side of the interaction, the role of all such agents, in the transfer of knowledge, are well documented not in the least, the role of the Jesuits; to critically examine which, is the main aim of this section of the paper.

The role and continued vibrant contribution of the Jesuits on education in India, specially in higher forms of education, in contemporary India is indisputable.[4] The lack of sources to study indigenous forms of higher education in the Indian subcontinent makes it challenging to measure the character of their contribution historically. However, there are rich sources of the history of the Jesuits in India in the form of archives of the Society of Jesus, among others, letters and reports they wrote covering their activities and challenges in India.

The strength of the Jesuits, albeit limited success in converting—which was their main aim—in the early modern period, lies in the fact that in order to communicate with the indigenous people about Christianity, they were forced to make immense efforts to learn local languages, wherever they aspired to spread the Gospel. In doing so, they created several notable works in various languages of South Asia. Notable compilations worthy of mention are Miguel d' Almeida's the first known Konkani dictionary, Constantius Beschi's first known Tamil dictionary, Leonardo Cinnami's first known Kannada Malayalam dictionary and Johann Ernst Hanxleden's Sanskrit grammar and dictionary and Xerome Xavier's attempts towards learning Persian.

There are two documented methods in which the Jesuits attempted to convert mainly Hindus, in southern India. This comes to us from studying the Madurai Mission of 1606. Seventeenth century South India has been defined as being "fluid", politically and in matters of religion and society, with various sects of Hinduism, Islam and Christianity co-existing in the segmentary states of Tiruchchirappalli, Tanjavur and Madurai, and each of these various warrior lineages challenged each other in wars and rituals, and for that reason cleared the space for religious innovation as long as it served the interest of conquest, state-building and status enhancement.[5] In comparable circumstances, Italian Roberto Nobili (1577 - 1656) practiced what has come to be known as the "Italian mode" of proselytizing, i.e., of accommodation (*accomodatio*). With his aristocratic approach, and attempts to understanding Tamil culture top-down, considered himself to be a Brahman, i.e, a learned man who had given up material possessions. His argument on the similarity between Christian priests and Hindu Brahmans was reposed linguistically, on the fact 'that they both use a special, difficult to learn, technical and divinely inspired language—Latin and Sanskrit, the Latin of the country'.[6]

In contrast to the top-down approach is the method adopted by Gonçalo Fernandes, who was not only in age and temperament different from Nobili, but their differences in class, education and nationality, probably led to the clashes of opinion on the method of converting. Fernandes had joined the Jesuit order after a brief career as a soldier in the Portuguese *Estado da India* and was by all means it's product. His method of conversion was to disassociate himself from all ways of the pagans and Portugalise them in all matters of life: religion, language, clothing, eating habits etc. With his scant theological knowledge, his only tool could be persuasion, accompanied by economic incentives, rather than theological discussions. His target group, therefore, were the non-elites.

Both European and Indian elite institutions, whether royal courts of the Nayakas or religious schools/ retreats (*matam*), encouraged literary production, the styles and forms of which varied with time, and other aesthetic projects (arts, architecture, etc.), because these were the pillars of their own status evaluation and the constitutive elements of their political legitimacy.[7] Jesuit missionaries endeavoured to appropriate, possess and control the indigenous textual and cultural tradition by compiling dictionaries and grammars, by translating and refuting non-Christian concepts, and by projecting it on a larger European theological and teleological frame. The play of differences, analogies and allegories varied with each and every epistemological move inscribed in the chessboard of time. 'Brahmanical' knowledge, as it came to be regularly defined, would thus resemble, oppose or configure European expectations, depending on political and geographical circumstances, and tropological transformations.[8]

What transpired in the court of Jehangir between his nobles and the Jesuits has recently been brought to light in an extensive article by Muzaffar Alam and Sanjay Subrahmanyam.[9] The authors, using the archives of the Jesuits as well as

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[3] Review of Raj (2007) by T. Roy in *Journal of Global History*, Vol. 3, Issue 01, pp. 129-131.

[4] Mendonca. (2003).is a compilation of papers presented in Goa, India, by educationists and researchers during a conference organised to commemorate the 450th death anniversary of St. Francs Xavier. As the title suggests, discussions were held on the future of Jesuits in India.

[5] Narayana & Subrahmanyam (1992).

[6] Zupanov (1999). p. 238.

[7] *Ibid.* p. 238.

[8] *Ibid*. p. 245.

[9] Alam, & Subrahmanyam (2009).

Persian records, bring forward a Mughal intellectual who studied the Europeans: Abdus Sattar ibn Qasim Lahauri, who could read Latin and had assisted in the production of a Persian translation of the Bible and complied works such as a summary of Sharaf al-Din Yazdi's *Guzida-yi Zafarnama* (??/ History of Timur), Samrat ul-Falasifa (The Fruits of Philosophers) and *Ahwal-i-Firangistan* (Account of the Land of the Franks) and *Majlis-i Jehangiri* (Assemblies hosted Jehangir, held during the nights where intellectuals were invited to debate over several issues). Sattar's Latin tutors were the Jesuits, in particular Jeronimo Xavier, grand-nephew of Francis Xavier. Sattar's main task in the Mughal administration was to translate European books, not just religious and historical ones, but also others having practical and scientific (*ilmi*) information, as astronomy. Sattar was commissioned to grasp the knowledge that lay locked away in Latin books and Jehangir was interested in knowing if the Jesuits would assist in the setting up of a printing press so that books could be printed in Persian.[10]

The story of the conflict on methods adopted for conversion shows, first and foremost, to a historian interested in learning about transfer, creation, assimilation and diffusion of knowledge, that the Jesuits reached both the elites and the non-elites in the complex Indian social milieu of Hindus, Muslims, Buddhists, Jains, Jews and Armenians. While Nobili, convinced of his "top-down" approach, interacted with the Brahmins, other more ambitions, attempted to convert the Mughal Emperor. While yet others practised piety and attempted conversion, and received some success, among the littoral communities of the Fisheries Coast.

While we have Persian sources to collate what transpired among the Jesuits and the Islamic theologians in Jehangir's court, we have, till now no known sources that will inform us the Brahmins' who once debated with Nobili in the villages around Madurai. We need to know their perpective on the debates. What we know for sure is that the indigenous were not always only interested in Christianity. There was a curiosity about the 'practical knowledge' of the Europeans, specially in the Mughal Court.

China

The year 1583, arrival of Matteo Ricci (利玛窦1552-1610) in Guangdong, marks the beginning of an important phase of Christianity in China. This phase lasted for about two centuries, during which Western science, propagated by the Jesuits, appealed to Chinese intellectuals. He studied mathematics under Christopher Clavius (1537-1612) and was quite knowledgeable in the fields of mathematics and astronomy.[11] Unlike their predecessors, Ricci and his followers realised that the fundamental task for the Jesuits was to counter and communicate with Chinese intellectuals and their cultural heritage. They thus worked hard to learn Chinese language and culture as well as traditions of Chinese learning so that they could engage in a real dialogue with the educated Chinese elites. Their strategies included: studying Confucian classics, adoption of Chinese names and adaptation of Chinese styles of living, clothes, cuisine and even rites and rituals. They also established close contact with Confucian elites and local gentry, introduced Western science and technology, translated Western scientific books and writing into Chinese and published books. For some of these they received the support of the Chinese emperors. Ricci had also won over, in the field of religious belief, many Chinese elites such as Xu Guangqi (baptised in 1611 in Nanjing), Li Zhizao (李之藻1656-1630; baptised in 1610 in Beijing) and Yang Tingyun (杨廷筠1557-1627; baptised in 1611 in Hangzhou). With the help of Xu Guangqi (徐光启1562-1633), a Chinese elite who had converted to Christianity and was the vice-minister of the Board of Rites, Ricci translated six books of Christophorus Clavius' edition of Euclid's Elements into Chinese (*jihe yuanben* 几何原本Elements of Geometry). They were published in 1607. Ricci also translated into Chinese three books on Western astronomy.

Xu Guangqi (徐光启1562-1633) stood at the top of the elite. He was the author of the *Nongzheng quanshu* (农政全书a complete treatise on agricultural administration)[12] and the compiler and translator of the *Taixi Shuifa* (泰西水法Water methods from the West, finished in 1612) written by Jesuit Sabbathinus de Ursis (Xiong Sanba 熊三拔). He became to advocate for 'concrete studies' (*shixue* 实学). (*Shi* 实could be translated into English as solid, practical or concrete.) Subjects such as agricultural administration and water control were regarded by Xu as 'concrete knowledge' that could bring tangible general benefits. Western learning conveyed by the Jesuits was seen by Xu as solid, as the way they produce knowledge could be verified by proper methods. Xu's own work on agricultural administration reflected his experimental attitude towards the accumulation of knowledge. But for Xu, subjects such as agriculture, water control were

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10 Ibid.

11 Many of the Jesuits who went to China studied at the Collegio Romano's Academy of Mathematics and the Jesuit College in Coimbra first. See Elman (2005), p.107.

12 The writing of the *Nongzheng* quanshu was a group enterprise, as it was completed, revised and edited by young scholars after Xu's death. See Francesca Bray and Georges Métailié, 'Who was the author of the *Nongzheng quanshu*?', in Jami (ed.) (2001),p. 323.

'useful' because they were relevant to statecraft and could contribute to the welfare of the state and the livelihood of the people (*guoji minsheng* 国计民生).[13]

Translation also played an important role in the transfer of Western science. In order to overcome the difficulties of translation and to facilitate communication, the Jesuits tried to introduce their knowledge by using the classical Chinese terms that were comprehensible by Ming literati (Elman, 2005: 109). The general translation process was that a Jesuit orally translated the Latin texts and dictated it to a Chinese person, who turned it into literary Chinese terms and concepts (Elman, 2005: 111).14 So, the translation work was in fact a cooperative work done by the Jesuits and Chinese literati. Between 1584 and 1790 the Jesuits and their converts in China translated or compiled as many as 437 works in the forms of books, journal articles and manuscripts. While 57% were about Christianity, 30% of the entire publication was concerned with sciences (Elman, 2005: 111).

In the following Qing China, the Kangxi emperor (r. 1662-1722)[15] himself took a great interest in scientific study. He founded a special academy for mathematics and science. In his early reign, he took daily learning on mathematics and astronomy under Jesuit Ferdinand Verbiest (南怀仁1623-1688). The Europeans responded to such patronage enthusiastically. In 1685, Louis XIV dispatched five Jesuits who were also mathematicians including Louis Le Comte (1655-1728) and Joachim Bouvet (白晋1656-1730). Bouvet, for example, stayed in Beijing and taught the Kangxi emperor mathematics and astronomy. The Jesuits' contribution to artillery (for example, cannon making) also met the military needs of the early Qing. Although these Jesuit mathematicians and astronomers gained the favour of the emperor, they gradually disengaged from dialogue with Chinese officials-scholars (Lee, 1991: 11). The Jesuits were also subject to criticism and denouncement from some Chinese nativist scholar-officials, for example, Yang Guangxian (杨光先 1557-1669) attacked Schall for spying and scientific incompetence in 1664.[16] Through the so-called the 'Calendar Controversy' (*lifa zhi zheng* 历法之争), Kangxi supported the service of the Jesuits for the court. In 1692, Kangxi issued an Edict of Toleration permitting the freedom of Jesuit missionaries in China. But the situation changed by the 'Rites Controversy' (*liyi zhi zheng* 礼仪之争) which led to the ban on Christianity in China in 1727.

The sciences served as a lure to the intended propagation of Christianity in China. But the Jesuits did not win as many converts as they expected. The Chinese always drew a distinction between the scientific teaching and the religious teaching by the Jesuits. Fang Yizhi (方以智1611-71) was the first to make this distinction, although he accepted portions of knowledge conveyed by the Jesuits. He said: 'The knowledge from the Far West which entered [China] in the Wan-li period is detailed in 'material investigations' but deficient in speaking of 'comprehending seminal forces' (quoted in Peterson, 1970: 398-399). The distinction drawn by the Chinese was due to a fundamental difference between the Chinese and the Europeans in terms of the views of universe and the Heaven. Confucian scholars served as 'filters' to imported Western science. They were also the medium by which the knowledge got transferred and accepted, albeit after modification or adjustments.

Japan

The Jesuits missionaries arrived in Kagoshima in 1549 when Japan was in the constant military conflicts amongst war lords called daimyo. Anticipating the trade with the Portuguese and Spanish to acquire European muskets and gun powder, local daimyo especially in Kyusyu region enthusiastically patronised the Jesuits missionaries. This promoted the rapid introduction of western techniques such as cannonry, shipbuilding, navigation, mining and metallurgy, which were gradually assimilated by local craftsmen.

As was the case in China and India, the Jesuits' initiative to introduce the western scientific framework was one of the strategies for winning over the Japanese elite for conversion. The Jesuits saw keen interests in natural philosophy amongst the Japanese. Francis Xavier (1506-1552) on his way from Japan to China recommended that the missioners recruited for the Japanese missionaries should not only be "philosophers… well trained in dialectic but also well trained with cosmic phenomena because the Japanese are enthusiastic about listening to explanations of planetary motions, solar eclipses, and the waxing and waning of the moon. All explanations of natural philosophy greatly engage that people's

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[13] On Xu Guangqi, see e.g., Jami (ed.) (2001).

[14] This method was also adopted after 1850 by Protestant missionaries and their collaborators in their translation of works of modern science primarily from English into classical Chinese. See Elman (2005), p.111.

[15] The most common way of referring to a Chinese emperor since the Ming dynasty is by his reign-title (*nianhao* 年号) rather than by his given name. The term 'Kangxi' is a reign-title rather than a name.

[16] Schall proved innocent of the accusation for scientific incompetence with the help of Jesuit Verbiest but other charges were never dropped. See Zurndorfer (2009), p.89.

minds"[17] Alessandro Valignano (1539-1606) also reported the keen interest in scientific knowledge such as geography and astronomy in Japan, and urged the Society of Jesuits to send more qualified missioners. Valignano was the founder of the Jesuits education institutions in Japan. The institutions consisted of seminario for primary schooling[18], noviciaso for secondary education and collegio in Funai located in Funai for further priesthood training. The curriculum of *collegio* reflected the Aristotelian doctrines of late medieval scholasticism, and included theology, philosophy, canon law, jurisprudence, logic, natural science, rhetoric and music.[19] The collegio served also as research institution of the Japanese language and culture: three Portuguese-Japanese dictionaries were published by the Jesuits and used in preparing printed translations of many Western and Japanese literatures for evangelical purpose.

Such Jesuits initiative did win Christian converts amongst Kyusyu daimyo. Notable Christian daimyo included Otomo Sorin (大友宗麟1530-1587) and Takayama Ukon (高山右近1552-1615)[20]. Oda Nobunaga(織田信長1534-1582), the most powerful warlord, also favoured them in order to counteract the aggressive Buddhist rebellions. However, it is important to note that, unlike the case in China and India where the Jesuits interacted with intellectual court elites to produce translations of scientific works, their brotherhoods in Japan operated under the protection by provincial daimyo, and their influence was limited in the cultural and intellectual capital of Kyoto.[21]

It is also fair to say that political conditions rendered the knowledge transfer of the Jesuits science to Japan largely incomplete. After the death of Oda Nobunaga, his successor, Toyotomi Hideyoshi (豊臣秀吉1537-1598) began to eliminate the Christian influence from Japan. Under the Tokugawa regime from the unification in 1603, the Tokugawa government made it clear that Christian faith was officially banned through a series of missionary expels and the execution of the converts in the 1630s.. In 1639, the government prohibited European nationals from entering the country except for the employee of the Dutch East India Company and also prohibited Japanese nationals from travelling abroad. Thus the Jesuits involvement in Japan lasted less than a century and it was confined to a limited part of the country. The strict suppression of the faith under the Tokugawa regime – underground followers were executed – made it difficult for the Jesuit knowledge to leave a systematic influence.

Nonetheless, the legacy of the Jesuits knowledge remained. Nagasaki became a repository of European scientific knowledge during the Tokugawa period. A group of Nagasaki interpreters kept the legacy of European knowledge and the imported Chinese translations of the Jesuits texts came through its port. The latest European knowledge of astronomy and medicine were introduced in Nagasaki through books and the Dutch East India Company officials. Scientifically minded physicians and scholars were attracted to Nagasaki from across the country to learn European knowledge. They gradually came to form the intellectual group of Dutch Learning by the mid eighteenth century, which played an important role in Japan's adaptation of Western science and technology in the nineteenth century.

Conclusion

Mughal India, late Ming and early Qing China, and Tokugawa Japan were as keen to access knowledge from the Europeans as the later were to gather information of the "exotic". The Jesuit then became an important conduit for the transfer of knowledge and their works present to us an opportunity to compare the diverse cultures on China, Japan and India. However, what the Jesuits transferred was not just science and technology but also a new way of teaching and learning coming from a different cultural context. The patterns of intellectual interactions and knowledge transfer were determined by cultural regimes which shaped the mentality of elites who either promote or restrain the generation and diffusion of 'useful knowledge'. Different kinds of elites had different definitions of 'useful knowledge' and ways of appropriating Western knowledge because of their cosmological orientations and intellectual traditions.

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[17] Quoted in Nakayama(1969), p. 81.

[18] An estimate suggested that there were some 200 *Seminario* run by the Jesuits in the 1580s. For the Jesuits missionary and their intellectual legacy, see Ebisawa (1978).

[19] On the Jesuit college, see ibid., p.65-69.

[20] On Christian daimyo, see Elisonas (1991).

[21] Sugimoto and Swain (1978), p. 196

References

On India:

ALAM, M., & SUBRAHMANYAM, S. (2009). Frank disputations: Catholics and Muslims in the Court of Jehangir (1608-11). *The Indian Economic and Social History Review.* 46, 457-511.

DU JARRIC, P., and C. H. PAYNE. (1926). (Routledge 2005 Reprint The Broadway Travellers Series). *Akbar and the Jesuits, An Account of the Jesuit Missions to the Court of Akbar*. New York: Harper & Brothers

MENDONCA, D. D. (2003). *Jesuits in India: vision and challenges*. Gujarat, Gujarat Sahitya Prakash and Xavier Centre of Historical Research.

NARAYANA R, V., SHULMAN, D. D., & SUBRAHMANYAM, S. (1992). *Symbols of substance, court and state in Nayaka period Tamilnadu*. Delhi, Oxford University Press.

RAJ, K. (2007). *Relocating Modern Science: Circulation and the Construction of Knowledge in South Asia and Europe, 1650-1900*. Houndmills, Basingstoke, Hampshire [England]: Palgrave Macmillan.

ZUPANOV I. G. (1999). *Disputed mission: Jesuit experiments and brahmanical knowledge in seventeenth-century India*. New Delhi, Oxford University Press.

On China:

ELMAN, B. (2005). *On Their Own Terms: Science in China, 1550-1900*. Cambridge, Mass.: Harvard University Press.

JAMI, C [et al.] (eds.). (2001). *Statecraft and Intellectual Renewal in Late Ming China: the Cross-Cultural Synthesis of Xu Guangqi (1562-1633)*. Leiden: Brill.

LEE, T H. C. (1991). 'Christianity and Chinese Intellectuals: From the Chinese Point of View', in T. H. C. Lee (ed.) *China and Europe: Images and Influences in Sixteenth to Eighteenth Centuries*, pp. 1-28. Hong Kong: The Chinese University Press.

LIU, J T. C. (1988). *China Turning Inward: Intellectual-Political Changes in the Early Twelfth Century*. Harvard: Harvard University Press.

NEEDHAM, J. (1959). *Science and Civilisation in China: Vol. 3*. Cambridge: Cambridge University Press.

PETERSON, W J. (1970). 'Fang I-chih: Western Learning and the "Investigation of Things", in W.. T De Bary (ed.) *The Unfolding of Neo-Confucianism*, pp. 369-412. New York: Columbia University Press.

On Japan:

EBISAWA, A. (1978). *Nanbangaku no kenkyu*. Toyo: Sobunsha

ELISONAS, J. (1991). 'Christianity and the daimyo' in J.W. Hall and J. L. McClain (eds.) *The Cambridge History of Japan*, volume 4, pp. 321-72.Cambridge: Cambridge University Press.

IKEGAMI, E. (2005). *Bounds of civility: aesthetic networks and the political origins of Japanese culture*, Cambridge: Cambridge University Press.

JACOB, M. (1997). *Scientific culture and the making of the industrial West*, New York and Oxford: Oxford University Press.

MINAMOTO, R. (1980). *Kinsei syoki jitsugakushiso no kenkyu*, Tokyo: Sobunsha

MOKYR, J. (2002). *The Gifts of Athena* , Cambridge: Cambridge University Press

NAKAYAMA, S. (1969). *A history of Japanese astronomy: Chinese background and Western impact*, Cambridge, Mass: Harvard University Press

SUGIMOTO, M. and SWAINE D. L. (1978). *Science and culture in traditional Japan A.D. 600-1854*, Cambridge Mass. : Massachusetts Institute of Technology Press

SUGIMOTO I., (1982). *Kinsei Nihon no Gakujutsu: Jitsugaku no tenkai wo chushin ni*, Tokyo: Hosei Daigaku Syuppankai

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 65.

A MEDIATOR BETWEEN DIFFERENT NATIONS? THE INTRODUCTION OF LABORATORY INSTRUCTION IN SCIENCE CURRICULA OF SECONDARY EDUCATION IN BELGIUM AND GERMANY (1880-1914)

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Abstract

The introduction of laboratory education at Belgian secondary schools during the years 1880-1914 was strongly influenced by German science education. A large part of school books and textbooks used by Belgian science teachers were translations of German publications and the laboratory apparatus in Belgian school laboratories was to a great extent of German origin. Belgian academic scientists got acquainted with German science education combining research and instruction during post-academic training at German universities and laboratories. Fascinated by the integration of experimental and instrument manipulations in German science education, they also ardently pleaded for the introduction of laboratory instruction at secondary schools, created from 1880 onwards at state schools. My paper deals with the characteristics of Belgian experimental education as well. This will lead to an analysis of the uniqueness of Belgian science education at secondary schools, also influenced by French science, illustrated by the following quote of a Belgian scholar concerning disseminating science: 'Belgium is surprisingly well placed to serve as mediator between different nations.'

Introduction

The study of science education also includes experimental instruction: scholarly attention for experiment in science education has risen during the last decade and 'learning by doing' as research topic recently increased in significance, not only in historical studies on popular science books and periodicals promoting a homemade science and on the distribution and the use of experimental kits, but also in education.[1] This paper on the implementation of laboratory instruction in the courses of chemistry and physics at Belgian secondary state schools at the end of the 19th century, aims on the one hand at being supplementary to former studies in national historiography rather emphasizing on institutional aspects (the history of schools, the evolution of educational laws) or on social studies of the target groups of 19th-century education. On the other hand, the pursuit of this paper is also to be complementary to the existing international historiography on secondary science education by explaining the characteristics of the Belgian case.

Because of its central geographical position in Western Europe, Belgium was pre-eminently a country of intellectual exchange, already agreed upon by the 19th-century contemporary actors as well. Belgian intellectuals promoted the idea of an 'international patriotism'; the nation's national individuality was precisely to be found in being international.[2] This was also the case for the circulation of science: Belgium was influenced by several different scientific cultures, especially by neighbouring countries France and Germany, the two leading scientific centres in the 19th century, and Belgian scientists coquetted with the idea that Belgium functioned as a country of intellectual passage, both for the production of knowledge in a scientific, rather closed area and for the communication of science in the public sphere. That did not alter the fact that 19th-century public discourse on science and science education mainly took Germany with its first-rate position in science and education as the standard.

Consequently, the aim of this paper is to find an answer to the question: to what extent Belgian laboratory instruction in secondary education meant for the elite and the upper middle classes, was influenced by foreign and in particular German educational and scientific cultures. This paper deals with secondary education, since natural sciences definitively got embedded in the curricula of secondary state education from the beginning of the years 1880 onwards, in contrast with Belgian primary education wherein mandatory courses in natural sciences were removed by catholic politicians. My investigation on laboratory research in secondary state education in Belgium is based on textbooks, brochures, official inspection reports and hand written accounts of teachers' meetings at state schools.

International or nationalistic character of science at Belgian secondary schools?

Secondary state education in Belgium was reformed during the liberal government by means of the law of 1 June 1850, providing for the establishment of ten Royal State Schools ('Athénées Royaux'), one in each provincial capital (Brussels, Antwerp, Ghent, Bruges, Hasselt, Liege, Mons, Namur, Arlon), plus Tournai. These state schools were public, secular institutions - although religion education remained a permanent issue of debate, particularly under catholic government - and they had a propaedeutical function for university education.

Belgian teachers were aware of developments and debates in foreign science education, for instance on the contribution of mathematics in physics and the debate on physics as an experimental science. Belgian official inspection reports explicitly referred to current developments (reforms, laws) in secondary education in neighbouring countries (Germany, France, The Netherlands and Great-Britain): developments in Belgian education were usually compared to these different nations and Belgian secondary education was embedded in an international context.[3] Belgian politicians were longing to copy the German structure of secondary education at the Belgian Royal State Schools: the German 'Gymnasium', a grammar school with a curriculum based on ancient languages was comparable to the Humanities section, 'Realschulen' (modern schools with scientific and technical objective and without Latin instruction), teaching science and preparing higher education at the 'technische Hochschulen' and the 'Höhere Bürgerschule', comparable to the professional section of the Belgian Royal State Schools.[4] Belgian architects drew inspiration from study trips to German secondary schools for the construction and the infrastructure of Belgian secondary schools. Following the German debate, in Belgium too the creation of practical science lessons at secondary schools threw doubt upon the ideal of classical education.

Furthermore, the admiration for German science education was mirrored in Belgian educational scientific collections. Belgian school laboratories usually possessed a large number of scientific instruments produced in Germany, 'in which one

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[1] V. Van Beek, 'Man lasse doch diese Dinge selber einmal sprechen. Experimentierkästen, Experimentalanleitungen und Erzählungen zwischen 1870 und 1930', in: *NTM. Zeitschrift für Geschichte der Wissenschaften, Technik und Medizin*, 17 (2009), 387-414; J. Simon, J. Ramon Bertomeu-Sanchez, A. Garcia-Belmar, 'Nineteenth-century scientific instruments in Spanish secondary schools', in: M. Lourenço and A. Carneiro (eds.), *Spaces and collections in the history of science: The laboratorio chimico overture*, Lisboa, 2009, 167-184; P. Heering and R. Wittje (eds.), *Learning by doing: Experiments and Instruments in the History of Science Teaching*, Stuttgart: Franz Steiner Verlag, 2011.

[2] J. Tollebeek, 'The hyphen of National Culture. The Paradox of National Distinctiveness in Belgium and the Netherlands, 1860-1918', in: *European Review*, 18 (2010) nr. 2, 207-225.

[3] *Rapport triennal sur l'état de l'enseignement moyen en Belgique. 1879, 1880, 1881*, Brussels, 1881 (introduction).

[4] *Rapport triennal sur l'état de l'enseignement moyen en Belgique présenté aux Chambres législatives, le 7 aout 1889. Douzième période triennale 1885-1886-1887*, Brussels, 1890, X.

can have absolute trust',[5] amongst others spectroscopes and polarimeters of the company Franz Schmidt & Haensch (Berlin) well-known for its optical instruments, or microscopes of Carl Zeiss (Jena).[6] German scientific apparatus were promoted in Belgian textbooks too; Jean Chalon wrote: 'The Berlin Company Klönne & Müller also offers a large number of microscopic preparations, beautiful and not expensive.'[7] Moreover, the use of (translated) German textbooks was usual. These books could also be found in school libraries, amongst others translations into French such as R. Fresenius' 'Traité d'analyse chimique qualitative', of Justus von Liebig's 'Traité de chimie organique' or F. Mohr's, 'Traité d'analyse chimique' (1888).[8]

However, this German influence in Belgian secondary science education did not exclude admiration for other foreign traditions. Moreover, the fact that Belgian secondary science underwent strong influences from abroad appeared to be an essential characteristic of national education. School libraries for instance also contained British and Dutch textbooks on experimental chemistry and physics. Belgian education was importantly influenced by French secondary education too, the French textbook of Adolphe Ganot's *Traité élémentaire de physique expérimentale et appliquée* (1851) and its later editions was used as official pedagogical material at Belgian Royal State schools and Teachers' Colleges. Ganot's physics textbook was largely distributed in the 19th century, amongst others in Great Britain,[9] but also in neighbouring country Belgium, where French was the communication language in all state schools.

The provision of textbooks changed over time, and if initially it was based mainly on (German and French) imports, subsequently they were replaced by textbooks written by Belgian authors (both teachers and academic scientists). Yet these textbooks on physics remained often inspired by textbooks such as those by Ganot: the illustrations in Ganot's textbooks were sometimes adopted or comparative references were publicly made to his book implying that it remained the standard.[10] Belgian textbooks had to follow the official educational programs and they had to meet other criterions as well: in regard with the lay-out, textbooks had to contain illustrations large enough to give pupils a pleasant impression. Concerning the content, a clear and succinct style on a basic level geared at young pupils was considered important just as an accurate description of phenomena, definitions, laws and experiments.[11] Some of these Belgian textbooks were approved by the Ministry of Education to be used as official textbook in state secondary education, amongst others 'Lecture on basic chemistry' ('Traité de chimie élémentaire') written by L. Michelet, doctor in physics and mathematics and teaching at the state Teachers' College of Brussels. His later published textbook 'Lecture on elementary physics meant for Teachers' Colleges and secondary schools' ('Traité de physique élémentaire à l'usage des écoles normales et des établissements d'enseignement moyen') would replace Ganot's textbook at Belgian Royal State Schools and Teachers' Colleges, for instance at the Ghent Teachers' College for state secondary education of inferior level from 1888-1889 onwards.[12] Other textbooks were used as national substitute for Ganot's textbook as well, such as 'Elements of physics meant for state secondary schools' ('Eléments de physique à l'usage des écoles moyennes') written by J. Fleury, teacher at the Royal State School of Liege or 'Lecture on elementary physics' ('Traité de physique élémentaire') written by Fleury and G. Duguet, repetitor at the Mining School of Liege. The latter was divided into two parts: the first part was devoted to the general characteristics of matter and the theory of heat, whilst the second part treated on acoustics, optics, magnetism and electricity. Every chapter ended with a number of exercises. This textbook also paid attention to new scientific discoveries and its applications and to the current status of modern science.

Remarkably enough, the aforementioned Belgian textbooks were not applied as instruments of patriotic education or as a means to pay a heroic homage to national scientists. The canon of scientists quoted in these textbooks remained international (e.g. Torricelli, Pascal, Regnault, Gay-Lussac, Otto de Guéricke, von Helmholtz).[13] The fact that secondary science education was not striving explicitly for nationalistic aspirations can also be illustrated by two meetings of the entire teaching staff of the Royal State School of Tournai around the turn of the century. Debates took place on the questions on how and to what extent teachers of general courses in their respective fields could contribute to education in 'national pride' and on whether or not pupils of secondary schools had to be instructed in a more systematic manner about the contribution of Belgium in the progress of the arts, literature, industry and of the sciences. The teachers in natural sciences and

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[5] J. Chalon, *Manuel de sciences naturelles*, Mons: Hector Manceaux, 1890, 40-41.

[6] State Archives Department Anderlecht, *Archives Athénée de Bruxelles*, nr. 44.

[7] J. Chalon, *Manuel de sciences naturelles*, Mons: Hector Manceaux, 1890, 45.

[8] Athénée Royal de Bruges. Programme des Cours pour l'année scolaire 1889-1890, s.l. s.d., 8.

[9] J. Simon, 'Circumventing the 'Elusive Quarries' of Popular Science: The Communication and Appropriation of Ganot's Physics in Nineteenth-Century Britain', in: F. Papanelopoulou, A. Nieto-Galan, E. Perdiguero (eds.), *Popularizing science and technology in the European periphery 1600-2000*, Farnham: Ashgate, 2009, 89-114, 95-96.

[10] J. Chalon, *Manuel de sciences naturelles*, Mons: Hector Manceaux, 1890, 9.

[11] *Revue de l'Instruction Publique en Belgique*. Tome XXVI, Ghent, 1883, 267-270.

[12] State Archives Department Ghent, *Archieven van de Middelbare Rijksnormaalschool Gent*, nr. 505.

[13] K. Wils, 'Vaderlandsloze kennis in dienst van de natie. Wetenschapsvulgarisatie in België', in: E. Witte (ed. et all), *Nation et démocratie 1890-1921. Actes du colloque interuniversitaire, Bruxelles 8-9 Juin 2006*, Brussels, 2007, 333.

mathematics decided unanimously that the contribution of Belgian scientists to scientific progress had to be explained to the pupils on a mere occasional basis. Indeed, biographies of Belgian scientists and explanations on the nature and the importance of their research could be explained at the right moment and on the right tone, geared at the pupils' level and without a sterile and pompous rhetoric. They added that only a few compatriots were prominent scientists.[14]

Experimental instruction and the implementation of school laboratories

Belgian secondary education was strongly orientated towards academic instruction, probably due to the fact that most of the teachers in chemistry and physics for secondary education on a superior level were academically trained and thus familiar with academic experimental education. As a result laboratory courses at secondary schools were inspired by academic experimental training. Experimental instruction at Belgian universities was in its turn following the example of German academic tradition, fiercely admired by Belgian scientists for its independent research done by students, its mandatory composition of a dissertation and its development of experimental research. Belgian scientists commended on the large investments of German universities into libraries, scientific collections and laboratories for chemistry, physics, physiology and anatomy. In the years 1860-1880 Belgian academic scientists were zealous advocates of the establishment of similar practical courses at faculties for Medicine and Sciences in order to introduce young students to scientific apparatus and research practice, resulting in a voted bill in 1890, explicitly encouraging the Belgian universities to orientate themselves towards research and to adopt seminars and practical teaching methods.[15]

Following these developments of the institutionalization and professionalization of natural sciences at universities installing practical seminaries and laboratories according to the German example from the years 1870 onwards, politicians, scientists and teachers pleaded for the establishment of experimental instruction at secondary schools as a substitute for the existing dogmatic and ex cathedra-education in natural sciences. Furthermore, the emancipation and professionalization of chemistry and physics in secondary science education curriculum required appropriate classrooms and didactic material. A Ministerial Order of 15 January 1880 summoned a commission of school inspectors and teachers to propose measures for the development of suitable infrastructure and didactical collections meant for science education based on observation and manipulation in the Royal State Schools, secondary state schools and the state Teachers' Colleges. In 1881 the government granted an allowance to the aforementioned institutions meant for the purchase of physics instruments.[16] Moreover, separate classrooms for the courses of physics and chemistry were provided. At the end of the years 1880 official inspection reports mentioned that most of the Royal State Schools had the disposal of both a cabinet of physics and a chemistry laboratory in order to support theoretic science lessons with experiments and manipulations, financed by both state and local authorities.

The Royal State School of Brussels for instance had chemistry and physics laboratories at its disposal from 1887 onwards; these science classrooms were financed by the city council. The physics laboratory and its adjacent physics auditorium were established on the initiative of Max Seligmann, teacher in physics at this school. Glass cases contained a large collection of instruments suitable for conducting experiments.[17] The chemistry laboratory of the Brussels Royal State School, its chemistry auditorium, library and room of balances, was known as an exemplary institution. It was equipped with six cages for evaporation, a steamer, a drying kiln and two sinks and amongst the scientific instruments were microscopes, objectives, stereoscopes, polarimeters, saccharimeters, spectroscopes, Bunsen burners, voltameters, small and large Ruhmkorff coils and apparatus for the manipulation of gases, including a gasometer of Gay-Lussac with a capacity of 36 litres.[18]

This chemistry laboratory was arranged according to the guidelines of Charles-Polydore Francotte (1851-1916), doctor in natural sciences at the Liege State University, teacher at the Royal State Schools of Namur and Brussels and from 1890 onwards lecturer and professor at the Free University of Brussels ('Université Libre de Bruxelles') holding the chair in animal embryology.[19] He excelled in experimental secondary science education by performing experiments and demonstrations and he emphasized the characteristic properties of secondary science education. Emphasizing the

[14] State Archives Department Tournai, *Archives Athénée Jules Bara*, nr. 108: Régistre des Conférences mensuelles du corps professoral, 14 February and 7 November 1907.

[15] P. Dhondt, *Een tweevoudig compromis. Discussies over universitair onderwijs in het negentiende-eeuwse België*, unpublished doctoral thesis K.U.Leuven University, Leuven, 2005, 272, 346; J. Wachelder, 'The German University Model and its Reception in the Netherlands and Belgium', in: R.C. Schwinges (ed.), *Humboldt International: der Export des deutschen Universitätsmodells im 19. und 20. Jahrhundert*, Basel: Schwabe, 2001, 180, 201-203.

[16] *Rapport triennal sur l'état de l'enseignement moyen en Belgique. 1879, 1880, 1881*, Brussels, 1881, LXIV.

[17] A. Sluys, *Exposition universelle de Bruxelles. Les compartiments scolaires. L'Allemagne - La Belgique - Les Pays-Bas*, Brussels: Imprimerie du Progrès, 1911, 131-132.

[18] State Archives Department Anderlecht, *Archives Athénée de Bruxelles*, nr. 44.

[19] E. Goblet d'Alviella, 'Notice biographique et bibliographique: Ch.-J.P. Francotte', in: *L'Université de Bruxelles pendant son troisième quart de siècle (1884-1909)*, Brussels, 1909, 230.

characteristic properties of secondary science education, Francotte excluded the installation of imitations of academic laboratories at secondary schools. He stressed that in contrast with academic laboratories meant for experimental instruction where students could work without constant supervision, school laboratories meant for young pupils unfamiliar with scientific manipulations had to be arranged differently for reasons of safety. The chemistry teacher had to supervise actively and continuously his pupils in order to avoid accidents. Authors of textbooks warned as well for the dangers of experimental training: 'In novice hands the first chemistry manipulations might result into serious, even mortal, accidents.'[20] Thereby, school laboratories had to be installed so that the teacher could oversee the entire class all the time. Therefore Francotte equipped his laboratory with four different manipulation tables with each a series of instruments and reagents and each table was meant for a group of four pupils.[21] Moreover, Francotte insisted on the huge importance of hygiene and ventilation in school laboratories: harmful gases had to be evacuated in a safe way. The arrangement of equipment implied precautionary measures by placing the cages of evaporation in the middle of the laboratory - in connection with windpipes reaching the ceiling - instead of against the walls. Pupils were only allowed to exercise with some of the laboratory equipment: in the Royal State School of Brussels for instance the room of balances contained several expensive balances, yet pupils had to practise with a cheaper specimen before they were allowed to use the more expensive ones.[22] Furthermore, teachers also argued on the necessity of the installation of ateliers where pupils could construct themselves scientific instruments, which already existed at Teachers' Colleges.[23]

The importance of laboratory instruction for the chemistry and physics courses instead of abstract, theoretical and memory-based science education was also stressed at the state Teachers' Colleges. Their educational program of 1881 stated that 'The education of natural sciences has to be based on observation and experimentation.' Teachers' Colleges for secondary education of both the inferior and the superior level paid attention to experimental education in the courses of chemistry and physics, learning future teachers how to use microscopes and to describe, explain and perform experiments for secondary education pupils.[24] Notwithstanding the fact that observation, manipulation (measuring and weighing of objects) and experimentation were promoted as the appropriate pedagogical method for the instruction of chemistry and physics, teachers agreed that memory work still had its place and they determined exactly what had to be memorized: formulae in physics had to be learned by heart just as the chemical and physical composition of solids in chemistry, but - again - experiments could stimulate and even guarantee remembering these definitions.[25]

Next to experiments, agreement existed on the importance of the inductive method in secondary science education, not only in chemistry, but also in physics. Teachers mentioned that the use of the deductive method in the instruction of physics did not teach anything new to the pupils, since the experiment executed by the teacher was diverting and only illustrating and affirming the aforementioned explained laws and theories, whilst the inductive method allowed the pupils to explain and to understand the observed phenomena in a heuristic and self-reliant way.[26] It was obvious that chemistry was suitable for experimental instruction, but teachers also insisted that physics was a pre-eminently experimental science, notwithstanding the fact that it was based on mathematical formulae and axioms. Following the tendencies of French and German education where the merely mathematical approach of physics lost ground in secondary education,[27] teaching staffs at Belgian secondary state schools discussed the contribution of mathematics in physics. Marcellin Chapaux, teacher in natural sciences at the Royal State School of Tournai, put forward that physics instruction had to offer insight in the natural phenomena and its laws and that physics 'was an experimental science above all.'[28]

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[20] J. Chalon, *Manuel de sciences naturelles*, Mons: Hector Manceaux, 1890, 9.

[21] A. Sluys, *Exposition universelle de Bruxelles. Les compartiments scolaires. L'Allemagne - La Belgique - Les Pays-Bas*, Brussels: Imprimerie du Progrès, 1911, 135.

[22] P.C. Francotte, Notice *concernant l'enseignement de la chimie à l'Athénée Royal de Bruxelles*, 2de éd., Brussels, 1911, 12; P.C. Francotte, *Description du laboratoire de l'Athénée Royal (en collaboration avec M. Matthieu)*, Brussels: Van de Weghe, 1897, 3.

[23] A. Sluys, *Exposition universelle de Bruxelles. Les compartiments scolaires. L'Allemagne - La Belgique - Les Pays-Bas*, Brussels: Imprimerie du Progrès, 1911, 133.

[24] State Archives Department Ghent, *Archieven middelbare Rijksnormaalschool Gent*, nr. 505; *Rapport triennal sur l'état de l'enseignement moyen en Belgique présenté aux chambres législatives le 7 août 1889. Douzième période triennale, 1885, 1886, 1887*, Brussels: Fr. Gobbaerts, 1890, CXLVIII.

[25] Archives Royal State School Hasselt, *Rapports des conférences professeurs (1900-1906). Compte-rendu de la séance du 28-II-1909.*

[26] L. Wouters, *Méthodologie de l'enseignement des sciences naturelles (extrait de la 'Revue des Humanités en Belgique')*, Tournai, 1898.

[27] *Exposition allemande de l'enseignement à l'Exposition universelle de Bruxelles 1910. I. Guide*, Berlin: Librairie Weidman, s.d., 52.

[28] State Archives Department Tournai, *Archives Athénée Royal Jules Bara*, nr. 108: Procès-verbal des Conférences des professeurs de mathématiques, des sciences naturelles et des sciences commerciales, 9 et 10 février 1911.

Conclusion

The admiration in Belgium for foreign traditions of secondary education was not only purely rhetorical though. Moreover, the fact that Belgian secondary science education underwent strong influences of Germany and France, appeared to be an essential characteristic of the national style of secondary education. Contemporaries agreed that Belgium was a country of intellectual passage and that Belgian secondary science education was situated in a transnational context. Internationalism was cultivated; Belgian education considered itself being appropriate to serve as mediator between different nations, according to a contemporary scholar: 'Why not draw abundantly from these [foreign] sources of education? (…) Within this context Belgium is surprisingly well placed to serve as mediator between different nations.'[29] The use of German and French textbooks and the promotion of an international canon of scientists in Belgian textbooks implicated the uncomplicated integration of 'foreign' patriotism in Belgian state secondary education. The explicit longing for the German laboratory courses (mainly at the Realschule) - traces of the German influence are to find in school infrastructure, laboratory apparatus, textbooks and inspection reports - followed the Belgian academic tradition.

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[29] E. Gauthy, 'Supplément à la partie scientifique du catalogue de la Ligue', *Bulletin de la Ligue de l'Enseignement*, 1867-1868, III, 65-74, 67.

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NOT OUT OF THE BLUE –THE GENESIS OF MODERN SCHOOL SCIENCE TEXTBOOK DESCRIPTIONS OF HISTORICAL EXPERIMENTS

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Abstract:

Recently, several papers in the field of science education have been published that analyse school and university textbooks with respect to accounts of certain historical experiments, such as Millikan's oil drop experiment or the photoelectric effect. In doing so, the authors generally criticize the accounts in today's textbooks as erroneous with respect to the historical experiment, as well as misleading with respect to aspects of the nature of science. Even though these criticisms are well founded, it remains an open question why these accounts can be found in textbooks. It appears to be evident that not every detail of the historical experiment can be included in the description; consequently, decisions are to be made on which aspects are relevant and which are superfluous. These decisions are not simply made, but are the outcome of a historical process –consequently it appeared to be interesting to examine the genesis of these descriptions.

Introduction

One might expect that educators who are advocating the use of history of science are happy when historical aspects or historical experiments enter science textbooks for schools. However, in several cases they are not. There are a number of reasons why this could be observed: There are some examples where history of science is still reduced to names and dates or used in a purely anecdotal manner. However, references to historical experiments should be an indication of a more adequate use, particularly when they go beyond giving a date and a name. Yet, things are not that easy, and experimental accounts may also turn out to be problematic. In this respect, at least three main reasons can be identified: The story is inadequate from a scientific perspective. As we are discussing only science textbooks and not history textbooks, it should probably be extremely exceptional. However, having such a mistake is of course extremely problematic as the authors should be expected to get at least this part correct. However, taking a closer look at historians' accounts of experiments reveals problems can occur in this respect with peripheral instrumentation or procedures. In this respect it is important to keep in mind that also science museums as well as historical accounts of experiments focus on what is considered to be the central unit of a device. Consequently, one should not be surprised that textbooks have a similar attitude as space is limited and thus considered to be precious. However, when judging these aspects, one has to keep in mind that it is not that obvious where an adequate simplification is made or whether something is already a misinterpretation. In any case, such accounts may lead to an oversimplification of experimental practices and could thus be misleading.

The story is inadequate from a historical perspective. This is certainly not that unlikely, and here criterions are also not that simple. Whilst misdating, misplacing or giving wrong names is fairly obviously erroneous, one could (and should) ask where an account of a historical experiment can still be considered as historical, and even more, as adequate from a historical perspective. Obviously, a science textbook is not able to give all the details about instrument makers, financial background, social interactions, philosophical beliefs of the main actors, etc. Whilst one cannot expect every detail to be included in a science textbook account, at least central historical aspects should be mentioned in order to take this as an account of an historical experiment.

One might expect that these are the central levels of misrepresentation of historical experiments; however, there is at least one more. The story might also be inadequate from a nature of science point of view. History of science is strongly advocated to enable students to develop a better understanding of the nature of science. Consequently, an account of a historical event (which could be an experiment) should support an understanding of the nature of science that is considered to be adequate.

From the perspective of a historian, one could and should ask how these stories about historical experiments came into the textbooks. Obviously, accounts on experiments in science education are nothing stable or constant, thus one might expect that accounts of historical experiments we find nowadays in modern textbooks are the outcome of a historical process of textbook writing.

This process may reflect different aspects: of course, textbooks reflect what is considered to be accepted knowledge, even though in the early 19th century, textbooks were also a means to communicate new knowledge. But there are also different aspects on what education should achieve, consequently different (aspects of) historical episodes may be considered as being relevant for the intentions of the textbook authors. This is not limited to explicit intentions which can be found in an exemplary manner in textbooks published during German fascism. There are also implicit intentions and beliefs that may play a role with respect to what historical episodes should by exemplified. And of course one has to consider the changing role of textbooks, consequently, accounts may be modified due to changes in the educational concept and the role textbooks have in this respect.

However, when taking a closer look at accounts of historical experiments, some aspects appear that seem to go beyond the individual cases. In the following, we will discuss three examples and finally are going to draw some more general conclusions.

The speed of light and Ole Rømer

Ole Rømer is frequently mentioned as the first person to measure the speed of light. In 1676, a short account on his work was published in the Paris Memoires de l'Académie Royale des Sciences. In this account, it was mentioned that Rømer aimed to related measurements of Brahe made at Uraniborg to those made in Paris (see Bobis & Lequeux 2008). Rømer observed a discrepancy between the time the moon Io vanished behind Jupiter (or in the shadow of Jupiter) and the time calculated from the period of Io's revolutions around Jupiter. When the Earth was moving away from Jupiter, the moon vanished somewhat later than calculated, when the Earth reduced the distance to Jupiter, the moon was too early. Rømer explained this discrepancy by arguing that the light needs some time to travel the additional distance between the two planets.

This observation is a classical example of an historical experiment that is found in many textbooks. However, as Wallasch already observed almost thirty years ago, these accounts are erroneous in several aspects: Remarkably enough, he even identifies several scientific mistakes in the accounts, to mention but one very striking: It is indicated that the vanishing of Io behind Jupiter is belated some 22 minutes with respect to the diameter of the orbit of the Earth. This is clarified by arguing that when the observation is made at C (see Fig. 1) and half a year later at A, the delay is accumulated to 22 minutes.

A central problem with this line of argumentation is the assumption that Io (and Jupiter) can be observed whilst being behind the sun – everyone who has ever tried to look 'through' the sun is well aware that this is pure nonsense.

Remarkably, Wallasch's paper seems to have had next to no impact on textbook authors, this description and the corresponding diagram is still to be found in most textbooks. Moreover, most textbooks that mention Rømer's work claim that he measured the speed of light. This is not what he did, nor did he have the intention. He wanted to compare data that were taken at two different observatories (by Cassini and Brahe) and thus tried to connect the times of observation through Io as a 'celestial clock'. And finally, the impression is created that Rømer's work was straightforward but not very accurate, most books mention a value of 220.000 km/s as a result of his 'measurement'.

To summarize it can be argued that the description of Rømer's work on the observations of Io is misleading in the scientific as well as in the historical and the nature of science perspective. At the same time it gets evident that such an approach has some stability even when it is criticized.

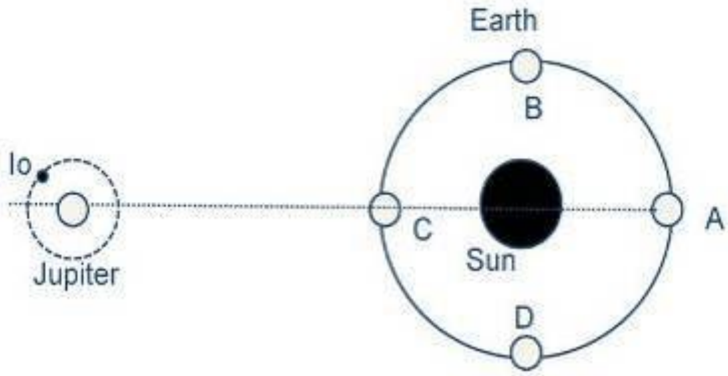

Fig. 1: Principal illustration of Rømer's observation (erroneous).

Millikan's oil drop experiment

Millikan's oil drop experiment to determine the elementary electrical charge is among the classical experiments from the history of physics. Actually, according to a non-representative survey among physicists, it is considered to be among the 'most beautiful' experiments of all time (Crease 2002). And of course the description of the experiment is found in almost every modern textbook for upper secondary school physics teaching. However, when looking a little bit more closely at these accounts, it gets evident that these descriptions are not really satisfying. As Niaz observed, "[m]ost textbooks do not present Millikan's oil drop experiment within a historical, much less philosophy." (Niaz 2000, 503, see also Rodriguez & Niaz 2004, and Niaz & Rodriguez 2005).

However, this criticism addresses modern textbooks. Niaz did not analyze historical textbooks or the genesis of these accounts.[1] When looking at the introduction and subsequent treatment of this experiment in German physics textbooks, some surprising details appear. The first description in a German school textbook can be found already in 1915 (Püning 1915). This description is remarkably clear and adequate, the working principle of the experiment is adequately reproduced and some details are given. However, this description is an exception, most textbooks until the 1950s do not mention Millikan or the oil drop experiment. Only then, it becomes a standard experiment in the context of physics teaching. In most of the textbooks, the experiment is placed in the context of the introduction of the elementary charge, it appears to be simple, straightforward, and its result unambiguous (which is not the case when the controversy between Millikan and Ehrenhaft is taken into consideration, see Holton 1981). Yet, it is not only the misleading description of the status of the experiment that has to be criticized. Actually, the procedures as described in many accounts do not represent the ones by Millikan; particularly the determination of the oil drop diameter is described in a manner that does not work. Moreover, several accounts describe a method where the oil drop is made to levitate due to the electrical field that is compensating the gravitational force. Even though Millikan initially tried this kind of procedure, he quickly realized that he had to modify the experiment as Brownian motion made it next to impossible to be sure that the oil drop was at rest.

When comparing different accounts, it gets evident that the description of the experiment is modified towards the modern account. This can be described as being an idealized version of the initial working principle. In some sense, the historical experiment is more and more transformed into an ideal of this experiment that shows even some resemblance with a computer simulation. This detail is also strengthened by the illustrations: Initially, there are sketches of the working principle, in the 1970s, photos of the standard school version of the experiment are included, whilst in the most recent books schematic illustrations are used that resemble a simulation and in particular does not provide any realistic impression of the dimensions of the experiment and the oil drops.

[1] Such an analysis has been carried out by Parlow, her results are discussed in Parlow & Heering (2009).

The example of the electron

For the analysis of the portrayal of electrons in schoolbooks 22 schoolbooks with publication dates from 1905 until 1950 were examined. The electron usually appeared in four different contexts, these being the nature of the cathode rays, of electricity, of the atom and of the electrolysis process. The results of the analysis that can be categorized are summarized here.

For almost every books the authors assume a position of conclusive authority when writing about the electron. Although it is noted in many cases that the respective theory is just that, a theory, most authors do not mention past failures in the search for answers, competing theories or possible future developments. They almost never mention the people behind a theory or important experiment and their struggles in their search for answers. And if they ever do, it is only with a delay of several years. Experiments are simplified into ideal representations of the actual real process with all its complications. This idealized arrangement of facts leaves the reader with still lifes of the actual historical process of scientific progress.

In 1897 Thomson formulated that the cathode rays are negative particles with a certain relation of charge and mass. In 1899 he could show that the particles found through thermionic emission and photo emission possess the same relation of mass and charge. In 1906 he was awarded the Noble Prize for his works regarding the electron. The cathode rays are mentioned for the first time in the schoolbook *Grundriß der Physik* by Spies from 1913. However, Thomson's name is never mentioned in any of the books.

Drude, who proposed the model of the electron gas to explain the electric current in 1900, and Tolman, who could produce experimental evidence for this concept in 1916, are not mentioned in the schoolbooks as well, although in their work, the electrons are directly connected with the conduction of electricity for the first time in the schoolbook *Lehrbuch der Physik* by Püning from 1915. The core facts of Drude's theory are still mentioned as late as in 1947 in Speitkamp's *Lehrbuch der Physik*.

The planetary model of the atom is mentioned for the first time in Meyer's *Physik und Chemie* from 1926, twelve years after Rutherford's discovery, who, again, is not mentioned by name. However, Bohr is mentioned but the most important details of his model are not, there is no difference to the planetary model of the atom. De Broglie's matter waves are not mentioned at all, although Davidson and Germer could demonstrate in 1927 the ability of the electron to behave like a matter wave.

A notable exception to this portrayal of the scientists related to the electron is the way Millikan is depicted. Schauff sings his praise when he writes in his *Grimsehl Lehrbuch der Physik* from 1928 that of all the attempts to measure the charge of the electron the most precise was Millikan's through and exhaustive oil drop experiment. However, as we have discussed already in the previous section, the accounts on Millikan's work until after World War II are few.

Just as people and controversies behind a theory or discovery are almost never mentioned, so are the experiments. This is particularly striking with respect to the methods of measuring the electron charge. Descriptions in the schoolbooks can be seen as being reduced into idealized versions of the experiments. They are stripped from their historical contexts and significantly simplified. As a result of these modifications, they appear as simple devices that only exist to prove a point. In his *Grimsehl Lehrbuch der Physik* Schauff even goes as far as to arrange the different topics relating to the electron, theories and experiments, in such a way as that they lead to the planetary model of the electron. Here the reader finds a remarkably idealized medial reproduction of the scientific process and, as part of that, a line of argumentation that doesn't have much to do with the actual historical process. However, at the same time the author is creating the impression of historical (as well as scientific) accuracy and correctness. Schauff does mention Millikan but for his arrangement of topics this doesn't matter because he describes a method for measuring the charge of the electron that is derived from electrolysis.

Schauff's book is certainly an extreme example, but almost all books under examination show this tendency to create still lifes of the scientific process and to assume an authoritarian tone by re-creating the physics of the electron on their pages.

However, when analysing the books one after the other it gets obvious that the concept of the electron changed significantly over time and from book to book. Over time, when it comes to the question whether positive electrons exist and from book to book regarding the question whether the electron is a particle with a mass or a quantisation of charge or both.

Positive electrons are mentioned for the first time in the schoolbook *Anfangsgründe der Physik* by Knops from 1906. He states that the elementary charge is composed of positive and negative electrons. Positive electrons appear in some of the books until they are doubted for the first time in Püning's *Lehrbuch der Physik* from 1915. "If one would assume according to Symmer two fluids, but only one being movable, the other fixed to the matter, this would correspond to Franklin's hypothesis. The movability of negative electricity is out of question. As one assumes nowadays, the fluids, at least the negative one, are purely composed of identical small particles, the so-called electrons". (Püning, 1915). His book is in several respects remarkable (as already mentioned in the analysis of the accounts on Millikan's experiment). Here, he seems to indicate that electrons are a hypothesis, he also creates the impression that there are still open questions (e.g. with respect to the positive charge), and he is making adequate historical references, thus putting the actual theory in a historical perspective. However, Püning also makes conflicting statements about the electron. In the contexts of electricity and electrolysis they are postulated but in the context of the cathode rays Püning doubts their existence. After 1915 positive electrons do not appear again in the context of electricity but are used in Meyer's *Physik and Chemie* from 1926 in order to explain the structure of the atom. Meyer argues that negative electrons float around a core that is mainly made up of positive electrons. They are mentioned still in another book in the context of the atom. In Schauff's *Grimsehl Lehrbuch der Physik* from 1928 there is already only an ambiguous statement about positive electrons in the context of the atom. He describes that some of the electrons might be build into the atomic nucleus. In Schippenkötters *Physik für höhere Lehranstalten* from 1931 the protons are mentioned for the first time. Schippenköter also describes a negative charge as an abundance of electrons and a negative charge as a lack of electrons. From then on, positive electrons are not mentioned anymore.

From this brief description it gets evident that the concept of positive electron changed its meaning at least twice. First, positive and negative electrons appear in order to explain positive and negative electric charges. Afterwards, positive electrons are no longer used to explain positive charges –they reappear in the context of the atom. They are now used to explain the positive charge of the atomic nucleus. Finally, there is the transition to our understanding of electric charge and the atom. In both cases the positive electron was first postulated, the concept was then weakened by ambiguous or conflicting statements and then replaced by a newer one. However, in each individual instance, the respective understanding is depicted as a 'fact'.

The question whether the electron is a quantisation of charge or a particle with mass or a particle that carries an elementary charge remains unsolved until the 1950s. For Knops the electron is clearly a charge quantisation when he writes in 1906 that the elementary charge is an electricity atom which is called electron: "One looks at these electrical elementary quantities as electrical atoms and names them electron (positive and negative electron)" (Knops, 1906). In Weiler's *Elektrizität und Magnetismus* from 1905 the author writes that the smallest particles with mass are the electrons to which the elementary charge is connected: "The smallest mass particles which are connected to the electrical elementary quanta are the electrons …" (Weiler 1905). In Günther's *Grundriß der Physik* from 1942 the author remains ambiguous on the question when he first writes that the negative electric charge, just like matter, is composed of atoms of electricity, called electrons, while in a later chapter he states that the electron is a particle with mass. These three ways of describing the nature of the electron change from book to book, with no recognizable pattern. The fraction describing the electron as a particle is the largest, with 13 books doing so. In three books the electron is described as a quantisation of charge and six books remain unclear on that matter.

Conclusions

It should be understood that this paper forms very much a workshop report, there appears to be more potential in schoolbook analysis than the details we have been able to discuss. Yet, some generalizations appear to be possible even despite this limitation. First of all, evidently the analysis of textbooks can help to develop an understanding of how accounts of (historical) knowledge production were created. This is on the one hand relevant to historians of science as our modern understanding of historical experiments is frequently shaped initially through these textbooks – this is the first contact with historical experimentation for future historians of science. Moreover, these accounts may help to clarify to a certain extent open questions with respect to theory development. Examples such as the discussion of the electrons show that they initially were more a theoretical entity than a real object. In this respect, school science textbooks seem to have developed towards a more positivistic (and maybe simplistic) position. Yet, another interpretation appears to be possible: The characterization of electrons as hypothetical particles can be taken as an indication of this aspect being still an open question among scientists. Thus, textbook descriptions can be seen as an indication of acceptance of certain issues that had been previously discussed controversially.[2]

At the same time, such a textbook analysis can be relevant for didacticians: these accounts may help to develop an understanding of the genesis of the accounts that are to be found in today's textbooks. This is on the one hand relevant for the perspective of future teachers on materials available to them. Such an analysis may enable them to develop a more critical perspective on recent textbooks. They can realize that these accounts are not necessarily the only possible way to describe scientific facts and their development. On the other hand, particularly the problematic aspects in the accounts of the historical development may cause difficulties in the acceptance and use of the history of science in science education. Again, this appears to be relevant as this is the first source science teachers have when they intend to use historical aspects in their teaching.

And finally, such an analysis may help to identify mechanisms that lead to a positivistic tone in modern textbooks by showing the increasing decontextualisation of the texts.

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[2] Such a claim cannot be made for the eighteenth and nineteenth centuries, at least university textbooks in these periods were also used to communicate new knowledge.

References

BOBIS, L.; LEQUEUX, J. (2008): "Cassini, Rømer and the Velocity of Light" *Journal of Astronomical History and Heritage* 11(2): 97-105

CREASE, R. (2002). "Critical point—the most beautiful experiment" *Physics World* 15 (9): 19–20

FRERCKS, J. (2004). "Disziplinbildung und Vorlesungsalltag: Funktionen von Lehrbüchern der Physik um 1800 mit einem Fokus auf die Universität Jena." *Berichte zur Wissenschaftsgeschichte 27*: 27-52.

GÜNTHER, E. (1942). *Grundriß der Physik – Band 2a – Für die Klassen 6 bis 8 der höheren Schulen*. Hildesheim – Leipzig: August Lax Verlagsbuchhandlung.

HEERING, P. (2010). "False Friends: What Makes a Story Inadequate for Science Teaching?" *Interchange* 41: 323-333.

HENTSCHEL, K. (2003). "Das Märchen vom Zauberer im weißen Kittel." *Physik in unserer Zeit* 34: 225-231.

HOLTON, G. J. (1981). *Thematische Analyse der Wissenschaft: die Physik Einsteins und seiner Zeit.* Frankfurt am Main: Suhrkamp.

KNOPS, K. (1906): *Anfangsgründe der Physik*. B. T, Baedeter Verlagsbuchhandlung.

LIND, G. (1992). *Physik im Lehrbuch 1700 - 1850: Zur Geschichte der Physik und ihrer Didaktik in Deutschland*. Berlin u.a.: Springer.

MEYER, W. (1926). *Physik und Chemie – Für Mittelschulen*, Frankfurt am Main: Verlag Moritz.

NIAZ, M.; RODRÍGUEZ, M. (2005). "The Oil Drop Experiment: Do Physical Chemistry Textbooks Refer to its Controversial Nature?" *Science & Education* 14: 43-57.

PARLOW, V.; HEERING, P. (2009). "Das Millikan Experiment und seine Behandlung in der Schule." in Heering, P. (ed.), *Der Millikansche Öltröpfenversuch zur Bestimmung der Elementarbildung.* Oldenburg: diz, 35-46.

PÜNING, H. (1915). *Lehrbuch der Physik für die Klassen höherer Lehranstalten, in Anschluss an desselben Verfassers Grundzüge der Physik*, 10. Auflage. Münster i.W.: Aschendorffsche Verlagsbuchhandlung.

RODRIGUEZ, M.A.; NIAZ, M. (2004). The oil drop experiment: An illustration of scientific research methodology and its implications for physics textbooks. *Instructional Science* 32, 357-386.

SCHAUFF, P. (1928). *Grimsehl Lehrbuch der Physik – Für die Oberstufe höherer Knaben- und Mädchenschulen*. Leipzig – Berlin: B. G. Teubner.

SCHIPPENKÖTTER, J. (1931). *Physik – Für höhere Lehranstalten*. Berlin – Bonn: Ferd. Dümmlers Verlag.

SPEITKAMP, H. (1947). *Lehrbuch der Physik – Für die oberen Klassen der höheren Lehranstalten*. Münster: Aschendorffliche Verlagsbuchhandlung.

SPIES, P. (1920). *Grundriß der Experimentalphysik – Zum Gebrauch beim Unterricht auf höheren Lehranstalten und zum Selbstunterricht*. Berlin: Verlag von Winckelmann und Söhne.

SUMPF, K. (1913). *Grundriß der Physik*. Hildesheim – Leipzig: August Lax Verlagsbuchhandlung.

WALLASCH, J (1982). "Olaf Roemer und die Lichtgeschwindigkeit oder wie Schulbücher dem Physiklernen schaden". physica didactica 9, 81-102.

WEILER, W. (1905). *Elektricität und Magnetismus*. Eßlingen und München: J.J. Schreiber.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 78.

VISUAL AIDS IN *THE NINE CHAPTERS ON THE MATHEMATICAL ART*: CONNECTIONS BETWEEN GEOMETRY AND ALGEBRA IN SECONDARY SCHOOL

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Abstract

The introduction of different procedures to solve problems in the mathematics classroom fosters the connection between contents and favours the students learning because it does not limit them to a closed and finished vision of the brought up problem.
The use of historical texts in the classroom is a good resource to show this variety of procedures, that enrich the learning and fosters a wider vision of mathematics as a science in continuous evolution.
The Nine Chapters on the Mathematical Art *(1st century AD), a classical reference text in ancient Chinese mathematics, contains more than two hundred problems that deal with subjects as diverse as calculation with fractions, use of proportions, root extractions, resolution of right-angled triangles, resolution of 1st and 2nd degree equations, and resolution of equations systems.*

Introduction

In this communication, we present a sequence of activities implemented to secondary school pupils, based on the problems of chapter 9 of *The Nine Chapters on the Mathematical Art*.

These activities have been designed from the fundamental figures described by Liu Hui (263) and Li Chunfeng (656) in the commentaries of the classical text, analysed in the bilingual translation by Karine Chemla i Guo Shuchun (2005) and following the suggestions by Siu Man-Keung (2000) on their pedagogical value.

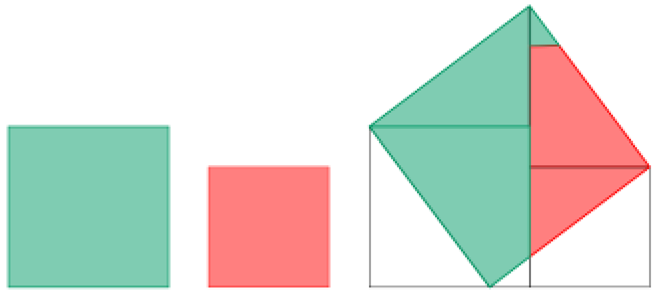


Fig. 1: Two squares and the bigger square which contains the other two.

The historical context

In 221 BC Qin Dynasty begins. Emperor Shi Huang Di, who became known as Shi Huangdi (First Emperor –literally: emperor begins) unified several walls made previously and began building the Great Wall of 5000 kilometres long.

They also built canals and bridges, which were intended to consolidate the power of the emperor, but also for public use.

Fig. 2: The Emperor, 221-210 BC.

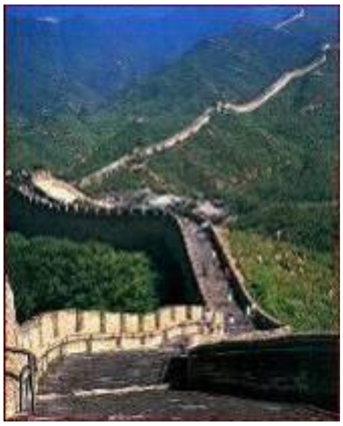


Fig. 3: The Great Wall.

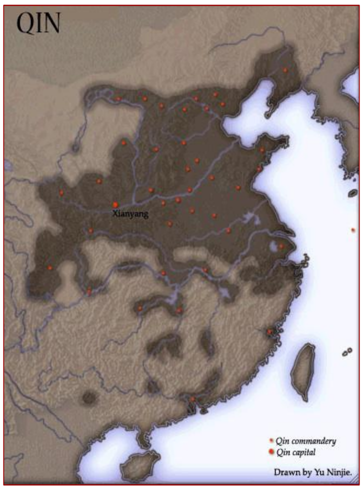


Fig. 4: China: Qin dynasty, 221-207 BC.

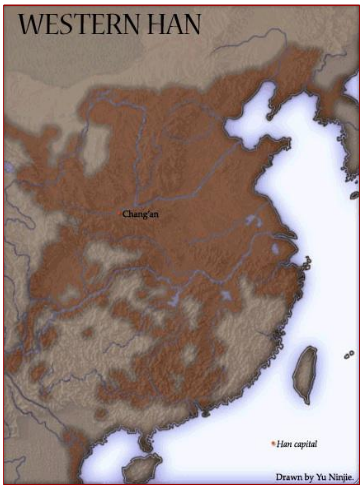


Fig. 5: China: western Han dynasty, 206 BC-220 AD.

Early Chinese texts in mathematics

The earliest Chinese texts devoted to mathematics that we know of date back from the time of the Han Dynasty (206 BC-220 AD). The unification of the Chinese Empire carried out by Qin Shi Huangdi (221 BC) and the subsequent consolidation of a centralized administration favoured the knowledge collection in different fields, and in particular those related to the field of mathematics

Suan shu shu, or *A Book on Numbers and Computations* (2nd century BC), discovered in 1984 in a tomb sealed in 186 BC, is considered the first known Chinese book strictly devoted to mathematics, related to administrative management (Qin / Han).

During the Han Dynasty, *Zhou bi suan jing*, or *Mathematical Classic of the Zhou Gnomon* (1st century BC) was written, a text related to topography, astronomy and calendar computing.

Fig. 6: Exemples of the bamboo strips on which the *Suan shu shu* is written. (Peng Hao, 2001).

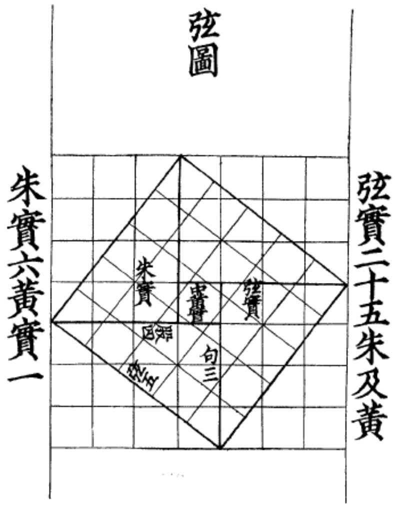


Fig. 7: Bao Huanzhi edition of *Zhou Gnomon* (1213).

The reference text

Our reference text is *Jiu zhang suan shu* (1st century AD), *The Nine Chapters on Mathematical Procedures* –from now on, it will be referred as *NC*.

It was a practical handbook for architects, engineers, surveyors and tradesmen. Candidates for civil service positions studied this text among others as a preparation for questions on the imperial examinations.

The 246 problems of the *NC* and their solutions show that the Chinese had access to a variety of formulas for determining areas and volumes of basic shapes, as well as methods for the solution of linear equations and equation systems with two and three unknowns.

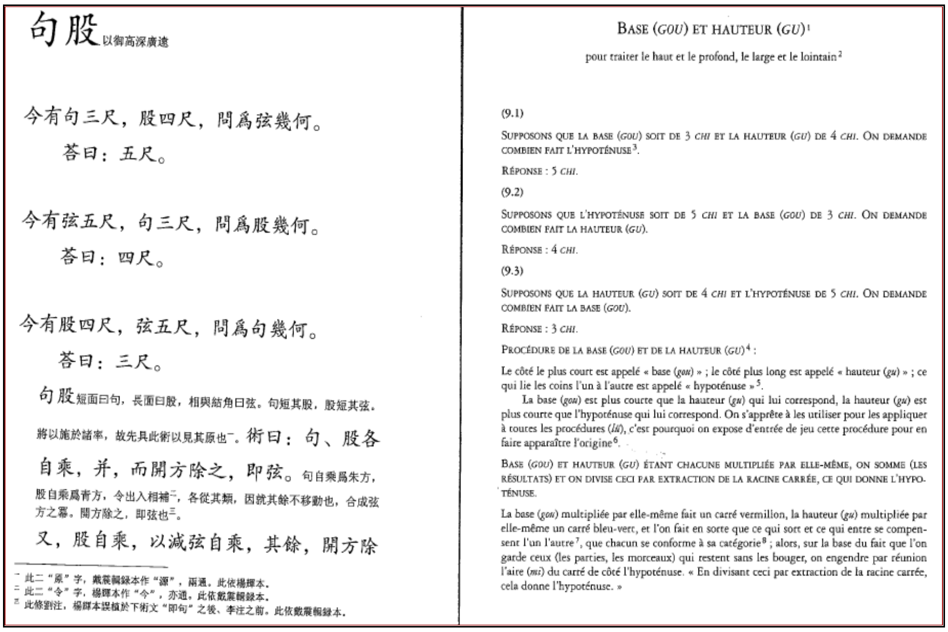
They used a decimal numeration system, including decimal fractions, and were the first known society to use negative numbers in their calculations.

The classical text and the commentators

The first witness to designate the *NC* as a classical text is the commentator Liu Hui (263). Later, it will be the commentator Li Chunfeng (656) who will continue this tradition. Their aim was to prove the correctness of the algorithms used in solving problems by making each step explicit, explaining the meaning of each operation to get the final result, and to prove that this result corresponded to the question posed. They worked with specific problems and algorithms applied to specific data, but they described the procedure very carefully so that it could be used with other numbers.

Fig. 8: From *Jiu Zhang Suan Shu*.
Version of about 1213 AD.

Fig. 9: Liu Hui, Wei, 220 - China, 280.



句股以御高深廣遠

今有句三尺，股四尺，問爲弦幾何。

荅曰：五尺。

今有弦五尺，句三尺，問爲股幾何。

荅曰：四尺。

今有股四尺，弦五尺，問爲句幾何。

荅曰：三尺。

句股短面曰句，長面曰股，相與結角曰弦。句短其股，股短其弦。將以施於諸率，故先具此術以見其原也[一]。術曰：句、股各自乘，并，而開方除之，即弦。句自乘爲朱方，股自乘爲青方，令出入相補[二]，各從其類，因就其餘不移動也，合成弦方之冪。開方除之，即弦也[三]。

又，股自乘，以減弦自乘，其餘，開方除

[一] 此二"原"字，戴震輯錄本作"源"，兩通。此依楊輝本。
[二] 此二"令"字，楊輝本作"今"，亦通。此依戴震輯錄本。
[三] 此條劉注，楊輝本誤植於下術文"即句"之後、李注之前。此依戴震輯錄本。

BASE (*GOU*) ET HAUTEUR (*GU*)[1]

pour traiter le haut et le profond, le large et le lointain[2]

(9.1)

SUPPOSONS QUE LA BASE (*GOU*) SOIT DE 3 *CHI* ET LA HAUTEUR (*GU*) DE 4 *CHI*. ON DEMANDE COMBIEN FAIT L'HYPOTÉNUSE[3].

RÉPONSE : 5 *CHI*.

(9.2)

SUPPOSONS QUE L'HYPOTÉNUSE SOIT DE 5 *CHI* ET LA BASE (*GOU*) DE 3 *CHI*. ON DEMANDE COMBIEN FAIT LA HAUTEUR (*GU*).

RÉPONSE : 4 *CHI*.

(9.3)

SUPPOSONS QUE LA HAUTEUR (*GU*) SOIT DE 4 *CHI* ET L'HYPOTÉNUSE DE 5 *CHI*. ON DEMANDE COMBIEN FAIT LA BASE (*GOU*).

RÉPONSE : 3 *CHI*.

PROCÉDURE DE LA BASE (*GOU*) ET DE LA HAUTEUR (*GU*)[4] :

Le côté le plus court est appelé « base (*gou*) » ; le côté plus long est appelé « hauteur (*gu*) » ; ce qui lie les coins l'un à l'autre est appelé « hypoténuse »[5].

La base (*gou*) est plus courte que la hauteur (*gu*) qui lui correspond, la hauteur (*gu*) est plus courte que l'hypoténuse qui lui correspond. On s'apprête à les utiliser pour les appliquer à toutes les procédures (*lü*), c'est pourquoi on expose d'entrée de jeu cette procédure pour en faire apparaître l'origine[6].

BASE (*GOU*) ET HAUTEUR (*GU*) ÉTANT CHACUNE MULTIPLIÉE PAR ELLE-MÊME, ON SOMME (LES RÉSULTATS) ET ON DIVISE CECI PAR EXTRACTION DE LA RACINE CARRÉE, CE QUI DONNE L'HYPOTÉNUSE.

La base (*gou*) multipliée par elle-même fait un carré vermillon, la hauteur (*gu*) multipliée par elle-même un carré bleu-vert, et l'on fait en sorte que ce qui sort et ce qui entre se compensent l'un l'autre[7], que chacun se conforme à sa catégorie[8] ; alors, sur la base du fait que l'on garde ceux (les parties, les morceaux) qui restent sans les bouger, on engendre par réunion l'aire (*mi*) du carré de côté l'hypoténuse. « En divisant ceci par extraction de la racine carrée, cela donne l'hypoténuse. »

Fig. 10: Bilingual translation by Karine Chemla and Guo Shuchun (2005).

***The commentators and the* visual aids**

The first mentions in Chinese texts on ***visual aids*** are due to commentators. They consist in figures and objects introduced to justify calculation algorithms in the classical text.

As ancient texts carry with them the counting board, probably they also had some visual aids, i.e. flat figures to calculate areas and wood blocks for the volumes of geometrical bodies. From the 11th century on, texts include drawings.

Several authors like Christopher Cullen (1996), Karine Chemla (2005) and Joseph W. Dauben (2007) have called these figures ***visual aids***.

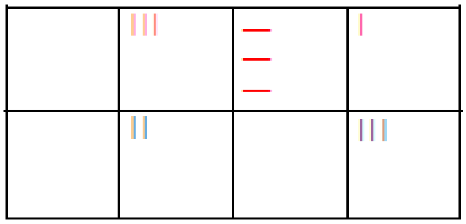


Fig. 11: The counting board.

***Description of* Nine Chapters**

The text in *NC*, as its name indicates, is structured in nine chapters.

- Chapter 1, *Fangtian* (Field measurement), discusses:
  1. how to calculate areas of cultivated land.
  2. how to do computations with fractions.
- Chapter 2, *Su mi* (Cereals), focuses on proportion problems, particularly those related to cereals exchange.
- Chapter 3, *Cui fen* (Distribution by proportion). It discusses problems related to proportional distribution, arithmetical and geometrical progressions.
- Chapter 4, *Shao guang* (What width?), deals with the calculation of side and length, when given an area or volume, as well as the procedures for determining square roots and cube roots.
- Chapter 5, *Shang gong* (Construction consultations). It discusses various kinds of calculations for constructions, in particular, calculations involving volumes of various solid shapes.
- Chapter 6, entitled *Jun shu* (Fair taxes), considers how to distribute grain and labour corves the best way based on population and distance between places.
- Chapter 7, *Ying bu zu* (Excess and deficiency), teaches a method of false position to solve certain types of systems involving two equations and two unknowns.
- Chapter 8 is entitled *Fang cheng* (Rectangular Arrays). It discusses solution methods for systems of simultaneous linear equations. Since negative numbers may arise through the use of this method, it also deals with the procedures for adding and subtracting positive and negative numbers.
- Chapter 9, *Gou gu*, deals with the Gougu theorem (Pythagorean theorem) and its use in solving right-angled triangles.

The text: base (*gou*) and height (*gu*) procedure

Chapter 9: Base* (gou) *and height* (gu)*: Solving the height and depth, width and length

24 problems on right triangles:

- Problems 1-12: procedure for base and height.
- Problems 13-24: ratio (*lü*), similarity between triangles.

The first problems in each block are geometric, with no context. Following them we find situations in context and the initial geometric situation appears.

The text: base* (gou) *and height* (gu) *procedure

In the classical text (1st century):

Base (*gou*) and height (*gu*), if each is multiplied by itself, joined (results) and divided by the square root extraction, the result is the hypotenuse.

Liu Hui (263) explains the vocabulary: The shorter side is called the base (*gou*), and the the longer side is the height (*gu*). The side opposite to the right angle is called the hypotenuse (*xian*). The *gou* is shorter than the *gu.* The *gou* is shorter than the hypotenuse.

Liu Hui (263) also explains the procedure: Let the square on the *gou* be red in color, the square on the *gu* be blue. Let the deficit and excess parts be mutually substituted into corresponding positions, the other parts remain unchanged. They are combined to form the square on the hypotenuse. Extract the square root to obtain the hypotenuse.

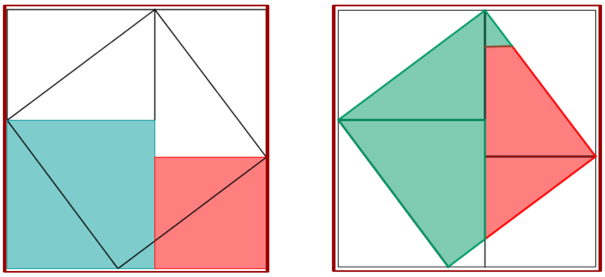


Fig. 12: Base and height procedure with diagrams.

The activities: Pythagoras' Theorem or *Gou gu* cutting and pasting

The area of the square on side ***c*** is the sum of the areas of the squares on the sides ***a*** i ***b***.

1. Construction of a square of side* c *from two squares of sides* a *and* b *respectively

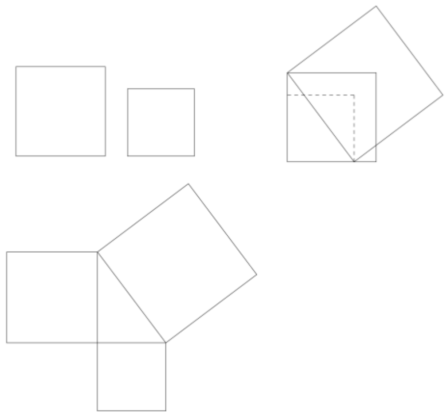


Fig. 13: Base and height procedure in modern notation.

Fig. 14: Construction paper.

2. The largest area of the square is the sum of the areas of two other squares

The proof –cutting and tracing figures or building the colour puzzle, cutting and pasting.

Fig.15: The puzzle of colours

3. Problem 9.5

術曰：以七周乘圍爲股[七]，木長爲句，爲之求弦。弦者，葛之長。

Suppose we have a tree 2 *zhang*[1] high and a perimeter of 3 *chi*. A climbing plant that grows from its base surrounds the tree seven times before reaching the top. The question is: how much is the length of the climbing plant? Answer: 2 *zhang* 9 *chi*, corresponding to shortlist (20, 21, 29)

Some help to solve the problem:

a) Roll up a sheet of paper forming a cylinder, simulating the trunk of the tree.

b) Draw the climbing plant around it.

c) Expand the sheet.

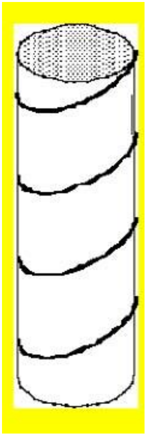


Fig. 16: The rolled paper and the climbing plant.

The activities: Construction of three key figures

1. The first key figure

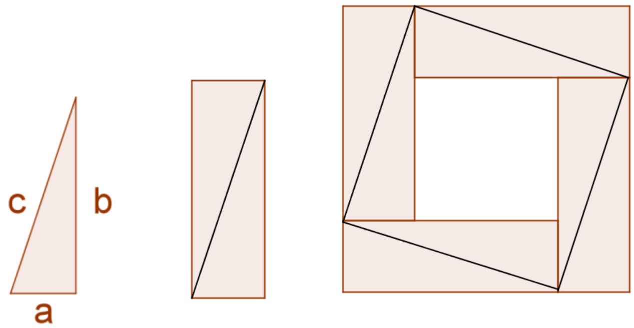


Fig. 17: Construction of the 1st figure.

It is the same initial square that we use to prove the *Gou gu* procedure. Fig. 19 shows the measurements of the square of side *a*+*b*.

[1] *zhang*, *chi*, *cun* are decimal unities, i.e. 1 *zhang* = 10 *chi*, etc.

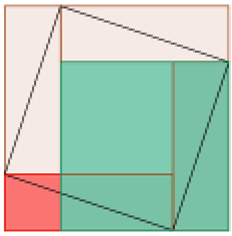


Fig.18: The 1st key figure with the initial two squares inside.

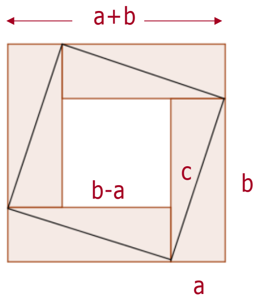


Fig.19: Relations inside the square.

2. The second key figure

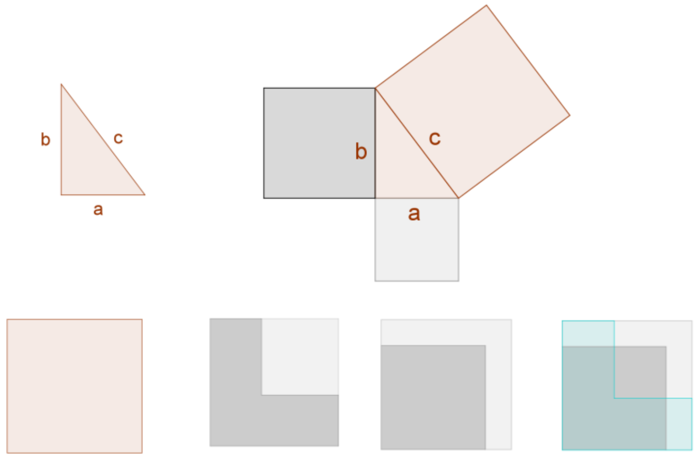


Fig. 20: Construction of the 2nd figure.

Measurements in the square of side *c*: the area of a square becomes the area of a gnomon and a rectangle of base *c*–*b* and height *c*+*b*.

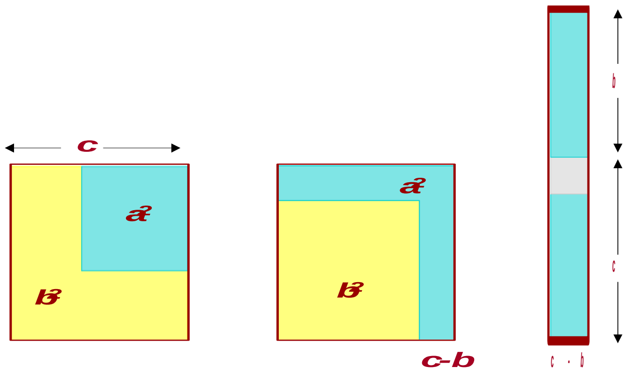


Fig. 21: The two gnomons and the rectangle of area b^2

3. The third key figure

Building a square of side $a+c$, and adding a rectangle of area b^2:

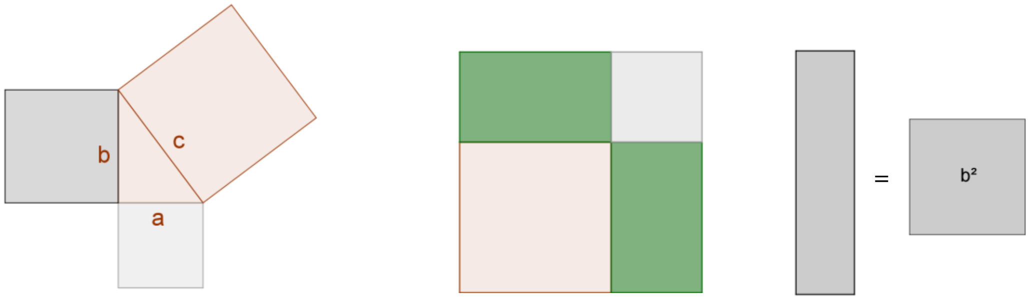


Fig. 22: Construction f the 3[rd] figure.

Measurements in the square, once added the rectangle of area b^2:

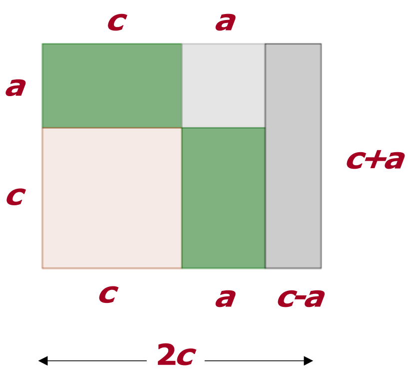


Fig. 23: Measurements.

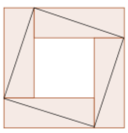
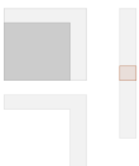
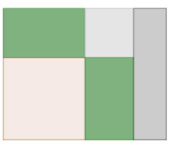
name and picture	description	relationships
1st 	square of side a+b mark a in each side alternately.	c, b-a -> a+b -> a, b
2n 	square of side c within a square of side b, the gnomon has area a^2.	a, c-b -> c+b -> b, c
3rd 	square of side c + a add a rectangle of area b^2. 2c is the side of the new rectangle.	b, c+a -> c

Fig. 24: Summary of the three key figures.

The activities: Some problems solved with the diagrams

1. Problem 6

The classical text:

> Suppose we have a square pond of 1 *zhang* side in the center of which there is a rod protruding one *chi* from water level. When you pull the rod toward the bank, it comes just at the tip. How deep is the lake, and how long is the rod?

With visual aids, using the second figure: a=5, c–b=1, b=?, c=?

Answer: b=12, c=13

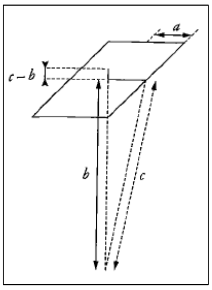


Fig. 25: Problem analysis, Karine Chemla, 2005: p. 667.

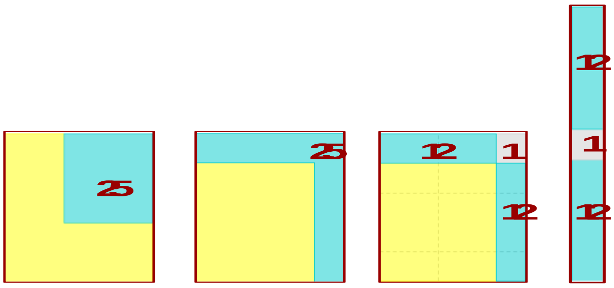


Fig. 26: Problem resolution through diagrams.

Some possible resolutions in analytical form:

- $a^2 + b^2 = c^2 \rightarrow a^2 = c^2 - b^2 = (c + b)(c - b) \rightarrow 5^2 = (c + b)\cdot 1 \rightarrow 25 = (c+b)$
- $c - b = 1 \rightarrow c = b + 1$; $a^2 + b^2 = c^2 \rightarrow 25 + b^2 = (b + 1)^2 = b^2 + 2b + 1 \rightarrow 25 - 1 = 2b$ $\rightarrow b = 12 \rightarrow c = 13$

2. Problem 11

The classical text:

Suppose we have a single leaf door where the height exceeds the width in 6 *chi* 8 *cun*, and where the distance between the two opposite angles is 1 *zhang*. How wide and how high is the door?

Answer: 2 *chi* 8 *cun* (width) and 9 *chi* 6 *cun* (height).

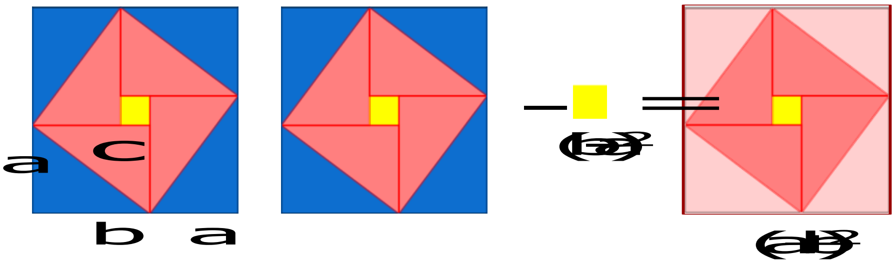


Fig. 27: Problem resolution through diagrams.

$b - a = 68$; $c = 100 \rightarrow c^2 = 10000 \rightarrow 2c^2 = 20000$

$(a + b)^2 = 2c^2 - (b - a)^2 = 20000 - 68^2 = 15376 \rightarrow a + b = \sqrt{15376} = 124$

$a = 28$; $b = 96$

Concluding remarks

The *Nine Chapters on Mathematical Procedures* is a classical text in Ancient Chinese Mathematics. This book, as several authors have pointed out (Man-Keung, 2000; Swetz, 1994), is still quite unknown in western mathematics education, but it contains a type of problems and solving approach that can result in a very prolific source of activities aimed at teaching maths.

The activities presented here are built from demonstrations and problems discussed in chapter nine of the text. It contains diagrams which are absent in the classic text but were incorporated by later commentators like Liu Hui and Li Chunfeng. These diagrams or key figures, as they are called by later historians, enable visual reasoning through geometry where this would appear to require more elaborate algebraic calculations.

It is a happy idea to use it in our classes when the algebraic language is still incipient in our pupils and students. Arguments used are based on the procedure of cutting and pasting and area conservation. The use of key figures (first, second and third) allow for a demonstration mode that combines numerical and geometrical reasoning. The application of this resource in teaching activities expands the students' possibilities of understanding because it opens two alternative, and at the same time complementary, ways of proof, both numerical and geometrical.

The examples presented were implemented in the classroom during the academic year 2009-10 at INS Badalona VII, in Badalona, Spain.

This communication is part of a broader research project entitled "The use of historical contexts in secondary mathematics classroom: The case of visualization of the geometry-algebra connection" in the form of a doctoral thesis in the Department of Didactics of Experimental Sciences and Mathematic of the University of Barcelona. In this work line during the academic year 2010-11, we have been discussing these and other activities undertaken by students during the academic year 2009-10. We have established a number of categories and studied the different processes that can be observed in problem resolution by students.

References

CHEMLA, Karine (s/d) "Aperçu sur l'histoire des mathématiques en Chine Ancienne dans le contexte d'une histoire internationale", 71-90. Article on line: http://www.reunion.iufm.fr/dep/mathematiques/Seminaires/Resources/Chemla22.pdf

CHEMLA, Karine; SHUCHUN, Guo (eds.) (2005) *Les Neuf Chapitres, le classique mathématique de la Chine ancienne et ses commentaires* [bilingual critical edition], París, Dunod.

CULLEN, Christopher (1996) *Astronomy and Mathematics in Ancient China: The Zhou bi suan jing*, Cambridge/New York, Cambridge University Press.

DAUBEN, Joseph.W (2007) "Chinese Mathematics" in *The Mathematics of Egypt, Mesopotamia, China, India and Islam. A sourcebook*. Victor J. Katz, Editor. Princeton University Press. 187-384.

JAMI, Catherine (1988) "Une histoire chinoise du «nombre π»", *Archive for History of Exact Sciences*, vol. 38, 1, 39-50.

HO PENG YOKE (2000) *LI, QI and SHU An Introduction to Science and Civilization in China*. Mineola, New York, Dover Publications, Inc.

LI YAN; DU SHIRAN (1987) *Chinese Mathematics. A Concise History*, Oxford, Clarendon Press.

LLOYD, Geoffrey E.R. (2001) *Explorant la ciència antiga*, Barcelona, Societat Catalana d'Història de la Ciència i de la Tècnica.

MARTZLOFF, Jean-Claude (1997) *A history of Chinese mathematics*, Berlin/Heidelberg/New York, Springer-Verlag.

MAN-KEUNG, Siu (2000) "An Excursion in Ancien Chinese Mathematics" in *Using History to Teach Mathematics. An International Perspective*. Katz, Victor J.(ed.) Washington.The Mathematical Association of America, 159-166.

STRAFFIN, Philip D. (1998) "Liu Hui and the First Golden Age of Chinese Mathematics", *Mathematics Magazine* 71, 163-181.

SWETZ, Frank (1979) "The evolution of Mathematics in Ancient China", *Mathematics Magazine* 52, 10-19.

SWETZ, Franz (1997) “Enigmas of Chinese Mathematics” in *Vita Mathematica*. Cambridge University Press. pp. 87-97.

VOLKOV, Alexei (1994) “Transformations of Geometrical objects in Chinese Mathematics and their evolution” in Alleton, Viviane; Volkov, Alexei (eds.) *Notions et perceptions du changement en Chine*, París, Collège de France / Institut des Hautes Études Chinoises, vol. XXXVI, 133-148.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 90.

THE RECEPTION OF MATHEMATICAL KNOWLEDGE THROUGH EUROPEAN TEXTBOOKS BY THE GREEK INTELLECTUAL COMMUNITY IN THE 18TH CENTURY

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Abstract

The subject of this paper is the transmission of European mathematical knowledge to Greece in the 18th and early 19th century, i.e. during the final decades before the Greek Revolution, mainly through mathematics textbooks. This event is illustrative of the dissemination of scientific information in the wider area of Europe.
In an age (the pre-revolutionary period), when there was an endeavour to advance Greek education and stimulate spiritual awakening, Greek scholars realised that the most important thing was not only or even mainly the production of original work, but the transmission of knowledge, in the form of translations or compilations, from Europe, where scientific and philosophical knowledge had long been developed to a high degree. And this was exactly what they did.
Of the 28 mathematics textbooks which circulated in printed form during the 18th and the first decades of the 19th century, 11 are translations of Western European works, such as those of Tacquet, Metzburg, Euler and others, while the rest are compilations, as is expressly stated in the title or preface of most. Apart from these, however, several other works which remained in manuscript form are also the result of compilation or translation efforts.

To begin, I would like to note that all the people mentioned in this paper are among the most important scholars of the Greek nation under Ottoman rule in the 18th and early 19th centuries.

The fall of the Byzantine Empire was followed by about two and a half centuries whose main feature was sterile scholasticism. Sciences became a marginal part of the curriculum[1], as Ottoman rule discouraged the diffusion of

[1] For more details, see M. Terdimou 2006.

contemporary theory and the Patriarchate remained staunchly faithful to theology and grammar as the only appropriate sources of education. During these centuries, Greek mathematical education was ensured by Byzantine reckoning manuals, Emmanuel Glyzonios's[2] *Arithmetic* (20 editions from 1568 [Venice] to 1823), and simple mathematical textbooks. Twelve anonymous Arithmetic manuscripts have been recorded up to and including the 17th century, with twelve anonymous manuscripts containing elements of Euclidean Geometry and two with excerpts from works by Euclid, Proclus, Psellos and Nicomachos Gerasinos (Karas 1992).

Of course, this situation arose from and was sustained by the particular circumstances of Hellenism, circumstances which not only did not stimulate interest in modern scientific thinking but even caused the isolation of the Greek nation from Europe. Thus, at the beginning of the 18th century, with rare exceptions (Sougdouris, Papavassileiou, Notaras), scholars had almost no scientific education.

In the late 17th century the Ottoman Empire, in its attempt to adjust to the new political climate developing in Europe, awarded privileges to the enslaved Greeks, who were eventually able to control trade and diplomacy to a great extent, especially following the Treaty of Küçük Kaynarca (1774), which contributed to the development of trade. The creation of the commercial class, the development of the Phanariot class and their rise to the rule of the Danube Hegemonies, resulted in the creation of novel views and a new intellectual awareness among the Greeks. More and more social classes realised that, from then on, society would improve only through knowledge arising from a systematic education and remediation of the sciences, and not through an empirical relationship with matters, which was no longer considered sufficient. The number of scholars studying in the West increased substantially.

During the first decades of the 18th century, the main places of study were the Italian universities, especially those of Padua and Pisa. During the last decades prior to the Greek Revolution of 1821, Greek scholars showed a preference for French and German or German-speaking universities (Vienna, Halle, Göttingen), particularly Vienna, where there was a significant Greek community and which was to become the most important publishing centre for Greek textbooks in the 18th century. It should be noted that during this period, there was a prevalent view that Germany boasted "first-class physicists and mathematicians", and that nowhere in the world were the natural sciences so solidly taught as at German universities (Karas 1993, pp 37-39).

After completing their studies, many scholars returned home, bringing with them the new scientific knowledge with which they had come in contact with. In other words, the return of sciences to the Greek intellectual world was a transference of knowledge from Europe, where the major scientific achievements of the 16th and 17th centuries had laid the foundations of modern mathematics and natural sciences. The channels of scientific thought traversing the Old World were gradually reaching the Greeks, in a typical example of the transmission of scientific knowledge in the wider European area.

From the first decades of the 18th century, efforts began for the foundation of schools and libraries, and the translation or writing and publication of mathematics textbooks to serve primarily educational needs.

Education was now organised, as it had not been earlier, and the rising commercial class was the most important social body for the cultivation of letters. Educators were no longer sought after in ecclesiastical circles alone, and the proposed education system began to specialise.

The number of mathematical works published, particularly from the latter half of the 18th century onwards, is impressive, especially compared to that of previous centuries. Approximately 30 titles were published, not counting commercial literature and equivalence tables. These are works of Arithmetic, Geometry and Algebra; some contain both Arithmetic and Algebra, while some even include Calculus.

On closer examination of these books, it is immediately apparent that 11 are translations of Western European texts, while the rest are compilations, as expressly stated in the title or preface of most of them[3]. Apart from these, however, several other works which remained in manuscript form are also the result of compilation or translation.

This observation, of course, is neither surprising nor impressive, but only to be expected: at a time when efforts were being made to improve education and awaken the intellect, scholars realised that what was most important was not only, or even mainly, the production of original works, but the transmission of knowledge, in the form of compilations or translations, from Europe, where scientific and philosophical knowledge were already developed. They knew that modern European thought, combined with their ancestral heritage, could reform the intellectual climate that had held sway over the enslaved Greek nation for centuries. They also knew that education is the strongest means of acquiring a national conscience, and therefore the best motivating force for people trying to gain their freedom. The following fragment from the Greek review *Kalliopi* (Vienna, 1819-1821) is characteristic of this mentality: *We steal ideas from philosophers and clothes from the erudite*

---

[2] Emmanuel Glyzonios or Glyzounis (1530-1596), from the island of Chios, studied medicine and letters in Italy. His *Arithmetic* was the master-key which covered the public's daily needs.

[3] Most contemporary works are "paraphrases, epitomes, compilations or simply translations of Western European works, from every branch of theoretical and scientific knowledge": Henderson (1977), p.17.

*to dress our nation. For this reason we came to Europe ... to steal from the original sources, from the best sources of what we do not have (*Karas 2003, p. 653).

The aim of translation is common to all: to benefit the Greek Race: "to benefit the Race I translated this book from the German into our own language, without taking the effort into account"(Kavras 1800, p. vii), says Zisis Kavras, while Psalidas states: "I wanted to translate the book "Mathematicae" of the Professor of Mathematics, seeing the use and benefit of mathematics to the public..." (Psalidas 1794, "To Anaginoskonti"), words used to justify their attempts to translate mathematical works. Koumas even promotes Psalidas's translation, writing, "But Psalidas by the *Mathematical Elements* of Metzburg, which he translated and taught, greatly benefited his countrymen"(Koumas 1832, p. 575).

In our opinion, this does not diminish the scholars' contribution and work in the slightest. The choice of subject for translation, if not made by chance, presupposes knowledge, consideration and a particular familiarity with the subject. The selection criteria, however, differ from person to person and period to period.

One of the first mathematical textbooks to be taught in contemporary schools, the *Introduction to Mathematics* (1695) by Papa-Vassilios, is a translation "from the tongue of the Latins". The same is true of the first printed mathematical book, published in 1749, the *Mathematical Way* by Anthracites, who had studied in Italy. Unfortunately neither of the original texts translated has yet been identified. Nor do we know the original of Glyzonios' s *Logariastike* (1568), the work reprinted most often for almost three centuries, although it is almost certainly some practical Italian Arithmetic.

The term Geometry at this time mainly referred to Euclidean geometry. A work of Geometry widely used by scholars was that of Andrea Tacquet (Netherlands, 1612-1660)[4], *Elementa geometriae planae et solidae, et selecta ex Archimede theoremata* (Antwerp 1654), a work essentially constructed from Euclid's *Elements* with material from Archimedes. It was translated into Greek, English and Italian (Gillispie vol. XIII, p. 235.). Many editions of *Elementa geometriae* were produced over the next 150 years: (1665, 1683, 1701 Antwep), (1761, Padova) and the revised editions by William Whiston (1703, Cambrige), (1725, Antwerp), (1727, London (English version)), (1745, Rome), (1762, Venice).

It was first translated into Greek by Eugenios Voulgaris (*Elements of Geometry*) in 1753-59 (Koumas 1832, p. 562), but did not circulate in printed form until 1805. Voulgaris used the Cambrige's latin edition revised by the protestant William Whiston, of 1703.

Voulgaris makes several interventions to Tacquet's text, both criticising Tacquet for omitting sentences from the *Elements*, and interposing many more proofs than the Euclidean ones in the original.

Approximately ten geometry manuscripts by Voulgaris have been recorded to date, all drawn from lessons he gave at the schools where he taught.

Iosipos Moisiodax also translated the same text, but his version remained in manuscript form (Moisiodax 1780, p. 43), under the title "Paraphrase and variation of Andrea Tacqet's Elementa…"[5].

N. Theotokis used Tacquet's textbook to write the first volume (Geometry-Arithmetic) of his three-volume *Elements of Mathematics* (1798). As for Benjamin of Lesbos, it is notable that Tacquet is the only European mathematician mentioned in the whole of his Geometry (Benjamin of Lesbos 1820, p. 59). From the study of the textbooks it also emerges that Benjamin was greatly influenced by Tacquet, probably via Voulgaris' translation.

The question that arises is why this particular text was so favourably received by Greek scholars. Iosipos Moisiodax justifies his decision to translate the textbook as follows: "I once translated the *Elements* of Andrea Tacquet and even his Trigonometry, both because that great man precisely follows Euclid, and because he is in this more accurate than any other of the moderns" (Moisiodax 1780, p. 43).

From another reliable source, European this time, we are informed that "this book of Tacquet' s was the most popular of all his works, and although it was no more than a paraphrase of works by Euclid and Archimedes, it was distinguished by the distinctness and clarity of the text. All his writings, in any case, played an important role in teaching, for whole generations of students were taught basic Mathematics from them" (Gillispie vol. XIII, pp. 235-236). This shows that the scholars mentioned did not choose this particular work by chance.

Another French mathematician whose mathematical textbook was translated into Greek, and moreover was the first Algebraic printed text, was Nicolas-Louis de Lacaille (1713-1762), professor of Mathematics at the Collège Mazarin, whose main work in Mathematics was *Leçons élémentaires de Mathématiques* (1741)[6].

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[4] Andrea Tacquet (1612-1660) studied mathematics, logic, physics and theology at Louvain. He wrote many good elementary texts designed as mathematics textbooks for Jesuit colleges. *Elementa geometriae* (1654) was his most popular teaching work.

[5] Library of Roman Academy, Bucurest, MS 1513.

Thirty five years after the latin edition of 1762 (Vienna) Spyridon Asanis and Ionas Sparmiotes translated the first part of the work (pp. 1-133). It was published in Venice in 1797, under the title *Elements of Arithmetic and Algebra.* The translators possibly used this latin edition of the work.

Regarding the above translation, Koumas notes that "Asanis enhanced it with much useful information on analysis and thus the first Algebraic text appeared through the medium of the press to the Race" (Koumas.1832, p. 575). In other words, this book is the first text purely concerned with algebra to circulate in Greek. From 1797 onwards, the appearance of this first complete algebraic textbook in Modern Greek education contributed, among other things, to the gradual creation of a dynamic in the area of algebraic education, something reflected in the burst of publishing in the field of Algebra from this point on.

The second part of Lacaille's work translated by Asanis and comprising Geometry, remained in manuscript form[7]. The third part was translated by Asanis and Koumas, and printed in Venice in 1803 under the title *Analytical Treatise of Conic Sections*. The translators also used the latin edition (1762) of the work.

Moisiodax translated this textbook as well (Moisiodax 1780 p. 44-45) but his translation has not survived. He describes the book as follows: "The most illustrious de Lacaille in two minuscule volumes discusses everything to do with either analytical or synthetic Mathematics, beginning as an introduction with Arithmetic...." (Moisiodax 1780, p. 44-45). Thus, Moisiodax regards it as a work suitable for teaching, something confirmed by the large number of reprints in the West. Moisiodax used Lacaille's text in teaching Mathematics at the School of Iasio.

Another favourable opinion of the book is contained in an 1818 issue of *Logios Hermes*, where it is described as follows: "This book, albeit in a single volume, contains the most essential and basic elements of almost all pure mathematics..."[8].

The work of Leonard Euler (1707-1783) could certainly not be absent from the translational interests of Greek scholars. An extract from the work *Eléments d'Algèbre, par M. Leonard Euler traduits de l'allemand…* (Lyon, 1774), a French translation of the *Vollstandige Anleitung zur Algebra* (St Petersburg 1770), was translated by Ionas Spermiotes[9].

According to information from *Logios Hermes*[10], the original of the textbook translated from the German by Zisis Kavras, the *Elements of Arithmetic and Algebra* (Jena 1800) was also by Euler. We believe that this really is a translation of the above work, which, according to Dirk Struik, "was the template for many later Algebra texts" (Struik p. 198) We do not know why Kavras' name is not mentioned either in the preface or on the title page; there is just the note "Translated from the German by a fellow countryman who loves his race, for his fellow countrymen". The absence of Euler's name is no surprise, as contemporary authors did not feel it necessary to acknowledge their sources in any form. Stefanos Dougas also recommends this text by Kavras as the most suitable for beginners.

"Asanis decided to write a textbook on differential and integral calculus, and he translated this great and erudite text by Euler" (Gedeon 1976, p. 181). This mention by Manuel Gedeon is the only information we have on the translation in question, which, if it was ever written, has not yet been found.

Athanasios Psalidas and Michael Christaris chose to translate a textbook by G. I. Metzburg. "I wanted to translate the book 'Mathematicae' by the Professor of Mathematics, who taught here at the Academy [of Vienna] for more than twenty years, because this treatise is intelligible, methodical and succinct..." (Psalidas 1794, pp. xiv, xv.), stressed Psalidas in the preface to his *Arithmetic*, the first volume of his work (Vienna 1794). It contains many alterations to the original text, as he says himself. The original, entitled *Gl. Georg Ignat L.B. de Metzburg, Mathematicae in unsum Tironum Conscriptae. Tom. I. ed. IV*, Vienna 1793, was the Latin translation from the German. This transmission of mathematical knowledge was rooted in the learning environment offered in Vienna, where Psalidas had studied from 1787 to 1795.

The second volume was to remain in manuscript form (Karas 1992 p.153). A full translation of the work was presented by Michael Christaris, entitled *Elements of Arithmetic and Algebra* (Padua 1804). The translator preferred the German edition of Metzburg's textbook, *Des Freyhernn von Metzburg... Arithmetik und Algebra* (Vienna 1788). Christaris does not give a specific reason for choosing this particular work. His choice was probably not based on his own informed opinion of the book, as he was a doctor rather than a mathematician. We should not overlook the fact that Metzburg was roughly contemporary with Psalidas and Christaris, and that Greek scholars generally tended to translate new texts

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6 Nicolas-Louis de Lacaille (1713-1762) mathematician and astronomer. His mathematical work *Leçons élémentaires de Mathématiques* (1741) was frequently reprinted (1764, 1768, 1770 and 1778), revised by Abbé Joseph-François Marie and translated into Latin, Spanish, Italian and English. Nielsen (1935), p. 221.

7 Library of Milies, MS 6.

8 *Logios Hermes*, 15 August 1818, pp. 420-424.

9 Olympiotissa Library, MS 43.

10 *Logios Hermes*, 20 October 1811, p. 355.

appearing in Europe for the first time. The ultimate aim, as we have said, was the transference of modern knowledge. This aside, however, Metzburg taught at the Academy of Vienna for 24 years (Psalidas 1794, p. iv), meaning that his work had already been tried and tested as a teaching textbook.

Christian Wolff (1679-1754) is better known as a philosopher than a mathematician. The Latin translation of his mathematical work (Halle 1717) (Gillispie vol XIV, p. 483) *Elementa matheseos universae* (Halle 1730), comprising two volumes, *Elementa arithmeticae and Elementa geometricae*, was quite widely used by Greek scholars.

Nikolaos Zerzoulis translated the whole of Wolff's textbook, as his surviving manuscripts attest. Wolff's work also certainly aided Theotokis in writing his *Arithmetic*.

Dimitrios Govdelas makes several references to Wolff in his book on algebra (Govdelas 1806, pp. 8, 422, 441, 510), and drew on the German scientist's work for his own textbooks (Camariano-Cioran 1974, p. 232).

The work *Cursus mathematicus* (Halle 1758-1767), by Segner (1704-1777)[11] was also chosen by Voulgaris as a suitable subject for translation, which was printed in 1767. Voulgaris justifies his choice in the preface, saying that "The German Academies, which are among the most celebrated in Europe, approved the Man's works as absolutely accurate".

The *Geometry* of Octavian Camett was translated by Dimitrios Razis because, as he says, "Camett set the Euclidean elements in a new order and applied a more comprehensible method" (Razis 1787, p. 12) and was published in Venice (1787).

Spyridon Asanis and Ionas Sparmiotes chose to translate Grandi' s (1671-1742)[12] *Conic Sections* (Florence 1750) in order to reinstate ancient mathematical learning, as always in the service of the Greek Race. The original text is the *Sectionum Conicarum Synopsis clar. viri D.Guidonis Grandi et Schenatibus aucta Ad. Octaviano Cametti*.

The work *Les éléments d' Euclide de M. Ozanam... démontrés d'une manière nouvelle et facile par M. Audriene* by Jacques Ozanam (1640-1717) was used, among others, by Nikiforos Theotokis in his *Geometry*. Ozanam was only a minor mathematician, but he wrote books many of which became very popular and were often reprinted (Gillispie vol. X, pp. 263-265).

In his chapter on "Conic Sections", Theotokis drew on Josepho Orlando's *Sectionum conicorum tractatus* (Naples 1744) (Karas 1992 p. 94).

In a manuscript by the same scholar entitled "Elements of Arithmetic and Geographic Science...", one section consists of "The Interpretation of the Logarithmic book by Gafendi, translated from the German into the Greek tongue" (Karas 1992, p. 96), which, however, is not included in the printed edition.

Alexis Clairaut was one of the French mathematicians linked to the philosophers of the Enlightenment. An extract from his work *Eléments de la Géométrie. De la mesure des figures et des solides* was used by Ioannis Phournaios in his Geometry, as he notes in one of his manuscripts: "Additions to Clairaut's Geometry. On the transformation of shapes"[13].

When writing a Trigonometry textbook, Phournaios also used the *Cursus Mathematicus…* by Gaspar Schott, as he mentions in a manuscript[14].

Koumas informs us that his eight-volume work, including Mathematics and Physics, is a translation from the French of a work by Jean-Claude Fontaine (1715-1807), professor of Physical and Mathematical Sciences at Turin University. This compendium had been recommended to him by Remigius Döttler (1748-1812), professor of Mathematics at the University of Vienna (Koumas 1832 p. 588). The work is Fontaine's textbook *Cours Encyclopédique et élémentaire de Mathématique et de Fysique* (Vienna 1800). During the course of translation, Koumas was unsatisfied with the result and carried out alterations to the material, using other, mainly French texts by modern authors Abbé Saury, E. Bezout and S.F. Lacroix (Koumas 1807 p. χβ.).

Koumas's *Synopsis of the Sciences for Beginners* is also a translation from German, as he says in the preface: "Second I placed the Geometry, having translated this also more freely than the Arithmetic, from the book written in German.entitled *Introduction to Geometry* (*Anleitung zur Messkunst*) used in the schools of the Austrian Empire" (Koumas 1819). Clearly, the original was a simple teaching textbook or books, but the author is not mentioned.

In a textbook on Spherical Geometry, Christoforos Koutaleus states that, "I copied sentences 46, 73, 75, 77, 89, 103, 114 from Korganov's geometry"[15]. Korganov was professor of Mathematics at a Russian school in St Petersburg.

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[11] Segner was Professor of Mathematics and Physics at Göttingen University (1735-1755). and later at Halle University. Cf. Gillispie, ibid, vol. XII, p. 283.

[12] Guido Grandi, Fellow of the Royal Society and Professor of Mathematics at the University of Pisa. Cf.. Gillispie, ibid., vol V, pp. 498-500.

[13] Romanian Academy Library, Bucharest, MS 740.

[14] Ibid, MS 1369.

Konstantinos Doukas, in the "Prelude" to his textbook *Practical Arithmetic* (Vienna 1820), expressly states that his work is a compendium of the three-volume work on arithmetic by S. Guntz, professor of Mathematics and headmaster of a school in Prague, with his own additions (Doukas 1820, "Proimnion", p. 1).

We also have some information on translations of works which remained unpublished or have been lost along with the corresponding manuscripts. These include translations of works by Jean-Batiste Biot, A. M. Legendre, S. F. Lacroix, Blaud, Hotzapfeln and others.

It should be noted here that, if we list the European authors whose works were either translated (whether published or unpublished) or used in compilations by Greek scholars, particularly during the last decades before the Greek Revolution and the two immediately following (Legendre, Lacroix, Mauduit, Biot, Bezout, Abbé Saury, Francoeur, Fontaine, Clairaut, De Lacaille, Camett, Ozanam, Euler, Metzburg, Segner, Wolff, Grandi, etc.), we see that most of them are French and German scientists. This, of course, is due to the fact that Greeks at the time studied mostly at French and German-speaking universities, where they would have come into direct contact with the works of the scientists mentioned above. It should also be added that most of the publications were printed in Vienna.

To sum up so far, we may say that all Greek mathematical textbooks were either translations or compilations of Western European texts, with the ultimate aim of transferring both Ancient Greek mathematical knowledge and modern scientific thinking. The scholars' basic criterion in selecting the originals was whether they constituted a modern proposal, whether they corresponded as far as possible to the scholars' own views on science, and whether they could be used as teaching tools.

References

Greek language:

BENJAMIN OF LESBOS (Veniamin Lesvios) (1820) *Euclid's Elements*, Vienna.

DOUKAS Konstantinos (1820) *Practical Arithmetic*, Vienna.

GEDEON Manuel (1976) *The Intellectual Motion of the Greek Race*, ed. A. Aggelou-F. Iliou, Athens.

GOVDELAS Dimitrios (1806) *Elements of Algebra*, Halle.

KARAS Giannis (1992) *The Sciences during the Period of the Turkish Occupation*, Manuscripts and Printed Matter. Vol. I: Mathematics, Estia Editions, Athens.

KARAS Giannis (1993) *German Influences on the Thought of the Modern Greek Renaissance*, Athens

KARAS Giannis (editor) (2003) *History and Philosophy of the Sciences in Greece (17th-19th centuries),* Metaichmio Editions, Athens.

KAVRAS Zisis (1800) *Elements of Arithmetic and Algebra*, Jena.

KOUMAS Konstantinos (1832) *History of Human Acts* vol. XII, Vienna.

KOUMAS Konstantinos (1807) *Series of Mathematical and Physical Treatises*, Vienna.

KOUMAS Konstantinos (1819) *Synopsis of the Sciences*, Vienna.

Logios Hermes, Periodical.

MOISIODAX Iosipos (1780) *Apologia*, Vienna (ed. A. Aggelou, rep. Ermis Editions, Athens 1992).

PSALIDAS Athanasios (1794) *Arithmetic*, Vienna.

RAZIS Dimitrios (1787) *New Order and Method of Geometry*, Venice.

SATHAS Konstantinos (1868) *Modern Greek Literature, Biographies (1453-1821)*, I. Chiotelis Editions, Athens.

TERDIMOU Maria (2006) *Mathematics in Greek Thought during the Ottoman Rule*, Athens.

THEOTOKIS Nikiforos (1798) *Elements of Mathematics*, Moscow.

15 National Library of Greece, MS. 1243.

Other:

CAMARIANO-CIORAN (1974) *Les Académies Princières de Bucurest et leurs professeurs*, Thessaloniki.

GILLISPIE C.C *Dictionary of Scientific Biography*, New York.

HENDERSON C.C (1977) *The Revival of Greek Thought 1620-1830*, Greek trans. F. Boros, Athens.

NIELSEN N. (1935) *Géomètres Français du Dix-Huitème Siècle*, Gauthier-Villars, Paris.

STRUIK D. *A Concise History of Mathematics*, Greek trans. Anna Ferendinou Nikolakopoulou, Zacharopoulos Editions, Athens.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 97.

HISTORY OF SCIENCES AND THE TEACHING OF LIFE AND EARTH SCIENCES IN FRENCH SECONDARY SCHOOL. WHAT KIND OF TEACHER'S TRAINING AND WHAT KIND OF TEACHING?

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Abstract

*Since 2005, learning programmes of Biology and Geology of pre-academics classes (pupils from 11 to 18 years) indicate that it' possible to introduce some elements of history of sciences relating to the science's concepts studing into these classes. Bu, It's just a possibility. Sciences teachers must follow the experimental method and not the historical one. It's through an investigative approach (*démarche d'investigation*) that historical elements can be used as well as experimentation or studies of current scientific documents. Usually the university training of Biology and Geology teachers does not include studies of history of sciences and the in-service training of teachers (of the national education) does not include training courses in history of sciences neither. Thus, teachers must be trained by themselves if they want to introduce elements of history of sciences into their sciences courses. The new textbooks present some pages of historical data, reduced to some lines of an historical source and some lines of context, or even just a photograph of a great scholar. There is a risk to reduce history of sciences to an illustrative and anecdotic role.*
This assessment established, the communication will discuss the reasons which are opposed to a real introduction of history of sciences into secundary courses of sciences, and which explain the lack of sciences history courses in teachers training.

What about sciences teachers training and competitive examination?

1. Recruitment of French state secondary teachers

First of all, we must have in mind the organization of our French secondary educational system. Teachers of the state secondary education are state civil servants, recruited by two distinct national competitive examinations: the "*Certificat d'aptitude au professorat de l'enseignement secondaire*" or the "*Agrégation*". From the 2011 examination, candidates must be master degree holders to be appointed in a secondary school. Until 2010, candidates had only to be holders of a licence degree, and most of them prepared examination in one year of special training within IUFM (*Institut universitaire de formation des maîtres*). At that time laureates were appointed both in IUFM to follow a professional training, and in a school as a teacher, teaching and being trained at the same time. Since September 2010, laureates (and also master holders) are directly posted in a school.

This recruitment mode on national competitive examination of the tenured teachers assures a big homogeneity in the teacher's initial training. Secondary education curricula being also national, the whole secondary education system presents a very strong structural and functional homogeneity. It is necessary to keep all these characteristics in mind, to measure the place of the history of sciences in French teaching of biological and geological sciences.

2. Teaching of life's and Earth's sciences in French secondary education

French secondary education includes the teaching of life sciences and Earth sciences ("*sciences de la vie et de la Terre*" or its acronym "SVT") from the first year (11 years old) up to baccalaureate (18 years old). This teaching is compulsory from first up to fifth year, and then compulsory, optional or absent in sixth and seventh years depending on the section. SVT is taught in the secondary since the setting up of the secondary education system in 1795. Many reforms, which went down in history of our education for two centuries, established step by step SVT as a general education subject, at least for most of the pupils.

For more than 150 years, the official syllabus of SVT has regularly recalled the possibility of introducing elements of history of sciences (HS) in the SVT class, but this possibility was never translated to a true introduction (Savaton, 2011). In 2001, the fifth year's syllabus of SVT proposed the introduction of some historical presentations of some scientific concepts studied in class, justified for cultural reasons.

"Science is not made of certainties, it is made of question settings and answers, which evolve and change with time. In many instances, nothing can replace historical account. This one has an irreplaceable cultural side, which locates scientific discovery in its temporal context but also shows how scientific discoveries influenced evolution of history. Historical account allows for measuring the difficulty that humanity met to resolve problems which can seem elementary today".

The 2005 secondary school new curricula (from first up to fourth year) have wished to develop inquiry-based science teaching (IBST), and proposed to introduce elements of history of sciences as a support for studies and exercises. Pupils must be able to locate scientific discoveries in time, and to conduct a critical study of historical texts. Examples are proposed: elements of history of digestion, blood circulation, antibiotics discovery or the Evolution theory, study of texts by Wegener on the continental drift, etc. It is not proposed to teach HS, but to use HS elements to teach SVT. The fifth year's class new syllabus on 2010 reduces the volume of knowledge to be acquired "to allow teachers to devote time to make understand what scientific knowledge, its mode of building, and its historic evolution, are". Processes based on history of sciences become an optimum way to develop IBST.

"History of scientific knowledge development, of its modification in the course of time, is useful means to include the nature of scientific knowledge and its mode of building with its advances and possible declines. It will be necessary to make sure that this approach does not lead to the simple evocation of a factual succession, and not to caricature this history so as to give a false idea of scientific demonstration; if some arguments have a major historical importance, it is rare that only one of them is enough to draw away a decisive evolution of scientific knowledge".

In 2011, in the scientific section of the sixth year, teachers will have to introduce some aspects of the history of the plate tectonics model. The main scientific lines of this model are studied in the third year, then completed in this sixth year after this historical study. It is the first time that an SVT curriculum invites teachers so apparently to introduce elements of history of sciences in concept or model teaching. In fact, the purpose is not to teach HS, but to introduce an historical epistemology (no an epistemological history). This teaching will introduce epistemology elements in the teaching of sciences; but to avoid a teaching of general epistemology, too much impregnated with philosophy of sciences, the syllabus *concepteurs* wanted to anchor them in the history of sciences. There is a will to introduce local (or regional) epistemology.

3. History of sciences in the university curricula of life's and Earth's sciences

It's rare to teach HS in university curricula of life's and Earth's sciences. Few universities offer courses in history of sciences, but when it is so, they are often of few hours. In the 1980s and 1990s, an optional course of history of sciences often took place in first and second years of sciences curricula, especially in mathematical or physical sciences sections (fewer in biology and geology). The university curricula reform in the 2000s and the creation of new licence and master degrees (LMD) often ended up with the disappearance of this teaching. The history of sciences, except in some special curricula, is present only in medicine and in engineers' schools, with very different contents and objectives.

European reform of LMD gives the students the possibility of building their own certificates by choosing their courses. But the reality of French university functioning is very different. Every university built its certificates by establishing a number of compulsory courses and offering some optional courses in the scientific domain of the certificate. It is therefore difficult for a student to really build his own certificate. It is a scientific choice also forced by organizational aspects like timetables or requirements on the minimum number of students to open a course. But such choice reduces introduction of HS very strongly.

4. Teachers training in university institutes of teachers training

Until 2010, the IUFM organized the preparation of the competitive examination and the professional training of laureates simultaneously. The year devoted to the examination preparation (PLC1) was almost exclusively dedicated to study subjects for the examination tests. Without tests about history of sciences, it was exceptional that IUFM offered a course of sciences history in PLC1 (Savaton, 2006). On the other hand, several IUFM included some hours of training in sciences history in the curriculum of the first training year of the civil servant trainee (PLC2). With few hours, it could only be some chronological landmarks in the HS, a study of an historical controversy, or a critical reading of texts. Ambitions were limited to introducing an historical perspective to think about the making of science, the discourse of sciences (with a very epistemological view), or limited to bring historical elements in support of a didactical discourse about the construction of the pupil's knowledge in school situation, or about special didactical or pedagogical tools to build their course.

A symposium organized in Montpellier (France) in 2005 by a group of investigators in history of sciences appointed in several IUFM (REFOREHST group: *http://plates-formes.iufm.fr/ehst/rubrique.php3?id_rubrique=6*) allowed to discuss the results of an inquiry about the EHST in courses and teachers trainings in IUFM. Most of the time, these trainings were very limited and depended on local leadership of few people.

The new masters creation specifically intended for the future secondary teachers, on September 2010, and the universities new autonomy let some people think that it could be changed. Each university set up its own certificate by associating courses on life's and Earth's sciences, didactical courses, practice in school, foreign language courses, and training in the use of information and communication technologies in education (ICTE). Half of them have introduced a course of history of sciences in masters in their first or second year. Courses of up to 50 hours are offered either alone or linked with epistemology and didactical courses. The 2002 national official texts, which specify the competences of future secondary teachers, pointed out that they should not only be specialists on scientific knowledge of their discipline, but also on history, epistemology and didactic of their discipline. But, as we will specify further, the absence of real recognition of these abilities in competitive entrance examinations tests reduced the weight of this text. The diversity of situations from one university to another, explained fundamentally by the building conditions of these new certificates (a certificate is often built and carried out by a group or some laboratories: it is not indeed a collective work including opinions and competences of all the actors in the university) and, locally, by the existence or not of research laboratories, teachers or investigators in history of sciences. In the absence of a true national framework, and in the emergency (new certificates should have been built in only some months), local powers were often decisive.

5. State competitive teacher's examinations

The reform of university training of future secondary teachers is the direct consequence of the reform of recruitment conditions of these teachers and of the competitive examination. Tests have been changed, both in their contents and in its form. But if tests are determined by official texts, their organization depends on the jury. Every school discipline has its own jury, composed of academics, teachers and pedagogical inspectors of secondary. The nature of the questions and their weight are determined by the jury composition. No historians of sciences were to be part of the 2011 jury for competitive examination of SVT teachers, even if official texts speak of recruiting secondary teachers who know the history of their disciplines. The jury, and with them general inspection of the secondary and scientific academics, do not like questioning candidates beyond very general knowledge in history of sciences, in the line of the general knowledge which is not specifically taught.

Recruitment by a competitive examination is a system which reinforces the university study of disciplines that the candidates will be questioned about in competition, and reduces or abolishes the university study of disciplines about which the candidates will not be, or not much, questioned. All investigations about examination history and curricula history have showed this very fact.

Obstacles or oppositions to the teaching of HST?

With or without these new SVT curricula in secondary education, history of sciences is little taught. During the Congress of the French Society of HST (SFHST) in 2004, a symposium called: «Teach the history of sciences today» (organized by Danielle Fauque and Hélène Gispert) has given us the occasion to discuss about obstacles of the history of sciences teaching in the teachers training. I raised then 4 arguments regularly opposed to this introduction: lack of time, absence of need, opposition between letters and sciences, relegation within the general knowledge (Savaton, 2004).

My analysis of obstacles and oppositions is based on my experience as a teacher, as a training officer in IUFM and as a lecturer in Earth's sciences and in history of sciences at the university. It is also based on my recent experience in the constitution of new masters, intended for secondary teachers, and at least on my research about the curricula history of life's sciences and of Earth's sciences in the secondary for two centuries.

Two types of blockages are found, in secondary education as well as in university, in the syllabus as well as in individual discourses: structural oppositions (construction conditions of certificates syllabus, discipline sharing out, length of school and academic years, competences of the teacher) and epistemological oppositions (interest of the history of sciences in sciences teaching). But this simplified categorization should not conceal the complexity of resistance, inertia and obstacles which very often blend several factors. I would try to present this question in the case of secondary education.

1. The «feeling of incompetence»

Teachers often declare themselves unable to introduce elements of history of sciences in their sciences lessons. Absence of an initial university training and lack of permanent professional training (hard reduction for several years) force teachers to form by themselves through readings. The schoolbook often becomes the only working source for teachers practice. Their treatment of HS is often limited to some pictures, some little text extracts or some dates in a discoveries chronology. Such sources don't push teachers to introduce elements of history of sciences; they only allow for adding a name or a date to the list of knowledge to be learned or to serve as motivation elements or attention trigger by their originality with regard to the class habits of the courses. When they present history of sciences, curricula official texts are instigations, not obligations. Pedagogical inspectors (ancient teachers) are not better formed in history of sciences and do not flourish either in this direction (their little support to the introduction of the HS in permanent training is eloquent).

This unpleasant feeling of incompetence can lead either to ignore instigations, or to protest against its introduction, according to people and contexts (establishment contexts, academy contexts, official texts comments). This opposition often relies on the argument of lack of time (not enough time to teach the whole syllabus) or of the pedagogical choice. This pedagogical choice can question the interest of HST by reporting absence of need (HST would be only a means among others), not enough pertinence (there are more efficient means) or "counterproductive" effects (the study of HS with its doubts, its dead ends and its historical "errors" would unsettle ongoing trainings).

2. The lack of time

The secondary syllabus is regularly reported as being overloaded, and the teachers first objective is often to finish the syllabus. Any pedagogical innovation requires more time and therefore leads to a slower progress.

Lack of time isn't an easy argument or a false argument in an educational system which cuts every discipline in a list of knowledge to acquire per year, and turns this knowledge acquisition into a condition of schooling success. Each pedagogical choice must take care of time. Study of historical texts needs time. The use of the historical method needs more time than the classical method, something between the dogmatic method and the inquiry method. Syllabus proposes the use of elements of HS in the inquiry method. This problem of lack of time has been regularly put forward, in the history of SVT teaching, against the introduction of an historical method. It is a time investment which requires that teachers are persuaded of the huge interest which there would be for their pupils to adopt it. It is one of the reasons why syllabus proposes to use historical elements in an inquiry method, and does not propose to use the historical one.

What can we do in two or four hours to study the history of Plate Tectonics Theory with sixth year pupils? No history –We don't have time to study texts and to understand their contexts. We'll just have time to present main ideas of one of the controversies which marked this history, and maybe to expose the conditions of its solution. School training time of is not the same as university teaching time, so we must test what kind of HS we can do in the time we have, and not project the time we need for what we want to do. Otherwise, we risk never having the necessary time to test an introduction of HS.

3. The epistemological obstacle

French secondary education in the 19th century is dominated by the teaching of letters. The introduction of sciences curricula was made at the price of a conflict with classical letters. The importance of sciences and technologies in the development of industrial societies at the end of the nineteenth century and in the beginning of the twentieth century reversed hierarchies and sciences became the determining syllabus for pupils' selection and career choice. In a system of hierarchy between school disciplines, life sciences and Earth sciences (the third in the order of sciences after mathematics and physical and chemical sciences) did not cease stressing their nearness with mathematical, physical and chemical sciences. This discipline changed its name in 1902, from *natural history* to *natural sciences*. It is not anecdotic.

Opposition between sciences and letters is very marked in the teachers discourse (and by impregnation in the pupils discourse), who distinguish those with a literary bent and those with a scientific bent. The first would be characterized by taste for detail, fancy and creativity, and the second, by taste for the essential fact and method. The history of sciences is received as an external view of letters about sciences.

Official instructions haven't ceased to remind teachers that they have to develop the scientific mind of their pupils but haven't added, until today, that HS could help to do it. Today, new SVT syllabus introduces history of sciences as a mean of scientific mind training for the critical detachment which it can bring. But, teaching practice depends of tradition, and changing the mind of official instructions is not enough to immediately change practice and representations by teachers. Wishing today that sciences teaching include elements of history of sciences to correct misrepresentations, is conceding that sciences teaching in these last years neglected its contribution. It is citicizing the sciences teachers community, declaring today that an historical lighting is otherwise essential, at least useful to build a better representation of science. Many teachers don't understand the reasons why they would have to teach "errors" and "dead ends" of the past while they miss time to teach the results of an up-to-date science. They justify the social importance of their discipline by the role of sciences and technologies in the development of our societies. They don't wonder about the nature of scientific knowledge which they try to transmit to pupils, because of a kind of naïve positivism which push them to keep mixing up science and progress, science and truth, and to refuse linking science with human mind, with societies, with powers, with ideologies, etc. This obstacle is not the least that we have to overcome to change the practice.

Conclusion

To conclude in a perspective of teachers training, the introduction of some little elements of history of sciences in the new SVT syllabus of secondary education could be an opportunity for secondary teachers to think about the nature of the science they teach and about the way they teach it. But for that, it could be necessary that our whole teachers training system, from the university syllabus up to the national competitive examination tests and permanent training, question their own representation of the nature of scientific knowledge and try to introduce an historical dimension.

As long as the university syllabus of sciences don't introduce HST into the students training, I'm afraid that this attempt of the secondary education syllabus to introduce elements of HS will be just one more invitation in the long list of never applied instructions. But introducing HS in science teaching needs to be discussed and specified. HST for what? For itself? For epistemological teaching? Who will teach HST in universities? Who will teach HST in initial and permanent teachers training? There are a lot of questions which should be answered before.

References

SAVATON, Pierre, (2004), *Enseigner l'épistémologie et l'histoire des sciences et techniques en 2e année d'IUFM. Prendre en compte les oppositions*, Congrès de la SFHST, Poitiers, 20 au 22 mai 2004.

SAVATON, Pierre, (2005), Eléments d'épistémologie et d'histoire des sciences : Introduction dans la formation disciplinaire des PLC2 SVT, In. C. Pellois & J.-F. Thémines (coord.), Approche de la professionnalité des formateurs d'enseignants, *Cahiers de la MRSH*, Caen : MRSH de Caen, N° spécial, octobre 2005, 169-180.

SAVATON, Pierre, (2006), Enquête sur les formations initiales et continues en EHST dans les IUFM, *Tréma*, N°26, octobre 2006, 11-19.

SAVATON, Pierre, (2011), Histoire des sciences et démarche historique dans l'enseignement des SVT. Quelle histoire de la tectonique des plaques ?, *Recherche en Didactiques des sciences et des technologies*, N°3.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 102.

WHY CAN IT BE DIFFICULT, ESPECIALLY FOR BIOLOGY TEACHERS, TO USE ON-LINE RESOURCES IN HISTORY OF SCIENCE FOR INQUIRY-BASED SCIENCE TEACHING?

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Abstract

We think that history of biology helps the understanding of scientific inquiry by pupils, and we prepare to offer some on-line resources for them. But then, how can we help teachers to use this means? And why do they need help specially in biology? In 1994, Charles Darwin's Theory of Evolution appears for the first time in France in the biology syllabus. A few years later, it is already forgotten. Teachers prefer teaching facts and evolution mechanisms rather than the evolution theory because students had many questions about moral and religious values. So why this confusion in science classes? Perhaps because students do not understand the scientific process. They do not know the nature and construction of a scientific theory. They do not understand that sciences only give us knowledge and means to understand the world. However, in the study of biology, moral values mobilized by the studied objects are often very important. The study of life –its fragility, history, links with the environment– affects us directly. But these values change with time. For example, talking about the link between other animals and humans is less difficult today, but it still affects some students. The use of resources in history of science can remove these difficulties. But, if we want history of science to help teachers to teach scientific methods and pupils to understand it, it is necessary to know the scientific concepts, the investigation method , but also the historical transformations and implications of these moral values. So when in France, and may be in Europe, we want to offer on-line resources in history of biology, we have to discuss about this sort of problem. What on-line resources in history of biology should we give to the teachers?

"If we had a real education system, there would of course self-defense intellectual."
Noam Chomsky.

In 1970, French Nobel Prize of Medicine Jacques Monod (1910-1976) declared that "true knowledge doesn't know of moral values": he excluded individual or collective ethics from his investigations for being "inherently non-objective", whatever the form: moral, religious or political discourses. Moreover, Sciences are intended to "attempt to values" –meaning "contemporary legacies secularised religious or philosophical myths of past centuries."[1] We often hear that "Sciences are amoral", but are scientific data devoid of any moral implications when we teach them?

Yet, these "moral implications" are most problematic for sciences teachers, and maybe for biology teachers. When they try to teach an investigative approach to science, using texts or images from books or on-line resources, they often find big problems with their students. But it's still worse when it is the theses from these on-line resources which raise a problem, because they have no historical and philosophical vision of the compound scientific question. Teaching sciences through on-line resources but without knowing history of sciences and epistemology is really a big, rather urgent and dramatic problem.

A first example on a French web for teachers

We want to begin with a recent example. It is not an historical example, but it concerns our subject because it has to do with the human being and the environment, and this text has a historical dimension. This is an on-line resource for French teachers in primary school. It's name is *My home, my planet... and I!*[2] The first sentence is already problem:

> "Every day, the world population increases of more than 200 000 people… In 2050, our planet will credibly count more than nine billion terrestrials!"

My house, my planet... and I! is an educational project in sustainable development, intended to make teachers, children and parents sensitive to environmental, social and sanitary aspects of the habitat. But why begin with the demographic aspect of this question? Is not this proposition connected to a Malthusian vision[3]? Apparently, writers ignore this Malthusian and scientistic dimensions of the problem –or want to ignore it, as it is very surprising to ignore this question since William Godwin's (1756-1835) work in 1820[4]. But, at first, writers accept the old Paul R. Ehrlich's conclusion –in 1968, in his famous book, *The Population Bomb*[5]. Or, maybe they know of the film *Solyent Green* (1973) which explains the same dark vision of the future[6]. Secondly, they agree with this kind of scientistic ecology "for the elite": "scientists know what is good for you". So we can see that the implicit values are really ignored or, worse, concealed.

Now, we must be honest; these values raise a big problem in a classroom. We have to inform teachers and on-line resource providers about these non-scientific dimensions of a scientific problem[7]. And we must admit our objectives, difficulties and *a priori*'s.

Knowing history of science to teach science

At first, we think that history of science –and there, history of biology– helps pupils and students to understand scientific inquiry, and maybe the links between sciences and society. Therefore, it is necessary to show the link between the history of science, philosophical history and economics of European society at the begining of the story, when the Scientific revolution of the 16th and 17th centuries took place.

The science that currently dominates was born during the struggles between Western Christians of the 16th and 17th centuries. After these philosophical wars, an economic vision and rationalization of the world will be placed above all other societal issues and will be confused with "progress". These struggles constitute the founding trauma of both the West and the science we practice. Faced with these endless, philosophical and deadly struggles, states and governments which were

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1 BLANCKAERT, Claude, *Des sciences contre l'humain,* Vol.1, Paris, Autrement, 1993.

2 www.MaMaisonMaPlaneteEtMoi.fr

3 The English economist Thomas Malthus (1766-1834) was campaigning for birth control and no social benefits to the poor.

4 GODWIN, William, Of population : An Enquiry concerning the power of Increase In the numbers of Mankind, Beign an Answer ot M. Malthus's Essay on that Subject, 1820.

5 EHRLICH, P., *The Population Bomb*, Ballantine Books : New York, 1968.

6 This film is taken from Harry Harrison's novel *Make room, make room*, published in 1966.

7 FERRIERE, Hervé, "Réflexion épistémologique sur les rapports entre enseignement et représentation des sciences, formation des enseignants, humanisme et démocratie." In *Actes des XIXèmes JIES de Chamonix*, 5-7 mai 2008.

being formed will almost imperceptibly drag the stakes of these struggles to another area of the "city life", a sector purportedly more neutral, objective, pragmatic and rational, and hence less so-called "ideological": trade. We are still far from the nineteenth century nations and the economic powers of bankers, but they are beginning to emerge and to become essential for human survival. In order to ensure civil peace and their own survival, the leaders of the time, by a sort of unspoken agreement, will place the "economic vision" and "rationalization of the world" above all other societal issues. States place themselves above the philosophical fray and ensure the freedom of some encroachment by that of others, a kind of moral neutrality and prosperity virtually universal (at least in the future). The "soft business" of all with all saves the Christians in their fight to death. As Voltaire said later, "When it comes to money, everyone has the same religion." Thus, only commercial competition shall be allowed and valued; first among individuals and between groups, countries and, soon, whole empires. But this fight will be considered positive, rational and pragmatic. It may even participate in people's happiness and help invent and illustrate the famous positivist "progress". The only valid and real criterion will be its effectiveness. Success and wealth will be the reward for the best of us. It is within this context that the so-called "Western science" will develop. It will therefore be marked from birth by an ambiguity still not assumed and mostly little known in its two main objectives: understanding of nature to escape his tyranny (slow understanding and motivated by an often altruistic and universalistic concern) and control to draw profit (profitability, immediate practical applications and technology). In both cases, it's about being efficient. It's in this concern for efficiency –and above all economic efficiency– that modern Western science will develop.

But, if we only give teachers resources without theses explanations, it is necessary to make five big bets:

- Pupils and teachers can understand historical texts, drawings, models or experiments (and sometimes only their telling);
- They can understand from current theories the most famous in biology: Evolution and Ecology theories;
- They can understand the complexity of these theories;
- They can rebuild the inquiry conducted in their elaboration;
- They can understand their moral implications.

But, and it may be the most important for us, we must take care because historical resources available to biology teachers could quickly cause problems in classrooms. And there are already problems.

A second example about Linnaeus' classification

We will show you a second example about the Evolution theory and the place for humans in the classification of animals. In 1994, Charles Darwin's Theory of Evolution appears for the first time in France in the biology syllabus. A few years later, it is already forgotten. Teachers prefer teaching facts and evolution mechanisms rather than the evolution theory because –so they say– students had many questions about values, moral and religious implications, and teachers didn't know how to answer[8].

Students ask questions about religion, creationism, social Darwinism, Malthusian theory, racism and eugenicist vision of society[9]. Teachers say "these questions are not biology." And they are right: teaching biology with history resources is more difficult than we expected.

For example, when teachers tried to study some historical texts, they encountered new difficulties, as classical texts were very hard to teach without some knowledge on history of biology and on general history. We want to show a very classical (but rarely known) text written by Carl Von Linneaus (1707-1778): *Systema Naturae* (1758), a book that we can find on the web.

Biology teachers wanted to read it to show how scientists saw classification in the eighteenth century, and they had a huge surprise.

At the top in nature was, of course, the human being. But, first surprise, there are two human species: the human being and *homo troglodytes*[10]: the orang-utan. And, second surprise, people in our species were more often divided according to culture, religion, customs and other such features.

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[8] COQUIDE, Maryline, et TIRARD, Stéphane, *L'évolution du vivant. Un enseignement à risque ?* Paris : Vuibert, Adapt, 2009. LECOINTRE, Guillaume, FORTIN, Corinne, *Guide critique de l'évolution,* Belin, 2009.

[9] FERRIERE, Hervé, L'homme, un singe comme les autres, éléments d'histoire des sciences et d'épistémologie pour enseigner l'évolution de la lignée humaine, Vuibert Adapt, 2011.

[10] He also briefly described a *second* human species, *Homo troglodytes* ("cave-dwelling man").

Mammals, Primates. Antropomorpha.

1°) Homo sapiens. Diurnal man. «Nosce te ipsum.» Endowed with reason, talking, standing and bimane To distinguish from Homo ferus, Wild man, four-footed, mute and hairy.

2°) Homo troglodytes. Night man or cave-dwelling man.

Linneaus proposed 5 *taxa* of a lower rank: *Americanus*, *Asiaticus*, *Africanus, Europeanus and monstruosus*[11]. These categories were based at first on place of origin and secondly on skin colour[12]. Whereas Linné used some physical characteristics in his division, he also wandered about attributing characteristics according to his view of social and emotional features. He also divided human being by how he thought they were governed: by customs, caprice, opinions and laws.

Americanus are reddish (or copper coloured), stubborn or obstinated, merry, easily angered and free. They have black, straight, thick, hair – wide nostrils– harsh face. They paint themselves with fine red lines, and are regulated by customs. The *Europeanus* are white, sanguine and brawny. They are gentle, acute and inventive and they are governed by laws. The *Asiaticus* are avaricious and easily distracted. The *Africanus* are phlegmatic, relaxed, negligent and governed by caprice. And, finally, we discover a strange category: *Homo monstrosous*. There we find: *Patagonian giant* or the *Dwarf of the Alps*, the *Monorchid* –who has only one testicle– or the *Macrocephalus* of China and the *Plagiocephalus* (or "flathead") in *Canada.*

We can see that the text contains many "clichés" that still exist today. Also, we can only see "racism"[13]. In fact, students always see "racism". But at the time of Linneaus the word "racism" did not exist with the modern meaning. For example, the word "racism" is found only in 1932 in the French Academia dictionary. Linneaus does not intend to talk about superiority or hierarchy[14].

In the classroom, the debate that will follow this reading has nothing to do with the question of scientific classification of animals and the human being. To summarize, there is a high risk of making mistakes and non-sense in the classroom by reading a text without explanations or information about its values. So must we forget it? Of course not.

First, this text is but a step in a scientific process and, second, this step has moral implications. But often, students do not understand that sciences only give us knowledge and means to understand the world. Sciences don't give us moral values.

The transformations of moral values in history

If we want history of science to help understand the Inquiry Based Science, it is of course necessary to know the scientific concepts and investigation methods, but also the transformations of moral values and word meanings in time. The resources that we want to give, should show this complexity, and the values have to be clearly identified. Values, representations and implicit assumptions must be shown, and it requires a sharp, meticulous and balanced work before presenting these resources. So when we want to offer on-line resources in history of biology, we have to discuss about this sort of problem. Teachers need help specially in biology because the study of life –its fragility, history, links with the environment– affects us directly, and because all these objects involve values.

So how can we help teachers to use this means? First, we must talk about these values and not deny them. Second, we must give a very "rich backdrop" of scientists work and scientific process. We must give them a large background, backdrop or a rich historical context! It's not enough to suggest only some "scenarios" for classroom.

A last English example about Darwin

We can consider the Web quest on the site of the British Museum. There are biographies of important characters: Charles Darwin (1809-1882), Alfred Russel Wallace (1823-1913), Thomas Henry Huxley (1825-1895) –Julian and Aldous Huxley's grandfather–, ... We find some links with Scientific Explanations and examples about the evolution process. Authors offer interesting investigations but, soon, these resources also pose us some questions. There is a short presentation of the

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[11] http://www.newworldencyclopedia.org/entry/Carolus_Linnaeus.

[12] SMEDLEY, Audrey, *Race in north America*, Westview Press, 1999. Ania Loombia, *Colonialism and postcolonialism*, Routledge, 1998, p.115.

[13] PICHOT, André, La Société pure, de Darwin à Hitler, Flammarion, 2000 and PICHOT, André, Aux origines des théories raciales. De la Bible à Darwin, Flammarion, 2008.

[14] TORT, Patrick, *Darwin n'est pas celui qu'on croit,* Cavalier bleu, 2010, and *L'effet Darwin*, Seuil, 2008. HOQUET, Thierry, Darwin contre Darwin, Seuil, 2009.

context in which Darwin worked (some pictures of the period and just a few words). The author's present, "Victorian England" with its Imperial power and England as a "trading and religious" nation. But what does that mean? Why show only one economic aspect: a "trading nation"? What about the industrial revolution and colonialism? In fact, it's difficult to understand what "Imperial power" means exactly. And what about the emergence of capitalism? What about the links between sciences and economy? And what about the Malthusian vision –which still exists today– and the hierarchical view of population? And, for example, what about the Sepoy War in India in 1857, while the famous Darwin's book is published in 1859? Why show only the religious aspect of the controversy?

Is it a choice to reduce the debate between science and religion? And we face the challenge posed to teachers: religious values, as if they were the only ones that can interact with science. It's an artificial issue and also a deliberate choice. But it is not enough to really understand this controversy. And this presentation only partially addresses students' questions.

We see that the work of historians is necessary here: biology teachers should be able to quickly have a fairly complete historical context. We must therefore speak about the general context of science process. We must speak about moral, social, economic and political construction of theories in biology to understand them better. Not because the context explains the science content produced, but because the context explains how Darwin's theory was perceived, received and translated far away from science. How his theory became a political speech, a social *a priori*, as, for example, with "the struggle for life". Sometimes, people have kept only the speech about values and not the scientific data: they believe that social and political conclusions are directly inspired by scientific laws.

When we teach the evolution theory, we only keep the data and forget the values speech, while students have to face this speech when they leave school. We must "draw the historical scene" as rich as possible, and see exactly the real biological questions that arise. We must not deny all the issues that biology can address. But after the exposition of these issues, after showing their links with the biological question, we must explain that science does not answer all the questions. Science does not answer questions about values. They talk about reality and not what we think good or bad in our life, our history and our society.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 107.

TEACHING SCIENTIFIC EXPLANATIONS AND THEORIES FROM A METHODOLOGICAL ASSOCIATION OF HISTORICAL-PHILOSOPHICAL STRUCTURE AND PEDAGOGICAL GOALS[1]

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Abstract

The role of History and Philosophy of Science for Science and Mathematics teaching has been a theme of several studies about a correlation of these three areas, particularly with the explicit relationship between history, philosophy and cognition.
Our research is inserted in the context of development of theoretical references for the creation of instruments to improve the learning of concepts and theories in Science and Mathematics. We believe that such creation must be in a structured, articulated, and integrated mode for achieving analytical learning.
Our first and main report involves identification and characterization of scientific models by historical-philosophical reconstructions, which presupposes the overcoming of that modeling –a proto-theory– for obtaining a universal theory. As an exemplar of analysis and findings, we present an application to the study of Beta decay (first identification of nuclear weak interaction) addressed to undergraduate physics students or in-service teacher education. This exemplar illustrates some pedagogical results of the application of this proposal in a discipline for undergraduate students of a physics course at State University of Londrina, Parana, Brazil. The second part of our work brings a synthesis of findings in several researches, characterized in two axis of investigation: in partnership with graduate students of our research team, and on the scope of a graduate program in Science and Mathematics Education at the same university.

[1] Research funded by Capes, CNPq and Fundação Araucaria, from Brazil.

The role of History and Philosophy of Science (HPS) in Science and Mathematics Education (SME) has been a theme of several studies about a correlation of these three areas, particularly with the explicit relationship between history, philosophy and cognition.

There are two traditional perspectives in these studies. The first one considers that historical reconstructions (with epistemological cares) or historical-philosophical reconstructions constitute accomplished contributions in the area of SME, since they present indications of their potential use. The second considers those reconstructions as a research stage, which should be followed by adaptations and even pedagogical transformations; then they would reach the status of scientific outcome.

The latter perspective has, yet, two axes. One considers that investigation of the process to articulate, structure and integrate history, philosophy and pedagogical knowledge is already a scientific result, since it presents a theoretical-conceptual-methodological analysis of the produced proposal. The second axis takes into account the investigated process, but considers only as resulting scientific reports the empirical findings obtained from the application of this process.

Our research is inserted in the context of the development of theoretical references for the creation of learning condition for concepts and theories in Science and Mathematics. We believe that such creation must be in a structured, articulated, and integrated mode for achieving an analytical learning.

Our ongoing studies aim to demonstrate that involving the philosophy of science in a correlative field requires knowledge and commitment with a balanced relationship between scientific subjects, theoretical-methodological referents, and application context. In the historiography of science, another related domain, development of historical reconstructions involve a theme in a historical period, with the indispensable care for reliability and intelligibility in the interpretations of primary and secondary sources.

Therefore, when one is making an investigation that approximates, connects, and articulates knowledge domains with Science Education, one should take in consideration at least the epistemological, methodological, and ontological assumptions. It is a careful and rigorous process, which cannot contain conflicts and inconsistencies in the interdisciplinary knowledge production.

However, there are some issues:

- What is the role of HPS in the development of this process?
- What is the public of an educational and formative action?
- How to develop an approach to reach our goal of teaching and learning about a scientific culture?

These mottos expose a very clear point to us: it is not enough, despite being necessary, to build historical reconstructions, or studies of philosophy of science, or even explicit historical-philosophical debates, to contribute to the knowledge production in the area of Scientific Education. We cannot confuse the Teaching of Sciences with Teaching of History and Philosophy of the Sciences, and the same applies to scientific investigations of those areas.

With this understanding, we have been developing research in the Graduate Program in Science and Mathematics Education, at State University of Londrina, Paraná, Brazil. This research has as central focus the search of scientific outcome that corroborates an interdisciplinary integration of the domains of History and Philosophy of Science with the domains of Scientific and Mathematical Education.

In the following synopsis we present exemplars of consolidated findings, obtained from our investigations or in partnership with advised graduate students, organized in two axes.

Axis of Research: Investigation of Historical and Philosophical Approaches for the Teaching of Scientific Knowledge in Basic Education

"Historical-philosophical Approach and Mathematical Education –A Proposal of Interaction among Domains of Knowledge"
(BATISTA, I. L., LUCCAS, S., 2004)

We present a historical-philosophical approach with the aim of contributing to research in Mathematics Education, based on analysis of epistemological, logical, ontological, and methodological elements of the structure and the articulations that a specific knowledge presents, from its creation until its current use in Mathematics Education. Additionally, we are concerned with the knowledge ability to solve mathematical problems, as defined by Larry Laudan. Based in theoretical references in History and Philosophy of Science and Mathematics, we provide an exemplar of application of our approach, with an historical-philosophical reconstruction of the subjects Systems of Linear Equations and Determinants, found in the works developed by two mathematicians: Takakazu Seki Kowa (1642-1708) and Gottfried Wilhelm Leibniz (1646-1716).

"Investigation of a Didactical Sequence with Historical-philosophical Focus for the Teaching of Trigonometric Functions in High School"
(SAMPAIO, H., BATISTA, I. L., 2007)

In this research, we investigated the process of construction of a historical-philosophical approach through a historical synthesis about Trigonometry, through documental research in books of History of Mathematics, especially the ones that specifically deal with the History of Trigonometry. We adopted a qualitative methodological approach, with the aim of investigating the construction of a didactic sequence, based on the historical synthesis of Trigonometry, focusing mainly in the study of trigonometric functions and its teaching in High School. We introduced a philosophical approach that incorporates an axiological discussion, constituted by a group of cognitive values of Science, and applied it in Mathematics Education. The historical-didactical sequence, based in Didactical Engineering and Meaningful Learning, was applied in the second year of High School, in Londrina, Paraná, Brazil. Our findings showed that the built approach was effective for trigonometry learning, making manifestation of cognitive values in Mathematics possible in the historical-didactical sequence and in the students' knowledge.

"A Historical-pedagogical Approach for the Teaching of Sciences in the Initial Series of Basic Education"
(BATISTA, I. L., ARAMAN, E. m., 2009)

This research, of qualitative character, promoted the integration of references in the elaboration of an appropriate historical-pedagogical approach to the initial series of Basic Education in Londrina, Paraná, Brazil. The reference frame contemplates the need for Scientific Literacy; Meaningful Learning through innovative activities in Science Teaching; elaboration of Conceptual Maps by students; and construction of activities that respects the children's cognitive development. The theme for that investigation was the Rainbow and its understanding in Physics. We elaborated a reconstruction of the historical episodes from ancient Greece until the physical understanding of this phenomenon in the 18th century. From this reference frame and the historical study about the phenomenon, we created a historical-epistemological sequence of experimental activities that was applied to students of fourth series of Basic Education. Evaluation of the learning process was done through elaboration of Conceptual Maps by the children, before and after application of the sequence. Our findings evidence that the construction of a historical-pedagogical approach to the learning of physical concepts in Basic Education is valuable, providing good outcomes in that teaching level.

Schema of integration of references

Scientific Literacy

History of Science
(Theme: Physics of the Rainbow)

Meaningful Learning
(Conceptual maps)

Sequence of empirical activities
(Execution, registration and analysis)

Cognitive Constructivism

Axis of Research: Teachers Knowledges and Interdisciplinary Focus

On the Perspective of the Education of Teacher and Researcher in Sciences

Findings of our research on the Education of Teacher and Researcher in Sciences have been indicating the significance of the use of HPS, as both formation and qualification search for some intrinsic aspects as described below.

In the initial training to disciplinary research, HPS focus is intrinsic to the preparation for:

- Creative research.
  A critical, non-dogmatic, contextualized, and opened view to innovation is necessary with a balance. An open mind without criticism could go to fantasies or senseless ideas. A critical mind out of balance could become intellectually sterile.
- Methodology and planning of research.
  The researcher becomes "blind" without a historical view of a science and without the dynamics that the proposed scientific problems present. In this way, one can only choose a research line by imitation, by blindly adopting the orientation of some group, or by following the trend, or just the *status quo*.
- Elements of decision.
  HPS provide access to structural difficulties, fruitful tendencies, clear and delimited objectives.

- How to inquiry.
  With this ensemble of elements, a rational and fertile decision about what to do in a scientific research is possible.

On the Initial Education of Teachers

It should integrate the elements listed above for the researcher and, complementarily, realize that the teacher is an investigator of her/his pedagogical practice. In addition, the didactical materials with historical-philosophical fundaments should suffer adaptations and pedagogical transformations before an application in classroom to obtain good quality results.

With this kind of education, the teacher should know how to implement HPS approaches in the teaching of each science. Approaches work with, for instance, reproduction of historical experiments or provide familiarity with the original scientific sources. In addition, some processes that transform a historically constructed knowledge in school knowledge, and that integrate HPS systematically in the school knowledge.

According to our findings, such a teacher should know and investigate:

- specific activities of HPS;
- references (theoretical-conceptual and methodological) for class preparation;
- didactical reconstructions of historical experiments;
- interdisciplinary activities, differentiating a specific scientific knowledge from educational knowledge.

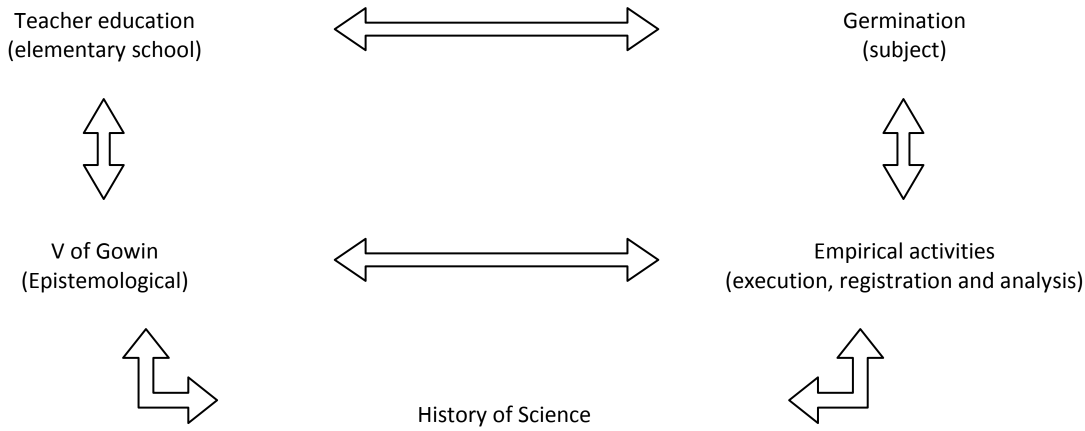
The literature in Science and Mathematics Education has demonstrated that with the current didactical and practical education of science teachers, few are empowered with these competences (TARDIF, 2002; BATISTA, 2004 and 2007). This indicates the requirement of curricular changes and an update of academical culture of the undergraduate courses and in the teachers updating who are in service. It is a new knowledge (disciplinary, methodological and interdisciplinary) to the teacher. Consequently, the focus needs changing in the styles and evaluation methods, and that has to be significant to the students. As all innovations, some unexpected institutional difficulties will probably appear, as it happens for any other approach different from traditional practice in undergraduate courses.

Exemplars of Teaching Scientific Explanations Using a Historical-philosophical Structure

"The History of the Science and the V of Gowin in the Science Teachers Education"
(NASCIMENTO, E. G., BATISTA, I. L., 2007)

It was an investigation about the construction of a proposal for initial education of teachers to science teaching in the first grades of Basic Education, in Brazil. From the references frame, we identified some needs for scientific literacy, and the Meaningful Learning suggests the use of innovative proposals to the discipline of science in this level, with the use of learning facilitators. Our proposal is based on an approach that includes the use of History of Science associated with experimental activities. Also, it includes Science Teaching research, with Gowin's V as a way of structuring the activities (implementation, registration and analysis). The theme chosen for this study was the seeds germination, and a synthesis was elaborated of historical episodes necessary for its understanding. Oriented by such synthesis and references frame, we developed and applied an historical sequence of activities to groups of senior students in an undergraduate course of Pedagogy. The monitoring of activities and the learning results was through analysis of V´s Diagrams constructed by the students themselves during the activities. The research results allow us the inference that this approach, when used in an appropriate manner, can become a significant tool in teachers education of the early grades of basic education.

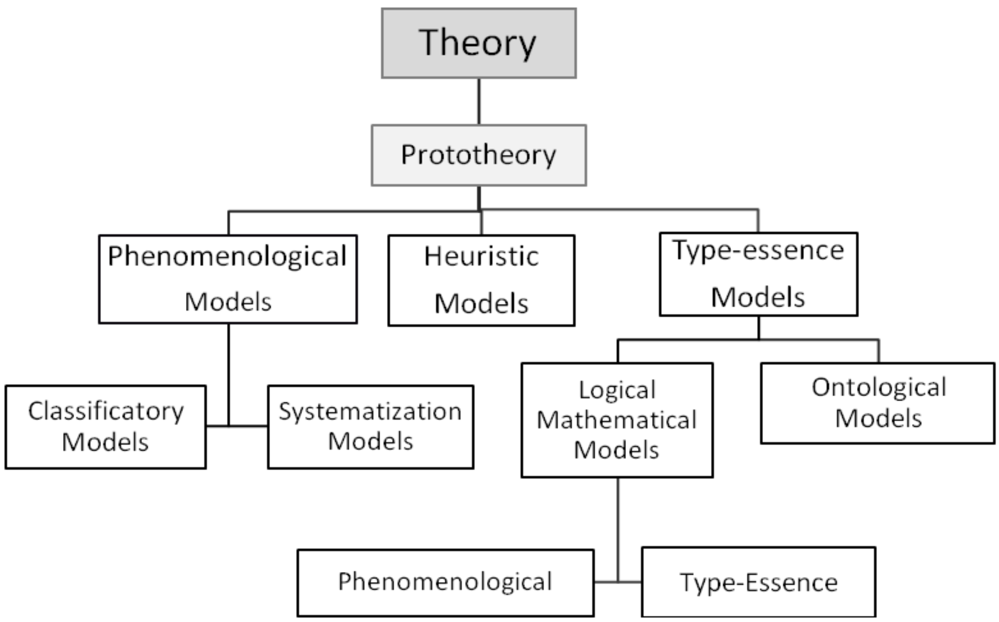
Schema of integration of references



"Undergraduate Discipline of Evolution of Concepts and Theories in Physics –The Routes of Theory Building"
(BATISTA, I. L., 2004 and 2007).

Our main report involves the identification and characterization of scientific models by historical-philosophical reconstructions, which presuppose the overcoming of modelling –a *proto-theory* (BATISTA, 1999)– and achieving a universal theory. As an exemplar of analysis and findings, we present an application of the historical-epistemological study of Beta decay (first identification of nuclear weak interaction done by Enrico Fermi, in 1933) addressed to undergraduate students of physics or in-service teachers education. This exemplar illustrates some pedagogical results of the application of our proposal in a discipline for undergraduate students of a physics course at State University of Londrina, Parana, Brazil.

Understanding the construction of theories: a *Theory Schema* (1999)



Definitions and explanation of the schema:

- Model:

  "[...] a model is a natural or artificial entity related, in some way, to the entity under study or some of its aspects. A model is able to replace the object (entity) and to serve as a relatively independent "quasi-entity", producing certain mediated knowledge concerning to that entity." (BATISTA, 1999 and 2004)

- Theory:

  "[…] is an explanation that is consistent with related empirical aspects, in a greatest scope, to explain the experimental data already known and any other forthcoming. It must be consistent with a chosen logic, classical or heterodox, with a syntactic structure (mathematical and/or linguistics), with the domain of applicability and with a set of rules that allow to connect the syntactic structure with the empirical domain."(BATISTA, 1999)

Building theories from models

Depending on the expression of the nature of the entity to be modelled, we judged advisable to split the heuristic models in phenomenological models (they describe certain aspects that characterize the way a physical process grows, but it doesn't explain precisely why it happens) and essence-type models (it supplies some preliminary interpretations of the essences and causes of the physical process as well as a mathematical structure).

Phenomenological Instance

Phenomenological models include classificatory, pictorial, and linear analogical models; they are typically empirical models. They apprehend regularities in specific relations of studied entities. However, they are not able to explain their essences (ontology, logical structure and syntax).

Type-essence Instance

These models provide a preliminary interpretation made by analogical hypotheses and generate ontological, causal and logic-structural formulations of a process in analysis. (BATISTA, 1999, p. 29).

Type-essence models are split in:

- Logical-mathematical

  "[...] are systems of logical elements, whose mathematical structure is analogous to the structure of physical entities. If they have such structure in a lower grade, they are called phenomenological logical-mathematical models; if they have it in a higher grade, they are called logical-mathematical type-essence models." (BATISTA, 2004, p. 468)

Logical-Mathematical elements can be translated as a logical-syntactical one for a possible application in different phenomena and/or different domains. We can figure out a formal representation –mathematical, linguistics, algorithmic–, whose structure guides and clarifies a concept in development. In this case, the rules of the linguistic structure become the rules of a scientific explanation. For example, the conservation of algorithmic properties represents and translates the conservation of natural properties of organic, geological, chemical systems. Each algorithmic manipulation, that is, an action in an equation system signifies a manipulation of the system itself.

- Ontological

  "[...] they are assumptions regarding to an essential peculiarity (about what exists) in domains of physical reality. Recognizing these peculiarities of real is to achieve relevant theoretical results." (BATISTA, 2004, p. 468)

Heuristic Instance

These models consist of a synthetical combination of certain explanations coming from phenomenological and type-essence instances, but still analogous to some previous scientific knowledge. They are a partial and phenomenological solution of the modelled problem.

However, we work to identify and to analyze Universal Theories. In this process, we create a way to supplant that profusion of model concepts found in literature and, mostly, to explain the transition and achievement to a well-established theory, an answer about what is the bridge between models and theory. We created the concept of *Proto-theory*, which is an early and intermediate epistemological stage between models and consolidated scientific theory. It becomes a new, original pattern and an underpinning to create new models for empirical and theoretical tests. It is needed to consolidate the new theory indicated by a proto-theory in a process of confirmation, universality, and improvements.

Proto-theory: scientific creation that is originated from a quite innovative and synthetical solution (general hypotheses, principles, new physical constant and new mathematical/syntactical structure) to problems enunciated by heuristic models.

For the explanation of Beta interaction, that probed itself as weak interactions between elementary particles, it has a phenomenological instance and a type-essence instance in the base of its construction, as well as the epistemological elements to ripen and become a (consolidated) scientific theory.

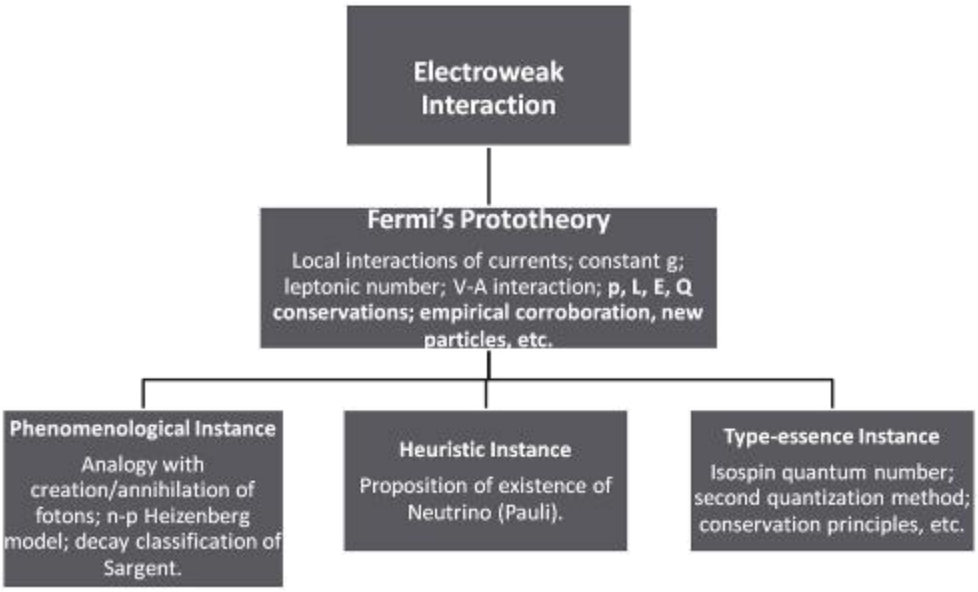
In the phenomenological instance, classificatory models are present and already systematized. In the essence-type instance, superior logical-mathematical models, whose mathematical structures are in analogy with the structure of physical entities, participate and produce ontology to the entities in study.

As a transition between investigation of the built model and the consolidated formulation of a new phenomena, it does not have a previous analogous solution in the same scientific domain and it is the beginning (*proto*) of a Universal Theory.

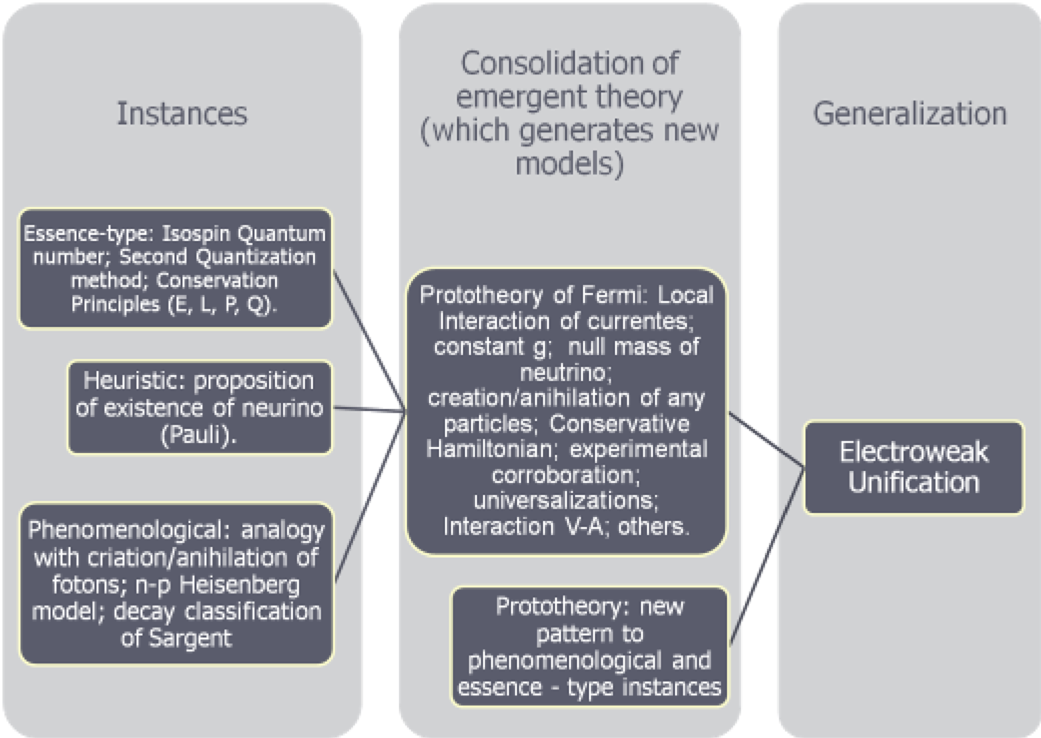
Differentiation between the proto-theory and heuristic models: in opposition to these last ones, which are a mediated intellectual acquisition of the unknown via another known entity, the proto-theory proposes quite new conceptual elements, reported with a new language and new ontology. These elements must be empirically confirmed, and they can be supported by a multi-valued logic, para-consistent logic, etc, having as consequence a new theory.

As a proto-theory, Fermi's elaboration showed originality, heuristic force and effectiveness. In addition, it showed deficiencies and limitations that implicated new improvements from theoretical to empirical studies in Physics. First, Fermi's proto-theory contains inconsistencies according to the classical logic, once it unites relativistic (leptonic current) and non-relativistic (nucleonic current) approaches. Second, in the theoretical structure, we found physical problems because the coupling constant is not renormalizable. However, in spite of its limitations, it is a good answer to the experimental results in low energy (about 300 MeV).

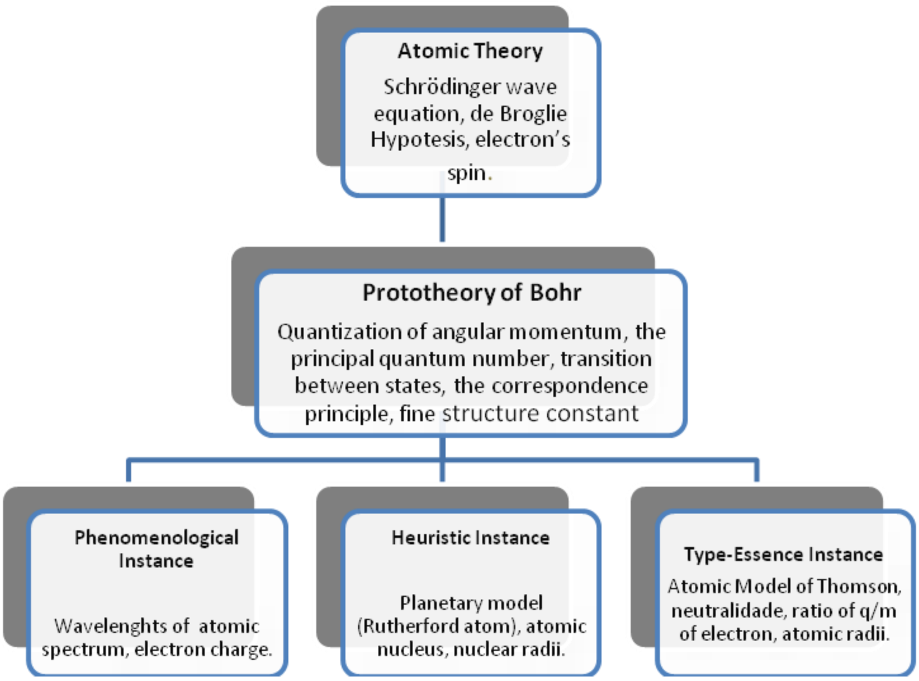
From the Called "Universal Theory of Fermi" (BATISTA, 1999 e 2001):
using an epistemological exemplar to undergraduate course of Physics



From the past to the future



Exemplar produced by undergraduate students after historical-epistemological studies of the schema and applied to Atomic Theory



About the Adoption of the History and Philosophy of the Science in the Development of the Interdisciplinary Knowledge of Teachers

With the incorporation of epistemological analysis and the critical exam of the History of the Science, we are looking for consistent bases, mainly if we want to understand the process of change of scientific knowledge. The Historical-philosophical approach of the Sciences, as a powerful instrument for transformation of educational practice, collaborates with the construction of educational and school knowledge. There are some justifications for such statements:

- Transformation of common sense, of the *doxa*, to a scientific justification, an *episteme*;
  The understanding of concepts, theories and scientific explanations is advisable to aknowledge their history by teachers. Without such understanding, the teacher usually organizes the content mostly without a context, sometimes reproducing graphic representations, symbols and mathematical process without meaning. Other times, this happens with an ingenuous view of experience in the development of a scientific explanation.
- Professional practice begins with the construction of an identity in the educational action;
  The teacher concerned with the History and Philosophy of Science is able to build bases for her/his educational practice. In other words, is able to think it and indeed to transform it, mobilizing her/his knowledge to the construction of educational knowledge. Thinking critically demands a philosophical position from the teacher, and it enables her/him with an epistemological position before the debates and impasses that exist in the studies of the philosophical conceptions of Science. For Kragh (1987), nobody knows a science in full while he/she does not know its History.
- Interdisciplinary production in the contents, providing examples to use at classroom;
  During the historical-epistemological studies of some subject, we observed the dynamical process of evolution of such knowledge and its importance to other sciences. Frequently we do not know how to use concepts in a multidisciplinary or interdisciplinary context. With an approach from the History of Science, the connections and the integration between disciplines reveal themselves spontaneously along the process of the construction of knowledge.
- Promotion of debates on cognitive values of the science;
  There are several areas of philosophy that can be studied, as epistemology, aesthetics, and logic, among others. The cognitive values from axiological studies of science are good sources and appropriate to the themes and contexts, basing discussions in order to promote scientific activity. Axiological studies enrich the education of the teacher once they enable him to understand, for example, the universality degree of a theory and its priority in teaching.

From an understanding of the evolution of sciences and their implications in human culture, the teacher assumes an investigative attitude and transforms a class into a creative atmosphere of new perspectives in sciences.

And research continues...

In conclusion, we sustain the need of continuous research regarding educational knowledge, in initial and in service education of teachers. As well, there is a need of investigated constructions of interdisciplinary approaches, integrating historical, philosophical and didactical studies, which will provide references and justifications, in a contemporary form, to the presence of the History and Philosophy of Science in Scientific and Mathematical Education.

Our findings has shown that the debate regarding historical-philosophical facts as a means of understanding scientific conceptions, looking at the problem of the nature of scientific knowledge from differentiated focuses, is capable to foment a differentiated educational practice. We have demonstrated that they contribute in a significant way to the development of a school knowledge recognized as interdisciplinary, and as such, it should be learned.

References

BATISTA, I. L., ARAMAN, E. M. (2009). Uma Abordagem Histórico-Pedagógica para o ensino de Ciências nas séries iniciais do Ensino Fundamental. *REEC, Revista Electrónica de Enseñanza de las Ciencias*, v. 8, p. 466-489.

BATISTA, I. L. (2007). Reconstruções histórico-filosóficas e a pesquisa em Educação Científica e Matemática In: NARDI, R. (org.). *A pesquisa em Ensino de Ciências no Brasil: alguns recortes*. São Paulo, Escrituras.

BATISTA, I. L., SAMPAIO, H. R. (2007). A Filosofia da Ciência como um Saber Necessário para a Teorização da Prática Docente, CO-34, *Atas, VI ENPEC*, Florianópolis.

BATISTA I. L. (2006). Abordagem Histórico-Filosófica no Ensino e na Aprendizagem de Ciências: Questões Interdisciplinares In: *Filosofia na Universidade*. Ed. Ijuí : Unijuí, v.9, p. 289-296.

BATISTA, I. L. (2004). O Ensino de Teorias Físicas mediante uma Estrutura Histórico-Filosófica. *Ciência e Educação*, vol. 10, no. 3, p. 461-476.

BATISTA, I. L., LUCCAS, S. (2004). Abordagem Histórico-Filosófica e Educação Matemática –Uma Proposta de Interação entre Domínios de Conhecimento. *Educação Matemática Pesquisa*, v.6, p.101 - 133.

BATISTA, I. L. (2001). A Teoria de Fermi para o Decaimento Beta: da sua formulação Inicial à sua Universalização. In: PESSOA JÚNIOR, Osvaldo. (Org.). *Fundamentos da Física 2* – Simpósio David Bohm., v. 2, p. 107-118, Livraria da Física, São Paulo.

BATISTA, I. L. (1999). *A Teoria Universal de Fermi: da sua formulação inicial à reformulação V-A*, tese de doutorado, Depto de Filosofia/FFLCH – USP.

CARVALHO, A. M. P., GIL-PÉREZ , D. (2001). Ensinar a Ensinar. Didática para a escola fundamental e média. In: CARVALHO, Ana Maria P. (org). *O saber e o saber fazer do professor*, São Paulo: Pioneira Thomson.

KRAGH, H. (1987). *An Introdution to the Historiography of Science*. Cambridge University Press, Cambridge, p. 87-107.

NASCIMENTO, E. G., BATISTA, I. L. (2007). A História da Ciência e o vê de Gowin na formação de professores das séries iniciais. CO-41, *Atas, VI ENPEC*, Florianópolis.

SALVI, R. F., BATISTA, I. L. (2008). A análise dos valores na educação científica: contribuições para uma aproximação da filosofia da ciência com pressupostos da aprendizagem significativa. *Experiências em Ensino de Ciências*, v. 3, p. 43-52.

SAMPAIO, H. R., BATISTA, I. L. (2007). Os valores cognitivos presentes nos critérios para a adoção da Trigonometria Plana no Ensino Fundamental e Médio. *Atas, CO-, Atas, VI ENPEC*, Florianópolis.

SCHÖN, D. A. (1995). Formar professores como profissionais reflexivos**.** In: NÓVOA, A (Org) *Os professores e sua formação*. Lisboa: Dom Quixote.

TARDIF, M. (2002). *Saberes Docentes e Formação Profissional*. São Paulo: Vozes, p. 255-256.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 117.

UNDERSTANDING OF PRE-GALILEAN MOTION TRAJECTORY BY PRESENT DAY STUDENTS. CAN AN ANCIENT OBSTACLE BE OVERCOME BY OUR STUDENTS?

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Abstract

A group of pre-service students answered two problems regarding the motion trajectory of a body which falls down, or is thrown, from moving vehicles. Problems were qualitative and in an everyday context. Written answers by students from two different educative levels were analysed, identifying ideas and arguments. Their summary is presented building Systemic Networks. From these networks, and considering the relationship among several of their categories, we can infer students' main difficulties and conceptions related to the comprehension of some aspects of Galilean Relativity, in particular about the motion trajectory relative to different Galilean Frames of Reference. Results show that a non-small group of students understand velocity and trajectory of a body as an absolute magnitude, and give explanations similar to others found in History of Science. A comparison with pre-Galilean understanding will be presented, especially with T. Brahe's, Copernicus', G. Bruno's and Kepler's interpretations. Didactical implications of these results will be commented.

Introduction and proposal

In today's context of science education, we consider that students arrive at school with a background knowledge that, for them, is undoubtedly valid and powerful enough to interpret their surrounding world. Based on the research of students' conceptions in science, we know that part of this knowledge is in conflict with scientific knowledge, and some students are reluctant to reconsider what they think they know. Beside knowledge of these students' conceptions, knowledge of relevant episodes in the development of science can help better understand students' thinking and some specific difficulties that students have in relation to some Physics' topics (Saltiel, 1978, Castells & Konstantinidou, 2008).

A research about students' ideas related to motion trajectory, related to different Galilean frames of reference (henceforth FR)

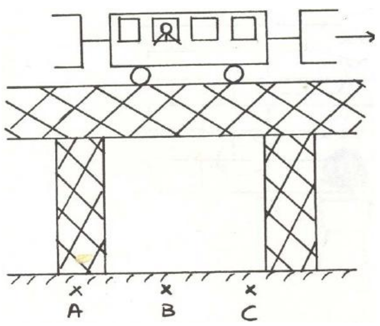
From a wider research on students' conceptions and difficulties in the comprehension of Galilean Relativity (Castells, 1998), and from other researches on students' ideas and reasoning related to this topic (Saltiel, 1978, 1980; Aguirre & Erickson, 1984; Lie et Al., 1985), we identified the main difficulties students have in relation to Motion Relativity. Saltiel (1980) thinks that the results obtained could be well accounted for the existence of an organized system which she called the 'Natural model' as opposed to the 'Kinematics model of the physicists'. The study of Panse, Ramadas & Kumar (1994) and Ramadas et al. (1996) identified students' ideas related to Galilean Kinematics. A common conclusion is that there is a Pseudo Relativity in the students, in which magnitudes are not considered relative to a FR, and that the FR notion is near to the object notion, along with properties of specific objects. McCloskey (1983) and Whitaker (1983) focused mainly on the students' difficulties related to an object trajectory of drops from a carrier in motion.

In the specific study we present here, we focus on ideas about the motion trajectory relative to different Galilean FR's.

Information is collected from the written answers of individual students to two problems which are very similar from the Physics point of view.

The sample are students from Primary Education Teacher Training (124) in the University of Barcelona from three levels: Science 3rd course (51), Science 1st course (28) and Non-Science 1st course (45).

Problem 1: A train passes through a bridge below which there are some workers making several arrangements. There are three groups of workers a little apart among them. An object falls down from a boy on the train whose hand is outside the window. If you were near the place where the men were working and you were afraid that this object could damage them,



Direction of the train's motion

Which group would you warn?

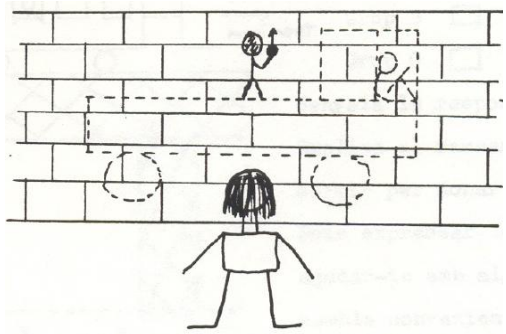
Group A

Group B

Group C

Mark your answer here and explain your reasons for it. You can express your answer either in words or with the help of a drawing, if you consider it more convenient.

Problem 2: A boy is playing on the platform of a lorry that moves at constant velocity. The lorry passes behind a wall which completely hides if from an observer on the other side of the wall. While the lorry passes behind the wall, the boy throws a ball up vertically.



Direction of the lorry's motion

a) Draw and explain the trajectory that the ball will follow from the perspective of the observer on the other side of the wall.

b) Do you think that the ball that has been thrown up by the boy will fall down back to his hands? Explain your reasoning.

Are problems similar to the ones proposed in our research discussed in ancient times of science development? Our problems are related to the history of acceptation of the Earth motion, which was not accepted in the ancient Ages, and as we know from HC, it was a very big "epistemological obstacle" to overcome (Bachelard, 1939). So, we will revise in the following paragraphs some points of the historical development of the Relativity Principle; the summary will focus mainly on the case of the acceptation of the Earth motion and of the trajectories relativity.

Some points of the History of Galilean Relativity that relates to our research on students' ideas and conceptions on this topic[1]

In the most ancient times Aristotle and Ptolemy (and Aristotelians) defended the impossibility of the Earth motion with some arguments from observation like: 'objects dropped from the upper part of a tower or from the ship's mast arrive to its foot', 'the object thrown vertically up from the Earth or from the ship's deck arrives to its base', 'clouds and birds, and objects and buildings on the Earth are not thrown away'.

Nicolaus Copernicus (1473-1543) in his book *De revolutionibus orbium caelestium* (1543) presented the cosmologic model with the Earth revolving around the sun and on its own axis. Copernicus reasons in terms of 'natural motion'; to Copernicus, this is obvious, and needs no demonstration. With the idea of participation in the Earth's motion, Copernicus could explain why we see things falling vertically from the top of a tower; he said we cannot notice the motion of these things, like the Earth motion, because we participate in the Earth motion too.

But to Aristotelians and to Tycho Brahe (1546-1601) this reasoning was weak. They asked: "Why do these two natural motions (the falling down motion and the one like the Earth motion) not disturb each other?" To Aristotelians, a body can only participate in one natural motion; it was like an accepted truth. It was Brahe who proposed the famous experience of the cannonballs, and proposed it in order to demonstrate the impossibility of the Earth motion, but he looked for a more 'modern' example, more of his days (the cannon was a recent invention in his time), than the ones that had been repeated for long from Aristotle and Ptolemy. Brahe (1592) proposed this new example answering Copernicus' argumentation saying:

> "…but what will happen, I ask, if a cannonball is fired to Orient with a big cannon and from the same place another on is fired to Occident? Can we think that the one as well as the other will run the same distance on the Earth?" (Tycho Brahe, 1592, *Epistolarum Astronomicarum liber due*, Uranieburg)

He didn't think this, and answered himself:

> "...By reason of the extremely fast Earth's diurnal motion (if it were), the cannonball thrown to Orient will never be able to run as much distance on the Earth, the Earth (because of its own motion) going ahead of it, as the one that, like the first, were thrown to Occident..." (Tycho Brahe, 1592, *Astronimicarum epistolarum liber*, Uranieburg)

Against Copernicus' reasoning, Brahe says that the 'natural motion' notion (in opposition to violent motion) doesn't admit a body with two different natural motions, and he adds that these two motions must disturb each other. With this thinking, Brahe follows Aristotle's ideas. Brahe doesn't accept the Copernican theory that states that any 'terrestrial' object must follow the Earth motion. He thought that, if it was so, and considering the cannonballs' motion caused by participation in the Earth nature, then, if the Earth was in rotation, the two cannonballs would run different distances in each direction, but because this doesn't occur, it means that natural motion by participation of the cannonballs doesn't exist, and the cannonballs only have the violent motion of firing. In fact, Brahe thinks about the motion of a body in an absolute way. As the cannonballs had, according to Brahe, their own velocity, independent of the Earth motion and the observer, the cannonball shot to Orient would be in disadvantage relative to the one to Occident because in the first case the Earth is moving in the same direction and in the second case in the opposite direction.

Giordano Bruno (1548-1600) went further than Copernicus and explained this participation in the Earth motion. He gave us the notion of Mechanical System (1584). This is 'an ensemble of bodies animated with the same motion, but it is as if this motion in which they participate did not exist'. Using this notion of Mechanical System, he could refute Aristotelian reasoning of clouds and birds against the Earth motion, and he explained why we see a body falling down vertically from the top of a tower and why a body thrown vertically upwards from a point of the Earth will fall vertically down to the same place. This is because they are part of the same Mechanical System due to their participation in a common motion. This notion of Mechanical System didn't have any meaning to Brahe and to Aristotelians. Bruno reached the following general conclusion:

> "It is impossible to recognize the motion of a Mechanical System from experiments made inside it". (Bruno, 1584, *The Ash Wednesday dinner, La Cena della Ceneri*, third dialogue)

Despite these ideas, Bruno didn't solve the problem completely –we could still ask why the cannonballs conserve the cannons motion in which they participated when they were part of the same Mechanical System. Bruno uses the notion of 'impetus' to explain this motion conservation, but this is an ancient notion; it doesn't lead towards the idea of inertia. We have to wait until Galileo. To Bruno, a body needs an action (impetus) to persevere in its motion. This is different from the

[1] This review of HC in relation to our Physics' problems is based on Koyré, A. (1939, 1957, 1961); Tonnelat, 1974; Saltiel, E. (1978), Bernardini, C. Ulisses Vol7 (1979) and some translations of books of the ancient scientists.

Classical Physics of Inertia. In summary, Bruno has a good appreciation of Motion Relativity, but he doesn't explain it enough. The notion of impetus cannot justify the notion of Mechanical System.

And now, only two words to situate Kepler's thinking. Johannes Kepler (1571- 1630) in some aspects goes backwards from Bruno. To him, the Mechanical System rests necessarily linked to a physical action. The separated bodies of the Earth don't follow it because are part of the same Mechanical System, but because there is a real attractive force between the Earth and these bodies (the Gravity force). The rock follows the Earth because it attracts the rock; this is not a mechanical state but a real physical force which is necessary to win the rock's inertia. To Kepler, inertia is like resistance.

In the *Dialogo sopra i due Massimi Sistemi del Mondo Tolemaico e Copernicano* (1632), Galileo Galilei (1564-1642) wanted to demonstrate the Earth motion, but he was not able to find arguments to prove it, so it was necessary, first of all, to refute arguments against this motion, specifically, that if the Earth rotated we would perceive it in everything. With this aim, he enunciates the general thesis that the common motion of bodies constituting a Mechanical System has no influence on the behaviour of these bodies in their relations between each other and so, this motion can't be demonstrated from this frame. We can find a very clear example of this thesis in Galileo's text from the Dialogues, 'From Venice to Alep' (Galileo, pp. 171-172), in which Venice, for example, and also Alep, can be used as a reference that doesn't participate of the ship's motion. For the study of the ship's motion, the Venice and Alep references, which are at rest one relative to the other, are equivalent. This will seem obvious to anybody, but for the study of the butterfly motion, referring to Venice or to Alep does not seem so obvious. Relative to Venice, anyone could say that the butterfly's motion is double: on a hand, it moves around from here to there, and on the other hand, it moves from Venice to Alep. Venice can be used as referential for both motions because it doesn't participate of any. But on the contrary, if we refer the butterfly motion to the ship, only the motion of moving around, which the butterfly doesn't share with the ship, can be studied. As for the other motion, the one shared by both the butterfly and the ship, it can't be studied in the ship reference, as it would result in 'nothing'. The question that we can ask, then, is the following: Do we obtain the same description of the butterfly moving around motion from the Venice reference or from the ship reference? Galileo's answer, which is not intuitive, is 'yes'. The reason is that the ship motion is uniform related to Venice, and this uniform motion, which is like 'nothing', doesn't make any change in the moving around motion of the butterfly. For the butterfly motion, both references in relative uniform motion are equivalent and will give the same description of the butterfly moving around motion. In this text, in fact, Galileo brings us the intuitive concept of FR, and the Classical Relativity Principle (Physics laws are the same in any FR that is moving in a uniform motion) are clearly expressed in another very beautiful and famous text also from the Dialogues (Galileo, 'The second journey', page 188).

Galileo uses the indefinite conservation law, uniformity and persistence of motion if no compulsion to stop it exists – the *Law of Inertia*– and the Relativity Principle. The thesis is that a motion is conserved, not because it is natural, but because it is *motion*. We know we can't assert that the Classical Relativity Principle is entirely due to Galileo, but it is still named the Galilean Relativity Principle. The limitation of Galileo's Inertia Principle to horizontal motions, which he considered on the Earth surface, transforms these horizontal motions into uniform circular motions around the Earth centre.

With this historical revision, we can say that the Classical Relativity Principle has been present in Physics long before it had been adequately formulated; it is not the discovery of a single man. The beginnings are due to Bruno, Copernicus, Kepler, Galileo and Descartes, who contributes to the Law of Inertia; followed by Huygens, Newton and Euler which in the 18th century recognizes the validity of the Relativity Principle to the entire field of Mechanics and even for the whole of Physics.

The analysis in our research

The analysis of the students' answers to the two problems previously presented includes two parts: 1) a deep analysis of the students' answers to each problem and 2) an analysis of comparison and relationships between the various aspects of the answers to both problems.

The problem to problem analysis begins with a **qualitative analysis** to the interviews we have done to some students and to the individual written answers. We try to establish some categories and subcategories, some of which may be mutually excluding while others may be produced at the same time. We collect this categorization structure using the Systemic Network technique (Bliss, Ogborn & Monk, 1983). After this qualitative data analysis, we perform a **quantitative data analysis** of both problems and its comparisons based on categories and subcategories of Systemic Networks.

1. Analysis of answers to problem 1 and problem 2

Because both problems are very similar from the Physics point of view, we will present the analysis of both problems together. At first, we interviewed two students immediately after they had answered the problems. Interviews have been of help to build the network from the written answers. We include and comment below some of the parts of these systemic networks that correspond to the big categories found in both problems.

The first big category is **Drawing**. Here we classified the students according to the **making or not making of a drawing** criterion, and according to the **type of drawing**. We don't present the network here.

The second category is type of **Answer** (problem 1 and problem 2), in which we classify the students according to the chosen answer. In problem 2 we also have the big category of type of **Answer to question b).** We represent here the systemic network of problem 2, which includes the network of problem 1 because the categories of the Answer to problem 1 are exactly the same of the Answer to problem 2a).

- Answer (Aa) problem 1 and problem 2a)
  - Vertically (Aa2)
  - Backwards (Aa3)
  - Two options (Aa4)
  - Not clear (Aa5)
  - No answer (Aa6)
- Answer (Ab) problem 2b)
  - Yes (Ab1)
  - No (Ab2)
  - No answer (Ab3)
  - Not clear (Ab4)

Fig. 1: Network of Answers to problem 1 and problem 2.

In our Teachers sample, the biggest percentage corresponds to the **correct** answer, but the percentages of **incorrect** answers (vertical and backwards) are also high. Students with higher scientific level answer better than non-scientific students. This problem is similar to the problems students usually solve at Physics classroom, so these results are not surprising.

In problem 2 a) the biggest percentage of students **answer Aa2 (vertically)**, an incorrect answer. But by levels, the biggest percentage of correct answers is at Science 3rd level, as in problem 1. Students with less or none scientific background **answer Aa2 (vertically)**, an answer that coincides with the ancient ideas we commented in the HC –these are pre-Galilean students. Most of the students **answer question b) in problem 2 incorrectly** at all levels. That means the scientific background students have is not useful to answer correctly this question.

The third big category is **velocity of the object/ball.** We include below the network of this big category**.** It would seem logical that, if students say that the object/ball will go forward, they must bear in mind that it should be at exactly the same **velocity of the train/lorry**, but it is not true in all the cases. Thus, we introduce a sub-categorization in order to count students that have a good understanding of this problem. (See fig. 2).

Velocity of the ball (V)

- Velocity in the carrier's same direction (V1)
  - The same velocity (V11)
  - Different speed (V12)
  - Not clear - the same speed (V13)
- Velocity not in the carrier's same direction (V2)
  - Opposite direction (V21)
  - No velocity (V22)
  - Not clear (V23)
- Not clear (V3)

Fig. 2: Network of Velocity of the object / ball in problem 1 and problem 2.

The fourth category is the **Type of Justification** students give when answering the problems. According to the general Physics' concept they use to justify their choice, we classify the students' justifications in five big categories that also have some subcategories (See fig. 3).

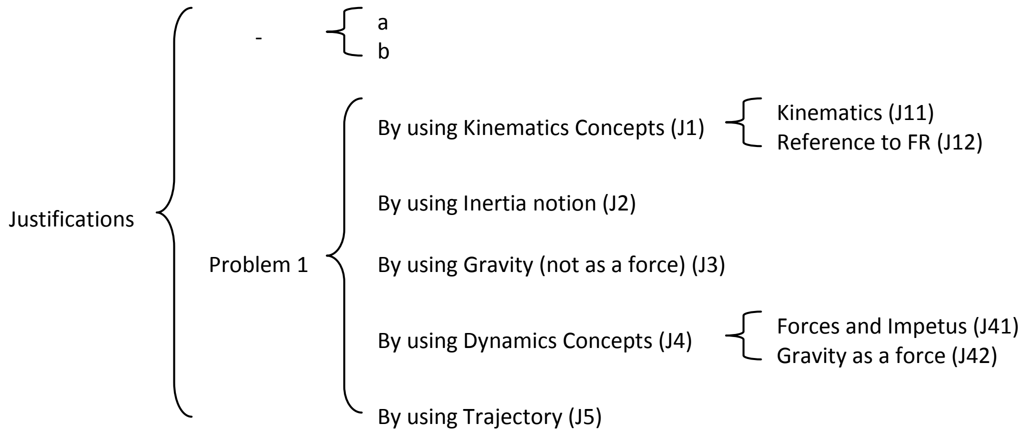


Fig. 3: Network of Types of Justifications answering problem 1 and problem 2.

3. Comparison between problem 1 and problem 2a)

After the analysis of each problem we will compare the students' answers to the two problems. We'll do this comparative study from the categories of the systemic networks.

The first category we compare is **Type of answer** (We won't count here **Not clear** answers).

Table 1: Comparison of types of answers.

Answer to problems 1 and 2a)	Forward	Vertically	Backwards	Not answered
Total problem 1 (122)	41.8 (51)	27.1 (33)	27.0 (33)	4.1 (5)
Total Problem 2a) (115)	33.0 (38)	47.8 (55)	16.5 (19)	2.6 (3)

The results collected in the table indicate that students interpret differently these problems, which are very similar from the Physics point of view.

We also compare the categories of the **velocity of the object/ball when it leaves its carrier in motion**. This comparison says that, mainly, students don't attribute velocity in the direction of the carrier's motion to the object that separates from it, and this category is significantly higher in problem 2 than in problem 1.

To compare the students justifications, we don't consider justification of **Trajectory** because most of these justifications can be considered as a **Kinematics** justification, thus we have a new table of **Reduction of justifications** with only five excluding categories. There are differences in the percentages related to the categories in the two problems. From the differences found in the results of the two problems, we can infer that students interpret each problem in a different way, and they find problem 2 more difficult than problem 1, but in both cases non-scientific students answer both problems worse than scientific ones.

Table 2: Reduction of justifications, excluding categories.

Justification. Summary	**Jd1 Kinematics**	**Jd2 Inertia**	**Jd3 Gravity**	**Jd4 Dynamics**	**Jd6 Not justif.**
Total problem 1 (117)	53 (62)	6 (7)	4.3 (5)	35 (41)	1.7 (2)
Total problem 2a) (112)	40.2 (45)	3.6 (4)	1.8 (2)	13.4 (15)	41.1 (41)

Comments and discussion

The performed research with these specific problems proposed to students has been very useful to evidence that nowadays students participate of many ideas and conceptions identified in pre-Galilean history of science. In particular, many students have a non-relative conception of motion trajectory and velocity; many of them have an absolute conception of these scientific entities.

Students mainly answer problem 1 better than problem 2. This result may be interpreted by the fact that this first problem is more similar to problems that students use to solve in Physics classroom. A very relevant characteristic of these problems is that, while through the question proposed in problem 1 students have to use a FR at rest, in problem 2, that has two questions, students have to use a FR in motion (relative to the observation point) and also a FR at rest.

An interesting comment we can do from these results is that only with question b) in problem 2 have we been able to really know about the students comprehension of the physical situation in which an object drops or is thrown away from a moving system. This interpretation brings us to the importance and relevance of some particular characteristics of problems or questions proposed to students.

Implied from research and comparison with HC, is that the HC would have to be included in the Teacher Training curriculum. Researches done tell us that the topic of Galilean Relativity is not easy, students have to change their own way to see the world in a new way that is against their common way of thinking. This can never be easy, and is more a persuasion issue than a discovering. Thus, teachers will have an essential role in convincing students, and the HC is an important source of new approaches and also of specific ideas for the design of appropriate activities, and also new multimedia resources can be useful; but without an adequate orientation and scaffolding by the teacher, convincing of these new ideas will be quite impossible.

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References

AGUIRRE, J. and ERICKSON (1984) Students' conceptions about the vector characteristics of three physics concepts. In: *Journal of research in Science Teaching*. 21 (5), pp 439-457.

BACHELARD (1939) *La formation de l'esprit scientifique*. Librairie Philosophique. J. Vrin.

BLISS, J., OGBORN, J & MONK, M., (1983) *Qualitative data analysis for Educational Research. A guide to users of systemic networks.* London: Croom Helm.

CASTELLS, M. (1998) *Patrons de comportament dels estudiants en resoldre problemes de relativitat galileana, i factors que els influeixen en les respostes i en els raonaments*. PhD., Servei de Publicacions, U.A.B., Bellaterra , Barcelona.

CASTELLS, M. KONSTANTINIDOU, A. (2008) The use of Galileo's Dialogue to obtain interesting questions and arguments to convince students in the Physics' classes. In: *Styles of thinking in Science and Technology*. The Third ICESHS Austrian Academy of Sciences, Vienna pp: 822 – 833.

MCCLOSKEY (1983) Intuitive Physics. In: Scientific *American*, 4 (248) pp114-122.

DRIVER, R. et al.(1996) *Young people's images of science*. Open University Press, Buckingham.

Galileo GALILEI (1632): Dia*logo di Galileo Galilei Linceo sopra i due Massimi Sistemi del Mondo Tolemaico e Copernicano*, English translation (1966) *Dialogue Concerning the two Chief World* Systems, with foreword of Einstein, A., translated by Stillman Drake (1962) Berkeley: University of California. USA.

KOYRE, A. (1939) *Études galiléennes*, Paris: Hermann, 1939.

KOYRE (1961) *La Révolution astronomique: Copernic, Kepler, Borelli*, Paris: Hermann.

LEACH and SCOTT, 1995, The demands of learning science concepts: Issues of theory and practice. *School Science Review*, 76 (277), 47-51.

LIE, S., SJOBERG, S., EKELAND, R. and ENGE, M. (1985) Ideas in Mechanics, a Norwegian study. *The many faces of teaching and learning mechanics in secondary and early tertiary education* GIREP/UNESCO, pp255-276.

OGBORN et al. (1996) *Explaining Science in the clasroom*. Open University Press, Buckingham.

PANSE, RAMADAS and KUMAR (1994) Alternative Conceptions in Galilean Relativity. Frames of Reference. *International Journal of Science Education,* 16 (1), pp 63-82.

RAMADAS et al.:.1996, Alternative conceptions in Galilean Relativity; distance, time, energy and laws. In: *International Journal of Science Education* 18 (4), 463-477.

SALTIEL, E. (1978) *Concepts cinématiques et raisonnements naturels: étude de la compréhension des changements de référence galiléens par les étudiants en sciences*. These Doctoral d'Etat. Université de París VII.

SALTIEL, E., Malgrange, J.L. (1980) Spontaneous' ways of reasoning in elementary kinematics, *European Journal of Physics* 1, pp73-80.

TONNELAT, M.A.: 1974, *Histoire du Principe du Relativity*. Paris: Flamarion.

VIENNOT, L. (1996) *Raisonner in Physics. La part du sens comun*. Practiques Pedagogiques. De Boeck and Larcier. Paris.

WHITAKER, R. (1983) Aristotle is not dead: Student understanding of trajectory of motion. *American Journal of Physics* 51 (4) pp: 352-356.

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MORE HISTORY OF CHEMISTRY, MORE INTEREST IN SCIENCE

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Abstract

A periodic table of elements, following the model of Andreas von Antropoff, was painted in 1934 in a classroom at the University of Barcelona's oldest building. It still has great historical value and has just been recently restored. So, in 2009 it was proposed by the Catalan Board of Chemists, Col·legi Oficial de Químics de Catalunya*, on St Albert's day that secondary school students worked the History of Chemistry preparing and discussing both Andreas von Antropoff's and the 150 years of Mendeleev's periodic table, together with their historical implications. In this study we expose the Catalan Board of Chemists' project disseminating the history of chemistry within Catalonian secondary schools.*

Introduction

Catalonia is one of the 17 Spanish Autonomous Communities with its own independent educational system[1]. So, most of the Spanish and Catalan students from primary and secondary school do not appreciate the impact of science on different aspects of our lives: health, nutrition (Genetic Modified Organisms), new technologies (rockets, computers, nanotechnologies), etc. And they also don't appreciate the impact of the history of science from antiquity to the present day, from the discovery of fire or the heliocentric theory to the laws of relativity. In our context, chemistry students in secondary school cannot appreciate the impact of the history of chemistry from the ancient times with the utilization of metals like copper, iron or gold to the synthesis of drugs or from Democritus' atom proposal to nuclear reactions.

[1] See: www.xtec.cat/estudis/eso/curriculum_eso.htm (Catalan science curricula from compulsory secondary schools, students 12-16 years old) and, www.xtec.cat/estudis/batxillerat/curriculum_bat.htm (Catalan science curricula from secondary schools, students 16-18 years old). [accessed October 29, 2010]

We want to change this adverse situation by the history of chemistry. Usually, the relationship between the history of science and young students is not easy. However, quoting Sir H. Butterfield[2], we cannot increase student's chemistry motivation or a tolerable scientific society without the history of chemistry. We should enhance the incidence of the history of chemistry in classroom. But, how can we do it?

Initially, it is known that there are some young students with not enough interest in science; this is a common problem around the World. What is the reason for this lack of interest in science between teenagers? Nowadays there is more than one reason.

In general, a lack of motivation following several and general studies are presented in our youth; students simply do not have enough motivation for socials, languages, laws, science or in our context for chemistry and its history. In addition, chemistry has a bad image in society; chemistry and science have often been a topic in mass media news, from the printed word to television. However, bad news are more remarkable in media that good news. Unfortunately, secondary school's science students are affected by these circumstances.

To increase students' motivation and make science and its history more enjoyable and understandable, several activities have been suggested correlating theoretical explanations with historical references in the classroom. In this way, many examples could be observed; authors presuppose that readers and science teachers could add several examples:

- Theatre is a good tool used by some teachers to introduce historical science events to students by themselves[3] or by a theatre company[4].
- Cartoons are also used to increase science's motivation in young students. Leopoldo de Meis' books[5] could be used in primary and secondary schools.
- Biographies are also prepared and read in the classroom by teachers and their students. Some examples are Copernicus, Lavoisier, Mendeleev or Watson[6] and others.
- Science museums organize shows about the history of science aimed at young students and general audiences. Only two examples of museums: Oxford Museum of the History of Science[7] with shows like "Observing the Universe", "Penicillin: the Wonder drug" and "From Alchemy to Chemistry". And the Milan Historical Museum[8] that is pleased to offer several Educational Programs with a curriculum in accordance with the Educational Guidelines.
- Finally, radio and digital media could easily be used as a diffusion instrument to give a new and balanced perspective to the history of science[9].

All of these activities are very important to make chemistry and its history understandable for every body, but taking each one of these separated approaches is not enough to stimulate students. If teachers want to have a stronger insight into the relationship between the history of chemistry and young students they will have to apply new educational procedures.

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[2] Herbert Butterfield (1965). *The origins of Modern Science*. A free press paperback Macmillan Publishing Co. Inc. New York. On this point, his words are instructive "We cannot construct a respectable history of Europe or a tolerable survey of western civilization without the history of science".

[3] Carme Zaragoza and Josep M. Fernández-Novell (2007) Young students turn the History of Science into an educational theatre. The *Global and the Local: The History of Science and the Cultural Integration of Europe*. M. Kokowski Ed. p. 143-148.

[4] Kinetic theatre for science: www.kinetictheatre.co.uk/ [accessed 10 November 2010]

[5] Leopoldo de Meis and Diucênio Afonso Rangel (1998) *A respiraçâo e a 1ª lei da termodinâmica*. Respiration and the 1st termodynamic law. 2ª ediçao. Leopoldo De Meis Ed. Universidade Federal do Rio de Janeiro. And also, Leopoldo de Meis and Diucênio Afonso Rangel (2002) *O Método científico*. The Scientific method. 3ª ediçao. Leopoldo De Meis Ed. Universidade Federal do Rio de Janeiro.

[6] For Copernicus biography see Manuel Yañez Solana (2006) *Copernico. Grandes biografías y series*. Copernicus. Great biographies. Spanish edition. M. E. Ed. Madrid. Spain. For Lavoisier biography see Soledad Esteban Santos (2002) *Introducción a la Historia de la Química* (Introduction to the History of Chemistry), UNED Editions, Madrid, Spain. For Mendeleev biography see Pascual Román Polo. (2002) *Mendeléiev. El profeta del orden químico* Mendeleiev The chemical order's prophet. Nivola Editions. Tres Cantos. Spain. And, for Watson see James Dewey Watson, (1978) *The double helix.* Spanish translation by Adolfo Martín. Plaza & Jané Ed. Esplugues de Llobregat, Spain.

[7] See www.mhs.ox.ac.uk/events [accessed October 27, 2010]

[8] See www.milanhistory.org/Education [accessed October 27, 2010]

[9] See Manoj Patairiya (2009) Science Communication through Digital Media: An Indian Perspective. *Proceedings of the 67th International Conference on Hands-on Science*.Costa MF, Dorrio BV, Patairiya MK (Eds.). pp. 1-10. And also Josep M. Fernàndez-Novell & Carme Zaragoza (2010) Broadcasting Science: a new bridge between science and society. M. Kalogiannakis, D. Stavrou & P. Michaelidis (Eds.). pp. 57-62.

When science teachers talk about science and its history in the classroom usually they focus mostly in its past, but too many students feel these things are not linked with them and not linked with the present day. For example, the discovery of the DNA structure (middle 20th century, 1953) or when the first humans walked on the Moon on 21st July 1969. These events feel like a long time ago for students although they happened when their parents probably were still children. It is very difficult for students understand these contexts because they are unable to put themselves in the past and think about these situations.

Furthermore, if teachers want to motivate secondary school students into enjoying science and its history they need to find some science activities which take place closer to the student's surroundings.

In this way, to increase student's understanding and motivation towards chemistry we need to find links between the history of chemistry and the present day. Some of these links are related with:

a) Science and street names.
Teachers could prepare exercises for students to find near to their homes, in Barcelona and other cities, many street names that are related to famous chemists and scientists, for example Marti i Franquès street surrounding the Faculty of Chemistry, the Puigvert gardens, the silver jewellers' street, etc.

b) Science Nobel Prize winners that visited the city.
Another interesting thing that teachers could do is to explain and organize different activities about the times when some of the most important scientists visited the students' cities. Catalan science teachers could prepare several works on Sir Alexander Fleming, Albert Einstein or other Nobel Prize winners when they came to Barcelona or came to their place. Working on this, students could obtain pictures from internet or local newspapers to recall that past meetings and put themselves in those times.

c) Promoting science conferences.
In this way it could be interesting that secondary school students could work on Sir Harold Kroto or/and on professor Josep Font conferences[10]. In the event organized by the Faculty of Chemistry from the University of Barcelona, as part of the International Year of Chemistry, Sir Harold Kroto, winner of the Nobel Prize in chemistry 1996, has been invited to give the talk "Carbon in nano and outer space" about fullerenes or buckyballs (they resemble balls used in soccer). Furthermore, several lectures delivered by chemists were organized by the Autonomous University of Barcelona, as part of the International Year of Chemistry in which secondary school science teachers were invited to participate with the aim of bringing chemistry closer to everyone. The first conference was "Molecules combating diseases and restoring health" by professor Josep Font, and it was about the history of medicines like quinine, aspirin, penicillin or AZT.

All these links could become an important tool to teach the History of Chemistry in primary and secondary levels and to motivate young students. We present in this article a collaboration between the Catalan Board of Chemists (CBC)[11] and the University of Barcelona for spreading chemistry and the History of chemistry to Catalan science secondary school students.

What is the project?

Our aim is to put emphasis on chemistry and history of chemistry understanding in secondary school classrooms. With this in mind, on Saint Albert's day 2009[12], the Technical Section of Education from the CBC in collaboration with the Faculty of Chemistry from the University of Barcelona prepared some activities directed to secondary school students about the classification of elements that link the history of chemistry to the present day.

To relate to the history of chemistry, it is necessary that we go back to 1934, to classroom 111 in the historical building of the University of Barcelona. In this classroom, after a proposal by professor García Banús, a periodic table was painted on one of the walls. This periodic table had the same structure, symbols and colours than the periodic table from Andreas von Antropoff[13].

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[10] The Nobel Prize in Chemistry 1996 was awarded jointly to Robert F. Curl Jr., Sir Harold W. Kroto and Richard E. Smalley *"for their discovery of fullerenes"* and professor Josep Font i Cierco, chemistry from the Autonomous University of Barcelona. http://www.uab.es [accessed February 27, 2011]

[11] The Catalan Board of Chemists (CBC), *Col·legi Oficial de Químics de Catalunya*. www.quimics.cat [accessed 20 october 2010]. In addition, the Catalan Board of Chemists' publication is named News for Chemists, *Notícies per a Químics*.

[12] Saint Albertus Magnus or Doctor universalis was declared in 1941 by Papal decree the Saint Patron of all who cultivate the natural sciences. In Spain Saint Albert's day is the 15th November.

[13] On this point see Eric R. Scerri, (2007) The periodic table: its Story and its Signifiance. Oxford University Press, Oxford; Eric R.Scerri (2009) *Selected papers on The Periodic Table*. E. Scerri Ed. University of California, Los Angeles, USA. And also, Claudi Mans (2009)

In addition, at that time in this classroom, professor García Banús put up a lecture hall with a big laboratory table with water facilities to be able to set up and present chemical experiments in front of the students. This allowed chemistry teachers to have a lecture and also to perform experiments while giving theory explanation. It was the first lecture hall used as a laboratory classroom in the University of Barcelona.

As time passed and the painted periodic table colours were fading, a restoration was needed. When we moved forward to the 21st century, the University of Barcelona decided to restore von Antropoff's periodic table. On 3rd April 2009 this renovated periodic table was shown again to the university members and general public[14].

This important restoration was combined with the celebration of the 150th anniversary of the first presentation in society of the "periodic table of the chemical elements" by Dmitri Ivanovich Mendeleev in 1859, one of the most important episodes in the history of chemistry[15].

These two events gave CBC in collaboration with the University of Barcelona the opportunity to prepare some activities around Saint Albert's day to involve secondary school students, chemistry and its history. These activities tried to engage the majority of secondary school chemistry teachers so that they could motivate more and more secondary school students.

The history of chemistry can raise new options to secondary school science teachers to increase science knowledge to many secondary school students. It is necessary to continue building bridges between chemistry and its history or between science and its history[16]. With this approach, we get more teachers involved with the history of chemistry explanations.

Methodology

The CBC and the University of Barcelona sent a letter, addressed to the chemistry teaching staff of secondary schools, proposing two different activities to commemorate Saint Albert's day.

First activity

The first activity was developed in science classrooms. Students had to work on the periodic table of the chemical elements and its history starting before Mendeleev's periodic table (an organization of macroscopic chemical properties of the chemical elements according to their atomic weight), continuing with Andreas von Antropoff's periodic table (an organization of the chemical elements according to their atomic number) and finishing with the standard periodic table that present day students use (chemical properties are an approximate manifestation of the electronic configurations of the atoms of the elements). For a review see Eric R. Scerri 2009, footnote number 13.

Teachers and students could work on the general vision of the periodic table of elements from the Zaragoza & Fernández-Novell's text[17], that was sent in pdf format to secondary school science teachers. In this article Mendeleev's periodic table was used to teach chemistry and to discuss the properties of the elements predicted before they were discovered. The study of Andreas von Antropoff's periodic table was supported by a Claudi Mans' article[18] in the magazine *Notícies Per a Químics,* News for Chemists, edited by the CBC. In this article, which was also sent to all secondary school chemistry teachers, the history of the periodic table painted in the University of Barcelona, its restoration and the recent history of chemistry from the University of Barcelona was exposed.

La taula periòdica de l'edifici històric de la universitat de Barcelona. *Notícies per a Químics*, 446, 5-10. Furthermore, you can visit Claudi Mans' web site, www.angel.qui.ub.es/mans/Documents/Textos/ [accessed July 16, 2010]

[14] See Josep M. Fernández-Novell (2009) La taula periòdica de l'aula García Banús The Periodic Table from García Banús' classroom. *Notícies per a Químics*, 446, 11-12.

[15] See Isaac Asimov (1985) *Nueva guía de la ciencia*. Plaza & Janés Editores. Barcelona. Translation from Asimov's *New guide to science*. Basic Books, Inc. New York 1960 and Isaac Asimov (1986*) La búsqueda de los elementos*. Plaza & Janés Editores. Barcelona. Translation from *The search for the elements*. Basic Books, Inc. New York (1962).

[16] See Carme Zaragoza & Josep M. Fernández-Novell (2006) Bridging the gap between secondary school and the History of Science: an educational experience. In: M. Kokowski (Ed.), *The global and the local: The History of Science and the Cultural Integration of Europe*. Proceedings of 2nd ICESHS (Cracow 6-9 September 2006). pp. 160-165.

[17] See Carme Zaragoza & Josep M. Fernández-Novell (2008) Teaching Chemistry Through History: The Importance of The Periodic Table. In: Bertomeu, Thorburn, Van Tiggelen (ed.) *Neighbours and Territories the Evolving Identity of Chemistry* , Proceedings of the 6th International Conference on the History of Chemistry.(Leuven, 2007) Belgique, pp. 685-693.

[18] See footnote number 13.

Second activity

The second activity took place in classroom 111 of the historical building of the University of Barcelona, where the restored periodic table and its history was showed, presented and explained to everybody. This presentation was performed in front of secondary school chemistry teachers, theirs pupils and the general public who attended the session.

Results

Participation

All data in the table were obtained from the information that the secondary school teaching staff had sent to the organizers.

The welcome and participation of schools in these activities were important. As it can be observed in this table, the participation was of more than 1500 students, boys and girls, between 15 and 18 years old. Doubtlessly, they transmitted their motivations and interests in chemistry and its history to their parents and friends.

Table 1. Results 2009

Participating Schools	54
Secondary School Students	547
High School Students	971
Total.	**1518**

The second activity took place in classroom 111 of the historical building of the University of Barcelona, and it was related to the presentation of recent history of chemistry to students and general public. The considerable participation of secondary school science teachers and students in this event showed the importance of linking the evolution of chemistry in the city of Barcelona, the University of Barcelona and secondary school students from Barcelona.

Working questions

Several examples of questions posed and worked in classroom by secondary school chemistry teachers are presented:

- How can you explain the differences between von Antropoff's and Mendeleev's periodic tables? Remember that in Mendeleev's times chemists didn't know about the atoms electronic configuration (teacher's suggestion).
- Who were Newlands, Döbereiner and Meyer?
- Could you write a little story about the classification of elements?
- Could you write a little story about the discovery of elements?
- How was chemical knowledge passed on from generation to generation?
- Controversies between the ancient idea of element and 19th century element classification.
- Which concepts guided chemical investigations?
- Which were the last elements found by Seaborg?

Each teacher worked on these activities in the best way for their students. So, as well as Mendeleev and von Antropoff's periodic tables, pupils worked on Döbereiner's "triads", Newlands' "octaves law" and Meyer's "elements classified by valence". They have to answer some questions about the history of the atom, the elements and the periodic table. These questions are related to the students' level: last years of compulsory secondary school students (15-16 years old) and non-compulsory secondary school students (17-18 years old).

It is important to realize that students' works, their classroom discussions about the historical discovery of the elements and their classification related to von Andropoff's periodic table were very successful (teacher's information).

Working the differences

Secondary school students found few differences between Mendeleev's and von Antropoff's periodic table of elements, summarized as:

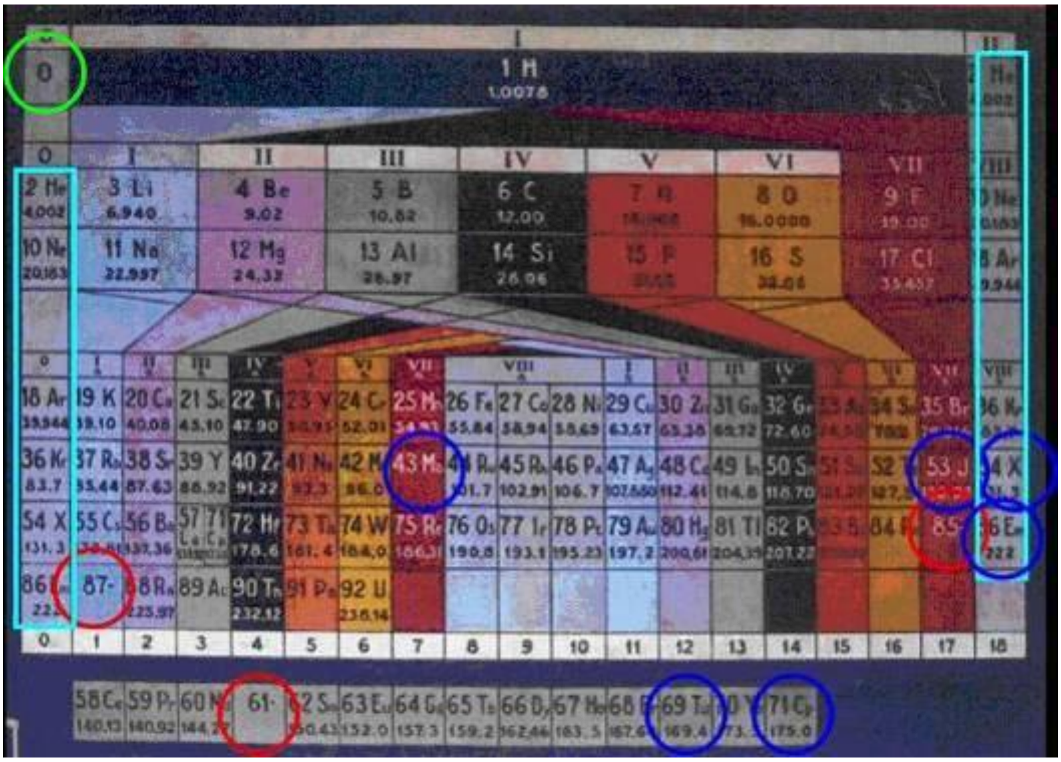
Mendeleev's periodic table was proposed supposing the property of atomic weight constant for each element, and Mendeleev's historical support was based on the fact that his predictions were successful. However, in the early twentieth century the property of atomic number was chosen as more appropriate.

The view that the periodic table should be thought of primarily as an organization of macroscopic chemical properties of the elements, rather than as a tool for accessing the electronic structure of the atoms of elements, is important to present the history of the periodic table to young pupils. Secondary school students must be taught that the elements classification was prepared when chemists didn't know about the electronic configuration of the atoms of elements.

Secondary school students found many differences between von Antropoff's periodic table, represented in the figure below, and the current periodic table that appears in their secondary school textbooks.

Surprisingly for secondary school students, von Antropoff's periodic table has an element with number 0, that is no protons and no electrons but only neutrons. It was a hypothetical element predicted by von Antropoff that doesn't exist. At this point students and teachers could discuss the importance of mistakes and controversies in chemistry evolution, because the advance of science involves not only established conclusions, but also strong controversies. Even mistakes could be helpful in chemistry education.

The group of noble gases has doubled; there is one column in each side of the periodic table.



The restored von Andropoff's periodic table at the University of Barcelona, with the points that need an explanation.

In this periodic table appeared three empty places: numbers 61, 85 and 87. These three elements were not discovered by 1934. These elements are: 61 Promethium (Pm), 85 Astatine (At) and 87 Francium (Fr). Students easily understand that the number of the discovered elements is period-depending and, for this, the discovery of elements is increasing with time up to the present day.

There have been many changes in the elements symbols from von Antropoff's periodic table to now:

- Number 43 before 1934 was thought to be the element Masurium, represented as Ma, but was synthesized in 1937 as the Technetium element, represented as Tc.

- Number 53 Iodine was represented as J because its name in old German language is Jod, now represented as I.
- Number 54 Xenon was represented as X, now as Xe.
- Number 69 Thulium was represented as Tu, now as Tm.
- Number 71 was thought until 1950 to be the element Cassiopeium, represented as Cp, but now it is named as Lutetium, Lu.
- Number 86 was named Radium Emanation from 1910 to 1960 and was represented as Em, now it is the element Radon, Rn.

Clarifying these differences increased students' interest in chemistry and its history, they were asking for an explanation that they put in context with the history.

Conclusions

- Collaboration between the CBC and the University of Barcelona allows the dissemination of the history of chemistry within Catalonian secondary schools.
- Finding links between the history of chemistry, the history of science and the present day is a prerequisite to involve young students in this venture.
- Working with a restored painted periodic table and the 150th anniversary commemoration of the social presentation of Mendeleev's periodic table was a good help for science teachers to spread the history of chemistry.
- This project allowed us to involve more teachers, more secondary schools, more classrooms and more secondary school students in disseminating the history of chemistry. A lot of secondary school chemistry teachers presented the history of chemistry in their chemistry courses.
- As Kuhn proposed, the history of chemistry produces a crucial transformation in the image of chemistry in young students. The majority of secondary school students that had participated in these activities got a better and more positive image of chemistry and the history of chemistry; they could spread this image to their family and friends.

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THE ORIGINS OF TECHNICAL EDUCATION IN INDIA: STUDY OF DIFFERENT APPROACHES

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Abstract

The present paper is a study of the different growth models of evolution of Technical Education in India. Initially it was the colonial model, in which engineering colleges were established mainly in five places in India. The colleges in Roorkee, Poona, Calcutta, Madras and Bombay were utilized to produce 'technical hands' for the empire. However, in the early twentieth century, three nationalistic models developed. In Calcutta, it was the National Council of Education, which, in defiance of the University Act of Curzon wanted to establish National Colleges and National Schools. Only the College of Engineering and Technology developed out of this model and became the Jadavpur University. In Banaras, Malaviya started the Banaras Hindu University, where Technical Education, Law and Medicine were imparted together with the inculcation with the spirit of Hindu religion 'for moral growth'. However, J. N. Tata, after negotiation and fighting, could establish the Indian Institute of Science (IISc) as a premier research institute in India. It was also a teaching institution, different than the Indian Association for the Cultivation of Science (IACS) established in Calcutta by Sir M. L. Sircar in 1876. This grew into a reputed school in India, with active collaboration of the British as well as National Industrialists –a major prerequisite for industrial and economic growth. There were other small attempts.
The Universities were mainly imparting arts and science education in India from 1857 and were affiliating bodies. Thus, Technical Education started in India in the 1860s, almost at the same time at UK, but the visions were different, sometimes conflicting.
It was only in 1951, the IITs (Indian Institutes of Technology) were established based on the Sarkar Commission Report in 1946. The paper concludes by tracing their growth as ranked institutes in world educational systems ranking.

Introduction

The history of Technical Education and its inter-relationship to development of technology, industry and economy is an important field of study. Few works have been done on the Indian scenario keeping this inter-relationship in view. A study of the development of Technical Education in India reveals the Technical Education in British India was not established with a view to build-up a technological reliant India.

A deeper study of growth of Technical Education in India took us to different roots: the colonial model, the nationalistic model, the research model etc. the present study tries to highlight the nature of the different models in technical education perspective framed in a historical background for India.

Colonial Models of Technical Education

1. Guindy Civil Engineering College, Chennai

The first survey school was founded at Guindy, Madras in 1794 by Michael Topping, the East India Company's astronomer, and geographical and marine surveyor at the Presidency of Fort St. George, to do the necessary survey works and to assist British surveyors because land use in India was of precedence. The survey school started training Subordinate Engineers, Upper Subordinates, Lower Subordinates and Overseers, giving them elementary knowledge of Civil Engineering. [6]

By 1842, it was realized that the survey school was inadequate for the needs of the Public Works Department, and the establishment of an engineering college was desirable.[11] An industrial school, attached to the Gun Carriage Factory, was established in 1842 in Madras, which amalgamated with the survey school in 1858 and was redesignated as Civil Engineering School. In 1859, the name of the institution was changed from 'Civil Engineering School' to 'Civil Engineering College'.

The subjects taught were Surveying, Plotting, Planning and Estimation, Mechanical Engineering, Hydraulics and Elementary Mathematics etc. The first department was created in 1862 to train Commissioned Officers and Civilians as Assistant Engineers. In 1886 the Civil Engineering College came to be known as the College of Engineering. [6]

2. Thomason Civil Engineering College, Roorkee

The first engineering college in colonial India, Thomason Civil Engineering College was established in 1847 at Roorkee for training of civil engineers for the PWD works with the initiative of James Thomason, the Lieutenant Governor of the North Western Provinces. The college provided education to a limited number of students under the overseer and subordinate categories. Thomason chose the location at Roorkee, near the Solani Aqueduct on the Ganga Canal, because large workshops there would provide facilities for instructing civil engineers. [8] The college started functioning on January 1, 1848. Later, the college was named after James Thomason as 'Thomason College of Civil Engineering' in 1854. Initially, only Civil Engineering was taught in the College, but after that Electrical, Mechanical, Textile Engineering etc. were incorporated in the curriculum. The Roorkee College became a model and catalyst for the other engineering colleges in India.

3. Calcutta Civil Engineering College

Being impressed by the success of Roorkee College, Lord Dalhousie granted the starting of engineering colleges in each of the presidencies. In Bengal, A College of Engineering was started by the name of 'Calcutta Civil Engineering College' on 24th November, 1856, in the premises of the Writers Building, Calcutta. The objectives of the college were the same as those of Roorkee College; to train candidates for different levels of employment for the public works and revenue departments. Hence there were courses for overseers and sub-overseers also. [7]

In 1865, the college merged with Presidency College, Calcutta and from 1865 to 1869 the college functioned as the Civil Engineering Department of Presidency College. In 1880 the engineering department was given back its separate identity as a college by transferring it to the buildings of the old Bishop's College at Shibpur in Howrah, so that both civil and mechanical engineering students would be able to receive theoretical training in the PWD workshops in Howrah. The college was named as 'Government College, Howrah'. It started imparting training in Civil as well as Mechanical Engineering. In 1921, the name of the college was changed to 'Bengal Engineering College'. The college had mechanical courses at the engineering and apprenticeship level from the very beginning.

4. Poona Engineering College

Bombay and Poona had a concentration of industries and another engineering college was established in Poona in 1856. Initially, the Poona Engineering College was started as the 'Poona Engineering Class and Mechanical School' to train subordinate officers for carrying out public works like making of buildings, dams, canals, railways and bridges. [9]

The courses in this college were more diversified and emphasised several more scientific disciplines than what was imparted at the other similar engineering colleges. Two new classes, an Agricultural class and a Forest class, were added to the college, and the name of the college was changed from 'The Poona Civil Engineering College' to 'The College of Science'. The objective was to open general science courses leading to B.Sc. degree of Bombay University.

In 1911 all non-Engineering courses closed, and the college renamed as 'College of Engineering, Poona' (COEP) and the name continues today. From then on, the college has gone on expanding adding new departments and new wings by the year. B.E. (Mechanical Engineering) course was introduced in 1914. An important feature of the college was its well equipped workshops where practical instructions were conveyed and at the same time various types of work were executed on orders from the government and the public. [1]

5. Victoria Jubilee Technical Institution

During the Golden Jubilee celebration of Queen Victoria's reign, Victoria Jubilee Technical Institution (VJTI) was founded in Bombay, in 1887 by private efforts to provide courses of instruction suited to the requirements of the skilled manpower for growing Bombay Textile Mill industries. Institute started with only two departments, namely,

- the Sir J.J. School for Mechanical Engineering and
- the Ripon Textile School

It continued with the diploma courses until 1944 when it expanded its scope to degree courses in Electrical Engineering, Mechanical Engineering and Textile Technology. This was the only institution in India at that time with largest number of machines and apparatus. And this feature attracted more students from the Punjab, Assam, Madras, Sind, the Central Provinces and from many other places of India. The great names associated with this institute included Dadabhai Naoroji, Pherozshah Mehta, Dinshaw Wacha, R.D.Ranade and philanthropic members of Petit, Jeejabhai and Wadia families. Some of the first teachers of the National College of NCE, Bengal were from VJTI. The institute was renamed as 'Veermata Jijabai Technological Institute' on January 26, 1997.

In England civil, mechanical, electrical and applied chemistry co-existed side by side. But until the 1920s only civil Engineering could be studied to degree level in the Engineering colleges in India. The curriculum and the training in Roorkee, Guindy, Poona and Shibpur were based mainly upon PWD, Railways and other Government requirements of only subordinate grades of employment, particularly in government services. [10] Civil Engineering remained the favoured subject. The colleges opened during the nineteenth century in India were mostly called 'civil engineering colleges', while the other branches – e.g., electrical, mechanical and mining engineering – remained neglected and repeated attempts to develop electrical and mechanical engineering degrees at Calcutta, Poona and Madras failed in the early years. [10] VJTI was an exception perhaps for the industrial and textile bases in Bombay.

The Nationalistic Models of Technical Education

1. Dawn Society to National Council of Education, Bengal to College of Engineering and Technology to Jadavpur University

The colonial model of technical education with the establishment of Guindy College, Thomason College, Calcutta Civil Engineering College, Poona Engineering College and Victoria Jubilee Technical Institution travelled in one direction. Parallelly, a national model was also attempted to be developed in Calcutta, at the National Council of Education, Bengal (1905-1906).

Generally speaking there were no institutions in India where higher technical instruction on modern lines as compared to Germany or U.K. could be imparted. "*Government was interested in only in technical and industrial schools, not in higher forms of scientific and technical education*". [1] To quote Satpal Sangwan "*The real fault in the British education policy was, the undue and disproportionate attention devoted to literature, as compared with physical sciences and the cognate branches of practical instruction*". [2] As a result, the scientific and practical educations were neglected. Students had to go abroad to get the requisite training. But only a limited number could avail of this opportunity. It was felt by a section of nationalists that scientific and technical education was a necessary precondition to economic and industrial growth of India, to put it at par with other industrially developed countries, particularly for an India, free from the shackles of foreign rule.

The Dawn Society established in 1902, by Sri Satish Chandra Mukherjee had been functioning as an association for the promotion of moral training and general culture as well as business and commercial habits among young men under the inspiration and guidance of the founder. On Nov 4, 1905, in a meeting, the Dawn Society encouraged the audience to go in for the establishment of a national university. Based on the ideas of Dawn Society, in 1906, a group of Bengali intellectuals and the leaders of the Swadeshi Movement decided to set up a truly National College that would challenge British rule by offering alternative education to the masses *"on national lines and under national control*". [12] Thus the National Council of Education, Bengal was founded on 11th March, 1906 without Government support and financial help. Its main aim was to provide education –literary, scientific and technical on national perspective i.e. a blend of the best of the east and the west. The Bengal National College and School, the model institution was established by the Council in August, 1906. Right from the beginning, the Bengal National College developed a model of 'three dimensional' system of education combining teaching with factory work. Initially, the institution had four departments –Literary, Scientific, Technical and Commercial.

Some of the leaders of the council started feeling that the extreme nationalist flavour became more a hindrance than a help to the academic progress of the college, and the economic problems of the country could be solved by the promotion of technical education alone. On June 1, 1906, Sri Taraknath Palit (breaking away from NCE) established a second organisation, rival to the National Council of Education, named 'Society for Promotion of Technical Education' (SPTE) and later founded the Bengal Technical Institute (BTI). This was the first nationalist technical school in India. *"They took their ideal of technical education from the systems that prevail and have succeeded so well in Europe and America and it was thought advisable that so far as it lay in their power their systems should be shaped on those models"*. [4]

In BTI, two courses were offered: primary courses which include practical training in Mechanical fitting, Electrical fitting, Carpentry, Drawing and Surveying. And the secondary courses included Mechanical Engineering, Electrical Engineering, Dyeing and Bleaching, Industrial Chemistry and Economic Geology and Mineralogy. BTI also had a manufacturing department. NCE and SPTE formed a combined institution in 1910. Later the institution was shifted to Jadavpur in 1924 and was renamed as the 'College of Engineering and Technology' (CET) in 1929. [13] The CET adopted the aims, ideas, academic and administrative policies of the BTI. [4] The National Council of Education, Bengal which was established in 1906 to provide education on national perspective had initially two technical courses in the National College – Mechanical and Electrical along with other subsidiary technical subjects. In 1921 it started the first Chemical Engineering Department in India. In 1956 the College of Engineering and Technology became the Jadavpur University and successfully established itself as one of the best scientific and technological institutions of India, with a strong engineering faculty and with Science and Arts faculties also.

The growth of this model, carried on by extremist Swadeshis was done without any Government help in the initial stages. However, the quality of education got recognised all over world.

2. Malaviya's Banaras Hindu University: Hindu Model

Another nationalistic model was initiated by Pandit Madan Mohan Malaviya with the establishment of the Banaras Hindu University with different objectives. According to him the two foremost priorities of the country were the freedom of the country and nation building. The priority of nation building could be achieved only through the rapid industrial and agricultural development and for this a greater facilities for technical and industrial education would be required. [14] One of the objectives was to make provision for imparting instruction and technical training in the different branches of engineering. Hence, in order to meet the future immense needs of the 'Resurgent Modern India', he visualized a 'Modern University' that combines the best of Indian education with the best tradition of modern universities of the west. The aim of the University, by Malaviya's vision, were to promote Hindu Shastras and Sanskrit for preserving and popularizing the best thought and culture of Hindus as well as to advance scientific and professional knowledge. [14]

Malaviya first propounded the idea of a Hindu university in 1904 at a meeting in Banaras, presided over by the Maharaja of Banaras who had been his co-worker in the founding of the University. It then took ten years for Malaviya to find a site and to raise the necessary funds with the help of Zamindars and Rajas. Malaviya, Follower of 'Hind-Hindi-Hindu' model, had already annexed to his brand of politics the emotional forces of Hindi and Hindu revivalism. [20]

The Banaras Hindu University Society was registered in July 1911 and the Benares Hindu University Act was passed in 1915. The foundation stone was laid in 1916. The already existing Central Hindu College, founded in 1898 by Mrs. Annie Besant, founder of the Theosophical Society and British reformer, served as a nucleus for this institution.

To explore the possibilities of industrial development in India, in 1916, the government of India appointed an Industrial Commission under the Chairmanship of Sir Thomas Holland. Malviya was one of the Indian members of the commission and gave a 'Note of Dissent' [5] which illustrated the industrial and economic problems of then India and an indictment of the British economic policy towards the country. [15] Malviya wrote "*in the present economic condition of India there is no branch of education for which there is greater need than scientific and technical instruction*". The most intriguing fact is that, he did not mention anything about the NCE model of national education, particularly technical in his note, while he had some connections with Sir Rashbehari Ghosh etc. However, he gave a proposal for the establishment of an Imperial Polytechnic. [15] Probably, he was not much enamoured with the ideas and achievements of Bengal renaissance men and he wanted to chart his own path, which was quite natural in the then condition of Indian struggle for freedom.

Education in Engineering and Technology commenced at the BHU with the establishment of the College of Engineering in 1919 with two departments, Mechanical Engineering and Electrical Engineering. Later a department of Mining and Metallurgy was added to the college and later it became a separate institution as College of Mining and Metallurgy (MINMET) in 1923. This was more in continuation with India's early achievements. Another college named 'College of Technology' (TECHNO) came up in 1938. In 1968, these three premier engineering Institutions had been amalgamated to form the Institute of Technology (IT-BHU). The first-ever combined Degree Courses in Mechanical and Electrical Engineering, as well as Mining Engineering, Metallurgical Engineering, Ceramic Engineering and Pharmaceutics in India were initiated at BHU. Pandit Madan Mohan Malaviya was greatly interested in industrial and technical development of the country and he had been making all efforts to promote technical education. [14] He combined his roles of politician, administrator and educator well by remaining the Vice Chancellor of BHU from 1919 to 1939.

3. Indian Institute of Science: Research Model

Apart from the swadeshis, there were some individuals and industrialists, and through their efforts and financial help Technical Education did make immense headway in India. The first research institution was started in Calcutta in 1876, the

Indian Association for the Cultivation of Science, by Dr. Mahendralal Sarkar. Its primary purpose was to foster high quality fundamental research in frontier disciplines of the basic sciences. But it did not have any programme for Technical Education.

In the western part of India, Mr. Jamshedji Nusserwanji Tata, one of the leading industrialists of the colonial India, realised the importance of scientific and technical education and research and this led him to conceive of an institution of higher learning which would pay greater attention to science and would train the bright young students after they left the universities. He symbolized the rising aspirations of the Indian bourgeoisie which were becoming conscious of the technological (societal) applications of scientific research. Mr. Tata's main aim was to build up a world class institution with well established link with Industry that will furnish a thorough post graduate courses of studies and supply one of the major needs of the country; scientific researchers and technocrat leaders. He took John Hopkins University of Baltimore, the first Post Graduate institution in the world, as a model for his institution. [16]

On 31st December, 1898, the provisional committee including J.N.Tata and Padshah met Lord Curzon, the newly appointed Viceroy with their proposal to build an institution. But, Lord Curzon, because of his anti Indian bias became the villain again. He tried to ruin Tata's plan for an Indian University of research, because he did not favour spreading higher level technical education in India. [11] He expressed his doubts about the scheme. Ultimately, the Government suggested that the scheme (the location, expenditure) for the institution be critically evaluated by one or two outside experts. The provisional committee on the advice of Padshah suggested the name of Prof. William Ramsay, who was one of the first British scientists to win Nobel Prize in 1904. Prof. William Ramsay toured India, visited several educational centres, and submitted a fairly detailed report on the academic, administrative and financial structure of the institution. On the academic side, he was clearly in favour of science and technology. Sir William Ramsay prunes Tata's scheme from a University to an Institute. [16]

The immense effort of Burjorji J. Padshah ultimately led to the establishment of the Indian Institute of Science in 1909 at Bangalore, as a research organization devoted mainly to experimental sciences. Best brains from the country, abroad and industry were pulled together. With Ramsay's recommendations the institute got its first director Morris Travers in 1909. Under the guidance of some very able Indian directors like C.V.Raman, J.C.Ghosh and Satish Dhawan the institute flourished as a premier post graduate research organisation and a rich source of talented scientific manpower in India.

4. Other Small Societies

In the mid-nineteenth century, there were also some individual and group efforts to popularize the modern western science through the medium of vernacular. The most important effort was the establishment of Aligarh Scientific Society in 1864 by Sir Syed Ahmed Khan.

"The Muslims in India were as alienated from western education as were the Hindus. Both colonial and structural factors were responsible", to quote Zaheer Baber. [5] Sir Syed Ahmed Khan tried to represent the Muslim viewpoint and initiated an educational project. One of the endeavors of this society was to translate English books in vernacular to percolate science and technology to masses. He established the Mohammedan Anglo Oriental College in 1877 in Aligarh and patterned the college after Oxford and Cambridge Universities. Finally this college rose to become the Aligarh Muslim University in 1920. Aligarh got a quite unusual amount of British patronage including a personal donation from the Viceroy, Lord Northbrooke.

On the same line Bihar scientific society was established in 1868 to publish scientific books in Urdu. Similarly, another society was formed by a group of teachers associated with the Delhi College. These societies played an active role in teaching and translating western scientific books into Urdu. However, they remained only fringe efforts.

University Model: the unfinished tasks

The foundation of Universities was another development under the British rule. Sir Charles Wood's Education Dispatch of 1854 can be regarded as the forerunner of modern university education in India. In 1857, three universities were established in the three provinces of Calcutta, Bombay and Madras. [17] The expansion of the University system was slow. In the first decade of the 2oth Century, there were only five Universities. All of them were modelled on the University of London and all of them were affiliating and examining universities, not teaching universities. Postgraduate work was confined to the colleges which were not properly equipped for such work and so the quality of postgraduate work was not of a very high order. The University curricula were inadequate to meet the needs of a scientific and industrial age. Thus the universities in its early stage, failed to contribute much to the advancement of knowledge in India, which was surely one of their main functions. When the Bombay University was established, it had no faculty of science and the only course in engineering was in Civil Engineering. Kolkata grew with a strong science base. Under the guidance of Sir Ashutosh Mukherjee, the Calcutta University produced the torchbearers of renaissance in Indian science like Acharya Prafulla Chandra Ray, Acharya Jagadish Chandra Bose and their students Prof. Satyendra Nath Bose and Prof. Meghnad Saha. [17] The University College of Science and Technology was established in 1912 in Kolkata, and many eminent scientists were associated with this institution. However, the progress of technical education was hampered due to the lack of Government support and funds as also an alienation of the University model from that of Germany in particular.

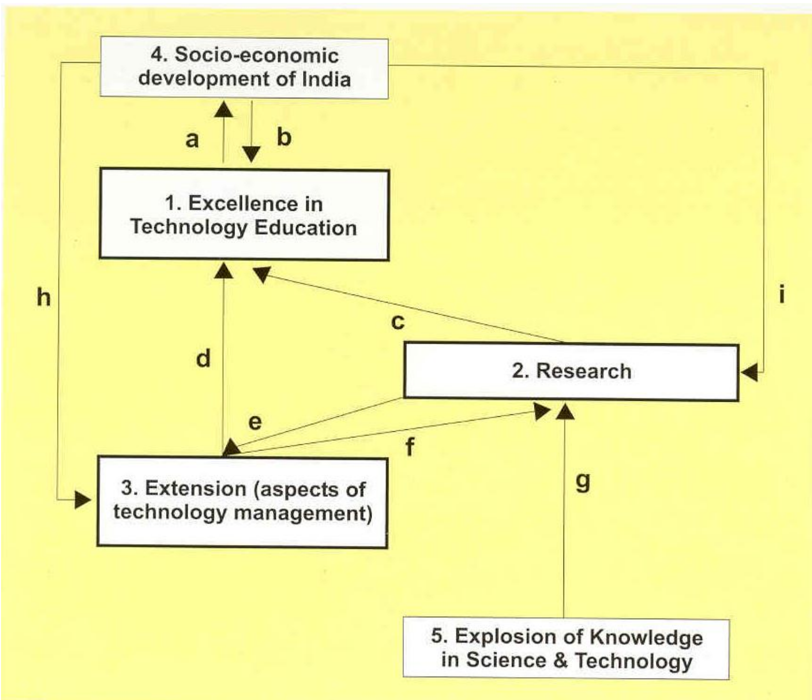
Although the universities offered a programme of undergraduate studies in pure and applied sciences, enrolment in these courses throughout the nineteenth century was very low, because, opportunities for Indians to advance in the scientific services were limited by colonial rulers. Most students preferred arts courses or professional courses like law or medicine. This naturally hampered the growth of higher scientific and technical education within the domain of the university system.

Some universities became predominantly technical but not technical institutions up to the 1960s, until the Kothari commission report came out. [21]

Indian Institute of Technology (IIT): PG and research model

During the Second World War period (1939-1945), the Government of India began to realise the importance of organising Technical Education in the country on systematic lines. The first major step in that direction was taken in 1945 when the Government of India on the initiative of Sri Ardeshir Dalal, a visionary director, Tata Iron and Steel Company, appointed a Committee of industrialists and educationists, under the chairmanship of Sri Nalini Ranjan Sarkar, to survey the entire question of Technical Education in India and to make definite and concrete recommendations in this respect. [18] Until the establishment of the Sarkar Commission in 1946 (the report was published in 1949) –there was no specific thrust towards higher technical education and research in India. In their interim Report in 1946, the Sarkar Committee recommended that four Higher Technical Institutions should be set up as soon as possible, one each in the east, west, north and south. [18] Establishment of the Indian Institute of Technology Kharagpur in 1951 started the realisation of this plan.

The five core elements of the IIT vision, first articulated by the Sarkar Committee, and their relationships as given in the following diagram, suggests that the priority of the IIT system was clearly on education, followed by research and extension. [19]



The IITs were created with foreign technical collaboration, UNDP assistance and modelled on the Massachusetts Institute of Technology and the University of Manchester pattern and to train scientists and engineers with the aim of developing a skilled workforce to support the economic and social development of India after independence in 1947. [18] The first IIT was established in 1951 in Kharagpur in the state of West Bengal with Sir J. C. Ghosh as the Director. The other 3 IITs came into existence in a short span of a decade in Bombay, Madras and Kanpur. The College of Engineering, Delhi and the Roorkee University were converted to IIT in 1961 and 2001 respectively and a new IIT was set up in Guwahati in 1994. Now, IITs are a group of fifteen autonomous premier engineering and technology-oriented institutes of higher education established and declared as *Institutes of National Importance* by the Parliament of India [IIT Act, 1961].

To review the work and progress of the IITs, the India Government had appointed the Nayadumma Committee in 1984 and Ramarao Committee in 2004. The Ramarao committee had highlighted several of the issues involved and made recommendations regarding governance, faculty matters, research enhancement, entrance exam, linkage with industry and funding policy etc. For recruiting new faculty members, the IISc practices were considered more flexible system than the IITs. The committee made a comparison of the IITs and IISc practices in this regard and recommended that IISc system may be adopted by the IITs for the faculty induction, assessment and promotion. [19]

The IITs have earned worldwide fame due to sincere efforts of faculty, students and administration. The IIT system is one of the major success stories of independent India and ranked amongst the topmost institutions in the world in all ranking systems. The autonomy and academic ambience created in these campuses have attracted best faculty and students all over the world. But how far the IITs succeeded in conducting research and development at internationally competitive levels and succeeded in producing engineers 'on par with the best in the world?' –are the most frequently raised questions and is subject to further study, as India still remains a low ranked country in terms of technological education and research.

Conclusion

India's growth is very nonlinear. The evolution of Technical Education is one area which shows the same characteristics.

Predominantly an agricultural country, India had a weak Industrial System compared to European Countries and America. Under the British rule, Technical Education developed in India 'by the colonials, for the colonials and of the colonials'. Resurgence of nationalism encouraged leaders to go for independent national models of Technical Education. Of these the National Council of Education, Bengal was developed by the moderate Swadeshis, but developed into a good research institute (Jadavpur University) only in the 21st century. However, the IISc model and the IITs, which were developed with the assistance of technically developed countries, one at the beginning and others during the middle of the 20th century, took quickly their position in the international rankings of higher education in the world.

This indicates, though technology was very country specific, a global connection together with nationalism helped in development of better models for Technical Education.

References

[1] Basu Aparna (1991). The Indian Response to Scientific and Technical Education in the Colonial Era, 1820-1920. In: Kumar Deepak (ed.), *Science and Empire: essays in Indian context, 1700-1947*. Anamika Prakashan.

[2] Sangwan Satpal (1990). *Science Education in India under Colonial Constraints, 1792-1857*. Oxford Review of Education, Vol. 16, No. 1, pp. 81-95.

[3] Kumar Deepak (2006). *Science and the Raj – A Study of British India*. 2nd edition. Oxford University Press.

[4] Sarkar Benoy Kumar (1946). *Education for Industrialisation. An Analysis of the Forty Year's Work of Jadavpur College of Engineering and Technology (1905-45)*. Chuckerverty Chatterjee and Co. Ltd.

[5] Baber Zaheer (1998). *The Science of Empire –Scientific Knowledge, civilization and colonial Rule in India*. Delhi, Oxford University Press.

[6] *Survey School to Tech. Temple (A History of College of Engineering, Guindy): 1794-1994* (1966). Guindy Engineers, College of Engineering, Anna University, Guindy, Madras.

[7] *Bengal Engineering and Science University, Shibpur*: 150th Anniversary Souvenir.

[8] Mittal K. V. (1997). *History of Roorkee University*. University of Roorkee, India.

[9] Sen S. N. (1991). *Scientific and Technical Education in India 1781-1900*. INSA, New Delhi.

[10] Kumar Arun (1995). Colonial Requirements and Engineering Education: the Public Works Department, 1847-1947. In: R Macleod, Kumar Deepak (ed.) *Technology and the Raj*. Sage publication.

[11] Biswas Gautam; Chopra K. L.; Jha C. S.; Singh D. V. (2010). *Profile of Engineering Education in India: Status, Concerns and Recommendations*. NAROSA Publishing House.

[12] Mukherjee Haridas; Mukherjee Uma (2000). *The origins of the National Education Movement (1905-1910).* NCE, Bengal, Calcutta, June.

[13] Lal Ananda, De Rama Prasad, Sen Amrita (2005). *The Lamp In The Lotus: A History of Jadavpur University*. Jadavpur University, 2005.

[14] DAR S. L., Somaskandan S (2007). *History of the Banaras Hindu University*. Banaras Hindu University, reprinted.

[15] Indian Industrial Commission 1916-1918. *A Report*. Calcutta Superintendent Govt. Printing, India.

[16] Subbarayappa B. V. (1992). *In Pursuit of Excellence: A History of the Indian Institute of Science*. Tata McGraw-Hill Publishing Company Limited New Delhi.

[17] *Hundred Years of the University of Calcutta*. (1957). University of Calcutta.

[18] *Sarkar Commission Report* (An Interim Report of the Committee appointed to consider the development of higher technical institutions in India) (1946).

[19] *IIT: Report of the Review Committee* (2004). P Ramarao Committee.

[20] *A Century: Indian Association for the Cultivation of Science* (July 1976). Sree Saraswathy Press Limited, Calcutta.

[21] SARKAR Sumit (1983). *Modern India: 1885-1947*. Macmillan.

[22] *Education and national development –Report of the Education Commission 1964-66* (Kothari Commission Report). (1971). National Council of Educational Research and Training.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 140.

EARLY SCIENTIFIC INSTRUMENTS FOR TEACHING PHYSICS IN THE UNIVERSITY OF BARCELONA

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Abstract

Although the University of Barcelona was founded in 1450, there is no evidence of the existence of scientific instruments for teaching until the 19th century. According to our archives, in the middle of this century it is on record that classes of Physics, Astronomy and Experimental Physics among others were taking place in the University of Barcelona.
For this reason, at that time the University acquired the first instruments for the teaching of Physics, following the recommendations made by the educational ministry, which increased notably in number until the end of the century and have continued to do so since then. Now the collection consists of about 250 instruments belonging to the sections of Kinematics, Mechanics, Statics, Dynamics, Hydrostatics, Acoustics, Optics, Static-Electricity, Electricity and Magnetism.
*This study analyses the existing scientific instruments acquired in the 19th century and correlates them with a handwritten list dated 1868 found in our archives. Then a further cross-correlation is carried out with the official lists that appeared as a consequence of the educational reforms promoted by the Directorate General of Public Instruction (*Dirección General de Instrucción Pública*).*

Introduction. The political background in Catalonia in the 18th and 19th centuries

In order to understand how the centres of science culture evolved, it is important to know the framework of Catalan society in the 18th century. At the beginning of this century student disturbances were very frequent, especially pro-autonomy movements in Catalonia. In response to all these facts, in 1714 king Philip V forced the University to move from Barcelona to Cervera [1]. This town is located in western Catalonia, about one hundred kilometres away from Barcelona. As a reaction against the imposition from Madrid and also for convenience reasons, several schools with university-like education were created throughout the century [2]. Among such schools with university-like education, we can point The Royal Academy of Science and Arts of Barcelona (*Reial Acadèmia de Ciències i Arts de Barcelona*, or RACAB, 1764 to present day), The Royal and Military Academy of Mathematics (*Academia Real y Militar de Matemáticas*, 1720-1803), The College of Surgery of Barcelona (*Colegio de Cirugía de Barcelona*, 1760-1843) and The Commerce Council (*Junta de Comercio*, 1769-1851) [3]. The latter had subdivision schools like the Nautical School, Arts School and others. One of these was the Experimental Physics School, set up in 1814, transformed into Experimental Physics Applied to Arts in 1840 and finally in 1851 it became the Industrial School of Barcelona (*Escuela Industrial de Barcelona*), which was the core of present-day Polytechnic

University of Barcelona (*Universitat Politècnica de Barcelona,* UPC), created in 1971 as a result of grouping together all Technical Schools [4].

In 1835, negotiations for returning the university to Barcelona finalised successfully, some chairs were created and the definitive restoration was concluded in 1842. It is interesting to note their different locations. After the aforementioned return some courses were in the Old *Convent del Carme*, until it was burnt down in 1835, and later in 1838 some sections were in the Convalescence House (dependent on the Santa Creu Hospital), all these buildings near the Ramblas. As there was insufficient space, although the original idea for the expansion was the construction of the new building in the place of the old *Convent del Carme*, the definitive location was in today's Plaça de la Universitat. The First Stone was laid in October 1863, and in December 1871, official inaugural lecture in the new building opened the academic year [5].

The collection of scientific instruments and its origins

The University of Barcelona dates from 1450, but the first evidence of experimental physics appeared in 1764 when a group of science enthusiasts gathered together in order to found the Experimental Physics-Mathematics Conference. In 1765 it was recognized by the central government in Madrid with the title of Royal Conference and later, in 1770, it became the aforementioned The Royal Academy of Science and Arts of Barcelona (*Reial Acadèmia de Ciències i Arts de Barcelona*, or RACAB) [2], aforementioned. Although in the 19th century the professors of Experimental Physics of the University of Barcelona were also members of the RACAB, there is no evidence that any instruments were shared in both centres.

The presence of instruments for teaching Physics in the mentioned schools and in the restored (in 1842) University of Barcelona was a consequence of the fact that at that time there was in all Europe a progressive trend towards teaching through experiments because observation was thought to be the best way to learn science. Moreover, scientific knowledge and experimental activities were conceived as crucial from the pedagogical point of view.

In 1846 a step forward was taken in teaching by the Minister Pedro José Pidal Carniado (1799-1865), who conceived new curricula and syllabuses and wrote a set of rules and recommendations regarding the content and organization of the different studies and academic years. This reform is important because it is the beginning of a standardisation process (and at the same time control) of the operating system and teaching activities in all Spanish universities. Later in 1857, the reform of studies by the Minister Claudio Moyano Samaniego (1809-1890) divided the two sections in the Philosophical Faculty of the University of Barcelona into two faculties, *Ciencias* and *Filosofía y Letras* (Science and Arts) [6]. Among the several rules of the mentioned 1846 Pidal reform, appeared a list of recommended instruments for teaching Physics, which we will look at later in more detail.

For this reason, at that time the University of Barcelona acquired the first instruments for the teaching of Physics, which increased notably until the end of the century and continued to do so since then. So over a period of about 150 years the Faculty of Physics has been accumulating scientific instruments for research and teaching.

Among scientific subjects in the Science Faculty were: Rational Mechanics Imponderable fluids (heat, light, electricity and magnetism), later Physics and Superior Physics, Experimental Physics, Extension of Experimental Physics, besides other related subjects such as: Mathematics, Astronomy, Chemistry and Natural History. Among the subjects taught, those of General Physics and Experimental Physics were always present in all the different syllabuses.

Summarizing, all the instruments are associated with the research of particular areas within the physics departments and also associated with practices and experiments corresponding to the experimental sessions. The continuous renovation and substitution led to the storage of the instruments. Unfortunately, no special regulations concerning the obsolete instruments were in force, which meant that storage conditions were very poor and in several cases the instruments were partially dismantled. Moreover, the continuous changes of departments and laboratories within the building, and later the fact that the complete section of the Physics department moved in 1969 to a new building in Diagonal Avenue [7], facilitated a dispersion of instruments and components. As a result of this constant to and fro-ing, the instruments suffered enormously.

Fortunately, between the mid eighties and the late nineties the restoration among the best preserved instruments was carried out, leading to a group of approximately fifty scientific instruments in good state and working order, which are now in five glass cabinets in the Board Room of the Physics Faculty. At the beginning of the new century, a new period of restoration began, thanks to an agreement with the *Escola d'Art i Disseny de la Diputació de Tarragona* in Tortosa and a continuous programme of restoration is currently under way [8]. In 2009 an inventory and cataloguing of 200 instruments was carried out thanks to the help of the project HAR2008-02580-E/HIST. Finally, it is important to add that, although in 1987 there was a project of creation of a University Museum, it was never consolidated, but in January 2010 a Virtual Museum of the University of Barcelona was inaugurated[1].

[1] See the communication in this *Proceedings of the 4th ICESHS*: Scientific exhibits from the Virtual Museum of the University of Barcelona. Website: http://www.ub.edu/museuvirtual/

A review of different lists of instruments

Among the novelties in the aforementioned reform of the minister Pedro José Pidal, a Circular appeared on 15 October 1846 published in the Boletín Oficial de Instrucción Pública with a Catalogue-Model of the machines and instruments necessary for a Chair of Experimental Physics. This catalogue contained 153 items and with the information of the price in FF (French Francs) taken from the catalogue of the maker Lerebours and Pixii from Paris[2]. Similarly, one year later a Catalogue-Model of the Physics-Chemistry instruments necessary for experiments in the provincial Institutes of Secondary Teaching, containing 116 items appeared in the *Real Orden* of 10th April 1847 published in the *Boletín Oficial de Instrucción Pública*.

By analysing both lists, some differences are evident: In the second case, the instruments were grouped in the different fields, whereas no subgroups appeared in the former. Although the number is not the same (153 and 116), in the first group some items were repeated or even redundant (e.g. different types of thermometers or manometers). In the second group an attempt to economize was evident, as can be seen by the smaller number of items and the lesser total value (5000 to 9531 FF), which is also logical if we consider that they were conceived for experiments in provincial Institutes of Secondary Teaching. By classifying the instruments of the first case, both lists are summarized in Table 1, in that the differences are only in a small reduction of elements in each group.

Table 1.

GROUP	1846 LIST	1847 LIST
Hydrost, Hydrodyn, Pressure, Heat, Molecular actions	64	48
Mechanics	23	14
Electrics and Magnetism	42	39
Optics and Acoustics	24	15
TOTAL items	**153**	**116**
Total cost (FF)	**9531**	**5000**

In the archive of the University of Barcelona there is a document with an inventory of 377 items contained in a 'List of equipment and objects in the Cabinet of Physics of the Faculty of Sciences', handwritten and signed by Professor Antonio Rave, on Barcelona, on June 1868[3]. The contents of this list is shown in Table 2.

[2] See page 548 of the site: http://books.google.es/books?id=kLmnLI8xOesC&pg=PA64&dq=boletin+oficial+de+instrucci%C3%B3n+p%C3%BAblica+15+enero+1846&hl=es&ei=dNNwTcTXK87oOdH8sL4G&sa=X&oi=book_result&ct=result&resnum=1&ved=0CDMQ6AEwAA#v=onepage&q&f=false

[3] Archive of the Universidad de Barcelona, ref.: ES CAT-AUB 02 25/2/7/4.

Table 2.

Contents of the 1868 inventory	
Fields	**Items**
Mechanics and gravity	26
Hydrostatics and Hydrodynamics	34
Pneumatics	27
Capillarity and Molecular actions	9
Dilation and thermology	20
Vapor and Hygrometry	24
Heat Radiant and conductivity	18
Heat and Pyrogeny	11
Magnetism	10
Static Electricity	50
Dynamic Electricity	42
Optics	53
Acoustics	26
TOTAL	**350**
And glass containers and tools	27
TOTAL	**377**

A first overview of the lists

An initial hypothesis was that this 1868 list should be similar to Pidal's both lists. As a first approach, if we look at the 'optics' subgroup and the ratio to total items, we can see an agreement in the following percentages in table 3.

Table 3.

List	Total items	Optics items	Percentage
1846	153	21	13.7 %
1847	116	12	10.3 %
1868	350	53	15.1 %

If we compare the contents of the different fields of the 1868 inventory with those of the 1847 Pidal list, we can identify 93 instruments (from a total of 350) as seen in table 4.

Table 4.

Group	1847 List	Common items in 1868
Hydrost., hydrodyn., Press., Heat, Mol.actions	48	35
Mechanics	14	13
Electrics and magnetism	39	30
Optics and Acoustics	15	15
TOTAL	**116**	**93**

The coincidence of 93 elements over 116 gives a good agreement and a percentage of 80.2%. This shows that the instruments registered in the 1868 inventory were based on the recommended Pidal list.

But if we look at coincidences or agreements between the elements of the 1868 inventory and the present collection, we find only 56 identified elements (and some of them with some uncertainty). The reason for this small number is clear, because the present collection has a large number of elements acquired later than 1868. Moreover it is very likely that a good many of the old instruments have been lost, and especially if we consider the changes of locations of the University between 1842 and 1872, the lack of conservation protocols, etc.

A step forward in the study of the present collection was done by analyzing the makers of the scientific instruments.

A look at the makers of scientific instruments

In the initial Pidal recommended Catalogue-Model of machines and instruments, only the French makers Lerebours and Pixii appeared. Noël-Jean Lerebours (1761-1840) made telescopes from 1799. Later, Marc François Louis Secretan (1804-1867), professor of mathematics in Lausanne, settled in Paris in 1844 where he became a partner of the instrument maker Lerebours. This firm was located at 13, Place du Pont Neuf, Paris and later (ca 1840-ca 1855) 23, rue de l'Est, Paris. Nicolas Constant Pixii (1776-1861), and his son, Antoine-Hippolyte Pixii (1808-1835) were at 2, rue du Jardinet, Paris (1818-1838), and later (1838 - 1855) at 18, rue de Grenelle, Saint Germain, Paris.

It is interesting to wonder why only these two makers were recommended if we consider that at the same time the following makers were established in France: Louis Bréguet (1804-1883), Antoine Bréguet (1851-1882), Jules Carpentier (1851-1921), Charles-Félix Collardeau-Duheaume (1796-1869), Jules Duboscq (1817-1886), Eugène Ducretet (1844-1915), Nicolas Fortin (1750-1831), Paul-Gustave Froment (1815-1865), Henry-Prudence Gambey (1787-1847), Marie Amédée Louis Jobin (1861-1945), Alexis Magny (1712-1777), Heinrich-Daniel Ruhmkorff (1803-1877), Jean-Baptiste François Soleil (1798-1878), Louis Joseph Deleuil father (1795-1862), Jean Adrien Deleuil son (1825-1894), Jules Salleron (1829-1897), Jules Dujardin (1857-1947), Rudolph Koenig (1858-1901)[4].

Grouping and comparing the instruments by makers

If we look at the makers of the present collection, among the 200 in the catalogue, we find in the following table 5:

Table 5.

Catalogue of 200 instruments of the present collection		
Unknown makers	54 instruments	
Signed Foreign makers	134 instruments	Corresponding to 93 different makers
Signed Local makers	12 instruments	Corresponding to 10 different makers

The local signed makers are: Anglo Española de Electricidad, Arriete Dalmau y Brunet, Arriete Dalmau Barcelona, Balanzas Cobos Barcelona, Fábrica de instrumentos de precisión Bastos y Laguna, Dalmau Montero constructores de aparatos de precisión, P. Agustí Barcelona, Productos Eléctricos JAC Barcelona, Sociedad Anónima Prieto Barcelona, Vda. de J. Rosell Barcelona. Although these local makers, all of which are from Barcelona except Bastos y Laguna (from Zaragoza), have not been studied in detail yet, but at first sight they seem to belong to a later period than 1868.

Another analysis has been made comparing the instruments of the 1868 Rave list. Among the present instruments (200 catalogued and 100 yet uncatalogued), 56 of them have been identified from the 1868 list. Among them, 9 correspond to signed makers, 26 belong to unknown makers among the collection of 200 but other 21 have been identified among the list of 100 uncatalogued instruments. All these results are summarized in the graph in Figure 1.

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[4] [12] The makers can be seen in several web sites, such as:
http://iuhps.org/refertxt/catalogs.htm of the Scientific Instruments Commission,
http://www.hasi.gr/makers of the Hellenic Archives of Scientific Instruments,
http://www.bibliotheque.polytechnique.fr of the Bibliothèque Centrale Polytechnique,
http://www.sil.si.edu/digitalcollections/trade-literature/scientific-instruments/CF/SIcompany-names-drilldown.cfm of the Smithsonian Institution libraries.

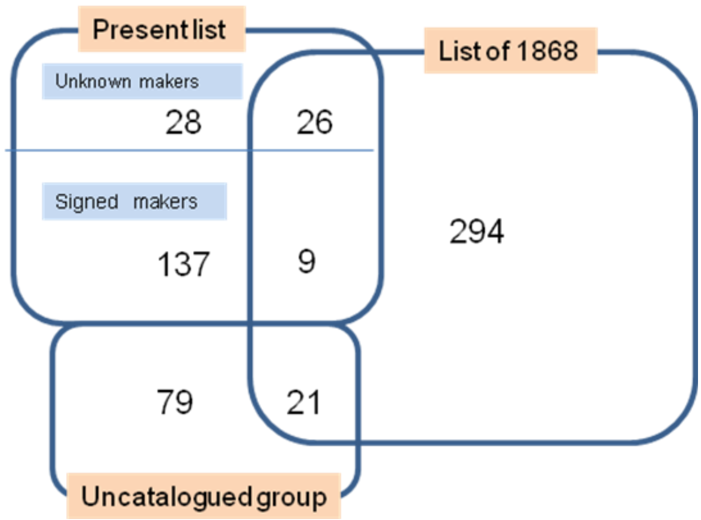

Fig. 1: Graph of the classification of the 56 identified instruments of the 1868 list.

The list of 9 identified instruments (see Figure 2) that corresponds to the known makers is listed below. (In brackets there is the date of birth and death):

a) Mirrors for light recomposition, by Deleuil, senior (1795-1862)? or son (1825-1894)?
b) Camera lucida, by Deleuil, senior (1795-1862)? or son (1825-1894)?
c) Bertin switch, by Ducretet, Eugène (1844-1915).
d) Pneumatic pump, by Pixii senior (1776-1861) & son (1808-1835).
e) Glass tubes for gas experiments, by Pixii senior (1776-1861) & son (1808-1835).
f) Solar microscope by Salleron, Jules (1829-1897).
g) Ruhmkorff coil, by Ducretet, Eugène (1844-1915).
h) Galvanometer of sinus or tangents by Salleron, Jules (1829-1897).
i) Atwood's machine, Gerboz, Pierre (instrument probably released on 1885).

By considering the dates, except the last one, all these instruments could belong to the 1868 list.

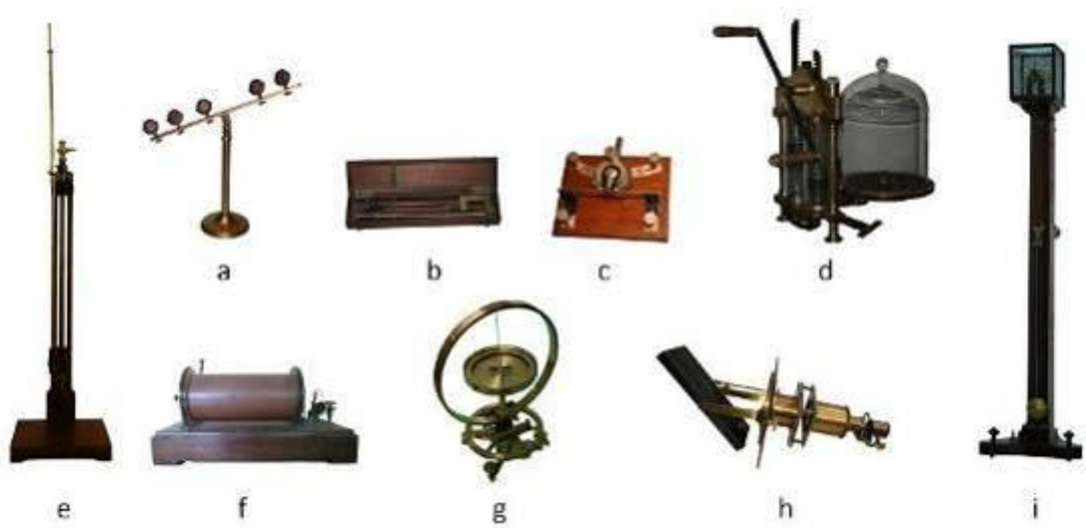

Fig. 2: Probably the oldest identified instruments with known makers.

But the interesting fact is that most of the coincidences (47 instruments) from the 56 identified instruments from the 1868 list correspond to unknown makers. This opens up the possibility that some of these unsigned instruments could have been made by local artists or artisans. As an example, the parabolic reflector for heat and sound experiments has no signature, but is very similar to that item drawn in the Lerebours catalogue, as seen in Figure 3.

Fig. 3: Unsigned parabolic reflector; and a page of the Lerebours catalogue.

Conclusions

The origins of the collection of scientific instruments of the Faculty of Physics of the University of Barcelona, and the influence of the political background have been described. As a basis for the study, a list recommended by the Ministry of Education, that is the Catalogue-Model of machines and instruments necessary for a Chair of Experimental Physics, published in 1846 and 1847, has been considered and also an inventory found in the archives of the University, written in 1868 by professor Antonio Rave. This 1868 list agrees with the recommended lists and moreover, there is a marked increase in the number of items. From this list, few instruments have been preserved to pesent day. Very few instruments from the 1868 list have been correctly identified. There are today only two instruments with the Pixii signature and none with that of Lerebours.

About half of those with no maker's signature were probably made by local makers. The analysis opens a door to the study of local makers or craftsmen.

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References

[1] J. Termes, S. Alcolea, J. Casassas, E. Lluch (1991). *La Universidad de Barcelona* Ed. Universitat de Barcelona, Barcelona.

[2] A. Nieto Galan, A. Roca Rosell (coord.) (2000). *La Reial Acadèmia de Ciències i Arts de Barcelona als segles XVIII i XIX : història, ciència i societat,* Ed Institut d'Estudis Catalans, Barcelona.

[3] J. Agustí Cullell (1981). *Ciència i Tècnica a Catalunya en el segle XVIII*: Ed Institut d'Estudis Catalans, Barcelona.

[4] C. Puig Pla (2000). De la física experimental a la física industrial (1814-1851). *Quaderns d'Història de l'Enginyeria*, IV, 90-132.

[5] J. Casassas, F. Gracia, J.M. Fullola (2008). *La Universitat de Barcelona. Libertas perfundet omnia luce (1450-)*. Ed Universitat de Barcelona.

[6] A. Palomeque (1979). *La Universidad de Barcelona desde el Plan Pidal de 1845 a la ley Moyano de 1857*. Ed. Universitat de Barcelona.

[7] J. Casassas, F. Gracia, J.M. Fullola (2010). *La Universitat de Barcelona. Libertas perfundet omnia luce. Història dels Ensenyaments (1450-2010)*. Ed Universitat de Barcelona.

[8] C. Clemente, F. Pérez-Blanco, S. Vallmitjana (2008). La restauració d'instruments de física de la Universitat de Barcelona, in *Actes d'Història de la Ciència i de la Tècnica,* Nova època volum 1 (1), p. 35-44.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 148.

LES MAGICIENS DE LA LUMIÈRE (*WIZARDS OF LIGHT*): A FILM FOR EDUCATION

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Abstract

This paper explores some uses of the film Wizards of Light *in the field of Education. Examples are given, ranging from junior high schools to undergraduate university level, with special attention to technological education and History of science teaching.*

Introduction

Les Magiciens de la Lumière or *Wizards of Light* is a history of science film (DVD format, 60 min., in French with English or French subtitles) tracing the history of the speed of light up to Foucault's spinning-mirror experiment (1862), a story closely related to the history of Astronomy. It focuses on Foucault's famous spinning-mirror experiment in 1862 which was an important item of our experimental approach of History of Science in Orsay. But other main characters of this enlightening story also appear or are evoked: Galileo with his lanterns, Römer and Cassini debating about Jupiter's moons, Bradley and the stellar aberration, Fizeau and the toothed-wheel experiment, Le Verrier, Froment and others. The main issue is to give an insight on the way in which science is conducted in its historical and social context. The film is a historical work of fiction, with professional actors, directed by the SCAVO[1] with the collaboration of the GHDSO[2] (see Appendix for details).

The DVD of the film is supplemented by a second DVD (DVD-2, "bonus"), in view to make the whole set a tool for Physics education. Included are:

- three filmed sequences: a long interview of William Tobin, author of *The Life and Science of Léon Foucault* [23, 24]; a *Science Café* during which "good questions" are answered by the scientific authors; a scene with modern students in Orsay repeating the spinning mirror experiment;

[1] Service de création audiovisuelle de la faculté d'Orsay.

[2] Groupe d'Histoire et de diffusion des sciences d'Orsay.

- an important printable documentation (PDF, 70 pages) available at the root of DVD-2. Included are: a history of the speed of light; a detailed description of the experiments shown in the film; a very complete glossary of technical and proper names; and an exhaustive bibliography.

One can consider three levels of understanding of the full document:

- at the ground level, the film itself, a cultural approach;
- at the second level, the three filmed sequences in DVD-2 giving a deeper insight on the subject;
- at the third level, the printable PDF-documentation in DVD-2, giving complete scientific and historical details useful, for example, for an educational use.

Both DVDS are distributed in a single package by the SCAVO-Université Paris-sud (see Appendix for information). The target level is from the end of high school to undergraduate university studies. They will equally interest institutions popularizing science, particularly science museums, and all age groups curious about how science is conducted.

In this article, we focus on the use of the film and its "bonus" in Education. We shall only suggest examples of uses in secondary and higher education and in technological courses – a non-exhaustive list; further, we shall examine more precisely the uses of the film in order to illustrate some concepts and aspects of History of science. We strongly emphasize that DVD-2, specially its printable documentation, is essential for an educational use of the film.

A brief history of the speed of light

Light has not always got a *velocity* through History, and this is rapidly evoked in the film. The question of its *instant-propagation* was already debated during the Greek Antiquity, and the conclusion –excepted Empedocles from Agrigento– was generally that the luminous phenomenon was instantaneous [17]. Nevertheless, during the Arab Golden Age, Ibn-al-Haytham (or al-Hazen, 965-1039) places great emphasis on the fact that light is a "body" and that it requires time to travel from the luminous object to your eye [18].

The debate will only resume during the first half of the 17th century, with Descartes and Galileo, two completely opposite approaches. Descartes is inclined to an instantaneous propagation, together with a "successive motion" in space, a rather ambiguous position. Galileo, on the other side, proposes –the first through History– an experimental test (shown in the film) to decide. Of course, he cannot conclude anything [13], but it is a completely innovative approach!

The solution will come from Astronomy. Discovered by Galileo in 1610, Jupiter's moons, providing people with a "perfect" clock in the night sky, are expected to solve the puzzling problem of longitudes, on dry land and specially at sea. Indeed, the longitude at sea was, in the beginning of the 17th century, a completely unsolved problem, responsible for many wrecks and thousands of sailors' deaths. Practically, to determine your longitude, you need a timekeeper giving the time at some fixed reference meridian and you get the longitude from its time difference with your local solar time. But no accurate clock available at the time! Thus, an intense activity in accurate observations of Jupiter moons started immediately. Jean-Dominique Cassini's ephemerides of Jupiter's moons, published in 1668, required 15 years of hard work at San Petronio's gnomon in Bologna. Though impracticable at sea from the moving deck of a ship with Galileo's telescopes, the method is excellent on *terra firma* and it will be used during two centuries for terrestrial longitudes.

A problem initially reported, but not interpreted, by Galileo, was that the motion of Jupiter's satellite Io around the planet didn't seem so regular as it should according to Kepler's laws. Furthermore, those irregularities seemed more or less correlated to the motion of the Earth. Compiling a large set of observations, Römer, in a famous paper in the *Journal des scavans* in 1676 [19, 20], explained that, granted that light requires time to come from Jupiter to us, the observed irregularities are accounted for (related in a scene of the film). He estimated a time of 22 minutes for light to cross the diameter of the Earth's orbit. Römer didn't immediately convince all his colleagues except Huygens, and Cassini himself – though actual initiator of this interpretation– was particularly reluctant to his conclusions, with some good arguments [3].

The situation will rapidly evolve with the *Traité de la lumière*, published by Huygens in 1690 but written well before, around 1678 [15, 3]. Huygens develops a wave theory of light similar to sound waves, and he absolutely needs a finite velocity, however high it may be! Thus, preceding any reflections about the nature of light, Huygens explicitly deduces from Römer's data a numerical value of the velocity of light "more than six hundred thousand times larger than that of sound, which is not at all the same thing as being instantaneous, since there is the same difference as between something finite and something infinite" (trad. in ref. [3]). For the first time, the word "velocity" is explicitly employed concerning light.

The scientific community will be definitively convinced of the finite speed of light only with the discovery of the stellar aberration by Bradley in 1728 [4, 16]. Once again, the solution came from a completely unexpected way. After looking for stellar parallaxes for years, which were expected to bring an observational proof in favour of Copernicus but were yet too small to be detected, Bradley discovers stellar aberration: a new small annual elliptical motion of all the stars. From which, given the accepted Earth-Sun distance, the velocity of light can easily be deduced. Similarly to Römer, Bradley doesn't produce any numerical value for the speed of light. However, the value one can deduce from his data reveals to us the high accuracy of his measurements.

Thus, in the first third of the 18^{th} century, from astronomical works, light has, in the end, got a velocity, and its value is quite close to the modern one. Things will remain roughly unchanged until the middle of the 19^{th} century.

The really *new* fact will be the possibility of *terrestrial* measurements of the speed of light, first evidenced by Fizeau in 1849 [7] with the toothed-wheel experiment between his attic in Suresnes and a mirror placed on Montmartre hill, 8 km away (an important scene in the film). Although not more accurate than the astronomical ones, this measurement is the starting point of a revolution: reversing the reasonings from which the velocity of light was deduced, (Jupiter's moon or stellar aberration), an *independent* value of the velocity of light, combined either with the period of Io or with stellar aberration, yields the mean Earth-Sun distance, a major astronomical problem for two centuries! And, as pointed by Arago, laboratory tools can be almost indefinitely improved [1]. Furthermore, contrary to the planetary parallaxes previously in use, the result no longer relies on the shape and size of the Earth. And so will the nearby stars distances be measured anew by Bessel from *stellar parallaxes*.

Thus, with Foucault's 1862 measurement –the first *accurate* terrestrial measurement in air– the scientific status of the velocity of light has undergone a dramatic change: while an item of intellectual curiosity for a long time, it becomes now the new standard for the measurement of the solar system and, beyond, the Universe (further details in DVD-2).

The film in Technology Education

Actually, the first use of the spinning mirror technique by Léon Foucault was not for an absolute measurement of the speed of light in air. In 1850, following a suggestion by Arago, he performed *a comparison* of the velocity of light in air and in water. Indeed, it was believed for long that, if light was a *body* (i.e. corpuscules or "emission" model), its velocity should increase in dense materials; conversely, if it was an *undulation* (waves), it should decrease. Following Foucault's result (a decrease) the "emission" model –as conceived at the time– was falsified [8, 9]. This experiment is evoked in the film.

Twelve years later, the French astronomer Le Verrier will urge Foucault to perform an *accurate* measurement of the velocity of light in order to corroborate his own calculations of the solar parallax, or Earth-Sun distance. Perfecting his previous spinning mirror and driving system, Foucault will obtain a very improved value, in full agreement with Le Verrier's calculations (the "final experiment" in the film) [10, 11].

One can take advantage of this story in Technology education:

- the 1850-experiment: why did Foucault beat his colleague and competitor Fizeau in the air-water experiment ? Likely due to his choice of a pneumatic drive (a small turbin) for his rapidly spinning mirror, instead of the heavy clock-machinery used by Fizeau. We emphasize that such pneumatic drives remain nowadays the best choice when you need to rotate very quickly a small object (example: solid-state NMR requiring several kHz spinning).
- the 1862-experiment: contrary to the 1850 one, the purpose of which was qualitative and "epistemological", the 1862 experiment is typically a work of *Metrology*. In such an experiment, nobody knows in advance the "good" result (except, maybe, in this particular case, Le Verrier!), and the technical constraints are quite different: accuracy is the main goal. Thus, it appears interesting, in a Technology course, to show how the initial experiment has evolved from a *qualitative* concern to a *metrological* one, with the *same* physicist, using the *same* principle of experiment and the *same* spinning mirror:
    - *improving the turbine*: 16 injection holes instead of 2, use of Aluminium for improving the axle equilibrium, and applying a new design of the blades according to the last mathematical developments will allow to reduce the necessary pressure to 30 cm of water column instead of half-atmosphere of steam (5 metres of water column). Let us hear the academician Girard: "applying this (new) principle, *I designed the blades* of the small turbine for Mr Léon Foucault … and his new experiments have shown how this small motor drived the mirror with high steadiness requiring only a very low air-pressure, as a consequence of its good-design" [14].
    - *improving the efficiency of the fluid*: replacing steam (373 K) by compressed air, more dense and colder (290 K), thus more efficient, and eliminating condensation problems; special bellows built by Cavaillé-Coll, the famous organ builder, to drive the turbine; accuracy of the pressure: 0.2 mm for 30 cm of water column.
    - *improving the adjustment and* measurement *of the rotational velocity of the mirror*: design of a special "stroboscopic clock" by Froment, instead of the 1850-diapazon; velocity stability: 1/10 000 "for minutes".

Some uses in secondary Education

1. In junior high schools: Galileo's lanterns

Though the theme of the film is a bit difficult for very young children, ask them to play the scene with Galileo's lanterns, using, of course, true lanterns with true candles: sure, you will get a clear success! Of course, they will not "measure" the speed of light, but they'll understand easily Galileo's procedure, specially: why did Galileo ask the protagonists to proceed at first *very close to one another?* Evidently, he aims at *calibrating the response time* of the experimenters. And young children can try this!

In order to get an evaluation of this response time, you can ask children to sing, at least to sing the scale! Remember that Galileo was a good musician and he used to measure time intervals by beating time while singing! If your children are reluctant to sing, they can at least "speak" the scale. And you get the correct answer: the response time is about the time for "C-D", say 1/10 to 1/5 of a second. Then you can develop any commentaries about this experiment, about the "science landscape" at Galileo's time, about Galileo's life and works and his place in History of Science.

2. In higher level high schools: Römer and Jupiter's moons; Bradley and stellar aberration

Both scenes in the film afford support to elementary demonstrations.

Jupiter's moons: improvise a short play aiming at making clear the phenomenon interpreted by Römer, "the man who discovered the finite speed of light": a broad-shouldered pupil, keeping still at the centre, is the Sun; the teacher is the Earth, slowly rotating about the Sun; at about five times the Earth-Sun distance, another –strong– pupil is Jupiter; a very light girl is Io, rapidly twirling around Jupiter. You explain it all!

Stellar aberration: explain what aberration is using analogy with falling rain, using an umbrella on a rainy day; ask children to run under the rain! Then, again with a still Sun at the centre, the teacher –pointing to a virtual star with a stick and moving around the Sun– shows how the stellar aberration results in a small ellipse described in one year by the star [4, 16]. From which the speed of light!

The film at University level

We shall here suggest only two specific uses, quite adapted for undergraduate students. The specific questions of History of science will be examined in the next section.

The spinning-mirror experiment: repeat Foucault's spinning mirror experiment. Modern adaptations are necessary –it is not a replication– but *in full respect of the main characteristics of the original experiment*. What do we intend? Of course, the heliostat can be replaced by any source (e.g. a laser) and the organ bellows drive by an electric motor displaying its rotation velocity: what you need is light, and to rotate the mirror, anyhow. On the other hand, the sizes of the mirrors, the rotational velocity and the order of magnitude of the distance between the two mirrors should be respected. Performing the experiment *à la Foucault* is an excellent approach to the story of the speed of light!

Römer's discovery and light models: An interesting discussion! Ask students to retrieve implicit models of light embedded in historical interpretations. For example, from Römer's data, Huygens deduces an explicit value of the speed of light, simply dividing the distance by the time : 22 minutes to cross the Earth's orbit. Is the result model-dependent (waves or particles)? Frame of reference dependent? In the classical wave model –including ether– shouldn't you need to know the Earth's velocity *relatively to ether*? Is it large? Null? Nobody knows. Huygens –the father of this classical wave model– ignores the question. Discuss that.

The film as a tool for History of Science teaching

Wizards of Light can be used as an excellent support for introducing some concepts of History of Science. We give below some tracks.

1. The speed of light as a side-effect of the longitude problem

As related in the second section of this paper, the finite speed of light was a quite indirect and unexpected consequence of the long search for a tractable solution to the problem of longitudes at sea (for a general public survey of this problem, see [21]).

From 1669, Cassini –the best expert in terrestrial longitudes based on Jupiter moon periods– is called by Colbert to the newly built observatory in Paris. There, he will soon encounter Picard, La Hire, Richer and Huygens –also called by Colbert as an expert in longitudes at sea– and the Danish Ole Römer. This is, maybe, the first example of a *planned research* program initiated by the State top authorities. Cassini comes with his newly published ephemerides of Jupiter moons, and what happens? As we know, the finite speed of light will unexpectedly result from the detailed studies of the satellites irregularities. We emphasize that, though the name of Römer remains attached to this discovery –likely due to his determination in supporting his interpretation– it results of the works of the whole crew of the Paris Observatory [3].

This is a very significant example of how science is conducted, mixing formal debates, theoretical models, planned research and chance, a very enlightening story: at first, a millenary formal debate implying most great philosophers and peaking with Descartes; on the other hand, at the beginning of the17th century, from practical needs of sailors, a long search for solving the longitude problem. The Jupiter moons discovery came just "by chance" as yielding a potential solution, unusable at sea but of great interest on dry land. Completely unexpectedly, the demonstration of the finite velocity of light will result. Such a transposition of a formal debate to the sphere of observational Astronomy makes the speed of light a component of what is usually accepted as the "scientific revolution" of Modern Ages.

2. The speed of light and the concept of infinity

There is some relationship between the speed of light and the concept of infinity which, as we know, was debated at the 16th and 17th centuries. The millenary questioning was: does light require time to travel from the observed object to our eye? Should we translate, in modern terms: "does light have a finite velocity"? Certainly not. And this is important in teaching History of science: we are continuously threatened by anachronisms!

The best example is with Descartes: a "successive propagation in space of a *tendency to motion* requiring no transport of matter", and nevertheless "out of time" as it appears from the parable of the stick and the blind man, and in the example of lunar eclipses. Furthermore, for Descartes, light propagates "more easily" in dense materials (water, glass) than in air. Should we translate by: "more rapidly", and consequently "with a velocity greater than infinity"? Obviously not. In addition, in a letter to Mersenne, Descartes maintains it "contradictory that an infinite velocity might exist in Nature" [5]. Thus, we shouldn't apply to Descartes' times our modern concept of velocity in the meaning of dimensional analysis, i.e. a length divided by a time. Good lesson!

In a history of science workshop, you can ask students to compare the practical definitions of "instant propagation" in Galileo and in Descartes, who worked roughly at the same times. A detailed examination of the text of the "Two lanterns" (scene in the film) and of Descartes' example of lunar eclipses, leads to the following conclusion: it is considered as "instant propagation" a motion in space too fast to be detected by our senses on usual tractable distances i.e. two or three leagues. This would mean, *for us*, that a velocity is considered as infinite if it is greater than, say, 100 km/s. And, curiously, the same order of magnitude is obtained either from Galileo or from Descartes. No "actual infinity", not even "potential infinity", just a step on the way of potential infinity.

We may remark, however, that a clear definition of "actual infinity" is given by Galileo in his "Two new Sciences" (a set that is isomorphic to one of its parts) and that, half a century later, Huygens' conclusions (above reported, second section) about Römer's data, imply the acceptation of an "actual infinity". This is well in the filiation of Bruno, and in clear opposition to Aristotle.

3. Motivations of a measurement

Following the film, you may ask students a reflection about the motivations of a measurement in Physics, and more generally in Science.

- In order to make a measurement, you need, at first, *something* to be measured. Concerning the speed of light, our story shows that it was not the case during many centuries. Following this millenary debate, the conclusion about the finite speed of light came only with Römer (1676) and Bradley (1728) as shown in the film.
- Once there is something to be measured, why should one measure it? The question is so less evident that neither Römer nor Bradley published an explicit value of the speed of light, even if it can easily be deduced from their data. Why? Actually, it was sufficient for them that the finite speed of light solved their problem: the irregularities in the period of Io and stellar aberration respectively. The explicit value of such a fantastic velocity didn't enter any theory nor any problematics and had no application. Indeed, the velocity of light will remain a matter of scientific curiosity until the middle of the 19th century.
- Thus, around the middle of the 19th century, the new feasibility of terrestrial measurements first evidenced by Fizeau [7] will, at last, renew the interest in the speed of light. The first measurement by Foucault in 1850 (evoked in the film), comparing the velocities of light in air and in water, was a *qualitative* one: to decide between two rival theories. This is a special type of motivation.
- On the other hand, the 1862 experiment, again by Foucault, was an accurate experiment of *Metrology*, strongly required by Astronomy. As shown in the third section of this paper, the motivations and the constraints are totally different! This experiment will give the speed of light a new theoretical and practical status: a standard for measuring distances (cf. second section). And the film ends on the suggestion that this velocity might be "independent of all".

The film and general public

With general public, for example in science festivals, any question can arise! They are so various that you cannot expect to predicting any. Be only ready, and be modest: remember that you are allowed not to know all and you can be unable to answer some questions. Avoid lengthy answers and one-to-one controversies about very specialized questions: think that you don't only have to answer the questioner, but your answer should arouse the interest of the whole audience. This is particularly true for questions about Relativity paradoxes or the basis of Quantum Mechanics, such questions – somewhat beyond the subject of the film– always arise! Apart from this reservation, all questions are welcome and the debate can be extremely rich.

References

[1] ARAGO, F. (1857, posthumous edition) *Astronomie populaire en 4 volumes*, J.A. Barral (ed.), vol. IV, p. 418.

[2] ARAGO, F. (1850). Note sur le système d'expériences proposé en 1838 pour prononcer définitivement entre la théorie des ondes et la théorie de l'émission, *C.R. hebd. Acad. Sci.*, 30, 489-495.

[3] BOBIS, L. & LEQUEUX, J. (2008). Cassini, Römer and the Velocity of Light, *J. Astronomical History and Heritage*, 11, 97-105.

[4] BRADLEY, J. (1728). An account of a new discovered motion of the fixed stars, *Phil. Trans. Roy. Soc. London*, 35, 637-660.

[5] COSTABEL, P. (1978). La propagation de la lumière sans transport de matière de Descartes à Huygens, *in* réf. [22], p. 83-91.

[6] DESCARTES, R. (1897-1913). *OEuvres*, Ch. Adams & R. Tannery (edts), Paris, Léopold Cerf, 13 vol.; 1964-1974, new revised and enlarged edition, Paris, Vrin-CNRS, 11 vol.

[7] FIZEAU, H. (1849). Sur une expérience relative à la vitesse de propagation de la lumière, *C.R. hebd. Acd. Sci.*, 29 , 90-94 et 132.

[8] FOUCAULT, L. (1850). Méthode générale pour mesurer la vitesse de la lumière dans l'air et dans les milieux transparents …, *C.R. hebd. Acad. Sci.*, 30, 551-560.

[9] FOUCAULT, L. (1854). Sur les vitesses relatives de la lumière dans l'air et dans l'eau, *Ann. de Chim. et de Phys.*, 3rd series, t. XLI, 129-164.

[10] FOUCAULT, L. (1862). Détermination expérimentale de la vitesse de la lumière; parallaxe du Soleil, *C.R. hebd. Acad. Sci.*, 55, 501-503.

[11] FOUCAULT, L. (1862). Détermination expérimentale de la vitesse de la lumière; description des appareils, C.*R. hebd. Acad. Sci.*, 55, 792-796.

[12] FOUCAULT, L. (1878) *Recueil des travaux scientifiques de Léon Foucault*, posthumous édition published par Mrs Foucault, his mother, in collaboration with C.M. Gariel, Paris, Gauthier-Villars.

[13] GALILEI, G., or "GALILEO" (1638). *Discorsi … intorno a due nuove scienze …*; 1890-1906 Firenze, Edizione nazionale, A. Favaro (ed.), t. VIII, p. 87-89; 1974, S. Drake, english edition, *Two new sciences*, Madison; 1995, M. Clavelin, French edition, *Discours concernant deux sciences nouvelles*, 2nd ed., Paris, PUF, p. 37-39.

[14] GIRARD, L.D. (1863). Nouveau mode d'action de l'eau motrice, et réalisation de très-grands siphons, *C.R. hebd. Acad. Sci.*, 56, 258-260.

[15] HUYGENS, C. (1690). *Traité de la Lumière*, Leyde ; 2000, new release, Dunod, Paris, p. 54-60.

[16] LEHOUCQ, R. & SAVOIE, D. (2002). L'aberration stellaire, *Pour la science*, nº 299, 20-23.

[17] PICOLET, G. (1978,). La vitesse de la lumière chez les auteurs anciens, *in* ref. [22], p. 1-17.

[18] RASHED, R. (1978). Lumière et vision: l'application des mathématiques dans l'Optique d'Ibn al-Haytham, *in* ref. [22] p. 19-44.

[19] RÖMER, O. (1676). Démonstration touchant le mouvement de la lumière trouvée par M. Römer de l'Académie royale des sciences, *Journal des sçavans*, Paris, 7 décembre 1676, 233-236.

[20] RÖMER, O. (1677). A demonstration concerning the motion of light, communicated from Paris, in the *Journal des sçavans*, and here made in English, *Philosophical Transactions of the Royal Society of London*, 12, 893-894.

[21] SOBEL, D. (1995). *Longitude*, New-York, Walter & Co; 1996, French version, Paris, J.C. Lattès.

[22] TATON, R., (ed.) (1978). *Roemer et la vitesse de la lumière*, Proceedings of the 1976 Paris Workshop, tercentenary of Roemer's discovery of the finite speed of light, Paris, Vrin.

[23] TOBIN, W. (2003). *The Life and Science of Léon Foucault*, Cambridge University Press.

[24] TOBIN, W. (2002). *Léon Foucault* (French adaptation par J. Lequeux), Paris, EDP-sciences.

Appendix: the film crew

Directors	:	Christine AZÉMAR and Serge GUYON (SCAVO)
Science writers	:	Pierre LAUGINIE (GHDSO) and Alain SARFATI (Lab. Aîmé COTTON and SCAVO)
Cameraman	:	Max RELID (SCAVO)
Documentation	:	Maria DESCARGUES (SCAVO)
Dialogues	:	Laurent BARATON (École polytechnique)
Producer	:	Université Paris-Sud-11, scavo
Co-producers	:	Paris Observatory, Triangle de la Physique
Texts (booklet, DVD-2)	:	Pierre LAUGINIE

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AN AIM FOR SCIENTIFIC EDUCATION IN FRANCE: THE IMAGE OF THE NATURE OF SCIENCE. THE CONTRIBUTION OF THE HISTORY OF SCIENCE IN PHYSICS COURSES

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Abstract

Physics and chemistry programs at the secondary school level in France recommend introducing components of the history of science as well as investigative activities. These two aspects are the result of an implicit general goal of these programs related to the representation of the nature of science and scientific inquiry. We first demonstrate how our historical and epistemological analysis led us to distinguish different learning goals associated with different points of view about science. We then show how these goals can generate classroom activities involving collective inquiry that is based on the implementation of "historical" documents which may or may not be paired with experiments. The approach used in some of our proposals is innovative: the reading assignment of the kit of documents is divided among all students and the synthesis consists in collectively drawing a diagram providing a synoptic visualization of the epistemological objective. Finally we discuss the tensions that our choices created among teachers.

Introduction

For several years, the introductions of the French science programs recommend to introduce the history of science (HS) at all levels of teaching[1]. Emphasis is placed on a "cultural" dimension which is poorly defined but essentially refers to elements of epistemological nature. By the same token, these introductions recommend approaches that are based on student inquiry. Although implementing these pedagogical situations satisfies above all the concern of familiarizing students with scientific process skills and attitudes as well as giving them better mastery of scientific concepts, this strategy also refers to the image of the nature of science and scientific inquiry. But in both cases the epistemological dimension of scientific

[1] For example, see the introduction of the high school science program: B.O. (2008).

education is more suggested than explicitly stated. The targeted level of competencies never addresses the issue of the representation of science; it focuses on scientific knowledge and process skills only. Moreover, the programs suggest few examples of activities based on HS: most of them are means to learn scientific content and convey a reductive and false image of science.

Besides, several studies (Guilbert & Meloche, 1993; Robardet & Vérin, 1998; Roletto, 1998; Mathews, 2003; Lederman, 2007; Höttecke, 2010a) conducted in France and abroad demonstrate that science teachers are unaware of the epistemological issue of scientific education. Even those for whom this is a significant dimension tend simply to privilege the acquisition of scientific knowledge and process. These studies also show that many teachers have an outmoded vision of the sciences (empirically inductive and naïvely realistic) and unconsciously convey this stance to their students through the way they teach.

Other studies (Leite, 2002; Mathy, 1997; Beaufils & Maurines, 2008) reveal that textbooks convey a reductive and false image of science, either through the approaches that are chosen, their vocabulary, or their choice of historical elements. These elements refer to only one person who is most often well known and has intuitive genius; they focus on the discovery of the laws of science and their interpretation.

This situation led us to start a research program that examines whether the introduction of the history of science in physics courses can help to address the epistemological issue of scientific education.

Questions explored

The issue of greater authenticity in representing the nature of science in physics classes has three main aspects. First, how do we characterize this authenticity? Can historical examples be found that are compatible with physics curricula and students' academic level? If so, what student activities support these objectives? The first question invites an historical and epistemological analysis. The other two are subtended to the approach taken by experimentation that seeks to develop pedagogical resources and activities that can be assigned to students.

The underlying hypothesis of our research is that resources and activities, which offer a heightened sense of the reality of the nature of scientific knowledge and activity, can provide a broader scientific cultural integration for students and have a positive effect on their relationship with science as an academic discipline. We confer with Solbes and Traver (2003) on that point. This is a significant societal issue, given students' current disaffection with the branches of basic science.

Our position is unique in France: almost all other recent work concerned with the use of the history of science in physics courses (Guedj, 2005; de Hosson & Kaminski, 2007) does not explore this aspect; its goal is the acquisition of scientific concepts. The historical elements introduced in these different studies are minimized (names of scientists, dates) and concern controversies over the interpretation of specific phenomena (vision, the movement of a projectile in a referential that is moving in relation to another, etc.). In English-speaking countries this position, known as NoS —"the nature of science"— has been used for more than twenty years. It allows specific goals of learning on the epistemological level to be clearly stated, as demonstrated by the recent publications of the group of experts working on Project 2061, which was begun in the United States in 1986 by the American Association for the Advancement of Science (AAAS). As we will demonstrate further, this pedagogical stance also allows teachers to create activities that are perfectly defined both from the point of view of the targeted goal and the topic under discussion.

The epistemological and historical framework that we have adopted privileges the individual[2]: from this historical perspective, scientific knowledge is considered to be the result of activities performed by individuals in the socio-cultural context of a given historical period. But these activities do not correspond to ordinary intellectual constructs, since they must follow rules and demonstrative procedures and also be validated by confronting them with the results of observation and experimentation.

Methods

Our work initially consisted of three complementary analyses. One concerned the former and current high school curricula. Another one dealt with old and new physics textbooks in French and English. The third one consisted of an epistemological analysis focused on scientific activity and different approaches (internal and external) taken by history of science. These analyses allowed us to discern different characteristics of science that may be used as temporal invariables and constitute key points of an authentic image of science and scientific activity upon which learning goals may be based *a priori*.

[2] We refer to Mathy (1997) and Gagné (1994) on that point.

These analyses led us to create and design resources and activities for students that were both appropriate to a given level of instruction and fulfilled a specific epistemological objective. Before spreading our pedagogical propositions, we sought to improve them in taking into account teachers' feedback. We contacted various colleagues who had replied to a first inquiry on their relationship to the use of HS in teaching (Beaufils et al, 2010) and expressed interest in receiving complete pedagogical resources. Each dossier was accompanied by a set of questions that allowed teachers to share their reactions to the objectives, documents, type of collective activity, and goal of the assignment (presented as a diagram). On a more general level, we sought an initial opinion of the feasibility of the dossier vis-à-vis the targeted teaching level (especially grade 10). The information returned to us completed the first synthesis of material that resulted from a session of ongoing teacher training and allow us to improve our proposals.

We have arrived at a new stage of our project, which includes trial runs of different resource kits by teachers who have volunteered for the task and the constitution of an informal group in order to adopt a symbiotic strategy in the future (Höttecke, 2010b).

Design of innovative pedagogical units. Epistemological learning goals

Our epistemological and historical analyses lead us to the "consensus view" of the nature of science which comprises a set of aspects widely accepted in standard documents (Project 2061 for example) and philosophy of science (Lederman, 2007). The list below (figure 1) presents some of these characteristics, ranging from the difficulties encountered by scientists to questions about the social dimension of the work as well as the different types of activities and methods and the nature of models, theories, and proofs. These features of science could be chosen to define the goals of introducing the history of science in the teaching of science.

- scientific activity is a nexus of controversy.
- scientific knowledge follows criteria of internal coherence, simplicity, and strength and has to be confronted with facts based on observation and experiments.
- a scientist does not work in isolation but within a community that participates in testing established scientific knowledge.
- there is a strong relationship between technical questions and the evolution of ideas.
- knowledge has evolved over time (through continuity and ruptures).
- there is an interdependence between science and society.
- there are relationships between science and beliefs.

Fig. 1: Some characteristics of scientific activity.

In our opinion, introducing the history of science in science courses highlights episodes of scientific activity of various lengths seen through a series of critical epistemological filters. We would like to observe first of all that since these goals focus on epistemological aspects, they may be independent of conceptual or process learning objectives, which therefore can be only secondary, if not completely absent. This position is close to the one presented by Lederman and Abd-El-Khalick (1998) about activities relating to NoS. This choice obviously creates tensions with teachers' own representations of the teaching of science and their daily teaching practices.

It is also important to note that the choice of an objective such as "showing the interaction between scientists" is obviously a reduction of a historical reality which generally results from many interacting factors. But this reduction is not the same as that which we denounce in the introduction. On the other hand, the idea is that some of these goals are worked at every level of education so that a student has all them discussed at the end of his schooling.

Choice for the elaboration of innovative pedagogical units

Several choices determined the direction of our pedagogical research.

The first one was to pinpoint the field of optics. This choice was the result in part of the area of specialization of our team members and the identification of several "historical situations" that could be integrated into physics curricula from grade 7 to grade 12.

The second choice was based on a didactic position that by general consensus seeks to assign an active role to the student and was to suggest student activities beyond simply providing documentary resources for teachers. We chose to design collective activities that permit complementary or contradictory interactions and the development of shared knowledge, that is to say to integrate them with investigative situations. Concurring with Morge and Boilevin (2007), we define an investigative situation as one in which *students learn by performing tasks that are not exclusively experimental in nature and by taking part in a process of validating their peers' results.*

We designed two types of investigative situations: some pair experiments and texts; others are based only on documents. According to a practice recommended for primary school teaching and also developed in biology and geology teaching at the secondary level, we believe that the documents can be introduced at any time in order to facilitate different tasks, for example in order to state or validate hypotheses related to the nature of science.

We created dossiers on various topics, including kit of texts for students and guideline for the teacher describing the objectives of the activity and how it is to be conducted. The selection of historical material was based not only on primary texts but also on texts written by historians or scientists keen on history. We have even adapted some of them. Indeed, our primary objective is to provide students with texts easy to read. The documents we elaborated present information in different forms —texts, diagrams, drawings, etc.

Experimental investigation

In our first study (Maurines and Mayrague, 2005), following the hypothesis that some teachers prefer situations that link texts with experiments, since they might not want to deviate from their preferred conceptual or procedural learning goals, the reading of historical texts was coupled with doing an experiment described or inspired by the texts. This choice would be useful when asking students to practice science while at the same time foregrounding the historical process of developing knowledge. This approach becomes most meaningful when the targeted goal stresses the process of validating a model by confronting its prediction with facts derived from observation and experimentation; the choice seems artificial, however, and therefore inappropriate in relation to a goal like studying "interactions between scientists."

We outline in figure 2 one possible way of structuring a science lesson on the epistemological objectives "scientists test their explanations against facts that result from observation and experiments" and "scientists valorize those explanations that allow for a large number of facts to be interpreted simply"; to develop the notion that white light is composed of different "colored lights"; and to ask students to practice science taking an experimental approach.

<u>Appropriating or/and constructing a problem</u>

- The presentation of an experiment involving the dispersion of white light through a prism.
- create a conflict: compare De Dominis' and Newton's models.
- state hypotheses inspired by the texts: Is white light homogeneous and are colors created uniquely by the action of the prism?

<u>Resolving of the problem</u>

- develop experiments that allow the hypothesis to be tested, revealing its consequences.
- confront expectations with the results of the experiment.
- invalidate the model of homogeneous white light.

<u>Establishing knowledge</u>[3]

- scientists test their explanations against facts that result from observation and experiments; they valorize those explanations that allow for a large number of facts to be interpreted simply.
- white light is heterogeneous and composed of colored light.

Fig. 2: A possible structural outline of an activity combining texts and experiments (Topic: the dispersion of white light).

[3] Establishing the knowledge collectively constructed by students consists in recognizing that it conforms to the knowledge shared by the scientific community.

In this type of investigative situation, the risk of drifting off course toward an emphasis on knowledge and methods is especially high. It is fundamentally important that the stage of establishing knowledge be related not only to the scientific notions developed but also to the historical process of developing knowledge and to what it teaches us about the nature of science and scientific practice.

But the choice of basing experiments on a historical foundation is problematic: not only does the presentation of knowledge as answer to a problem not require an historical reference, but on the contrary, the historical reference that is introduced to convey a synoptic idea of scientific practice in a particular historical period requires that students put themselves into the conditions and circumstances of scientific and technical knowledge applicable to the historical period in question. This strategy raises the possibility of asking students to comprehend what was actually at stake then—for instance, the ingenuity of a particular approach or the revolutionary nature of an idea in its time and the nature of the historical elements that are introduced.

Documentary investigation

Other resources (Maurines et al, 2009) propose learning situations that we describe as "documentary investigation." These situations are based on kits usually composed of about ten interrelated documents characterized by a "point of entry" that is directly related to the goal to be achieved: this might take the form of scientists' names, for instance, or technical achievements (see examples in Appendix 1). The reading assignment is divided among all members of the class and the synthesis is made in class under the teacher's "guidance". Figure 3 shows a way of structuring the work done in class:

<u>Appropriating or/and constructing a problem</u>

- integrate the topic into the teacher's learning sequence for the class; highlight questions of a historical nature: *how do we know that...?*
- specify a specific problematic that sets the goal to be achieved.

<u>Resolving the problem</u>

Documentary work (as homework or in class, individual or in small groups)

- present documentary resources.
- distribute the documents among the members of the class.
- read documents and prepare the lesson in which the synthesis will be done, guided by instructions that specify the type of information to be culled from the text; who, what, how, why, etc. (in relation to the documents and the question).

Reaching a synthesis (students will do this collectively based on each class member's contribution).

<u>Establishing knowledge</u>

- the teacher generalizes the results that were found by pupils and shows the connection between how science worked in the past and the present situation.

Fig. 3: A possible structural outline of an investigative situation based on historical texts.

Since the reading assignment is divided among all members of the class, the synthesis that is made in class under the teacher's "guidance" is supposed to produce a specific type of result: a diagram (see example in figure 4, another one is presented in Appendix 2) that provides a synoptic visualization of the information that has been gathered and illustrates the targeted goal. The choice of constructing a diagram to represent the synthesis corresponds to the idea that this type of synoptic representation is adapted to our pedagogical goals. In the case of the goal-related scientific attribute "interactions among scientists", it is important actively to resist the image of the solitary scientist who had a stroke of genius; instead, the approach we are proposing will guide students toward the image of a complex interaction that was developed over time.

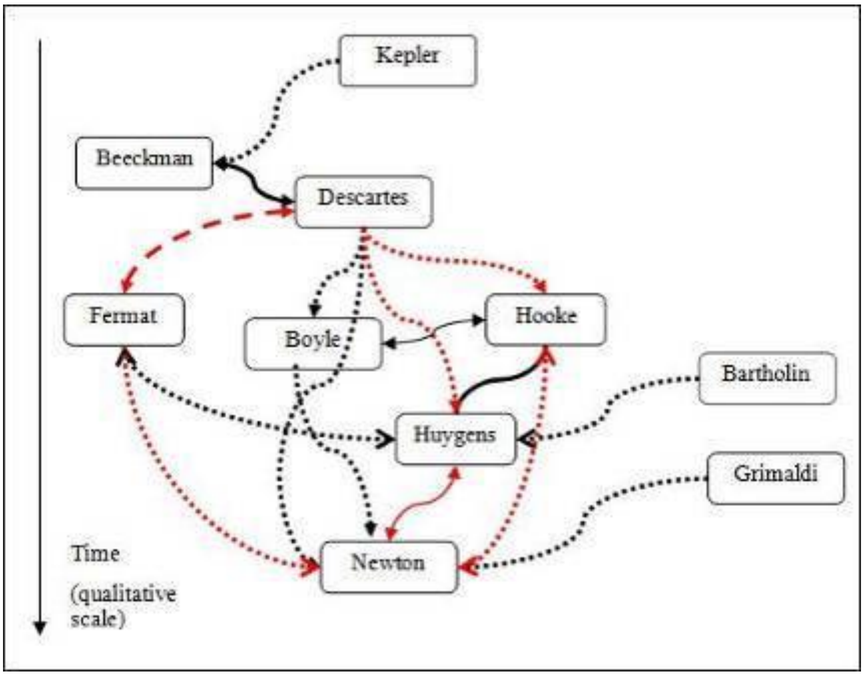

Fig. 4: example of diagram on the epistemological objective "interactions between scientists and the topic "laws of refraction"[4].

First feedbacks from teachers

Some of the "historical" activities that we suggest are innovative in several ways. The issues we propose and their specific goals concern the representation of science and scientific activity and not the mastery of a scientific concept. Our document kits go against the grain of the insets and similar texts included in current textbooks. Our suggested activities involve students collectively. Finally, the goal of these activities is to create and design synoptic presentations, especially diagrams that use semantic networks as a model.

The first kit we elaborated concerns the topic of refraction at the grade 10 level and suggests a documentary investigative situation on the epistemological goal "interaction between scientists". The majority of responses given by the 15 teachers who accepted to send us back the questionnaire on the feasibility of our proposal indicated that the objective was both important and interesting, stressing the fact that the evolution of science, with its complexity and social dimension, should be demonstrated. Responses were largely positive in favor of presenting this kit at the grade 10 level but some colleagues expressed reserve and were concerned that the intellectual level of the students would be overly challenged by documentary activities involving reading that was beyond their critical ability. The answers to the questions on the pedagogical organization range from positive feedback arguing for a simple activity that could be integrated into the complete set of activities students are asked to perform, to negative feedback, arguing that the texts were too difficult, that the synthesis students were asked to make (a diagram) was too abstract, and that the context was too exclusively cultural, independent of notional learning, requiring highly motivated students.

By way of conclusion: some remarks on the teachers' relationship to the history of science in science teaching

From the one hundred and twenty three responses we received (to open and closed questions), we have highlighted two points: 1) their responses to the assistance they expected in integrating the history of science into the teaching of physics and chemistry; and 2) the place of the history of science in academic disciplines.

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[4] The arrows are different because they are different types of interactions. Continuous double arrow: direct interaction. Dashed double arrow: epistolary interaction. Dotted single arrow: knowledge of the works. Red double arrow: interaction with controversy. Red single arrow: opposition of points of view.

In response to teachers' assistance in introducing the history of science in their courses, we asked them to choose from among five different resource kits: 1) references to Internet sites; 2) a historical bibliography; 3) a pedagogical bibliography; 4) annotated historical documents; and 5) complete lesson plans. More than half the teachers chose two resources, and from among them, the pair "annotated historical documents + complete lesson plans" was a popular choice. That result was a positive affirmation of our project.

One question remained, however, as to the amount of time teachers were willing to devote to the history of science in their courses. If 60% of answers received were very in favour of student activities (documentary group work, for instance) focused on the history of science in the case of an interdisciplinary assignment, no more than 45% were supportive of this type of activity in physics and chemistry classes.

Bibliography

American Association for the Advancement of Science. (1993). *Benchmarks for Science Education*. http://www.project2061.org/publications/bsl/online/index.php

BEAUFILS D. & MAURINES L. (2008) La loi des sinus : quelques considérations épistémologiques et didactiques. *Le Bup,* 909, 1345-1351.

BEAUFILS D.; MAURINES L. & CHAPUIS C. (2010). Compte-rendu d'enquête sur l'histoire des sciences auprès d'enseignants de physique et chimie. *Le Bup*, 924, 581-598.

B.O. (2008). Programme du collège. Physique-chimie. N° 6 du 28 août. http://media.education.gouv.fr/file/special_6/52/7/Programme_physique-chimie_33527.pdf

DE HOSSON C. & KAMINSKI W. (2007). Historical Controversy as an Educational Tool: Evaluating elements of a teaching-learning sequence conducted with the text "Dialogue on the Ways that Vision Operates". *International Journal of Science Education*, 29 (5), 617-642.

GAGNE B. (1994). Autour de l'idée d'histoire des sciences: représentations discursives d'apprenti(e)s-enseignant(e)s de sciences. *Didaskalia*, 3, 61-78.

GUEDJ M. (2005). Utiliser des textes historiques dans l'enseignement des sciences physiques en classe de seconde des lycées français, *Didaskalia*, 26, 75-95.

GUILBERT L. & MELOCHE D. (1993). L'idée de science chez des enseignants en formation: un lien entre l'histoire des sciences et l'hétérogénéité des visions? *Didaskalia*, 2, 6-30. http://documents.irevues.inist.fr/handle/2042/20182

HÖTTECKE D. & SILVA C.C (2010a). Why implementing history and philosophy in school science education is a challenge: an analysis of obstacles. *Science & Education* DO1 10.1007/s11191-010-9285-4.

HÖTTECKE D.; HENKE A.& RIESS F. (2010b). Implementing history and philosophy in science teaching: strategies, methods, results and experiences from the European HIPST project. *Science & Education* DO1 10.1007/s11191-010-9330-3.

LEDERMAN N.G. (2007). Nature of Science: Past, Present and Future. In S.K. Abell & L.G. Lederman (Eds). *Handbook of research in science education*. 831-839. Erlbaum.

LEDERMAN N. G. & ABD-EL-KHALICK F. (1998). Avoiding de-natured science: activities that promote understanding of the nature of science. In W. McComas (Ed). *The nature of science in science education. Rationales and strategi*es. 83-126. Kluwer.

LEITE L. (2002). History of Science in Science Education: Development and Validation of a Checklist for Analysing the Historical Content of Science Textbooks, *Science & Education* 11: 333–359.

MATHEWS M. (2003). The nature of science and science teaching. In Fraser B.J. & Tobin K.G. (Eds). *International Handbook of science education*. 981-999. Kluwer.

MATHY P. (1997). *Donner du sens aux cours de sciences. Des outils pour la formation éthique et épistémologique des enseignants.* De Boeck, Paris, Bruxelles.

MAURINES L. & MAYRARGUE A. (2005). How the investigations of the students' reasoning on waves and the history of physics can help us to develop research-based innovative units for secondary students on the velocity of light ? *8th IHPST conference*, Leeds.

MAURINES L.; BEAUFILS L. & CHAPUIS C. (2009). Travailler l'image de la nature des sciences et de l'activité scientifique grâce à l'histoire des sciences. *Actes des 6ième rencontres de l'ARDiST.* Nantes. http://ardist.aix-mrs.iufm.fr/wp-content/textes%20communications/22_Maurines%20L

MORGE L. & BOILEVIN J.M. (2007). *Séquences d'investigation - Physique-chimie* CRDP d'Auvergne.

ROBARDET G. & VERIN A. (1998). L'enseignement scientifique vu par les enseignants. *Aster*. 26, 3-10. http://documents.irevues.inist.fr/bitstream/handle/2042/8689/ASTER_1998_26_3.pdf?sequence=1

ROLETTO E. (1998). La science et les connaissances scientifiques : points de vue de futurs enseignants. *Aster*, 26,11-30. http://documents.irevues.inist.fr/bitstream/handle/2042/8690/ASTER_1998_26_11.pdf?sequence=1

SOLBES J. & TRAVER M. (2003). Against a negative image of science: history of science and the teaching of physics and chemistry. *Science & Education*. 12, 703-711.

Appendix 1: examples of texts from the student kit on the objective "interaction between scientists" and the topic "interpretation of the law of refraction"

Document DidaScO | *Ne pas diffuser svp*

Dans son ouvrage *Histoire de l'arc-en-ciel*, B. Maitte retrace, depuis l'Antiquité la multiplicité des questions que l'on a pu, à chaque époque, se poser, et auxquelles correspondent autant de descriptions différentes….

Parmi les savants, Robert Boyle, chimiste anglais…

[…]

Ses expériences partent d'un présupposé tiré de Descartes dans une direction autre que celle empruntée par Hooke et Huygens : alors que ceux-ci retiennent l'hypothèse d'un monde plein et sont partisans d'une théorie ondulatoire de la lumière, Boyle en fait un flot de « particules qui composent chaque rayon particulier et parviennent dans l'organe des sens », provoquent des chocs sur la rétine, ainsi que « le *très ingénieux Descartes l'a parfaitement bien supposé* »

Pour Boyle, ces particules arrivent dans un ordre et avec un degré de vitesse différents sur les parties superficielles des corps. La lumière est alors troublée, ce qui fait que nous disons « *que le corps vu est soit blanc, soit noir, soit rouge, soit jaune, soit d'une autre couleur déterminée* » La couleur n'est donc pas inhérente aux corps, mais une propriété prise par la lumière sous l'influence des corps. Est-elle « *un mélange [de lumière] avec des ombres… la rotation des globules célestes cartésiens, ou… quelque autre voie.., je ne prétends pas l'affirmer* ».

[…]

Note : Robert Boyle, scientifique anglais (1627 - 1691) est l'un des premiers adeptes de la méthode scientifique et l'un des fondateurs de la chimie moderne. Il ne s'intéresse guère à la lumière en elle-même, mais aux modifications colorées qu'elle subit en relation avec la composition de diverses substances.

Document DidaScO *Ne pas diffuser svp*

Dans son cédérom Histoire des idées sur la lumière, C. Bracco consacre un chapitre à Christiaan Huygens mathématicien, astronome et physicien néerlandais du XVII[e] siècle.

Spath d'Islande montrant l'effet de la double réfraction (photographie Pierre Thomas – NdR)

Il place deux spaths[1] l'un derrière l'autre. Le faisceau incident est dédoublé par le premier ; les faisceaux émergents constituent deux faisceaux incidents pour le second. Il observe que chacun de ces faisceaux se dédouble en général, si bien qu'il obtient quatre faisceaux émergents comme il pouvait s'y attendre. Cependant ces faisceaux n'ont pas la même intensité et deux d'entre eux s'éteignent pour des orientations particulières du second cristal.

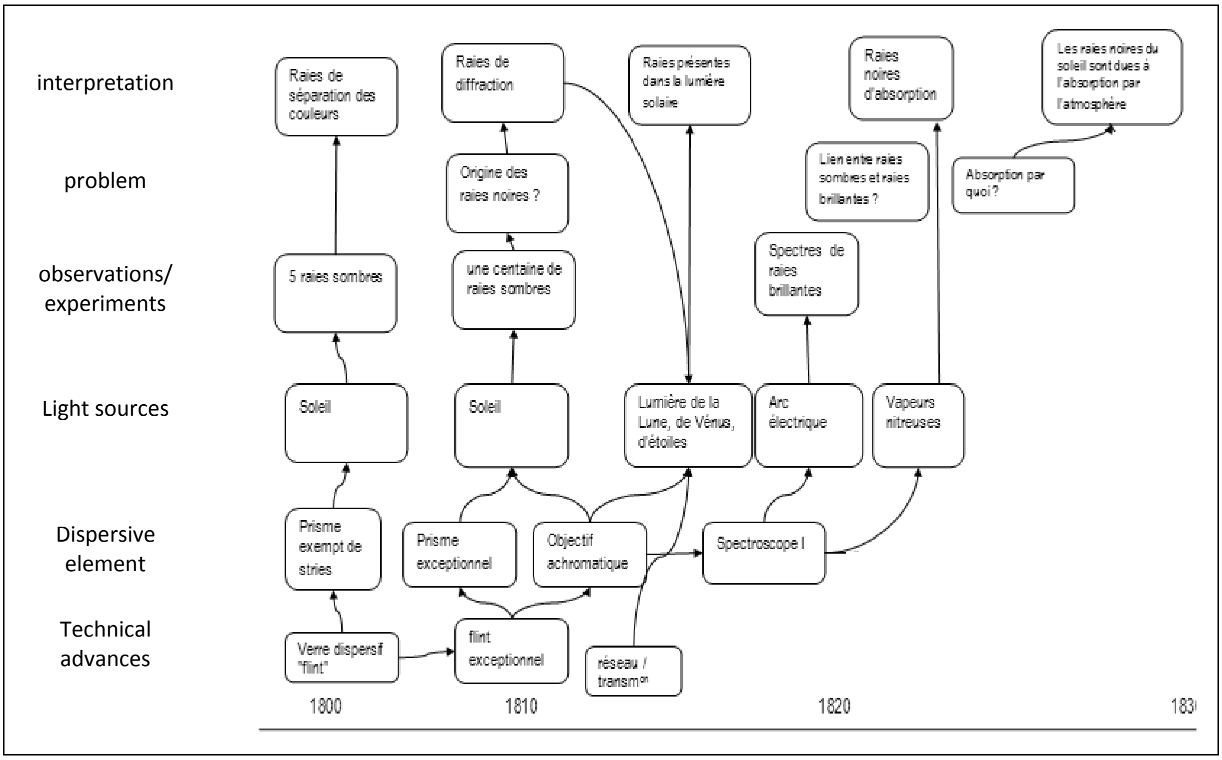
Huygens n'a pas d'explication de ce phénomène et écrit : « … *car bien que je n'en aie pas pu trouver jusqu'ici la cause, je ne veux pas laisser pour cela de l'indiquer, afin de donner occasion à d'autres de chercher. Il semble qu'il faudrait faire encore d'autres suppositions outre celles que j'ai faites ; qui ne laisseront pas pour autant de garder leur vraisemblance, après avoir été confirmées par tant de preuves […]. Il semble qu'on est obligé de conclure que les ondes[2] de lumière, pour avoir passé le premier cristal, acquièrent certaines formes ou disposition… mais dire comment cela se fait, je n'ai rien trouvé jusqu'ici qui me satisfasse* ». (Huygens. *Traité de la lumière*,1690)

Cet aveu d'échec sera un des arguments utilisés par Newton, après la mort de Huygens, pour discréditer cette théorie.

Note : l'application la plus spectaculaire de la théorie de Huygens, savant néerlandais (1629-1695) est l'explication de la biréfringence[3] du spath d'Islande rapportée par Érasme Bartholin[4] en 1669 : ce cristal possède la propriété de dédoubler les images des objets.

[1] Spath d'islande : calcite, carbonate de calcium cristallisé. Quand la calcite est transparente, on l'appele spath d'islande, car les premiers échantillons provenaient d'Islande. Ces cristaux tapissaient les parois de fissures et de cavités rocheuses (geodes).
[2] Voir document Huygens2
[3] Désigne le fait que le materiau provoque deux refractions différentes.
[4] Rasmus Bartholin, ou Erasmus Berthelsen, né à Roskilde le 13 août 1625, était un médecin danois.

Appendix 2: example of diagram on the epistemological goal "relationships between technical questions and the evolution of ideas" (Topic: birth of the spectroscopy)



ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 165.

HOW TO READ THE HISTORY OF SCIENCE IN SCIENCE SCHOOL TEXTBOOKS FROM A SOCIOLOGICAL PERSPECTIVE: THEORETICAL AND METHODOLOGICAL CONSIDERATIONS INSPIRED BY THE IDEAS OF BRUNO LATOUR ABOUT NON-HUMANS AND NETWORKS

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Abstract

We have carried out a historical research of chemistry school textbooks published in Spain in the course of the twentieth century. Based on our results we point out some theoretical and methodological considerations which are important to do a re-read of the way the history of science is commonly presented in school science textbooks. We propose to stress in two essential ideas of Bruno Latour's thought: 1) The role and agency of non-humans in the scientific knowledge and 2) An interpretation of history in science textbooks as a complex network, where there are not clearly-defined limits between history, philosophy and sociology of science and where history can be understood as a part of a "structure", which becomes more or less stable depending on the way as their "fibres" are "interwoven" in the narrative of science textbooks.

This is not a work on History of Science; this work is framed in one of the research lines between Science Education, the HPS approach. Researchers in HPS work on didactic reflection about the relevance of including contents of History and the Philosophy of science to improve learning and teaching science. The importance of the HPS approach in Science Education has been recognized by multiple authors attributing hundreds of arguments that justify their explicit inclusion not only in the curricular contents and classroom, but also in pre and in-service teacher programs. We have grouped those advantages in five categories, an extended frame of the three operational concepts for the west education in the proposal by Kieran Egan (Schulz, 2009):

Initially an intellectual dimension, which recognizes that science is historically and culturally dynamic, a cumulative body of knowledge that generates the techno-social company with an impact on societies and cultures. In the second instance we can recognize an individual contribution in which the HPS allows students and teachers to develop their potential as autonomous individuals including the culture of creativity and ability to think critically. A socioeconomic dimension where the HPS approach contributes in orienting education towards useful socio-economical and socio-political aims, acting on three levels: application, civic responsibility and democratic citizenship. A didactic dimension that can help improve learning science, and an affective dimension that personalizes, humanizes and demystifies scientists and scientific activity.

In practice these aims are translated to diverse actions that go from direct interventions in the classroom of which we can mention:

Historical episodes (AAAS, 1990; Klassen, 2009; Nersessian, 1995; Russ et al., 2009); historical anecdotes (Herron, 1977); discussions about historical explanations (Lochhead and Dufresne, 1989), discussions about the historical context of different discoveries (Arons, 1988), discussions about historical texts (Pessoa de Carvalho and Infantosi Vannucchi, 2000); staged historical episodes (Kipnis, 1998; Solomon, 1989); historical readings combined with experimental activities (Kipnis, 1998; Lin, 1998); historical reconstructions (Allchim, 2006; Izquierdo, Vallverdú, Quintanilla and Merino, 2006); historical cases (Bonini Viana and Alves Porto, 2001; Dawkins and Glatthorn, 1998; Elkana, 2000; Heilbron, 2002; Klassen, 2009; Kølsto, 2008); historical narratives (Clough, 1997, 2006; Conant, 1957; Irwin, 2000; Klopfer and Cooley, 1963; Matthews, 1994; Stinner, 1995; Stinner et al., 2003; Solomon et al., 1992); time lines (Metz et al., 2007); analytical reviews of researcher's original works (Jaffe, 1938; Klopfer, 1969); historical experiments (Ellis, 1989; Heering, 2009; Kipnis, 1996; Seroglou, Koumaras and Tselfes, 1998); historical vignetes (Roach and Wandersee, 1995); historical controversies (Kipnis, 2001; Madras, 1955; Niaz and Rodriguez, 2002); biographies (Izquierdo, 1996; Jaffe, 1938); historical models (Dolphin, 2009; Forge, 1979; Justi and Gilbert, 1999a, 1999b, 2000; Ryder and Leach, 2008; Stinner, et al., 2003); different activities using historical data or historical materials (De Berg, 2007; De Castro and Pessoa de Carvalho, 1995; Solbes and Traver, 2003).

In teacher formation programs, in curricular documents and textbooks. Indeed, these last ones are what we stressed in this work. Why do we focus our interest in them?

First of all, because there is a high dependency of teachers on textbooks for teaching history of science (McComas, Almazroa and Klough, 1998; Stinner, 1992; Wang and Schmidt, 2001), it has been explained by the lack of formal education that they receive in this field (Leite, 2002). Additionally, the type of historical material that is used is mainly what determines the image of science, of scientists and of scientific practice that is taught to the students, each history of science is teaching something on the nature of science (Allchim, 2004), and finally the fact that we have failed at persuading writers of textbooks about the relevance of the history of science (Heilbron, 2002), a situation complicated when analyzing the relentless demands of the market, characterized by the omission of these "decorative" subjects (Chang, 1999).

Moreover, various difficulties emerge alongside these facts, related to the historical contents themselves (scientific, historical and NOS inadequacy), the fact that the history present in textbooks obeys schemes of whig and internal history. In addition, scientists are mythicized, the historical reconstructions are armed in retrospective to satisfy the canons of positivism and the insistence of including the same "official" histories, the one of Lavoisier, Newton and Dalton, those of the 19th century school history (Kolsto, 2008), among others. This panorama has made investigators within Science Education ask continuously about it and to formulate many questions: How can the distorted presentation of the history of science in textbooks be corrected? (Brackenridge,1989) To whom should we appeal: the authors, publishing houses, the formation of more critical teachers, or to a deeper reconstruction of the same history and philosophy of science? (Smolicz y Nunan, 1975); How do we make school history of science the product of an interdisciplinary work in which scientists, philosophers, science educators and historians participate, and that serves the needs of science education? (Heilbron, 2002; Kubli, 2001) How do we resolve the tension between "historical truth" and "didactic utility"? (Kragh, 1992), and finally, should we give in to those who regard the advantages of the approach with scepticism and prefer not to include history of science in the science education?

We want to add an additional question to these: What has happened with the sociology of science in Science Education? To where and why has it been relegated? And What could its contribution be to our discipline in order to understand scientific knowledge in our present scene in which the borders between history, sociology and philosophy of sciences as disciplines become less and less rigid (Bloor, 1998)?. My doctoral thesis work is framed in these considerations and we propose two main research questions:

- Can we obtain a characterization of the contents of history and philosophy of science in science school textbooks published in Spain with a sociological perspective?
- Can we propose a new form of telling the history of science in science school textbooks from this sociological perspective?

In order to try and answer these questions, we are analyzing the contents of atomic theory, atomic structure and atomic models presented in Spanish science school textbooks from the secondary education level, published from 1860 to today. In our methodological and theoretical framework we use some ideas from a French sociologist: Bruno Latour. Latour (2001) conceives scientific activity as a complex network in which the connection between politicians, scientists, equipment, laboratories, non-humans, models and institutions, allows science to circulate. This circulation can be characterized in five knots:

- *Mobilization of the world*: How science objects, the non-humans, become mobile and move around the world.
- *Autonomization*: The way in which a discipline, a profession, a clique or a group of two colleagues becomes independent and forms its own criteria of evaluation and relevance.
- *Alliances*: How the scientist persuades and interests groups of people who did not have any interest in science before, and become involved in the history.
- *Public representation*: Refers to the massive socialization of novel objects and how the associations between people and non-humans are modified.
- And finally, the *Links*: The role of concepts and scientific ideas which hold many heterogeneous resources (from the other 4 nodes) together.

Somewhere in this network are the textbooks which can be considered like elements, widely connected and bound to the knot of public representation; but that we can also conceive in themselves as containers of many different networks. One of them is the history of science that they present, a network that we suppose can be characterized by the nodes proposed by Latour.

In order to establish if the history of the science contained in school science textbooks can be characterized as a network that allows us to characterize scientific activity, we used social network analysis in which the networks are made up of nodes and their relations. In the network analysis it is not the attributes of the nodes that are important, but how they are connected. In this way, for example, Rutherford and Bohr share various attributes –they both are European physicists, they are scientist who set forth a model for the atom– but this doesn't necessarily mean they belong in the same network, because there would have to be a relationship linking one to other, for example, that Bohr's main objective was to explain the instability of the Rutherford's model. That's the kind of criteria that we follow in our analysis. To represent the networks we use graph theory, and UCINET software.

In our analysis we have defined different categories that adjust to the five knots established by Latour (figure 1):

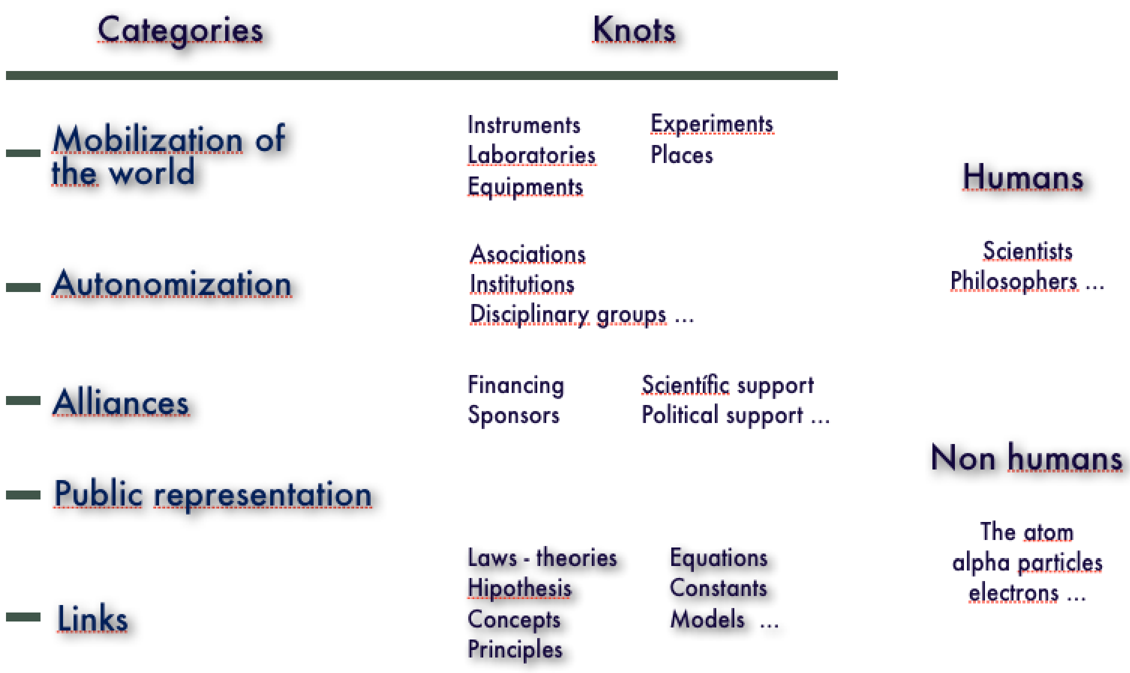

Fig. 1: Categories, knots, humans and non-humans define our networks.

When we represented these nodes and their relations we obtain more or less complex networks that tell us things about how these elements have been socialized in the historical narrative in the textbook. Thus it is possible to see how these different types of networks socialize some or other elements. Let's see some examples.

The first example is this book published in the 50's of last century (figure 2). Its network socializes only non-humans (the red nodes) and concepts (the green ones): we can talk about a "non-human" network. By chance, in this book there are neither dates, nor places either, it's a book without history of science. Figure 3 presents a network where few humans appear, the blue nodes, alongside them are the dates and the instruments. It's a little more complex network where the elements are more socialized. This book was also published in the 50's. The third book (figure 4) was published in the 40's which is the reason why we cannot make conclusions about a supposed relationship or dependency between the date of publication and the complexity or degree of socialization of the elements in the network. In this case in spite of having a network with less elements than the previous one, more things are socialized, here the places and the disciplinary groups appear, two important elements for circulating science.

Finally we have a book published in 1998 (figure 5), we could call it a contemporary textbook. In it socialization, connections, relations between non-humans and humans are widely overlapping. There are institutions, instruments, a congress, places, but first of all what stands out is the presence of experiments, the yellow nodes. These elements that Latour associates with the node of the mobilization are an important part of the historiographic narrative in this book and in others published after the mid 80's that we have already analyzed. Now we're working on trying to find when chemistry textbooks changed from prioritizing connections between non-humans and concepts to become books where the experiment is part of history, because the experiment is an important element which changes the way we can relate the history of science.

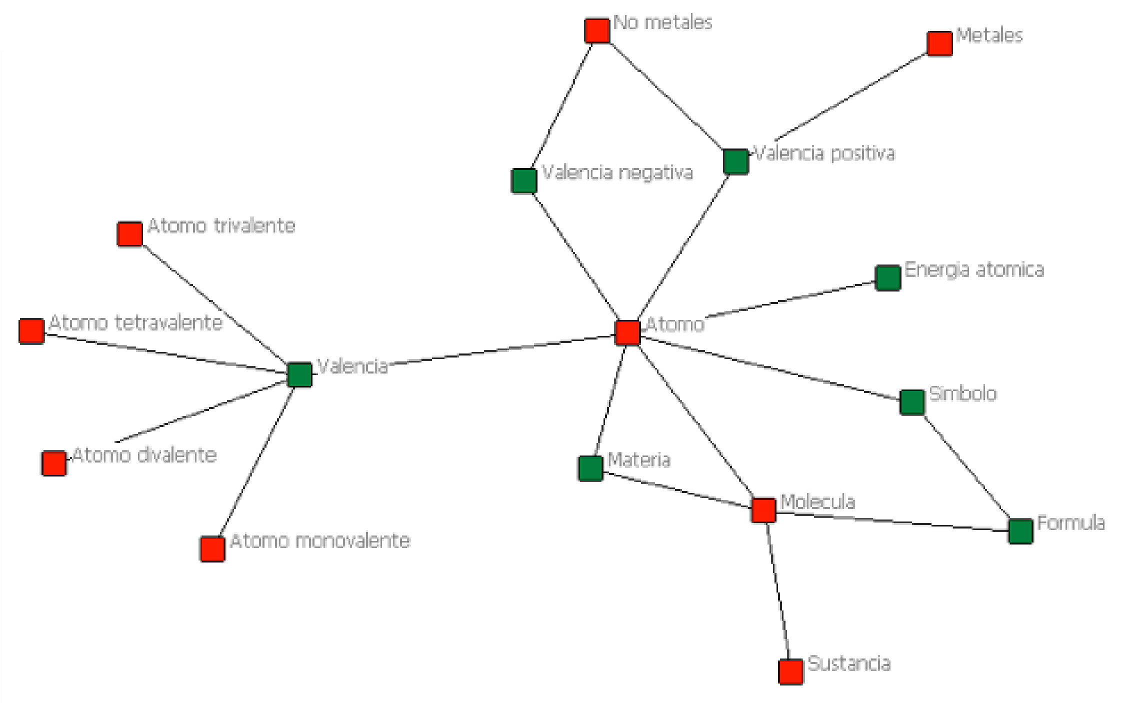

Fig. 2: An non-human network (Mendiola Ruiz, Jesús. *Física y química: Cuarto curso*. 195-. Editorial Cantabria).

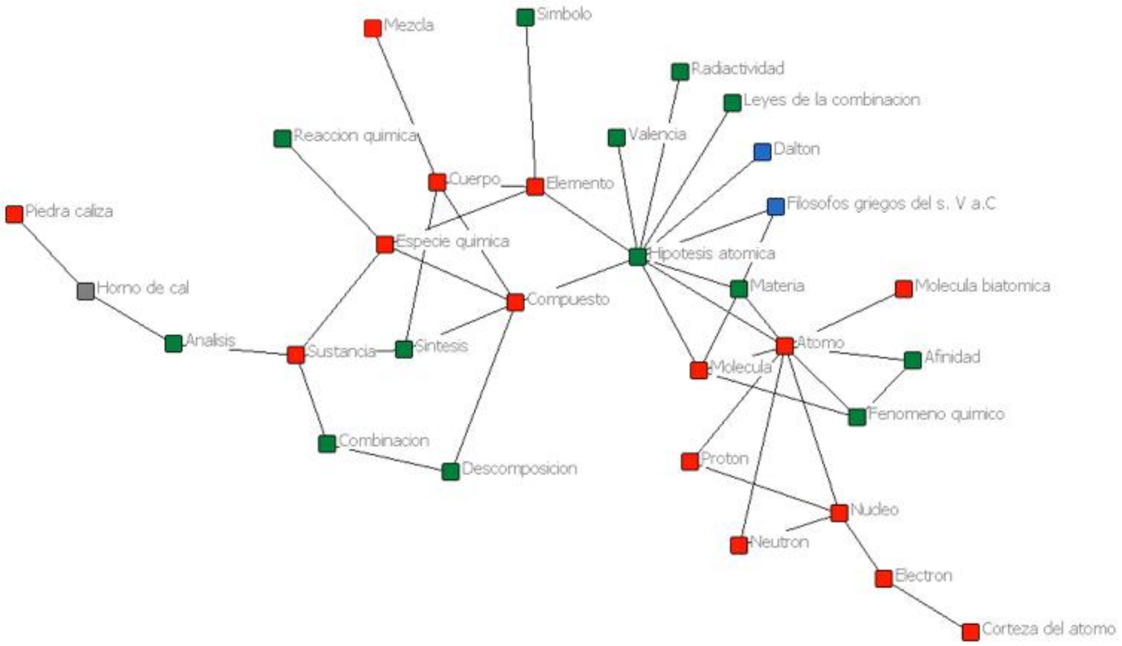

Fig. 3: Few humans appear. *Física y química: Cuarto curso de bachillerato* (Plan 1953). 1953. Ed. Bruño.

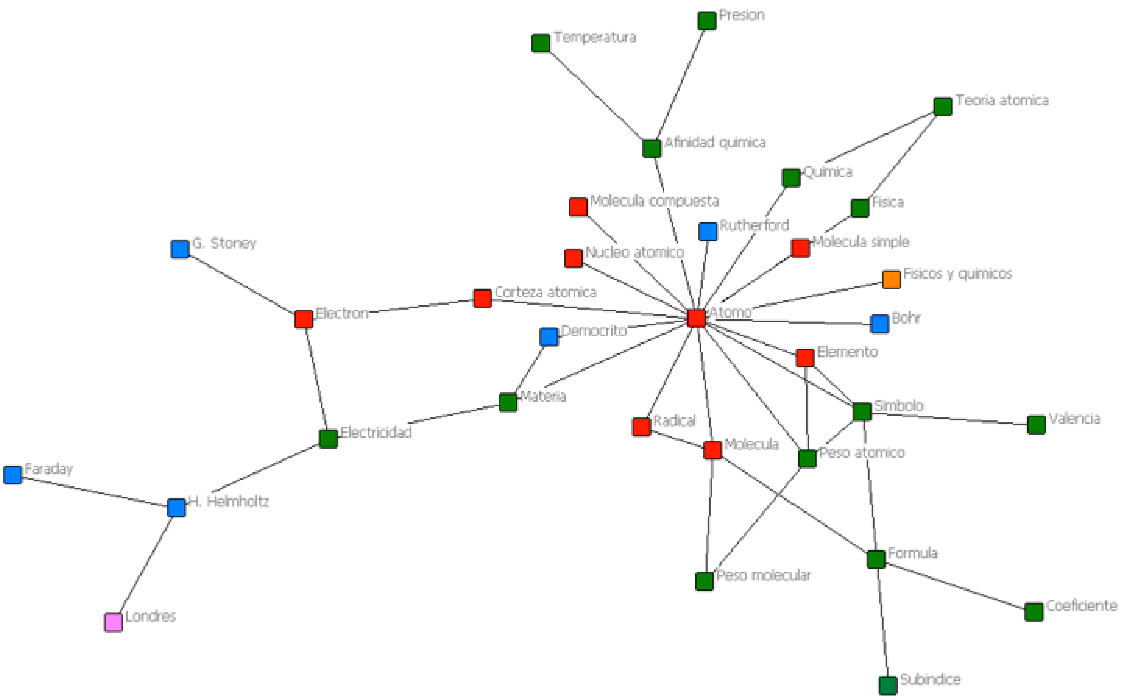

Fig. 4: A more socialized network (Moreno Alcañiz, Emilio. *Física y química: Cuarto curso*. 5th Ed. 1940. Editorial Heraldo de Aragón).

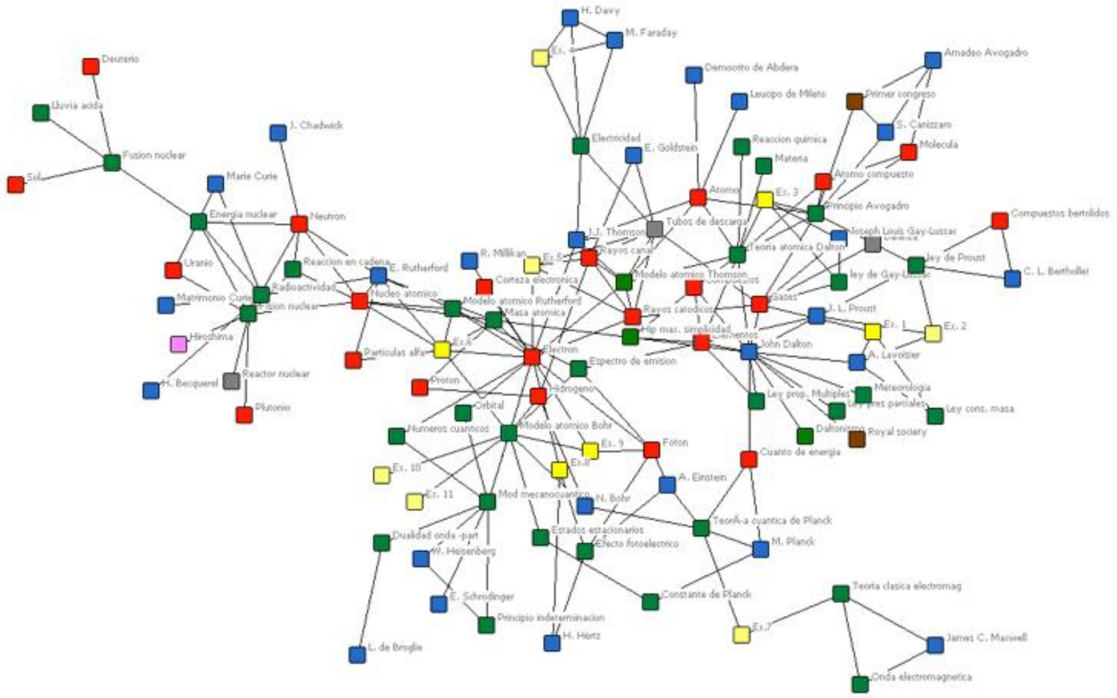

Fig. 5: A “contemporary” textbook network (García Pozo, Tomás; García-Serna Colomina Julio Rafael. *Química 1*. 1998. Edebé).

Until now we have analyzed 20 textbooks, but still there are many things that we have found when tracing the networks. Despite that, we can already see if there is a difference in the way the elements of the history of science that we have defined from our more sociological frame are socialized in the textbook. After every textbook is analyzed new questions rise, and we are working on them: Is a more socialized human/non-human textbook associated with a better HS? What happens with HS in textbooks where experiments appear as actors? What happens when the atomic models appear? Does changing to more “experimental” science textbook have any relation with changes in PS?

References

American Association for the Advancement of Science. (1990). *Science for all americans. Project 2061*. Oxford University Press.

ALLCHIN, D. (2004). Pseudohistory and Pseudoscience. *Science & Education* 13 (3) 179–195.

ALLCHIN, D. (2006). Why Respect for History and Historical Error Matters. *Science & Education*. 15 (1), 91-111.

BLOOR, D. (1998). *Conocimiento e imaginario social*. Gedisa Editorial: Barcelona. 288 p.

BONINI VIANA, H. E; ALVES PORTO, P. (2010). The Development of Dalton’s Atomic Theory as a Case Study in the History of Science: Reflections for Educators in Chemistry. *Science & Education*.19 (1), 75–90.

ARONS, A. B. (1988) Historical and philosophical perspectives attainable in introductory physics courses. *Educational Philosophy and Theory*, 20(2), 13-23.

BRACKENRIDGE, J. B. (1989) Education in Science, History of Science, and the Textbook – Necessary vs. Sufficient Conditions. *Interchange* 20 (2), 71-80.

CHANG, H. (1999). History and Philosophy of Science as a continuation of Science by other means. *Science & Education* 8, 413-425.

CLOUGH, M. P. (1997). Strategies and Activities for Initiating and Maintaining Pressure on Students’ Naive Views Concerning the Nature of Science. *Interchange*, 28(2-3), 191-204.

CLOUGH M. P. (2006) Learners’ reponses to the demands of conceptual change: considerations for effective nature of science instruction. *Science & Education* 15(4):465–494.

CONANT, J. B. (ed.) (1957) *Harvard Case Histories in Experimental Science*, 2 vols., Harvard. University Press, Cambridge, MA.

DAWKINS, K. R.; GLATTHORN, A. A. (1998) Using historical case studies in biology to explore the nature of science: A professional development program for high school teachers: W. F. McComas (ed.) *The Nature of Science in Science Education, 163-176*. Kluwer Academic Publishers. Printed in the Netherlands.

DE BERG, K. (2007). The Conversion of a Recipe-Based Laboratory Exercise to an Inquiry-Based Laboratory Exercise using Historical Materials and Recent Context-Based Materials: The Case of the Preparation of Tin (IV) Oxide. *Ninth International History, Philosophy and Science Teaching Conference*, Calgary/Canada, June 24 – 28, 2007.

DE CASTRO, R. S.; DE CARVALHO, A. M. P. (1995) The historic approach in teaching: Analysis of an experience. *Science and Education*, 4 (1) 65-85.

DOLPHIN, G. (2009) Evolution of the Theory of the Earth: A Contextualized Approach for Teaching the History of the Theory of Plate Tectonics to Ninth Grade Students. *Science & Education*.18 (3-4), 425–441.

ELKANA, Y. (2000). Science, philosophy of science and science teaching. *Science and Education* 9, 463-465.

ELLIS, P. (1989) Practical chemistry in a historical context In M. Shortland and A.Warwick (eds), *Teaching the History of Science* (Oxford: Basil Blackwell), 156-67.

FORGE, J. C. (1979) A Role for Philosophy of Science in the Teaching of Science. *Journal of Philosophy of Education*. 13, 109- 118.

HEERING, P. (2009). The role of historical experiments in science teacher training: experiences and perspectives. *Actes d'Història de la Ciència i de la Tècnica*, 2 (1), 389-399.

HEILBRON, J.L. (2002). History in Science Education, with Cautionary Tales about the Agreement of Measurement and Theory! *Science & Education* 11: 321–331

HERRON, J. D. (1977) *The Place of History in the Teaching of Chemistry*. J.Chem. Ed, 54 (1), 15-16

IRWIN, A.R. (2000) Historical Case Studies: Teaching the Nature of Science in Context, *Science Education* 84(1): 5–26.

IZQUIERDO, M. (1996) Relación entre la historia y la filosofía de la ciencia y la enseñanza de las ciencias. *Alambique*, 8, 1-21.

IZQUIERDO, M.; VALLVERDÚ, J.; QUINTANILLA, M.; MERINO, C. (2006) Relación entre la historia y la filosofía de las ciencias II. *Alambique* 48, 78- 91.

JAFFE, B. (1938) *The history of chemistry and its place in the teaching of high-school chemistry*. J. Chem. Ed. 15, 383-389.

JUSTI, R.; GILBERT. J. (2000). History and philosophy of science through models: some challenges in the case of 'the atom'. *Int. J. Sci. Educ*. 22 (9), 993- 1009

JUSTI, R.; GILBERT. J. (1999) History and Philosophy of Science through Models: The Case of Chemical Kinetics. *Science and Education* 8 (3), 287-307.

JUSTI, R.; GILBERT. J. (1999 b) A Cause of a historical Science Teaching: Use of Hybrid Models. *SciEd* 83 (2), 163–177.

KIPNIS, N. (1996). The "historical-investigative" approach to teaching science. *Science & Education*. 5 (3), 277-292.

KIPNIS, N. (1998) A history of science approach to the nature of science: learning science by rediscovering it. En: W. F. McComas (ed.) *The Nature of Science in Science Education*, 177-1 96. 1998 Kluwer Academic Publishers. Printed in the Netherlands.

KIPNIS, N. (2001) Scientific Controversies in Teaching Science: The Case of Volta. *Science & Education*. 10 (1-2), 33–49.

KLASSEN, S. (2009) The Construction and Analysis of a Science Story: A Proposed Methodology *Science & Education* 18 (3-4), 401–423.

KLOPFER, L. E. (1969) The teaching of science and the history of science. *Journal of research in science teaching*. 6 (1), 87-95.

KLOPFER, L.E.; COOLEY, W.W. (1963) The History of Science Cases for High Schools in the Development of Student Understanding of Science and Scientists , *Journal of Research in Science Teaching* 1(1): 33–47.

KOLSTØ, S. D. (2008). Science education for democratic citizenship through the use of the history of science. *Science & Education*.17 (8-9), 977–997.

KRAGH, H. (1992) A sense of history: History of science and the teaching of introductory quantum theory. *Science & Education*. 1 (4), 349-363.

KUBLI, F. (2001) Galileo's 'Jumping-Hill' Experiment in the Classroom – A Constructivist's Analysis. *Science & Education*. 10 (1-2), 145–148.

LATOUR, B. (2001). *La esperanza de Pandora*. Primera Edición. Barcelona: Editorial Gedisa S.A.

LEITE, L. (2002) History of Science in Science Education: Development and Validation of a Checklist for Analysing the Historical Content of Science Textbooks. *Science & Education*. 11 (4), 333–359.

LIN, H-S. (1998) The Effectiveness of Teaching Chemistry through the History of *Science Journal of Chemical Education*, 75 (10), 1326-1330.

LOCHHEAD, J.; DUFRESNE, R. (1989). Helping Students Understand Difficult Science Concepts Through the Use of Dialogues with History. In: Herget, D. (ed.), *History and Philosophy of Science in Science Education (Proceedings of the First International Conference)*, Science Education and Department of Philosophy. Florida State University, Tallahassee, Florida, pp. 221-229.

MADRAS, S. (1955). The Historical Approach to Chemical Concepts. *Journal of Chemical Education*, 32, 593–598.

MATTHEWS, M. (1994). Historia, filosofía y enseñanza de las ciencias: la aproximación actual. *Enseñanza de las ciencias*, 12 (2), 255-277.

MCCOMAS, W., F.; ALMAZROA, H.; CLOUGH M. (1998). The nature of science in science education. *Science & Education*, 7 (6), 511-532.

METZ, D.; KLASSEN, S.; MCMILLAN, B.; CLOUGH, M.; OLSON, J. (2007) Building a Foundation for the Use of Historical Narratives. *Science & Education*, 16, (3-5) 313–334.

NERSESSIAN, Nancy J. (1995) Opening the Black Box: Cognitive Science and History of Science. Osiris, 2nd series. 10, *Constructing Knowledge in the History of Science*, pp. 194-211.

NIAZ, Mansoor; RODRÍGUEZ, María A (2002). Improving learning by discussing controversies in 20th century physics *Phys. Educ*. 37 (1) 59-63

PESSOA DE CARVALHO, A. M.; INFANTOSI VANNUCCHI, A. (2000). History, philosophy and science teaching: Some answers to "How?". *Science & Education*, 9 (5), 427-448.

ROACH, L. E.; WANDERSEE J. H. (1995) Putting People Back Into Science: Using Historical Vignettes. *School Science and Mathematics*, 95 (7), 365-370

RUSS, R. S.; COFFEY, J. E.; HAMMER, D.; HUTCHISON, P. (2009). Making Classroom Assessment More Accountable to Scientific Reasoning: A Case for Attending to Mechanistic Thinking. *Science Education* 93 (5), 875 – 891.

RYDER, J.; LEACH, J. (2008). Teaching About the Epistemology of Science in Upper Secondary Schools: An Analysis of Teachers' Classroom Talk. *Science & Education*. 17 (2-3), 289–315.

SCHULZ, R. M. (2009) Reforming Science Education: Part I. The Search for a Philosophy of Science Education. *Science & Education* 18(3-4), 225–249.

SEROGLOU, F.; KOUMARAS, P.; TSELFES, V. (1998).) History of science and instructional design: The case of electromagnetism. *Science & Education* 7(3), 261- 280.

SMOLICZ, J. J.; NUNAN, E. E. (1975). The Philosophical and Sociological Foundations of Science Education: The Demythologizing of School Science. *Studies in Science Education*. 2, 101- 143

SOLBES, J; TRAVER, M. (2003). Against a Negative Image of Science: History of Science and the Teaching of Physics and Chemistry. *Science & Education* 12 (7), 703–717.

SOLOMON, J. (1989) Teaching the history of science: is nothing sacred? In M. Shortland and A. Warwick (eds), *Teaching the History of Science* (Oxford: Basil Blackwell), 42-53.

SOLOMON, J.; DUVEEN, J.; SCOT, L.; MCCARTHY, S. (1992) Teaching About the Nature of Science Through History: Action Research in the Classroom. *Journal of Research in Science Teaching*, 29(4), 409–421.

STINNER, A. (1995) Contextual Settings, Science Stories, and Large Context Problems: Toward a More Humanistic Science Education , *Science Education* 79(5): 555–581.

STINNER, A. (1992) Science Textbooks and Science Teaching: From Logic to Evidence. *Science Education* 76, 1–16.

STINNER, A.; MCMILLAN, B. A.; METZ, D.; JILEK, J. M.; KLASSEN, S. (2003). The Renewal of Case Studies in Science Education. *Science & Education* 12 (7): 617–643.

WANG, H. A.; SCHMIDT, W. H. (2001) History, Philosophy and Sociology of Science in Science Education: Results from the Third International Mathematics and Science Study. *Science & Education*. 10 (1-2), 51–70.

CENTRES AND PERIPHERY IN EUROPE: THE STEP RESEARCH PROJECT

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LABORATORY CULTURES IN EUROPE. THE 'GERMAN MODEL' IN THE EUROPEAN PERIPHERY

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Abstract

In the last decades of the nineteenth century, many European countries adopted a laboratory science system, in which the German model served as the prime example to be imitated. This is often taken as proof of the central role accorded to German science, and positions followers of the German model on the periphery. In this paper another interpretation of this diffusion process is proposed, stressing the complementary nature of centre and periphery. The centre embodies the shared values of the wider community and serves to connect various members of that community. The centre only emerges as such as a focal point for aspirations fostered at the periphery. The diverging and often contrasting interpretations of the German model of laboratory science exemplify this complex and ambivalent relation between centre and periphery.

In the introduction to *L'Europe des sciences. Constitution d'un espace scientifique* (2001), the editors Michel Blay and Efthymios Nicolaïdis explain how it was their intention to make a study of the influence of scientific knowledge on the homogenization of societies. They believe that European science as such constitutes not only a geographical category, but also an "intellectual unity": the particular knowledge created in Greek Antiquity was transformed in Europe during the Scientific Revolution, and subsequently exported towards other regions of the world. In particular for Europe, travels, translations and centre-periphery relations have all contributed to the unification of a single scientific space.[1] But the existence or rather the creation of a unified space raises many questions. How can distant participants of a scientific community interact with each other over long distances? How do shared values arise from different local traditions? How is a 'homogenized' science constructed, received and appropriated by scientists in various contexts, and how is scientific knowledge itself affected by the processes of diffusion and adaptation?

Sociologists have used for a long time the concept of a unified world-science system. Following Braudel and Wallerstein, an understanding has grown of the characteristics of global science, its hierarchical structure and its homogeneity, but also its differentiation and segmentation. World-science is often considered merely an extension of the structural features of the local scientific community, where scientists share their ideas in exchange for recognition and

[1] Michel Blay and Efthymios Nicolaïdis (eds.), *L'Europe des sciences. Constitution d'un espace scientifique* (Paris, Editions du Seuil: 2001), "Introduction", p. 9-11.

emulation.[2] But in as much as there exists a unified scientific space, there are different levels of integration which cannot be ignored. Every scientist is part of individual, national and international communal formations, which have different ways of operating. The integration of these communal formations may depend on political, ideological or economic factors, which may destabilize the global system and lead to the decommunalization of world science.[3]

What can be deduced from these studies, is that the existence of a unified space necessarily presupposes the existence of a centre. In the centre, the values of the epistemological space are articulated and embodied. It is not necessarily linked to a geographical location. Rather, the centre is, according to Edward Shils, the representation of the order of symbols, of values and beliefs, which govern the community.[4] As such, the centre is a powerful actor that holds a community together. The centre is closely related to the notion of 'elites', and attains a formal status by virtue of institutionalization. The periphery, on the other hand, is characterized by exclusion and distance from the centre. The views of Shils were further developed with respect to the international scientific community by Rainald von Gizycki in 1973. Basing his ideas on a comparative study of nineteenth century science in Germany, France and Great Britain, Gizycki defined the centre "by the fact that works produced there command more attention and acknowledgement than works produced elsewhere. […] the theories and observations produced there become sources of influence and objects of emulation."[5] There may be different centres for different scientific disciplines, but there is a tendency for the centres to be clustered. At the same time, there is a tendency for centres to be displaced by new, emerging centres.

Gizycki's view on scientific centres is based on competition, a race for scientific leadership. Research located at the centre of the scientific community is in all respects superior to research in peripheral regions. Countries in the periphery are countries which have lost the race, albeit only temporarily. The periphery inevitably attempts to imitate the centre or to modify its own structures to bring them closer to the models drawn from the centre. Gizycki observes:

> "A scientist who wishes to make an important contribution to science and thus to 'make his mark', must, if he lives and works at the periphery, conform with the standards embodied and espoused by the centre. Het must know the substance of the centre's accomplishments by reading its scientific literature; he must know the language of the centre. He is forced to live in its shadow."[6]

In this model, the only activities granted to the periphery are imitation, adaptation and emulation of the centre. In essence, the periphery is mostly unproductive of knowledge and a passive, perhaps even a superfluous bystander of the scientific community. To understand the scientific community and the history of scientific developments, it is sufficient to look at the centre.

Recent studies have presented alternatives to this model. In global studies, students of 'non-Western science' have questioned the putative unity of science and its central values. As Kapil Raj noted: "In place of a unique 'modern science', it is now accepted that there are many national and local knowledge traditions and dynamics spread across most of North and West Europe, with diverse, and at times contradictory, intellectual agendas and influences throughout the early-modern and modern periods."[7] The unidirectional spread of Western Science, as modelled in the 60s by George Basalla,[8] was not a simple emanation from a pre-existing centre, but the result of complex process of conflict, acculturation, and appropriation. Marcos Cueto has equally rejected the traditional notion of periphery as being unhelpful in understanding the dynamics of a 'peripheral' region's scientific development.[9] In Europe, the community of scholars constituting the STEP group has equally questioned the notion of periphery. In a joint paper, Kostas Gavroglu and others have pictured the periphery as an active receiver, making "a shift from the point of view of *what has been transmitted to the view of how, what was received has been appropriated*."[10]

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[2] Xavier Polanco (ed.), Naissance et développement de la science-monde. Production et reproduction des communautés scientifiques en Europe et en Amérique Latine (Paris: Éditions La Découverte, 1989).

[3] Thomas Schott, "World Science: Globalization of Institutions and Participation", *Science Technology Human Values* 18 (1993) 196-208.

[4] Edward Shils, "Centre and periphery", in: *The Logic of Personal Knowledge: Essays Presented to Michael Polanyi*, (London: Routledge & Kegan Paul, 1961), 117-30.

[5] Rainald von Gizycki, "Centre and periphery in the international scientific community: Germany, France and Great Britain in the 19th Century," *Minerva* 11 (1973) 4, 474-494. Quotation on p. 474 and 475.

[6] Gizycki, *op.cit.*, p. 479.

[7] Kapil Raj, Relocating Modern Science. Circulation and the Construction of Scientific Knowledge in South Asia en Europe. Seventeenth to Nineteenth Centuries, New Delhi, 2006, p. 7.

[8] George Basalla, "The Spread of Western Science," *Science* vol. 156 (no. 3775, 5 May 1967), pp. 611-622.

[9] Marcos Cueto, "Excellence in Twentieth-century Biomedical Science," in J.J. Saldaña (ed.), *Science in Latin America. A History* (Austin: University of Texas Press, 2006) p. 231-239.

[10] Kostas Gavroglu et al., "Science and technology in the European periphery: Some historiographical reflections," *History of Science* 46 (2008), 153-175.

To many scholars the dichotomy between centre and periphery should probably be left as a descriptive category to researchers of the present day world science system, but without further historical or explanatory value. Yet, I argue that the centre-periphery dichotomy can be fruitful if the perspective is shifted from a competitive model towards a complementarity model. In this model, a centre is not simply a place where knowledge is produced and standards are defined, but a place to which, by actors at the periphery, certain meanings and values are attributed. The centre is not the origin of the space around it, but it is created by the projection of local ideologies on a symbolic object. This presupposes a two way mechanism of appreciation and emulation between centre and periphery, in which both have important roles to play. Centre and periphery participate as complementary forces in the production of shared community values. The very use of the centre and periphery vocabulary may help to understand how a common identity within the community is being created and shared.

Much of the scholarly literature focuses either on the characteristics of the centre, or on the development of the peripheral region. The actual relation between centre and periphery has been taken for granted as if it seems to follow immediately from their respective positions. In this paper I consider the centre and periphery to be mutually dependent on each other. The centre emerges when it is recognized as such by members of the community, whereas by this very act these members show themselves to adhere to commonly shared values and to be an integral part of the larger community. I maintain that the centre-periphery model should not be used to make a distinction between two geographical or cultural spaces, but rather that it can help to underscore their connection. A close analysis of the centre-periphery metaphor, used by different actors in different locations and from different perspectives, may show how values and practices are perceived and interpreted.

One example of a typical centre-periphery narrative is the creation of modern laboratories at the end of the nineteenth century. In many countries, this laboratory movement was supported by reference to the lead taken by Germany in science and industry. But the role model of Germany in this process as interpreted according to the simple competition model of Gyzicky cannot explain a series of complex and ambiguous problems. There is no exact definition of what the German model was. In fact, many nineteenth century observers stressed contrary features of this model, in order to advance their own agendas. Also, no single country in fact copied the German model, but produced a locally adapted form of a laboratory system. What was common, however, was the agreement between many scientists and politicians, that if a model existed, it had to be found in Germany. That this 'German model' was in fact a construct adapted to local needs, shows how the relations between centre and periphery should be reconsidered.[11]

Laboratory Cultures and the German Model

During the second half the nineteenth century, the modern laboratory emerged as a new, important institution of science. Laboratories had existed before, and were recognized as privileged spaces for experimental work, in particular in chemistry.[12] But the new laboratories of the nineteenth century were different in scope and institutional status. In universities large laboratories were built for pedagogical reasons, instructing students in routine scientific manipulations, while providing some space for more advanced research. Somewhat later, laboratories were set up for agricultural and industrial research, transforming the nature of technical research and invention. More generally, the laboratory became an abstract institutional category of scientific practice, unconnected to a certain location or researcher, and very often referred to in the plural. This institutional role of the laboratories inspired Pasteur to describe them as "temples of the future, wealth and well-being".[13]

The building of laboratories was typical of the modernization era of science (1850-1950), when laboratories were considered a first and necessary step towards the implementation of scientific practices within a national culture. In university education, the laboratory underscored the rise of pure science as a basic element of intellectual culture. In government policies the evolution towards a modern, enlightened state was translated in the creation of experimental agricultural stations, food analysis laboratories and bacteriological institutes. Laboratories thus transformed many aspects of intellectual life, industrial activities and medical practices.

One of the characteristics of these laboratories was their uniformity to an international standard. This uniformity warranted access to the international scientific community and became a recognizable feature of modernization. Very often, the so-called 'German model', based on both the academic laboratories in German universities as well as on the industrial

[11] We restrict our research to European countries, where the use of the term 'periphery' refers to a different situation compared to the periphery on a global scale. Whereas European countries always felt related to the European centres, colonial and postcolonial experienced a much sharper difference with Western countries, both in size of the scientific community as in the cultural distance purposely maintained by colonial powers. Also, for briefness, I will not discuss the development of the laboratory system in Germany and the internal response to the (externally constructed) German model.

[12] Maurice Crosland, "Early Laboratories c.1600-c.1800 and the Location of Experimental Science," *Annals of Science* 62 (2005) 2, p. 233-253.

[13] Quoted in René Vallery-Radot, *La vie de Pasteur*, (Paris, 1900) p. 215.

laboratories in the German dye stuff industries, was put forward as the ideal model to replicate. In particular in peripheral countries, adaptation of this German model was a strong rhetoric for underscoring the quality of their own scientific culture.

The role of the German model in the reform of universities and the creation of laboratory facilities has been amply studied in the cases of France and England.[14] Competition between nations was a driving force in these debates, with Germany clearly considered in the lead, but the outcome was as much determined by domestic issues as it was by simple imitation of a scientific centre. As George Weisz observed on the French debates: "It is impossible to cite more than a few concrete instances of institutional imitation. […] A highly idealized image of German universities served to symbolize a variety of goals and aspirations. The most contradictory positions were defended by appeals to the German example."[15] And with respect to England, George Haines wrote: "To claim in each instance [of newly founded English colleges] direct German influence would be preposterous. But the widely and continuously cited German example, the intensively fostered fear of German industrial competition, and the German experience of many of the leading teachers played important roles in providing an impulse, otherwise so long delayed as to be almost inexplicable."[16]

The cases of France and Germany demonstrate already how the 'German model' was a malleable concept which could be used for almost any purpose. In many instances, the laboratories and universities of Germany were only described in abstract generalization, and different aspects of the German system (support by the State, laboratory research as part of university education, emphasis on direct links with technology) were variously given as fundamental features. The German inspiration of reformers was in any case more important than any German example to be copied or imitated.

Apart from these core countries, there are only a few studies on peripheral countries. References to the German model are not absent, but almost never critically analyzed.[17] The very definition of what the German model represented could vary widely. The target audiences may have been different, as also the outcome of the reform. Involvement of government institutions and the interest of local industries may have changed the message of reform substantially. The connection to agriculture, health policy or industrial development must certainly have added a variety of local flavour. We have, as yet, no overall understanding of how the German model was actually defined, used and implemented in most European countries. But from what can be gathered from exploratory readings, some themes for further research readily emerge.

1. At what time, and through which channels, did the German model actually find its way into 'peripheral' debates?

In most countries the apostles of the German model had studied for some time in Germany or were personally acquainted with German scientists. In other cases, German scientists settled in peripheral countries. In any way, the influence of German science is clear, but to go from personal experiences to a general 'German model' presupposes a creative step in defining what the German model was about.

The conscious use of the 'German model' appears to have come into use only in the 1860's. As yet, I have not found any use of the German model in 'peripheral' countries, before it became in vogue in France and England. This may indicate that these countries were actually instrumental in promoting Germany as the leading country in science. One suggestive example can be found with the Portuguese physiologist Costa Simões who in 1865 visited universities in Germany (Bonn, Würzburg, Heidelberg, Munich, Göttingen and Berlin), France (Paris), Belgium (Brussels, Leuven, Ghent and Liege), Holland (Amsterdam, Leyden and Utrecht) and Switzerland (Zurich). He decided to organize his own laboratory in Coimbra on the example of the Berlin University, but he did so after receiving advice from different experts. The status of German physiology was a topic of conversation between many colleagues, not only German. The instruments he acquired for his laboratory were bought in several locations (Paris, Berlin, Liege and Vienna), which may indicate that his actual implementation of the Berlin example was broader than a simple imitation and in some cases was only indirectly linked to the German sources.[18]

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[14] E.P. Hennock, "Technological Education in England, 1850-1926: the uses of the German model," *History of Education* 19 (1990) 299-331; George Haines, "German Influence upon Scientific Instruction in England 1867-1887," *Victorian Studies*, 1 (1958), 215-244; G.W. Roderick and M.D. Stephens, "Scientific Education in England and Germany in the Second Half of the Nineteenth Century," *Irish Journal of Education* 16 (1982) p. 62-83; Harry W. Paul, "The Issue of Decline in Nineteenth-Century French Science," *French Historical Studies*, 7 (1972) pp. 416-450; Robert Fox, "The View over the Rhine: Perceptions of German Science and Technology in France, 1860-1914," in Yves Cohen and Klaus Manfrass (eds.), *Frankreich und Deutschland. Forschung, Technologie und Industrielle Entwicklung im 19. Und 20. Jahrhundert* (Munich: C.H. Beck-Verlag, 1990), p. 14-24.

[15] George Weisz, *The Emergence of Modern Universities in France, 1863-1914* (Princeton: Princeton University Press, 1983), p. 62.

[16] Haines, *op.cit.,* p. 227.

[17] E.g. for Italy, the German model has been studied in relation to the creation of a scientific community. See G. Pancaldi, "The German model and the origins of the Italian *Riunioni degli scienziati*", in Dietrich v. Engelhardt (ed.), *Zwei Jahrhunderte Wissenschaft und Forschung in Deutschland. Entwicklungen – Perspektiven* (Stuttgart: Wissenschaftliche Verlagsgesellschaft mbH, 1998), pp. 171-178; Maria Pia Casalena, "The congresses of Italian scientists between Europe and the Risorgimento (1839-75)", *Journal of Modern Italian Studies* 12 (2007) p. 153-188.

[18] Maria Burguete, "Laboratories at the Faculty of Medicine of the University of Coimbra in the XIX century," *Scientific Research and Essays,* 5 (2010), p. 1402-1417.

Likewise, the Lisbon physician Marck Athias became aware of the debate on German science during his studies at the Faculty of Medicine in Paris in the closing years of the nineteenth century. Athias never studied in Germany. His French background was apparent in his ten year career at the Pasteur Institute in Lisbon. As head of the Lisbon Institute of Physiology his work was characterized by the positivist ethos of French science, although his scientific practices was modelled on the German tradition.[19]

Even when personal acquaintance with German science was available, the outcome was not necessarily only directed towards Germany. Early physiologists such as the Russian Ivan Mikhailovich Sechenov studied with a.o. Carl Ludwig in Vienna, Emil Du Bois-Reymond in Berlin and Claude Bernard in Paris. Although he used Du Bois-Reymond's experimental apparatus in his own laboratory, his work bears the influence of all the great physiologists he worked with.[20] Finally, the Cracow physics professor Zygmunt Wroblewski had studied in Berlin, Heidelberg and Munich but consequently made an extensive tour of other European research institutes, ending up in Paris. When he set up his laboratory at Cracow, he brought the needed apparatus from France.[21] If the German model was a powerful rhetorical device, it cannot be assumed to be an actual well-defined model to be copied.

2. Which values were associated with the articulation of the German model?

What was the rhetorical power of the German model? Was it based on an objective evaluation of scientific output? Some examples suggest that admiration for German science was often part of a larger cluster of values. Germany was heralded for its artistic and literary culture, its national vigour (in particular after the unification of the German states in the 1860's), its industrial successes, the honours bestowed on science by State and the general public, its victorious clash with France, etc. It has been suggested by Andrée Despy-Meyer and Didier Devriese in their study of the Belgian physiologist Paul Heger and his collaboration with Ernest Solvay to create new research laboratories, that the enthusiasm for German imperial science among progressive liberals should be understood by the obvious appeal of the anti-clerical politics of Bismarck, whereas the admiration from French positivism had been erased by Germany's *Kulturkampf*.[22] On the other hand, in France, German science was sometimes associated with a medieval world view and under the influence of religion.[23] In the Habsburg Empire many German speaking scientists integrated themselves in the German scientific community, in the hope of contributing to some ethnic German community, but non-German speaking scientists only grudgingly accepted the domination of German inspired institutes. In Prague the university obtained the liberty to teach in Czech language, as Cracow did in Polish.[24]

These examples suggest that the positive mention of a German model should always be interpreted against a much larger field of values associated with the German state and German culture. It becomes important therefore to carefully position the advocates of the German model in the social, cultural and political spheres of their country. Possibly, the values associated with the German model connected to the cultural ideals of existing or upcoming elites. For France and England, it has been established that the outcome of the 'German reforms' supported the rising social status of bourgeois intellectuals. For peripheral countries in particular, the relationship of 'German-minded' scientists with social elites and public authorities has to be looked into. In particular in agriculture local circumstances and ownership of the land had great impact on the implementation of agricultural stations and laboratories. Russian agricultural stations grew from laboratories on private lands, then developed into regionally based *zemstva* stations and finally to state bureaus.[25] Possibly the involvement of the Russian state in the promotion of the French Pasteur institutes was more effective than the abstract rhetoric of the German model.[26]

3. How did the German model transform the national science system?

The science system comprises all cognitive and institutional resources available to a country for its industrial and cultural development. Universities, research institutes, scientists, government agencies and industrial actors all contribute to produce knowledge and wealth. At the same time, the system itself is a distributor of values: it conveys status on important

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19 Isabel Amaral, "The Emergence of New Scientific Disciplines in Portuguese Medicine: Marck Athias's Histophysiology Research School, Lisbon (1897-1946)", *Annals of Science*, 63 (2006), p. 85-110.

20 Galina Kichigina, The imperial laboratory: Experimental physiology and clinical medicine in post-Crimean Russia (Amsterdam, New York: Rodopi, 2009).

21 Henk Kubbinga, "A tribute to Wróblewski and Olszewski," *Europhysics News* 41 (2010), 21-24.

22 Andrée Despy-Meyer and Didier Devriese, "Paul Heger, maître d'oeuvre des instituts d'enseignement et de recherche en sciences médicales voulus par Ernest Solvay à Bruxelles (1891-1895)," *Gewina* 16 (1993) p. 90-103.

23 Weisz, *op.cit*., p. 63.

24 Alois Kernbauer, "The Scientific Community of Chemists and Physics in the Nineteenth-Century Habsburg Monarchy," Centre for Austrian Studies, Working Paper 95-4, 1997.

25 Olga Elina, "Planting Seeds for the Revolution: The Rise of Russian Agricultural Science, 1860–1920," *Science in Context* 15 (2002), p. 209–237.

26 John F. Hutchinson, "Tsarist Russia and the Bacteriological Revolution," *Journal of the History of Medicine and Allied Sciences*, 40 (1985) p. 420-439.

actors and authority on institutions, legitimating scientific practices while undermining rivalling traditions. The import of a new model inevitably disturbs the existing science system, bringing new actors to the front and displacing other from their positions. The introduction of the German model, with its emphasis on the building of large laboratory facilities, caused increased tension with traditional forms of knowledge such as embodied in scientific societies or non-laboratory based disciplines. When in Belgium the large laboratories were built in the final decades of the nineteenth century, it put the power of scientific research in the universities, while the Royal Academy of Science lost much of its influence in Belgian scientific life. The introduction of agricultural stations replaced traditional agricultural practices. Louis Ferleger has argued that the success of the German agricultural stations actually caused a setback of agricultural efficiency in most North Sea countries where the German stations were admired and copied. Those countries neglected to provide enough funding for sustained agricultural research and relied too much on German knowledge, which was not adapted to their local needs.[27] Similar evolutions can be seen in the introduction of bacteriological laboratories, replacing the expertise of hygienists. Last but not least, the German model interfered with the balance between pure and applied research. Introducing laboratories in engineering education sometimes served to make the engineer appear more scientific, thus increasing the social distance with lower technical personnel. Similarly, the large laboratories available to university researchers emphasized the difference between professional researchers and amateurs.

Conclusion

The unified space of European science arguably emerged with the general enthusiasm, spread throughout European countries, for the German model. The German model, however, was not a static, well defined system which other countries envied and were ready to copy. Rather, the very definition of the German model, the values associated with it and the actual consequences of the adoption of the model varied largely among European countries. A better way to understand the international appeal of the German model, may be to look at it from the complementarity model of centre and periphery. The German science system, as conceptualized by numerous authors throughout Europe, embodied the central values of modern science and was thus a crucial binding element to bring different scientific traditions together within one community. Importantly, the label 'periphery' which suggests itself in studying the spread of the German model, should to be seen as an intentional localization of European countries around the centre. The 'adoption' of the German model, instead of being a mark of emulation of the centre by the periphery, can be understood to emphasize the modernity of the adopting country, its very nearness to the centre, and its acceptance of the central values of modern science.

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[27] Louis A. Ferleger, "European Agricultural Development and Institutional Change: German Experiment Stations, 1870–1920," *The Journal of The Historical Society* 5 (2005), 417-428.

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CENTRES AND PERIPHERIES IN EUROPE: THE CASE OF GREGOR MENDEL'S DISCOVERY[1]

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Abstract

Mendel's elucidation of hereditary processes based on his experimental model with plant hybrids in 1865 was a solely standing achievement in the 19th century biological science. The biologists were working within the mainstream of science influenced by Charles Darwin who brought the up to then hereditary research into the context of heredity of acquired characters. From this viewpoint Mendel was solving a question that was not considered as problematic in the nineteen-sixties. Mendel worked in the Brno Economy and Agriculture Society intended for practical exploitation of knowledge in the improvement of sheep and plants to contribute to the industrialization of the Habsburg Austrian Moravia following the British progressive model of society drawing wealth from the industry production. Brno was a suburb of Vienna in Mendel's days focusing its attention at founding a university and technically oriented school of higher learning. Open scientific societies implemented the experimental method and cultivated science as a mighty instrument in achieving societal welfare. Many an Austrian dissident scientist found a refuge in Brno that attracted motivated people to engage in the starting industry, business, banking and education. On the other hand the documents testify that Mendel was a loyal teacher implementing the school reform of the establishment enthusiastically. Mendel in his experiments did the artificial pollination alone. As a son of a peasant he did most of the 'dirty' gardening work alone. Not using white gloves in performing artificial pollination Mendel was a man at the fringe of the academic style. The shift took 35 years. In 1900 Mendel's discovery became the fundament of a new science –genetics– making revolution in the study of life systems.

Periphery of time

Mendel's work on the experiments with plant hybrids has been considered a most significant document in the history of the ascent of human thought in general, and in the history of science in particular. Although the substance of Mendel's paper fell into oblivion for 35 years. The question is, why? It was published in two lectures on serialization in 1865 in the meetings of a local Nature Research Society and published in the fourth volume of the Society's journal.[2] The first page of

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[1] Supported by MK no. 00009486202.

[2] Mendel G. Versuche über Pflanzen-Hybriden. *Verhandlungen des naturforschenden Vereines in Brünn 4*,1865, Abhandlungen 3-47, 1866, p. 3-47.

Mendel's manuscript has been known to many, but few know that the note in the upper left hand corner says in German '40 reprints' and its author is Gustav Niessl, a botanist and secretary of the Nature Research Society. It was Niessl who persuaded Mendel to publish his lecture that otherwise would never be printed. Moreover, shortly after 1900, when scientists regretted that Mendel's paper remained unnoticed so long, Niessl gave a heavy argument that Mendel's discovery paper had not been unnoticed, but was intentionally neglected.

Mendel's discovery was pushed to the periphery of scientific interest because it did not comply with the then biological knowledge. In 1904 Niessl reported that Mendel's discovery work was well known but was neglected because significance was attached to the ideas of Darwinism. In those days Mendel's concept seemed to contribute little to Darwinian studies in spite of the fact that in the first pages of his paper Mendel stated clearly that his work was significant for the solution of the question of the development of living forms.[3]

Periphery of specialization

Mendel's paper on plant hybrids had no special value for the then botany because its results attracted no attention. It became known to a few hybridizers working in the field of the origin of new forms. But the progeny of Mendel's hybrids reverted to the old parental forms according to the famous segregation ratio 1:2:1. Thus his scientific conclusions were implausible for the then evolutionary studies. Even though his experimental achievement was known, it was overlooked.[4]

Mendel complained to professor Naegeli of Munich University that nobody found interest to repeat his experiments.[5] Mendel was of the opinion that it was due to the absence of verification of his experimental data caused by the Bruchus pisi disease that devastated the pea breeding in the Brno region in those years. Mendel did not realize that the main obstacle of the reception of his paper laid in the incompatibility of his language as a physicist (speaking of 'genotypes') with the mainstream science botany that was concerned with 'phenotypes'. Mendel's concept of invisible elements that he grasped by means of mathematical statistics and theory of probability could not appeal to any botanist of his day.

Invisible elements

In his days Mendel had no possibility to reach the elementary structures and their function in the process of inheritance. Mendel was facing a typical problem of the so-called black box formulated one hundred years later by Norbert Wiener, the founder of cybernetics.[6] Plainly saying if we study a hidden mechanism, a black box, into which we cannot look into, there is only one possibility how to construct an idea about its functioning with help of given principles, e. g. Occam's razor. The construct comes from one's control of inputs and outputs into the black box and out of it. Comparing their mutual interactions one can trace their mutual dependence on each other. Respecting the basic methodological principles (causality, dynamics, Occam's razor etc.) one can construct a quite exact model of the hidden mechanism in action.[7]

In experimental conditions such an approach is possible only when **the initial conditions** of each experiment are given exactly to enable a precise comparison with the results. Mendel is the only experimenter of his time who specified clearly the initial conditions before opening his hybridizing experiments. Hybridizing experiments whose results could not be compared with the exactly defined initial conditions (inputs) were worthless, i.e. they could not lead to discovery of the hidden mechanism of inheritance within the black box.

Thus Mendel did not verify the concept of heredity by means of blending characters of organisms. Mendel offered a model of transfer of genetic information instead. Mendel's concept of inheritance is an information process in fact which is testified by his definition of the essence of the element in his Concluding Remarks:

> "This development proceeds in accord with a constant law based on the material composition (*materielle Beschaffenheit*) and arrangement of the elements (*Anordnung der Elemente*) that attained a viable union in the cell."[8]

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[3] See ref. 1, p. 4.

[4] See Weiling F. Niederschlag der Arbeiten J. G. Mendels in der Literatur bis 1900. *Folia Mendeliana 6*, Moravian Museum Brno 1971, p. 139-142.

[5] In Mendel's letter to Nägeli, April 18. 1867. See *Gregor Mendels Briefe an Carl Nägeli 1866-1873. Ein Nachtrat zu den veöffentlichten Bastardirungs-versuchen Mendels,* ed. Correns C., in: XXIX, Vol. Der Abhandlungen der mathematisch.-physischen Klasse der königl. Sächsischen Gesellschaft der Wissenschaften, No. 3, Leipzig 1905, p. 199.

[6] For more details see Wiener N. The Human Use of Human Beings. Cybernetics and Society. New York 1954.

[7] See Sekerák J. Mendel and the Age of Information. In: *Styles of Thinking in Science and Technology*, Proceedings of the 3rd International Conference of the European Society for the History of Science, Vienna, September 10 - 12, 2008. p. 408 – 411.

[8] See ref. 1, p. 42.

Quite intentionally Mendel stressed **the arrangement** of elements. Information in the present-day-thinking is the arrangement of elements (*in-formation*) in the material carrier.[9]

Periphery of place

Brno of Mendel's days was a provincial town and centre of Moravia. In the Austrian monarchy it played the role of Austrian Manchester. Its many textile factories chimney stalks filled the horizon of Brno competing in number with the monastic and church spires so typical for Habsburg sphere of influence. The dynamic industry in Brno attracted many entrepreneurs from various parts of Europe. The Brno melting pot became the refuge of many a nonconforming scientist of the Habsburg Empire.[10] The Economy and Agriculture Society in Brno initiated foundation of new modern schools that taught 'real' subjects such as natural science, accounting, engineering, book-keeping, architecture etc. The new educational trend needed an appropriate educational staff and Mendel seemed to be in the right time in the right place. Majoring in physics, he minored in natural science and was an extraordinary qualified help to the headmaster in fulfilling the official task to produce well trained students capable of industrializing the country.[11]

In the fifties of the 19th century Brno started a school reform forging a new way to the technically oriented education. New middle and high schools were established balancing the real subjects with the old concept of humanities. The documents testify that Mendel was a loyal teacher implementing the school reform of the establishment enthusiastically.[12] Next to middle and high schools institutes of higher leaning were founded to generate a new élite of businessmen, bankers, export/import experts and a highly motivated class of chemists, engineers, architects, economists, accountants, transporters, road and railway builders, sellers of various goods, customers and improvers of all sorts. In the fifties of the 19th century first gas lamps were lit in public places in Brno.

Research into nature specialized in looking for natural resources, growth of the effectivity of fodder plants and animal improvement. The demand for teachers in real subject was extremely high. Mendel head lessons on physics and natural science in Vienna from 1851-3 and was immediately offered a place of a substitute professor both at the high technical school and the technical institute of higher learning. In 1854 he became a member of the Economy Society that was a sort of the regional academy controlling industrialization, education and societal welfare in Moravia. Mendel's colleagues in the Economy Society appreciated 'Mendel's époque making experiments' in plant hybrids because the solution of that problem was crucial in their effort to achieve new colour varieties of ornamental plants. Nobody else considered Mendel's findings significant in spite of the fact that the Nature Research Society's mailing list comprised more than 130 names of institutions and scientists of European reputation and even overseas addresses to whom Mendel's paper was sent.[13]

Periphery of style

Mendel's detailed experiment was a result of the overlapping of physics and botany. Overlapping as a phenomenon is extremely improbable to occur in the centre. This style of Mendel was unique and unparalleled before. What was the difference between a botanist hybridizer and a physicist performing a hybridizing experiment in plants? Mendel gave precise initial conditions of his experiment that a botanist does not. To extend precision of the initial experimental parameters, Mendel worked for two years testing the plants on stability of characters, to prove their constancy in as many succeeding generations as possible. Even today such minute characteristics would appeal to a geneticist, not a botanist. Even some of those conventional evolutionists neglected the significance of genetics.[14]

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[9] See Sekerák J. An Interpretation of Mendel's Discovery in the 21st Century. *Folia Mendeliana 46 1/2*, 2010, Moravian Museum Brno, p. 23 – 40.

[10] See Sekerák J. Mendel's Discovery in the Context of Moravian and World Science. In: *Treasures of Moravia. Story of a Historical Land*, Galuška L., Mitáček J., Novotná L. (eds.). Moravian Museum Brno 2010, p. 199 – 204.

[11] See Matalová A.. Mendel's Contribution to the Implementation of the Postrevolutionary School Reform in Brno. *Folia Mendeliana 46, XCV, 1/2*, Moravian Museum Brno 2010, p. 5-21.

[12] *Ibid*.

[13] Sekerák J. Gregor Mendel and the Scientific Milieu of His Discovery. Proceedings of the 2nd ICESHS *"The Global and the Local - The History of Science and the Cultural Integration of Europe"*. [E-book (with the online and CD-ROM versions); Cracow: The Press of of the Polish Academy of Arts and Sciences, 2007], p. 242-247. Online edition of the Proceedings: http://www.2iceshs.cyfronet.pl/proceedings.html

[14] See ref. 6, p. 409.

From the periphery to the centre

We can assume that genetics could originate without Mendel as well. The expansion of science reached a certain point around 1900 and what came was a sort of crisis of a traditional Darwinist concept. New ways of answering questions are discovered and Mendel belongs to them. It means that they discover someone who has coped with those facts better than they did. They felt his authority and relied on Mendel, that's why the classical era of genetics is called Mendelism.

Mendel (1822-1884) did not live to see the acknowledgement of his theory of inheritance.

In the 20th century, after the so called "rediscovery" by Correns, De Vries and von Tschermak, Mendel's model of the transfer of hereditary elements or genetic information from generation to generation was often interpreted in different even contradictory ways. In Mendel's days (or more exactly in Mendel's obituary in a newspaper) only the members of the Brno Section for Pomiculture, Viniculture and Horticulture in the Economy Society appreciated Mendel's hybridizing experiments as marking a new époque.

For centuries the role of the environment in hereditary processes was considered decisive for evolution. Mendel's theory of (on) stability was long misunderstood as contradictory to change. Mendel explained the process of change as rooted in the ways of organization and reorganization of hereditary elements. It took rather a long time to establish complementarity of Darwin's question 'what changes' and Mendel's answer 'what persists'.[15] Prior to establishing integrity of heredity with evolutionary views, fierce debates on the relationship between Mendelism and the inheritance of acquired characters. The geneticists opposed the inheritance of acquired characters. This debate seemed to seize after the Great Evolutionary Synthesis in the USA in the thirties of the 20th century.[16] This process culminated in this: how can we satisfactorily answer the question posed by R.A. Fisher: *What did Mendel actually discover and what did he think he discovered?*[17]

References

BATESON W. (1909). *Mendel's Principles of Heredity.* Cambridge: Cambridge University Press.

CALLENDER L. A. (1988). Gregor Mendel: An Opponent of Descent with Modification. *History of Science*, No. 26, p. 41-75.

DITROCCHIO F. (1991). Mendel's Experiments: A Reinterpretation. *Journal of the History of Biology 24*, p. 485-519.

DARWIN Ch. (reprint 1964). *On the Origin of Species: A Facsimile of the First Edition.* Cambridge, Mass.: Harward University Press.

DUNN L. C. (1965). *A Short History of Genetics.* New York: McGraw-Hill.

FONG P. (1973). Thermodynamic and Statistical Theory of Life: An Outline. *Biogenesis, Evolution, Homeostasi,* Locker A. (ed.). Berlin, Heidelberg, New York: Springer-Verlag.

FRANKENBERGER Z. (1955). Review of the book by M. Hašek "Vegetativní hybridisace u živočichů (Vegetative hybridisation in Animals)". *Čs. Biologie*, No. 2. p. 118-120.

HEIMANS J. (1969). Ein Notizblatt aus dem Nachlass Gregor Mendels mit Analysen eines seiner Kreuzungsversuche. *Folia Mendeliana*. Moravian Museum, No.4, p. 5-36.

MATALOVÁ A. (ed.). (1999). *Gregor Johann Mendel.* Brno: Mendelianum, Moravian Museum, 24 p. ISBN 80-7028-150-2.

MATALOVÁ A. (1997). The Difference in Research Style Documenting the Heredity/Environment (Nature/Nurture) Controversy in the 1950s. *Biology Integrating Scientific Fundamentals.* München, p. 423-431.

MATALOVÁ A. (2002-2003). Gregor Mendel between the empirical and the rational. *Folia Mendeliana 37 – 38*, p. 37 – 43.

[15] See ref. 8.

[16] See Matalová A., Sekerák J. *Genetics Behind the Iron Curtain*. Moravian Museum Brno 2004, 120 p. Also see Orel V. *Gregor Mendel the First Geneticist*. Oxford University Press 1966, p 294-319.

[17] Fisher R. A. Has Mendel's Work Been Rediscovered? *Annals of Science*, 1936, No.1, p. 115-137.

MATALOVÁ A. (1999). Mendel and Soft Computing. *Mendel '99, 5th International Conference on Soft Computing, Brno University of Technology*, p. 88-89.

MATALOVÁ A. (1995). Mendel's theory and Complementarity. *Folia Mendelaina 30*, p. 43 – 49.

MÜLLER-WILLE S.; OREL V. (2007). From Linnean Species to Mendelian Factors: Elements of Hybridism, 1751-1870. *Annals of Science 64*, No.2, p. 171-215.

MENDEL G. (1866). Versuche über Pflanzen-Hybriden. *Verhandlungen des naturforschenden Vereines in Brünn* 4, 1865, Abhandlungen, p. 3-47.

MENDEL G. (2008). *Pokusy s hybridy rostlin.* Brno: K-public, Mendelianum Musei Moraviae, 125 p. ISBN 978-80-87028-02-5.

OLBY R. C. (1979). Mendel No Mendelian? *History of Science*, No.17, p. 53-72.

OLBY R. C. (1966). *Origins of Mendelism.* New York: Schocken Books.

OREL V.; HARTL D.L. (1994). Controversies in the Interpretation of Mendel's Discovery. *History and Philosophy of the Life Sciences 16*, p. 263-267.

OREL V. (1996). *Gregor Mendel: The First Genetist.* Oxford Eng.: Oxford University Press, 363 p. ISBN 0-19-854774-9.

OREL V. (1968). Will the Story of "Too Good Results" of Mendel's Data Continue? *BioScience 18*, p. 776-78.

PRIGOGINE I.; STENGERS I. (1984). *Order out of Chaos. Man's new dialogue with nature.* London.

SAPP J. (1990). The Nine Lives of Gregor Mendel. *Experimental Enquiries.* Ed. LE GRAND H. E. Netherlands: Kluwer Academic Publishers, p.137-166.

SEKERÁK J. (2000-2001). Mendel's Black Box of 1865: A Hidden Mechanism in Action. *Folia Mendeliana 35 – 36*, p. 43 – 44.

SEKERÁK J. (1996-1997). So-called vegetative hybridization as a means of achieving genetic change tested on animals in Prague. *Folia Mendelaina 31 – 32*, p. 29 – 32.

SHULL G. H. (1915). Genetic Definitions in the New Standard Dictionary. *The American Naturalist 49,* 1915, p. 52–59.
http://www.esp.org/foundations/genetics/classical/ghs-15.pdf

STEIBITZ F. (1937). *Základy antických názorů o dědičnosti.* Brno : Filosofická fakulta MU.

The Concise Oxford Dictionary of Current English. Fifth Edition. (1964). Oxford : Oxford University Press, p. 572.

Webster's Third New International Dictionary of The English Language Unabridged. (1966). GOVE P. B. (ed.). G.C. Merriam Company, Publishers, Springfield, Massachusetts, U.S.A. Vol. 1.

WIENER N. (1954). *The Human Use of Human Beings. Cybernetics and Society.* New York.

WRIGHT S. (1966). Mendeľs Ratios. *Origin of Genetics.* STERN C. a SHERWOOD E. (eds.). San Francisco: W. H. Feeman, p. 173-175.

CIRCULATION OF IDEAS, TECHNIQUES AND SCIENTIFIC PERSONAE. –THE ROLE NETWORKS DID PLAY, FROM THE GENDER PERSPECTIVE

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STUDIES OF MEDICINE FROM THE GENDER PERSPECTIVE

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Abstract:

The beginning of natural sciences came to predominate in medicine with the emergence of natural scientific thinking in the first half of the 19th century. A great deal of physiological progress in the 19th century was centered in Germany, where apparatus developed by physicists were judiciously used in physiological experiences. This trend is clearly illustrated in my previous article (Burguete, 2010).
Therefore, the creation of the Laboratories of Experimental Physiology, Histology, Toxicology and Pathological Anatomy was the result of the reorganization of the Medicine Faculty at Coimbra University in 1866-1872, according to the model of the Berlin School of Medicine.
In the sequence of this transition comes the initiation of experimental work providing better conditions for the learning process of medical sciences for the Coimbra faculty of Medicine. As the public understanding of science as well as scientific literacy became accessible to a wider public (women included), a kind of a revolution from the gender perspective naturally takes place in the world of knowledge, not only in Portugal but also in Germany and all around Europe. I will present only two cases studies: Portuguese and German.

Introduction

Why Science & Gender? This is a controversial topic and at a first glance we can look over the positions and works of women researchers from different scientific areas and visualize the frontiers they had to overcome just because they were women doing science. We need to correlate gender questions with science questions and this task is a very difficult one.

Ruth Hubbard, a great scientist from Harvard University, where she had worked side by side with her husband, George Wald (Nobel prize in Medicine or Physiology,1967), only after her 50th birthday did she realize the influence of extrinsic factors (*society*, *behaviour* and *mentality*) upon intrinsic rationality of science while producing science itself. She says that*, "such as writers, all that scientists can do is to tell stories and just like in literature, those stories should be based upon political and social reality from actuality."*

So, let us tell a story about two women, both living under the influence of a monarchic political system and belonging to a similar social reality:

- **Domitilia Miranda de Carvalho (1871-1866)** – Portugal.
- **Rachel Hirsch (1870-1953)** – Germany.

Curiously, what happened with Domitilia Miranda de Carvalho (fig. 2) in the 19th century concerning the way she had to dress herself still happens today in a more sophisticated way, but the reaction still exists from the world of men when a woman does not follow a pattern previously approved by men, the so-called intrinsic rationality…

False paradigms pretended to support unquestionable questions in medicine and behavioural sciences to push women aside. In spite of women having been admitted at European and American universities one century ago, in 1950 we could still hear conversations with sentences like this:

> ”it was not worthy for women to submit as a candidate for *professor of biochemistry*!”

This is unbelievable in the 20th century!

Development

In the first half of the 19th century, natural scientific thinking came to predominate in medicine - The Dawn of the Natural Scientific Era (Burguete, M. 2010).

Philosophical approaches became less relevant and research concentrated on the biological, physiological and chemical foundations of life.

Finally, in the 2nd half of the 19th century, women could already complete university studies in many countries such as Portugal and Germany.

In this work we have two case studies where the stories of the very first women to be medical doctors in Germany – **Rachel Hirsch (1870-1953)**– and Portugal –**Domitilia Miranda de Carvalho (1871- 1966)**– are presented.

Portuguese Case Study: the First Medical Doctor Woman

For the first time in 1894, a woman, Domitilia Miranda de Carvalho, was accepted at Coimbra University (fig. 1) to study Mathematics and Philosophy under one condition:

> “By expressed decision of the Rector, she would have to dress discretely and always in black with a black hat also discrete, in order not to disturb her masculine colleagues, also obliged to wear black clothes.”

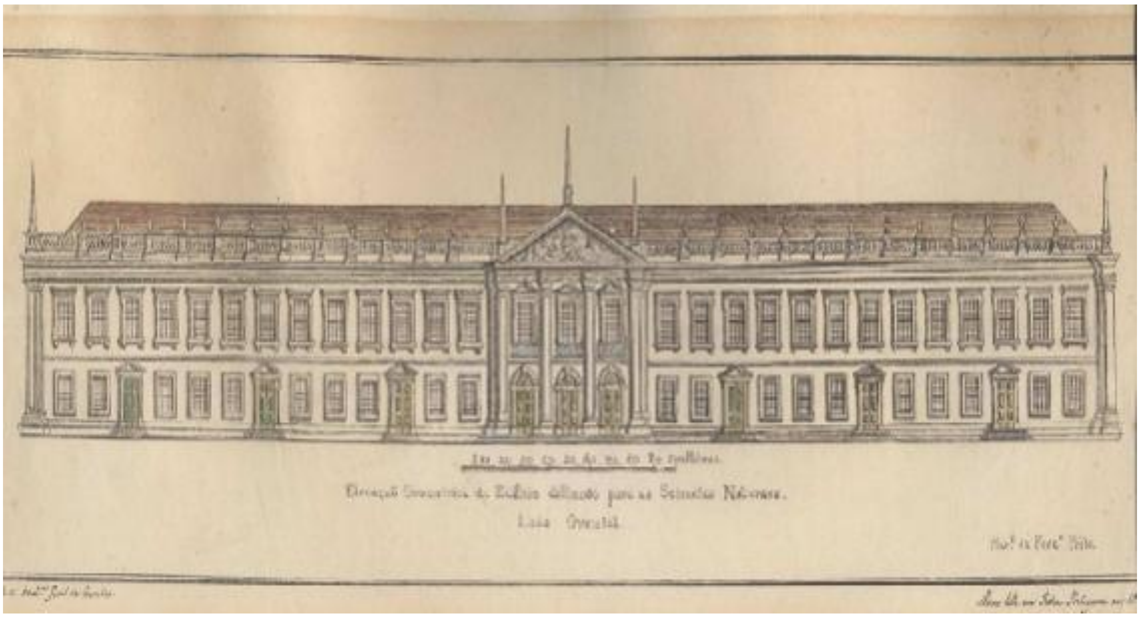

Fig. 1: Faculty of Medicine of Coimbra University in the 19th century.

Why the imposition of such conditions? Because many professors and male students agreed: women did not belong at university!

After the conclusion of Mathematic and Philosophy courses in 1894 and 1895 respectively, she wanted to study Medicine. However, women who wanted to become physicians had a particularly difficult position: the male leaders feared competition and publicly discussed the "naturally feeling of shame of women" or even "the physiological idiocy of women".

Fortunately, she was a monarchist and also a great friend to Queen Amélia de Orleães (fig. 3), the last Queen of Portugal, who supported all costs and expenses, therefore creating all the needed conditions in order to be able to finish her medical course in 1904 with 16, an excellent result. Therefore, she was the very first medical doctor woman in Portugal.

Fig. 2: Domitilia Miranda de Carvalho.

Fig. 3: Queen Amélia (1865-1951) married King Carlos (king of Portugal). With the age of 24 she was Queen of Portugal.

Afterwards she proceeds with her professional life as clinical doctor in Lisbon and at the public hospital. Later on she accepted a place as a teacher of the first secondary school – *Liceu D. Maria Pia* – created only for girls in 1906. She was also invited to be a Rector of this Lyceum until 1912. Then she lectured several subjects including Mathematics, which she never stopped, even after retirement.

Besides her activities as a lecturer and as a medical doctor she also published several books (related to education and poetry) at Coimbra, participating actively in the Educational Reform leadership by Carneiro Pacheco in 1936.

In spite of her monarchist and conservative profile, she was one of the three women (along with Mª Guardiola and Mª Cândida Parreira) invited by the government of Salazar maintaining cordial relationships between Salazar and the Queen Amélia de Orleães, with whom she always kept a friendly correspondence. She was deputy at the National Assembly with the age of 64. Some of her **parliamentary actions**:

- Law project to introduce hygienic and puericulture as subjects to be taught in every secondary school (1935).
- Creates Tropical Medicine Institute (1935).
- Creates a special support program to help poverty (1936)
- Creates special law for Fisherman Houses Project (1937)
- Construction of Justice Palace of Lisbon and Oporto (1938).
- Law projects for the regulation upon primary and secondary examinations as well as the introduction of fees for the High Schools.

She was also a correspondent member of the Lisbon Academy of Sciences.

German Case Study: the first Medical Doctor Woman

In the second half of the 19th century, women could already complete university studies in many countries and in Berlin in 1896, the Prussian Minister for Culture permitted women to have guest status at universities such as Berlin University (fig. 4) but nevertheless it was very difficult for them to become a physician.

Fig. 4: Berlin University – The Medical Faculty (1810).

In Berlin, women' s rights activist Helene Lange (1848-1930) offered courses for women beginning in 1898, thus giving the graduates their first pre-requisites for university study. In 1896, the Prussian Minister for Culture permitted women to have guest status at universities. Many professors and male students agreed, however: women did not belong at university!

There is a curious story about women and dissection that involves a renowned medical doctor, Wilhelm Waldeyer (1836-1921), who started university studies in mathematics and the natural sciences in nearby Göttingen. Soon after attending lectures by the famous anatomist Jakob Henle (1809–1885), the young Waldeyer was determined to become a lecturer in anatomy and therefore turned to medicine. He continued his studies in Greifswald and Berlin, where he graduated in 1862, and earned the title of medical doctor. He was made full professor of pathological anatomy in Breslau in 1867, still waiting for an opportunity to enter anatomy, his desired specialty. In 1883, he moved to the prestigious Chair of Anatomy in Berlin, now the Imperial capital.

One of Waldeyer's peculiarities that deserve mention here is his explicit opposition towards the admission of female students to medical school. Women studied medicine from as early as the 1830s in the USA and from 1864 in Switzerland. Prussia, however, which was still an administrative entity within the Empire, would not admit them to state exams until 1908, another nine years after the parliament of the German Empire had made a decision to this effect. Waldeyer tried to justify his opinions, which were certainly backward even for his time, by historical and "scientific" arguments. In a 15-page paper published in 1888, he held, e.g., that obstetrics, a long-time female domain, only flourished as a science when it came into the hands of men, and that the many women who came to power throughout history would testify against the view that women's abilities were simply restrained by society.

He added that according to "recent scientific evidence" the gyres of the male brain were more pronounced than those of female brains, leading to a larger cortical surface, the "substrate of our intellectual abilities" (Waldeyer, 1888).

It was obviously unthinkable for Waldeyer that women should enter the (then) male sphere of the dissection room and, perhaps even more shocking to him, that they should dissect men's bodies. It took several petitions of potential female students and the intervention of a minister for Waldeyer to finally allow female students into the anatomy laboratory, if only in a separate room and not taught by himself. In the Winter of 1904/05 the first 22 women (see fig. 6) therefore dissected under the supervision of Waldeyer's associate, Hans Virchow (1852-1940), the son of the famous Berlin pathologist Rudolph Virchow (1821-1902), (Stieve, 1942).

Hans Virchow took a strong stand concerning the question of the medical school for women and offered separate preparation courses for women in the "museum room" of the anatomical institute starting in 1904.

In fig. 6 we can see Elisabeth Litzmann (1883-1974) in the front to the right, as well as the interim report card from this student, dated from 5 November 1908, when Prussia finally gave women the right to full matriculation, and therefore also freed the way for female physicians. However, a regular academic career for women was only possible after 1920, when they were permitted to complete the postdoctoral qualification of a "Habilitation".

Fig. 5: Hans Virchow.

Fig. 6: Special dissection course for women (top) and Elisabeth Litzmann's Report card from November 1908 (below).

Rachel Hirsch (1870-1953) (fig. 7) was born in Frankfurt and studied at Zurich, Strasbourg and Leipzig. She was the first Medical Doctor woman in Germany because she got permission to practice medicine at Charité Hospital since 1903 when she started working with Dr. Kraus doing scientific works later published in the field of experimental physiology. Three years later in 1906, she discovered that solid particles are able to pass from the veins and arteries into urine (*Hirsch Effect*).

The results of her investigation were severely criticized, the result of which was that she had to give up her position. Only in 1908, Prussia gave women the right to full matriculation in Medical School and therefore also freed the way for female physicians, being Rachel Hirsch the first Prussian female medical professor in 1913. Her discovery, which had been so greatly criticized, was named after her. Many years later, she gave up her position and established a private practice in the Berlin district of Wilmersdorf.

Fig. 7: Rachel Hirsch in the laboratory (from Leo Baeck Institute[1]).

Under the Nazis, she found it increasingly difficult to work; in early October 1938 she left Germany and went to England, where she remained until her death on October 6th, 1953. Not allowed to practice in London, she underwent a serious crisis. When she died, she was impoverished. More than a decade later, she was posthumously admitted into the *"Gallery of famous Jewish scientists"*.

Conclusions

Scientific organizations have evolved exponentially during the last two centuries.

While scientific thinking developed according to his "logical and empirical needs" there was neither time nor space to recognize the "masculine mark" in the knowledge system.

This question was opened by philosophers, historians and sociologists by calling attention to the fact that scientific knowledge was influenced by the social and political context.

Therefore, social and political context influence the kind of knowledge produced by science and also "who" does science (Londa Schiebinger, 1999).

William Whewell (1794–1866), one of the most important and influential philosophers in nineteenth-century Britain wrote extensively on numerous subjects, including mechanics, mineralogy, geology, astronomy, political economy, theology, educational reform, international law, and architecture, as well as the works that remain the most well-known today in philosophy of science, history of science, and moral philosophy Whewell invented the terms "anode," "cathode," and "ion" for Faraday. Upon the request of the poet Coleridge in 1833, Whewell invented the English word "scientist"; before this time the only terms in use were "natural philosopher" and "man of science". While defining the word "scientist" Whewell in 1834 said:

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1 Many of the works on display –including rare documents, unpublished manuscripts and critically acclaimed publications from the holdings of the Leo Baeck Institute– attest to the spirit, courage and competence of these largely unknown and unacknowledged women. While we may not know them by name, we do know that the feminist movement is but one aspect of their astonishing legacy.

"In fact, there is a "sex in our minds".

Almost 200 years later this situation still remains… if the sex is located in our minds or in our culture or in none of them, is still a controversial question!

However, it is not difficult to understand that while this question remains unsolved women will naturally feel uncomfortable in the world of professional science.

This is a tribute to the ingeniousness, creativity and perseverance with which Jewish women have shaped their destinies. Through necessity or choice, these women often became both catalysts for social and political changes and imaginative forerunners of science development.

References

PIMENTEL, Irene Flunser (2001) - *História das organizações femininas do Estado Novo*. Lisboa: Temas e Debates, p. 419).

BURGUETE, Maria (2010) - *Medical Studies at Coimbra in the 19th Century*, LAP Lambert Publishing (ISBN 978-3-8433-6969-5)

SCHIEBINGER, Londa (1999) - *Has Feminism Changed Science?.* (Cambridge, Massachusetts: Harvard University Press, p. 1-18).

WALDEYER, Wilhelm (1888) - Das Studium der Medicin und die Frauen – *Verh Deutsch Ges Naturforsch Ärzte* 61:31-44.

STIEVE, H. (1942) - Hans Virchow zum Gedenken. *Anat Anz* 92:297-349.

Britannica Online Encyclopedia (2911) - Whewell, William: http://www.britannica.com/EBchecked/topic/641764/William-Whewell

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 194.

EXCHANGE OF SCIENTIFIC INFORMATION BETWEEN THE SANITARY PROFESSIONALS PARTICIPATING IN THE INTERNATIONAL SANITARY CONFERENCES IN THE 19TH CENTURY

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Abstract

The archive and the library of the Spanish physician Pere Felip Monlau (1808-1871) are located in the Balearic Library in Palma of Majorca. This archive contains all the correspondence exchanged between Monlau, Spanish delegate in the three first International Sanitary Conferences, and the other delegates of the countries represented in the Conferences.

The archive has letters of Guglielmo Menis (1793-18??) a delegate from Austria, François Melier (1798-1866) from France, A. Costi from Greece, José Maria Grande (18??-1858) from Portugal, Mühling from Prussia, C. O. R. Rosenberger (1806-1866) and Eugene Pelikan (1824-1884) from Russia, Angelo Bo (1801-1874) from Sardinia, Pietro Betti (1784-1864) from Toscana, Giuseppe Carbonaro (1800-1858) from Two Sicilies, Bartoletti from Turkey, John Sutherland (1808-1891) from the United Kingdom and Agostino Cappello (1784-1858) from the Vatican State.

The aim of the paper is to analyze the information related to the fight against epidemics, mainly cholera, contained in these letters in order to evaluate its influence in the conclusions of the International Sanitary Conferences.

Pedro Felipe Monlau (1808-1871)[1] was the sanitary delegate for Spain at the first three international sanitary conferences held in Paris (23-7-1851/19-1-1852), Paris (9-4/30-8-1859) and Constantinople (13-2/26-9-1866). During these years and up until his death in 1871, he maintained a close relationship with the European hygienists and epidemiologists of the second half of the 19th century.

Following Monlau through his daily notes of the lectures, his correspondence, documents from the Medical Institute of Valencia(1841-1894) and its Bulletin (*Boletín del Instituto Médico Valenciano*, 1841-1896) and the Monitor of Health (*Monitor de la Salut*, 1858-1864) we can see how Monlau became friends with almost all of the sanitary delegates for the states represented in the ISCs[2] and with other Spanish and European professionals, friends of the above concerned about epidemiology and hygienism contacted by Monlau using the aforementioned means.

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[1] Pedro Felipe Monlau Roca (1808-1871), born in Barcelona, began his studies at the Conciliar Seminary of Barcelona and subsequently studied science at the Royal Academy of Natural Sciences and Arts of Barcelona and at the Board of Trade of the same city. Finally he graduated and earned his doctorate at the School of Surgeons in 1830 and 1833 respectively. In parallel, he studied botany, French, Greek, English and Italian, knowledge of which he was to benefit from at a later time. In 1832 he went to work at the Military Hospital of Barcelona and in 1835 at the Royal Academy of Sciences and Arts in Barcelona as a lecturer in geography and chronology. During this time he declared himself politically as a progressive liberal by participating in journals such as *El Popular* (1834), and afterwards in *El Vapor* (1833-1837) and in *El Constitucional* (1837-1843), which he ran between 1835 -1836 and 1837 respectively. In 1837, following the victory in the general election of the moderate liberals (conservatives), he had to go into exile in Paris, where he remained until the progressive liberals won the elections in 1839. His exile in France, however, was extremely positive for Monlau's integral education, since it allowed him to come into contact with leading health professionals, scientists, and social agents in addition to providing first-hand knowledge of the progress of civilization. So he established contact with doctors and teachers at Parisian academic institutions, including Alfred Velpeau (1795-1867), Mateu Orfila (1787-1853), François Magendie (1783-1855), François Broussais (1772-1838), Pierre Gerdy (1797-1856), Philibert Roux (1780-1854), August Chomel (1788-1858), Jacques Lisfranc de St. Martin (1790-1841), Jules Cloquet (1790-1893), Achile Richard (1794-1852), Philippe Ricord (1799-1889), Joaquim Hysern (184-1883) and the liberal economist Adolphe Blanqui (1798-1854). The knowledge of all kinds which he picked up at this time would shortly be made known to Spanish citizens through various publications. Back in Barcelona in 1839, he returned to his classes at the Royal Academy of Sciences and Arts in Barcelona and in 1840 was appointed Professor of Literature and History at the University of Barcelona. In 1844, with a new moderate government in Madrid, he is banished to Valencia where he remained working as a doctor at the military hospital in the city. His stay in Valencia was to be another important milestone in his biography, as he struck up a deep friendship with the founding group of the Medical Institute of Valencia (*IMV*) (1841-1894) for instance Antoni Navarra (18??-1865), which meant he was able to use the *IMV* and its newsletter *BIMV* (1841-1896), as an Archimedean point to make his publications and activities known throughout Europe. At the end of 1846, with the aid of his friend the Majorcan doctor Ramon Frau (1795-1861), a moderate liberal and a Mason, at that time Director General of Military health, he made the pragmatic decision to collaborate with the moderates and moved to Madrid to join a committee responsible for drafting new regulations for military hospitals. From this last time on, Monlau moderated his way of expressing his health and social ideas and began a meteoric race to make his ideas known all over the world in such a way that his books can now be found in a large number of Spanish and European libraries ranging from nuns' convents to the main libraries of academic and scientific institutions. In 1847 he won the public exam for a chair in psychology and logic at the San Isidro Institute in Madrid (secondary school institute) and settled in Madrid once and for all. This same year, 1847, he was appointed secretary of the Health Council for the Kingdom (the highest advisory body on health) thanks again to the good offices of his old friend Ramon Frau and of his new friend Mateo Seoane who he coincided with in so many things. At the Council he coincided and established friendship with the other great Spanish hygienist Francisco Méndez Álvaro (1806-1883). These were the years that saw the introduction of the study of hygiene into secondary education and medical studies in Spain, and were also when Monlau published his most important books on hygiene. In 1850 he was appointed lecturer in psychology and logic at the Normal School of Philosophy. In 1851 he was appointed sanitary delegate for Spain at the first International Sanitary Conference (ISC) in Paris, likewise at the second one in Paris in 1859 and also in the one in Constantinople in 1866. Due to his participation in the ISCs he established a network of personal and scientific relationships across Europe, thus as well as the delegates at the ISCs, and as a result of these relationships, he was put in touch with the Frenchmen Michel Lévy (1809-1872) a famous hygienist, Antoine Barthelemy (Clot-Bey) (1793-1868) one of the most important epidemiologists of the time, and Brier Boismont (1798-1881) an important phrenologist; and also the Italians Francesco Torchio ((17? -1864), Gian Battista Massone (1817-1864) a hygienist and epidemiologist, Paolo Mantezzaga (1831 to 1910) a hygienist very similar in character to Monlau who influenced him very much, Salvatori Renzo (1800-1872) a hygienist and medicine historian, and the likewise hygienist and paediatrician Isacco Galligo (1822-1869). In 1857 he was appointed, at the same Normal School of Philosophy, Professor of Latin, Spanish, Limousin, and ancient Galician, simultaneously teaching this subject alongside logic and psychology. In 1858 he founded an emblematic journal on hygiene in Spain *El monitor de la Salud*, which he ran the whole time the journal was published, specifically until December 1864. Finally, with regard to his teaching work, in 1866 he was appointed professor of public hygiene and epidemiology for the doctoral studies in the Faculty of Medicine of the University of Madrid, but was stripped of it in 1868. As a result of the relationships established in the ISCs he was appointed member of many European academies of medicine and also received several medals for sanitary merit. At the same time, Monlau had developed a writing career that led him to be elected member of the Royal Academy of the Spanish Language in 1858 and of the Royal Academy of Moral and Political Sciences in 1870.

[2] International Sanitary Conferences.

Thus, we have been able to establish that as of the first ISC in Paris, a network of health professionals was steadily created - initially made up of sanitary delegates at the ISC - which, at times from opposing epidemiological and hygienic criteria, created a common state of opinion concerning epidemic diseases and how to eradicate them, through the exchange of letters, journals and books that were produced due to their knowledge at the ISC.

From this starting point which will be documented in the development of the research project, we seek to demonstrate that the personal opinions of the health professionals thus connected were decisive to the adoption of concordant health actions by the countries participating in the ISCs, and to their gradual emulation by the rest of the countries in the world.

The process that will be documented in subsequent communications where we will deal with all the ISCs, culminated in the creation in 1938 of a stable organization that covers practically all the countries in the World and which has been known since that date as the World Health Organization (WHO).

Starting then from the documentation contained in the personal Archives of Pere Felip Monlau and his own private library deposited in the Library of the Balearic Islands located in *La Real* Monastery in Palma de Mallorca, we have been able to conduct several research activities –in an initial first stage of this project– which have led us to investigate in archives and libraries in France, England, Italy and Spain in order to study the relationship of Pere Monlau with the sanitary delegates for the aforementioned countries as well as their relationships between each other, along with the influence that these relationships and the conclusions of the conferences produced on the sanitary decisions made in these countries after the successive ISCs.

Pere Felip Monlau, when the First International Conference in Paris was convened, already had a wide knowledge of publications related to the health of populations as such and of individuals in particular. Hence, in 1846 he had already *published Elementos de Higiene Privada* (Elements of Private hygiene) and in 1847 *Elementos de Higiene Publica* (Elements of Public Hygiene) published in two volumes that were widely distributed throughout Spain in public and private libraries and libraries belonging to universities, secondary school institutes, teacher training schools and church seminaries.

Fig. 1: Pere Felip Monlau.

Figure 2: Mateo Seoane Sobral.

He was introduced into the sanitary environments of Madrid thanks to his Catalan and Majorcan liberal doctor friends Pere Mata (1811-1877) and Ramon Frau (1895-1861), established in the capital some years before, as well as the likewise liberal doctor Mateo Seoane (1791-1870), Grand Commander of Spanish progressive Freemasonry. Thus we find him established in the "Health Council for the Kingdom of Spain" as of 1849, where he would remain until well into the 60's. In 1851 when the 1st ISC is convened in Paris (a city that Monlau knew very well both from a social and health point of view, as he had lived there in exile from 1837 to 1839), the president of the Health Council for the Kingdom (CSR)is Mateo Seoane, and Monlau is a member.

The Government appointed the journalist, playwright and liberal diplomat Antonio María Segovia (1808-1874) as the diplomatic delegate for Spain at the ISC and Pere Felip Monlau as the sanitary delegate, while at the same time it commissioned Mateo Seoane to coordinate the sanitary proposals to be defended at the ISC from the perspective of the presidency of the CSR[3].

This 1st ISC was held due to the major epidemics of yellow fever and plague that European states had suffered since the late 17th century, and the early 30's of the 19th century with regard to cholera[4].

Despite the different military and political difficulties that had taken place in France in recent decades, France was, in 1851, the great quintessential European power; French science was regarded as the leading scientific power and French the language of science and diplomacy as is English nowadays.

From this point, Louis Napoleon (1808-1873) President of the French Republic after numerous consultations convened the 1st ISC in Paris which opened on July 23th, 1851 with the following sanitary delegates from the accredited countries:

From Austria, Guglielmo Menis (1793-1853); from the Kingdom of the Two Sicilies, Giuseppe Carbonaro (1800-1858); from the Papal States, Agostino Cappello (1784-1858); from France, François Melies (1798-1866); from Greece, A. Costi; from Portugal, Jose Maria Grande (1799-1858); from the United Kingdom, John Sutherland (1808-1891); from Russia, Carl O. R. Rosenberger (1806-1866); from the Kingdom of Sardinia, Angelo Bo (1801-1874); from the Duchy of Tuscany, Pietro Betti (1784-1863); from Turkey, Bartoletti; and as sanitary secretary of the Conference, Antonin Jean Desormeaux (1815-1894).

At the Sanitary Conference in Paris in 1859 there were no sanitary representatives officially attending because the states were more concerned with controlling the possible resolutions and their consequences on trade rather than finding the best way to prevent diseases from spreading. It could be said that in the 1st Conference in Paris there was a balance between commercial interests and public health interests; and that in 1859 the dictates of commercial interests reached their highest point, whereas in 1866 the interests of public health began to impose themselves on the commercial ones.

The ISC of Constantinople in 1866 which opened on 13th February was attended by the following sanitary delegates:

From Austria, J. Polak and Sotto; from Belgium, the Count of Noidans; from Egypt, Selim Bey; from the Papal States, Ignaci Spadaro; from France, Antoine Fauvel (1813-1884); from Greece, A. Maccas; from Italy, Luigi Bossi (1809-1883) and G. Salvatori; from Persia, Sawas Effendi; from Portugal, Bernardino Antonio Gomez (1806-1877); from Prussia, Georg Hermann Mühlig (1826-1907); from the United Kingdom, Dalzel Edward Dickson (1816-1900) and Edward Goodeve (1816-1880); from Russia, Yevgeniy Ventseslavovich Pelikan (1824-1884) and A. Bykow and Lenz; from Turkey, Salih Effendi; and as sanitary secretary of the Conference, G. Naranzi (18?? -1877).

In an initial first stage of the research project we focus on the relationships established between Pere Felip Monlau and the sanitary professionals from Spain, France, Italy and the United Kingdom, and all of them between each other. Simultaneously we will study the influence that these relationships, discussions and the conclusions reached by the ISCs on the administrative and legislative provisions on public health that were put into practice in the above mentioned "States".

One objective of this project is to highlight the contribution of the health professionals in the ISCs through the free exchange of their personal knowledge which led the states to bring down the barriers to the free movement of knowledge and data on public health in their respective states, a fact which according to the documentation compiled makes quite clear that they were initially more than evident.

The Conferences lasted for many months and gave rise to many informal meetings between delegates and, in these, the calm exchange of points of views on the problems that had brought them together. This can be deduced from the letter (N-2) dated 18/07/1852 in Paris, written by Antonio María Segovia to the "First Secretary of State" (from the Ministry of Foreign Affairs) that we find in the AHD:[5]

> *... Coincidentally, two government delegates entered to present it at the same time, that is, Sir Magnetto, Consul General, (in Egypt) and Dr. Angelo Bo. The latter is a person of special*

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[3] Health Council for the Kingdom (CSR). In the Council there was a "Special Commission for Epidemics".

[4] Shortly before, there had been other initiatives to convene a SC. This is evident from the document found in the *Archivo Historico Diplomatico* (AHD, Diplomatic Historical Archives) in Spain dated February 4th 1850, entitled *The Sardinian Government invites His Majesty to take part in the Health Congress that will meet in Livorno, sending a delegate*. This extensive document explains not only the initiative on behalf of the Kingdom of Sardinia, but that Spain also wanted to convene a meeting along the same lines, in which at least the other state in the Iberian Peninsula, Portugal, would participate because it was already obvious that some epidemics had spread to Spain from Portugal. The document draws attention to the fact that it is absolutely necessary for the Spanish consuls to immediately inform the Central Government of any epidemic outbreak detected within the territory of these consuls.

[5] TR 488 , file 001.

knowledge on epidemic diseases and health legislation; and last year wrote on behalf of the Commission of the General Council of Genoa Maritime Health[6] *a remarkable report on "quarantine against yellow fever from America", a printed copy of which I was able to obtain before my departure from Madrid. Mr. Conde Gallina welcomed me with the utmost delicacy and warmth; gave me fine offerings, and introduced the aforementioned delegates and me to each other. Making the most of such favourable attitudes, I raised in conversation the object of our journey, and bluntly asked the Sardinian commissioners what spirit they had come in. Dr. Bo, who seems highly well-informed, and who speaks with great ease (in his Tuscan language) spoke frankly, and with much warmth and zeal. The Minister also spoke, as did all those present for the space of about two hours, with this turning into a true preliminary, yet very important, conference, due to the interest that Sardinia has always shown on this point, and due to the influence that I foresee this Power will have at the Paris Conference ...*

Another highly illustrative letter (N-3) also found in the AHD is signed by Segovia on 23rd July 1851 and also addressed to the "First Secretary":

... The French Government in addition to the Hall of Sessions has prepared for the Conference a room where delegates can go directly to consult the large number of books, maps, sanitary regulations, reports, tariffs, and other documents collected for this event; what is more it has declared that it will provide anything else that is requested. In this special library-archive, all delegates have agreed to deposit all the documents they possess, and those requested from their governments; and I believe I am also in the position to appeal to Your Excellency to send me as soon as possible the regulations and tariffs in force in Spain, and also a duplicate of the Official Bulletins that make up part of the documentation of my instructions, and any other documents that Your Excellency may deem useful to illustrate the issues, and thus influence in the deliberation ...

With these two letters it can be clearly deduced that the Conferences (in this case the first one) which in addition to carrying out discussions around public health measures to be applied in case of epidemic diseases, were used to enrich the sanitary knowledge of all participants.

The Spanish delegates to the Congress of Paris in 1851 received at the end of August the following instructions dated August 14, 1851 drawn up by Mateo Seoane as president of the Health Council for the Kingdom:[7]

Instructions for the Spanish Delegates in the International Sanitary Conference held in Paris:

Health Council for the Kingdom

Hon. Mr.

In compliance with what was offered in a final report concerning the conferences of the Sanitary Congress currently held in Paris, this Council has issued the following instructions to the Spanish Delegates of Spain at this Congress.

This Council, in performing the assignment to draw up the instructions to serve as a rule to the Spanish Delegates at the Sanitary Congress, could do no less than take into account the need not to bind them to the point in which this could impede them of any action required to embrace or reject the proposals that are presented, especially when there is no room for consultation in doubtful cases. It is much more necessary to allow them sufficient freedom of action to fulfil their mission in the present case, as the true object of the French Government in convening the Sanitary Congress in Paris, has been doubtless to commit other governments to making as many concessions as they can possibly get out of them, in order to establish the sanitary measures in favour of trade in such a way that as it is not easy to directly remove the obstacles that exist at present, indirect measures can be gradually adopted in order to achieve this objective little by little. The main one of these indirect measures, which will be discussed later, is well known: but as it is difficult to foresee what other ones will be proposed, and as it is equally difficult to calculate what turn the discussions will take, it is necessary to leave to the wisdom and knowledge of the Delegates who, taking as a guide the general principles laid down by this Council and adopted by the Government, regarding the need, uniformity and sufficiency of maritime sanitary measures, may embrace all that is in accordance with these principles, and reject that which opposes them ...

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[6] The specific book is the following: *Sulle quarentene contro la febbe gialla d'America e sulla inefficacia degli ordinamenti quarentenari della Francia: Relazione di una commissione creata dal Consiglio Generale di Sanità Marittima sedente in Genova /redatta dal d. re Angelo Bo ... relatore della Commissione*. Genova. Tipografia dei Fratelli Pagano. 1850.

[7] We Find the document in the papers belonging to Mateo Seoane in box 247 of the Monlau Archive in Balearic Library .

In view of this document from the main authority for public health in Spain (everything to do with public health depended on the Ministry of Interior) it could be said that the Spanish delegates at the first ISC had enough freedom when it came to expressing their opinions and defending their points of view or to refute or in to the opinions of other delegates, but this was not the reality. As the body which really decided the postures to be defended was the Ministry of Foreign Affairs of each participating state as we have seen in all the documentation in our possession compiled from the Diplomatic Historical Archive of the Ministry of Foreign Affairs in Spain, from the documentation concerning the ISCs found in the National Archives of the UK, from the documentation we have collected from the *Archivio Storico Diplomatico del Minnistero degli Affari Esteri* of the Italian Republic , and from documents that are in the Vatican Secret Archives or the *Archivio di Stato* in Naples. This means that the interests of state were above the interests of public health. This very point was one of the points of discussion prior to holding the Conferences, thus Spain along with other Mediterranean states formally called for the Government of France to guarantee that the needs of trade would not impede the adoption of sufficient public health measures to ensure no spread of disease.[8]

Our group, the GIHS at the UIB aims to publish, in the next two years, in high impact journals, the relationships of Pere Felip Monlau and Spanish sanitarians involved in hygiene and epidemiology, and their impact on Spanish sanitary legislation on public health between 1851 and 1871. The same will be done in relation to the British, French and Italians.

In a later stage, and if our initial theses are confirmed, we will continue to investigate these same relationships and influences on the other countries participating in the ISCs.

Bibliography

LÓPEZ PIÑERO, J.M. (1984).*Mateo Seoane: la introducción en España del sistema sanitario liberal (1791-1870)*. Madrid. Ministerio de Sanidad y Consumo.

BUJOSA, Francesc; MARCH, Joan (2006) Els llibres científics de la Biblioteca Balear (1933-2004). In: Batlló J. et al, *Actes de la VIII Trobada d'Història de la Ciència i de la Tècnica*.Barcelona. Societetat Catalana d'Història de la Ciència i de la Tècnica. Pp. 103-114.

BUJOSA, Francesc; MARCH, Joan (2008). Pere Felip Monlau i els epidemiòlegs i higienistes italians. *Actes d'Història de la Ciència i de la Tècnica*. Nova Epoca/ Volum 1(1). Pp. 307-320.

[8] We have taken into account Juan Bta Mateos Jiménez (2006) "The birth of International Health". *Revista Española de Salud Pública*. V. 80. nº6 Nov.-Dec. Madrid.

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NATURALISTIC OBSERVATIONS IN APULIA DURING THE 19TH CENTURY. VINCENZO DE ROMITA AND ENRICO HILLYER GIGLIOLI

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Abstract

After 1861, year of the Italian Unity, a new and different circulation of goods, and then of naturalistic finds and scientific instruments, emerges among the different areas of the country. The south, far from Europe, poor in both material and intellectual resources, goes on finding its reference centre in Naples. In Naples there is the University and so people go to Naples to study and to teach.

In Naples there has been for some years the Zoological Station with the Aquarium, made by Anthon Dohrn (1872). But something happens also in the far province of Bari (Apulia), where a young scholar, Vincenzo de Romita, manages to create, with personal sacrifice, two collections: a naturalistic one and another of Neolithic finds. He goes down into the caves, catches snakes, and walks tens of miles looking for carved flint stones. But, most of all, he weaves an important exchange net with the most important scholars of his time. In particular, he is friend of Enrico Hillyer Giglioli, who studied at the London School of Mines where he knew Darwin and Huxley, and who was back from an adventurous travel around the world with the Magenta corvette. Twelve letters found in the La Specola Museum of Florence witness a decade of exchanges. Finally, Giglioli entrusts him the responsibility of writing the observations on the Apulia avifauna for the making of the First Report of the Results on the Ornithological Inquiry in Italy (1890). De Romita's collection; which soon reaches some notoriety, is the basis of a series of publications: Apulia avifauna (1883), Addition to the Apulia Ornithology (1890), New Additions to Apulia Ornithology (1900) and Materials for the Fauna of Bari (1900). This last one will be presented during the universal Exposition of Paris in 1900 inside the volume The Land of Bari from the historical, economical and naturalistic point of view realized by the Province of Bari.

The Mediterranean dream

Between the end of the 19th century and the beginning of the 20th century, observations and research initiatives in the naturalistic field increased in Italy and Europe. Southern Italy was, in this period, destination for foreign scholars, attracted by the Mediterranean faunal richness. Particularly, Naples and its gulf (but also Sicily) appealed to many scientists, most of them German, who, following Goethe, pursued “the South, the Sun, the orange gardens sung by Johann Wolfgang Goethe and set in music by Franz Schubert”[1] but they also knew they can count on the promises of an unexplored, full of life, sea.

The special compulsion to particular initiatives given by the debate caused by Darwin is to be added. It should be sufficient to remember, among the others, the creation of the Zoological Station and the attached Aquarius inaugurated by Anton Dohrn in Naples in 1874 (figure 1).

Fig. 1: The Zoological Station in Naples (1874).

There have already been native initiatives of research, even notable, before the south was affected by this “Mediterranean dream”, that led Haeckel among the “ostricari” (oystermen) in S. Lucia and generations of scholars to bow their heads on the microscopes of the Neapolitan laboratory. Think about the monumental work by Oronzio Gabriele Costa about the fauna in the Kingdom of Naples. You can find numerous southern scholars interested in natural sciences also in the post-unitary period.

Even if Naples remained the obliged reference and unavoidable destination of the intellectual immigration as it happened for the Apulian Salvatore Trinchese or Achille Costa, there were also figures who remained in such a way anchored to Apulia, doing a naturalistic inventory of it. The names of Achille Bruni from Barletta, Achille Barba from Salento, Alfonso Palanza from Abruzzo, the author of the *Flora della Terra di Bari* who taught at the Liceo Carmine Sylos in Bitonto, Antonio Jatta, lichen scholar, come to our mind. But the forgotten, or nearly, and the lost collections are many.

I would like to dedicate a bit of attention to one of these stories that sees scientific passion and personal sacrifice together with the desire to overcome cultural isolation by creating a wide and even international net of relations.

Vincenzo de Romita

Vincenzo de Romita (figure 2), son of Raffaele and Felicetta Laudati, was born in Bari the 23rd May 1838 at 22 via Palazzo di Città, a short walk from the white Saint Nicholas Basilica and nearby Mercantile place and the ancient walls, then directly overlooking the sea and housing inside the ships shelters.

He studied in Naples where he knew Achille Costa, who will remember him in his *Relazione di un viaggio nelle Calabrie fatto nella state del 1876*.[2]

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[1] B. Fantini, *La Storia della Stazione zoologica Anton Dohrn di Napoli in La scienza nel mezzogiorno dopo l’Unità d’Italia*, Rubettino, Catanzaro 2009, pp. 353-356.

[2] *Relazione di un viaggio nelle Calabrie fatto nella state del 1876*. Memory from the IXth volume of *Atti della Reale Accademia delle Scienze Fisiche e Matematiche di Napoli*. Read during the Meeting of the 11th May 1881. Tipografia dell’Accademia Reale delle Scienze, Napoli 1881, p.2.

> Having left Naples the evening of the 11th July by train, I stopped in Bari the 12th, being very uncomfortable going on towards Calabria. I spent a few hours in the city with Mr. Vincenzo de Romita, already scholar of our University and now professor of Natural History in that Technical Institute, who with zeal deals with the collection of the most particular and interesting things the region could offer him: objects I could observe, others in the Institute office, others in his particular collections. For example I could notice various species of birds not easy to find in southern Italy. Among the Reptiles he had the *Coluber leopardinus*, variety described by Pallas with the name of *Col. Lineatus*. Among the fish, a notable two-headed Shark foetus, a not common monstrosity in that class of Vertebrates. Finally, among the insects there was the *Cicindela dilacerata, D.c.j.*, which appeared for the first time in the Fauna of the Neapolitan provinces.

The collection Costa talks about is the naturalistic finds one, that the scholar, then aged thirty-eight, had started some years ago both privately and as a collection of didactic use at the “Reale Istituto tecnico e nautico”, where he had been teaching science since 1874. Later, in order to classify and describe the Apulian Fauna (with particular reference to ornithology), de Romita will publish *Avifauna pugliese* (1883), *Aggiunta all'Ornitologia pugliese* (1890), *Nuove aggiunte all'Ornitologia pugliese* (1900) and *Materiali per una fauna barese* (1900).

Fig. 2: Vincenzo de Romita (1838-1914).

During his youth he devoted himself with great enthusiasm and energy to search the countries of Canosa, Toritto,Terlizzi, Altamura, Cassano, Alberobello, Monopoli, Fasano, looking for Neolithic artefacts and moving further to Gargano and Basilicata. These excursions allowed him to set up a collection of more than one thousand and five hundred exemplars such as arrow heads, knives, spears, hatchets, scrapers. He presented a significant selection of them during the “Esposizione italiana di Antropologia ed Archeologia preistorica” (Italian Exposition of Prehistoric Anthropology and Archaeology).

The Province of Bari could so appear, not making a bad impression[3] on the other hand, during the exposition, which came alongside the International Congress of Bologna in 1870.

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[3] V. de Romita, *Gli avanzi antistorici della provincia di Bari*, Stabilimento tipografico Cannone, Bari 1876. Anastatic reprint in P. Zeller (edited by) *Vincenzo de Romita e il suo tempo,* Adda Editore, Bari 2010 , p. 4. The contribute of de Romita to the Exposition is also reported in L. Pigorini, *Relazione sulla Esposizione italiana di antropologia*,1871, pp. 30-31. See also: *Bull .paleontol.it*, II, 1876, p. 207.

In a booklet of 1876 named *Gli avanzi antistorici della provincia di Bari* he wanted to describe these researches of his own, the long soil searching, the findings emotion, the hard talks with the farmers who, considering the silica objects provided of "the virtue of keeping the evil eye away", appeared very reluctant to give them up.

"Not without great difficulties" –as he writes– "and using all kinds of persuasion, I sometimes succeeded in taking away an arrow head, handed down from generation to generation and attached of the strangest virtues." He proceeded from the fact that the province of Bari had been, from this point of view, not much studied "even if very rich in rock artefacts" and it had not had "so far someone who had designedly collected and studied the prehistoric archaeology treasures which it was throughout strewed with". And he came to a surprising result: "My researches were crowned by such an splendid outcome that it overcame my conceived hopes."[4]

The criteria he followed trying to date the finds on the basis of the possible remains of the ancient fauna were also, if one might say so, modern as: "trying to date the human artefacts when the fauna horizon which goes with the finds is missing you run the risk of judging rashly."[5] Starting from 1875, and for at least 10 years, de Romita had a correspondence with Enrico Hillyer Giglioli (figure 3), then director of the "La Specola" museum in Florence as the 12 letters kept in it attest.[6]

Enrico Hillyer Giglioli

Enrico Hillyer Giglioli studied in England as a young man at the Royal School of Mines, where he knew Charles Darwin, Thomas Huxley and Charles Lyell.

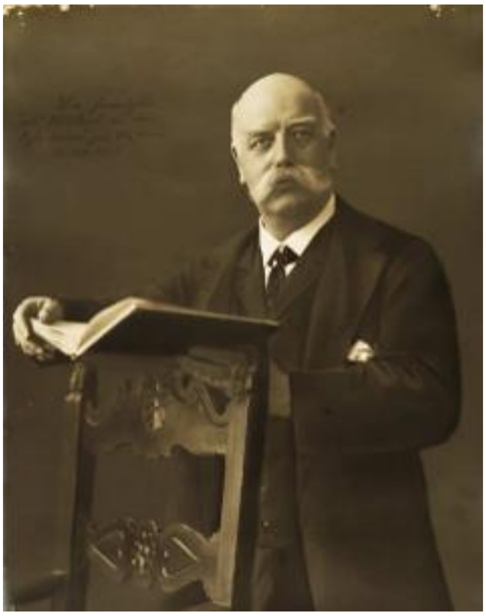

Fig. 3: Enrico Hillyer Giglioli (1845-1909).

He rather edited the publication of Huxley's lessons on the journal *Lancet* before being obliged to come back to Italy because of the missed renewal of his own grant.

The following year, aged only twenty, on the scientist and senator Filippo De Filippi's recommendation, he embarked the Magenta "pirocorvetta" for a voyage of circumnavigation around the world. In Hong Kong, on his way back, De Filippi, struck down by amoebic dysentery, died at the age of 53. At the end of the voyage Giglioli stayed for some time at the "Regio Museo Zoologico"(Royal Zoological Museum) to reorganize the wide collected material under the management of Michele Lessona, who by then had also replaced De Filippi in the role of Italian divulgator of Darwinism.

[4] *Ibidem*, p.4.

[5] *Ibidem*, p.7.

[6] See: *Vincenzo de Romita e il suo tempo*, cit.

Moved to Florence, in 1874 he was appointed full professor of Zoology and Compared Anatomy of the Vertebrates. He started to create, in those years, the Central Collection of the Italian Vertebrates as well as ethnographical and anthropological collections.

He wrote 400 scientific publications, most of which about natural sciences, among them *Zoologia della Magenta*, a wide report of his adventurous voyage, praised by the minister Quintino Sella. He made a lot of research trips to Europe and organized a wide net of relations also aimed at obtaining exemplars to improve his collections. A considerable part of his attention was occupied by ornithology. In 1879 he started the publication of the *Iconografia dell'Avifauna italica* (incomplete), the aim of which was that of offering a realistic picture of all the bird species in Italy. In 1881 he published two works about the Italian Avifauna and afterwards he organized an ornithological survey by sending modules to the observers all over Italy. The *Primo Resoconto dei Risultati dell'Inchiesta Ornitologica in Italia*, his first account of the results of the ornithological survey in Italy, was published in the years 1889-1890, while the *Secondo Resoconto dell'Inchiesta Ornitologica* in 1907. He died the 16th December 1909 after a heart attack.

The de Romita-Giglioli correspondence

The correspondence between de Romita and Hillyer Giglioli is mainly made of proposals of exemplars exchange or, sometimes, selling. However their personal relation went far from these negotiations and materialized into mutual visits. De Romita remembered in his *Materiali per una fauna barese* a work later presented in the Universal Exposition in Paris in 1900, one of his visit in Apulia:

> I have to thank the fortune of having accompanied the eminent professor during an excursion in Taranto for ichthyologic researches if I have the pleasure to number also this species (*Cygnus Bewickii*), native of the extreme north of Europe and Asia, in the *Avifauna pugliese*. In the "Swan pharmacy" we admired one exemplar which had been caught in the gulf of Taranto and properly in the Mar Piccolo during the winter of 1878.[7]

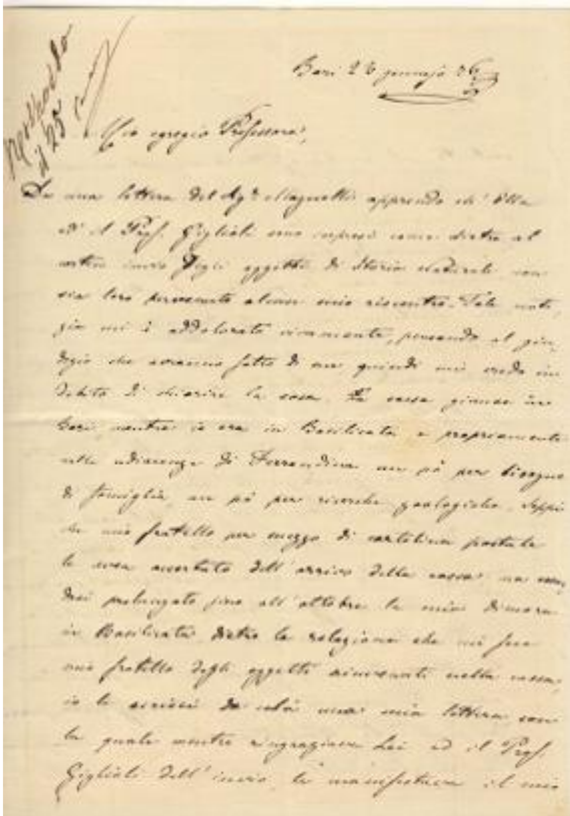

Fig. 4: A letter from the correspondence between de Romita and Hillyer Giglioli.

[7] *Materiali per una fauna barese*, p.63. Anastatic reprint in P. Zeller (edited by) *Vincenzo de Romita e il suo tempo*, cit.

We also know from the correspondence that de Romita submitted the proofs of the *Avifauna pugliese* to the younger but very famed professor for examination. In a letter of 1885 we can read:

> Finally my poor work about the Apulian Avifauna is at the printing house and within few days I will be able to collect the first proofs! I also would like to contribute to the development of the Italian ornithology; and if I succeed with this work I will let you and the experts judge. Meanwhile I would have a singular fortune if, before releasing it, I could submit it to you for examination, for this reason I will send you the proofs as soon as they are ready so that you may write your observations down in the margins. But this only if it would not cause you too much trouble, therefore I look forward to your adhesion.[8]

Actually the work was published only months later without the examination of the proofs:

> The printing of my memories about the Apulian birds embarrasses me a lot because of the typographer, who after leaving whole weeks pass without composing a word, when he in a rush has composed a sheet, he does not give me the necessary time to correct it thoughtfully. As it happened with the first sheets, for which it was not possible for me to submit them to your examination. Meanwhile I send them to you adding the composed pages, not yet printed. I will consider with great favour all the observations you will do about it and I will wait for the corrections of the part not yet published, with the hope of using it.[9]

Hillyer Giglioli was the representative of a prestigious research centre, provided with adequate means. "Not on another pact the poor can give to the rich, and I am very poor, and you are very rich", de Romita wrote him in March 1883, talking about the necessity for confidently waiting for the exchange compensations despite of the great delays.

In this symmetry, nevertheless, we surely know that Giglioli really appreciated the work of his "poor" colleague, so much that he got him to provide the data concerning Apulia for his famous ornithological survey. In the *Primo Resoconto dei risultati dell'inchiesta ornitologica in Italia* he wrote: "This list has been filled in by the eminent professor Vincenzo de Romita in Bari, author of an excellent work on the Apulian Avifauna published in Bari in 1884; in this list there are a lot of observations, unpublished so far."[10]

Also later, in the second report in 1907, he will mention him dozens of times.

Ultimately we can here register, as in the post-unitary context, the need for nets of cultural relationships is getting stronger in order to achieve a deep comprehension of the national territory and the local realities. In such a pattern both great initiatives, as Giglioli's survey aimed at evaluating the Italian faunal reality, and local but not isolated initiatives, directed to didactic and research aims, found their place.

De Romita's work (both the collections and the publications) gives us back the picture of Apulia as characterised by a richness of species, a biodiversity which can nowadays only appear as extraordinary. We can now read about the Great bustard (*Otis tarda*) again, here extinct, "this majestic bird, which was reasonably called the ostrich of Europe, not seldom appears in the Capitanata plains among the endless cultivated wheat fields […]." We can also read about the Little bustard (*Tetrax tetrax*), also disappeared, "which you can meet with more frequency and nesting in Apulia." About the beautiful Purple swamphen (*Porphyrio porphyrio*), de Romita writes: "two exemplars of this beautiful small bird ("rallo") were sent to prof. Giglioli, from the padule surrounding Lesina lake and, according to the eminent ornithologist, it may be sedentary and not rare there."[11]

Sightings of golden eagles, spotted eagles and *Bonelli*'s ones in Bitonto, Santo Spirito, Taranto, talk about a far-off time in which Apulian skies were crossed by the shapes of impressive birds of prey.

Worries about the excessive and indiscriminate destruction made by hunting were not already then extraneous to these observations. His critics are often direct and explicit, sometimes in the context of impressionistic or poetic descriptions as in the case of the robins which "are in winter the most beautiful characteristic ornament of the fields where there is thick vegetation […] and despite this fact their massacre by snares is really dreadful."

Talking, for example, about grebes he tells how:

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[8] Letter of the 17th February 1885 in P. Zeller (edited by) *Vincenzo de Romita e il suo tempo*, cit.

[9] Letter of the 25th April 1885 in P.Zeller (edited by) *Vincenzo de Romita e il suo tempo*, cit.

[10] See E.Hillyer Giglioli, *Primo Resoconto dei Risultati dell'Inchiesta Ornitologica in Italia. Parte Seconda. Avifaune locali. Risultati della inchiesta ornitologica nelle singole province compilato dal dott. Enrico Hillyer Giglioli.*, Le Monnier, Firenze 1890, p. 560.

[11] *Materiali per una fauna barese*, Anastatic reprint in P. Zeller (edited by) *Vincenzo de Romita e il suo tempo*, cit., p. 58.

> In winter much more than in autumn or in spring, you can see on the sea at a short distance from the shore some couples or a small number of this very moving creatures, singular as other congeners not only for their organization but for their essentially aquatic life. The bright white of their lower parts, the gracefulness of their movements, the really colourful poses they assume on the wave, their continuous diving and floating back to the surface cannot pass unnoticed and the passer-by cannot help stopping ecstatic in a beautiful winter day and admire the show. Unfortunately our hunters' lead often takes this enchantment away.[12]

The worries for the hunt destructiveness are shared also by other (not that many) scholars of the time such as Giacinto Martorelli, Joseph Whitaker, Cecilia Picchi, Giovanni Salvatori, Arturo Francelli.

Martorelli, for example, wishes "the preservation of our whole fauna, the components of which are all so linked by very complicated biological relations we still know very little about, that it would be very risky trying to cut any."[13] In the opening relation of the "Congresso Cinegetico" (Cynegetic Congress) in Rome the 12th November 1911, he admonishes: "Seeing continuously coming to us the innumerable crowds of passing birds with marvellous constancy of dates, without realizing their sensibly decreased number over the years, let us think their production was endless and we squandered killing them in millions."[14]

Ultimately, the post-unitary period appears signed not only by the highest impulse in constituting zoological collections but also by the first worries (then often disregarded) for preservation of a heritage of which we realize the extraordinary vastness only now. With the new collections, among which the one wanted by Hillyer Giglioli in Florence stands out (The Central Collection of the Italian Vertebrate Animals), stricter criteria of cataloguing are also needed. The labelling of each exemplar (date, place, gender, age), reported in a general catalogue, is substituting the custom according to which the information about date and place of collecting were generally omitted. Giglioli writes:

> Each species, when necessary, should be represented by the places in which it lives, in order to obtain precise knowledge about its local distribution. No piece will be admitted in the collection if it has not been determined, registered, catalogued and labelled, so that the collection is always well-ordered.[15]

De Romita's collection (figure 5), who takes care of placing the vernacular names next to the traditional classification in his catalogues, certainly represents this new trend as it constitutes an important example of the so widespread appearing of local collections (private, in schools or in religious institutes) outside the great academic and museum institutions.

Fig. 5: De Romita's collection in Bari, in the present day.

[12] *Ibidem*, p. 73.

[13] See: Barbagli F., Giacinto Martorelli: ornitologo, illustratore, protezionista, in Martorelli G., *Monografia illustrata degli uccelli di rapina in Italia (1895), Memorie della Società italiana di Scienze Naturali e del Museo Civico di Storia Naturale di Milano*, Milano, 2002, vol. XXI, number II, pp. XII-XIII.

[14] *Ibidem*.

[15] *Ibidem*.

References

BARBAGLI F., VIOLANI C. (1996), *L'inchiesta ornitologica di Enrico Hillyer Giglioli*, in Riv. ital. Orn., 65: 136-146.

COSTA O. G. (1850-86), *Fauna del Regno di Napoli, ossia enumerazione di tutti gli animali che abitano le diverse regioni di questo Regno e le acque che le bagnano contenente la descrizione de' nuovi o poco esattamente conosciuti con figure ricavate da originali viventi e dipinte al naturale*, Azzolino, Napoli 1829-1850; Sautto, Napoli.

DE JULIIS E.M. (Edited by) (1983), *Il Museo archeologico di Bari*. Adriatica Editrice, Bari, 1983.

DE ROMITA V. (1876), *Gli avanzi antistorici della Provincia di Bari*, Stabilimento tipografico Cannone, Bari.

DE ROMITA V. (1884), *Avifauna pugliese*. *Catalogo sistematico degli uccelli osservati in Puglia* ." Annuario del Regio Istituto Tecnico e Nautico di Bari", Anno 1883 vol. II, Stabilimento tipografico Cannone, Bari, pp. 62-205; reedited Forni, Sala bolognese, 1978.

DE ROMITA V. (1900), Materiali per una fauna barese, in S. La Sorsa (a cura di), *La Terra di Bari sotto l'aspetto storico, economico e naturale,* vol. III,Vecchi, Trani, pp. 245-338; reedited. Levante editori, Bari, 1986.

HEUSS T. (1953), L'Acquario di Napoli e il suo fondatore Anton Dohrn, Edizioni Casini, Roma.

HILLYER GIGLIOLI E. (1890), *Primo Resoconto dei Risultati dell'Inchiesta Ornitologica in Italia. Parte Seconda. Avifaune locali. Risultati della inchiesta ornitologica nelle singole provincia compilato dal dott. Enrico Hillyer Giglioli*., Le Monnier, Firenze.

HILLYER GIGLIOLI E. (1907), *Secondo Resoconto dei Risultati dell'Inchiesta Ornitologica in Italia. Nuovo elenco sistematico delle specie di uccelli stazionarie, di passaggio o di accidentale comparsa in Italia*. Stabilimento tipografico S. Giuseppe, Firenze.

MARTORELLI G. (1906), *Gli Uccelli d'Italia*, L. F. Cogliati, Milano.

ZELLER P., (edited by) (2010), *Vincenzo de Romita e il suo tempo.* Adda Editore. Bari: 1-247.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 208.

ERNA LESKY, GENERAL AND DIPLOMAT. NETWORKING AS A POWER TOOL FOR THE HISTORY OF MEDICINE

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Abstract

Erna Lesky was the first female professor at the Medical Faculty of the University of Vienna. She was able to use perfectly the mainly male occupied behaviour patterns and power tools to her advantage on the way to the head of the Institute for the History of Medicine. Lesky knew how to demonstrate authority and power and how to gain allies. Diplomatically built up networks and an enormous determination for performance had stamped her career success. With publications focusing on the Vienna Medical School, Lesky marked the Austrian way of writing the history of medicine. Her methods, focusing on medical efforts, became standard, allowing no other modern, interdisciplinary access. This paper is a differentiated presentation of the career of a female physician and historian of medicine who broke with the traditional role model and perceptions of women in medicine.

A short biography of a female physician

Erna Klingenstein was born on the 22nd of May 1911 in Hartberg, Styria, a federal country of Austria. She took her A-levels at the Academic Gymnasium in Graz, the capital of the Austrian federal country Styria, in 1929. At school, she also became acquainted with her future husband, Albin Lesky, who was her teacher for Latin and Greek. Klingenstein decided to study medicine and graduated at the University of Vienna in 1936.[1] In 1938, Lesky divorced his first wife Dr. Grete Lesky[2] and married Klingenstein. Two years before, he was given the chair for classical philology at the University of Innsbruck. Erna Lesky worked as a physician at the pediatric clinic of Innsbruck from 1938 to 1940 and from 1940 to 1945 at the NSV mother and child home in Igls, the Tyrol, an institution founded by the National-Socialist Party.[3] In 1949, the couple moved to

[1] Michael HUBENSTORF, Vom Erfolg und Tragik einer Medizinhistorikerin. Erna Lesky (1911–1986). In: Christoph Meinel, Monika Renneberg (edd.), *Geschlechterverhältnisse in Medizin, Naturwissenschaft und Technik. Im Auftrag des Vorstandes der Deutschen Gesellschaft für Geschichte der Medizin, Naturwissenschaft und Technik* (Bassum-Stuttgart 1996) 98–109, here 99.

[2] Divorce documents from Dr. Grete Lesky, 29th September 1938. Personal documents 1902-1981, 131.97.01 (Archives of the University of Vienna).

[3] HUBENSTORF, Vom Erfolg und Tragik einer Medizinhistorikerin. In: Meinel, Renneberg (edd.), *Geschlechterverhältnisse in Medizin, Naturwissenschaft und Technik*, 99.

Vienna,[4] where Lesky stepped out of the position of the "wife of a famous scientist" and began to manage her own career, with the full support of her husband. Her strategical thinking proved successful in establishing contacts, building up confidence and cultivating relationships with the intention to exchange relevant informations. Organizing power by intensive social networking became a key element of Lesky's science management.

Strategical thinking

Lesky was interested in the history of medicine, and through her husband linked up well in the Scientific Community of Austria, Germany and Switzerland. In 1949, Paul Diepgen, who represented the editorial office of the history of science journal *Centaurus* in Germany and which was soon to appear in Copenhagen, offered to publish articles written by Lesky.[5] Two years later, Lesky published *Die Zeugungs- und Vererbungslehren der Antike*[6] by the publishing house of the Austrian Academy of Sciences, which would later become her habilitation.[7] Lesky's husband, as a classical philologist, was able to support her research and supply her with literature accordingly. In this way, he offered his wife a strong academic backing.[8] Lesky planned to habilitate in the history of medicine before she had even enrolled for history studies at the University of Vienna. Lesky expected better chances in this humanities orientated medical subject, rather than in clinical subjects. There a female professor was hardly conceivable for the male professors, even in the middle of the 20th century.

Diepgen, Lesky's adviser and mentor, informed her out of his experience, "that she would have difficulties with the habilitation, without being the assistant of a professor of the history of medicine at her university".[9] The expert and provisional head of the Institute for the History of Medicine at Vienna University at this time was Leopold Schönbauer who already had a female assistant he needed and favoured: the internist Marlene Jantsch. Diepgen recommended Lesky "not to loose courage", because if he would intercede on his "initiative […] without request" of Schönbauer, he would only cause "harm". It would be better to ask an "influential member" of the Medical Faculty who would patronize her and ask him about her "qualification for the lectureship". Diepgen recommended her to use the "waiting period" until her habilitation by writing popular-scientific essays to make the history of the medicine popular in doctors' circles. "It has to be clear to ourselves", Diepgen explained, "that we must talk a little bit primitively before the public, if we want to promote the history of medicine for our medical colleagues".[10]

The woman behind

In 1956, Lesky gained a doctorate in history[11] and habilitated only one year later in the history of medicine. On the same day as Lesky habilitated, her competitor, Jantsch, qualified as a professor.[12] The scientific career-path of Marlene Jantsch, née Ratzersdorfer, is a demonstrative example of the exploitation of the working capacity and willingness of female physicians. Six years younger than Lesky, she was born on the 26th September 1917, in Osterwieck, Germany, but took her A-levels at the Girls Grammar School in Döbling, Vienna, in 1936. On the 26th July 1941, she graduated from the Medical Faculty of the University of Vienna and became an internist at the First Surgical Clinic of the General Hospital in Vienna, chaired by Schönbauer. As Schönbauer's personal assistant she wrote numerous essays and articles in the history of

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[4] *Ibid*., 101.

[5] Paul DIEPGEN, letter to Erna Lesky, 8th November 1949. In: Estate of Erna Lesky, handwritings- collection 4.700/21 (Institute for the History of Medicine, Medical University of Vienna).

[6] Erna LESKY, *Die Zeugungs- und Vererbungslehren der Antike und ihr Nachwirken* (The procreation- and genetics doctrines of the antiquity) (= Abhandlungen der Geistes- und Sozialwissenschaftlichen Klasse, Österreichische Akademie der Wissenschaften und der Literatur 19, 1950, Mainz 1951).

[7] Karl SABLIK, Zum Gang der medizingeschichtlichen Forschung in Wien seit 1945. In: *Mensch. Wissenschaft. Magie. Mitteilungen. Österreichische Gesellschaft für Wissenschaftsgeschichte* 20 (2000) 53–58, here 54.

[8] Personal information, given by Lesky´s former assistant professor Karl Sablik, 25th August 2010. I want to thank all former assistent professors of Erna Lesky for the precious informations I was given. They can describe Lesky´s career from their point of view and know her personality very well.

[9] DIEPGEN, letter to Lesky, 8th November 1949. In: Estate of Lesky, handwritings-collection 4.700/21 (Institute for the History of Medicine, Medical University of Vienna).

[10] DIEPGEN, letter to Erna Lesky, 22nd November 1951. In: *Ibid*.

[11] Erna LESKY, Staat und Heilkunde im Zeitalter des aufgeklärten Absolutismus in Österreich (State and Medicine in the Age of Enlightened Absolutism in Austria) (Dissertation Universität Wien 1955). The thesis was published by Erna LESKY, *Österreichisches Gesundheitswesen im Zeitalter des aufgeklärten Absolutismus* (Austrian Public Health in the Age of Enlightened Absolutism). In: Österreichische Akademie der Wissenschaften, Philosophisch-historische Klasse, Historische Kommission, 122,1 (1959).

[12] HUBENSTORF, Vom Erfolg und Tragik einer Medizinhistorikerin. In: Meinel, RENNEBERG (edd.), *Geschlechterverhältnisse in Medizin, Naturwissenschaft und Technik*, 101.

medicine, all of which were not published under her name, but his. Together with her husband Hans Jantsch, she researched for Schönbauer's book *Das medizinische Wien*[13] which was published in 1944 but once again, only by Schönbauer. None of them were named as authors. Jantsch supervised different departments at the Josephinum,[14] the former Military Medical Academy, where the Institute for the History of Medicine was housed now. There Schönbauer was the provisional head from 1944 to 1960.[15] Throughout this time, Jantsch remained "the woman behind". It was only after 1948 that Jantsch was named for the first time as an author in publications of the Viennese Institute for the History of Medicine.[16]

Referring to Lisa Fischer, men constructed the reality in science and had the "power of definition". If women succeeded in developing a self-image, contrary to "the male external definition", they had to suppress it in order "to maintain the patriarchal system".[17] Female physicians at the Medical Faculty of the University of Vienna did not succeed in reaching academic top positions till the middle of the 20th century. They were assigned to scientific auxiliary services and worked on publications which were published under the name of the male superior. Their knowledge was used, success was male encoded.

The "habilitation war"

Due to their different social environments and political views, Lesky and Jantsch also held contrary ideas as to their 'female' roles in medicine and the history of medicine.[18] Lesky did not range into the existing system, but broke with the traditional role model and perceptions of women in medicine. For Schönbauer she was, compared to Jantsch, less qualified for a habilitation in the history of the medicine. He classified Lesky as a "historian of philology" and not as a historian of medicine.[19] Lesky did not allow this comment to discourage her. Because Schönbauer had forbidden her to give lectures at the Josephinum, the former Military Medical Academy, where the Institute for the History of Medicine was housed, Lesky began in 1957 to lecture at Richard Bieling's Institute of Hygiene, University of Vienna. [20]

The "habilitation war", as the historian of medicine, Erwin H. Ackerknecht, calls the conflicts in his private correspondence with Lesky, concerning the professional and personal acceptance of the historian of medicine on the terrain of the Viennese Medical Faculty,[21] was won by diplomatically built up networks and an enormous determination for performance. These social competences influenced the decision of the Medical Faculty and while Jantsch remained bonded to Schönbauer, Lesky established international contacts. She integrated well into a scientific community of male order. Her correspondence with historians of medicine in Europe and the USA proves how extremely well she was able to build up relationships. In the search for expert opinions according to her ability to become a professor, she remained clever, but ambitious. In contrast to Jantsch, Lesky used the male occupied instruments of power and science policy strategically to conquer a man's domain.[22] She was able to negotiate her way through the medical faculty and successfully break up the old structures which existed there. Michael Hubenstorf deals with this conflict for the first time in the essay *Success and tragedy*

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13 Leopold SCHÖNBAUER, Das *medizinische Wien. Geschichte, Werden, Würdigung* (Medical Vienna. History, Development, Appreciation) (Berlin-Wien 1944).

14 Sonja HORN, Gabriele DORFFNER, „... männliches Geschlecht ist für die Zulassung zur Habilitation nicht vorgesehen". Die ersten an der medizinischen Fakultät der Universität Wien habilitierten Frauen. In: Birgit BOLOGNESE-LEUCHTENMÜLLER, Sonja HORN (edd.), *Töchter des Hippokrates. 100 Jahre akademische Ärztinnen in Österreich* (Wien 2000) 117–138, here 137.

15 HUBENSTORF, Vom Erfolg und Tragik einer Medizinhistorikerin. In: MEINEL, RENNEBERG (edd.), *Geschlechterverhältnisse in Medizin, Naturwissenschaft und Technik*. 101.

16 HORN, DORFFNER, „... männliches Geschlecht ist für die Zulassung zur Habilitation nicht vorgesehen". In: BOLOGNESE-LEUCHTENMÜLLER, HORN (edd.), *Töchter des Hippokrates*, 137.

17 Lisa FISCHER, Weibliche Kreativität – oder warum assoziieren Männer Fäden mit Spinnen? In: Jürgen NAUTZ, Richard VAHRENKAMP (edd.), *Die Wiener Jahrhundertwende. Einflüsse – Umwelt – Wirkungen* (= Studien zu Politik und Verwaltung 46, Wien-Köln-Graz 1993) 144–158, here 144.

18 HORN, DORFFNER, „... männliches Geschlecht ist für die Zulassung zur Habilitation nicht vorgesehen". In: BOLOGNESE-LEUCHTENMÜLLER, HORN (Hgg.), *Töchter des Hippokrates*, 137.

19 HUBENSTORF, Vom Erfolg und Tragik einer Medizinhistorikerin. In: MEINEL, RENNEBERG (edd.), *Geschlechterverhältnisse in Medizin, Naturwissenschaft und Technik*. 101.

20 SABLIK, Zum Gang der medizingeschichtlichen Forschung. In: Mensch. Wissenschaft. Magie. Mitteilungen. Österreichische Gesellschaft für Wissenschaftsgeschichte 20 (2000) 54.

21 Erwin ACKERKNECHT, letter to Erna Lesky, 6th November 1956. In: Estate of Erna Lesky, handwriting- collection 4.700/5 (Institute for the History of Medicine, Medical University of Vienna). Ackerknecht found it sad that the "habilitation war" was so embarrassing for Lesky´s scientific work.

22 Lesky´s private and professional correspondence is an important source to reconstruct her career steps up to the professorship.

of a historian of medicine.[23] According to Sabine Schleiermacher, the chosen career-paths of female physicians would be understood easier, if the "decision-making processes" were seen in their "relations to women as well as to men". Only then could "difference and equality" be made clear.[24]

During the era of National-Socialism, Schönbauer was a member of the NSDAP. Nevertheless, immediately after 1945 he tried to get rid of this stigma by asking socialist politicians, like vice chancellor Adolf Schärf, to intervene in the governement.[25] However, Lesky, working for a national-socialist institution as well, found support from former members of the national-socialist party and in the Christian-democratic political sphere around her husband. The classification of Lesky in a political category could make her career more understandable.

Lesky's "reconquered Josephinum"

In October 1960, Lesky became the manager of the 'Josephinum', though at the beginning without a salary. After receiving calls from the Free University of Berlin, the University of Göttingen and the University of Hamburg, as well as having numerous publications, Lesky now had enough qualifications to become an associate professor in 1962. In 1964, she represented the Medical Faculty of the University of Vienna at the 600th anniversary of the University of Cracow, together with the rector for the academic year 1963/64, Albin Lesky.[26] Erna Lesky restructured the Institute for the History of Medicine, organized numerous conferences and international congresses whereby she presented 'her' Viennese institute to the scientific community. On the occasion of the six-hundred-years celebration of the University of Vienna in 1965, Lesky invited the surgeon Lorenz Böhler to her "reconquered Josephinum" with its precious collections and libraries.[27] She became an internationally recognized science organizer. The historian of medicine attended international conferences as part of networking in science to present her research results to acknowledged physicians and historians of medicine.[28] While Lesky was promoted up to the habilitation mainly by historians of medicine from abroad, she was able to win, with a lot of energy, prestige for her call from institutions in Austria and representatives of the Federal Government.[29] In 1966, Lesky received a chair at the Medical Faculty of the University of Vienna. She emphasized impressively that she was the first female professor since the university was founded in 1365.[30] In international comparisons, this was extremely late. Peter Voswinckel states that the Harvard Medical School had its "first female faculty member" with Alice Hamilton in 1919. In 1920, Winifred Clara Cullins received a chair of physiology in England. Even in Germany, Selma Meyer became professor for pediatrics in 1922.[31]

Due to consistent urging at the Ministry of Education in Austria and by successful sponsorship of the Welcome Trust in London, the "big doyenne" of the history of the Viennese Medical School put through a complete renovation and re-orientation of the Institute for the History of Medicine at the University of Vienna. Lesky knew how to derive personal benefits for her representative institute from her international networks. The Josephinum became the centre for the Austrian historiography of medicine, also because the other medical faculties at universities in Austria did not establish their own

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[23] HUBENSTORF, Vom Erfolg und Tragik einer Medizinhistorikerin. In: MEINEL, RENNEBERG (edd.), *Geschlechterverhältnisse in Medizin, Naturwissenschaft und Technik*. 101.

[24] Sabine SCHLEIERMACHER, Ärztinnen im Kaiserreich: Ein Forschungsprojekt zur Geschlechtergeschichte. In: MEINEL, RENNEBERG (edd.), *Geschlechterverhältnisse in Medizin, Naturwissenschaft und Technik*, 217–224, here 222.

[25] Vice chancellor Adolf SCHÄRF, letter to the dean of the Medical Faculty of the University of Vienna, 28th December 1945 (Akademischer Senat, Sonderreihe ZI 141, 1945/46, Archives of the University of Vienna), cit. in: Wolfgang NEUGEBAUER, Peter SCHWARZ, *Der Wille zum aufrechten Gang. Offenlegung der Rolle des BSA bei der gesellschaftlichen Reintegration ehemaliger Nationalsozialisten, ed. Bund sozialdemokratischer Akademiker Innen, Intellektueller und KünstlerInnen* (Wien 2005) 50.

[26] HUBENSTORF, Vom Erfolg und Tragik einer Medizinhistorikerin. In: MEINEL, RENNEBERG (edd.), *Geschlechterverhältnisse in Medizin, Naturwissenschaft und Technik*. 102.

[27] Lorenz BÖHLER, letter to Erna Lesky, 15th Dezember 1965. In: Estate of Erna Lesky, handwriting-collection 4.700/8 (Institute for the History of Medicine, Medical University of Vienna): "On 18th February 1965 you have written a long and amiable letter to me, where you invited me to visit your reconquered Josephinum."

[28] Helmut WYKLICKY, Erna Lesky. In: Institut für Geschichte der Medizin. Gewidmet dem Angedenken an Max Neuburger, Gründer des Instituts für Geschichte der Medizin der Universität Wien. Institute for the History of Medicine, University of Vienna. Dedicated to the memory of Max Neuburger, founder of the Institute for the History of Medicine, University of Vienna (Wien 1999) 55–56, here 56. Wyklicky explains further that the institute frequently participated at congresses by lending objects for display.

[29] Personal information given by Lesky´s former assistant Professor Karl Sablik on 3rd June 2008.

[30] SABLIK, Zum Gang der medizingeschichtlichen Forschung. In: Mensch. Wissenschaft. Magie. Mitteilungen. Österreichische Gesellschaft für Wissenschaftsgeschichte 20 (2000) 53.

[31] Peter VOSWINCKEL, Frauen-Kontingent im Biographischen Lexikon hervorragender Ärzte von 1933. In: MEINEL, RENNEBERG (edd.), *Geschlechterverhältnisse in Medizin, Naturwissenschaft und Technik*, 225–236, here 229.

institutes for the history of the medicine.[32] In general, the history of science in Austria shows that, compared to other European countries, there is a big deficit: not before 2008 did Friedrich Stadler receive the first chair of history and philosophy of science at the University of Vienna.[33]

The "big doyenne" of the history of the Viennese Medical School

Leskys diligence and engagement for the history of medicine were reflected in her publications. Her '*opus magnum*', *The Vienna Medical School of the 19th Century*,[34] was first published in 1965, the second edition appeared in 1978 and in 1976 it was published in English.[35] It is certainly one of the high quality standard works of history of medicine and will remain so "for a long time" says Karl Sablik, one of Lesky's former assistants.[36] Lesky's mainly sources-oriented methodology which concentrated on ideas and institutions, still excluded the sociological and political context of medical developments and institutional conflicts. Her biographies of physicians often remain heroic epics that focus on medical achievements. Sablik, who received the first lectureship in the social history of medicine in German-speaking countries in 1989, noted with regret, that an interdisciplinary re-orientation of the Institute for the History of Medicine in Vienna was prevented by Lesky.[37] Thomas N. Burg is convinced that Lesky had "paralyzed" the development of newer research attempts, especially as outside of the Medical Faculty of the University Vienna, hardly any research in the history of medicine took place in Austria. As an extraordinary and full professor at the Medical Faculty, Lesky determined "*qua persona*", what had to be understood by the history of medicine in Austria until the end of the 1970s.[38]

Albin Lesky was a full member of the Austrian Academy of Sciences and was president in 1969/70, while Erna Lesky was only appointed a corresponding member. In 1973, she was elected an honorary member of the Austrian Academy of Sciences,[39] however, the German Academy of Scientists the *Leopoldina*, appointed Lesky as a full member on 24th November 1965.[40] In 1970, she was elected the "first female senator", a representative of her region.[41] Memberships and important positions in scientific organizations have always been an important part in the advancement of science and science communications. In 1979 Lesky applied for early retirement and moved to Innsbruck.[42] Hubenstorf, who was at this time a medical student, had the impression in personal conversations with Lesky, that she was not ready to support the university reform of 1975, which was essentially aimed at the co-determination and new organisation of the universities. Participation of students in university politics was unimaginable to Lesky.[43] "Behind me is nothing", she said to her assistant Manfred Skopec, before she left the institute.[44] Lesky "found it extremely difficult to part from the institution which she had

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[32] Thomas N. BURG, *Medizin in der Geschichte – ein Register österreichischer Forschung*, 2, online: http://randgaenge.net/wp-content/uploads/register.pdf (9th January 2010).

[33] University of Vienna, Institute for Contemporary History, Friedrich Stadler: http://www.univie.ac.at/zeitgeschichte/friedrich-stadler/ (14th January 2011)

[34] Erna LESKY, *Die Wiener medizinische Schule im 19. Jahrhundert* (= Studien zur Geschichte der Universität Wien 6, Graz-Köln 1965).

[35] Erna LESKY, *The Vienna Medical School of the 19th Century* (John Hopkins University Press, Baltimore, Md., 1976). See Ralph FRACKELTON, letter to Erna Lesky, 20th September 1969. In: Estate of Erna Lesky, handwriting-collection 4.700/32 (Institute for the History of Medicine, Medical University of Vienna): Frackelton, a young physician, came from Ohio to Vienna to research at the Institute for the History of Medicine, University of Vienna in 1969. In this letter he wanted to thank Lesky for the perfect management of the institute, but mainly for her scientific efforts, "Finally let me compliment you on your exhausting summary of the Second Wien[!] Medical School. No greater school has had a better historian to project it for further generations."

[36] SABLIK, Zum Gang der medizingeschichtlichen Forschung. In: Mensch. Wissenschaft. Magie. Mitteilungen. Österreichische Gesellschaft für Wissenschaftsgeschichte 20 (2000) 55.

[37] *Ibid*., 57

[38] BURG, *Medizin in der Geschichte*, 2.

[39] Cardinal KÖNIG, letter to Erna Lesky, 3rd June 1973. In: Estate of Erna Lesky, handwriting-collection 4.700,45 (Institute for the History of Medicine, Medical University of Vienna).

[40] BÖHLER, letter to Erna Lesky, 15th December 1965. In: Estate of Erna Lesky, handwriting-collection 4.700/8 (Institute for the History of Medicine, Medical University of Vienna).

[41] HUBENSTORF, Vom Erfolg und Tragik einer Medizinhistorikerin. In: MEINEL, RENNEBERG (edd.), *Geschlechterverhältnisse in Medizin, Naturwissenschaft und Technik*, 104.

[42] WYKLICKY, Erna Lesky. In: SKOPEC (ed.), *Institut für Geschichte der Medizin der Universität Wien*, Institute for the History of Medicine, University of Vienna, 56.

[43] HUBENSTORF, Vom Erfolg und Tragik einer Medizinhistorikerin. In: MEINEL, RENNEBERG (edd.), *Geschlechterverhältnisse in Medizin, Naturwissenschaft und Technik*, 104.

[44] Personal information, given by Lesky´s former assistant professor Manfred Skopec on 10th August 2007, Institute for the History of Medicine, Medical University of Vienna.

rebuilt so sucessfully after the Second Wold War and to hand over the reigns to a successor".[45] Because of her authoritarian leadership, professor Lesky was called by her assistants secretly "the general",[46] highlighting the 19th century prejudice that medical studies for women were "rather harmful, since it produces masculine characteristics".[47]

In 1985 Lesky attended the 200th anniversary of the Josephinum and showed the Austrian President, Rudolf Kirchschläger, once more the treasures of the Institute for the History of Medicine in Vienna. Helmut Wyklicky, her former associate professor and successor on the chair for the History of Medicine, Medical Faculty Vienna,[48] found it sad, "to see the lady [leaving] her former domain, mortally ill and lonely […]. She died in Innsbruck the following year".[49] In 1991, Salome Waelsch, professor of Genetics at the Albert Einstein College of Medicine, analysed women in their academic careers in medicine from the experience of half a century. Women were "more intuitive, more imaginative [and] more idealistic" than men in their access to the sciences.[50] They disposed an "extreme emphasis on causality, on one thing causing something else to happen". Referring to Waelsch, "Attention to causality" is not a purely masculine quality.[51] Out of her experience Waelsch noticed that women in medicine "are as competitive as men". "They were equal to men concerning competitiveness in the sciences".[52] Hence, for Regula Leemann neither the scientific culture nor the structures of the academic organisations were gender-neutral. Due to the high prestige, which professors of medicine and the doctor's profession had in society in general, they still operated "with gender-stereotypical social allocations" of the 19th century. A woman who tried to penetrate into the "social space", where men strengthen their power in historical tradition, was almost forced to act after their values. The "homo-social science culture of medical faculties accepted the other gender only slowly". This led to the result that "an equality of the careers of women and men was prevented or became at least complicated"[53]

Conclusion

This paper discussed whether the research-results of the psychologist Guido Strunk, Research Institute for Health Management and Health Economics at the Vienna University of Economics, are valid for Lesky, too. His provocative hypothesis is: "A woman must be a man to make a career".[54] Economists make a distinction between subjective and objective career success. The subjective success expresses itself in the satisfaction with the career and the acquired respect in the professional sphere, the so-called ascribed success. However, the objective career success becomes visible in the salary and the management responsibility as well as the number of the subordinated employees.[55] Lesky was a highly respected historian of medicine. She managed to succeed in a man's domain through diplomatic networking. Self-representation, performance, leadership motivation, and competitive orientation, all determined her career success. Her objective career success was, compared to medical professors, heading a clinic, small.

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[45] Karl HOLUBAR, Die Nach-Lesky Aera in den letzten beiden Dezennien des Jahrhunderts (1979–1999). In: SKOPEC (ed.), *Institut für Geschichte der Medizin*, 18–19, here 18.

[46] Personal information, given by Lesky´s former assistant professor Helmut Wyklicky in December 2002 at the Institute for the History of Medicine, Medical University of Vienna. He told me that he saw himself as "the porter of Lesky´s umbrella".

[47] Claudia HUERKAMP, *Bildungsbürgerinnen. Frauen im Studium und in akademischen Berufen 1900–1945* (= Bürgertum, Beiträge zur europäischen Gesellschaftsgeschichte 10, Göttingen 1996) 152.

[48] SABLIK, Zum Gang der medizingeschichtlichen Forschung. In: *Mensch. Wissenschaft. Magie. Mitteilungen. Österreichische Gesellschaft für Wissenschaftsgeschichte* 20 (2000) 53: Wyklicky headed the Institute for the History of Medicine after Lesky´s retirement from 1979 to 1989, the first two years provisionally.

[49] WYKLICKY, Erna Lesky. In: SKOPEC (ed.), *Institut für Geschichte der Medizin der Universität Wien*, Institute for the History of Medicine, University of Vienna, 56.

[50] Harriet ZUCKERMAN, Jonathan R. COLE, Interview with Salome Waelsch. In: Harriet Zuckerman, Jonathan R. Cole, John T. BRUER (ed.), *The Outer Circle. Women in the Scientific Community* (New York-London 1991) 71–93, here 72.

[51] *Ibid*., 74.

[52] *Ibid*., 76.

[53] Regula Julia LEEMANN, *Chancenungleichheiten im Wissenschaftssystem. Wie Geschlecht und soziale Herkunft Karrieren beeinflussen* (Zürich 2002) 39.

[54] Guido STRUNK, *Eine Frau muss ein Mann sein, um Karriere zu machen. Ergebnisse aus dem Vienna Career Panel Project (ViCaPP)*. Forschungsinstitut für Gesundheitsmanagement und Gesundheitsökonomie, Wirtschaftsuniversität Wien (Wien 2005) online: *http://www.meduniwien.ac.at/homepage/fileadmin/HP-Relaunch/pdforganisation/gleichbehandlung/ Veranstaltungen/Exzellenztagung_4.10.2010/Strunk_Eine_Frau_muss_ein_Mann_sein__um_Karriere zu_machen.pdf* (21st October 2010).

[55] Wolfgang MAYRHOFER, Michael SCHIFFINGER, Amanda DUNKEL, Michael MEYER, Spieglein, Spieglein an der Wand… Zum Verhältnis von objektiven und subjektiven Karriereerfolg. In: Wolfgang MAYRHOFER, Michael MEYER, Johannes STEYRER (edd.), *Macht? Erfolg? Reich? Glücklich? Einflussfaktoren auf Karrieren* (Wien 2005) 25–50, here 27.

Whether it is possible to realize a desired career depends on various factors, mainly on the personality structure and the motivation to reach the goal.[56] Factors of influence on career are the place of residence during childhood, the educational level and social layer of the parents and the social origin.[57] Focusing on Erna Lesky's career, the following factors become visible: Lesky spent her childhood in a small town with a rural environment. Her family belonged to the lower middle class. But because Lesky attended a Grammar school in a capital, she had the chance to use social mobility through education.

Further factors influencing Lesky's career were a very good conscientiousness and capacity for self-representation, high flexibility and achievement motivation. Her leadership motivation and competitive orientation were very good too, but Lesky showed only little interpersonal skills towards her assistents. She was not team orientated and did not have as much of an emotional stability as she pretended to.[58] As she grew up in the inter-war time, this might be understandable. Thus, for the career to succeed, the following factors were crucial for Lesky: She was very good in making friends and allies, in establishing and maintaining contacts. She understood how to emphasize her own skills and ideas.[59] To conclude, her career tactical behaviour was excellent.[60]

Comparing the careers of Jantsch and Lesky a discourse opens. Aurelija Novelskaite stated in her paper, given in 2006 at the Prague conference "Science policies meet reality", three different types of female access to an academic career. She discerned between "independent initiators, dependent followers and contended stayers". "Independent initiators" act and react actively. In interviews they told her: "I wanted to do it and I did that, you have to choose or I have just decided". They were highly motivated. "Dependent followers" and "contended stayers" act and react passively. They told Novelskaite: "One must be chosen, one should be patient and wait till her time comes or it happened in natural course". They thought they were "lucky".[61]

Jantsch applied to the strategy of dependent followers: Her research issue came from the field of Schönbauer. He conducted her research in the history of medicine. Jantsch was assigned to his research. She worked hard as well, but her main career-related decisions were taken by others. In contrary, Lesky followed the strategy of independent initiators. She knew that she would "get the chair, now or never". Independent initiators work hard continuously and take the main decisions independently. Lesky began her career, dependent on her husband, his research and his international relations, as a dependent follower. She became an independent initiator by her indomitable will.[62] However, nearly fifty years ago, she also had the possibility to stay in a leading position, because there was "the man behind", her husband Albin Lesky.[63]

References

ACKERKNECHT, Erwin. (1956), letter to Erna Lesky, 6th November 1956. In: Estate of Erna Lesky, handwriting-collection 4.700/5 (Institute for the History of Medicine, Medical University of Vienna).

BÖHLER, Lorenz. (1965), letter to Erna Lesky, 15th December 1965. In: Estate of Erna Lesky, handwriting-collection 4.700/8 (Institute for the History of Medicine, Medical University of Vienna).

BOLOGNESE-LEUCHTENMÜLLER, Birgit; HORN Sonja (edd.), (2000). *Töchter des Hippokrates. 100 Jahre akademische Ärztinnen in Österreich* (Wien).

BURG, Thomas N. *Medizin in der Geschichte – ein Register österreichischer Forschung*, 2, online *http://randgaenge.net/wp-content/uploads/register.pdf* (9th January 2010).

Divorce documents from Dr. Grete Lesky, 29th September 1938. Personal documents 1902-1981, 131.97.01 (Archives of the University of Vienna).

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[56] Guido STRUNK, Anett HERMANN, Susanne PRASCHAK, Eine Frau muss ein Mann sein, um Karriere zu machen. In: MAYRHOFER, MEYER, STEYRER (edd.), *Macht? Erfolg? Reich? Glücklich?* 211–242, here 229.

[57] *Ibid*., 230.

[58] See *ibid*.

[59] See *ibid*., 231.

[60] See *ibid*., 229.

[61] Aurelija NOVELSKAITE, The glass ceilings, the sticky floors or ...? Women's strategies of (ill) success in Lithuanian academic/scientific community. Institute for Social Research, Vilnius, Lithuania. In: *Science policies meet reality: Gender, Women and Youth in Science in Central and Eastern Europe, National Contact Centre* – Women and Science Institute of Sociology, Academy of Sciences of the Czech Republic, Prague, Dec 1-2, 2006. I would like to thank Aurelija Novelskaite for sending me the power-point-presentation of her paper, given in Prague.

[62] See *ibid*.

[63] Telephonic information, given by Karl Holubar on 14th November 2010. He headed the Institute for the History of Medicine, University of Vienna, after Wyklicky´s retirement from 1989 to 2000.

DIEPGEN Paul. (1949), letter to Erna Lesky, 8th November 1949. In: Estate of Erna Lesky, handwriting-collection 4.700/21 (Institute for the History of Medicine, Medical University of Vienna).

DIEPGEN Paul. (1951), letter to Erna Lesky, 22nd November 1951. In: Estate of Erna Lesky, handwritings-collection 4.700/21 (Institute for the History of Medicine, Medical University of Vienna).

FISCHER, Lisa. (1993), Weibliche Kreativität – oder warum assoziieren Männer Fäden mit Spinnen? In: NAUTZ, VAHRENKAMP (edd.), *Die Wiener Jahrhundertwende*, 144–158.

FRACKELTON, Ralph. (1969), letter to Erna Lesky, 20th September 1969. In: Estate of Erna Lesky, handwriting-collection 4.700/32 (Institute for the History of Medicine, Medical University of Vienna).

HOLUBAR, Karl. (1999), Die Nach-Lesky Aera in den letzten beiden Dezennien des Jahrhunderts (1979–1999). In: SKOPEC (ed.), *Institut für Geschichte der Medizin der Universität Wien. Institute for the History of Medicine, University of Vienna*, 18–19.

HORN, Sonja; DORFFNER, Gabriele. (2000), „... männliches Geschlecht ist für die Zulassung zur Habilitation nicht vorgesehen". Die ersten an der medizinischen Fakultät der Universität Wien habilitierten Frauen. In: BOLOGNESE-LEUCHTENMÜLLER, HORN (edd.), *Töchter des Hippokrates*, 117–138.

HUBENSTORF, Michael. (1996), Vom Erfolg und Tragik einer Medizinhistorikerin. Erna Lesky (1911–1986). In: MEINEL, RENNEBERG (edd.), *Geschlechterverhältnisse in Medizin, Naturwissenschaft und Technik*, 98–109.

HUERKAMP, Claudia. (1996), *Bildungsbürgerinnen. Frauen im Studium und in akademischen Berufen 1900–1945* (= Bürgertum, Beiträge zur europäischen Gesellschaftsgeschichte 10, Göttingen).

LEEMANN, Regula Julia. (2002), *Chancenungleichheiten im Wissenschaftssystem. Wie Geschlecht und soziale Herkunft Karrieren beeinflussen* (Zürich).

LESKY, Erna. (1950), *Die Zeugungs- und Vererbungslehren der Antike und ihr Nachwirken* (= Abhandlungen der Geistes- und Sozialwissenschaftlichen Klasse, Österreichische Akademie der Wissenschaften und der Literatur 19, Mainz 1951).

LESKY, Erna. (1955), *Staat und Heilkunde im Zeitalter des aufgeklärten Absolutismus in Österreich* (Dissertation Universität Wien).

LESKY, Erna. (1959), Österreichisches Gesundheitswesen im Zeitalter des aufgeklärten Absolutismus. In: *Österreichische Akademie der Wissenschaften, philosophisch-historische Klasse, Historische Kommission*, 122,1.

LESKY, Erna. (1965), *Die Wiener medizinische Schule im 19. Jahrhundert* (= Studien zur Geschichte der Universität Wien 6, Graz-Köln).

LESKY, Erna. (1976), *The Vienna Medical School of the 19th Century* (John Hopkins University Press, Baltimore, Md.).

MAYRHOFER, Wolfgang; MEYER, Michael; STEYRER, Johannes (edd.), (2005), *Macht? Erfolg? Reich? Glücklich? Einflussfaktoren auf Karrieren* (Wien).

MAYRHOFER, Wolfgang; SCHIFFINGER, Michael; DUNKEL, Amanda; MEYER, Michael. (2005), Spieglein, Spieglein an der Wand… Zum Verhältnis von objektiven und subjektiven Karriereerfolg. In: MAYRHOFER, MEYER, STEYRER (edd.), *Macht? Erfolg? Reich? Glücklich?* 25–50.

MEINEL, Christoph; RENNEBERG, Monika (edd.), (1996), *Geschlechterverhältnisse in Medizin, Naturwissenschaft und Technik. Im Auftrag des Vorstandes der Deutschen Gesellschaft für Geschichte der Medizin, Naturwissenschaft und Technik* (Bassum-Stuttgart).

NAUTZ, Jürgen; VAHRENKAMP, Richard (edd.), (1993), *Die Wiener Jahrhundertwende. Einflüsse – Umwelt – Wirkungen* (= Studien zu Politik und Verwaltung 46, Wien-Köln-Graz).

NEUGEBAUER, Wolfgang; SCHWARZ, Peter. (2005), *Der Wille zum aufrechten Gang. Offenlegung der Rolle des BSA bei der gesellschaftlichen Reintegration ehemaliger Nationalsozialisten, ed. Bund sozialdemokratischer AkademikerInnen, Intellektueller und KünstlerInnen* (Wien).

NOVELSKAITE, Aurelija. (2006), The glass ceilings, the sticky floors or ...? Women's strategies of (ill) success in Lithuanian academic/scientific community. Institute for Social Research, Vilnius, Lithuania. In: *Science policies meet reality: Gender, Women and Youth in Science in Central and Eastern Europe, National Contact*

Centre – Women and Science Institute of Sociology, Academy of Sciences of the Czech Republic, Prague, December 1st–2nd.

SABLIK, Karl. (2000), Zum Gang der medizingeschichtlichen Forschung in Wien seit 1945. In: *Mensch. Wissenschaft. Magie. Mitteilungen. Österreichische Gesellschaft für Wissenschaftsgeschichte* 20, 53–58.

SCHLEIERMACHER, Sabine. (1996), Ärztinnen im Kaiserreich: Ein Forschungsprojekt zur Geschlechtergeschichte. In: MEINEL, RENNEBERG (edd.), *Geschlechterverhältnisse in Medizin, Naturwissenschaft und Technik*, 217–224.

SCHÖNBAUER, Leopold. (1944), *Das medizinische Wien. Geschichte, Werden, Würdigung* (Berlin-Wien).

SKOPEC, Manfred (ed.), (1999), *Institut für Geschichte der Medizin. Gewidmet dem Angedenken an Max Neuburger, Gründer des Instituts für Geschichte der Medizin der Universität Wien. Institute for the History of Medicine, University of Vienna. Dedicated to the memory of Max Neuburger, founder of the Institute for the History of Medicine*, University of Vienna (Wien).

STEYRER, Johannes; MAYRHOFER, Wolfgang; MEYER, Michael. (2005), Karrieren. Eine Einführung. In: MAYRHOFER, MEYER, STEYRER (edd.), *Macht? Erfolg? Reich? Glücklich?* 12–24.

STRUNK, Guido. (2005), *Eine Frau muss ein Mann sein, um Karriere zu machen. Ergebnisse aus dem Vienna Career Panel Project (ViCaPP)*. Forschungsinstitut für Gesundheitsmanagement und Gesundheitsökonomie, Wirtschaftsuniversität Wien (Wien) online: http://www.meduniwien.ac.at/homepage/fileadmin/HP-Relaunch/pdforganisation/gleichbehandlung/Veranstaltungen/Exzellenztagung_4.10.2010/Strunk_Eine_Frau_muss_ein_Mann_sein__um_Karriere_zu_machen.pdf (21st October 2010).

STRUNK, Guido; STEYRER, Johannes. (2005), Dem Tüchtigen ist die Welt nicht stumm. Es ist alles eine Frage der Persönlichkeit. In: MAYRHOFER, MEYER, STEYRER (edd.), *Macht? Erfolg? Reich? Glücklich?* 51–77.

STRUNK, Guido; HERMANN, Anett; PRASCHAK, Susanne. (2005), Eine Frau muss ein Mann sein, um Karriere zu machen. In: MAYRHOFER, MEYER, STEYRER (edd.), *Macht? Erfolg? Reich? Glücklich?* 211–242.

VOSWINCKEL, Peter. (1996), Frauen-Kontingent im Biographischen Lexikon hervorragender Ärzte von 1933. In: MEINEL, RENNEBERG (edd.), *Geschlechterverhältnisse in Medizin, Naturwissenschaft und Technik*, 225–236.

WYKLICKY, Helmut. (1999), Erna Lesky. In: SKOPEC (ed.), *Institut für Geschichte der Medizin der Universität Wien. Institute for the History of Medicine, University of Vienna*, 55–56.

ZUCKERMAN, Harriet; COLE, Jonathan R.; BRUER, John T. (edd.), (1991), *The Outer Circle. Women in the Scientific Community* (New York-London).

ZUCKERMAN, Harriet; COLE, Jonathan R. (1991), Interview with Salome Waelsch. In: ZUCKERMAN, COLE, BRUER (edd.), *The Outer Circle*, 71–93.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 217.

HOW COULD RUSSIAN INTELLECTUAL WOMEN CONTRIBUTE TO AGRICULTURE? –CIRCULATION OF IDEAS IN EDUCATION AND CAREER DEVELOPMENT OF WOMEN AGRONOMISTS, LATE 19TH - EARLY 20TH CENTURY

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Abstract

In the second half of the 19th century, Russia, compared to the other European countries and the USA, faced a rather disturbing situation regarding women high education and career opportunities. Universities as well as agricultural institutes were still forbidden for women; a few specialized institutes prepared mostly 'educated housewives', and only later opened up for future physicians and teachers. There were a few women enthusiasts who managed to receive special permission from both the Ministry of Agriculture and the Ministry of Education to enter Moscow Agricultural institute. Pioneers of the movement Mariya S. Bevad and Anna K. Kosto-Sudakovich became the first Russian women agronomic scientists.

In the end of the 19th century, the great hope for Russian women with passion for agricultural science became the establishment of an Association (Society) for the Advancement of Women's Agricultural Education (AAWAE). The AAWAE was set up in 1889 by men of liberal views, led by professor of agricultural science Ivan A. Stebut. This association became the first in Russia in the field of agriculture to give women full membership (in other associations women were only allowed to attend public lectures and entertainments). One of the most active female members was Nadezhda P. Dolgova, who lectured extensively on the development of women's agricultural education. She travelled around Europe, met her women colleagues to get information on the subject, and wrote a lot of articles and books on agricultural education abroad. As a result of the AAWAE activity, High Agricultural Courses for Women were set up in St. Petersburg (1904), Moscow (1908), and other cities. In the 1910s about 400 women graduated from these institutions every year to become agronomists and agricultural scientists.

The presented paper examines for the first time the circulation of ideas and the formation of networks in the field of women agricultural education and career opportunities in Russia.

Gender asymmetry marked the development of science and education until the second part of the 19th century. It was especially pronounced in Russia. Compared to the other European countries and the USA, Russia faced disturbing situation regarding women's high education and career opportunities, which became critical in the agricultural science.[1]

Universities as well as agricultural institutes were forbidden for women; few specialized institutes prepared mostly 'educated housewives', and only in late 19th century opened up for future physicians and teachers. There were some women enthusiasts who managed to receive education abroad, mostly in Switzerland. For example, in the 1870s over 100 Russian women registered for medical study in Swiss universities and Paris.[2] These early pioneers, many of whom formed enduring friendships with their West European colleagues, became the leaders of Russian *women's liberation movement* and opened the doors to the acceptance of women as fellow students along with men into high school environment.

Attending lectures in agronomy for Russian women met extreme obstacles both of psychological and bureaucratic character.

In spite of the state's status as a *country of peasants and agriculture*, in mid 19th century Russian agronomy was not considered as a prestigious area of education and scientific career for nobility who dominated universities. It was true for both male and female discourses. Things changed after abolition of serfdom and other modernizing improvements in the period of the Great Reforms. In the 1860s - 1870s young generation of *raznochintzy* –intellectuals of non-aristocratic descent, usually from merchants and clerics– came to high school. They established a *fashion* for so-called *going to peasants* (practical enlightenment work in the countryside) and developed profound interest in natural sciences' and agronomy education. These ideas attracted many young women, mostly from aristocracy. Thus the circulation of education and career ideas in Russia took place not only in gender space; these ideas were also transferred between social clusters.[3]

However, young ladies' passion for agriculture met uncompromising protest from their noble families. While a decision to attend university lectures was a disappointment for parents (who traditionally regarded daughter's future only in terms of marriage), intention to become an agronomist disgusted, led to family crises and resulted in dramatic severance, fictive marriages to avoid ruined reputation, etc.

Bureaucratic obstacles looked even more serious. Career of an agronomist was traditionally regarded as an exclusively male occupation. Spending half a year doing field experiments and improving peasant farming in Russian backward countryside seemed physically inappropriate for women. This stereotype was changing very slowly especially in bureaucratic circles. That's why for a female candidate to enter the agricultural high school demanded first to apply for a special permission from the Ministry of Agriculture (which could be obtained only through informal channels with assistance of high-ranking relatives etc.); for some schools the permission from the Ministry of Education was also necessary. In case of a positive resolution ("on the judgment of the institute's management") the second step was to persuade the director, the dean, and finally individual professors to give permission to attend their courses.[4]

These insuperable barriers explain why just few women enthusiasts managed to clear a hurdle. I found only two files of the female-students in the archive of Moscow Agricultural Institute for the 19th century. As their files tell us, they received special permission personally from the minister of agriculture Aleksey S. Ermolov (who was known for liberal perception of the question[5]) and entered Moscow Agricultural Institute in late 1890s. These pioneers were Mariya S. Bevad and Anna K. Kosto-Sudakovich who became the first Russian women agronomists. Unfortunately I couldn't find any traces of their further careers in agronomy.

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[1] For the detailed discussion, see: Watts, R.. *Women in Science. A Social and Cultural History*. London: Routledge, 2007. P. 128. See also Bonner, Th. N. *To the Ends of the Earth. Women's Search for Education in Medicine*. Cambridge (Mass.): Harvard Univ. Press, 1992. PP. 31–37, 49–54. For discussion of Russian situation, see: Yukina, I. *Russian women's liberation movement*. S. Peterburg: Aleteya, 2007; Koblitz, A. *Science, Women and Revolution in Russia*. Amsterdam: Harwood Academic Publishers, 2000.

[2] See: Ivanov, A. E. *Vysshaya shkola v Rosii v kontse XIX – nachale XX veka*. Moscow: Nauka, 1991; *idem*, *Studenchestvo v Rossii. Sotsial'no-istoricheskaya sud'ba*. Moscow: Nauka, 1999.

[3] For the situation in Russian agriculture and agronomic education, see: Elina, O. Yu. "Planting Seeds for the Revolution: Emergence of the Russian Agricultural Science". *Science in Context*. 2002. 15 (2), pp. 209–237; Joravsky, D. *The Lysenko Affair.* Chicago & London: Univ. of Chicago Press, 1986; McClelland, J.C. *Autocrats and Academics: Education, Culture and Society in Tzarist Russia.* Chicago & London: Univ. of Chicago Press, 1979; Robinson, G.T. *Rural Russia under the Old Regime. A History of the Landlord-Peasant World and a Prologue to the Peasant Revolution of 1917.* Berkeley & Los Angeles: Univ. of California Press, 1967; Vucinich, A. *Science in Russian Culture, 1861–1917.* Stanford (Cal.): Stanford Univ. Press, 1970.

[4] Shimkov, A.P. *Agricultural Institute*, 1904–1907. Moscow, 1916.

[5] See, for example, his paper: Ermolov, A.S. "Enlargement of Women Educational Rights" // *Izvestiya.* 1897. № 23.

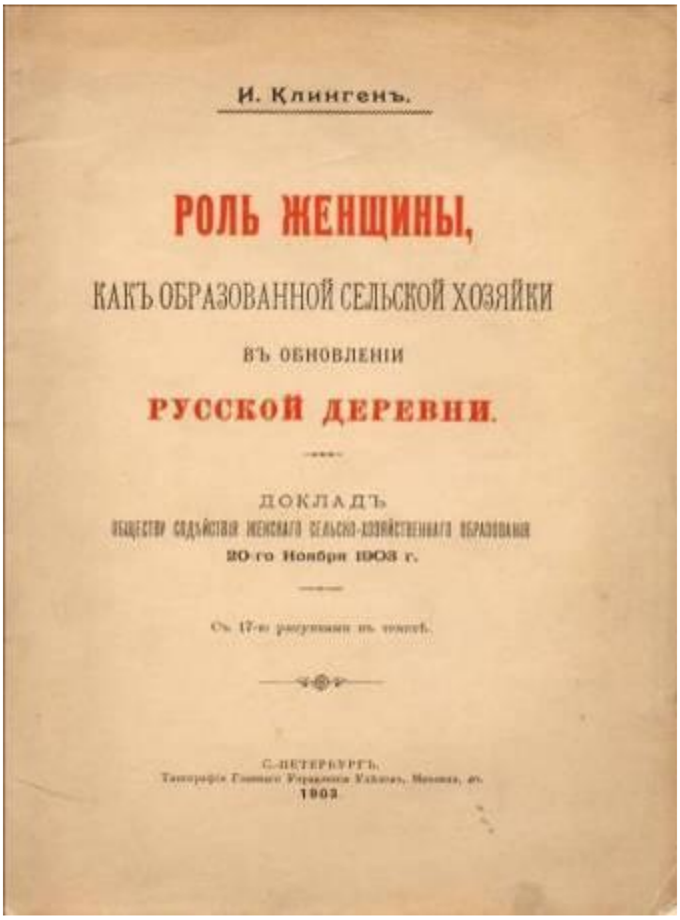
In the end of the 19th century agricultural science began growing in prestige, mostly due to its promotion by Russian agricultural scientific societies. And due to activities of *zemstvo* –local public self-governed administrations– which established numerous agricultural experiment stations and supported agronomists in provinces.

At the same period the great hope for Russian women became the establishment of an Association for the Advancement of Women's Agricultural Education (AAWAE) –a forum for discussing and lobbing on an education reform. The AAWAE was founded in 1899 and was led by the renowned professor of agriculture Ivan A. Stebut.

During his long career in agricultural science, Stebut published a number of articles and books on the problem of agricultural education for women in Russia. His writings directly influenced the formation of the association. In 1891 at the Second Conference for the Development of Technical Education, Stebut presented a paper, in which he argued the necessity to set up a special body for assistance to agricultural education of women. The paper was published immediately in a special issue of a popular weekly *Russkie Vedomosty* (Russian Gazette) entitled "Agricultural Education for Women, its Significance, and Means of its Realization".[6]

Stebut's ideas were echoed by other male professors of liberal views who also wished Russian women to enter the sphere of education and career in agriculture. Among them were Alexey F. Fortunatov, Dmitriy N. Pryanishnikov, and also Boris N. Klingen, who published intensively on the question; the citation from his books I use as a *key question* in the subtitle of my paper.[7]

Fig. 1: Ivan A. Stebut (1833–1823), professor of agriculture, Moscow Agricultural Institute, Head of the Scientific Committee of the Ministry of Agriculture.



И. Клингенъ.

РОЛЬ ЖЕНЩИНЫ,

КАКЪ ОБРАЗОВАННОЙ СЕЛЬСКОЙ ХОЗЯЙКИ

ВЪ ОБНОВЛЕНИИ

РУССКОЙ ДЕРЕВНИ.

ДОКЛАДЪ

20-го Ноября 1903 г.

С.-ПЕТЕРБУРГЪ.

1903

Fig. 2: Ivan Klingen: *The Role of female educated member of agricultural community in the modernization of Russian countryside*. St. Petersburg, 1903.

6 See, for example: Stebut, I. A. "Agricultural Education for Women, its Significance, and Means of its Realization" // *Russkie Vedomost* . 1891. № 23; *idem*, "If Russian Intellectual Women need in Special Agricultural Education?" //*Izbran. Soch.* Vol. II. Moscow: Sel'khozgiz, 1967. P. 586–614.

7 Klingen, I. *The Role of female educated member of agricultural community in the modernization of Russian countryside*. St. Petersburg, 1903.

However important the male initiative was in the creation of the AAWAE, the major role in its activities was played by female members. The Council of the Association was formed exclusively of women. The most active female members were agronomist Nadezhda P. Dolgova, physician Anna K. Malyarevskaya, and also leaders of Russian *women's liberation movement* Anna P. Phylosophova and Varvara P. Tarnovskaya.

The celebrated example of a woman challenging Russian women's inferior status in education was Anna P. Phylosophova. As her colleague A. Tyrkova wrote, "she was respected for all that she had consistently done for the diffusion of ideas of women's education –from supporting young girls who decided to enter the universities to creation of famous Besstuzhevskiye High Courses for Women in St. Petersburg".[8] In spite of her age (she was over 60 at that time), Phylosophova became a powerful figure in the AAWAE providing it with organizational knowledge and financial support (Phylosophova belonged to Russian aristocracy and many *decision makers* and financial *bigwigs* –high rank bureaucrats, members of the Tsar's family– were her close friends).

Fig. 3: Anna P. Phylosophova (1837–1912), an activist of women liberation movement, one of the founders of Besstuzhevskiye High Courses for Women in St. Petersburg.

The intellectual centrepiece of the Association was Nadezhda P. Dolgova. She was the only Russian female author who wrote intensively on women's agricultural education abroad.[9] Dolgova graduated from Geneva University and spent some time travelling Europe where she met her colleagues and gathered information on the subject. She also attended the

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[8] Tyrkova, A. V. Pamyati Anny Pavlovny Phylosophovoi // *A.P. Phylosophova i ee vremya*. Petrograd, 1914. P. 35.

[9] See, for example: Dolgova, N. P. *Zhenskoe sel'skokhozyaistvennoe obrazovanie v Zapadnoi Evrope I Amerike*. S.Peterburg, 1905.

First World Women's Congress at the World Fair in Chicago in 1893. Later she returned to Russia and lectured extensively on the development of women's agricultural education in the country.

The Association supported private agricultural schools for girls which were set up in the 1880s .- 1890s mostly by women-landowners in their private estates. The most interesting example was Preobrazhenskaya School in Chernigov province founded by Anna N. Neplyueva and her daughters. Such a school enabled girls (from peasant families mostly) to get primary education and special knowledge and practice in agricultural subjects like farming, horticulture, husbandry. Some of Neplyueva's students tended to continue education in gymnasia and later in high school financed and encouraged by the patroness.[10]

The key result of the AAWAE activity, however, was getting a permission to create the first private high school in agriculture. In 1904 High Agricultural Courses for Women were set up in St. Petersburg, named after Stebut next year.

In 1908 Moscow High Courses for Women started their teaching activity due to patronage of Countess S.K. Golitsina. The institution was headed by Stebut's pupil Dmitri N. Pryanishnikov; other professors were renowned Russian scientists A.F. Fortunatov, G.M. Turskiy, A.G. Doyarenko, N.I. Vavilov. Later the same was done in Saratov (1913), Novocherkassk (1916) and other places. There also appeared agricultural high schools for both genders (Petersburg Agricultural Courses, 1906). All these institutions were non-governmental, i.e. created by individual patrons or due to public assistance.

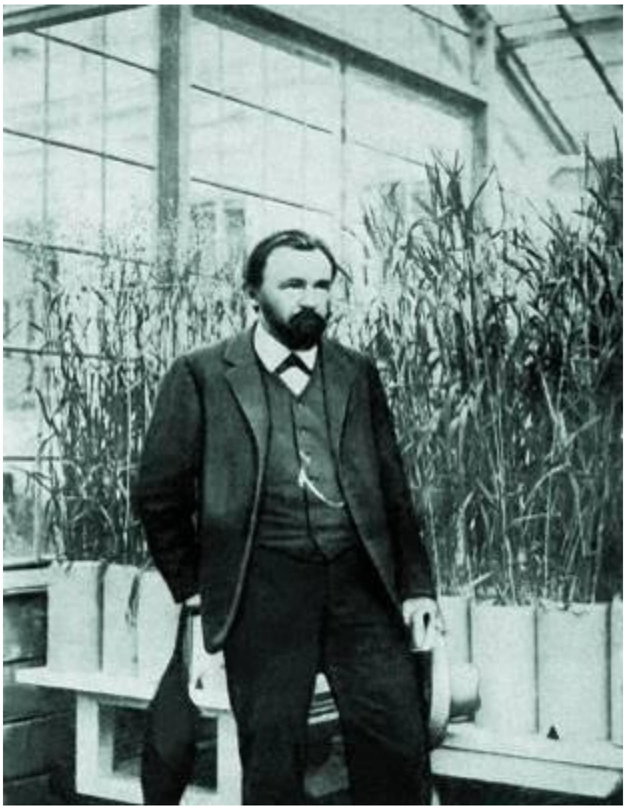

Fig. 4: Dmitriy N. Pryanishnikov (1865–1948), professor of agriculture, director of Moscow High Agricultural Courses for Women.

Students for the courses were recruited mostly from families of merchants, or clerics; many of them were daughters of university and agricultural schools professors who belonged to impoverished estate gentry or personal gentry. There was no disagreement in their families concerning young ladies' future occupation. Female agronomic students were emancipated persons; some of them looked rather provocative cutting their hair and refusing to wear traditional women's hats.[11]

There were some women among the professors of the courses. I would like to mention two of them that lectured at Stebut Courses. The first one was Nadezhda P. Dolgova, who became a deputy director of the Courses. The second one

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[10] Ustav Preobrazhenskoi nizshei sel'skokhozyaistvennoi shkoly 2 razryada v imenii Neplyueva. S.Peterburg, 1895.

[11] See a discussion on the question in: Yukina, I. *Russian women's liberation movement*. S. Peterburg: Aleteya, 2007.

was a graduate of the courses Sofia L. Ivanova, who was appointed to hold summer practical course in plant breeding –a new discipline for Russian agricultural science which demanded from a professor profound knowledge and sophisticated training. Male professors were agronomist V.E. Brunst, meteorologist P.S. Brounov, chemist G.N. Bong, plant breeder N.R. Nedokuchaev, and zoologist S.S. Averintsev.

Fig. 5: Ivan A. Stebut (on the steps, stands left) and Nadezhda P. Dolgova (next to Stebut) after the ceremony of the opening of Agricultural Courses, September 1904.

The number of female students was growing every year. The dynamics of students' numbers at St. Petersburg Agricultural Courses for Women is: 1907/08 – 196 students; 1913/14 – 525; 1917 – 1200.

The statistics for total number of female students for all schools was even more impressing. For ten years their number rose in more than ten times: in 1907/08 – 258 students; 1913/14 – 1150; in 1917 – 2773, or ten times as much. However, this considerable number looks rather modest, even poor in comparison with a number of male students; in percentage these numbers are 3.5 % for women and 96.5% for men. Meanwhile, in 1910s from 400 to 500 women graduated from agricultural high schools every year with a diploma to become agronomists and agricultural scientists.[12]

It goes without saying that field summer practices at specialized agricultural stations were of crucial importance for future agronomists. But it was rather difficult for newly established schools (always restricted in money) to find additional funding for organizing their own experimental plots. Only Stebut Courses managed to set up their private station.

In other cases it was only the assistance by state high schools and agricultural experiment stations (which were for granted *male institutions*) that helped to settle the problem. For example, students of Moscow High Courses for Women were having their summer practical courses at Butyrskaya experimental farm of Moscow Agricultural Institute and at some *zemstvo* agricultural experiment stations of the Central Region.

Naturally female graduates of the courses were invited to serve as agronomists and researchers at these stations (for example, at Mysovo station set up by mutual patronage of Moscow *zemstvo* and the government).

Usually they occupied not very high positions and could not pretend to rise further than head of laboratory (department). It is interesting that even in Soviet Russia, remarkable for its high-ranked women, in natural sciences, education, diplomacy, etc. at least until the Second World War female agronomists still belonged mostly to assisting scientific staff. This perhaps could be explained by traditional perception of occupation in agronomy as a *male* one.

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[12] *Spravochnik po zhenskomu sel'skokhozyaistvennomu obrazovaniyu.* S. Peterburg, 1912; [n.a.] [n.n.] // Tekhicheskoe I kommercheskoe obrazovanie. 1916. № 2. С. 40–42; *Spravochnik po zhenskomu sel'skokhozyaistvennomu obrazovaniyu.* S. Peterburg, 1917; Ivanov, A. E. *Vysshaya shkola v Rosii v kontse XIX – nachale XX veka*. Moscow: Nauka, 1991.

Fig. 6: Students of Moscow High Agricultural Courses for Women.
Summer field lessons at Butyrskaya experimental farm, July-August 1910.

To conclude, I would like to stress that ideas of *entering women into agriculture*, or *necessity of agriculture for women*, circulated both in international and Russian national gender spaces; these ideas were as well transferred between social clusters – from *raznochintsy* to *aristocracy*. My argument is also that private patronage and public assistance became the key element in the development of female agricultural education. In spite of rapid growth of number of female students in agronomy, the career opportunities for women agronomists for years remained rather modest.

So in the presented paper I tried to examine the development of networks and the formation of institutions in the field of women agricultural education and career opportunities in Russia.

Archives consulted

Central State Historical Archive of Moscow (TsGIAM): *Collection of Moscow Agricultural Institute; Collection of Moscow High Agricultural Courses for Women*.

Russian State Archive of Economy (RGAE): *Collection of Moscow Agricultural Institute*.

Russian State Historical Archive (RGIA): *Collection of the AAWAE; Collection of Ministry of Agriculture; Collection of A.I. Stebut High Agricultural Courses for Women in St. Petersburg*.

Private archive of V.P. Velikanova's family.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 224.

INTERNATIONAL NETWORKS FOR SUPPORTING SCIENTIFIC CAREERS OF WOMEN IN SPAIN, IN THE FIRST THIRD OF THE 20TH CENTURY

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Abstract

In the early twentieth century, mobility for scientific training was not easy, let alone for Spanish women, who only gained access to university on an equal basis as men in 1910. The Board for Advanced Studies and Scientific Research (JAE in its Spanish acronym), the institution charged with the Spanish scientific policies since 1907 until the Spanish Civil War (1936-1939), played a key role in the careers of Spanish scientists by awarding them fellowships to travel and stays abroad. For careers of Spanish women scientists, besides the JAE, some networks of international solidarity for women were important too.

Through trajectories of Spanish women scientists who went abroad and of some foreigners women who worked in Spain, particularly in Physics, Chemistry and Biology, it is highlighted, first, the welcoming attitude of the Spanish republican institutions, and secondly, the benefits obtained by women from the international women's organizations engaged in promoting scientific training for women.

Between 1907 and 1936, at least 30 women enjoyed the opportunity to broaden their studies or research abroad. Two came to Spain from other countries, while 28 Spaniards received scholarships to the United States (12), Germany (8), France (6), England (4), Switzerland (3), Belgium (3), Austria (1), Denmark (1) and Italy (1).

This support was crucial for the inclusion of Spanish women in the emerging scientific community in Spain, unfortunately miscarried by the wars in Spain and Europe.

Introduction: the relationship between US women and Spanish women

In September 1921, a newspaper published in New York and written in Spanish brought the following news: "A brilliant group of Spanish ladies will join the American university course". What had happened?

What had happened was that four Spanish women who had received a scholarship in order to specialize in various scientific matters had come to the United States. Their names were: Carmen Castilla, Concepción Lazarraga, María Luisa Cañomeras and Loreto Tapia. Their scholarships for the course 1921-1922 were the first to be granted under the agreement signed by the Spanish Ministry of Education and Martha Carey Thomas, who was acting on behalf of the American Association of University Women (AAUW).

Carmen Castilla went to Smith College for further studies in Science education; Conception Lazarraga to Barnard College for studies in Chemistry as did Mª Luisa Cañomeras; and Loreto Tapia went to Bryn Mawr. Next to them were traveling Herminia Rodriguez (graduate in Pharmacy and Chemistry) and Nieves González Barrios (Ph.D. in Medicine from the University of Salamanca), who was intended to study bacteriology and childhood diseases in a prestigious clinic, supported by a pension from the JAE.

M. Carey Thomas, recognized as one of the most important leaders for education of women in the United States, was the president of Bryn Mawr College in Philadelphia (Pennsylvania, USA) and a member of the International Relations Committee of the Association of Collegiate Alumnae (ACA), organization born in 1881 for advancing educational and professional opportunities for women in the USA and in the rest of the countries. In 1919, the ACA grouped more than ten thousand scholars, among which were included presidents, deans, directors and professors from all U.S. states and Canada. The ACA changed its name to AAUW in 1921.

The first connection with American women's associations had begun with the establishment of the "International Institute for Girls in Spain" (IIGS), created in 1877 as a girls' school in Santander, Spain, which moved in 1903 to Madrid, where it was known as the "International Institute" or "Boston Institute".

The American-Spanish relation intensified with the foundation of the *Residence for Ladies* (the "Residencia de Señoritas"), an institution for women students founded in Madrid, in 1915, directed by Maria de Maeztu. The Residence and the IIGS were physically close and shared its main educational orientations. This closeness led to set exchanges with the American women's college who supported the IIGS.

Some clues on the situation of women in Spain at the beginning of the 20th century

The Spanish economic reality of the late nineteenth and early twentieth century was characterized by the predominance of a rural society, low levels of industrialization and a structural weakness of the middle classes. At the ideological level, the exorbitant influence of the Catholic religious ideas in Spanish society was notorious, and at the political level, there was a sharp polarization between conservative and liberal positions. The improperly called universal suffrage, widespread voting for men, was reached in Spain in 1868 and will become truly universal for men and women in the Second Republic in 1931.

The Spanish universities and high schools were first open to women in 1868, as part of the political revolution which occurred in September that year, which would imply a series of liberalizing laws, among others, that of *Freedom of Education*, that of *Freedom of Religion* and the *Universal Male suffrage*. In those years, Spanish law was among the pioneers in recognizing the right of women to receive higher education. Later, however, conservative governments backed down to size.

Obtaining degrees and the practice of the profession were two difficulties for the first women scholars. In our country, the professions will be one of the problems. Concepción Arenal, one of the most enlightened voices of the moment, in her book *The Woman of the future*, wrote about the contradiction posed by the fact that in Spain a woman could be Queen and tobacconist, but she could not practice any intermediate profession.

In 1900, 66% of Spanish women and 45.3% of Spanish men were illiterate. They lacked the means to pay tuition fees, centred as they were on tasks of survival. In those days the *Normal Schools for Teachers* were the most popular channel for women to achieve an education and to exercise a profession.

In Spain, the law that allowed women the free access to university was passed in 1910. That year, 21 women were studying at Spanish university among 15.000 men, what then meant a short 0.1%.

Liberal groups and Krausists, through the *Free Institution of Teaching* (ILE in its Spanish acronym) and the *School Institute* ("Instituto Escuela") constituted the nucleus in favour of changing the traditional conception of education for women.

Institutional mechanisms and Networks from which women benefited

The institutional mechanisms and networks from which some Spanish and international women scientists benefited (obtaining support for travelling and studying abroad), between 1907 and 1936, can be classified in three categories:

- Institutional mechanisms: The scientific politics of JAE (1907-1936)
- Networks of women's organizations
  - Fellowships given by the ACA and the International Association of University Women (IAUW)

    - The interchange established through the relationship between the International Institute for Girls in Spain and the Residence for Ladies (Residencia de Srtas.)
- Political international networks
    - Zionists networks
    - Personal and familiar networks

1. Institutional mechanisms: The scientific politics of JAE

Since 1907, the JAE was the institution responsible for articulating the scientific policy in Spain. Particularly, they were responsible of everything related to scholarships for study abroad. From the outset it established a systematic program of sending researchers, faculty and graduates abroad. The Founding Act of the JAE affirms that to promote scientific research, connection with the "scientific and educational movement of the most cultured nations"[1] is essential. And to achieve this purpose was imperative to promote and support a fellowships program for stays abroad.

The policy of the JAE was characterized by the following features: an open mind without rigidities nor bureaucratic red tape, a special attention to people with professional prestige, a prioritization of the issues that met the needs of the country, support to the current research topics and special interest, a respect for the reports of expert committees, the continuing support for the renewal of teaching and flexibility to adapt to increases and decreases in budget and to the political changes occurred over the years.[2] To these features it could be added a positive sensitivity for to not discriminate against women in the provision of fellowships.

The JAE was integrated by men (only a woman, Maria de Maeztu, belonged to it) of the Free Institution of Education, liberal thinkers as Giner, Cossio, Luis de Zulueta or Castillejo. They maintained a favourable disposition towards the promotion of women in science. Example of this progressive spirit is that throughout its existence (1907-1938) the JAE was increasing the rate of fellowships awarded to women (see table 1), from a 4% in the 1910s decade to a 13% in the 1930's.

Table 1: Number of fellowships for women, conceded by the JAE[3].

Period	**Nº of fellowships for women**	**Total number of fellowships**	**% Fellowships for Women/ Total**
1908-1919	27	645	4
1920-1929	41	539	8
1930-1934	53	410	13

In the first third of the 20th century, the number of women who received a fellowship from JAE, for scientific studies abroad, was 18. The fellowships received by those women were 20[4].

The name of women scientists, their degrees, the subject of study and place of destination, of those who received a fellowship in the fields of Physics and Chemistry are included in table 2. The same information for the field of Natural Sciences is included in table 3. The same information for the field of Medicine is included in table 4.

[1] Royal Decree of January 11, 1907.

[2] MARIN ECED, Teresa (1990) *La renovación pedagógica en España (1907-1936)*. Madrid, CSIC, pp. 87-89.

[3] The total number of pensions, included in the table, has been taken from MARIN ECED, 1990, *Op. Cit.*, p. 88. The pensions given to women, have been taken from CAPEL, Rosa María (1986), *El trabajo y la educación de la mujer en España (1900-1930)*. Madrid, Ministerio de Cultura, pp. 569-581.

[4] The data included in tables 2, 3 and 4 were taken from the Archive of the JAE, accessible at the following website: http://archivojae.edaddeplata.org/jae_app/

Table 2: Physics and Chemistry.

Name	Degree	Destination – Subject
Martina CASIANO MAYOR	Teacher at Teaching Training School	GERMANY, Leipzig, 1912-1913 – Chemistry
Luisa CRUCES MATESANZ	Teacher and graduated in Pharmacy	GERMANY AND FRANCE, 1912 – Phisics and Chemistry BELGIUM, 1927 – Agricultural chemistry
Concepción ESPESO GONZÁLEZ	Doctor in Chemistry	FRANCE, 1926 – Organic chemistry
Dorotea BARNÉS GONZÁLEZ	Doctor in Chemistry	USA, Smith College, 1929 – Spectroscopy AUSTRIA, 1932 – Raman Spectroscopy
Rosa HERRERA MONTENEGRO	Graduated in pharmacy and in Natural Sciences	ENGLAND, London, 1929 FRANCE and SWITZERLAND, 1930 – Chemical analysis
Pilar de MADARIAGA ROJO	Graduated in Chemistry	USA, Columbia Univ., 1930-31, 1931-32 – Spectroscopy
Jenara Vicenta ARNAL YARZA	Dr. in Chemistry	SWITZERLAND, 1930 GERMANY, 1931 – Electrochemistry
Felisa MARTÍN BRAVO	Doctor in Physics	ENGLAND, Cambridge, 1932 – Meteorology
Teresa SALAZAR BERMÚDEZ	Doctor in Chemistry	FRANCE, 1934 – Chemistry
Piedad de la CIERVA VIUDES	Doctor in Chemistry	DENMARK, 1935 – Physics

Table 3: Natural Sciences.

Name	Degree	Destination – Subject
Dolores CEBRIÁN VILLEGAS	Teacher at Teaching Training School	FRANCE, Paris, 1912 Fontainebleau Lab. Citology, 1913 – Teaching of Natural Sciences
Margarita COMAS CAMPS	Teacher at Teaching Training School	ENGLAND, Belford College, 1920 FRANCE, Lab. d'Evolution des Êtres Organisés, 1926 – Zoology

Table 4: Medicine.

Name	Degree	Destination – Subject
Herminia RODRÍGUEZ	Graduated in Pharmacy and Chemistry	USA, 1921 USA, Trinity College (WAS), 1922 – Bacteriology
Mª Nieves GONZÁLEZ BARRIO	Doctor in Medicine	USA, 1921 USA, The College of Saint Teresa (MIN), 1922
Jimena FERNÁNDEZ DE LA VEGA	Doctor in Medicine	GERMANY, AUSTRIA, 1923-27 ITALY, 1933 – Genetics
Josefa BARBA GOSÉ	Graduated in Pharmacy	ENGLAND, London, 1928
Isabel TORRES SALAS	Graduated in Pharmacy	GERMANY, 1934, – Vitamins
Luisa BELTRÁN LOGROÑO	Graduated in Pharmacy	SWITZERLAND, 1934 – Food analysis

The figures of women who went abroad for scientific training are higher if we consider other ways of obtaining fellowships that benefited women and are specified below.

2. Networks of women's organization

In the first third of the 20th century, in Spain, women also benefited of the fellowships given by women's associations, which established networks for supporting high female education around the world.

Two were the ways by which women could obtain the necessary support for implementing their careers:

- Fellowships given by the ACA-AAUW and the International Association of University Women (IAUW).
- The interchange established through the relationship between the *International Institute for Girls in Spain* and the *Residence for Ladies* ("Residencia de Señoritas")

As it has been said before, on behalf of the AAUW, Martha Carey Thomas, visited Madrid and offered the Spanish government the possibility of exchanges for young post-graduated women. An agreement was put in place to facilitate scientific studies abroad for many Spanish women. These scholarships were aimed at those women student who, having studied at university, wished to pursue specialized courses in American universities. The total amount covered the costs of accommodation and teaching but not travel expenses. At the time this proposal was being studied, an exchange program already existed for fellows; it came out of the relationship between the *Residence for Ladies* and the *International Institute for Girls*.

This mechanism explains the significant growth of women traveling to the USA. The number of women who received a fellowship from these networks was 12. The fellowships received by those women were 14[5].

The name of women scientists, their degrees, their subject of study and place of destination, of those who received a fellowship this way, in the fields of Physics, Chemistry, Natural Sciences and Medicine, in the 1920s decade are included in table 5. The same, in the 1920s/1930s decade are included in table 6. The same, in the decade of 1930s are included in table 7.

[5] The data included in tables 5, 6 and 7 were taken from: Carmen MAGALLON (2004) *Pioneras españolas en las ciencias*, Madrid, CSIC, and from the mentioned Archive of the JAE.

Table 5: Fellowships from women's organizations, 1920s.

Name	**Degree**	**Destination – Subject**
Carmen CASTILLA	Higher Teacher and Inspector	USA, Smith College, Northampton, MA, 1921-1922 – Teaching of Sciences
Concepción LAZARRAGA	Graduated in Pharmacy	USA, Barnard College, New York, 1921-1922 – Chemistry
María Luisa CAÑAMERAS	Graduated in Pharmacy	USA, Bryn Mawr College, Philadelphia, Penn., 1921-1922 – Chemistry
Loreto TAPIA	Student of Medicine	USA, Bryn Mawr College, Philadelphia, Penn., 1921-1922 – Chemistry

Table 6: Fellowships from women's organizations, 1920s/1930s.

Name	**Degree**	**Destination – Subject**
Pilar CLAVER SALAS	Teacher of Physics and Natural Sciences in the Training Teaching School	USA, Vassar College, Poughkeepsie, N Y., Middlebury College, Vermont, 1926-1927 Connecticut College, New London, 1927-1928 – Teaching of Sciences
Pilar MADARIAGA ROJO	Graduated in Chemistry, Researcher in the INFQ	USA, Vassar College, Poughkeepsie, N Y., Standford University Palo Alto, California, 1929-1930 Columbia University, 1932-1933 – Spectroscopy and Physical Optics
Dorotea BARNÉS GONZÁLEZ	Doctor in Chemistry	USA, Yale, New Haven, Sterling Chemistry Laboratory, 1930-1931 – Chemical Spectroscopy

Table 7: Fellowships from women's organizations, 1930s.

Name	**Degree**	**Destination – Subject**
Manuela GONZÁLEZ ALVARGONZÁLEZ	Researcher in the INFQ	USA, Bryn Mawr College, Philadelphia, Pennsylvania, 1931-1932 – Chemistry
Paz GARCÍA DEL VALLE	Researcher in the INFQ	USA, Radcliffe College, Harvard University, Cambridge, 1932-1933 – Spectroscopy
Arsenia ARROYO ALONSO	Graduated in Chemistry	USA, Bryn Mawr College, Philadelphia, Pennsylvania, 1936 – Chemistry
Justa ARROYO ALONSO	Graduated in Chemistry	USA, Smith College, Northampton, Massachusetts, 1936 – Chemistry

3. Political international networks

We found that some women got help for their studies or careers in different countries due to the influential situation of their families in the political context of the time. Example of this would be the following:

3.1. Zionists networks

It is the case of Dina Scheinkin (1898-1933). Born in Odessa (Ukraine), she later studied Natural Sciences in Madrid. She was the daughter of a Russian-born Zionist activist who in 1906 emigrated to Israel / Palestine. He was expelled from Israel and emigrated to the USA, while his wife and daughter settled in Madrid. In the science faculty, Dina contacted Antonio de Zulueta and the Natural Sciences Museum, where she later served as assistant. Between 1920 and 1922, she taught natural history in the mentioned "Instituto-Escuela". The only award she won in Spain was that of the JAE to take a course in the *Laboratory of Marine Biology* in La Coruña in the summer of 1920. However, from October 1923 to October 1924 she was in Berlin at Kaiser Wilhelm Gesellschaft as a guest of Richard Goldschmidt, according to research by Vogt (2008)[6]. Later, in 1932-1933 Dina Scheinkin was co-director, along with J. Carmin, in *The Independent Biological Laboratory* in Tel-Aviv (founded in 1928 and active until the fifties). It should be noted that Dina's father, Menahem Sheinkin, was one of the founders and early inhabitants of Tel-Aviv.

3.2. Personal and familiar networks

It is the case of Carmen Gómez-Moreno (1914-2008) who was the daughter of archaeologist Manuel Gómez-Moreno and Maria Elena Rodríguez-Bolívar. She belonged to one of the most influential families in Granada and Spanish society, related to Giner de los Ríos, Ferrant, Menéndez-Pidal, etc. She studied at the "Instituto-Escuela" and then entered the university and graduated in Natural Sciences in 1936. After the war and post-war hiatus, 1950 was a turning point in her life and her career, moving to America to study art history. She left a promising career as a scientist to follow her father's footsteps. Receiving an M.A. from Harvard University, she began her curatorial career at the Metropolitan Museum of Art in New York (MMA) in 1956. As she told her friends, she left Spain under Franco, as there really were no opportunities in Spain for her. She was curator of medieval art, and was the author of a book on Spanish painting and essays on treasures such as The Apse from San Martín at Fuentidueña. She was responsible for the monumental task of transferring the apse from the citadel church dedicated to San Martín at Fuentidueña in the vicinity of Segovia (a loan from the Spanish government arranged by her fathers) to New York, where it was installed at The Cloisters.

Other outstanding cases

In other publications[7] we have written about some of the women scientists included in the tables, those who worked on the research teams in Spain in those years and who contributed to scientific advancement in their fields. Here we will point out some of the less known and equally remarkable.

1. Käte Pariser (1893-1953), who came to Spain with a fellowship from the IAUW

She began his experimental work in genetics at the *Kaiser Wilhelm Institut für Biologie* in Berlin with Richard Goldschmidt, completing his doctorate in 1927. With the support of Zulueta, Goldschmidt and the German scientist Hilde Mangold, Pariser asked a scholarship for foreigners to the Madrid *Association of University Women* (a member of the *International Federation of University Women*). These scholarships were granted with funding from the Cultural Relations Board, Ministry of State. Before knowing the answer, she had to leave Germany because of the Nazi seizure of power. After staying three years in Madrid (1933-36), when the Spanish Civil War started she flew to Tel-Aviv. As far as we know, she then migrated to Australia and probably returned to Germany by the 1950s.

2. Jimena Fernández de la Vega (1895-1984)

One of the early women to graduate in Medicine in Spain (1919), she was the introductor of the genetic theories and techniques in Spanish medicine. She learned them in Germany and other European countries as a scholarship holder, working with scientists such Friedrich Kraus, Theodor Brugsh, Erwin Baur and Eugen Fisher (Berlin), Hermann Poll (Hamburg), Julius Baur (Vienna) and Nicola Pende (Geneva). In Spain she collaborated with outstanding physicians and

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6 VOGT, Annette (2008) *Wissenschaftlerinnen in Kaiser-Wilhelm-Instituten. A-Z*, Berlin: 2nd rev. edition (Veröffentlichungen aus dem Archiv zur Geschichte der Max-Planck-Gesellschaft, vol. 12).

7 On women biologists: DELGADO-ECHEVERRÍA, Isabel (2007) *El descubrimiento de los cromosomas sexuales,* Madrid, CSIC. On women physicists and chemists: Carmen MAGALLÓN-PORTOLÉS: (1998, 2004) *Pioneras españolas en las ciencias,* Madrid, CSIC; (2010) "Del Laboratorio de Investigaciones Físicas a la Meteorología: la primera española doctora en física, Felisa Martín Bravo", en J. Manuel SÁNCHEZ RON y José GARCÍA-VELASCO (eds.) *100 años de la JAE. La Junta para Ampliación de Estudios en su centenario*, 762-791 y (2011) "Químicas en la Edad de Plata", *Anales de Química*, 107(1), 94-101.

physiologists such R. Novoa Santos, G. Pittaluga and G. Marañón. In 1933, she was designated director of Genetics and Constitution department at Medicine Faculty in Madrid.

3. Margarita Comas Camps (1892-1973)

She was the first daughter of a liberal teacher, who participated on the "Institución Libre de Enseñanza". In 1915 she started as a Physics-Chemistry and Natural Sciences teacher at the *Normal School for Teachers* in Santander. In 1920 she was granted by the JAE for staying a year in the Belford College for Women in London (England), in order to study sciences teaching methodology. In 1921 she entered Natural Sciences faculty, although she had to work simultaneously, and graduated in 1925 with the best marks. Between 1926 and 1928 she stayed in Paris making her doctoral research. The JAE gave her a leave of absence (as a professor in the *Normal School for Teachers s*he was a civil servant and she needed a special permit for travelling). From 1932 on, she worked at the Pedagogy department of Barcelona University. In 1937, during the Spanish Civil War, Margarita Comas went to England in order to take care of refugee children, and, because of political persecution, she and her husband had to go into exile. She got an appointment as a teacher of biology at Darlington Hall School (Devon), living in England until she died in Exeter.

Conclusions

Our study underlines the fact that, in the historical period of this study, women participated in international exchange responsible for the science improvement in our country. Women participated in these exchanges in significant percentages, although limited by the small number of women who had access to higher education (Normal Schools for teachers, and Universities). Social extraction of these women was characterized, not by wealth but by the proximity to liberal environments and the fact of belonging to families of Spanish and international intellectual elite.

On the other hand, many of these women entered university after having completed studies in Normal Schools for Teachers, and they were mostly devoted to teaching duties. That explains the late age at which many had access to scholarships, compared with men, and also why they did not obtain the most prestigious international destinations. For example, any doctors in Natural Sciences had a scholarship to the Naples Zoological Station – a privilege that American women enjoyed thanks to their associations, as happened to Nettie Stevens, Marcela Boveri, and many others, while in our country this privilege was limited to a few men.

They were few women who could later engage in scientific research in Spanish institutions (some did, as Dorotea Barnés or Jenara Vicenta Arnal Yarza at the *National Institute of Physics and Chemistry*). Family support or friendship environment provided some others access to jobs *despite their gender*. This was the case of Jimena Fernandez de la Vega, appointed in 1933 director of the "Genetics Service" established at the Faculty of Medicine in Madrid, an institution that failed to develop as research centre: just after the death of her mentor, Nóvoa Santos, it became a "Seminar for the Study of Genetics", theoretical, instead of becoming the experimental laboratory that was originally planned.

Later most scholar women were devoted to teaching in the Normal Schools and High Schools. This had a positive impact however, because their preparation helped to improve the scientific training of the next generation of women students. Also, exchanges with American colleges and visits to other countries increased greatly the handling of different languages, especially English –an improvement that unfortunately did not continue in subsequent periods.

Many were the benefits obtained by the fellowships to women, among them, the results of its own investigations, the contacts they established with research centres, the exchange of publications, techniques and materials, and in some cases, the introduction in Spain of the latest developments in their fields (for example, Jimena Fernández de la Vega or Margarita Comas in genetics and Dorotea Barnes, in Raman Spectroscopy).

This could not have been possible without the support and encouragement offered to our country by university women associations. The wisdom of those women in creating a wide net for promotion of other women, the help and cooperation smoothed the way for improving the gender balance in science, which not only resulted in significant changes in the social organization of science, but also in its advancement.

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ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 233.

BELL'S TRAVELS TO PARIS AND THE INTRODUCTION OF HIS TELEPHONE INTO FRANCE (1877-78)

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Abstract

In mid-August 1877 Alexander Graham Bell started what he had planned as a short stay in Great Britain but finally lasted until the end of October of the following year. Despite general scientific recognition and continuous engagements to present his invention, at the beginning of this period he was eager to work personally on the more commercial aspects of its introduction in Europe but, increasingly disenchanted with the difficulties encountered by his patents, he ended up leaving business completely in the hands of the concessionaries.

To the author's knowledge the French case has received little attention. Based, among other sources, on family letters made available on the internet by the Library of Congress, this communication will show that Bell travelled at least three times to Paris from London, each trip corresponding to a different state of his affairs and attitude towards them. This research will also serve to pinpoint three initial phases in the history of the telephone in France: the heralding work of Alfred Niaudet and the Maison Bréguet; their efforts, together with the concessionaire, Cornelius Roosevelt, to fight the imitators and develop more powerful transmitters; and finally the appearance of Bell's friend Frederic Allen Gower which would eventually lead to the formation of the first telephone company.

After months of improvements and public exhibitions of his invention in the United States, Alexander Graham Bell got married on July 11, 1877, in Cambridge, Massachusetts. His bride, Mabel, was a daughter of Gardiner Green Hubbard, a long-time sponsor of his work, who only two days before had been elected trustee of the newly-established Bell Telephone Company. On August 4 the couple sailed for England, carrying with them a supply of telephones on what was intended to be a short honeymoon trip including some promotional activities, but Mabel's early pregnancy made a longer stay advisable. They were not back in America –with their baby daughter– until November 10, 1878.

During this time the Bell's settled in London, but the inventor's lecturing and business engagements took him to many places in Great Britain and Ireland, as well as –so it turned out– to Paris, on at least three occasions. No mention of the trips to France has been found in the literature, not even in Bruce's biography, which gives the most complete coverage of the period[1]. What follows is a brief account of these trips, relying heavily on the information contained in the family letters made available on the internet by the U. S. Library of Congress. An attempt is made to put such travels in the context where they belong, that is, the early history of the telephone in France.

The first trip: trying out a business role

Telephones made by his company preceded Bell in France. Correspondence from late October and early November indicates that his father-in-law had sent a box of them to one Le Gay, at 3, rue Scribe in Paris, intended for "the Scandinavian and other agencies"[2]. Besides, Bell himself had in London given two such telephones to Alfred Niaudet, "one of the firm of Breguet Bros. of Paris, the best electricians in France", for exhibition purposes[3]. Louis Breguet, the master of the renowned factory, showed these telephones to his colleagues of the Academie des Sciences during its session of October 29[4], and Niaudet took them to the Société Française de Physique on November 2[5], where he announced that Bell "had promised him formally to go soon to Paris and speak in a scientific meeting"[6]. Niaudet also demonstrated the telephone at the École Polytechnique at the opening of Jules Jamin's physics course[7], and, on December 7, at the Société des Ingénieurs Civils[8].

At the time of his marriage Bell had made his father-in-law the sole trustee of his patent rights in the five European countries (Britain, France, Belgium, Germany and Austria) where he had filed applications or was about to do so[9]. Once in Britain, where an agent had already been appointed, he grew increasingly concerned about the development of the telephone business on the continent, partly because he thought that if Hubbard placed it under his supervision he could obtain extra income in the face of rising family expenditures. Mabel was certainly of the same opinion, as shown in a letter of October 25 to her mother-in-law, Eliza Symonds Bell:

> *"Then there are his patents in France, Belgium, Germany, Austria and Italy. All except the last one in the control of my father as my trustee, but of course when he is so far away and has so much to occupy him at home, he cannot manage the business so well as some one here could…"*

Three days later Bell referred the matter to Hubbard:

> *"It has struck me that it might be possible to make the continental patents benefit us now as well as hereafter – if some arrangement could be made – to appoint me General Agent for these Patents*

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[1] Bruce, Robert V., *Bell: Alexander Graham Bell and the Conquest of Solitude*, Cornell University Press, 1990. No reference to the trips is found either in Aulas, Pierre, *Les origins du téléphone en France (1876-1914)*, Paris, Association pour le développement de l'histoire économique, 1999.

[2] Hubbard to Bell, November 2, 1877. This "Chal Le Gay", for whom Hubbard had asked Bell on November 13 to send fifty more devices, "to furnish several parties with telephones on trial", may be the Paris merchant Charles Le Gay, mentioned, in relation to wine exports to the U. S., in the book *Franco-American Commerce / Statements and Arguments in Behalf of American Industries Against the Proposed Franco-American Commercial Treaty*, San Francisco, 1879, p. 133. He may also be the same person whose death was reported by *The New York Times*, January 10, 1909: "Paris. Jan. 9.– Charles Le Gay, a former American, who had lived in Paris for some years, died here to-day".

[3] Bell to Hubbard, October 28 and November 1, 1877. In the former, Bell writes: "I have sent telep[hones] to Paris to be exhibited", and in the latter he specifies: "I have given Mr. Niaudet telephones to exhibit in France". This is in agreement with Niaudet's statement in his December 7 presentation to the Société des Ingénieurs Civils: "Une circonstance fortuite m'a conduit à Londres le mois dernier [an error in the transcription or the speaker's own confusion: this had happened in October, not in November]. Je fus mis en rapport avec l'inventeur. Je lui proposait de faire connaître son invention en France. Il me confia les deux premiers téléphones qui aient touché le continent européen, et que voici". Bell improperly calls Maison Breguet "Breguet Brothers". The current owner was Louis François Clément Breguet, assisted by his son Antoine and his nephew Alfred Niaudet.

[4] *Comptes rendus hebdomadaires des séances de l'Académie des Sciences*, vol. 85 (2nd semester of 1877), pp. 776-777.

[5] *La Nature*, November 10, 1877, pp. 383-384.

[6] Also *La Nature*, September 29, 1877, wrote the following when it published the translation of William H. Preece's report on the telephone at the Plymouth meeting of the British Association (pp. 274-276): "... les progrès qu'elle [l'invention du téléphone] fait de jour en jour nous obligent à y revenir aujourd'hui. M. Bell est du reste attendu à Paris, et tout le monde pourrra bientôt apprécier sa découverte".

[7] "Revue des Sciences" by Henri de Parville, *feuilleton* of *Journal des Débats*, November 29, 1877.

[8] *Mémoires et compte rendu des travaux de la Société des Ingénieurs Civils*, vol. 30, Paris, 1877, pp. 839-849.

[9] British patent 4,765 of December 9, 1876. French patent 119,626 of July 25, 1877. Bell may have applied for the other patents, which the author has not located, at the same time as the French patent.

> *until the organization of companies there – with a commission [...] It is necessary to do something here at once on the continental if I had power in the matter could negotiate I think profitably".*

Such was the situation when Bell made his first trip to Paris. On the evening of November 21, just arrived from London, he sat down in his room at the Hôtel Wagram to write a letter to his wife. He had scarcely had time to tell her about the rough seas he had encountered between Folkstone and Boulogne, when at eight o'clock he was interrupted by Niaudet's visit. After an interview lasting two and a half hours he resumed his letter: Niaudet would take care of translating and publishing in France a non-specified lecture by Bell[10]. Next day Bell was to see Le Gay and another dealer called Aymler[11], and also the chief of the French Telegraphs, Pierret, and the Minister of War, to whom he intended to give telephones for experimental purposes.

Concerning this trip, there is also an undated letter of Mabel to her husband, which she must have written on the same day of his departure, just after receiving a telegram of his from Folkstone Harbour asking, among other things, for a letter from Aymler that he had probably forgotten to take along with him. Mabel tells him encouragingly that the more she thinks about it, the more important it seems to her that he is in Paris[12].

No other documents have been found that allow the duration of Bell's stay in Paris to be established. This was presumably short, and –despite his alleged promise to Niaudet of addressing the scientific community– without any public engagements, judging from the silence of the great Parisian press. Nevertheless, some of the five papers reviewed, *Le Figaro*, *La Presse*, *Le Temps*, *Le Gaulois* and *Journal des Débats*, contained news about the telephone on those dates. In particular, they reported on a meeting of Louis Breguet with journalists on November 28, at which he showed the devices and said that he had tried them successfully between Paris and Mantes-la-Jolie, 58 km apart, and intended to repeat the experiment with Nancy[13]. Surprisingly enough, on December 14 *Le Temps* published a long article about the telephone under the heading "Chronique", written in the first person but unsigned, the last paragraph of which contains an intriguing reference to what may have been Bell's impression of his trip:

> *"It seems that to begin with, M. Graham Bell has not been delighted with the reception he has met with in France; he could have been taken for an eccentric; this is the excuse that routine usually opposes to progress"[14].*

The second trip: helping the business of others

Hubbard was not responsive to Bell's demands for a more active role in managing the introduction of the telephone in the countries where his son-in-law had given him control of the patent rights. In the case of France, he decided of his own accord to appoint Cornelius Roosevelt as concessionaire. A first cousin of the future U. S. president Theodore Roosevelt, he belonged to a wealthy New York family and had returned to America after the failure of his initiatives to introduce private telegraphy into France –the so-called district telegraph or "telegraphe de quartier"[15]. His brother Hilborne Lewis, who was one of Hubbard's partners in the telephone business, may have recommended him in view of his knowledge of the French language and customs[16]. On December 1, Cornelius sailed back to France with a letter of presentation for Bell.

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[10] Probably the one he had given to the Society of Telegraph Engineers on October 31. *La Nature* published the translation, "presque en totalité", in two parts, on April 27 and May 4, 1878, pp. 337-342 and 355-359, under the title "L'histoire du téléphone racontée par son inventeur". In England it appeared in book form: "The Telephone. A Lecture Entitled Researches in Electric Telephony, by Professor Alexander Graham Bell, delivered before the Society of Telegraph Engineers, October 31st, 1877. Published by the Society [...] London [...] New York [...]1878. "

[11] Perhaps John Aylmer, civil engineer, at 4, rue de Naples, who in 1877 was honorary secretary in France of the Society of Telegraph Engineers, London, and in 1881 was appointed secretary of the British commission for the International Electrical Exhibition to be held in Paris that year.

[12] There is further evidence of the letter corresponding to this trip, despite a misleading abbreviation that looks like "Oct" in the place where Mabel usually indicates not the month but the day of the week. In fact, she tells his husband that the notes of his "Glasgow conference" have arrived, in probable reference to one of the several talks he gave there between November 8 and 14 (see the letter dated November 12 from Mabel to her mother-in-law). On the other hand, the letter was sent from "Jermyn St.", which was the Bells' London address only until the end of November (see the letter dated December 2 from Mabel to her mother-in-law).

[13] *Le Figaro*, November 29, 1877, referenced by *Le Temps* of the following day.

[14] "Il paraît que M. Graham Bell n'a pas été tout d'abord ravi de l'accueil qui lui a été fait en France; on l'aurait pris pour un excentrique; c'est la défaite que la routine oppose d'ordinaire au progress".

[15] He was granted French patent 117,492 for a "télégraphe de quartier". He applied on March 13, 1877. (*Bulletin des lois de la République Française*, 1878, first semester, p. 769).

[16] Hilborne Lewis Roosevelt, younger than Cornelius by two years, died prematurely in 1888 at the age of 37. He has gone down in history as the remarkable builder of the first electrically-actuated, large organs in the United States. He also patented in this country some telephonic devices and was one of the founders of the Bell Company of New York.

In the letter, dated November 29, Hubbard highly commended its bearer. The following day he wrote privately to Bell: "I think you will like Mr. Roosevelt very much, and that the arrangement will please you. It is made to depend on your approval". On December 11 he insisted only on the first part of the message: "I trust you will be pleased with Mr. Roosevelt", adding: "He has not as much business experience as some, but he is thoroughly honest and is better capable of organizing a company for France than anyone we could send you". No indication has been found that Bell dared to disagree openly with Hubbard in this matter, but he certainly took his time to meet Roosevelt. It seems this did not happen until a new trip to Paris, where he probably arrived on Saturday, January 19, 1878, this time after crossing the English Channel from Dover to Calais. Only five days later he was back in London, according to a letter dated January 25 from his wife to her mother-in-law, which gives valuable details of his movements[17]:

> *"He has had an interview with the best Parisian lawyers whom Mr. Roosevelt has employed to conduct a prosecution against a firm who are infringing Alec's patents. They said at first that the patent was not worth anything because it had been applied for too late, but Alec succeeded in convincing them that the case was not so hopeless as they had thought and they will begin proceedings at once. Still it will take two years to prove whether the patent is valid or not, and the infringers will probably go on manufacturing – but the prosecution will deter others who were just about to begin to manufacture, and in two years Mr. Roosevelt will have time to establish himself and do a good business even if the patent fails. The French Government recognize Alec and that is a very great thing. Mr. Roosevelt, who some time ago spent long months and large sums of money in the vain effort to get government permission to build private lines and to form a District Telegraph Co. in Paris, says Alec accomplished in half an hour what no one else could do, namely get Mr. Perret, Under Minister of Telegraphs, to offer to build at government expense any number of private lines for telephones, and to give him every facility for trying the telephone on all government lines. Mr. Roosevelt says it quite took his breath away. The Government is going to erect a telephone line from Paris to Versailles, 20 miles, and at Alec's request, began the very next day to put up a line for Mr. Roosevelt. Alec called on M. Léon Say, Minister of Finance, and the Ministers of War and Navy. On Mr. Roosevelt's telling Mr. Léon Say that the empress Eugenie had requested Alec to show her the telephone, he agreed with Mr. Roosevelt in thinking it could be well for Marshall Mac Mahon to see it first, and he will ask him to appoint a day".*

Bell had certainly felt obliged to react to distress calls coming from France. His instrument was simple and easy to copy, so that at least one Paris merchant had managed to have telephones ready for sale on Christmas. This was Guillaume Walcker, the owner of Bazar du Voyage, a large department store at place de l'Opéra[18]. Roosevelt immediately began advertising in the press that Maison Breguet had been chosen as the exclusive manufacturer and warning unlicensed makers that they would be prosecuted[19]. But to no avail. According to one source, Walcker "made and sold some two or three thousand" telephones "in defiance of Bell's patents"[20].

This time Bell's stay in Paris resulted in a few paid lines in at least two of the city newspapers. The reports which have been found are alike, although they disagree on the dates, and mention telephone demonstrations at Say's residence as well as a dinner offered to Bell by his concessionaire, attended by "several French *savants*"[21]. It is surprising that Mabel, always so eager to praise Bell's feats to his mother, did not refer to this meeting in the letter quoted above.

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[17] From the Hôtel Wagram Bell wrote his wife a relaxed, undated letter bearing only the indication "Saturday", which contains a funny description of his fellow travellers in the train from London to Dover. He also tells her that his first move in Paris has been to send a note informing Roosevelt of his arrival. According to Mabel's letter of January 25, Bell had been away for six days and returned on the 24th, which is consistent with his having left on the 18th and arriving in Paris on the 19th, a Saturday. An earlier second trip to Paris should be discarded, among other reasons because Bell's mother wrote him and Mabel on February 25, telling them that Mrs. Hubbard had sent her some of their letters: "therefore we are pretty well up to your doings, till you departed a second time for Paris, from whence we hope you have before this returned". It is likely that two undated letters from Mabel to her husband correspond to this trip. These letters may have been sent on January 20 and 23 and begin, respectively, "I imagine you hard at work with Mr. Roosevelt..." and "I will only write a short note this time..."

[18] See news of these telephones (p. 3) and ad (p. 4) on *Le Figaro*, December 24, 1877. The Bazar du Voyage in Turgan, Julien, *Les grandes usines*, vol. XIV, Paris, 1882.

[19] See, for instance, *Le Figaro*, December 25, 1877, *Le Temps*, December 30, 1877 and *Le Gaulois*, January 27, 1878. The ad on *Le Figaro* was probably prompted by the publicity of Walcker's telephone that the paper carried the day before.

[20] George Lewis Gower, brother of Frederick, the telephone pioneer, in a letter to the *Chicago Daily Tribune*, September 3, 1879.

[21] *Le Figaro*, January 24, 1877, in the "Petite gazette" section, and *Le Gaulois* of the following day.

The last trip: receiving some scientific rewards

Maison Breguet and Niaudet counterattacked the forgers by developing modified/improved versions of the telephone. They were shown at the Universal Exhibition, which opened in Paris on May 1, 1878. Besides describing changes in the construction of the conventional apparatus, the journals reported on a “watch” or “snuff-box” pocket model, a modified box type for long lines and a fairly simple arrangement to call the correspondent station by singing into the telephone[22]. In June, Bell’s former assistant Frederick Allen Gower arrived from England[23], joined Roosevelt and also began to work on the telephone, his first achievement probably being a further modification of the box model which he patented with his new partner in September[24].

That same Summer, on the occasion of the Universal Exhibition, Roosevelt and Gower were very worried about the interest aroused in Paris by the telephonic work of Thomas Alva Edison and other American inventors, such as Elisha Gray and George May Phelps. On September 7, after Roosevelt had tried unsuccessfully to see Bell in London, they jointly wrote him a letter asking him to stand by for “an overwhelming blow”: “a grand exhibition” of his improved telephones, which would be demonstrated between Paris and Versailles[25]. However, Bell did not go. His mood in those days is clearly reflected in a letter that he sent to his wife on September 9 from Greenock, Scotland, where for a short time he was happily back to teaching speech to the deaf:

> *“Of one thing I am quite determined and that is to waste no more time and money upon the telephone. If I am to give away any more of my time – it must be for the object that is nearest my heart. If it is absolutely necessary for Mr. Roosevelt’s interests that I should go to Paris he must pay my expenses from Greenock to Paris and back – and remunerate me for my time – otherwise I will not dream of stirring. I have been twice to Paris for Mr. Roosevelt at considerable expense and inconvenience to myself and neither time was my presence absolutely necessary although he led me to believe so. I don’t intend to go the third time. I am sick of the telephone and have done with it for altogether – excepting as a play-thing to amuse my leisure moments”.*

Considering the fact that when Bell made his first trip to Paris in November 1877, Roosevelt had not yet appeared on the scene, his words suggest that he returned to the city in the interest of the concessionaire after his first interview with him in January 1878. No family correspondence or press reports referring to this third trip have been found[26], but a fourth one is well documented. This time Bell’s priority in the invention of the telephone was at stake, as Hubbard had bluntly expressed in a letter of September 26, referring to the Universal Exhibition:

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[22] “French telephones at the Paris Exhibition”, *Engineering*, June 21, 1878, p. 500. See also “Exposition Universelle. Matériel et procedés de la téléphonie.– Téléphones et accesoires de la téléphonie (Société Anonyme des téléphones Bell, 1 rue de la Bourse, Paris)”, *Les Mondes*, May-August 1878, pp. 368-373. Besides the mention of the apparently new company (located at the concessionaire’s address), the article contains interesting information: “... after the appearance of the American instrument, we have received many questions, all with the same purpose: to find out how to procure the best telephones. Unfortunately we have remained for a long time undecided about the answer, because of the many forgeries of which the apparatus has been the object. Lastly, at the Exhibition, we were going through one of the rooms dedicated to telegraphy when our attention was attracted by a telephonic showcase, on top of which one could read ‘Sole concessionaire of the French patents’. We wanted to be sure of the value of this title and with the greatest satisfaction we attended demonstrations on the premises of the concessionaire himself, who asked us to examine his patents”.

[23] Frederick Allen Gower (born Sedgwick, Maine, July 25, 1851 – died somewhere in the English Channel, July 18, 1885) was a journalist with *The Providence Evening Press* of Providence, Rhode Island, when he met Bell on the occasion of his telephone demonstration at Salem, Massachusetts, on February 23, 1877. Bell became very fond of him “both as a friend and a business man”, as he wrote in those days, and made him his assistant in charge of his telephonic shows. At the start of the Bell Telephone Company, Gower was appointed general agent for New England (excepting the city of Boston), and after Bell’s departure he was also given the exclusive right to lecture on the invention in the United States, for which purpose he developed a so-called “telephone harp”, a generator of electric signals that could be transmitted over the line and produce the sound of musical notes in the telephone receiver. He rejoined Bell in England in April 1878, and during the following month took the opportunity to show his instrument at the Royal Society and use it to illustrate James Clerk Maxwell’s Cambridge University lecture on the telephone. After going to France in June to help Roosevelt, his career developed quickly, his “Gower-Bell” telephones being sold all over the world. He was a very rich man when in 1881 he became interested in controlling balloons for aerial warfare, an endeavour that cost him his life in a flight from Cherbourg.

[24] French patent 126,511, filed September 12, 1878, “Perfectionnements dans le téléphone à boîte” (*Bulletin des lois...* 1879, second semester, p. 670). The improvement is mentioned in Du Moncel, Th., *Le téléphone, le microphone et le phonographe*, 1st. edition, Paris, 1878, pp. 73-74. This pioneering book must have appeared around the end of August, given that the 15th of this month is the latest date referred to in the text, and also that the author presented a copy to the Académie des Sciences at its meeting of September 15.

[25] In the letter, Roosevelt also appealed to Bell’s honour: “The Edison and Gray with Phelps have combined their interests and have made quite a stir here by exhibiting at the Exposition, before the press, etc. They draw a very long bow and to judge from their tone one would imagine that Bell was an impostor and a cheat”.

[26] In the letter of January 25 quoted above, written just after Bell’s return from Paris, Mabel tells her mother-in-law that “he will have to go again in a fortnight”.

"The newspapers in this country have been full of advertisements and notices of the fact that Gold Medals were awarded to Mr. Gray and Mr. Edison and none to you. This advertisement is a great injury to us, and we think it was obtained through fraudulent representations, and that the final award will not be made until the 21st day of October. The Chairman of the jury thinks the invention so great a wonder that it could only have been made by a scientist and electrician. Gray's agents have represented that Gray was a scientist and electrician, that you were not; and that you stole the invention from him. Now we want to have you go at once [to] Paris at our expense, take Gray's letters with you[27], see the Chairman and jury and make your own explanation. That alone will be enough, anyone on a little conversation will see that you know enough to have invented the telephone and that you are both a scientist and electrician. I have also sent by book post, Prescott's book on the telephone which contains Gray's caveat, showing how little he knew, and a copy of your patent to show that it was on hand two weeks before it was filed[28]. Your pecuniary interest in this matter is too great to be neglected, and your honour also is somewhat at stake".

Hubbard's information was not quite correct. What the jury really did was not to deprive Bell of a prize but to put him on the same level of his rivals Gray and Edison by awarding each of them the highest distinction, a "grand medaille", for their telephonic work, which in Edison's case also acknowledged his invention of the phonograph. Besides, Edison was made Chevalier de la Legion d'Honneur[29]. Bell seems to have taken the decision rather sportingly. On October 20 he was again at Hôtel Wagram[30], ready to attend the official state award ceremony the following day, after which he would leave immediately for London and then to Oxford, where the philologist Max Müller had invited him to give a series of lectures on speech, his last scientific commitment before departing for America[31].

The only record found of this trip to Paris is a letter dated October 20 written to Mabel by Adam Scott, one of Bell's fellow travellers[32], and continued on the same sheet of paper by Bell himself. After jokingly reporting on a dinner at Roosevelt's house the previous evening, Scott goes on with important news:

"This morning (Sunday) Alec was introduced to the Count du Moncel, and thoroughly converted him to his views – This is specially important because du Moncel is a great authority here and has allowed himself to be misled by the Gray and Edison party – He is vexed at having thus done Alec injustice, and will set matters right – The second edition of his Book on the Telephone would be published this week, and Alec was just in time to stop the proofs – Du Moncel will correct the views given in the book, and would go further but for the prudence of keeping the actual evidence quiet – He has given Alec a copy of the first edition and Alec is reading it, to become acquainted with its contents, as the proofs of the second edition are to be sent to him tonight..."

Bell, writing after Scott, confirms the good news and adds:

"My arrival here is most fortunate at this time. The Western Union will probably publish extracts from Du Moncel's work – but what a triumph it will be for your father to be able to come down on the Western Union with the second edition!!"

A comparative reading of the first two editions of Du Moncel's *Le téléphone, le microphone et le phonographe* shows clearly the outcome of this episode, which in effect was very favourable to Bell's claims[33].

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[27] One of the letters Hubbard had in mind was probably that sent by Gray to Bell from Chicago on March 5, 1877, in which he gave him "full credit for the talking feature of the telephone" (see Bruce, op. cit, pp. 220-223).

[28] George Bartlett Prescott's *The Speaking Telephone, Talking Phonograph and Other Novelties*, New York, 1878. Gray's *caveat*, filed, as is well known, after Bell's patent specification on the same day, February 14, 1876, is described in pp. 201-205.

[29] *Le Figaro*, October 27, 1878, reporting on this in "Échos de l'Exposition", wrote that Edison was "the inventor of the phonograph, the telephone and so many other curious discoveries".

[30] Bell must have given some hint of his intentions, because in a short letter dated October 9 Roosevelt told him politely that any day he might fix would suit him, and added with some resentment: "But I must tell you that your visit, aside from the pleasure of seeing you, will be of no great value to us now as the time is past and it is too late. That Gray and Edison have injured your reputation there is no doubt as I have repeatedly written you and even if you can 'effectually demolish their claims' the time has gone by".

[31] See latter of Bell to his mother, October 4, 1878.

[32] Adam Scott, Bell's friend from their childhood, worked for the London Bell telephone agency, which had been entrusted to colonel William H. Reynolds, a Providence cotton broker, in the Summer of 1877. On April 23, 1879 Scott read before the Society of Telegraph Engineers, London, a paper entitled "Recent improvements in Professor Bell's telephone", in which he described a new model devised by Gower (*Journal of the Society of Telegraph Engineers*, 1879, pp. 327-336).

[33] Major changes appear in two places in the second edition: the paragraphs dealing with the question of priority between Gray and Bell in the point "Un coup d'oeil historique" that opens the book (pp. 7-9, corresponding to 8-10 in the first edition), and the whole point "Part de M. Elisha Gray dans l'invention du telephone" (pp. 57-61, corresponding to 56-60 in the first edition).

Further developments

On October 31, 1878, Bell sailed from Liverpool to Quebec, subsequently to face a long period of patent litigation in the U. S. Gower, who later said in an interview that in France Bell's patent "was unfortunately too late to be of legal value"[34], continued working on his friend's invention, publishing the results and filing new patents with Roosevelt, the last one on June 18, 1879. On the 6th the French Government had issued a call for proposals to establish public service telephone networks. Three companies were authorized to operate in Paris, based respectively on the equipment of Gower, Edison and Francis Blake. In late 1880 they merged in one[35]. In that same year Bell returned to Paris to receive the Volta prize (50,000 francs), awarded him by the French Government for the invention of the telephone. In sharp contrast with the visits reviewed in this paper, on this occasion he attended a session of the Académie des Sciences on October 11, toured other relevant institutions and met a number of French scientists and engineers.

Two Roosevelt's ads mentioned in the text.

Avis divers

TÉLÉPHONE-BELL

Les propriétaires du brevet français font savoir qu'ils ont choisi M. Breguet comme seul fabricant pour la France des Téléphones de M. Alexander Graham Bell. Ils poursuivront avec toute la rigueur des lois les contrefacteurs du brevet, fabricants, importateurs, marchands et les acheteurs des objets contrefaits.

On est invité à s'adresser à M. Cornelius Roosevelt, 1, rue de la Bourse, qui est seul concessionnaire pour la France.—Paris, le 24 décemb. 1877.

Le Temps, December 30, 1877.

Tous les **TÉLÉPHONES** véritables du Professeur ALEXANDRE GRAHAM BELL, devront porter la marque de fabrique ci-après.

Les Téléphones non munis de cette marque sont des contrefaçons ou imitations et tous importateurs, fabricants et vendeurs seront poursuivis suivant la loi.

CORNELIUS ROOSEVELT,
seul Concessionnaire des Brevets pour la France.
1, Rue de la Bourse, PARIS.

Le Gaulois, January 27, 1878.

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[34] "Telephone in France", *Chicago Daily Tribune*, July 18, 1879, a report by the *New York Herald* correspondent, dated in Paris on the 1st.

[35] For a comprehensive account of these events, see Aulas, op. cit. (note 1).

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 240.

THE RAM-TORTOISE OF HEGETOR OF BYZANTIUM IN THE TREATISE *ON MACHINES* BY ATHENEAUS MECHANICUS. SOME REMARKS ON ITS RECONSTRUCTION

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Abstract

In this paper I present a war-machine, the huge ram-tortoise by Hegetor of Byzantium, whose description is to be found in three ancient sources: Athenaeus Mechanicus, Vitruvius and the Anonymous Byzantine. Because of the general difficulty and obscurity of all these texts, reconstructions of the machine have raised a variety of conflicting interpretations. Basing on a new philological analysis of the text of Athenaeus Mechanicus, which is in my opinion the most reliable source about this machine, I present here a new and more authentic reconstruction.

Introduction

In the ancient literature the name of Hegetor of Byzantium (third century BC), an engineer of military engines, is only found in two works of the second half of the first century BC: Athenaeus Mechanicus' treatise *Περὶ μηχανημάτων* (*On machines*) and in Book X of Vitruvius' *De Architectura* (10.15.2-7). A third mention can be found in the somewhat later essay on poliocertics, written in the tenth century AD by an Anonymous Byzantine, *Παραγγέλματα πολιορκητικά* (*Manual on siegecraft*). However, none of these three works venture into a discussion of Hegetor himself, rather they describe the invention that made him famous: a ram-tortoise of imposing dimensions, known as Hegetor's engine.

A tortoise was a vehicle with wheels entirely covered by planking, which must have resembled, apart from its notably bigger size, the carapace of an animal tortoise. There were smaller and lighter tortoises, which could be carried by hand, like wicker-tortoises, beak-tortoises and many more. But the bigger and more dangerous were undoubtedly the ram-tortoises, which carried a ram used to launch powerful broadsides against the city-walls. Within these models of ram-tortoises, Hegetor's was certainly the most imposing and ambitious in all antiquity.

The descriptions given of this engine by Vitruvius and Athenaeus are surprisingly similar: they follow the same order of topics and rarely does one contain information that the other lacks. On the other hand, the Anonymous Byzantine's description is more autonomous and synthetic. All three descriptions present obscure passages and are therefore difficult to understand. Hence, reconstructing the engine is fraught with complexity. The first attempts in this sense date back to the beginning of indirect tradition of Athenaeus' text. Some copyists wanted to enclose a drawing in the text, but with poor results. Careful philological studies on the text, which have been going on now for almost one and a half centuries, have certainly supplied progressively better representations[1]. However, in spite of these, we still have not obtained a completely satisfying reconstruction.

In my recent edition on Athenaeus Mechanicus' *Περὶ μηχανημάτων*[2] I dealt with all machines he describes and so also with Hegetor's engine. In order to understand it I payed constant attention to the other two sources: Vitruvius and the Anonymous Byzantine. As Athenaeus' text shows itself to be the more complete, I will consider only this treatise here. What I will try to reconstruct is therefore not the engine *tout court*, but the conception of this engine that Athenaeus Mechanicus must have had. Nevertheless it is not unlikely that such reconstruction is actually the most reliable of all.

The results I am about to present derive from a renewed philological work in Athenaeus' text. A different selection of the variants and new interventions on the text have played a determining role in the creation of this new reconstruction. Thus, the reason to set my report like a step-by-step examination of singles passages. The whole description of Hegetor's machine can be divided in four parts: a) the vehicle (i.e. the tortoise, lines 197-223[3]), b) the weapon (i.e the ram, lines 223-236), c) the drawbridge (lines 236-240) and d) mass and performance datas about the whole engine (240-245). In this paper I deal with the first two parts, which are the longest and most complex ones.

The tortoise

First of all Athenaeus describes the supporting base, called έσχάριον[4]. It had a rectangular plan (42x28 cubits) and on the inside a grid network composed by διαπήγματα and περιπήγματα (horizontal and vertical beams) not unlike the grid of the έσχάριον in other tortoises (for instance the filler tortoise). Then he depicts an element that is absent in all other types of tortoise: the σκέλος[5] (otherwise mentioned by Athneaeus just in the description of the Towers of Diades). It must have been a particularly long vertical post, strengthened with a juxtaposition of two timbers fastened together. Almost all scholars have so far imagined that these four σκέλη were placed at the four corners of the έσχάριον. In reality, Athenaeus merely says that they were έπὶ τού έσχάρίον, i.e. on the έσχάριον, without specifying their position. It has to be noted though, that successively (214) another two σκέλη are mentioned, taller than these first two and clearly described as being placed έν μέσω τής χελώνης, i.e. in the central part of the έσχάριον. The position of the mentioned σκέλη will then have to be identified also according to their interaction with the other two. It is therefore better to postpone this question until we will have read the passage concerning the two central σκέλη as well.

After having dealt with the wheels of the engine (201-204), Athenaeus goes back to the surface of the έσχάριον , in order to describe the **colonnade**[6]. Though Athenaeus does not say so expressly, it is clear that these columns were to be

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[1] 1See: Sackur, Walter (1925). *Vitruv und die Poliorketiker: Vitruv und die christliche Antike: Bautechnisches aus der Literatur des Altertums*, Berlin, W. Ernst & Sohn; Lammert, Friedrich (1938a). *Die antike Poliorketik und ihr Weiterwirken*, «Klio», XXXI: 389-411; Lammert, Friedrich (1938b). *Zu den Poliorketikern Apollodoros und Athenaios und zur Poliorketik des Vitruvius*, «Rheinisches Museum für Philologie», LXXXVII: 304- 333; Lendle, Otto (1975). *Schildkröten: antike Kriegsmaschinen in poliorketischen Texten*, Wiesbaden, Steiner; Lendle, Otto (1983). *Texte und Untersuchungen zum technischen Bereich der antiken Poliorketik*, Wiesbaden, Steiner; Whitehead, David – Blyth, Philip Henry (2004). Athenaeus Mechanicus, *On machines = περὶ μηχανημάτων*, Stuttgart, Steiner.

[2] Gatto, Maurizio (2010). *Il Περὶ μηχανημάτων di Ateneo Meccanico. Edizione critica, traduzione, commento e note*, Roma, Aracne. Athenaeus's Editio princeps dates back to the 1693 and it is included in the *Veterum mathematicorum Athenaei, Bitonis, Apollodori, Heronis, Philonis, et aliorum opera, graece et latine pleraque nunc primum edita*. by Melchisedech Thévenot (Paris, Typographia regia). The first critical edition is to be found in: Wescher, Carl (1867). *Πολιορκητικά καὶ πολιορκίαι διαφορών πόλεων. Poliorcétique des Grecs*. Traités théoriques. Récits historiques, Paris, Imprimerie impériale.

[3] The numeration of the lines of Athenaeus' text is taken from my edition.

[4] Ath. 197-198.

[5] Τά δέ σκέλη τά έπὶ τού έσχαρίου πηγνύμενα δ συντίθεται καὶ έκαστον έκ β ξύλων συνημ-μένων τὸ μήκος έχόντων πήχεις κδ τὸ δέ πλάτος πηχυαία (198-201).

[6] Κίονες δέ πήγνυνται έπὶ τού έσχαρίου δωδεκαπήχεις, πλάτος μέν έχοντες παλαιστάς γ, πάχος δέ ι δακτύλους. Απέχει δέ άλλος απ΄άλλου κίων παλαιστάς ζ (204-207).

found on the perimeter of the έσχάριον. In order to find their number, Schneider-Schwartz[7] have made a series of calculations which led them to the conclusion that there were 24 of them on each of the long sides and 16 on each of the short sides. Unfortunately their reasoning makes two basic mistakes. Firstly, they compute on each side a number of interspaces equal to the number of columns, whereas the number of interspaces is actually equal to the number of columns minus one (as only one interspace is between two consecutive columns, so that n columns form n-1 intercolumnations). Secondly they suppose that the four σκέλη stand on the four corners of the έσχάριον. For reasons that I will explain later, I am convinced that they were not part of the perimetric colonnade. Thus the calculation must be outlined as follows. Let n_c be the number of columns and n_s the number of the intercolumnations, then $n_s=n_c-1$. Considering that the breadth of each single column is equal to 3 palms and that each interspace stretches for 7 palms, we will get $252=3n_c+7(n_c–1)$, where the first addend represents the sum of the thickness of all the columns expressed in palms, and the second ddend, the complete sum of all the interspaces, always in palms. With $n_c=26$, the sum of the two addends is 253, which is very close to the actual 252. With a good approximation we can therefore assume that the columns on each of the long sides were indeed 26. On each short side, by outlining a similar reasoning we have $168=3n_c+7(n_c–1)$. With $n_c=17$ the sum of the two addends is equal to 163, which is quite close to the actual 168. The 5 palms in excess in that case could have been equitably distributed between the two more lateral interspaces and the two more central (10 dactyls each). Obviously the angular columns are included both in the calculation of the long side, and in that of the short side. So, whilst calculating the total sum of the columns present in the entire perimeter, one must be careful to not count them twice. Therefore if n_{ct} is the total number of all the columns present on the perimeter of the έσχάριον, it will be $n_{ct}=2x26+2x17–2=84$.

On the colonnade laid four architraves on which raised a **roof** made of rafters and a top beam (208-211). Athenaeus says that the architraves placed on them were 3 palms high, which is a half cubit, and the sloping roof was another 8 cubits high. So, considering that the posts were 12 cubits high, we can calculate the high of the whole tortoise as being 12+0,5+8=20,5 cubits. The Tortoise was covered by **planks and rawhide** in order to protect it from fire (211-212). This suggested to Lendle[8] another resemblance with the aforesaid tortoise, not expressly mentioned in the text. He claimed that also on the έσχάριον of Hegetor's tortoise were fixed some protruding rafters (ξύλα ύπερέχοντα). Thus reckoning that Hegetor's engine must also have had a protruding frame around its έσχάριον, where the lower ending of the roof rafters were supposed to be fixed, so creating a covered ringroad around the έσχάριον and in this way enlarging the base of the engine. However, this interpretation by Lendle about the position and the function of the ξύλα ύπερέχοντα is actually incorrect even if applied to the filler tortoise. So, in the case of Hegetor's tortoise he is wrong twice: firstly, because he misinterpreted the structure of the ξύλα ύπερέχοντα itself, and secondly because here such protruding rafters are not even mentioned and there is therefore no reason to believe in their presence. Athenaeus mentions a missile battery (βελοστασία, 213-214) as well, resting on the architraves. Given the presence of the rafters, the planks and its cover, we are forced to imagine that loopholes had been created somewhere slightly above the μέση στέγη to enable the artillerymen to shoot their projectiles out.

After this, Athenaeus talks again about σκέλη[9] (legs). It is clear though that he still cannot talk about those mentioned before (198). In fact four σκέλη were there mentioned, each one 24 cubits high. While here we are talking about only **two σκέλη**, moreover of different dimensions, being ashigh as 30 cubits. The problem of identifying the position and the orientation of the first four had been postponed until now, when the remaining two were mentioned in the treatise, in order to face the problem in its entirety. This is undoubtedly the thorniest question of the whole passage about Hegetor's engine. Luckily, in this second passage about the σκέλη Athenaeus is more generous with information. Firstly, in relation to their position he informs us that the bigger ones were placed έν μέσω τής χελώνης (in the centre of the tortoise) which must clearly mean in the centre of the έσχάριον (base). Secondly, in relation to their orientation, he tells that they were όρθια as well as συμβεβλημένα i.e. vertical and convergent. Although these two Concepts might appear to be contracting each other, we must make an effort to understand what such a term-combination might want to express. The most likely hypothesis is that he wanted to suggest that they were almost vertical, but slightly inclined, so to converge without touching one another. Statically that is the most favourable position[10]. Besides their orientation in space the most surprising thing is their height, which is superior to that of any other element in the engine. We already saw that the four 24-cubit σκέλη exceeded the roof of the tortoise for about 4 cubits but these two central 30-cubits σκέλη exceeded it even for 10 cubits. This means that all the 6 σκέλη passed beyond the covering roof. Certainly they could not be used to keep the position of men or artillery, as they would have been exposed to the enemy's fire. The only possible alternative is to assume that they had some use for the ram. Therefore not only the highest ones, expressly associated to the κριοδόχη, but also the other four must have had the same

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[7] Schneider, Rudolf – Schwartz, Eduard (1912). *Griechische Poliorketiker III. Athenaios. Über Maschinen*, Abhandlungen der Königlichen Gesellschaft der Wissenschaften zu Göttingen, Philologisch-Historische Klasse, 12.5 Neue Folge, Berlin, Weidmann, 62.

[8] Lendle (1975), 51.

[9] Ιστανται δέ καί όπίσω τής κριοδόχης σκέλη β συμβεβλημένα ορθια έν μέσω τής χελώνης, έχοντα τὸ μήκος τριάκοντα πήχεις, τὸ δέ πάχος αυτών πηχυαίον τὸ δέ πλάτος τριπαλαιστιαίον (214-216).

[10] It is not by chance that this is how human beings lay their legs (they keep them slightly apart) when they want to assume the position of maximum balance while standing upright.

function. But exactly what was the κριοδόχη in this context? From the description of another ram-tortoise treated both by Athenaeus and by Vitruvius, i.e. the ram-tortoise of Diades, we learn that the word κριοδόχη means the compartment of the ram. However, in Diades ram-tortoise was installed a rested ram, thus making it easy to imagine the κριοδόχη as a sort of cavity placed on the ἐσχάριον, on the inside of which the ram would slide. Here we have to face a suspended ram instead, hanging from a height of a 30 cubits and swinging over the covering roof of the tortoise. Clearly then, the κριοδόχη cannot be composed (like in Diades' tortoise) of a supporting floor or of a closed compartment. Thus, we need to imagine it differently. The continuation of the text gives us other elements on the matter.

In the following sentence Athenaeus completes the description of the κριοδόχη[11]. It is interesting to notice that here Athenaeus mentions for the first time the ram. The fact that the ram appears exactly in relation to upright elements on the two central σκέλη, confirms what we said earlier: that the two central σκέλη were used to raise and hold a structure made of beams, winches and ropes, aimed at supporting the ram. But let us follow the passage.

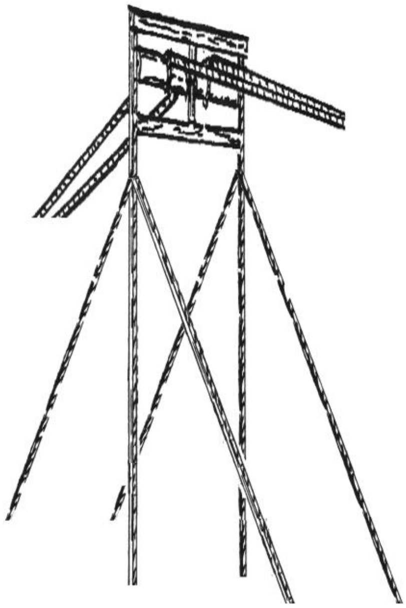

Fig. 1: the κριοδόχη.

At their upper tips were placed a περικέφαλον. It could not have been a simple beam but something wider, like a small platform (ἐφαρμόζω). This is the highest element of the engine. Lower, between the two σκέλη is then fastened a horizontal crossbar (διάπηγμα), parallel to the περικέφαλον, which creates a rectangular vertical space enclosed between the two horizontal crossbars and the upper part of the two σκέλη. The two horizontal crossbars are then fastened at their midpoint by an upright timber resulting in a splitting of the rectangular space longitudinally in two parts. Onto each of them is fixed a winch, from which hang the cables holding the ram. The ropes of the ram go therefore through this rectangular vertical space. It would be reasonable to imagine these winches a few cubits lower than the top of the central σκέλη (28 cubits). The ram however would have hung lower and precisely in the space between the second crossbar and the transverse beam of the tortoise's roof. However, besides the specific positions of each element, it is clear that this structure represents the κριοδόχη (the ram compartment) of the engine as it is aimed to receive, hold and move the ram. In order to complete the κριοδόχη we must also insert the other four σκέλη, whose function is now finally clear. When the ram was in action, the κριοδόχη must have suffered from strong swings. Just two σκέλη, for as solid as they could be, would never be enough on their own. It is therefore reasonable to believe that the purpose of the other four was to shore up the first two. Unlike the two central legs, there is no mention of them as being όρθιος, which might presuppose that they were placed in a markedly more slanting position. So here is the solution I would like to advance. Each 30-cubit σκέλος had at its front and at its back, a couple of 24-cubit σκέλη bowed exactly in the direction of the main σκέλος and converging onto it. In this way the whole structure holding the ram gained stability and balance.

[11] Εφαρμόζεται δέ έπαυτών περικέφαλον καί μέσος άλλο διά τών σκελών διάπηγμα. Καί άπά μέσον τού τε περικέφαλου καί τού διαπήγματος πήγνυται ξύλον ορθιον καί έφ΄έκατέρου μέρους τού παγέντος καί τών σκελών έμβάλλονται όνίσκοι τετορνευμένοι έξ ών τά öπλα έξήρτηται τά άνέχοντα τòν κριόν (217-221).

After that, a device called θωράκιον is introduced (221-223). Most likely it was a big metal parapet working as a shield for the men (no more than a couple) who stood on the περικέφαλον working as lookouts. Athenaeus says that the περικέφαλον was fixed to the κριοδόχη, which makes us understand that he didn't consider the περικέφαλον as an element belonging to the κριοδόχη dìqh. This sounds strange because it lies on the 30 cubit legs of the κριοδόχη, but in a certain way we could reasonably agree, because the περικέφαλον itself didn't play any role in holding the ram.

The ram

From now on the subject switches from the tortoise to the ram carried on it. First of all Athenaeus provides its **dimensions** (224-226). After that he describes its **iron tip** (226-229) and then he deals with the ύπόζωμα ύπόζωμα, an ingenious system of assembling several wooden juxtaposed beams (229). In a nautical context such systems were used to strengthen the keel of ships. Here however it is obviously used to create particularly long rams. Both in the case of the ram as well as in the case of ships, the binding element was made by ropes or wooden splints which were then placed longitudinally to the elements to connect and then fastened with ropes in order to keep them firmly joint together.

The ram was held in the middle by **chains**[12] (αλύσεις) wound three times around its trunk. These chains were described as being πηχυαίαι (of a cubit), but Schneider-Schwartz[13] preferred amending πηχυαίαι with παχείας, i.e. heavy. They probably assumed that the measure of one cubit does not match with the context, as it is too big if referring to the thickness and too small if referring to the length. On the contrary there is no problem in assuming that it refers to the length, as long as we imagine their position and their function slightly differently from the way it has been done previously. The chains did not stretch for the entire distance between the winches of the κριοδόχη and the ram (in that case Schneider-Schwartz would be right to consider it as being too small) but only along its last stretch. For the first part, this distance was covered by ropes coming from the winches of the κριοδόχη. These ropes —already discussed by Athenaeus in 220 (τα όπλα [...] τα ανέχοντα τόν κριόν) and soon again in 233 (όπλα αποτεταμένα [...] έκ τής κριοδόχης)— were then linked to these chains, which increased their extension for the measure of a cubit before being wound around the ram. There must have been more than one of such bindings. In fact Athenaeus goes on talking about the central one (230-232), which lets us imagine that there have also been some other ones. It was wound around the ram for a length of 5 palms and was surely the strongest. Although there are no ways to prove it, it's very likely that there were in all three bindings and consequently three suspending points, located one in the front, just venid the iron part, one in the middle and one in the back. Such a configuration is in fact ideal for controlling the movements of the ram.

After telling rapidly that the ram was covered with rawhide, Athenaeus goes back to the **ropes holding the ram**[14], which he had already mentioned in 220-221: τα όπλα [...] τα ανέχοντα τόν κριόν. But because he had just talked about another order of όπλα, i.e. those of the ύπόζωμα (229), he repeats some characteristics of other ones in order to prevent any confusion with his current subject. After that, he moves on talking about the connection between these ropes and the iron chains extending them. Here we learn that the rope of each of the three suspending points were not connected just with one chain but with four of them.

Obviously also these **chain-tracks were covered** with rawhide[15]. Then he explains the reason for this covering and here he says something really unusual, namely that the chains should be kept out of the sight of the enemies. In fact in all manuscript it is written: πρός τό μή όρασθαι, but this expression does not make any sense in this context, nor can a plausible solution be found by strictly sticking to the dictation of the codexes. Probably it is only the verb όρασθαι that is wrong. Giving a better look at the manuscripts, we notice that most of them have a blank space exactly before this verb. It's then not unlikely that the form όρασθαι is wrong and that originally another one was at its place. So όρασθαι must be changed with a longer verbal form, which, on one hand, could fill the blank space, and the meaning of which, on the other hand, could be more relevant to the context. A form which seems to satisfy both these requirements is συγκεκρασθαι. In this way the cover of rawhide around the chains would gain a plausible justification, as it would be used to avert the danger that they unduly intertwined.

Now the whole pass from the κριοδόχη to the ram is complete. The ram was held by three suspending points, in each of which it was bound by chains, from whose windings four chain-ends emerged. All this endings were connected just to one rope, which reached the κριοδόχη and hung from it. At this point one could rightly wonder where these hanging parts of these ropes would go. Athenaeus does not explain it. A reasonable answer could be that the ropes, after being fastened to the winches of the κριοδόχη, would descend vertically through the centre of the machine down until the έσχάριον, where a

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[12] καί διαλαμβάνεται κατά μέσον έκ τριών διαλημμάτων άλύσεσι πηχυαίαις (230).

[13] *Op. cit.*, 26.

[14] Τά δέ öπλα άποτεταμένα έκ τών όνίσκων τών έκ τής κριοδόχης καί άνέχοντα τὸν κριόν έχει τάς άρχάς άλύσεσι σιδηράς τετραπλάς πεπλεγμένας (232-235).

[15] Καί περιβεβύρσονται αί άλύσεις πρὸς τὸ μή συγκεκράσθαι (235-236).

team of men (safe from any danger thanks to the planking of the roof) would control the movements of the ram by pulling and releasing them. If this is the case, as I believe it is, we need to suppose that the central storey with the missile battery (βελοστασία) mentioned before, was only placed in the anterior part of the engine, while in the central part, the volume between the έσχάριον and the covering boarding, was made of a single space, in which the ropes could freely go through.

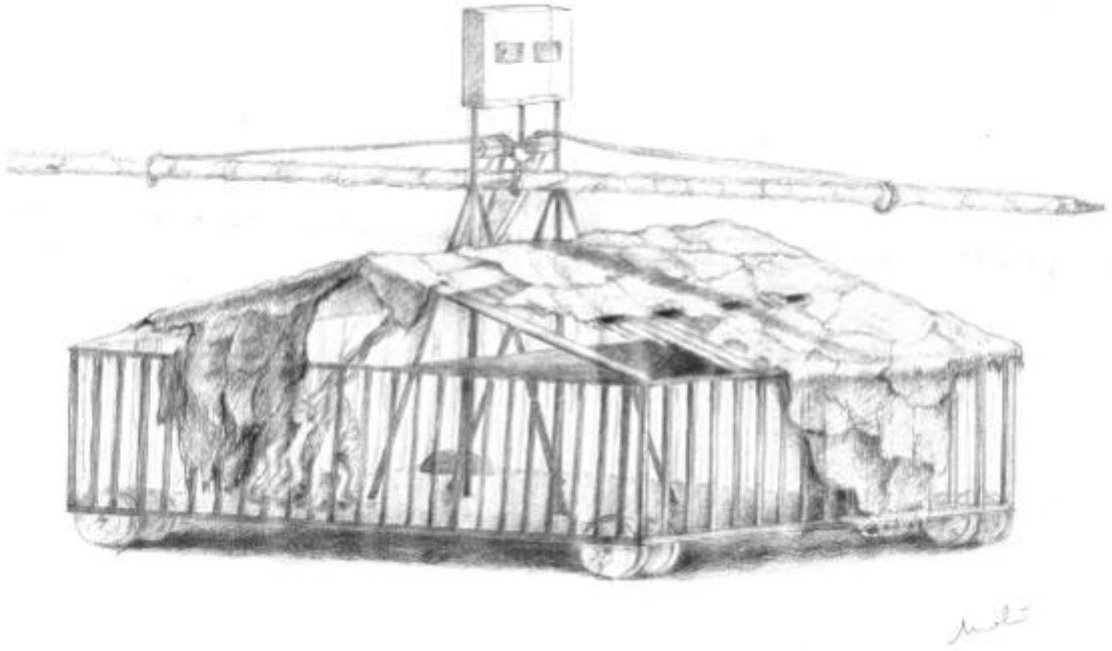

Fig. 2: Hegetor's ram-tortoise. My reconstruction

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 246.

ALENTEJO (PORTUGAL) AND THE SCIENTIFIC EXPERTISE IN FORTIFICATION IN THE MODERN PERIOD: THE CIRCULATION OF MASTERS AND IDEAS

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Abstract

The question of mobility in architecture or engineering is an ancient process that grows with the rise of the professional group. History testifies several forms of this mobility (migration, emigration, mission, availability) and with a direct relationship to the importance of the professional status or the power of political and economic institutions (public, private, military and civilian, secular and religious). During the modern period, master architects, engineers and experts in a wide range of other fields travelled all over Europe disseminating their ideas, and in Portugal specialists of other nationalities had a significant influence. The advent of gunpowder and greater sophistication in construction techniques meant that the art of fortification came to be regarded as constituting military engineering. A large number of architects and engineers working in Portugal from the 16th century followed a geometrical pattern featuring angular bulwarks and regular shapes, and they were important figures in the training of Portuguese architects and engineers and in Portuguese military architecture, which ended up be exerting an influence abroad. Around a hundred engineers from abroad worked in Portugal following the Iberian Union, taking part in operations during the Restoration War (1641-1668) to reinforce the land border, specially in Alentejo, because the topography of the region made it vulnerable to attack by land, accounting for the high concentration of fortresses; coastal fortifications were only reinforced/constructed much later, although some plans were produced during this period. John IV, in 1640, set up the Council of War and the Border Commission, whose well-defined role was to inspect fortifications and deal with all questions in this area. The translation of treatises, the national book production in the field, the richness of maps and imagery produced, are also witness of the vitality and interest aroused by fortifications in Portugal.

During the modern period, fortresses and strongholds reflect synchronous scientific and technological advances, also denoting the deepening of specific knowledge that would derive into the appearance of schools and of an immense corpus of treatises of diverse, although confluent, subject matters. Indeed, the scientific character of these fortifications and the works of military engineering allow us to demonstrate and strengthen the relationship between science and technology from the

16th to the 19th centuries. Fortifications, during this period, expressed, beyond their military content, an ideological testimonial, offering an example of harmony between military techniques and the historical moments that frame them, and therefore could be interpreted as multidisciplinary documents: urban quality and landscape environment, architectural and urban value, typology and morphology.

The appearance of pyroballistics and its widespread usage in battlefields led to alterations in the fortified spaces, in order not only to improve the fortified structures themselves, so as to enable them to resist attacks, but also to respond to the need of installing artillery pieces and cannons for defence. Indeed, the bulwarked fortification, relying on angular bulwarks, was conceived not only to protect the surrounding terrain, but also the territory behind it. From the middle of this century (the 16th) we see a multiplication of fortification plans in several European cities, in which we can notice the existence of transitional plans by the presence of rounded bastions, adapted to artillery.

The circulation of masters and ideas, in the most diverse domains, characterizes the modern period in a European level. Architecture and engineering were determining fields in this panorama, and, in respect to Portugal, the presence of Italians, Flemish, French, and others, would end up influencing creation in these matters. Considering the classical division of architecture into civilian, religious and military, the latter was always considered minor by historiography and treatise writers, especially given the primary objective of a strict functionality. Still, the advent of pyroballistics and the greater technical demands in constructions in the modern period, in terms of military architecture, lead to the latter being increasingly considered as a science that should be grounded in practical knowledge, in the work field of military engineers. Already in 1557 Lanterini (*Due Dialoghi*) compared fortification to the medical science: not only was it a practical work, it was also a faculty, a science and an art form, because it called upon mathematical principles in search for forms and proportions. The Vitruvian conception, that considered fortifications as public buildings, also concurred to the slow acknowledgement of military architecture as an autonomous field within architecture; we should also bear in mind that, for Sebastiano Serlio engineers were *"architects of war"*, that designed other kinds of buildings during peace time.

Portugal, in the early modern period, witnessed several initiatives due to the confirmation of their borders; in this matter, we should highlight the work of Duarte d'Armas in 1509, by mandate of king D. Manuel, and who, during approximately 5 months, confirmed the defence points along the border with Spain, from Castro Marim to Caminha. This confirmation lead to the survey of 55 castles and fortresses, a work he would organize in two volumes, which hold 114 panoramic charts of 57 settlements and plans of 51 forts along the border. In its drawings and legends, the author reveals details about the constructions and their implantation, today considered essential for their understanding and analysis: apart from the military elements, such as strongholds of the castle and barbicans, *atalaias* [tower houses] and watchtowers, living quarters of the garrison and of the *alcaide* [governor of the fortress], access zones, arrow slits and cisterns, we can notice as well the presence of elements from the daily lives of local populations, (wooded areas and cultivated zones, water lines and bridges, mills, churches and convents).

Geometric organization of space with angular bulwarks and regular layouts had several foreign interpreters in Portugal from the 1500s on, who were instrumental both in the education of national masters and in Portuguese military architecture itself that manifested in facts surpassing the national geographic context. Indeed, this change in building techniques took shape all over the Portuguese space, in the continent as well as overseas (Moreira: 1988, 1989, 2001). These interpreters (approximately 100 French, Flemish, Italian, Dutch, Swedish and English engineers and architects), stayed in Portugal after the dominance of the *Philip kings*, and later participated to the reinforcement of the terrestrial border during the Restoration War (1640-1668).

Thus, in Portugal, during the 16th century, we find the office of «master of construction of walls and fortresses», belonging to many Italian masters: Tomaso Benedetto da Pesaro; Filippo Terzi; Giacomo Palearo (Captain Fratini); Giovanni Battista Cairati (in the fortification of Malaca, Ormuz, Mombaça); Tiburzio Spannocchi; Giovanni Vicenzo Casale; Leonardo Turriano[1]. The Philippine period, particularly under Philip III of Portugal, corresponds to a policy in which fortified spaces served the image of the reigning power: it is an architecture without ornaments, in which geometry and proportions so highly prized by the Iberian monarch and so widely practiced by the military engineers he financed, were prioritized. This policy would manifest in the construction of small fortified places above pre-existent burgs, dominating them, a policy that, in order to prevent the uprising of local populations, the Spanish monarch would end up not implementing in Portugal. Here, he chose to reinforce the coastline or to build new bulwarked perimeters surrounding old castles. Military engineers were particularly alerted to the need to include the symbology of royal power though visible elements in these fortifications, which were also meant to impress the enemy: the emblems over the doors, on the walls and on the bulwarks were designed for this purpose.

[1] Before the Philippine period, we had Ravenna, Tiburzio Spannocchi, being born in the dukedom of Urbino and in the Pontifical States, excluding the rival powers such as Venice, the Spanish viceroyalties and their political allies such as Genoa and Florence. Under Phillip II technicians came from regions faithful to Spain, such as Lombardy (the Cairati, the Fratini, Turriano), some Florentines (Casale, Filicaia) and from the Abruzzi (the Antonelli).

The political convulsions experienced in the European panorama lead to the importance given to military construction, that could only be afforded by the greatest monarchies or some great Houses (such as the dukedom of Urbino) due to the expenses they implied, but that, at the same time, served as a statement for those who could afford them. These expenses were due to their high level of technical expertise and to the specialization required for designing them, as well as to the materials used in these defensive systems, and also to the need to assure the education in the art of building fortifications, above all of military engineers. In the case of the Iberian Peninsula, foreign military engineers were well paid and acknowledged; they remained in contact with the Court and with powerful nobles, which enabled them to assure the livelihood of their close relatives who shared the same craft, leading to the appearance of whole generations of engineers such as the Fratini or the Antonelli. A similar phenomenon would later take place with the generations already educated in Portugal, as was the case of the Serrão Pimentel.

In the 17th century, the Restoration War between Portugal and Spain dictated a need to reinforce the terrestrial border. A new attitude towards the importance of a defensive reinforcement had taken seed with the ascension to the throne of king D. João IV, in December 1640. Parallel to the appearance of a permanent army and auxiliary bodies, the Council of War and the Border "Junta" [Assembly] were created, each with very well defined functions, meant to inspect and deal with all matters relating to fortifications. Both this Council and the Junta acted upon new geographical, administrative as well as military policy, due to the creation of six military provinces, each with its own governor, Alentejo being the largest, with more than twenty six thousand square kilometres.

As for this last region, we should highlight its topographic characteristics, that made it vulnerable, hence the strong concentration of military posts between Moura and Castelo de Vide (responding roughly to the location of the Spanish ones, opposing them from the other side of the border), and the priority given, also during this period, to the defensive reinforcement of inland cities such as Évora and Beja. To this concentration also corresponds a diverse typology, with distinct interventions, from the wholly bulwarked enclosure (Elvas), to the building of defensive points of lesser dimension (isolated bastions, forts) that varied as well according to the priority of enemy attacks. We underline the predominance of elements privileging dynamism and mobility, from which the artistic perspective should not be alienated. Their construction was the most impressive source of State expenses, exhausting, in 1641, 60% of its revenues, given that those interventions were synonymous to a high level of expectations regarding construction, and implied knowledge of new techniques and military tactics.

Within this context we find the character of Luís Serrão Pimentel, author of the *Methodo Lusitano* (published posthumously by his son, Francisco Pimentel). He was responsible, since 1647, for the Lecture on Fortification and Military Architecture for the education of Portuguese military engineers, at a time in which those working on fortifications were mainly foreign engineers (French, Italians and from the Low Countries), some of which had already been in the Peninsula serving the "Philip kings", and whose work reflected the abandonment of the Italian models for the Nordic ones (from the Dutch and German schools), with fortifications designed for the interior of the polygons, terraced profiles in taluses adapted to the different terrains, the return of angular watchtowers, an abundance of external works, elevating the scientific level of the fortification. Serrão Pimentel basically inspired himself, from a theoretical point of view, in Adam Freitag, Mathias Dogen, Goldman, Marolois, Coheorn and Stevin, while at the practical level he coexisted with some foreigners active in Portugal at the time. We should also stress the fact that the question of topographic survey cannot be separated from the works of military engineering and architecture: the design of fortifications either depended upon pre-existent work at this level or lead to the assessment itself, hence the relation between 16th century Portuguese cartography and military engineering. We also believe that several of the implemented projects, if valuable under a military perspective, do not prove as much under a strictly cartographic point of view (absence of details from the terrain).

At the same time, we find, in the Alentejo of the 1600s, among many others, João Pascácio Ciersman (Dutch), a Jesuit priest, known as Cosmander (in charge of observing all strongholds along the border, suggesting the reinforcement or the creation of fortifications according to the "Dutch method"), Nicolau de Langres (French engineer), Jean Gillot (Dutch), P. Santa Colomba, the author of treatises Manesson-Mallet, all under the orientation of chief-engineer Charles Lasart. During the long period of conflict between Portugal and Spain (1640-1668), many of these engineers worked for both kingdoms (Langres, P. Santa Colomba, Cosmander), while some Portuguese military engineers also started to develop work in this Province, as was the case of Mateus do Couto (nephew).

With Serrão Pimentel we see the achievement of the ambition to educate national masters in the area of military engineering, that would continue in the following century and that Azevedo Fortes's book *O Engenheiro Português [The Portuguese Engineer],* implements. This author acknowledges as indispensable to the education of a good engineer knowledge of the areas of Arithmetic, Elements of Euclid, Practical Geometry, Trigonometry, Fortification, Attack and Defence of strongholds, the usage of the Mathematical instruments pertaining to his profession, the method of designing plants and topographical charts with their profiles, elevations and façades and the means to draw; he also should not relinquish some knowledge in the area of artillery. All of these matters, to the exception of the last, are treated in the two volumes of the above-mentioned work.

We find some references to these matters, that we emphasize, in the Portuguese Treatise written in the 17th century. In Luís Mendes de Vasconcelos's *Arte Militar*, in 1612, we have allusion to Arithmetic, Geometry, Astronomy, Geography and to the practice of the Military Art[2].

This corpus of treatises reached its greatest splendour in the period after the Restoration, in the above-mentioned work of Luís Serrão Pimentel – *Methodo Lvsitanico de Desenhar as Fortificaçoens das Praças Regulares & Irregulares [Lusitanic Method of Drawing Fortifications of Regular and Irregular military posts].* In this work, the author, who had been the kingdom's chief-engineer since 1671, criticises some European theorizers, such as Blaise François, Count of Pagan, from the so-called second French school of fortification, before Vauban's advent. Pimentel inspired himself in the example from Holland, praising the ability to resist proven by their fortifications before the Spanish. Only after another half century do we see the appearance of an author of similar importance: the already mentioned Azevedo Fortes, also chief-engineer of the Portuguese kingdom since 1719, with his work *O Engenheiro Português [The Portuguese Engineer]*, published in 1728 and 1729. Although he praised Vauban, specially in his first method (initiated in Lille, in 1667), he claims to follow *"(…) hum novo methodo de fortificar as Praças tirado do mesmo Vauban, do Conde de Pagan, e do Cavalleiro de Ville, (…) e soube fazer escolha do que cada hum delles trás mais accomodado à melhor defença, ajuntando-lhe as suas proprias refleçoens militares (…)"* ["(…) A new method of fortifying military posts taken from the same Vauban, from the Count of Pagan and from the Chevalier de Ville (...) and knew how to chose whatever from each one of them was better adapted to the best defence, adding to these his own military reflections (…)"][3]. This method, defended by an anonymous writer, ventured on an aggregate of Vauban, Pagan and Antoine de Ville's ideas. Still, Azevedo Fortes does not shy from mentioning the influence of the Dutch fortifying method in Portugal during the period after the Restoration, stating that even engineers coming from France at the time followed the Dutch method.

During this time, several translations also circulated within Portugal, for instance, of the writings of Deville (1646), Fournier (1649), Antoine de Ville Tolozano (1708), and Mons. Pfeffinger (1713). At the same time, Manesson Mallet published in Paris, in 1671, *Les travaus des Mars*, in which we can find engravings representing fortifications in Portugal up to that period. These works are now part of the valuable collections of some libraries in Portugal, particularly the National Library [*Biblioteca Nacional*] in Lisbon and Évora's Public Library.

The practical application of these treatises as far as construction is concerned can be found in several fortresses in the Southern part of Portugal, particularly Elvas, the "key to the Kingdom"; in this case, worked Cosmander, in 1643[4], Pedro Girles Saint-Paul[5], Nicolau de Langres[6] and another French military engineer, Pierre de Santa-Colomba.

Notwithstanding, the foreign influence would continue in Portugal, as when the Marquis of Pombal solicited, in 1762, the services of Guilherme de Schaumbourg-Lippe[7], who intervened in the construction of the fort of Graça, in Elvas, according to the teachings of Sébastien Vauban; the project was implemented, with some additaments by coronel Guilherme Luiz António de Valleré, until 1792.

Apart from the many projects that came down to us, one of the most interesting iconographic evidence of military architecture and engineering from the modern period in Southern Portugal is offered by the superb glazed tile panels from the Espírito Santo convent, the main building of Évora's University.

The ensemble of glazed tiles, called *"azulejos"*, dating from the last years of the 1730s and early 1740s, was artistically inspired by Dutch models, even though its manufacture was Portuguese. Their application to the building walls resulted from a building campaign under the reign of D. Joao V, and happened at the same time as the application of marble elements and exotic woods. The subject matter of the tiles adorning the rooms around the main cloister of the Colégio do Espírito Santo is very broad; each room develops a specific theme. These range from themes inspired by Classical Antiquity to those conspicuously religious; particular attention is given to the scientific context and to the technological advances from that period; we should highlight the room devoted to Astronomy and Geometry. Not only do we see instruments represented – a quadrant, compasses with straight and curved points, squares, levels – but we can observe their usage and practical demonstration. The importance of calculation and its relation to architecture and military engineering continue in the following panels, in which scenes from an attack to a bulwarked military post are represented; we can notice the positioning of the

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[2] Évora Public Library, N. Res. 1476 – Arte Militar. 1612.

[3] Manoel de Azevedo Fortes, *O Engenheiro Português*, 2º tomo, p. 71.

[4] Cf. Edwin Paar "As fortificações seiscentistas de Elvas e o primeiro sistema holandês de fortificação", in *A Cidade – Revista Cultural de Portalegre*, nº 12 (nova série), 1998; Id., "Fortificações urbanas de Elvas: o melhor exemplo actual da Primeira Escola de Fortificação Holandesa" in Clio*: revista do centro de História da Universidade de Lisboa*, 2006.

[5] In Elvas since 1641, and whose presence in the military post of Almeida would also be noticed.

[6] Who also worked in the fortifications of Évora, Elvas, Estremoz, Campo Maior, Castelo de Vide, Juromenha and Moura.

[7] Know in Portugal as the "count of Lippe", he was Field Marshal of the Portuguese army and commander of the Anglo-Portuguese forces during the last year of the Seven Years War (1756-1763). His actions manifested essentially regarding discipline and the instruction of the troops, hence the publication of the text as *Regulamentos de Infantaria e de Cavalaria [Infantry and Cavalry Regulations].*

army, the observation of the stronghold in order to establish a strategy for attacking it, the trajectory of the attacking projectiles, as well as the defensive response of the stronghold under siege. The coat of arms of the Portuguese Royal House is also present, reminding us of the ubiquity of the monarchy in the great building projects designed for the kingdom. In this case, the implicit presence of D. João V, not only through the royal commission for the tile panels but also in its up-to-date characteristics, reflecting upon the whole kingdom the fact that the king knew the very best of what was happening in Europe concerning science and technique, and their application on bulwarked fortified structures.

On the other hand, all through the remaining rooms we can observe the presence of several military elements – garrisons, forts, gates – and their relation to the surrounding landscape; the armies of the time, their deployment and positioning in the terrain, what weapons they used, are also constant.

In another location within the same building, we find tile panels devoted to the four elements – Air, Earth, Water and Fire – in which we can observe, precisely in the panel devoted to Fire, allusions to the casting of cannons, to pyrotechnics, and to fireworks, as well as the only representation in all the tile panels in the building of individual firearms.

From the entire ensemble, we retain two references that we find essential to consider about the theme we proposed to develop: the circulation of masters and ideas in Portugal in the area of bulwarked fortifications:

- On one hand, the tile panels demonstrate as for their conception a clear predominance of the Dutch school: regarding the landscape elements, the relief, the constant presence of water, always relating to elements of military architecture. Given the date of the panels, some fortifications incorporated into the landscape or near the water lines end up presenting characteristics pertaining to the so-called "transition fortification", retaining semi-circular and cylindrical elements next to angular forms, normally pentagonal, but still without the bulwark, a characteristic element of modern fortification.
- On the other hand, and if indeed the drawing of the panels was inspired by Dutch models, the fact that they were made in Portugal is also synonymous of their acceptance, as the typology of the fortresses represented seems to be a common element of the Portuguese landscape of the time. This acceptance regarding foreign influences continues, in these tile panels, in the representation, already in their latter phase (this one, possibly not only made but designed exclusively by Portuguese) of the Japanese world, their people, their ways and their heritage.

Conclusions

1. Bulwarked fortifications or fortresses "in the modern way" imposed themselves in Europe through different schools and for a long period of time, leading to a whole set of adaptations and options in relation to their original programs, in order to improve them;
2. Portugal, and in particular the Alentejo region, have been the stage, from the 16th to the 18th centuries, for the presence of technicians from the fields of military architecture and engineering, originating from different European countries; these technicians, by the monarch's request, worked and participated in the reinforcement of Portugal terrestrial and coastal borders as well as in some inland settlements, considered to be strategically significant;
3. The analysis of the creation and evolution of the fortified systems, besides giving undeniable evidence of the political situation at the time, the territorial dimensions of the country and its overseas dominions, the technical and military advances, also allows to consider the importance of the legacy that remained;
4. Of this vast heritage, not only should the buildings themselves be acknowledged and preserved but also their remaining images and representations; if ancient cartography and the royal commissions are a particularly important field of study, the evidence provided by glazed tile panels also appears to us as essential for the understanding of foreign influences in Portugal, particularly in the Alentejo.

References

ALBUQUERQUE, Luís de (1986). A Geometria em Portugal no início do século XVIII. Separata de *"Clio", Revista do Centro de História*, vol. 5, Universidade de Lisboa, pp. 89-97.

ALBUQUERQUE, Luís de (1972). *A "Aula da Esfera" do Colégio de Santo Antão no século XVII*. Lisboa: Junta de Investigação do Ultramar.

BARATA, Manuel Themudo; TEIXEIRA, Nuno Severiano (2003). *Nova História Militar de Portugal*. Lisboa: Círculo de Leitores, Vol. I.

BARROCA, Mário Jorge. "Tempos de resistência e de inovação: a arquitectura portuguesa no tempo de D. Manuel (1495-1521)", *http://ler.letras.up.pt/uploads/ficheiros/3875.pdf*, Access January 2009.

BRISAC, Catherine (1981). "La fortification bastionné", in *Grand Atlas de l'Architecture Mondiale*.

BUISSERET, D. (Org.) (1992). *Monarchs, ministers and maps. The emergence of cartography as a tool of government in early modern Europe*. Chicago & London: The University of Chicago Press.

CARDINI, Franco (1992). *La culture de la guerre, Xe-XVIIIe siècle*. Paris: Gallimard.

CARITA, Rui (1986). *A descrição do Reino do Algarve de Alexandre Massai*, 1617-1621.Funchal.

CARVALHO, Ayres de (1977). *Catálogo da Colecção de Desenhos*. Lisboa: Biblioteca Nacional.

FORTES, Azevedo (1728). *O Engenheiro Português*.

MOREIRA, Rafael – "Um tratado português de arquitectura do século XVI (1576-1579)", in Universo urbanístico português (1415-1822). Lisboa, 1998, p. 353-398.

MOREIRA, Rafael (1989). *História das Fortificações Portuguesas no Mundo*. Lisboa: Ed. Alfa.

NUNES, António Lopes Pires (1982). *Arquitectura militar e arte de fortificar*. Lisboa, Instituto de Altos Estudos Militares.

PIMENTEL, Luís Serrão (1680). *Proemio de Methodo Lusitano de desenhar fortificaçoens das praças regulares e irregulares, fortes de campanha e outras obras pertencentes a arquitectura militar, distribuído em duas partes, Operativa e Qualitativa*. Lisboa: Imp. António Craesbeeck de Mello.

VITERBO, Francisco Marques de Sousa (1899-1922). *Dicionário Histórico e Documental dos Arquitectos, Engineers e Constructores Portugueses*. Lisboa, Imprensa Nacional.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 253.

THE TECHNOCRATS AGAINST "TECHNOCRACY": MOTORWAYS AND BOTTLENECKS IN ENGINEERING IDEOLOGIES[1 2]

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Abstract

This paper focuses on the rise and partial fall of the Catalan civil engineers and their heterogeneous technological ideals during the II Spanish Republic (1931-1939). The international exhibition of Barcelona, the international economical crisis and the birth of an autonomous government in Catalonia prompted civil engineers to actively participate in economics and politics.

The study of the Catalan engineering ideals and projects will take account of the local analysis on Technocracy which have recently appeared (e.g., France, Greece, USA), in order to complete the international panorama and to understand the continuous articulation between the national and international issues as "sides of the same coin". Like machines or technical skills, local engineering thought cannot be explained by linear diffusion or "spontaneous generation"; then, it will be necessary to explore specific (socioeconomic, cultural, psychological) contexts.

[1] This paper is a transcription of my communication at the 4th International Conference of the European Society for the History of Science (Symposium "Knowledge and technology in the Mediterranean basin", coord. Àngel Calvo, 20/11/2010). However, some minor changes and bibliography have been included. It is a summary of a chapter of my PhD Thesis, *"Tecnocràcia i Nacionalisme tècnic a Catalunya als anys 1930. Els enginyers industrials, de la matematització de la màquina a la racionalització de l'Estat"* ("Technocracy and Technological Nationalism in Catalonia during the 1930s. The industrial engineers, from the mathematization of the machine to the rationalization of the State"), supervised by Antoni Roca Rosell (UPC) and supported by the Catalan Government (AGAUR, FI-B grant). Besides, this work is part of the research project *"Ingeniería, matemáticas y sociedad en Cataluña y España, siglos XVII-XX"* (Spanish Ministry of Science and Innovation, HAR2010-17461, HIST).

[2] In this paper, I use the capital letter "T" or the lower case letter "t" in aims to distinguish Technocracy, the Technocrat Movement targeted in 1933, from technocracy, a long-term ideology.

"Why should I bother trying to save energy anyway?
We simply have to use less energy, more efficiently.
Most energy experts agree…".

(e·On Advertisement, Green Park Tube Station, London, 11/11/2010)

"There are a large number of different bookkeeping devices whereby the distribution to, and records of rate of consumption of the entire population can be kept. Under a technological administration of abundance, there is only one efficient method—that employing a system of *Energy Certificates*".

(*Technocracy Study Course*, 1934)

Introduction

In August of 1933, industrial engineers in Catalonia publicly expressed their opinion about a powerful word. This word had been shocking the world -and, particularly, Catalonia- for several months. This word directly brought their profession into the focus of public attention but it seemed to disturb them. It was a kaleidoscopic word which circulated so quickly as the Lockheed Vega airplane. The word was: "Technocracy".

In August of 1933, *Tècnica*, the Journal of the Association of Industrial Engineers of Barcelona, dedicated its editorial to the affair. This would turn out to be the first and the last time "Technocracy" appeared in its pages. It seemed to be a metaphor for an ephemeral international debate that rose and fell in 1933. Nevertheless, the ephemeron contained a former reason which would permanently shape governments and societies: more "-cracy" to "Techno-", more government to technicians.

A word in transit

In 1933, at the worst point of the 1929 crisis, a word full of "mystery" (according to contemporaries), produced a "strange furore [sic]", "raising hopes, fears and doubts" [Arkright, 1933; Allen, 1933]. The furor started some months before, when the director of the Energy Survey of North America, Howard Scott, let slip about a smash ideology beyond Capitalism, Socialism, Communism or Fascism in an interview reproduced in the *New York Times* and the *New York Herald Tribune*. Scott claimed: "(America) finds herself in the dilemma of having to desert one system which has become obsolete and simultaneously to design a system to take its place" [quoted in: Allen, 1933].

"Technocracy" appeared like mushrooms in magazine articles and newspapers columns [Adair, 1970]. The Continental Committee on Technocracy, the group of technologists led by Scott, defined its methods as the "result of a synthetic integration of the physical sciences that pertain to the determination of all functional sequences of social phenomena" [Scott, 1933]. And, they proclaimed to have found a scientific technique of making decision when "no political method of arriving at social decisions (was) adequate" [Scott, 1933].

A huge debate was animated by enthusiasts, reformers and critics of the radical theory, by the Right and the Left, by women and men, by specialized experts and generalist speakers. Obviously, in these attempts to popularize it, the word was changing in meaning to serve the visions of the popularizers.

Nevertheless, the continuously reformulation of the ideas was integrated in a former process. The word was coined in 1919 by the Californian engineer William Henry Smyth, and Howard Scott and Co appropriated it. At the same time, the redefinition took into account other early economic criticisms, e.g., Veblen's or Frederic Soddy's. Moreover, the scientific framework came from long ago, especially from late nineteenth century thermodynamics [Rabinbach, 1990]. Thus, there was no real novelty in Technocracy: neither in the container nor in the content. But, the globalized economic struggle and the social hopelessness gave a good opportunity for this astonishing lexicon to gain political ground.

In North-America, Technocratic associations and committees grew up and joined up marginally employed people (with high number of women involved). Supporters even imagined a utopian "life in a Technocracy" where, for example, adultery or jealousy had no place [Loeb, 1933]. However, the movement never rose broadly. The main US engineering associations did not support them, nor did entrepreneurs and politicians, nor the Left wing [Akin, 1977].

"Technocracy" soon arrived on the other side of the Atlantic. England seems to be the first reception *nest* of these ideas from the New World. But the European focal point was Geneva, during the International Conference on unemployment. Spanish newspapers picked up on the discussions in Geneva. One week after, the most recognized Catalan engineer at this moment, the military engineer Marià Rubió i Tudurí, was one of the first voices to open the discussion in Catalonia. He wrote in the conservative newspaper *La Vanguardia* (22/01/1933): "Technocrats seem inclined to become a sect". More benign was Pedro Gual Villalbí, the secretary of the employers' association in Catalonia, Foment del Treball, and member of the Francoist National Economic Council just after the Civil War (1936-1939): "It is not a tendency to neglect; who knows if it contains the germ of principles for the future organization of society" (*La Vanguardia*, 06/05/1933). But he was the only public person who wanted to be identified with the Technocratic theories. Fierce contrary opinions were reflected in magazines and newspapers related to very different ideological tendencies, such as catholic Catalanism (*L'Esplai*), humanist Catalanism (*La Revista*, *Mirador*), Spanish Ultraconservatism (*Acción Española*) or Anarchism (*La Revista Blanca*). Everybody took Technocracy to be an attack to their particular projects of nation.

All over Europe, several books about Technocracy were published in 1933 –and not beyond this year. And Spain was not an exception. The *Revista de Occidente*, edited by Ortega y Gasset, translated Raymond Allen's *What is Technocracy?* And Allen's book became the best-known text about the old-new word in Spain. Nevertheless, there were also other attempts to popularise, discuss and critique the term. The only Spanish book specifically focussed on Technocracy was written by Eduardo Llorens, a scholar of the University of Murcia. He pointed out "the wrong starting point and the dismal failure of the methodology of the Technocrats" [Llorens, 1933]. Despite there was no evidence of real or potential support for Technocracy in Spain, more than one hundred pages were dedicated to this "failure". In addition to French, German and English texts, Spanish readers could also refer to a Columbian work, by Jorge Álvarez Lleras, who tried to link Technocracy with the theories of economist Julio Garavito Armero. But what public impact were specialist books having? Álvarez Lleras himself highlighted the limited reach of his own work [Álvarez, 1933].

One of the groups that could have participated in the social discussion with expert language (academic sphere) as well as with lay language (public sphere) was the Catalan industrial engineers. They had become the most reputed and organized professionals of technicians in the beginning of the 1930s [Lusa, 2006]. They represented the "applied scientists" that were placed in the midst of the debate [Nolan, 1997]. Moreover, they aimed to become the new "third class", between workers and employers, beyond politicians. Obviously, they had to know about Technocracy. But, publicly, they remained silent for several months. Actually, the library of the Association of Industrial Engineers doesn't preserve any copy of publications on Technocracy, nor internal documents, nor discussions in the assemblies of the Association, nor other evidences. But, the multidirectional transit of knowledge and ideologies –despite being hidden to recent historians– were clearly tangible to individuals at this time. The circulation of Technocratic ideas and ideals had mainly been supported by people who did not support them and the best way to block this centrifugal circuit was to neglect them.

Technocracy in *Tècnica*

Finally, in August, *Tècnica* broke the silence. Its editorial did not refer to the initial focus point in the US nor specific opinions, but to an ambiguous generalist idea around it. In response to the "comments" on Technocracy, the industrial engineers claimed: "Politics is AN ART [sic], and, then, it has no discipline but the weighted sense. On the other hand, it is forbidden to speak about politics in these pages". But it immediately continued with these words: "There is a lack of responsibility of government when it forgets corporatist technicians".

From the beginning of the editorial section, the introductory pages contained plenty of politics about "The industrial engineers in front of the Autonomous Statute of Catalonia" (September 1932) or "The engineers and politics" (January 1933) where engineers celebrated their access of engineers to high-ranking offices in the Catalan Parliament. Then, they did not mind speaking about engineering and politics: they rejected the use of Technocracy, likewise they had rejected to talk of other radical visions of technicians' empowerment, e.g., Le Corbusier's, Jacques Duboin' or Salvador Dalí's [Corbusier, 1933; Duboin, 1932, 1935; Dalí, 1928]. And the final cause is not related to the disagreement with some internal meanings of the word, but to the agreement with others. A lot of common language was shared by the faithful to Technocracy and the technocrats against Technocracy, such as governmental efficiency, abundance of material goods or improved distribution.

Engineers wanted to maintain their image of neutrality, objectivity and scientificity beyond the future changes of governments and the political ideologies in conflict. Curiously, the same image which had feverishly strengthened the new word (Technocracy) was being challenged by the word itself. The more engineers denied participating in politics, the more they could participate in politics.

Catalan industrial engineers were not the "baffled, unemployed mass of technicians who (had) been deprived of their functions by the decay of capitalism", as north-American Marxists characterized their compatriots [Foster, 1933]. Catalan industrial engineers had reached a public acknowledgment as well as an urban spatial position. They were part of the elite during the thirties, as I argued in some unpublished papers [Valentines, 2009, 2010]. In these years, the Association of Industrial Engineers was consolidated and had a well-defined project. Moreover, during the first years of the II Spanish Republic (1931-1939), the establishment of new autonomy for the Catalan territory permitted redefinitions in power correlations. One after another, the requests of industrial engineers were accomplished; the legislation of these years gives us evidence of the engineers' project of modernization, concerning technical offices, territorial planning, hydroelectric survey…: a project closely linked to the construction of the Catalan nation from long ago [Roca, 1988].

Last but not least, the Association alleged that the current political system –actually, any political system, except Technocracy– integrated within the Catalonian Statute could be acceptable as long as they had a firm position in the organization of the State: engineers were servants of the II Republic as long as the II Republic was servant of them. The editorial of July 1933, just the one before the editorial about Technocracy, was clear: "A lot of roads lead to Rome, and it can be added that we can go by different vehicles; by means of very different political systems (from English democracy to German Hitlerism), we can see how economic struggles are solved in every nation".

Nevertheless, the political neutrality of the Association was an illusion, a construction to unify very different professionals. It contrasts with the very different political and economic visions of them. In fact, some of the members of the Association had a very active role in the economic debates in Catalonia. In Spain, economics was a discipline in process of consolidation within the academic system. Historically, despite other specific institutions, the School of Industrial Engineering –the centre related to the Association– had provided the knowledge and skills to be an economist [Artal, 1976]. Then, the engineer and the economist usually were the same person.

Some engineers exalted Capitalism, such as Josep M. Tallada, who wrote a very well-documented book against the Soviet State [Tallada, 1935]; some others embraced Socialism, such as Estanislau Ruiz Ponsetí, who had participated in the Technicians Union of Catalonia from the twenties and who became one of the leading lights of the New Economy during the Civil War [Ruiz, 1937]; others were attracted by Italian fascism or by the reactionary modernism of the Roosevelt government. But, all reclaimed organization for the State and high-qualified experts for the organization. In fact, far beyond the theoretical corpus by Howard Scott, technocracy is a "transversal ideology", as Frank Fischer notes [Fischer, 1990]. In fact, such as productivism –the other parent of the 20th century modernity–, technocracy is "political promiscuous" [Rabinbach, 1990; Nolan, 1997].

Actually, some months after the rejection of the Technocrat movement, the Association started to prepare a set of economic lectures to debate the best structure to organize the state. Finally, in 1936, these were materialized. Technocracy, of course, was absent. For the first time, the Association conferences were broadcasted by Barcelona radio with the aim of improving "the education of our nation", it was written in *Tècnica* (April 1936). Nevertheless, the filo-fascist Antoni Robert claimed in his conference: "It is up to us, Engineers [sic], we are called to write the economic standing of our nation. We hope that our action will rise to the occasion" (*Tècnica*, May 1936).

As it was proclaimed in 1941, despite the defeat of Technocracy alternative debate, Technocracy triumphed [Akin, 1977]. The related consequences of 1929 and World War II gave the opportunity for the technicians to implement technocratic projects in very different political systems. The Greek "Technical State" (1931-1936) and the Vichy Republic seem to be the most radical, as Yiannis Antoniou and Antoine Picon have pointed out [Antoniou, 2007; Picon, 2007]. Nevertheless, the cases of the USA and Great Britain were more consolidated, as they have been studied by William Akin, David Edgerton, Michael Weatherburn [Edgerton, 2006]. The Spanish Civil War and the revolutionary context on the Republican side let some engineers, architects and technicians to think that a new era was starting: "the moment to implement the research results made by modern technicians to organize the new society" ["És el…", 1937].

Epilogue

The circulation of science and technology not only involves texts or practices, written or tacit knowledge, instruments or machines, but also the deep social ideologies inserted into them. Nevertheless, this core piece of science and technology usually has been overlooked by recent historians of scientific popularization, except in some case studies [Pohl-Valero, 2008]. These ideals are carried out on a broad "road network", with very different avenues, directions, traffic signals, vehicles and drivers. Transport –any transport– is regulated according to these factors as well as the merchandize being carried.

It can be noted that non-appropriation of arguments and technology can alter the traffic as much as appropriation. Appropriation and non-appropriation are always in tension, just like the public and non-public relationship.

Technocracy movement was very ephemeral, inconsistent: a bluff, at least in Europe. However, it permits historians to dialogue with the ideologies "in transit" about technology during the thirties and the actors involved in them [Secord, 2004].

References

ADAIR, David (1970). *The Technocrats 1919-1967: A case study of conflict and change in a social movement.* A thesis submitted in partial fulfillment of the requirements for the degree of Master of Arts in the Department of Political Science, Sociology and Anthropology, Simon Fraser University.

AKIN, William Elmus (1977). *Technocracy and the American dream. The Technocrat Movement, 1900-1941*. University of California Press, Berkeley, London.

ALLEN, Raymond (1933). *What is Technocracy?* McGraw-Hill Book Co., New York, London.

ÁLVAREZ LLERAS, Jorge (1933). *La Tecnocracia y sus conclusiones: cuestiones económicas y sociales de actualidad*. Cromos, Bogotá.

ANTONIOU, Yiannis; ASSIMAKOPOULOS, Michalis; CHATZIS, Konstantinos (2007). "The National Identity of Inter-war Greek Engineers: Elitism, Rationalization, Technocracy, and Reactionary Modernism", *History and Technology*, 23:3, 241-261.

ARKRIGHT, Frank (1933). *The ABC of Technocracy. Based on authorized material*. Harper & Bros., New York & London.

ARTAL, Francesc *et al.* (1976). *El Pensament econòmic català durant la República i la Guerra. 1931-1939*. Edicions 62, Barcelona.

CORBUSIER, Le (1933). *La Ville Radieuse. Eléments d'une doctrine d'urbanisme pour l'équipement de la civilisation machiniste*. Ed. de l'architecture d'aujourd'hui, Boulogne.

DALÍ, Salvador; MONTANYÀ, Lluís; GASCH, Sebastià (1928). [*Manifest groc*]. Fills de F.Sabater, Barcelona.

DUBOIN, Jacques (1935). *La Gran revolució que ve...* Proa, Badalona.

DUBOIN, Jacques (1932). *La Grande relève des hommes par la machine*. Ed. Nouvelles, Paris.

EDGERTON, David E. H. (2006). *Warfare state: Britain, 1920-1970*. Cambridge University Press, Cambridge.

"És el moment" (1937). *A.C. Documents d'activitat contemporània*, 25, June, 3.

FISCHER, Frank (1990). *Technocracy and the politics of expertise*. Sage, London.

FOSTER, William Z.; BROWDER, Earl (1933). *Technocracy and Marxism* (with "The Technical Intelligentsia and Socialist Construction" by V. M. Molotov). Workers Library Publishers, New York.

LLORENS, Eduardo (1933). *¿Qué es la Tecnocracia?* Revista de derecho privado, Madrid.

LOEB, Harold (1933). *Life in a Technocracy. What it might be like*. Viking Press, New York.

LUSA MONFORTE, Guillermo (2006). "La Escuela de ingenieros de la dictadura a la República (1927-1936)", *Documentos de la Escuela de Ingenieros Industriales de Barcelona*, 16, 3-119.

NOLAN, Mary (1997). "Productivism and Technocracy in Historical Perspective". In: MYKLEBUST, Sissel (ed.). *Technology and Democracy: obstacles to democratization. Productivism and Technocracy, proceedings from Workshop 3*. Centre for Technology and Culture, Oslo, 149-169.

PICON, Antoine (2007). "French Engineers and Social Thought, 18-20th Centuries: An Archeology of Technocratic Ideals", *History and Technology*, 23:3, 197-208.

POHL-VALERO, Stefan (2008). "The Circulation of Energy: Thermodynamics, National Culture and Social Progress in Spain, 1868-1890". In: PAPANELOPOULOU, Faidra; NIETO-GALAN, Agustí; Enrique PERDIGUERO (*eds.*). *Popularizing science and technology in the European periphery, 1800–2000*. Aldershot.

RABINBACH, Anson (1990). *The human motor: energy, fatigue, and the origins of modernity*. Basic Books, New York.

ROCA ROSELL, Antoni (1988). "Ciencia y sociedad en la época de la Mancomunitat (1914-1923)". In: SÁNCHEZ RON, José Manuel (*ed.*). *Ciencia y sociedad en España*. El Arquero/CSIC, Madrid, 223-252.

RUIZ I PONSETÍ, Estanislau (1937). *Les empreses col·lectivitzades i el nou ordre econòmic*. Edicions U.G.T., Barcelona.

SCOTT, Howard (1933). *Introduction to Technocracy. By H. Scott and others etc*. John Day Co., New York.

SECORD, James A. (2004). "Knowledge in Transit". *Isis*, 95, 654-672.

TALLADA, Josep M. (1935). *L'organització econòmica a la Rússia soviètica*. Lliga Catalana, Barcelona.

VALENTINES ÁLVAREZ, Jaume (2009). "Identitats a l'enginyeria i enginyeries a les identitats a l'esclat de la Guerra Civil. Catalanisme tècnic i tecnologia ideològica". Paper presented at: VII Jornades Memorial Democràtic a la UPC. Tècniques, Medicines i Ideologies abans de 1939, Barcelona, December 3rd.

VALENTINES ÁLVAREZ, Jaume (2010). "Geografía profesional en la Barcelona de 1930". Paper presented at: 6è Seminari d'estudiants d'història de la CTM. La tuberculosis y el espacio urbano barcelonés entre 1929 y 1936, Bellaterra, May 5th.

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FRANCESCO MAUROLICO AND THE RESTORATION OF EUCLID IN THE RENAISSANCE

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Abstract

The first printed edition of the Elements*, based on the medieval recensio of Campanus of Novara, appeared in Venice in 1482; some years later, in 1505, Bartolomeo Zamberti published a new (and quite different) translation based on a Greek code. The contemporaneous availability of two Latin editions –we could say two* editiones principes– *of the* Elements*, both unsatisfactory for different reasons, gave rise to different reactions among the European mathematicians: some of them embraced the cause of Campanus, some others the cause of Zamberti, and others rejected both Campanus' and Zamberti's redactions. In the last case, the absence of an established, shared and trustworthy Euclidean text, let some mathematicians write 'their'* Elements*. In this paper I describe the main features of the transmission of the* Elements *in the early Renaissance Europe and I focus my attention on the figure of the mathematician Francesco Maurolico (1494-1575). Maurolico (*http://www.dm.unipi.it/pages/maurolic/intro.htm*) was very unsatisfied with the available editions of Campanus and Zamberti and in a letter dated 1532 he announced an original publication programme about Euclid's* Elements*, founded on the following essential points:* 'emaculare'*, or to correct the available editions by mathematical mistakes,* 'reddere faciliorem'*, or to make easier, if possible, the Euclidean proofs;* 'coaptare' *or to choose every time the best logical architecture, the best proof, the best language between the two editions. In other words, he composed a new text coming from the joining of the two Renaissance traditions with many additions of his own. The* Elements "ex traditione Francisci Maurolyci" *–which are going to appear in the volume* 'Elementa geometriae' *of the* Edizione Nazionale dell'opera matematica di Francesco Maurolico– *were only partially published in 16th century, nevertheless it's possible to detect some influence in Clavio's fundamental recensio of the* Elements *(1574) and in Borelli's* Euclides restitutus *(1658).*

The first Euclidean project

In 1528, the Sicilian mathematician Francesco Maurolico (1494-1575) lectured on Spherics and Euclid's *Elements* on behalf of the Senate of Messina, his hometown. As he explained in a subsequent dedicatory letter to his patron, dated 1532, the unsatisfactory level of the available editions of the *Elements* convinced himself to prepare a new edition of the Euclidean text.

In the first decades of the Sixteenth century the editions of the *Elements* available to the scholars and known by Maurolico were essentially two: the *editio princeps*, printed in Venice in 1482 by Erhard Ratdolt and based on the medieval version of Campanus from Novara, and the Venetian edition of 1505, based instead on the translation of a greek code, made by the humanist Bartolomeo Zamberti. Maurolico surely knew also the version edited by Jacques Lefevre d'Etaples (Jacobus Faber Stapulensis), printed in Paris in 1516 and followed by numerous reprints and editions. In several documents, unfolded over an extended period of time, Maurolico expressed quite severe criticisms against Campanus and Zamberti. Actually, the medieval *recensio* showed additions, changing of definitions or differences in numbering sometimes questionable, while the translation by Zamberti mercilessly highlighted the very poor geometrical talent of the Venetian humanist. Finally Stapulensis, instead of critically merging the two traditions, simply juxtaposed the two texts, in the vain attempt to balance their shortcomings.

Faced with this situation, Maurolico set out to produce an edition of the *Elements* which was able to collect the best of the known traditions, possibly supplemented by his original contributions, in order to simplify and shorten the Euclidean proofs.

The guidelines of this first Euclidean project, which appears to be regardless of any literary integrity of the text or any philological respect, reveal instead a clear orientation to its mathematical restoration, a tendency which is after all shared by all the editions of the Classics that Maurolico was able to complete; it is not a chance that the titles of these works are always followed by the quote *«ex traditione Maurolyci»*, which connotes the works of Maurolico rather than that of any author treated[1]. As P. L. Rose has written, «to Maurolico's mind … there was nothing wrong with correcting classical treatises, as long as it be done by an expert mathematician. Nonetheless, Maurolico was certainly the most radical of Renaissance mathematicians in this respect».[2]

Announced in 1532 and developed, at least partly, in the following decade, the programme of restoration of the *Elements* would remain unfinished, but in the second half of the Sixties it picked up again, taking the form of an abridged edition of the *Elements*.

In this contribution we provide a brief description of the extant texts (§2) and, after analysing their most relevant mathematical features (§3), we try to trace the evolution of the Euclidean project by the Sicilian mathematician (§4), highlighting the problems that are still open (§5).

The extant Euclidean texts

The collection of Maurolican writings related to the *Elements* includes documents of various kinds: printed editions, holograph drafts, re-readings, compendia and numerous related fragments. A chronological ordering of the survived material leads, in the first instance, to classify the writings into two major groups: the texts written during the years 1532-1541 and the compendia of the years 1563-1567.

The various autographic writings dating back to the decade 1532-1541 witness a real dedication spent by the author in the construction of a new edition of the *Elements*. In fact, we have the following texts:

- a draft containing the first ten propositions from Book II, retained in the Ms. San Pantaleo 115 of the *Biblioteca Nazionale* of Rome, ff. 21*r*-22*v*, dated January 21, 1532;
- the drafts of Book V (November 5, 1534) and Books VII, VIII, IX (November 9, 14 and 19, 1534, respectively), corresponding to the folia 1*r*-39*v* of Ms. San Pantaleo 116;
- the draft of Book X (August, 1541), retained in the same code (ff. 40*r*-107*v*).

At present, we do not have any news concerning the missing drafts of Books I-IV, VI, XI-XII.

The extant texts are written with care and show just a few erasures. Maurolico's attention is focused on the proofs, since definitions and postulates are completely absent. Propositions, although numbered in the margin, are lacking in the statements. In this group of writings, the edition of Books XIII-XV of the *Elements* must also be considered, drawn at least in its essentials in 1532 and published posthumously (1575) in the miscellaneous volume titled *Opuscula Mathematica*; it is the only printed document provided by Maurolico about his plan of restoration of Euclid.

The second group of Euclidean writings consists of the following texts:

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[1] Concerning Maurolico and the restoration (*restitutio*) of the Classics, see the papers by P.D. NAPOLITANI, Maurolico e Commandino, in *Il Meridione e le scienze*, edited by P.NASTASI, Palermo 1988, 281-316; Le edizioni dei Classici: Commandino e Maurolico, in *Torquato Tasso e l'Università*, edited by W. MORETTI e L. PEPE, Olschki, Florence 1997, 119-141.

[2] P.L.ROSE, *The Italian Renaissance of Mathematics*, Genève, Droz, 1975, 166.

- the compendia of Books XI and XII of the *Elements*, dated December 3, 1563 (ff.34*v*-39*v* of Ms. San Pantaleo 115)
- the compendia of Books I-X, written from January 28 to March 11, 1567 (ff. 2*r*-43*r* of Ms. Lat. 7463 of *Bibliothèque Nationale de France* in Paris).

The previous drafts appear fairly accurate on the whole, presenting few corrections and some additions in the margin. The only exception is the draft of Book X, characterized by successive additions and re-workings.

Maurolico interpreter of Euclid

1. The re-readings of the Thirties-Forties

The aforementioned dedicatory letter of 1532, which accompanies the printed edition of Books XIII-XV of the *Elements* (1575), ends with the promise of a global edition of the Euclidean work inspired by specific criteria: *emaculare*, that is to clean and correct the numerous errors of the printed editions, *reddere faciliorem*, that is to simplify and shorten, where possible, the Euclidean proofs; *coaptare*, i.e., each time choosing, among the known editions, the most streamlined logical architecture, the most convincing proof, the most suitable terminology. It is not excluded, in particular cases, that the existing proofs would be replaced by new Maurolican proofs.

The drafts of Books V and VII-X of the *Elements* dating from 1534 to1541, perfectly fit into this program, because they are a critical re-fusion of the Greek tradition with the Arab-Latin one, supplemented as necessary by the original contributions of Maurolico, who added, shortened and in case omitted proofs. To better understand what is meant by this statement, we see a little closer the Maurolican reworking of the theory of proportions, i.e. Book V of the *Elements*, starting from a brief description of the versions provided by Campanus and Zamberti.

Book V *ex traditione Campani* differs substantially from the translation by Zamberti in the following points: a different (and controversial) definition of proportionality[3], the inversion of two propositions (the 12th and 13th) and the addition –in the medieval recensio– of nine final propositions on the properties of inequalities between ratios. Maurolico does not slavishly follow one or another tradition, but tries to recast them critically. His draft, although free of definitions, clearly shows to adopt the definition of proportionality of the Greek text. The Sicilian mathematician also respects the 'greek' sequence of propositions, but does not hesitate to join eight of the nine Campani additiones concerning the inequalities between ratios. As regards the lexicon, as a rule Maurolico shows a preference for the one of the Venetian humanist Zamberti, but also partly follows the terminology of Campanus, for example to indicate some manipulations on the proportions.

Finally, when the proofs of the same proposition differ from each other, Maurolico tends to favour the version proposed by Campanus rather than the one by Zamberti, although in few cases opts for a personal solution.

The problem of restoring Greek mathematics, to Maurolico's mind, becomes the problem of establishing a comprehensive and mathematically reliable text. The Sicilian mathematician does not arise philological scruples, being convinced of the impossibility to really restore the true Euclidean text, so full of mistakes "of scribes and translators that Euclid himself, returned to life, could nor purge their works".

2. The **Elementorum compendia***: a reworking of the* **Elements** *in arithmetical key*

The impression we can get even from a superficial reading of the Euclidean compendia of the Sixties, is that of a clear distinction between the first four books and the next ones. In fact, while the compendia of the books dedicated to plane geometry (I-IV) are not significantly different from the tradition of the *Elements*, the remaining ones are reinterpreted according to a strongly arithmetical perspective.

The previous rigid dichotomy does not regard the compendium of Book II, which has always been interpreted by Maurolico according to this peculiar point of view. Actually, all the Maurolican survived drafts of Book II, including that of 1532, provide arithmetical alternatives to the traditional geometrical proofs of the first ten propositions. However these proofs are not completely original, because they are based, as stated by Maurolico himself in a scholium of Book IX, on the topics of the Campani adnotationes to proposition IX.16, which interpret in a numerical key the first ten propositions of Book II.

On the contrary, starting from the epitome of Book V, i.e. from the theory of proportions between magnitudes, Maurolico distinguishes himself by a highly personal approach, in which the definition of proportionality is the kernel of the theory, favouring the comparison of ratios rather than equimultiples.

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[3] For a framework of the discussion on the theory of proportion in the early sixteenth century, see E.GIUSTI, *Euclides reformatus. La teoria delle proporzioni nella scuola galileiana*, Turin 1993.

The Sicilian mathematician constructs a general theory of proportions between magnitudes trying to bring the irrational ratios to rational ones, proving that the former can always be 'contained' in an interval bounded by the latter. In this way, the author tries – even if with some weaknesses– to ensure the generality of the theory by actually studying only the properties of rational ratios. It becomes immediately evident that the propositions in Book VII of the *Elements*, dedicated to proportions between numbers (VII.4-19), appear completely redundant to Maurolico, which summarizes them in one proposition only: "It has been proved in Book V everything regards ratios and proportions in general; this also holds for numbers".

Maurolico, however, does not restrict himself to eliminate this group of propositions from the arithmetical books, but he completely re-arranges their logical architecture. It should be noted, indeed, that the arithmetical books especially apply to be rearranged, because they are nearly free from a hierarchical logical-deductive structure, being rather grouped –especially Book VII– in main themes. Let us briefly see how Maurolico alters the structure and contents of the arithmetical books.

The central problems solved in Book VII, according to the Euclidean tradition, are the determination of the greatest common divisor and least common multiple of two and then three integers (VII.1-3, VII.34-36), and moreover the characterization of representatives of a given ratio and reduction to minimum terms (VII.20-22, VII.33). Instead, in the compendium of Book VII, Maurolico maintains the propositions regarding the determination of greatest common divisor and anticipates –from Books VIII and IX, respectively– some properties of plane and solid numbers, and of a geometrical progression. In this context, he inserts the following proposition that relates the algebraic powers to terms in continuous proportion: "The unit, the unknown, its square, its cube and its square-square are in continuous proportion" *(«Unitas, radix, quadratus, cubus et secundus quadratus, sunt continue proportionales»*, VII.8). This is a reminder of some interest, since Maurolico also left us a brief treatise on algebra[4] actually closely linked to this result, which allows him to present an equation as the equality of side powers of the unknown[5] and reduce the traditional six *«regulae algebrae»* to just four.

In the compendium of Book VIII, Maurolico inserts the propositions excluded from Book VII (that is the propositions mainly related to prime numbers) and the first 27 propositions of Book VIII, concerning the determination and the study of continuous proportions, the characterization of square and cubic numbers, plane and solid numbers.

The compendium of Book IX opens with six propositions devoted to the square and cubic numbers, and after propositions 7-21, which are not confirmed in the Euclidean *Elements*, the epitome concludes with the famous result on perfect numbers: «If as many numbers as we please beginning from a unit continuously be set out in double proportion, until the sum of all becomes first, and if the sum into the last multiplied make some number the product will be perfect» (proposition IX.23 *«ex traditione Maurolyci»*, i.e. proposition IX.36 of the *Elements*). Some applications regarding continuous proportions and most of the results related to prime numbers are then removed from the compendium, as well as the treatment of even, odd, even-times even and even-times odd numbers. Therefore, as it is evident, Maurolico deeply modifies the Euclidean arithmetical books, both in content, in methodological approach and logical architecture.

Maurolico and the educational project of the Jesuits

In the late Forties, the Jesuit Jerónimo Doménech, with the help of Ignatius of Loyola and the Viceroy of Sicily Juan de Vega, founded in Messina the Collegium primum ac prototypum with the purpose of creating a model of Jesuit University opened to external students. According to the intent of Doménech, the Collegium had to be the University of Messina, but managed by the Society of Jesus, which had to plan the courses and organize the lectures.

For about five years, i.e. from 1548 to 1553, the Collegium of Messina essentially supplied elementary and secondary teaching. Due to a series of clashes that arose between the Jesuits and the Senate of Messina, the project was not able to definitively take off and the educational activity of the Collegium was considerably reduced but, from 1564, a renewed understanding led to the activation of the Studia superiora, within the framework of the genuine *Ratio studiorum*[6]. The need to realize a new educational system related to the advanced courses, and the intention of involving the local scholars in the educational project, led the Jesuits of Messina to strengthen the relations with Francesco Maurolico.

The terms of the cooperation offered by the old mathematician to the Jesuits, were gradually extended from occasional advices to the development of an ambitious editorial plan for arranging all the sciences in an encyclopedia

[4] The electronic edition of *Algebra* is available on the website of the *Progetto Maurolico* (www.maurolico.unipi.it)

[5] Given the scale of algebraic powers, Maurolico considers 'side powers' of an assigned one, the equidistant powers from it.

[6] On the vicissitudes of the *Collegium primum ac prototypum* of Messina and the role of the Mathematics teaching, see in particular the contributions by R.MOSCHEO, *I Gesuiti e le matematiche nel secolo XVI. Maurolico, Clavio e l'esperienza siciliana*, Messina 1998, and by A. ROMANO, Il Messanense Collegium Prototypum Societatis Iesu, in *Gesuiti e università in Europa* (*secoli XVI - XVIII*), edited by G.P.BRIZZI, R.GRECI, Bologna 2002, 79-94.

structured in compendia. Such compendia were to be used in the teaching of the Collegium of Messina and, possibly, in every *Collegium* of the Society.

If one considers the chronology of Maurolico's writings, in the second half of the Sixties a significant increase of scientific activity can be registered, mainly oriented to the redaction of epitomes. In particular, in 1567 Maurolico wrote the Euclidean and astronomical compendia, which fell within the basic teaching of the Jesuit Universities (and not only). To be precise, in the early 1567, he completed the compendia of the first ten books of the *Elements* and reworked a text of some years earlier (1563), dedicated to stereometry, transforming it in the compendium of Books XI and XII. To conclude, he then wrote that the old edition of Books XIII-XV – the one dated 1532, to be clear – added to the most recent epitomes, would have finally completed the whole *Elementorum compendia*.

In April 1569, Maurolico vainly tried to advocate at the Superior General of the Society the publication of the compendia, emphasizing their general utility and need[7].

Unfortunately, the editorial project wrecked and the old mathematician, now seventy years old, had several writings left, ready for publication. The few extant documents, show that in this same period, precisely in 1570, Maurolico invested all his remaining energies in a new editorial operation, probably born from the ashes of the just faded one, which had to end, after many vicissitudes, with the publication of the *Opuscula mathematica*, which occurred in 1575, just few days after his death. Among the published opuscula, we find, surprisingly, the edition of Books XIII-XV of the *Elements*, without any trace of the remaining compendia.

The presence of the edition of the only Books XIII-XV and the subsequent exclusion of the compendia of the first twelve books, appears to be very astonishing, given the explicit will of Maurolico to avoid the separation of the compendia. The fact that his will has been disregarded would lead us to think that, unless some sudden rethinking of the author, there may have been some external interference, not yet identified, in the selection of the texts to be published in the *Opuscula*[8].

Concluding remarks

In 1528 Francesco Maurolico gave lessons on the *Elements*; in the same year, in the preface of his grammar textbook *Grammaticorum rudimentorum libelli sex*, he wrote to be ready to lay out a full scale programme for the renaissance of Greek mathematics. The first step of his ambitious plan was the restoration of the *Elements*, so to have a new edition *«ex traditione Maurolyci»* able to replace the worst printed editions. In the following years he reworked Books XIII-XV and in 1534, over a period of intense arrangement of his writings, he prepared a nearly final version of Book V and arithmetical books. Few years later he devoted himself to writing Book X.

The Maurolican editions of 1530's and 1540's essentially appear to be a critical merging of the traditions by Campanus and Zamberti; nevertheless, sometimes the author does not hesitate to replace Euclidean proofs with more easily intelligible ones (from his point of view, of course) or to add new propositions when he thinks incomplete the Euclidean theory. Although these writings are clearly not based on any kind of philological restoration of Euclid, they are not significantly far from the spirit of the *Elements*.

Unfortunately, at this time we are unable to establish whether the editions of the 'missing' books of the *Elements* –i.e. Books I, III, IV, VI, XI and XII– are lost, nor to categorically exclude that they have never been written. Anyway, we can say, with a certain validity, that the Euclidean edition of the years 1532-1541, although limited to few books, is not, as might appear at first sight, an unused work set aside until a future completion. Maurolico, in fact, considered and used these writings the same way as a real edition of the Euclidean work, far superior to those available concerning what regards mathematical rigour and completeness of treatment. To take an amazing example, in his *Arithmeticorum libri duo* (printed posthumously in 1575 together with *Opuscula Mathematica*) Maurolico sometimes cites Euclidean propositions existing only in his edition *«ex traditione Maurolyci»* of the Thirties.

While the drafts of the Thirties follow in the wake of existing Euclidean traditions, the successive compendia of the Sixties are not only a reasoned synthesis of the *Elements*, because they are completely influenced by a deep arithmetical interpretation of the topics, from the theory of proportions to the relationships between regular polyhedra. Following this peculiar approach, Maurolico often chooses to replace, where possible, geometrical proofs with arithmetical ones. The compendia of the fifteen books of the *Elements* –grouped on the basis of the autograph instructions left by Maurolico– show tracts of deep originality, but at the same time they constitute a very heterogeneous work, as their complex genesis testifies.

[7] See P.D'ALESSANDRO, P.D.NAPOLITANI I primi contatti fra Maurolico e Clavio: una nuova edizione della lettera di Francesco Maurolico a Francisco Borgia, *Nuncius*, 16 (2001), pp. 511-522.

[8] In this regard, we would remember that Rosario Moscheo has highlighted the role played by the Jesuit Vincenzo Le Noci in promoting the publishing venture culminated in the print of the *Opuscula* (MOSCHEO, *I Gesuiti e le matematiche…*, p.221 and following). It is plausible that Le Noci may have influenced Maurolico also in the choice of the writings to be published.

The edition of Books XIII-XV, for example, seems to adhere more to the editorial criteria of the Euclidean project of the Thirties, rather than to the style of the compendia of the Sixties.

The Euclidean writings as a whole, set different kind of problems: although the textual tradition is rather poor, the edition of these works has to include partial redactions of the *Elements «ex traditione Maurolyci»*, compendia written on different periods and many related fragments. The difficulty of placing such different texts into a coherent framework does not only regard the editorial aspect, but also and especially the interpretative one. The crucial points are, first of all, a reliable reconstruction of the two different Euclidean projects undertaken by Maurolico, and a precise evaluation of the actual influence of the Euclidean edition on his scientific production, especially concerning the close relations existing among arithmetical and Euclidean writings.

The first step to reach this aim is the critical edition of the Euclidean texts, which will be published within the National Edition of Maurolico's mathematical works (*Francisci Maurolyci Opera Mathematica*); anyway, a first transcription of such texts is available in the website of the *Progetto Maurolico* (*www.maurolico.unipi.it*).

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 265.

PLAYFAIR'S *GEOMETRY* CROSSES THE MEDITERRANEAN: TRANSLATION OF AN ENGLISH GEOMETRY TEXTBOOK INTO ARABIC

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Abstract

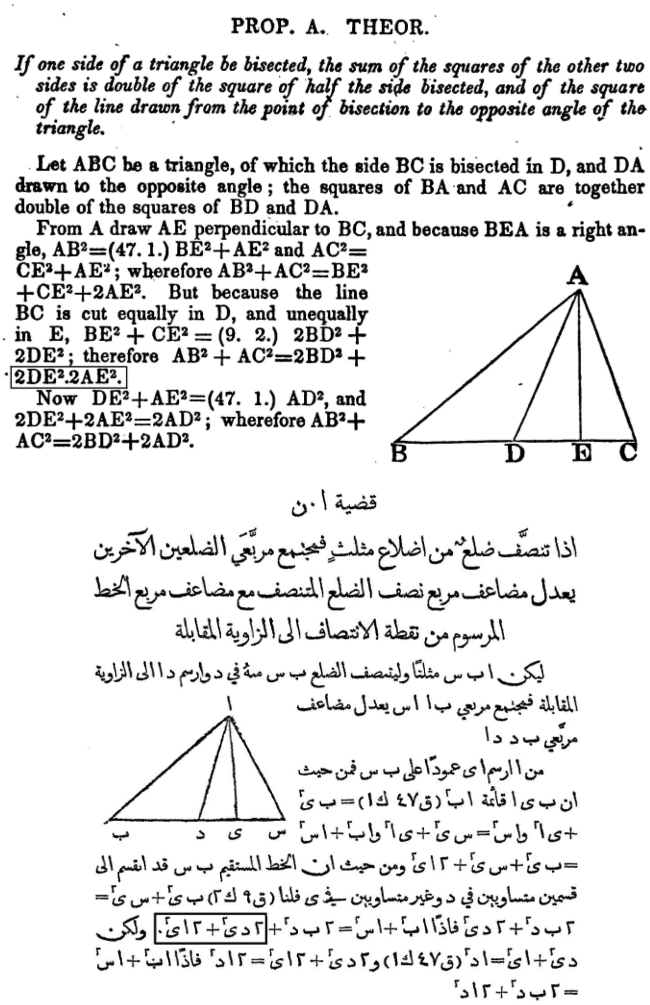
Cornelius Van Alen Van Dyck (1818-1895), best known as a translator of the Bible into Arabic, also translated or wrote numerous Arabic textbooks on mathematics and the natural sciences. In this paper we examine his translation of Playfair's Elements of Geometry *into Arabic. We describe some of the interesting features of the translation itself and offer suggestions why Van Dyck chose to translate this textbook.*

The *Elements of Geometry* was John Playfair's first major publication (1795). A half-century after it appeared, it traveled across the Mediterranean to appear in a very different cultural and linguistic context – a classic example of knowledge in motion. Before it appeared in Arabic form, the text had already traveled considerably. It proved immediately popular in Britain (13 editions before mid-century) and even more popular in America (some 33 editions and printings, not including summaries, extracts, and further re-workings).[1] In this paper we shall focus on the transmission of this classic of Anglo-American mathematics pedagogy into Arabic.

John Playfair (1748-1819) is better known today for his explanation of Hutton's essay on the geological history of the earth. He had, however, developed an interest in mathematics and mathematics education very early in life. His mathematical talents were widely acknowledged when, at age 18, he competed (unsuccessfully) for the chair of mathematics at Marischal College, Aberdeen. He finally acquired a formal academic position, becoming joint professor of mathematics (together with Adam Ferguson) at Edinburgh University in 1785. No doubt it was from his own pedagogical experience that he wrote his textbook. Throughout his life he also wrote reviews of mathematical works, mostly originating from outside England, for the *Edinburgh Review*.

[1] See A. Ackerberg-Hastings, "Analysis and Synthesis in John Playfair's *Elements of Geometry*," *British Journal for History of Science* 35 (2002), 43-72 (page 43, note 1).

Playfair made no claim of originality for his textbook. His *Geometry* was admittedly based directly on the classic *Elements of Euclid* of fellow Scots mathematician, Robert Simson (1687-1768).[2] Playfair's justification for producing a new introduction to Euclid was that the scholarship displayed in Simson's text made it not easily understandable to beginning students in mathematics.[3] Playfair's goal was to produce a text more readily accessible to students. In his introduction, he mentioned only two innovations that he had introduced: (1) he used "algebraic notation" to express the arguments in book V of the *Elements* which discusses ratios of magnitudes (Figure 1)[4] and (2) he substituted a new axiom ("Playfair's Axiom") in place of Euclid's perennially criticized parallel lines postulate.[5]



PROP. A. THEOR.

If one side of a triangle be bisected, the sum of the squares of the other two sides is double of the square of half the side bisected, and of the square of the line drawn from the point of bisection to the opposite angle of the triangle.

Let ABC be a triangle, of which the side BC is bisected in D, and DA drawn to the opposite angle; the squares of BA and AC are together double of the squares of BD and DA.

From A draw AE perpendicular to BC, and because BEA is a right angle, $AB^2=$(47. 1.) BE^2+AE^2 and $AC^2=CE^2+AE^2$; wherefore $AB^2+AC^2=BE^2+CE^2+2AE^2$. But because the line BC is cut equally in D, and unequally in E, $BE^2+CE^2=$(9. 2.) $2BD^2+2DE^2$; therefore $AB^2+AC^2=2BD^2+$ $2DE^2.2AE^2.$

Now $DE^2+AE^2=$(47. 1.) AD^2, and $2DE^2+2AE^2=2AD^2$; wherefore $AB^2+AC^2=2BD^2+2AD^2$.

قضية ا٠ن

اذا تنصّف ضلعٌ من اضلاع مثلثٍ فمجموع مربّعَي الضلعين الآخرين
يعدل مضاعف مربع نصف الضلع المتنصف مع مضاعف مربع الخط
المرسوم من نقطة الانتصاف الى الزاوية المقابلة

ليكن ا ب س مثلثًا ولينتصف الضلع ب س منه في د وارسم د ا الى الزاوية
المقابلة فمجموع مربعي ب ا ا س يعدل مضاعف
مربّعي ب د د ا

من ا ارسم ا ى عمودًا على ب س فمن حيث

Fig. 1: Top: Playfair's use of "algebraic notation" in added proposition A of book II. J. Playfair, *Elements of Geometry* (New York: W. E. Dean, 1846), p. 59. A typesetting error, recurring in several editions, is highlighted by a box. The error is found in multiple editions. Bottom: Van Dyck translation of the proposition. C. Van Dyck, *Kitāb fī Uşū al-Handasa* (Beirut, [1857]), p. 68. Van Dyck has corrected the typesetting error. This translation is an early example of algebraic notation in Arabic typesetting.

[2] R. Simson, *The Elements of Euclid, viz. The First Six Books, together with the Eleventh and Twelfth*, (Edinburgh: J. Balfour, First edition, 1756; Sixth edition, 1781).

[3] Despite its uncompromising erudition, Simson's treatise was remarkably popular as a textbook –some 26 editions in Britain– and had been translated into Portuguese, Spanish, French, and German. See Ackerberg-Hastings, "Analysis and Synthesis," pp. 47-48.

[4] This claim is not correct. Similar notation had been used as early as Isaac Barrow's 1660 edition. See J. Barrow-Goodman, " 'Much necessary for all sortes of men': 450 Years of Euclid's Elements in English," *Bulletin of the British Society for History of Mathematics* 21 (2006): 2-25 (p. 19, figure 12).

[5] This axiom states: Two straight lines that intersect one another cannot both be parallel to the same straight line. It had been known since the time of Proclus, and Playfair made no claim of originality. Nevertheless, the formulation has now become known by his name.

The translation of Playfair's *Elements of Geometry* into Arabic was the work of American missionary, Cornelius van Alan van Dyck (1818-1895).[6] Although he received limited formal schooling, he enrolled in Jefferson Medical College (Philadelphia), receiving his degree in 1839. Moved by the winds of religious revival sweeping New England, he enlisted as a missionary and was dispatched in 1840 to serve in the Ottoman province of Syria (comprising most of what are today the nations of Syria, Lebanon, Jordan, and Israel) under the auspices of the American Board of Commissioners for Foreign Missions (ABCFM). He spent the rest of his career in Syria. He quickly revealed exceptional talent in both written and spoken Arabic. Today his name is associated with the translation of the King James Bible into Arabic. The bulk of his oeuvre in Arabic, though, was a series of science textbooks: geography (1852), algebra (1853), geometry (1857), chemistry (1869), logarithms (1873) and logarithmic tables (1873), astronomy (1874) and history of astronomy (1874), as well as a medical textbook on pathology (1878), all expounding the modern European scientific tradition.

When Van Dyck produced his translation of Playfair's *Geometry*, the Arabic-reading world already had a tradition of discussion of Euclidean geometry dating back almost 1000 years. This tradition employed a well established technical vocabulary and formulaic structure. When he made his translation of Playfair's *Geometry*, however, Van Dyck ignored this tradition and instead used vocabulary and discourse styles taken directly from Playfair's English treatise. A few examples, shown in Table 1, will suffice to illustrate the situation.

Table 1: Examples of non-traditional terminology used by Van Dyck in his translation. In most cases, his choice of vocabulary can be explained as a literal word-for-word substitution of Arabic terms for Playfair's mathematical terms.

Traditional		**Van Dyck**	
	book / independent sub-unit		book / codex
	geometrical proposition		proposition / legal principle
	conversely		reversal / inversion
	Another demonstration		"Otherwise"

Not only did Van Dyck face decisions about whether to use traditional mathematical vocabulary or to coin new terminology, he also was called upon to decide which Arabic letters to use to represent the letter labels used in geometric diagrams to identify points, line segments, areas, etc. In the Arabic Euclidean tradition, these labels were usually assigned following the *abjad* order of the letters of the alphabet.[7] One option that Van Dyck might have chosen would be to replace Playfair's English letters with their corresponding equivalents in the Arabic alphabet. Another option might have been to replace Playfair's letters with the corresponding letters from the *abjad* sequence. Van Dyck, however, did neither. Instead, he often used a more-or-less phonetic system. For example, Playfair's letter C was replaced in Van Dyck's Arabic by the letter whose name sounds like the English word "seen". Similarly, Playfair's letter E became, for Van Dyck, the Arabic letter , which functions very often like the letter Y in English and, when used as a vowel, sounds like the Y in the English word "really" (Fig. 1). The situation is complex, though, for Van Dyck decided to replace Playfair's letter I with the Arabic letter ("meem"), the phonetic equivalent of M and he never used the Arabic letter to transliterate its phonetic equivalent Z.

Van Dyck tended to copy Playfair's diagrams exactly, although we might have expected him to use mirror images. Since Arabic is read from right to left, the labels on the Arabic diagrams are now in an unnatural order (Fig. 1). A native reader of Arabic would certainly have found this arrangement odd, if not actually awkward. But Van Dyck evidently wished to remain as close as possible to a reproduction of the diagrams in Playfair's treatise.

Despite his over-all tendency toward literal translation, Van Dyck was willing on several occasions to alter the text of Playfair.[8] For example, he modifies the verbal statements of several definitions of book I. The first two definitions are given by Playfair as:

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[6] Almost the only source of information on Van Dyck's life is L. Sa'di, "Al-Hakîm Cornelius Van Alen Van Dyck (1818-1895)," *Isis*, 27 (1936): 20-45.

[7] *Abjad* ordering differs from lexical order. It may be derived from Greek alphabetic numerals.

[8] It is possible that Van Dyck is literally translating an edited version of Playfair's textbook. In the absence of explicit evidence, however, I attribute the alterations to Van Dyck.

1. A point is that which has position but not magnitude.
2. A line is length without breadth.

These definitions were rendered by Van Dyck as:

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-

Translating these statements literally into English, we have:

1. The point is something that has position only *and does not have length or width or height*.
2. The line is length without breadth *or height*.

We see that in both definitions Van Dyck has added to the statement of Playfair the phrases indicated in italic text. These additions don't change the meaning of Playfair's definitions, but they do diverge from the strictly literal word-for-word translation that Van Dyck often seems to favor. It is not clear why Van Dyck thought it best to add additional explanation to Playfair's original statement.

Definition 3 illustrates another kind of modification to Playfair's text. Playfair's statement reads: "If two lines are such that they cannot coincide in any two points, without coinciding altogether, each of them is called a straight line."

Van Dyck renders this statement into Arabic as:

Translating this definition literally into English we have: "Two lines that do not coincide in two of their points unless they coincide in all are called straight. *And one may also say that the straight line is the shortest distance between two points*."

In this case, Van Dyck has added an alternate definition, indicated in italic type, that does not appear in Playfair's text. Where did it come from? I believe that the most probable source is Legendre's introduction to geometry.[9] He formulates his definition as: "La ligne droite est le plus court chemin d'un point a un autre." Why Van Dyck decided to add this alternative form of the definition we may never know.

Other changes are also occasionally introduced into Van Dyck's translation. On a few occasions, he has altered the labeling of the diagrams of Playfair (figure 2). On another occasion, Van Dyck substituted a different proof for one of Playfair's propositions (figure 3).

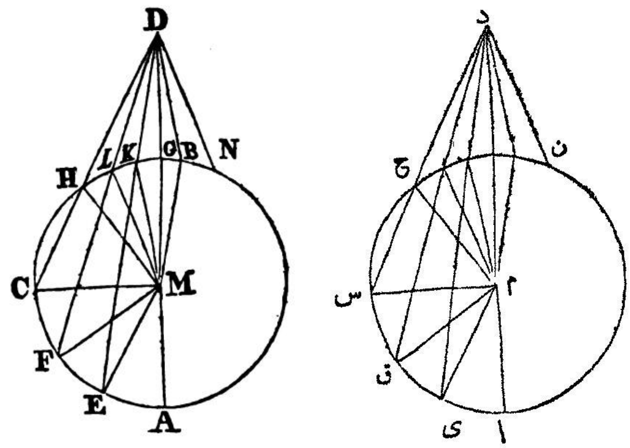
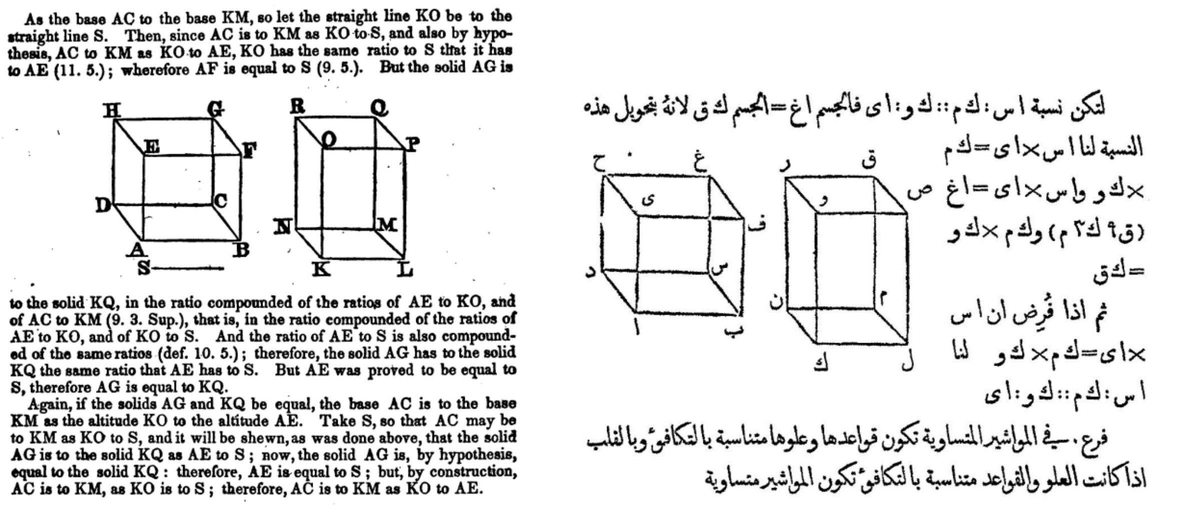


Fig. 2: Diagrams for proposition III, 8. Left: from J. Playfair, *Elements of Geometry* (New York: W. E. Dean, 1946, p. 67. Right: C. Van Dyck, *Kitāb fī Uşūl al-Handasa* (Beirut, [1857]), p. 77. Whether this simplification in the Arabic diagram was done by Van Dyck or his printer is unknown.

[9] A. M. Legendre, *Élements de géométrie*, septieme édition (Paris: Didot, 1807), p. 1.



As the base AC to the base KM, so let the straight line KO be to the straight line S. Then, since AC is to KM as KO to S, and also by hypothesis, AC to KM as KO to AE, KO has the same ratio to S that it has to AE (11. 5.); wherefore AF is equal to S (9. 5.). But the solid AG is to the solid KQ, in the ratio compounded of the ratios of AE to KO, and of AC to KM (9. 3. Sup.), that is, in the ratio compounded of the ratios of AE to KO, and of KO to S. And the ratio of AE to S is also compounded of the same ratios (def. 10. 5.); therefore, the solid AG has to the solid KQ the same ratio that AE has to S. But AE was proved to be equal to S, therefore AG is equal to KQ.

Again, if the solids AG and KQ be equal, the base AC is to the base KM as the altitude KO to the altitude AE. Take S, so that AC may be to KM as KO to S, and it will be shewn, as was done above, that the solid AG is to the solid KQ as AE to S; now, the solid AG is, by hypothesis, equal to the solid KQ: therefore, AE is equal to S; but, by construction, AC is to KM, as KO is to S; therefore, AC is to KM as KO to AE.

لتكن نسبة ا س : ك م :: ك و : ا ى فالجسم ا غ = الجسم ك ق لانهُ بتحويل هذه النسبة لنا ا س × ا ى = ك م × ك و و ا س × ا ى = ا غ (ق ٩ ك ٣ م) و ك م × ك و = ك ق

ثم اذا فُرِض ان ا س × ا ى = ك م × ك و لنا ا س : ك م :: ك و : ا ى

فرع . في المواشير المتساوية تكون قواعدها وعلوها متناسبة بالتكافؤ وبالقلب اذا كانت العلو والقواعد متناسبة بالتكافؤ تكون المواشير متساوية

Fig. 3: Top: The demonstration for proposition 10 of Supplement III from J. Playfair, *Elements of Geometry* (New York: W. E. Dean, 1846), p. 206. Bottom: the demonstration from C. Van Dyck, *Kitāb fī Uşūl al-Handasa* (Beirut, [1857]), p. 237. The differences are not purely cosmetic by substituting algebraic notation. The demonstration itself has been altered. Van Dyck no longer needs line S found in Playfair's diagram, so it is omitted.

In sum, Van Dyck produced a fairly literal translation of Playfair's text for his students. He did make a few modifications, but these changes are more noteworthy for their infrequency than for their impact on the mathematics of the treatise. But what motivated his effort? Since the traditional introductory text in Euclidean geometry, the *Tahrīr Kitāb al-Uşūl* of Naşīr al-Dīn al-Ţūsī had already been printed in Istanbul in 1801, why did Van Duck feel it necessary to translate a European introduction to Euclidean geometry? Was it simply a matter of the earlier printed version being out of print? But if there were strong demand for the printed version, it is difficult to imagine that this demand would not soon be translated into new printings. Or was the printed edition simply not available in Syria? Again, it seems unlikely that demand for the treatise would not produce an increased importation of the book.

Van Dyck has told us little about the rationale for his translation efforts. The brief author's preface to the translation of Playfair mentions only a perceived need for a textbook on Euclidean geometry. As rationale for choosing Playfair, Van Dyck's brief introduction says only that Playfair made some improvements (which are not described) in the restoration of the text originally produced by Simson. These brief sentences, although they may be accurate enough, provide very little concrete information. Perhaps an examination of Van Dyck's life and work against the broader context of the Protestant missionary endeavor in 19th century Syria will shed some light on his science translation activity, his choice of Playfair as the text to translate, and some of the unusual choices of mathematical vocabulary and related architectural features.

The Protestant mission station of the ABCFM in Ottoman Syria where Van Dyck spent most of his working life was only one local manifestation of a world-wide mission movement that arose in Protestantism early in the 19th century. At least two streams of thought converged in this mission movement. One was an intent to bring cultural reform and the benefits of Christian society to parts of the world perceived by the missionaries as "backward" socially and politically. This emphasis grew out of an early form of social gospel. Its advocates pursued social reforms as a means to improve the physical lives of people so that they would be more open to the call of the gospel to repent and reform their spiritual lives.

The second strand was an outgrowth of the rising pre-millenial interpretation of Scripture, a sense that the history of the present age was coming to completion and that Protestantism was poised to announce the end of the reign of "false religions" and to prepare for the coming reign of Christ on earth. "Prophecy, history, and the present state of the world seem to unite in declaring that the great pillars of the Papal and Mahommedan impostures are now tottering to their fall... Now is the time for the followers of Christ to come forward boldly and engage earnestly in the great work of enlightening and reforming mankind."[10]

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[10] J. Morse, S. Worcester & J. Evarts, "Address to the Christian Public, November 1811," in the *First Ten Annual Reports of the American Board of Commissioners for Foreign Missions* (Boston: Crocker and Brewster, 1834), p. 28. The passage is quoted in M. Elshakry, "The Gospel of Science and American Evangelism in Late Ottoman Beirut," *Past and Present* 196 (2007), p. 174.

Van Dyck's translations can be seen as an integral part of the missionary strategy of the ABCFM outpost in Syria. When direct conversion proved difficult to effect, the missionaries began to turn to indirect methods that might lead to the desired conversions.[11] One of the more effective strategies was to create schools to teach the modern scientific theories that had been developed in Europe. This strategy was partially successful – it attracted many students into mission schools where they were exposed to mandatory religious instruction and required to attend at religious services and prayers.[12] As one missionary report was to remark, they were sometimes "obliged to bait the hook with arithmetic" although in a few cases "God's word was a sufficient charm in itself" to draw students into these schools.[13]

The mission schools very often emphasized the sciences as well as religious instruction. The emphasis on science was two-fold. On one level, the goal was simply to attract students into the schools where they could then be introduced to Protestant religious instruction. But on another level, the sciences were also emphasized as a way toward God in their own right. The sciences offered a multiplicity of metaphors and analogies to instill in students a desire for the divine. A scientific understanding of nature encouraged a sense of religious awe and wonder. Moreover, the use of rational argument to win assent in a scientific debate encouraged the hope that religious converts might be won through similar tactics.[14]

These mission schools grew rapidly and became quite successful, attracting many students. Despite all their efforts, though, there were still very few conversions in the sense that individuals explicitly changed their ecclesiastical affiliation. Although the ABCFM governing board became increasingly unhappy about the expense of the Syrian mission in relation to the number of converts produced, the missionaries forged ahead, ever hopeful that, with continued effort, their labor might begin to bear the spiritual fruits both they and their financial supporters desired.

But if you are planning to teach modern scientific studies, you will need appropriate textbooks, and there were none available in Arabic. One solution, of course, was to force the students to learn English. As the missionaries had noted already in 1837, "We have endeavored to advance [the students in our school] as rapidly as possible in the acquisition of our language… Without a knowledge of English, it would be impossible to give them a thorough education, as there are no suitable elementary books in Arabic."[15] It was to remedy this lack of appropriate textbooks that Van Dyck devoted his considerable skill as a linguist and translator. In this paper, we have restricted our focus to his translation of Playfair's *Geometry*. And so we return to the question asked earlier: why Playfair, when traditional Arabic introductions to Euclid were widely available both in print and in manuscript?

I shall suggest several possible reasons. First, Playfair's text had been introduced into the United States as early as 1806 and proved at least as popular in that context as in Britain. There were more than thirty American editions and reprintings of the text during Van Dyck's lifetime.[16] These American editions were produced mainly in New York or Philadelphia. And since Van Dyck grew up in upstate New York and received his medical training in Philadelphia, it is tempting to imagine that he might well have used Playfair's treatise in his own education. Moreover the treatise was deserving of its popularity, for Playfair was both an educator and a skillful expositor with an easy style of presentation that made his work exceptionally accessible to students.

Moreover, Playfair had been associated with many of the leading luminaries of the Scottish Enlightenment in Edinburgh. These men focused attention on geometry as a key part of education. Through study of geometry, students would be exposed to the processes of right reasoning so necessary to any educated person. Geometry fit well with the Scottish (and American) penchant for the practical and a conscious focus on the common sense interpretation of human experience. Under Scottish leadership, instruction in geometry began to change from rote memorization of results to study of the proofs and argumentation of Euclid. If the mandate of education was to develop the moral sense of the citizen, geometry was widely perceived as an essential tool for it taught right reasoning on which moral life was built.

The tie between Scotland and American higher education had been close since the early years of the eighteenth century. The ideas of the Scottish Common Sense school infused American classrooms both by way of Scots professors and through textbooks published by Scottish authors. From these roots came the emphasis on liberal arts education with a practical bent that increasingly characterized American curricula. Seen against this backdrop, the popularity of Playfair's textbook in America is easy to understand. Not only is it a masterful exposition of Euclid, but it came to American classrooms

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[11] For a reflection on the difficulties and obstacles faced by the Mission in their attempt to make converts, see "Peculiar Obstacles in Syria," *Missionary Herald*, 54 (1858), pp. 139-140.

[12] "Extracts of a Joint Letter of the Missionaries at Beyroot," *Missionary Herald* 33 (November, 1837), p. 445.

[13] "Syrian Mission – Turkey: Station Reports," *Missionary Herald*, 54 (1858), p. 142.

[14] M. Elshakry, "Gospel of Science," pp. 183-184. For a similar appeal to nature metaphors in evangelism, see S. Sivasundaram, *Nature and the Godly Empire: Science and Evangelical Mission in the Pacific, 1795-1850* (Cambridge: Cambridge University Press, 2005).

[15] "Extracts of a Joint Letter of the Missionaries at Beyroot," *Missionary Herald* 33 (November, 1837), p. 444.

[16] See A. Ackerberg-Hastings, "Analysis and Synthesis," p. 43.

already infused with ideals that meshed almost seamlessly with the rising American interest in practical and liberal arts education.

The choice of Playfair to translate also fits well with this pattern. Not only was Van Dyck an American, he was also a Protestant missionary intent to spread the gospel both in a theological sense and in a cultural sense. Hence his desire to provide a form of liberal arts education in Syria, culminating in the establishing of the Syrian Protestant College (later to become the American University of Beirut). Mathematics played a key role in this liberal arts education, since it was the key by which students learned to think clearly and systematically. Developing the reasoning skill through mathematics would permit students not only to perceive the errors of "false" religion (whether Catholicism or Islam) but also would encourage an enlightened and critical view of the "decadent" social system of the crumbling Ottoman state.

Van Dyck's translation often appears excessively literal, with choices in translation and "architecture" that seem calculated to repel the reader familiar with the more traditional Arabic discourse on Euclidean geometry. I suggest that these choices may well have been deliberate and conscious, a way to distance Playfair's approach, based on Common Sense ideals, from the more traditional study of Euclid. Van Dyck's translation is a transplanting of Playfair into a different cultural milieu in the hopes that the new mathematics would take root and lead ultimately to the production of a new citizenry prepared to take the lead in reforming Syrian society, reshaping it to become more like the Puritan Protestant society of New England. Although the hope that modern mathematics would translate into spiritual renewal was rarely realized, Van Dyck's works did contribute, at least indirectly, to the Arab intellectual revival of the 1880s.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 272.

STUDIES ON THE PROBLEM OF MINIMUM AND MAXIMUM IN CONIC SECTIONS' TRADITIONS. APOLLONIOS OF PERGA AND SERENOS OF ANTINOEIA

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Abstract

The treatise of Apollonios of Perga Conics *has put its sign on the development of mathematical sciences in the Arabic world since the 9th century and Apollonios is the most referenced and studied mathematician after Euclid. The Arabic translation of the 7 books of the* Conics *has been done in Baghdad in the 9th century, after the demand of Banu Musa from Hilal ibn Hilal al-Himsi and has been controlled by Thabit Ibn Qurra. This translation has inaugurated a tradition, and has influenced the interest on solid problems –especially the trisection of an angle and the construction of a regular heptagone– the application of the conics on problems from other fields like the optics (burning mirrors and lenses), geometric resolution of the 3rd degree equations (works of Al-Khayyam and Sharaf al-Din al-Tusi) or even the theory of astrolabes, sun clocks or the perfect compass. All these applications will lead to the reinvention of new properties on these curves – focus properties, study of asymptotes, local and harmonic properties. In the 5th book of the* Conics *which survived only in Arabic, Apollonios studies the problem of minimum and maximum. The treatise* Section of a Cone *of Serenos of Antinoeia (4th century A.C.) deals with the areas of triangular sections of right or scalene cones made by planes passing through the vertex and either through the axis or not through the axis, showing when the area of a certain triangle of a particular class is a maximum, under what conditions two triangles of a class may be equal in area.*
In the context of this paper we are going to present a comparative study of these two treatises and show the influence of the work of Apollonios on the level of some enunciations to the approach of Serenos.

Division of the treatise *On the Section of a Cone*

Serenos of Antinoeia is the author of two mathematical treatises *On the section of a cylinder* and *On the section of a cone* that the manuscript tradition transmitted to us following the first four books of *The Conics* of Apollonios from Perga. This article provides an outline on the content of the treatise *On the Section of a Cone* where Serenos is doing a comparative study on the sections produced in a right or an oblique cone by plans passing by its vertex. We identify, classify and study the different problems Serenos treats in the three sections of his treatise. The treatise of *On the Section of the cone* of Serenos can be divided in three relatively independent parts. In the first part, propositions 1-14, he works on the right cones; in the second, propositions 15-57, he works on the oblique cones. The last propositions 58-69, constitutes a separate section where he looks at the relationship between volumes of two right cones in relation to the heights, the bases and the surfaces of the triangular sections which pass by their axis (Heath; 522). In each part Serenos poses several problems and gives their solution We will also try a first attempt to find relations between this treatise of Serenos and the Book V of *The Conics* of Apollonios of Perga.

Lemmas in the treatise *On the Section of a Cone*

We should underlie a big amount of auxiliary lemmas (propositions 1, 2, 17, 18, 19, 21, 33, 37-39, 52, 53, 54, 55, 56) the majority of which were used several times to solve various problems that Serenos posed in this treatise. Others are put in the treatise to serve only once in the demonstration of a proposition.

First part of the treatise *On the Section of a Cone*: the case of right cones

In this part of his work we notice the existence of several theorems and problems by which Serenos tries to resolve three different problems that we can gather in the following way.

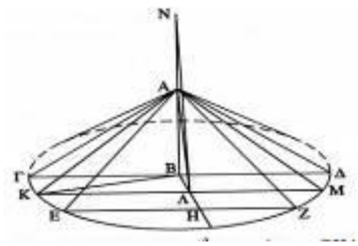
In the first problem (propositions 5 and 8) he shows that if the axis of the cone is not smaller than the radius of the base, the axial triangle is the greatest among those which are determined in this cone and conversely (Heiberg; 126-128 and 134-136). Proposition 6 (Heiberg; 128-132) is the generalization of 5. In proposition 6, he builds the bases of nonparallel triangles (in the demonstration of prop. 5 the bases are parallel) between them and which meet in a certain point.

The triangle ΑΓΔ is bigger than the triangle AEZ (prop.5).

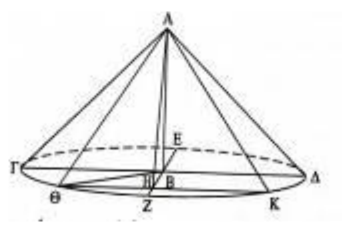
The triangle ΑΓΔ is bigger than the triangle ΑΖΔ (prop.6).

In the second problem he shows initially that if two plans carried out by the vertex, one passing by the axis, the other out of the axis, determines triangles whose surfaces are equivalent, the cone is obtuse-angled, i.e. the axis of the cone will be smaller than the radius of its base (propositions 10 and 12). Proposition 12 (Heiberg; 142-144) gives us the construction which Serenos needs in the demonstration of proposition 10 (Heiberg; 138-140).

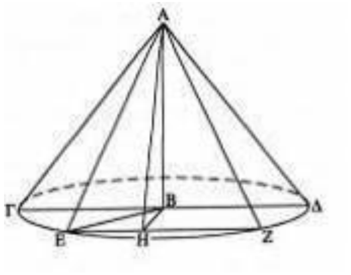
In proposition 11 (Heiberg; 140-142), he also shows that any plan, carried out by the vertex, between the two plans of equivalent triangles determines a triangle bigger than these equivalent triangles. We can notice that in this case (propositions 10-12) the problem takes the following form: "To cut an obtuse-angled cone with axis smaller than the radius, so that the triangular section obtained is either equal to an axial section (proposition 12) or it will be maximum (proposition 11)".



Triangle AKM is bigger than triangles ΑΓΔ and AEZ (prop.11).

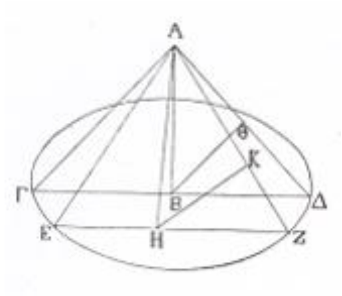


Triangle ΑΓΔ is equivalent with the triangle ΑΘΚ (prop.12).



The axis AB is smaller than the radius BΔ if AΓΔ is equivalent to ABZ (prop.10).

In the third problem, proposition 13 (Heiberg; 144-148) and proposition 14 (Heiberg; 148-150) he shows that, in the obtuse-angled right cone the triangle determined by a plan passing by the vertex is maximum then its height is equal to half of its base, i.e. with the radius.



Triangle AΓΔ is bigger than all the non similar triangles established in the cone if height equals radius (prop.13).

In the first part Serenos noticed a significant difference in behavior between the acute-angled and obtuse-angled right cones, or as he prefers to state, between the cones whose axis h is major the radius r of the base and those of which it is minor.

In this part he obtains the following results:

If the axis is smaller than the radius of the base, the intermediate triangle is bigger than the other triangles (proposition 11).

If the axis is equal to the radius, the axial triangle is bigger than all the non similar triangles (proposition 13).

If the axis is bigger than the radius, the axial triangle is bigger than all the triangles determined in the cone (proposition 5 and 6).

Second part of the treatise *On the Section of a Cone*: the case of oblique cones

In the second part of *On the Section of the cone* (propositions 15-57) Serenos considers the oblique cones and compares the triangular sections of one of the following three kinds with other sections of the same kind by considering their surfaces.

The three kinds of the triangular sections are (Heath; 523):

1. the axial section, which contains all the sections passing by the axis.
2. the isosceles section, which contains the sections in which bases are perpendicular to the projection of the axis of the cone on the plane of the base.

3. the parallel section, which contains triangular sections whose bases are (a) the diameter of the circular base which passes by the foot of the perpendicular by the vertex of the plane of the base and (b) the chords of the circular base which are parallel to this diameter.

In this part, Serenos, as in the first part, poses also several problems which he gathers and he tries to find a response to each one of these problems. In the following, we will examine each problem separately.

Problem 1

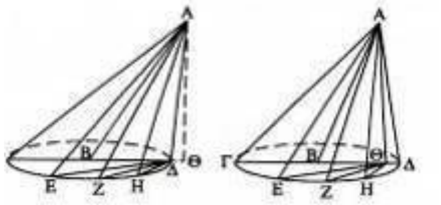


Fig. 1.

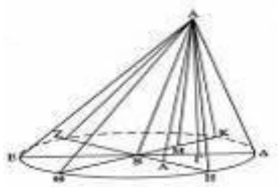
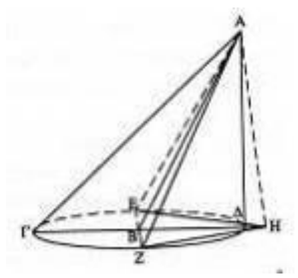


Fig. 2.

In proposition 16 (Heiberg; 152-156) Serenos demonstrates that the line ΑΓ will be maximum and the line ΑΔ will be minimum and among the other lines, that which is brought closer to the greatest will be bigger than that which is the most distant.

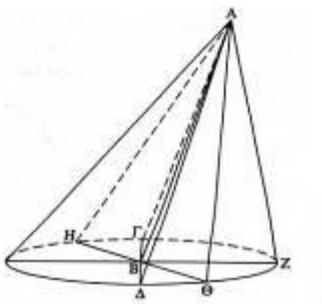
Problem 2

In propositions 22 and 23 (Heiberg; 170-172), the axial sections which have their bases perpendicular to the line ΓΔ (diameter of the circle of the base) are isosceles.



Triangle AEZ is isosceles (prop.22).

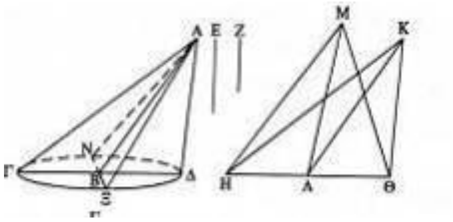
Problem 3



In proposition 24 (Heiberg; 172-176) he demonstrates that the isosceles triangle ΑΓΔ is the maximum (μέγιστον) of all the triangles which pass by the axis, and the triangle AEZ, perpendicular on the plane of the base is the minimum (ελάχιστον).

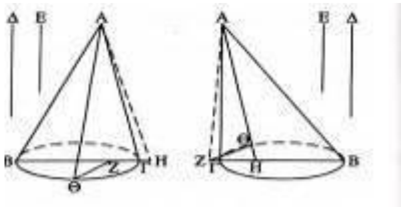
The formulation of this proposition resembles that of proposition 16. In proposition 16 Serenos finds the maximum and minimum lines, while in proposition 24 he uses this property to find the maximum and minimum triangles. Indeed, we can say that in proposition 16 he seeks the maximum side and in proposition 24 he seeks the maximum triangle.

Problem 4



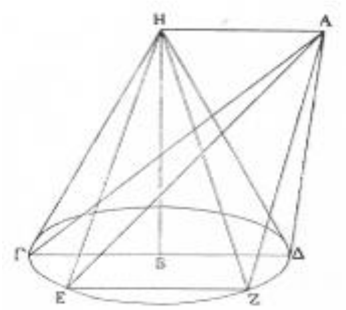
His efforts, in proposition 27 (Heiberg; 182-188) are focused on the construction of the triangle ANΞ so that to be to the triangle AΓΔ in the ratio of the line E to the line Z.

Problem 5



In proposition 25 (Heiberg; 176-178) he is trying to construct a segment AΘ that is to segment BA as Δ/E (Δ/E = BA/AH = BA/AΘ).

Problem 6



In proposition 29 (Heiberg; 188-192) Serenos took the third kind of the sections with bases parallels to diameter.

If perpendicular AΔ is not smaller than radius, the maximum triangle AΓΔ is that which is perpendicular to the base of the cone and is bigger than all those who have their bases parallel to ΓΔ.

In proposition 30 (Heiberg; 192) he demonstrates the property: If perpendicular is smaller than radius, the triangle AΓΔ in question is not the maximum of the triangles.

In these two propositions, Serenos examines for bases parallel to the diameter, the two different cases:

1. height > = radius.
2. height< radius.

and he concludes that:

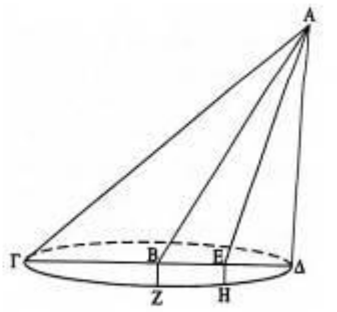
1. If triangle HΓΔ< triangle HEZ, then triangle AΓΔ< triangle AEZ.
2. If triangle HΓΔ> triangle HEZ, then triangle AΓΔ> triangle AEZ.
3. (3) If triangle HΓΔ= triangle HEZ, then triangle AΓΔ= triangle AEZ.

Serenos, in the following propositions, works on the isosceles section which passes by the axis.

We can thus distinguish the following problems which concern the isosceles sections.

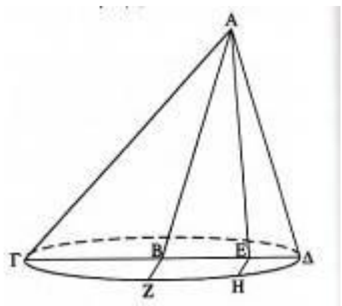
Problem 7

In proposition 31 (Heiberg; 192-194) he states that if axis is bigger than the radius or equal to the radius, the isosceles axial triangle is the maximum among all isosceles triangles established on the side where the axis is inclined. He establishes proposition 32 (Heiberg; 196-198) to examine the same problem but for the case where the axis a is smaller than the radius r.



The isosceles triangle carried out by the line AB is the biggest of the given isosceles triangles having their bases located between the points B and Δ.

Problem 8

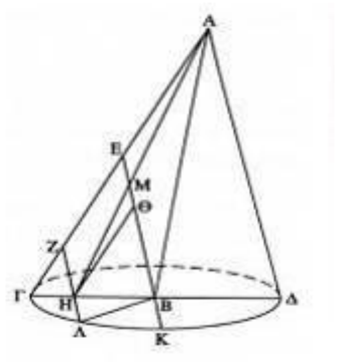


In proposition 34 (Heiberg; 200) he stresses the property: if the isosceles triangle AE, EH is equivalent with the isosceles triangle from AB, BZ then EA > AB.

In the following proposition 35 (Heiberg; 200-204) he demonstrates one problem of the same kind: if the isosceles triangle AE, EH is equivalent with the isosceles triangle from AB, BZ then AB < BΔ.

Problem 9

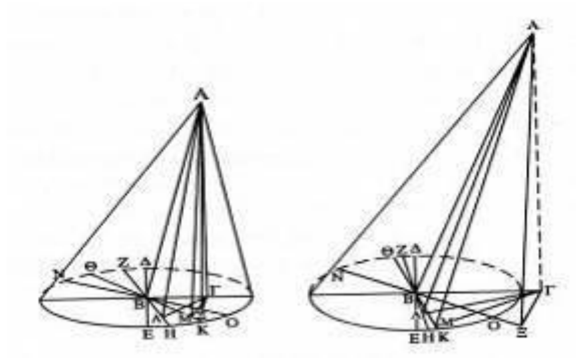
The ninth problem of the treatise *On the Section of a Cone* is demonstrated in the context of propositions 36 and 40-44 (Heiberg; 206-208 and 216-230): If, in an oblique cone, isosceles triangles are established on parallel bases on the side where the axis is inclined, the isosceles triangle passing by the axis will be neither biggest nor smallest of all the isosceles triangles.



The isosceles triangle passing by the axis is not the smallest of all the isosceles triangles which have their bases between the points Γ and B.

Problem 10

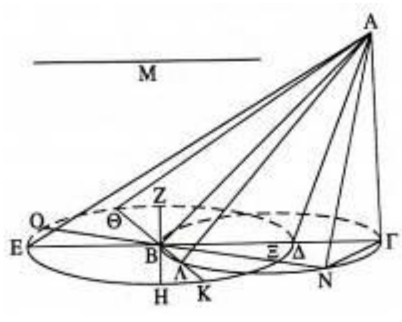
It is also very interesting the problem in proposition 45 (Heiberg; 230-234) in which Serenos shows that the place of orthogonal projections of the perpendiculars of an oblique cone on the diameters of the base is a circumference of only one circle, located in the same plane with the base of the cone and described around the diameter consisted by the cut out line, in this plan, between the center of the base and the perpendicular carried out of the vertex on this plane.



The points B, Λ, M are located on the circumference of only one circle whose diameter is the line BΓ.

Problem 11

In proposition 47 (Heiberg; 236-242) he examines the following problem: "being given in an oblique cone one of the triangles passing by the axis which is neither the greater nor the smaller, to find another triangle passing by the axis which, jointly with the triangle given, is equivalent with the sum of the biggest and the smallest of the triangles passing by the axis".

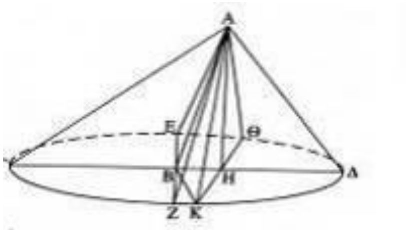


Triangle ΘAK + triangle OAΞ = triangle ZAH + triangle EAΔ.

Where ΘAK is the triangle passing by the axis, ZAH is the maximum triangle, EAΔ is the minimum triangle and OAΞ is the triangle searched.

Problem 12

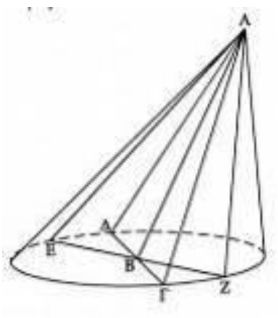
In proposition 50 (Heiberg; 246-248) Serenos demonstrates that: If the axis is equal the radius, then to construct the following ratio EAZ / ΓAΔ = ΓAΔ / ΘAK.



Where the triangle ΓΑΔ which is the minimum triangle of those passing by the axis, will be to the isosceles triangle ΘΑΚ as the triangle ΕΑΖ, which is the maximum triangle of those passing by the axis, is to the minimum triangle ΓΑΔ.

Problem 13

The second part of *On the Section of the cone* ends by the problem of proposition 57 (Heiberg; 268-272) in which he proves that the biggest of the given triangles has the biggest perimeter and conversely. For this demonstration Serenos uses a series of preceding lemmas (propositions 52-56).



The triangle ΑΓΔ is bigger than the triangle AEZ, thus the perimeter ΑΓΔ is bigger than the perimeter AEZ.

Third part of the treatise *On the Section of a Cone*: an extension of the Book XII of Euclid's *Elements*

The last group of propositions 58-69, constitutes a separate section of the treatise dealing with the volumes of right cones in relation to their heights, their bases and the areas of the triangular sections passing through the axis. With these propositions, he enriches propositions already established by Euclid in his book XII of the *Elements*. The third group of propositions is completely separated from the preceding propositions. In this section Serenos gives always couples of propositions and in every couple he proves a property and its opposite.

If r is the ray of the base, h is the height, B the base, T the surface of the axial triangle and V the volume of the cone. We know the existence of the formulas which connect the radius, the height, and volume, the circle of the base and the axial triangle of an oblique cone:

$$V=\pi r^2 h/3, \quad B=\pi r^2, \quad T=rh.$$

If we use the notations r', h', B', T' and V' for the same elements of a second cone, propositions of this part of the treatise of *the Section of the cone* take the following form (Loria; 733-734):

- Propositions 58-59 (Heiberg; 272-276): If V=V', we will have T/T' = r' / r and conversely.
- Propositions 60-61 (Heiberg; 278-282): If B/B' = V^2 / V'^2, we will have T=T' and conversely.
- Propositions 62-63 (Heiberg; 282-286): If h=h ', we will have V/V' = T^2 / T'^2, and conversely.
- Propositions 64-65 (Heiberg; 286-290): If V/V' = h' /h, we will have T=T' and conversely.
- Propositions 66-67 (Heiberg; 290-296): If V/V' = B' /B, we will have T/T' = r'^3 / r^3 and conversely.
- Propositions 68-69 (Heiberg; 296-302): If V/V' =B^2 / B'^2, we will have T/T' =r^3 / r'^3 and conversely.

Book V of *The Conics*

This book deals with normals to conics regarded as maximum and minimum straight lines drawn from particular points to the curve. In this book Apollonios has put a series of propositions which lead to the determination of the evolute of each of the three conics. This book is much more original than the preceding ones. He considers various points and classes of points with reference to the maximum and minimum straight lines which it is possible to draw from them to the conics i.e. as the feet of normals to the curve (Heath; 159).

In lieu of Conclusion

In propositions 16 and 24 of the treatise *On the Section of a Cone* Serenos is searching the maximum and minimum sides (prop. 16) and triangle (prop. 24). For the other sides or triangles, he writes that "the most approaching (line) or the most approaching triangle of the maximum is bigger than the most distant". This expression evokes the following expressions used by Apollonios. In proposition 6 of the Book V of *The Conics* (Rashed;238) Apollonios uses the following formulation: "... As for the other straight lines, those of them that are closer to the minimal straight line are less than those that are farther from it". This shows that perhaps there was a tradition of mathematicians working on the problem of maximum and minimum for rectilinear figures in both traditions. Apollonios was working on the problem of maximum and minimum for 3rd order equations and Serenos was working on the problem of maximum and minimum on 2nd order equations as Euclid shows us in the Books II and VI of *The Elements.*

We can find the same formulation in the enunciation of propositions 7-10, 12, 34, 35, 64-67, 70 of the book V of *The Conics*, "while among the other lines, this which is closer of this minima line will be smaller than the one which will be farther". In proposition 16 of the same book he writes "If a point is taken on the minor of two axes of an ellipse such that the segment of the minor axis between it and the vertex of the section is equal to the half of the latus rectum, then of the straight lines drawn from the point to the section the greatest is the part of the minor axis which is equal to the half of the latus rectum, and the smallest is the complement of the minor axis and of the other straight lines [so drawn] those of them drawn closer to the maximal straight line are longer than those drawn farther, ..."

With this formulation he develops the enunciation which corresponds to the lines of propositions 16, 17, 19-21, 73-77 under the form: "this which is more closer of the minima line will be greater than the one which is farther".

Finally in proposition 72 (Rashed;460) Apollonios writes that: "If a point is taken below the axis of a parabola or a hyperbola, and it is possible to draw from it two straight lines such that the part which the axis cuts off from each of them is one of minimal straight lines, then the closer of those two straight lines to the vertex of the section is greater than all [other] straight lines drawn from that point to the arc of the section from the vertex of the section to the other, second, straight line, and of the remaining straight lines drawn to that arc on both sides those drawn closer to it are greater than those drawn farther, and second straight line is smaller than all straight lines drawn from the point to the remaining [part] on that side of the section, that is the complement of the first arc on that side, and of the remaining straight lines drawn to that other [complementary] arc those drawn closer to it are smaller than those drawn farther". Propositions 18 and 72 use the two forms on the lines approaching the maxima and minima lines.

We can mark the relations between the formulations of the enunciation of propositions 16 and 24 of the *On Section of a Cone* and those of propositions 6-10, 12, 16-21, 34, 35, 64-67, 70, 72-77 of the book V of *The Conics*. We can not say that Serenos Knew the Book V of *The Conics* but we can see the existence of similar expressions – enunciations between these two treatises.

References

HEATH, T.L. (1921), *A History of Greek Mathematics*, Vol. 2, Oxford.

HEIBERG, J.L., (1896), *Sereni Antinoensis Opuscula*, Leipzig.

LORIA, G., (1914), *La scienze esatte nell' antica Grecia*, Vol II, Milano.

RASHED, R., (2008), *Apollonius de Perge, Coniques, Tome 3: Livre V*, Walter de Gruyter, Berlin-New York.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 281.

ASTRONOMY IN LATE BYZANTINE ERA: A DEBATE BETWEEN DIFFERENT TRADITIONS

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Abstract

In the Late Byzantine era (late 13^th^-15^th^ c.), specially in Constantinople, the capital city of the Byzantine Empire, we could find few scholars, who wrote about astronomy following the Ptolemaic tradition. Conversation about this mathematical science was very popular between scholars, and the main subject was renovation of astronomical data. In the same period, in Mongolian Persia, near the Byzantine Trebizond, there were healthy observatories at Maraga and Tabriz, where scholars followed the Islamic tradition, and in Provence the Jewish common of Caraites created a new astronomical school, which was based in Hebrew rules.

Soon, the basic principles of Persian and Jewish traditions were transferred in Constantinople. In these conditions, three teams were created about astronomy: first, the scholars who followed the Ptolemaic astronomy and believed that the solution of the problems was the right study of ancient Greek corpus, second, the scholars who imported principles of Persian astronomical texts, and, third, the scholars who accorded with the principles of Caraites.

In this paper we will present, first, the transmission of ideas, second, the three schools with their representatives, and, third, the debate between them. Then, we will try to interpret the main arguments in this conflict and the ideology behind them, and at least, the effects of this fruitful conversation.

Introduction: Astronomy in the Palaeologan Era

In the second half of the 13th century, after the restoration of the Byzantine Empire in Constantinople, in the era that was commonly called "Palaeologan Renaissance", a special interest in science and especially astronomy appears in the small Empire, particularly by a group of scholars in the Imperial Court. This interest is particularly evident, judging from the works that are saved and by the debate and the discussion they caused. It is therefore interesting to see from the one hand the results of this interest on the scientific level, and on the other hand the motivations, the various influences, the debate and its outcome, and how they fit in the socio-cultural and political context of its time and influence it accordingly.

Traditionally, the interest of the Byzantines in astronomy, from Late Antiquity, is focused in finding the positions of celestial bodies, predicting eclipses and making calendar calculations. The basis of Byzantine astronomy was the works of Ptolemaic astronomy, namely the Almagest (*Μαθηματική Σύνταξις*) and Handy Tables (*Πρόχειροι Κανόνες*), as well as the work of commentators, especially the work of Theon of Alexandria, Small Commentary on Ptolemy's Handy Tables (*Θέωνος Αλεξανδρέως Υπόμνημα είς τούς Προχείρους Πτολεμαίου κανόνας*). The important difference in this last period of

Byzantium is both the increase of the number of astronomical works and the introduction of different astronomical traditions from East and West.

This fact is closely related to the real need to renew the Ptolemaic work, but also in opening the now small Empire in a cultural level. Byzantine scholars appear to be the recipients of scientific ideas that come from the Persia of the Mongols, and from the West, Italy and the Jewish communities of Provence. The official public relations and marketing communications with both the Empire of the Mongols in the late 13th century and the West, in an effort to unite the churches, seem to have played an important role in this phenomenon. Byzantium at this stage appears as the crossroads between the ancient Greek tradition and foreign influences. It therefore creates a cycle in the context of imperial power with slots for the renewal of the existing and attraction of the new scientific ideas, which are processed efficiently, with consequences that will not appear until much later, in the scientific domain as well as in the internal ideological conflicts that divide the empire.

Traditions and persons

From the late 13th century, Byzantine scholars are interested in mathematical astronomy, and this is particularly evident from the manuscripts that are saved and from the discussion that they have caused. It is typical to organize public debates among scholars,[1] a first form of Academies, which will be developed later in the West, focusing on astronomy. On this basis, three main schools of astronomy were formulated: the Ptolemaic, the Persian and the Western. It is interesting to see the progress, their development but also the persons who played an important role in the formation of traditions.

The Ptolemaic tradition

The founder of the Ptolemaic tradition is Theodore Metochites, a senior official at the court of Andronicus II Palaeologus. Metochites had studied the work of Claudius Ptolemy, and of other astronomers, mentioned in his works: Theon, Apollonius of Perga, Serinus, etc. The practice of astronomy had been very productive: he wrote an *Introduction to the Almagest* (*Προεισαγωγή είς τήν τού Πτολεμαίου Σύνταξιν*)[2] and *Astronomike Stoicheiosis*[3] (*Elements on the astronomical science*, *Στοιχείωσις έπὶ τή αστρονομική έπιστήμη*). His aim was a work that would combine the astronomical theory of Ptolemy, as shown in *Almagest*, with practical applications, as stated in the *Handy Tables* and in the Theon's *Commentaries*. The aim of Metochites is to make accessible to a wider audience through his work, a science difficult and demanding as astronomy is.

What is new in the case of Metochites is the perception of nature and its link with the overall policy stance. According to Metochites, the main concern of people is to be aware of the natural world. The natural world is subject to constant change (flow, ροή), and understanding requires knowledge of ancient Greek science and observation through the senses.[4] It is important therefore to investigate by observation and mathematical knowledge of the universe, to interpret the apparent disarray in order to prove the laws that determine the changes and to see the final order (in the tradition of saving the appearances). So man will be driven to ground peace through knowledge, observation and senses.[5] In this context, the special interest of Metochites for predicting eclipses and the encounter with contemporary theocratic-Hesychast views which prevailed at the clergy is explained. The orientation of Metochites then is secular, with major importance in the ancient Greek knowledge.

Metochites' interest in astronomy spread "like wildfire",[6] according to his contemporaries, in Constantinople, then finding work in a continuing series. One of the most important of them was Nikephoros Gregoras, also a high official in the imperial Court, who, after studying on the Ptolemaic works, submitted a proposal to reform the calendar to emperor Andronicus II. This proposal was ultimately not accepted for reasons of political and social equilibrium, but eventually was imposed nearly two centuries later, in 1578, by Pope Gregory XIII. In the same tradition participate Nicholas Kavasilas, Isaac Argyros, etc.

The Persian tradition

The persian-islamic was the second astronomical tradition which developed in Constantinople after the 13th century. From the 13th century, the Empire of Mongols had developed the study of astronomy, with emphasis on observation, as

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1 Igor P. Medvedev, "The so-called θέατρα as a form of communication of the Byzantine intellectuals in the 14th and 15th centuries", N. G. Moschonas (ed.), *The Communication in Byzantium*, IBR/NHRF, Athens 1993, pp. 227-235 (in Greek).

2 Published in K.N. Sathas, *Medieval Library* (*Μεσαιωνική Βιβλιοθήκη*) I, Phenix, Venice 1872.

3 Published in K.N. Sathas, *Medieval Library* (*Μεσαιωνική Βιβλιοθήκη*) I, Phenix, Venice 1872, pp. πδ΄-ριη΄.

4 *Theodori Metochitae Miscellanea philosophica et historica*, C.G. Müller, T. Kiessling (eds), Leipzig 1861, and A.M. Hakkert, Amsterdam 1966.

5 Aggeliki Konstantakopoulou, *The Byzantine Thessaloniki*, University of Ioannina, Ioannina 1996, pp. 134-135 (in Greek).

6 H. Hunger, *Byzantine Literature*, National Bank of Greece Cultural Foundation, Athens 1994, t. 3, p. 52 (in Greek).

shown by the two observatories that were established in Maragha and Tabriz. At this time, through trade and diplomatic relations with the Mongols, and the Empire of Trebizond, which functioned as an intermediary station, the Byzantines were in contact with the works of astronomers who followed the Persian school.

Gregory Chioniadis was the rapporteur of this trend. Chioniadis visited Tabriz and studied close to Shams al-Din al-Bukhara and translated into Greek a series of Arabic and Persian texts. The translations are of the *Zij as-Sanjari* (al-Khazimi, c. 1120), the *Zij al-Ala'I* (al-Fahhad, c. 1150), and the instructions of the Persian astronomer Shams Bukhari. These translations came into the hands of a monk in Trebizond, named Manouel.[7]

George Chrysokokkes,[8] a Byzantine astronomer, physician and geographer, studied astronomy with Manouel in Trebizond and around 1347 wrote one of the most important and most well-known astronomical works of the 14th century, titled *Introduction to the Syntaxis of the Persians (Είσαγωγή είς τήν Σύνταξιν τών Περσών)*.[9] This work became extremely popular in the Byzantine Empire, mainly in Constantinople, as well as in the West, while it continued to be reproduced for a long time after the Ottoman occupation (in the Greek world, in particular, it was still being reproduced during the 19th century). Its numerous copies, dozens of which have survived in manuscript form, are a strong evidence of its popularity. George Chrysokokkes actually introduced Persian astronomy to the 14th century Byzantine Empire and the West. It was a period when Byzantium bitterly criticised the work of Ptolemy, which seemed to fall short of astronomical calculations, as we said before. As a result, the circumstances were suitable for turning to other astronomical traditions and methods of calculation. The work consists of the prologue, 51 chapters and the astronomical tables. The astronomical tables of the Introduction are based on *Zij-I Ilkhani*, the work of Nassir al-Din Al-Tousi.[10] The pattern of the content is as follows: the introduction recites the headings of the following chapters, according to the pattern of scientific works of Late Antiquity. The first chapters are dedicated to calendar questions, while the subsequent chapters study the motion of the Sun, the Moon and of the five known planets (Saturn, Mars, Venus, Jupiter, Mercury). Some chapters are dedicated to the eclipses of the Sun and the Moon, a subject that concerned particularly those who studied astronomy in the 14th century. The last part includes chapters dealing with astrological matters, such as drawing astrological charts and finding the horoscope. The way the position of celestial bodies is calculated facilitates both the drawing of astrological charts and calendar calculations, such as finding the exact date of Easter. *Introduction* includes some methodological mistakes resulting in considerable variances in calculations. Based on the tables of Al-Tousi and without having the relavant text, Chrysokokkes made some mistakes. For example, he mistakenly supposed that the first meridian (72º east) passed by a non-existent city he called Τυβήνη, while he implemented Ptolemaic and Persian formulas at the same time, etc.

In the next years the *Introduction* was followed by a series of books on Persian astronomy, directly influenced by the particular work. One of them was the third volume of the *Three Books on Astronomy (Αστρονομική Τρίβιβλος)*[11] by Theodore Meliteniotes, which was released as an independent text titled *Tradition in the Persian Rough Tables (Παράδοσις είς τούς Πρόχειρους Κανόνας)* and was attributed to Chrysokokkes until recently. Another work directly connected with the *Introduction*, which seems to simplify its tables, was found in Cyprus and must have been written shortly after Chrysokokkes' work.[12]

Theodore Meliteniotes, though following Ptolemy, knew and introduced elements of the Persian astronomy for the sake of comparison. Meliteniotes in his work aims to compare the very long Ptolemaic tradition with the more recent Persian one. He used the same examples in both books so that the differences could be more easily understood. His work had three chapters. In the first book, the writer presents those elements of arithmetic considered necessary for astronomy students, while in the second book he presents the basic elements of the Ptolemaic system. The third book is focused on Persian astronomy. Meliteniotes in this part of the work follows Chrysokokkes and makes the same mistakes, although he attempts

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[7] Maybe the author of *Almanac of Trebizond* for the year 1336. See Anne Tihon, "Numeracy and science", in John Haldon, Robin Cormack, Elizabeth Jeffreys (eds), *The Oxford Handbook of Byzantine Studies*, Oxford University Press, Oxford 2008, p. 807.

[8] Efthymios Nikolaides, "The edition of George Chrysokokkes, *Introduction to the Syntaxis of the Persian*", in Y. Karas (ed.), *The Sciences in the Hellenic Space*, National Hellenic Research Foundation/Trochalia editions, Athens 1997, pp. 136-141 (in Greek).

[9] Gianna Katsiampoura, Efthymios Nikolaidis, "George Chrysokokkes, Introduction to the Syntaxis of the Persians", Digital Encyclopaedia of the Hellenic World, vol. 1, Asia Minor, http://www.ehw.gr/l.aspx?id=7601

[10] R. Mercier, "The Greek *Persian Syntaxis* and the Zij-I Ilkhani", *Archives Internationales d'Histoire des Sciences*, 34, 1984, pp. 34-60; D. Pingree expresses reservations in "Gregory Choniades and Palaeologian astronomy", *Dumbarton Oaks Papers* 18 (1964), pp. 144-145, supporting that the work is based on *Zij al-Alai* written in 1176.

[11] Gianna Katsiampoura, Efthymios Nikolaidis, "Theodore Meliteniotes, Three Books on Astronomy (Tribiblos)", Digital Encyclopaedia of the Hellenic World, vol. 1, Asia Minor, http://www.ehw.gr/l.aspx?id=8495

[12] Anne Tihon, "Un traité astronomique chypriote du XINe siècle I", *Janus*, 64, Amsterdam 1977, pp. 279-308; Anne Tihon, "Un traité astronomique chypriote du XINe siècle II", *Janus*, 66, Amsterdam 1979, pp. 49-81; Anne Tihon, "Un traité astronomique chypriote du XINe siècle III", *Janus*, 68, Amsterdam 1981, pp. 65-127.

to improve the Ptolemaic tables in order to make them seem more accurate.[13] The work aimed to introduce the science of astronomy to the readers and offer them all they needed to continue studying this science.

The Western tradition

The third tradition was influenced by the Karaites Jews, included scholars like Michael Chrysokokkis and Dimitrios Chrysoloras.

One of the main tasks of this tradition is Michael Chrysokokkis' *Book of six wings* (*Εκδοσις γεγονυία είς τό Ιουδαϊκόν Εξαπτέρυγον κατα τό 6943 έτος από τής αρχής τού παντός or Ιουδαϊκόν Εξαπτέρυγον*). The work was written in 1435 and had a considerable spread in the Byzantine and post-Byzantine period. At least 15 manuscripts and reviews survived and also supplement the rules, like those of Damascenos Studite (? - 1577).[14]

This is the Greek version of *Kanfe nesharim* of Immanuel ben Jacob Bonfils from Taraskon. Bonfils belonged to the Jewish community in the Languedoc-Provence, which flourished intellectually in the 13th and 14th century. The *Kanfe nesharim* (*The wings of eagles*, refers to the Exodus) became known as the *Sepher shesh kanafayim* (*Book of six wings*), because the astronomical tables are divided into six groups or wings (refers to Isaiah 6:2).

The book was written around 1365,[15] was translated into Latin in 1406 by Johannes Lucae e Camerino and used by Pico delle Mirandola. It has been also translated into Russian.

A key element of the spiritual connection of Provence in 1365 and Byzantium in 1435 is the history of Karaites. The "textual" Karaites Jews because of their philosophy that denied the oral tradition, and the writings made it evident, were translators to and from various languages: Hebrew, Latin, Arabic, Greek. The Karaitism spread in Europe since the second half of the 11th century by the Byzantines and Spanish students of Abu al-Faraj Furkan, who translated Arabic and Greek texts. Over the following centuries, the Karaites were a sufficient nexus between the various Jewish communities around the Mediterranean. Chances are that Michael Chysokokkis, acquired through the Jewish Karaites circuit prototype.

The main concern of *Book of six wings* was the calculation of eclipses.[16] The calculation of eclipses, as we saw, was one of the chief concerns of the Byzantine astronomers of the 14th and 15th century, and this explains the interest of both Chrysokokki to translate and transfer rules, and subsequent users.

The debate

The introduction of different traditions in Byzantine culture caused a series of controversies. Although the need to revise the Ptolemaic project was mutually acceptable, the proposals put forward differed. Metochites in his two works aimed to clean astronomy contaminants from Eastern, Persian and Islamic influences, which had come through translations. As he writes in *Stoicheiosis,* everything coming from the Persians, the Indians and the Scythians should be avoided, because the Byzantines, as Greeks, had sufficient supply.[17] He believed that only the study of the ancient could provide a solution to the problems that had become apparent. In this context, he claimed continuity with the ancient Greeks and of course, defended the emancipation of philosophy from theology.

On the other hand, the introduction of elements of Islamic tradition had to face a general Byzantine distrust toward anything Muslim. Gregory Chioniadis, as example, because it remained too long in Persia, was forced to draw up a Confession of Faith.[18] Another facet of the great interest in astronomy is the conflicts that broke out between scholars themselves. The most famous of these is the conflict between Nikephoros Choumnos and Theodore Metochites[19] and that between Barlaam of Calabria and Nikephoros Gregoras[20] in response to the prediction of eclipses. The important thing here

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[13] See Efthymios Nikolaidis, "The sciences in Byzantium. The historical tradition of modern Hellenism", in Y. Karas (ed.), *History and Philosophy of Science in Hellenic Space*, National Hellenic Research Foundation, Athens 2003, p. 41.

[14] *Παρέκτασις τών ιθ΄ έτηρίδων Μιχαήλ τού Χρυσοκόκκου*, Dependency of Holy Sepulchre 317.

[15] According M. Steinschneider, *Die Mathematik bei den Juden*, repr. Hildeswheim, 1964, p. 155, but a previous version is possible, because the canons begun in 1340.

[16] P. Solon, *The Hexapterygon of Michael Chrysokokkes*, Brown University, 1968 (unpublished dissertation). P. Solon, "The *Six Wings* of Immanuel Bonfils and Michael Chrysokokkes", *Centaurus,* 15, 1970, pp. 1-20.

[17] «Ελλην ίσθι, τά δέ Ινδών φθέγγεσθαι, ή τά έκ Σκυθών, ή Περσών, ή τούτων δή τίς θάτερα ταύτα δή ταλλότρια», K.N. Sathas, *Medieval Library* I (*Μεσαιωνική Βιβλιοθήκη*), op.cit., pp. πη-πθ.

[18] See Efthymios Nikolaidis, "The sciences in Byzantium. The historical tradition of modern Hellenism", op.cit., p. 40.

[19] I., Ševčenko *Études sur la polémique entre Théodore Métochite et Niképhore Choumnos*, Editions de Byzantion, Bruxelles 1962.

[20] Βλ. T. Mogenet, A. Tihon, D. Donnet, Barlaam de Seminara: Traités sur les éclipses de soleil de 1333 et 1337, Editions Peeters, Louvain 1977, and J. Mogenet, A. Tihon, R. Royez, A. Berg, Nicéphore Grégoras, Calcul de l'Éclipse de Soleil du 16 Juillet 1330, Corpus des Astronomes Byzantins I, Amsterdam 1983.

is that the debate about astronomy covers a range of other causes, especially political strife and struggle for power in the imperial Court. In this case the money by means of scientific knowledge and power in the exercise.

Some first conclusions

From the above, it is clearly outlined a society that goes beyond the strict limits imposed by cultural Christianity and open to different cultures. Byzantium seems to be the crossroads of different cultural traditions, which brings its own cultural capital and creates a new tradition. This creates an identity that includes both the Greek scientific tradition, and others, without religion to no longer be the main feature. This tradition through the manuscript and diplomatic relations will be exported mainly to the 15th century in the West and there will affect the cultural processes.

On the other hand, at this stage, Byzantium, claiming its ancient heritage, led to a secularization, which would conflict with hesychasm. The image of an open and changing universe and man who can know by observation and predict natural phenomena contributed to this perception. The intensity of the relationship between scholars and secular authority in the church-religion will be seen later in the attitude towards the union of churches, which is discussed in the Council of Ferrara-Florence.

Another interest point is also the attitude and the social position of intellectuals. Byzantine scholars appear to be involved with –and to influence– the power, and knowledge seems to be an instrument of social and political recognition.

References

HUNGER H. (1994), *Byzantine Literature*, National Bank of Greece Cultural Foundation, Athens, vol. 3 (in Greek).

KATSIAMPOURA Gianna, NIKOLAIDIS Efthymios, "George Chrysokokkes, *Introduction to the Syntaxis of the Persians*", *Digital Encyclopaedia of the Hellenic World*, vol. 1, Asia Minor, *http://www.ehw.gr/l.aspx?id=7601*

KATSIAMPOURA Gianna, NIKOLAIDIS Efthymios, "Theodore Meliteniotes, Three Books on Astronomy (Tribiblos)", *Digital Encyclopaedia of the Hellenic World*, vol. 1, Asia Minor, *http://www.ehw.gr/l.aspx?id=8495*

KONSTANTAKOPOULOU Aggeliki (1996), *The Byzantine Thessaloniki*, University of Ioannina, Ioannina, pp. 134-135 (in Greek).

MEDVEDEV Igor P. (1993), "The so-called θέατρα as a form of communication of the Byzantine intellectuals in the 14th and 15th centuries", in N.G. MOSCHONAS (ed.), *The Communication in Byzantium*, IBR/NHRF, Athens (in Greek).

MERCIER R. (1984), "The Greek *Persian Syntaxis* and the Zij-I Ilkhani", *Archives Internationales d'Histoire des Sciences*, 34, pp. 34-60.

MOGENET J., TIHON A., DONNET D. (1977), *Barlaam de Seminara: Traités sur les éclipses de soleil de 1333 et 1337*, Editions Peeters, Louvain.

MOGENET J., TIHON A., ROYEZ R. (1983), BERG A., *Nicéphore Grégoras, Calcul de l'Éclipse de Soleil du 16 Juillet 1330*, Corpus des Astronomes Byzantins I, Amsterdam.

NIKOLAIDES Efthymios (1997), "The edition of George Chrysokokkes, *Introduction to the Syntaxis of the Persian*", in Y. KARAS (ed.), *The Sciences in the Hellenic Space*, National Hellenic Research Foundation/Trochalia editions, Athens (in Greek).

NIKOLAIDIS Efthymios (2003), "The sciences in Byzantium. The historical tradition of modern Hellenism", in Y. KARAS (ed.), *History and Philosophy of Science in Hellenic Space*, National Hellenic Research Foundation, Athens (in Greek).

PINGREE D. (1964), "Gregory Choniades and Palaeologian astronomy", *Dumbarton Oaks Papers*, 18, pp. 144-145.

SATHAS K.N. (1872), *Medieval Library* (*Μεσαιωνική Βιβλιοθήκη*) I, Phenix, Venice.

ŠEVČENKO I. 1962), *Études sur la polémique entre Théodore Métochite et Niképhore Choumnos*, Editions de Byzantion, Bruxelles.

SOLON P. (1970), "The *Six Wings* of Immanuel Bonfils and Michael Chrysokokkes", *Centaurus,* 15, pp. 1-20.

SOLON P. (1968), *The Hexapterygon of Michael Chrysokokkes*, Brown University, (unpublished dissertation).

STEINSCHNEIDER M. (1964), *Die Mathematik bei den Juden*, Olms, Hildesheim.

Theodori Metochitae Miscellanea philosophica et historica (1966), C.G. MÜLLER, T. KIESSLING (eds), Leipzig 1861, and A.M. HAKKERT, Amsterdam.

TIHON Anne (2008), "Numeracy and science", in John Haldon, Robin Cormack, Elizabeth Jeffreys (eds), *The Oxford Handbook of Byzantine Studies*, Oxford University Press, Oxford.

TIHON Anne (1977), "Un traité astronomique chypriote du XINe siècle I", *Janus*, 64, Amsterdam, pp. 279-308.

TIHON Anne (1979), "Un traité astronomique chypriote du XINe siècle II", *Janus*, 66, Amsterdam, pp. 49-81.

TIHON Anne (1981), "Un traité astronomique chypriote du XINe siècle III", *Janus*, 68, Amsterdam, pp. 65-127.

THE TRAVELS OF SCIENTISTS IN EUROPE SINCE THE 16TH CENTURY

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INVISIBLE TRAVELLERS AND VIRTUAL TRACKS: KNOWLEDGE CONSTRUCTION IN *COLÓQUIOS DOS SIMPLES E DROGAS DE INDIA...* OF GARCIA DE ORTA (GOA, 1563)[1]

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Abstract

In 1563 it was published in Goa the first modern book about the Asian natural world. Its author, Garcia de Orta (c.1500-1568), was a Portuguese physician who had lived in Asia for more than thirty years. The Colóquios dos simples *contains an accurate description of some of the most important Oriental plants, drugs and spices. The information published by Orta was not only the result of his erudite readings and medical practices, but also of the experience of several field actors.*

In fact, Orta was at the centre of a complex network of political elites, administrative officers, apothecaries, merchants, adventurers and other credible informants. These men were particularly able to answer to specific inquiries about the Asian natural resources. Even if their written reports had a limited circulation, they were available to Orta. Some of these informants were clearly referred by Orta in his Colloquies; *others were only recently identified.*

In this talk, I intend to follow the invisible tracks of some of the informants of Garcia de Orta. These virtual travels will show the different roles played by each field actor in the emerging of a new botanical knowledge about the Asian Portuguese Empire.

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Introductory notes

Colóquios dos simples was the first modern book on plants, spices and fruits from Asia edited by an European. It was published in Goa, in 1563. The text was written by Garcia de Orta, a Portuguese doctor who lived in India from 1534 to 1568. Each chapter of the book describes a virtual conversation between Orta and his interlocutor, a Spanish physician named Ruano. In the first colloquy, Orta presents to his readers his intellectual project. Recently arrived to Goa, the curious Ruano begins the dialogues saying to Orta:

> "I came with a great desire to know about the medicinal drugs (such as are called the drugs of pharmacy in Portugal) and other medicines of this country, as well as fruit and spices. I further wish to learn their names in different languages, and the trees and the herbs which they are taken."[3]

Through the fifty-eight chapters, Orta attempts to reformulate the knowledge about the Asiatic natural world. Aware of the diversity of the printed, manuscript and oral information that circulated in ports, markets and hospitals, both in Europe and in Asia, the physician wants to re-establish coherence to this knowledge.

Starting off from the texts, Orta travels through testimonies which he has reinforced with experience. A new order is thus established through a wide-ranging and patient work of collecting and validating the news he valued as legitimate from each source.

In this article I want to illustrate Orta's *modus operandi* in his appropriation of novelties from the India's natural world. In order to establish himself as an authority, Garcia da Orta uses both his own experience and the testimony of people in whom he trusted.

Texts

Garcia de Orta appears to know profoundly a vast bibliography of medicine and botany[4]. He reveals to the reader his wide medical practice, which in 30 years of living in India has made him well acquainted with the uses of drugs unknown in Europe. If his trust in *bezoars*, *herbs of Malabar*, *teriacas, pau-de-cobra, pau-de-Malaca* or *raiz–da-China* demonstrates his openness to what is new[5], it is also remarkable his ease in the therapeutic application of some products from the West Indies, such as the guaiaco which he already took with him in 1534 when he set off to Goa[6].

The belief in these new products, unknown to the Latin, Greek and Arab authors, opened to Orta the perspective of becoming the new Authority from those faraway places. Nevertheless, if Orta's project did not fulfil the requisites of credibility demanded by such an epistemological change, his work would have resulted in a major failure. Therefore, Orta's methodology had to obey rules that were implicit to the minds of the community of wise men from which he came, and by which he wanted to be recognized.

That is why Orta never dispensed with the texts. Texts were, as often as possible, the departure point for each intervention. It was this knowledge, fixed in words, that Orta wanted to challenge. But Orta did not base himself solely on books. Besides a vast library, the physician had an important collection of precious stones and bezoars, as well as samples of seed, gums, resins, fruits and woods, which he exhibited to his curious visitors. In the same way, he kept an orchard which trees and vegetables which provided him with flavoursome fruits and with the materials for his therapeutic experiments[7].

Orta only stands for what he has ensured that is true[8]. The novelty that he describes results, first and foremost, from his experience. The drugs which Orta saw or experimented were the ones he described more assuredly[9].

Orta –who produced the drugs of the King[10], who served in the courts of Cambay and of Deccan, who had close contact with rulers, religious men and civil servants posted to the Orient, who practiced medicine in a hospital in Goa, who

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[3] Garcia de Orta, *Colloquies on the Simples and Drugs of India,* translated by Sir Clements Markham. Indian Medical Science Series n°5, Delhi. 1987, 1-2.

[4] R. M. Loureiro, *Garcia de Orta e os Colóquios dos Simples: observações de um viajante sedentário.* Colóquio Internacional e interdisciplinar Alexander van Humboldt-Garcia de Orta: errâncias, investigações e diálogo de culturas. Lisboa, 2007, 135-145.

[5] Garcia de Orta, *Colloquies*,145.

[6] Garcia de Orta, *Colloquies,* 379.

[7] Teresa N. Carvalho, *Colóquios dos Simples de Garcia de Orta: conversas no interior da Índia*. Colóquio Internacional e interdisciplinar Alexander van Humboldt-Garcia de Orta: errâncias, investigações e diálogo de culturas. Lisboa, 2007, 165-174.

[8] As Orta said to his interlocutor: "Do not try to frighten me with Dioscorides or Galen because I merely speak the truth and say what I know." Garcia de Orta, Colloquies, 60.

[9] Garcia de Orta, *Colloquies*, 125.

debated knowledge with apothecaries and physicians, who talked with merchants, travellers and *men worthy of faith*– validated a wide diversity of knowledge.

The merchant routes, the main ports of supply, the origin and destination of the drugs, prices, weights or measurements were information to which Orta attributed as great a value as to the names of the drugs, the description of the plants, their qualities or ways of application.

News that Pliny, Dioscorides or Avicenna had gathered and, since the arrival of the Portuguese to the East, were complemented and sometimes put in question, by reports like those of Duarte Barbosa or Tomé Pires, whose manuscripts had a wide circulation.

These Portuguese travellers were pragmatic observers. They witnessed, to a Europe *deprived* of novelties, an empiric approach to the Asian natural world. And yet, myth continued to be a part of the language they used. The possibility of the existence of the wonders of Pliny, Polo, Conti, Mandeville or Varthema remained in those lands that these men could not reach.

Orta tried to denounce some of their excesses, maintaining nevertheless the possibility of survival of some of the myths. His amazement, faced with the unexplained behaviour of the *árvore-triste*, of the *erva-mimosa* or of the tamarind leaves, lead him to simply register the mysterious, without searching for a justification.

On the other hand, the myth around the magical properties of the unicorn horn or of the *coco-das-maldivas* leaded him to caution. He promised in these instances to keep his readers informed on the progress of investigations. We find the same prudent approach on the disclosure of therapeutic virtues of some bezoars or of the hedgehog stone.

Context

The practices of a whole group are then gathered in *Colóquios dos Simples*. In his text Garcia da Orta includes reports of merchants, physicians, apothecaries, sovereigns or administrative officers. As I will show bellow, the type of information that each professional gives to the doctor depends on his own practical competence. In order to validate each testimony, Garcia da Orta associates the content of the news to the technical ability of his informants. He does not ask a merchant for his medical views, as he does not interrogate a lapidary on horses' prices.

The credibility of Orta's speech depends therefore on his judgement in analysing the news that are brought to him, as well as on the way in which he describes his own experience. He aims at confronting the past at every opportunity. His knowledge emerges not from a whim, nor from odd observations, not even from a simple pragmatic interest, but from a dialogue with Antiquity.

The Courts at Cambay and Deccan

Orta divulges in his work the information collected at the Courts of these exceptional interlocutors. He relates the veracity of the information to the high standard of their provenance.

At the Court at Cambay, Orta associates the strategic importance of the alliance established between the Sovereign and the Portuguese elites with the trust in the information brought by his Persian, Turkish and Arab doctors. The news about the *raiz-da-China*, secretly entrusted to him by a nobleman[11]; on the effects of the bangue, which Orta collected from Bahadur's secretary, or the confidences from the Sultan to Martim Afonso de Sousa[12], all this gathers greater relevance for being associated to the elites of Cambay. The success of the assault of the Capitan-General Martim Afonso de Sousa to Diu –an important achievement of the Portuguese political and commercial strategy– seems to go beyond the tactical value of the Portuguese presence at that fortress. The relationship that was established between the two politicians or between the Portuguese elites and the local courtiers manifested its richness also in the construction of a new knowledge.

The same mechanism was used to divulge the news collected at the court of the Deccan. Nizamoxa, the powerful and wise king, is introduced to us as our doctor's personal friend. The friendship between the two men is presented as an opportunity to reveal confidences.

[10] Garcia de Orta, *Colloquies*, 152.

[11] Garcia de Orta, *Colloquies,* 379.

[12] Garcia de Orta, *Colloquies,* 55.

But even if it seems that Orta might have cured Nizamoxa several times, who rewarded him with much wealth and amazing offers[13], he still could not manage to get the sovereign to entrust him with the secret composition of *teriaca*[14]. The physician insistently questioned the king on the list of components of that magical medicine, and yet never managed to get more than an evasive reply.

Informants

Administrative officers and merchants

Information gathered from administrative officers seems to equally dispense with proof. Coje Percolim[15], Jorge Gonçalves[16], Diogo Pereira[17], António Pessoa, António Galvão, Simão Alvares or Bastião Lobato are characters whose technical expertise or professional abilities seem sufficient to validate the information which they provide to Orta. The reliability of the novelty is therefore ensured by the credibility of these agents. The fact that this empirical information was shared with Orta by the King's officers reveals that the physician was close to the Portuguese elites in Goa and attests to the fact that Orta was part of the decision-making circles. In a two-way relationship, these men bring him information, expecting from Orta the validation of their knowledge as well as the eventual establishment of commercial networks.

But Orta also trusts merchants. He solicits information about the origins and prices of the drugs they trade. Perhaps as evidence of their friendship and appreciation, some offer him valuable gifts[18], others confide him with their secrets[19]. We thus learn how to falsify camphor or how to discern true from fake gems. Still others confirm to Orta the whereabouts of cities they know, like Babylon[20] or Bukhara[21]. And yet not all of them manage to inspire the physician's trust. In fact he alerts his readers to the lies told by lapidaries[22], as well as to the lack of scruples of the merchants in Diu's bazaar[23]. The sharing of secrets is a transitional mechanism often used by Orta. Rather than keeping to himself these secrets, liable to be of interest to many, he shares them with his audiences. By teaching the reader to be able to distinguish fake from true, Orta gains the trust of those he addresses.

Apothecaries and doctors

Doctors and apothecaries were also privileged informants. The doctors of Cairo, Aleppo and Damascus with whom Orta has relationships provide him with reliable information resulting from their own experiences[24]. The Arabs instruct him on the synonymy in the various regional languages. The Gentile doctors explain Orta the efficacy of some of the local folk medicines[25].

The apothecaries take a prominent place in the *Colóquios dos Simples*; because they made the drugs of Cambay, they had profound knowledge of their trade and were particularly trustworthy. In two separate chapters they are presented as close interlocutors to the Governors[26]. Politicians seem to have greater trust on the pragmatism of these men on the ground and on their good sense in the relationship with local chiefs, rather than on the knowledge of academics. Nevertheless, Orta demonstrates that the knowledge of the apothecaries does not surpass his own.

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13 Garcia de Orta, *Colloquies,* 68.

14 Garcia de Orta, *Colloquies,* 30.

15 Garcia de Orta, *Colloquies,* 7, 43.

16 Garcia de Orta, *Colloquies,* 282.

17 Garcia de Orta, *Colloquies*, 27.

18 Garcia de Orta, *Colloquies,* 388.

19 Garcia de Orta, *Colloquies,* 92.

20 Garcia de Orta, *Colloquies,* 282.

21 Garcia de Orta, *Colloquies,* 42.

22 Garcia de Orta, *Colloquies,* 355.

23 Garcia de Orta, *Colloquies,* 433.

24 Garcia de Orta, *Colloquies*, 110, 374.

25 Garcia de Orta, *Colloquies*, 218, 223.

26 Garcia de Orta, *Colloquies*, 108, 374.

'People worthy of faith'

Finally, 'people worthy of faith': this anonymous mass that Orta trusts, brings him samples, information and news from the regions through which they travel. It is through them that the doctor complements his information on the benjuy[27], cloves[28], camphor[29], *coco-das-maldivas*[30]*, cubebas*[31]*, avacari*[32]*, coru, lacre*[33], incense[34] or the metallic aloe.

None of these men and women, pragmatic people and keen observers, bothers with the past of these drugs. Their knowledge focuses on the present and projects itself onto the future. They have little or no interest in comparing the sample with the text or with the drawing, and even less in understanding the intrinsic value that each plant has within the local culture. Taken out of their mystical context, each plant, stone and animal has merely the value of a drug or a panacea. To these pragmatic informants, only the empirical knowledge, the practical use, the price or the strategic potential are of interest.

Orta's credibility

Orta evokes multiple artefacts to affirm himself as an authority. The care he takes in the selection of credible informants and in the divulgation of plausible news makes his text believable.

The arrival of correspondence during the conversations between Orta and his interlocutor and the close vigilance paid to the comings and goings of ships at Goa's harbour reveal a physician who is deeply attentive and informed about all the news which arrive from the four corners of the World to the capital of the Portuguese Overseas Empire.

The doctor also needs to present himself as a witness of facts. Orta demonstrates his insertion in the Portuguese context. The military campaigns in which he accompanied Martim Afonso de Sousa allow him to visit the *Elefanta* Temple[35]; the news which he collected about the second siege of Diu, or the statements he makes about the betrayal of Magalhães, all these give credibility to Orta before a Portugues audience. The reliability of his account, and the exactitude of his historic discourse, legitimate the novelty of all that Orta saw, heard, experienced and enquired about the Asian natural world. His importance in the complex and fragile game of political interests is also made clear by the trust that he gained with the rulers of Deccan, Cambay and Cochin.

The news which Orta wants to validate is therefore supported by its clear insertion in the world of the Portuguese political elites… but not only in Goa, since the physician makes a clear reference to the property in Bombay which was given to him by the King[36].

To assert his own authority, Garcia de Orta aptly manages both scientific knowledge and personal relationships.

The relationship which Orta establishes with his readers presupposes complicity. By sharing in *Colóquios dos Simples* his secrets, Orta makes the reader his accomplice.

More that cyanide or hemlock, Orta feared the "*idle people and the sharp tongues"*[37]. The physician hopes to obtain recognition of his authority through the sharing of his knowledge with his readers.

Final notes

During three decades of living in Asia, Garcia de Orta established invisible links that attached him to each of his informants. Written to legitimize his authority as a physician, *Colóquios dos Simples* is Garcia de Orta's greatest tribute to all those who, through those first decades of Portuguese presence in Asia, travelled through the lands of the Orient, enquiring,

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27 Garcia de Orta, *Colloquies*, 59.

28 Garcia de Orta, *Colloquies*, 217.

29 Garcia de Orta, *Colloquies*, 92.

30 Garcia de Orta, *Colloquies*, 145.

31 Garcia de Orta, *Colloquies*, 170.

32 Garcia de Orta, *Colloquies*, 233.

33 Garcia de Orta, *Colloquies,* 241.

34 Garcia de Orta, *Colloquies,* 448.

35 Garcia de Orta, *Colloquies*, 444.

36 Garcia de Orta, *Colloquies*, 193.

37 Garcia de Orta, *Colloquies*, Carta ao muy ilustre Senhor Martim Afonso de Sousa.

taking samples, taking risks, aiming to build a new body of knowledge about Asia's natural world. A utilitarian and empirical view which completes and corrects the truths for a long time established in text. In 1563, *Colóquios do Simples* relinquishes to Europe a plural knowledge, resulting from a new way of approaching reality.

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THE GERMAN AND EUROPEAN NETWORK OF THE PROFESSORS OF MATHEMATICS AT HELMSTEDT IN THE SIXTEENTH CENTURY[1]

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Abstract

This paper addresses the 16th century international scientific network of the professors of mathematics appointed at the University of Helmstedt. It presents part of the results of a research project accomplished at the Herzog August Library of Wolfenbüttel (Germany) in 2010.
The Protestant University of Helmstedt had two chairs of mathematics as was prescribed by Melanchthon's University Reform. Since its foundation (1576), it was strictly connected to Wittenberg and other Protestant universities. Most of its 16th century professors of mathematics came from the University of Rostock. They were supported by Heinrich Brucaeus, a Flemish Professor of mathematics and medicine who had studied in Gent and Bologna, taught in Rome, and worked as a physician in Belgium and Portugal and, apart from that, was well acquainted, among others, with the Danish astronomer Brahe. Brucaus helped his countryman Cornelius Martini to become part of the Philosophical Faculty of the University of Helmstedt and introduced Magnus Pegel (Professor of lower mathematics 1575-81), Franciscus Parcovius (lower mathematics 1586-90), and the Scotsman Duncan Liddel (lower mathematics 1591-93, superior mathematics 1593-1600). Other academics were: Erhard Hofmann (Professor of superior mathematics 1576-93), who came from the University of Jena, and Simon Menz (Professor of inferior mathematics 1593-1606), a Melanchthonian humanist from Wittenberg. Remarkably, the latter was involved, together with the Scott Magister John Johnston, in a cosmological dispute against theses of the Italian philosopher Giordano Bruno, who participated in the cultural life of Helmstedt University around 1588-1590.
The biographies and works of the Helmstedt mathematicians show a late-Renaissance scientific network over a North-European area (German Lutheran Universities-Breslau-Denmark-Flanders-Scotland). I would also like to highlight the importance of this network for the circulation of scientific theories (especially in connection with post-Copernican astronomy).

[1] This research was accomplished in Wolfenbüttel and Berlin with the support of fellowships from the *Herzog August Library* and the *Max Planck Institute for the History of Science Berlin* in 2010.

The University of Helmstedt was founded in 1576 as the *Academia Iulia Helmstediensis* by Duke Julius of Braunschweig (1528-1589) in order to consolidate the Reformation which he had introduced into his realm. In fact, Julius considered the university to be a powerful means to forge a new class of Lutheran theologians and administrators. One of the principal authors of its statutes was the learned theologian David Chyträus (1531-1600), a professor at Rostock who endorsed Philipp Melanchthon's cultural program and thus organized the new university following the models of Wittenberg and Rostock. In particular, the curriculum was inspired by the so-called 'German late humanism,' or *deutscher Späthumanismus*.[2]

The University flourished especially after Duke Heinrich Julius (1564-1613) succeeded his father in 1589. Thanks to his patronage and his renown as a learned man, he attracted to Helmstedt some leading exponents of the humanist culture, science and philosophy of the time: the man of letters Johannes Caselius (1533-1613) in 1590, the mathematician Duncan Liddel in 1591 and the logician Cornelius Martini (1568-1621) in 1591. Moreover, the Renaissance philosopher Giordano Bruno resided in Helmstedt as a member of the university and a protégé of the Dukes from January 1589 until April 1590. It should be noted that these people were foreigners or of foreign origins: as Caselius's family came from the Netherlands, Liddel came from Aberdeen, Martini from Antwerp, and Bruno from Nola by Naples. Duke Heinrich Julius himself was open to foreign cultural influences: among other things, he established at his court in Wolfenbüttel a company of English actors,[3] he personally visited the Danish astronomer Tycho Brahe at his observatory-castle on the island of Hven,[4] and resided at the magnificent court of Emperor Rudolph II in Prague from 1607 until his death in 1613, when Kepler was imperial mathematician there.[5]

Following Melanchton's *ordo studiorum*,[6] the University of Helmstedt attached great importance to mathematics. Two chairs of the philosophical faculty were devoted to the teaching of mathematics, at least at the beginning. They were divided into a lower (or elementary) class and a higher. The following table sums up all available information on the lectures during the half century after the opening of the University:[7]

Semester[8]	**Lower mathematics**	**Higher mathematics**
1581 A	(Prof. Pegel) -Geometrica ex Euclide -Astronomica ex Cornelio Valerio	(Prof. Hofmann) -Arithmetica Frisii -Theoriae planetarum -Doctrina triangulorum
1582 A	vacant	-Elementa geometriae Euclideae -Precepta arithmetices Gemmae Frisii -Doctrina secundorum mobilium eorundemque usum in Tabulis Prutenicis
1587 B	(Prof. Parcovius) -Arithmetica vulgaris Gemmae Frisii -Cosmographia Honteri	-Secundorum mobilium theoria -Doctrina triangulorum planorum et sphaericorum -Praecepta arithmeticae cossicae
1594 B	(Prof. Menz) -Geometriae tractatus -Arithmetici libelli -Ratio conscribendi calendaria anniversaria de syderum motibus atque congressibus -Doctrina sphaerica e libello Ioannis de Sacrobusto	(Prof. Liddel) -Geometriae fundamenta figurarum usum et geodesiam una cum triangulorum doctrina -Theoriae coelestium motuum iuxta triplicem hypothesin una cum tabularum tum Alphonsinarum quam Prutenicarum explicatione

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2 See the introduction to *Die Statuten der Universität Helmstedt*, ed. P. Baumgart and E. Pitz (Göttingen 1963).

3 R. Friedenthal, *Herzog Heinrich Julius von Braunschweig als Dramatiker. Sein Leben. Mit besonderer Berücksichtigung seines geistigen Werdegangs* (1922), ed. G. Biegel (Braunschweig 1996).

4 Cf. Gassendi, *Opera Omnia* (Lugduni 1658, anastatic reprint Stuttgart-Bad Cannstatt 1964), V, 468.

5 H. Lietzmann, Herzog *Heinrich Julius zu Braunschweig und Lüneburg (1564-1613). Persönlichkeit und Wirken für Keiser und Reich* (Braunschweig 1993).

6 Cf. H. Kathe, *Die Wittenberger philosophische Fakultät 1502-1817* (Köln-Weimar-Wien 2002), II, "Reformation und protestantische Humanismus 1517-1560".

7 Here and in the following information about lectures is derived from: http://uni-helmstedt.hab.de (7 Nov. 2010).

8 The capital letter A indicates the summer semester and B the winter semester.

Semester[8]	Lower mathematics	Higher mathematics
1595 B	-Sphaericae doctrinae elementa -Anniversaria calendaria -Arithmetica Gemmae Frisius	-Theoriae coelestium motuum iuxta triplicem hypothesin, una cum tabulis Alphonsinis et Prutenicis -Quadripartitum Ptolomei -Tabulae Directionum
1597 A	-De primo motu doctrina sphaerica -Rationes geometrice investigandi insignorum urbium, insularum et regionum intercapedines ex tabulis sinuum rectorum sive semichordarum -Arithmetica Gemmae Frisii	-Secundum et tertius liber Pomponii Melae una cum historia et descriptione regionum secundum recentiores -Quadripartitum Ptolemei et Tabulae Directionum.
1599 A	-Doctrina Sphaerica -Tractatus de Iudiciis ecclipsium et cometarum -Arithmetica practica	-Doctrina sinuum et triangulorum -Theoriae planetarum, secundum hypothesin Ptolomaei et Copernici, et illam Mundani systematis hypothesin, quam describit Tycho Brahe, lib. 2 De aethereis phaenomenis -praecepta calculi Alphonsini, et Prutenici, ac praeterea supputatione apparentiarum coelestium in singuliis theoriis, ex observationibus Ptolemaei et Copernici, per doctrinam triangulorum
1600 B	-Tractatus quadripartitus -Quaestiones sphaericae Hartmanni Beieri -Doctrina de calendariis anniversariis conscribendis et prognosticis astrologicis subinde attexendis	vacant
1602 B	-Elementa mathematicis -Arithmetices practices (Gemma Frisius) -Sphaera a Iohanne de Sacrobusto conscripta	(Prof. Schaper) -Doctrina secundorum mobilium, iuxta hypotheses Copernici et Alphonsinorum
1603 B	-De Sphaera libellum -De tempestatibus praecognoscendis regulas astrologicas -Arithmetica practica -Computus ecclesiasticus Iohannis de Sacrobusto	-Geographica -Priores Euclidi libri
1604 B	-Arithmetica -Sphaericum libellum D. Casparis Peuceri de circulis coelestibus et primo motu	-Euclidis -Pomponius Mela de situ orbis
1613 B	vacant	-Doctrina planetarum iuxta Alfonsinos et Copernicum
1620 A		-Doctrina Sphaerica seu motus coeli primi
1623 A		-Regulae arithmeticae practicae vulgares -Doctrina primi mobilis
1625 A		-Doctrina primi mobilis seu sphaerica

From this overview, however incomplete it is due to the lack of documents relative to many semesters, it is possible to trace some relevant features of the *curriculum studiorum* at Helmstedt. Programs show a certain continuity over the years: Euclidean geometry, spherical astronomy and arithmetic in the introductory class, and trigonometry, planetary theory and astronomical computation in the higher. It should be remarked that geography also was part of the regular curriculum of mathematics. Some professors (Mencius and Liddel) taught astrology and calendar computation. Hofmann offered lectures on algebra. Beginning in 1594, thanks to Professor Liddel, Copernicus's planetary hypotheses were presented to students along with the traditional Ptolemaic system (and, in the case of Liddel, also the geo-heliocentic system of Tycho Brahe).

From 1576 to 1629, six professors taught mathematics: Magnus Pegel, Erhard Hofmann, Franz Parcovius, Duncan Liddel, Simon Menz and Heinrich Schaper. Here an overview follows:

Year	Chair of lower mathematics	Chair of superior mathematics
1575	1. Magnus Pegelius	
1576		2. Erhardus Hofmannus up to his death (1593)
1581	Pegel abandons Helmstedt	
1582	Hofmann holds both chairs?	
1586	3. (3 October) Franciscus Parcovius	[John Johnston's astronomical disputations]
1590	Parcovius moves to the Faculty of Medicine	[Giordano Bruno at Helmstedt, January 1589-April 1590]
1591	4. (24 July 1591) Duncan Liddel	
1593	5. Simon Mencius, already professor of Latin (dies in 1606)	Duncan Liddel up to 1600
1601		6. Heinrich Schaper
1606	vacant (Schaper both chairs?)	
1629		(October) Schaper's death

As a matter of fact, all professors who occupied a chair of mathematics in the sixteenth century had received their education at a Lutheran university: Jena, Wittenberg, Frankfurt on Oder and Rostock, or Helmstedt itself. This already conveys the sense that Helmstedt belongs to a network of universities sharing a confessional background. In this framework, Rostock had a special importance. Pegel, Parcovius and Liddel, but also other eminent professors of the philosophical faculty like Caselius and Martini, came from there. In particular, the three mathematicians were tied with the Rostock professor Heinrich Brucaeus, himself a Flame, and, following in his footsteps, they chose a further career as physicians.

Scholar	Education
Magnus Pegel	Rostock
Erhard Hofmann	Jena
Franz Parcovius	Rostock
Duncan Liddel	Frankfurt (Oder), Breslau, Rostock
Simon Menz	Wittenberg
Heinrich Schaper	Helmstedt

The 'Rostock professors' of the philosophical faculty shared a humanist background and an inclination toward Melanchthon's teaching, or 'Philippismus.' Menz had studied in Wittenberg under the guidance of the *praeceptor Germaniae*. It is therefore not surprising that he chose, for his classes on astronomy, a textbook of Melanchthon's son-in-law Peucer. Another noteworthy aspect of the University of Helmstedt is the presence of many foreigners in the philosophical faculty. Liddel and Martini were born beyond the borders of the Empire, Caselius had Dutch origins, and their friend and supporter Brucaeus was Flemish. To the list of foreign scholars one could also add Bruno. Furthermore, it is remarkable that several Scotsmen seem to have studied, at least for a period, at Helmstedt: for instance Master John Johnston who, around 1586, defended the Aristotelian cosmology in two above-mentioned disputations, and, a few years later, the alchemist Duncan

Burnet of Aberdeen who took his degree in medicine under Parcovius in 1608.[9] Additionally, Caselius hosted Duncan Liddel in his house and, as we read in a letter of his, a nephew of John Craig.[10]

Apart from the classics from antiquity and the Middle Ages, also the choice of the textbooks for the teaching of mathematics clearly indicates a north-European cultural horizon. The following table sums the textbooks mentioned in the syllabi:

Author, *Textbook*	**Semester**
Euclid, *Elements*	1581A, 1582A, 1603B, 1604B
Gemma Frisius, *Practical Arithmetic*	1581A, 1582A, 1587B, 1595B, 1597A, 1599A, 1602B, 1603B
Sacrobosco, *Sphere*	1594B, 1602B
Sacrobosco, *Computus ecclesiasticus*	1603B
Ptolemy, *Quadripartitum*	1595B, 1597A, 1600B,
Pomponius Mela, *Chorography*	1597A, 1604B
Alfonsine Tables	1594B, 1595B, 1599A, 1602B, 1613B
Reinhold, *Prussian tables*	1582A, 1594B, 1595B, 1599A, 1602B?, 1613B?
Regiomontanus, *Tabulae directionum*	1595B, 1597A
Valerius, *Physica*	1581A
Brahe, *De mundi aetherei recentioribus phaenomenis*	1599A
Copernicus, *De revolutionibus?*	1599A?
Peucer, *De circulibus coelestibus et motu primo*	1604B
Beyer, *Quaestiones in libellum de sphaera*	1600B
Honter, *Rudimenta cosmographica*	1587 B

Helmstedt professors lectured on several standard works: Euclid's *Elements* for geometry, Sacrobosco's *Sphere* for spherical astronomy, Ptolemy's *Tetrabiblos* for astrology and Pomponius Mela's *Chorography* for geography. It shall be however stressed that Melanchthon encouraged the employ of such classics, as emerges in particular from his introductions to Sacrobosco and *Tetrabiblos*. Exercises of astronomical computation relied upon the *Alfonsine Tables* as well as the 'Copernican' *Prussian Tables* (*Prutenicae tabulae*) of the Wittenberg professor Erasmus Reinhold (1511-1553). Among sixteenth century textbooks, the *Arithmetic* of the Flemish mathematician, geographer and instrument maker Reiner Gemma Frisius, professor at Louvain, was used uninterruptedly. The textbook of astronomy employed in the summer class of 1581 refers to a work of the Louvain professor of the Trilingual College Cornelius Valerius (1512-1578), perhaps his *Physica seu natura philosophiae institutio* (Antwerp, 1572) which contains a traditional description of the cosmos, celestial spheres, elements and meteorological phenomena. Apart from Reinhold's *Prussian Tables*, other works stemming from the Wittenberg academic milieu are the commentary on Sacrobosco, *Quaestiones in libellum de sphaera Ioannis de Sacrobusto*, of the theologian and mathematician Hartmann Beyer (1516-1577) and the *Elementa doctrinae de circulis coelestibus et primo motu* of the Wittenberg theologian and professor of mathematics Kaspar Peucer (1525-1602). The book on geography for the winter semester 1587 is plausibly the *Rudimenta cosmographica* of the fervent Lutheran Johannes Honter of Transylvania. Professor Liddel's lectures are apparently the most ambitious, as he lectured on advanced scientific books: Tycho Brahe's *De mundi aetherei recentioribus phaenomenis* (Uraniborg, 1588) for planetary theory and Johannes

[9] D. Burnet, Propositiones... de virginum cachexia pro licentia sumendi doctoris medicinae (Helmstedt 1608).

[10] J. Caselius, epistle to J. Craig (Helmstedt, 1 Maz 1607), in D. Liddel, *Ars medica* (Hamburg 1608), f. †8*r*.

Regiomontanus's *Tabulae directionum* for trigonometry. It is also plausible that he used Copernicus's *De revolutionibus orbium coelestium*, of which he owned two copies, to introduce the heliocentric hypothesis, as announced in the syllabi. Even relative to geography, he sought to integrate classical and modern sources, as he lectured on Pomponius Mela as well as on "histories and descriptions of lands according to the most recent [explorers/scholars]" (*historia et descriptione regionum secundum recentiores*). One can assume that he employed a book like Simon Grynaeus's *Novus orbis*, a collection of reports on western as well as oriental countries.

In a study on the international network of the University of Helmstedt from its origin until its closing in 1810, Rolf Volkmannm stressed its early connection to northern-European protestant countries.[11] As this scholar showed, in the first years many students as well as the professor of physics Nicolaus Andreas Granius (ca.1569-1631) came from Lutheran Sweden. Several professors where Protestants from Flanders, in particular Antwerp (the physician Johann Böckel, Cornelius Martini, the theologian Johannes Fuchte) or had studied abroad, in particular at Louvain (the jurist Virgilius Pingitzer) and Basel (the jurists Johann Borcholten, Johann Jagemann, Andreas Cludius and others) - as an exception to this northern-European horizon, one can mention the fact that Caselius had studied also at the Italian universities of Bologna and Pisa. In the seventeenth century Helmstedt developed close contacts with the University of Leiden in the Netherlands.

It shall be remarked that the religious element influenced not only the teaching of theology, but also the international network of the mathematicians. This fact clearly emerges from the biographies of the above mentioned professors. I would like to show this on the basis of two of them: Magnus Pegel and Duncan Liddel.

* * *

Magnus Pegel (1547-ca.1618?) was born in Rostock and studied in his hometown where he graduated in 1569. He was appointed at the University of Rostock as a professor beginning in 1572. Thanks to the support of his father-in-law David Chyträus and, perhaps, of Brucaeus, he was appointed at Helmstedt as a professor of mathematics already in 1575, that is, a year before its opening. In 1581, he was dismissed for his dissipated behaviour (possibly alcohol abuse). Duke Julius wanted to keep him in Wolfenbüttel as a court mathematician, perhaps to benefit from his technical competencies, but Pegel preferred to return to Rostock. There, he took a degree in medicine and maybe worked for a while as a physician. In 1591, he became a professor of mathematics at Rostock and taught there until 1605 when he was dismissed. He fled to Prague where he stayed at the court of Rudolph II until the Emperor's death in 1612. From his writings, we know that he was familiar with the Landgraves Wilhelm IV and Moritz of Hessen-Kassel, who were generous patrons of astronomy, and their instrument builder Jost Bürgi.[12] Pegel reported also that he visited Brahe whom he admired for his astronomical instruments and data recording but not for his geo-heliocentric cosmology which he, in fact, rejected.[13] He also sojourned at some point in Florence where he accomplished some stellar observations in order to compare the latitude of the Italian town with that of Rostock and measure the Earth's radius.[14]

Pegel publications are very rare: *Universi seu mundi diatyposis* (Rostock, 1586) and *Aphorismi thesium selectarum de corporibus mundi totius primariis* (Rostock, 1605), both of which tackle astronomy and natural philosophy, *Thesaurus rerum selectarum* (Rostock, 1604), which presents medical and technical inventions as well as considerations on jurisprudence, and a discourse on language and printing.[15] These publications reveal an extraordinary technical fantasy and very original natural views. Among the inventions presented in the *Thesaurus*, a work dedicated to the Emperor Rudolph II, some are quite ambitious and surprising, for instance the project of a submarine (*navigium submarinum sive subaquaeum singulare*), the feasibility of which is however quite difficult to believe. His most innovative philosophical and cosmological theses are: the cosmos is a finite sphere included in infinite space; the material spheres deputed, according to traditional cosmology, to transport planets, do not exist; the sky is homogenous, and constituted of air; all celestial bodies (stars, planets and comets) are made out of the same elements; stars and planets are alive; astronomy would do better without mathematical hypotheses because physical explanations should be preferred to mathematical ones. Moreover, Pegel supported the Capellan planetary system, according to which the inferior planets, Mercury and Venus, encircle the Sun. He also reassessed the possibility of physical vacuum, surprisingly identifying it with absolute space (*locus sine corpore*) and a vitalistic principle.[16] All of these theses are revealing of a radically anti-Aristotelian world view. Additionally, the requirement of an astronomy *sine hypotheses* can be traced back to the French philosopher Pierre de la Ramée (1515-1572), who

[11] R. Volkmann, Academia Julia. Die Universität Helmstedt (1576-1810) (Helmstedt 2000).

[12] M. Pegel, *Thesaurus rerum selectarum* (Rostock 1604), 73-4.

[13] *Ibid.*, 75-6.

[14] Id., *Aphorismi thesium selectarum de corporibus mundi totius primariis* (Rostock 1605), ff. B1r-v.

[15] Id., *Oratio*, in Herzog Augusti of Braunschweig, *Orationes et edicta publice proposita* (Rostock 1594), ff. A3*v* and ff.

[16] Cf. P.D. Omodeo, "Disputazioni cosmologiche a Helmstedt, Magnus Pegel e la cultura astronomica tedesca tra il 1586 ed il 1588," *Galilaeana* 8 (2011).

rejected the traditional mathematical approach to astronomy.[17]

Connections: Rostock (education through Konrad Pegel, David Chyträus and Brucaeus); Hven (Brahe); Kassel (Wilhelm IV and Moritz of Hessen-Kassel, and Bürgi); Hven (Brahe); Florence; Prague; Stettin.

* * *

Duncan Liddel (1561-1613) was a mathematician and physician. Much information about his life can be derived from a letter (Helmstedt, May 1607) of Caselius to the Scottish mathematician and court physician to King James VI of Scotland and I of England. Liddel left Scotland to study in Europe. He sailed to Danzig and reached Frankfurt on Oder, at which University he matriculated. He attended the classes of Craig, who was at that time professor there. Liddel stayed then in Breslau, where he entered the humanist and scientific circle of the Italo-Hungarian man of letters Andreas Dudith-Sbardellati (1533-1589) and the physician von Krafftheim (1519-1585). There, he studied mathematics under Paul Wittich (ca.1546-1586), one of the most highly thought of German mathematicians of the time, who strongly influenced Brahe on his way to the invention of the geo-heliocentric planetary model.[18] Liddel then returned to Frankfurt on Oder (1582-1583) to study medicine and teach mathematics and philosophy. He subsequently headed to Rostock (beginning in 1585), at which University he was warmly welcomed by Heinrich Brucaeus, and met Caselius, who would later invite him to Helmstedt. In this period, Liddel visited Brahe on Hven (in 1587) and became familiar with the research projects accomplished at the latter's observatory. Caselius reports that Liddel taught Copernicus's planetary hypotheses along with the Ptolemaic and the Tychonic already in Rostock, and that it was the first time that such doctrines were taught together at a German university.[19] This information about Liddel's lectures of astronomy is confirmed by a letter of a student of his, Daniel Cremer, who attended the courses of mathematics in 1588 and 1589 (*Docuit Duncanus Liddelius Scotus in Academia Rostochiana Mathemata, quando ego auditor fui anno 88 et sequenti*): in his classes, the professor taught planetary theory (or the doctrine of 'second motions') according to Ptolemy (the 'followers of Alfonso X of Castille' are explicitly indicated), Copernicus and the 'third new' hypothesis, that is, geo-helicentrism (*prima [hypothesis] Alphonsinorum, secunda Copernici, et alia tertia nova*).[20]

Liddel came to Helmstedt on Caselius's advice. In a letter of recommendation to the Academic Senate preserved in the *Niedersächsisches Staatsarchiv Wolfenbüttel*, Caselius (on 1 January 1591) emphasized Liddel's mathematical expertise and stressed his close connection to Brucaeus and the acquaintance with Brahe.[21] At Helmstedt, according to Caselius's report and the extant *ordines lectionum*, Liddel continued to teach the three concurring hypotheses on the planetary system. He stayed in Helmstedt until 1607, when he returned to Aberdeen with his mathematical books, among which were two copies of *De revolutionibus* and a rare handwritten copy of Copernicus's *Commentariolus*.[22] He endowed the local University with a fund for the support of poor scholars in 1612 and the Marischal College with a chair of mathematics in 1613.

Liddel was very diligent in publishing his medical writings, for instance a collection of his numerous medical disputations, *Disputationes medicinales* (Helmstedt 1605), and an *Ars medica* (Hamburg 1607). By contrast, his mathematical writings are very rare. Still, two disputations by him are preserved in Wolfenbüttel: *Propositiones astronomicae de dierum et annorum differentiis et caussis* (Helmstedt 1591), and *De philosophia eiusque instrumentis* (Helmstedt 1592).[23] The latter was defended by Cornelius Martini, professor-to-be at Helmstedt, and a close friend of Liddel. The disputation *De philosophia* is revealing of Liddel's philosophical conception of mathematics, which he regarded as one of the three speculative disciplines together with metaphysics and physics. The corollaries (*coronides*) to the theses are a refutation of Ramism. In particular, the fourth corollary rejects de la Ramée's requirement of an astronomy 'without hypotheses,' a program which, by contrast, had been embraced by Pegel. Liddel was also the author of a lost introduction to mathematics, titled *Parerga mathematica* (Helmstedt, 1595), mentioned by Brahe in a letter to Liddel's pupil Cramer in which the Danish

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17 See N. Jardine, and A. Segonds: "A Challenge to the Reader: Ramus on 'Astrologia' without Hypotheses," in M. Feingold, J. S. Freedman and W. Rother, *The Influence of Petrus Ramus* (Basel 2001), 248-66.

18 O. Gingerich and R. Westman: "The Wittich Connection: Conflict and Priority in Late Sixteenth-Century Cosmology," *Transactions of the American Philosophical Society* 78/7 (1988).

19 Caselius, epistle to J. Craig, *cit.,* f. †4*r*.

20 D. Cramer to Rosenkrantz (Stettin, 31 March 1598), in Brahe, *Opera*, VIII, 37-43.

21 *Nidersächsisches Staatsarchiv Wolfenbüttel* , 37 Alt 379, *Acta M. Duncani Liddelii*, Caselius's letter to the Academic Senate of Helmstedt (1 January 1591).

22 Gingerich, *An Annotated Census of Copernicus' De Revolutionibus (Nuremberg, 1543 and Basel, 1566)* (Leiden 2002), 264-7.

23 A manuscript eulogy of mathematics by Liddel will be the subject of a further study.

astronomer protested, with quite rude expressions, that Liddel did not acknowledge his authorship of geo-heliocentrism.[24] Concerning Liddel's opinion on cosmology, very little can be said: he was probably a crypto-Copernican. To his classes he presented the heliocentric hypothesis from a mathematical point of view, that is, with no open commitment relative to its physical reality, as shown by the fact that he taught the Copernican system along with the concurring models of Ptolemy and Brahe. Nonetheless, from Cramer we know that he was inclined to accept heliocentrism also from a physical point of view and objected the physical tenability of Brahe's model.[25]

Connections: Aberdeen, Frankfurt on Oder, Breslau (Dudith, Crato, Wittich), Rostock (Brucaeus), Denmark (Brahe), Hamburg (printing of the Ars medica), Edinburgh and London (Craig).

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To sum up, the teaching of mathematics played an important role in the first years of the University of Helmstedt. Brilliant scholars, well inserted in the actual scientific debate, were attracted to the newly-founded institution. The originality of their achievements is witnessed by the conceptions of Pegel, the ambitious lectures of Liddel and the writings of Bruno. A significant aspect is the cosmological interest witnessed by Pegel, Bruno and Liddel who contributed, in a way or another, to the post-Copernican debate. Relative to the connections of the Helmstedt mathematicians, they are revealing of an essentially northern-European network; it includes Lutheran universities (Rostock, Wittenberg, Frankfurt on Oder, Jena), important centres of astronomical research (Hven, Kassel, Prague), as well as Flanders/Netherlands, England and Scotland. I would define it as a northern-European Protestant network. The confessional element seems to have played an indirect role in the international contacts of the professors of mathematics, because the political and theological context determined their concrete possibilities of collaboration and scientific exchange. No significant connections with Catholic countries can be detected, apart from the fact that several Flemish scholars escaped from their country precisely for religious reasons and took refuge in Rostock and Helmstedt. Bruno was a kind of comet in the history of the University of Helmstedt, yet he could have influenced the atmosphere of tolerance and openness toward scientific novelties characteristic of the philosophical faculty and, in the seventeenth century, also of the theological.

[24] T. Brahe, *Opera Omnia* (repr. Amsterdam 1972), VIII, Brahe's letter to Cramer (16/26 September 1599), 184-7: 1.

[25] *Ibid.*, Cramer's letter to Rosenkrantz (Stettin, 31 March 1598), 37-43.

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VOYAGES ALONG MERIDIAN LINES IN EUROPE

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Abstract

After the creation of the Paris Observatory meridian line, in 1667, Picard made measurements along it from Amiens to La Ferté-Alais. After his death (1682), Cassini enlarged it in 1683 up to Bourges, later (1700-01), with Cassini II, up to the Pyrenees. The end came only in 1718 from the northern to the southern borders. After measurements in Lapland (1736-37), a new campaign is performed in France by Lacaille around 1740. This may explain why this peculiar meridian was chosen in 1791 to give birth to "une mesure révolutionnaire: le metre", whose definitive length was fixed, after new measurements (1792-98), in 1799. Other voyages along a meridian line occurred in Europe (1800-20) in Lapland, Spain, Great Britain and in Central Europe (1816-55) with the longest meridian arc ever measured. Despite these new measurements, the length of the "metre" has never been changed, leading 50 years ago to adopt its modern realization for the "Système international d'unités", the SI, in 1960.

Scientific travels in Europe, in the present days, are frequent but our predecessors were very numerous as well, according to the conditions of their time; Copernicus, Halley and others could be mentioned and, for France, Picard in Denmark and along the French coasts with La Hire, Delisle in Russia and Siberia, Cassini II in England, Cassini III in Austria and many others.

Among scientific voyages in Europe, the paper concentrates on those whose purposes, being mostly organised at national or European level, were made to get a better knowledge of the shape of planet Earth. They concern, more or less, from West to East, Great Britain, France, the Low Countries, Spain, Central Europe, Sweden and Finland. Their influence over science is consequently considered.

First travel for France along a meridian arc

Influenced by Frisius (1508-1555) and Snellius (1580-1626) with a method named *triangulation*, Picard (1620-1682) launched, in France, the idea to measure a meridian arc, in view of the improvement of the map of France, requested by Colbert, minister of Louis XIV, around 1663. After the creation of the *Académie Royale des Sciences* in 1666, followed a few months later, in 1667, by the foundation of the *Observatoire Royal,* Picard and one of his colleagues, academician Auzout

(1622-1691), built a portable quadrant (1667) equipped with refractors, themselves having a filar micrometer they had built in 1666.

A portable quadrant, a second pendulum clock recently (around 1655) made by Huygens (1629-1695) in the Low Countries and a zenith sector, which later would influence Bradley (1693-1762), third Royal Astronomer of the Royal Observatory in Greenwich, was Picard's equipment to perform the measurement of a meridian arc. It was more or less centred in latitude on the Paris Observatory the symmetry axis of which represents the reference meridian line. For measuring the base (about 11km) of his triangulation (about 111km), Picard used pieces of wood about 4 meters long, all data being here given in the Decimal Metric System, not in use at that time.

Picard worked on this triangulation during several short trips he made between 1669 and 1670. The instruments were carefully carried in horse or donkey carriages but, for his long (3.25m) zenith sector, for latitude determinations from the two extremities of triangulation, they were moved by men in a special carriage. One can easily think of the difficulties encountered in such a geodetic operation. After its end Picard published (1671) his *Mesure de la Terre*, which represents, for France, the creation of geodesy.

It is said that the result of Picard's length for one degree (57 060 toises of 1.95m), made known to Newton (1643-1727) in 1684, provided him a confirmation for his famous law of gravitation. What is sure is that in Newton's *Principia* (1687) many numerical data are given provided by French scientists of the time, mostly astronomers.

First travels along a complete meridian arc for France

After Picard's death, in 1682, Cassini I (1620-1712) took care of the project Picard had proposed (1681) to the French *Académie Royale des Sciences*. The project, related to the shape of the Earth, was bearing the idea Richer (1630-1696) had, having to reduce the length of the second pendulum when in Cayenne (1672), concerning the sphericity of our planet.

Cassini I began the prolongation of the meridian arc, from Picard's Southern point to Bourges, a town situated in the centre of France; but Colbert's death stopped the operation. It could only be pursued by himself and his son Jacques, in 1700 and 1701, leading them to the Pyrenees, the limit between France and Spain. The Northern part could be measured only on the occasion of Louis XIV's Flemish war. The achievement occurred in 1718 and the corresponding publication came in 1720 by Cassini II (1677-1756), his father being dead. The end of the measurements led, as well as those made with his father, to a prolate Earth, contrary to Newton's law.

The controversy between partisans of Newton and the two Cassinis, had as consequences, under the leadership of the French *Académie Royale des Sciences*, two expeditions as close as possible to both the equator and the pole of the Earth.

Meanwhile, in France, Lacaille (1713-1762) and Cassini III (1714-1784) had re-measured the *Méridienne de France* in 1739-1740, having new and better-made instruments. The results were published under the title *La Méridienne de l'Observatoire Royal de Paris, vérifiée dans toute l'étendue du Royaume* […] in 1744, confirming the oblate shape of the Earth. The complete and scientifically established map of France could be achieved, covering the country with an incredible set of triangles, including four along meridian lines to make linkages more secure, in combination with some parallels.

Cassini IV (1748-1845), who had only helped his father before his death in 1784, had to achieve and to present the map to the *Assemblée constituante* in 1790 after the *Révolution Française* (1789), which leads him to abandon his position of the now named *Observatoire de la République* in 1793.

During all the 18th century in France, there is no special mention of great improvement for travelling with scientific instruments despite makers' efforts to improve carriage of various types. But what is insured is that instruments makers had been able to build more accurate instruments: metallurgy had improved to the point of having more flat limbs whose graduation was more regularly obtained; parts of them were made with better quality optical glass; clocks had been improved after the Harrison's (1693-1776) success with his mariner clocks, but were not yet available at affordable prices for current uses. Some improvements occurred in instruments, but mostly for determinations at sea. Improvements for terrestrial measurements would arrive by the end of the century.

Travel along a meridian arc in Lapland

The *Expédition de Laponie*, as it is named in France, was placed under the leadership of Maupertuis (1698-1759), but it included other French members of the *Académie Royale des Sciences* and also, a Swedish, Celsius (1701-1744), to become famous for the degree of his temperature scale.

Maupertuis, who had been in England in 1728, came back convinced about Newton's theory, perhaps also influenced by Voltaire (1694-1778), who had attended his grandiose burial ceremony the previous year. In 1735 an expedition had left France to measure a meridian arc in Equator, at that time named Peru, with Godin (1704-1760), La Condamine (1701-1744) and Bouguer (1698-1758), together with two Spanish officers to become famous: de Ulloa (1716-1795) and Juan y Santacilia (1713-1773).

For determining their position at sea, they had a newly built octant by Hadley (1682-1744). Maupertuis, who went to Lapland by boat, had a similar one, but also what was needed for geodetic and astronomical observations --his portable quadrant is in Berlin from the time he was a member of the Berlin Academy of Sciences, while the one employed by the Spanish is in Madrid. Both are improved versions of Picard's quadrant, for geodetic measurements, and of his sector, for latitude determinations. They also had, of course, clocks, ephemerides and it was decided to perform gravitational measurements with a pendulum. Following Celsius' suggestion, the base was measured (1736/37 winter) on the ice of the river, and the triangulation extended between Kittis in the North and Tornea in the South.

To have a clear impression of measurements, made only between 1736 and June 1737, the best is to read the book published by the Abbé Outhier (1694-1744) or, for young children, E. Badinter's book illustrated in colour by J. Duhême (Seuil Jeunesse, 2003). Among other things, Outhier had mentioned that mosquitoes were so numerous that when eating their bread slices, toasted with grease and despite some veil covering their heads, they were indeed eating mosquito toasts! Outhier also recalls the pleasure they had to sleep between sheets, after having had to sleep with their clothes and boots on, without being able to remove them for two weeks!!

The *toise* they employed is preserved, under the name *Toise du Nord*, in the collections of the Paris Observatory as well as the one brought by the *Expédition du Pérou* named *Toise du Pérou*. According to Maupertuis' calculations, compared with Picard values, the conclusion was favouring Newton.

Travel along a meridian arc for a metre

A new linkage between British and French meridians of reference for longitude was launched in 1784 by geodetic means. Ramsden (1735-1800) was working to built a new instrument, a theodolite of altazimuth type, while Borda (1733-1799) requested the help of the instrument maker Lenoir (1744-1832) for a new system: the repeating circle issued from Mayer's (1723-1762) full circle, known from mid-18th century. Both would be ready for the operation in 1787; it was successful, so that the repeating circle would be chosen for the determination of a new standard for length in view of the uniformisation of weights and measures in France, decided in 1790, more or less as a consequence of the French Revolution.

While latitude and longitude were being determined in Great Britain (1796-1798), the French were travelling once again along the *Méridienne de France* (1792-1798). Travel conditions are difficult due to the circumstances, including the *Terreur* and some war between France and Spain. Manuscripts from Delambre (1749-1822), Méchain (1744-1804) and their collaborators let us know about the difficulties encountered, the discussions, at European level of the time; a definitive length was given to the metre in 1799.

In Great Britain, the 1796-1798 operations were made from Dunnose (Isle of Wight) to Nettlebed, later to Clifton, in the North of the country, to the Tees' mouth, crossing Arbury Hill. While Delambre and Méchain's measurements confirm, like Lacaille and Cassini III, the diminution, from North to South, for lengths of meridian arcs covering one degree in latitude, Mudge (1762-1820) and his collaborators find the opposite. They had the new instrument by Ramsden, achieved by one of his collaborators. Other measurements are made in 1801, again in 1802 and, in 1803, the conclusions given by Mudge to the Royal Society confirm the diminution, contrary to Newton's law. The discrepancy, at Arbury Hill, of the order of five arcseconds in latitude has a great effect on the conclusions for the arc. Mudge recalls the anomaly found by Méchain (3.24") in Spain, close to Barcelona.

Travels along meridian arcs in Sweden, Great-Britain, Spain and Finland

The development of Newton's works in France by Clairault (1713-1765), D'Alembert (1717-1783), followed by Legendre (1752-1833), Lagrange (1736-1813) and Laplace (1749-1827) will bring new travels along meridian arcs, at the beginning of the 19th century. After his first works, Laplace had shown in 1793 that Newton's flatness (1/230) of the Earth was given different numerical values when issued from meridian arc measurement (1/250) compared with gravimetric methods (1/320). Later, after other studies by Legendre and Lagrange, it was seen that the Lapland meridian arc showed anomalies, while data from Peru were in agreement with the measurements in France. In using them, and not the Lapland one, Delambre had found 1/307,8.

New measurements were decided by the Royal Swedish Academy of Sciences, and Svanberg (1771-1851) was in charge for measuring an arc as close as possible of the preceding one. It was indeed a little bit longer; two bases were measured: Maupertuis' one, between Avasaxa in the North and Niemisby in the South; and a second one between Pullingi and Avasaxa. Apparently, Maupertuis' error had come from his astronomical measurements, not from the angles. After Svansberg's measurements, Newton was not discussed any more. But Mudge had still the anomaly.

In 1806, in Spain, Biot (1774-1862) and Arago (1786-1853) left France in view of pursuing the measurements along the French meridian from Dunkerque to Barcelona, to prolongate it up to the Balearic Islands. This new travel along this meridian line will allow the arc to extend, with a symmetrical length, on each side of latitude 45 degrees, for which the influence of the flatness of the Earth is almost negligible. The results are discussed in *Base du Systeme métrique décimal* (Vol. III), by Méchain and Delambre (1810); the last one examined several aspects for the length of the metre. The conclusion is that the difference between a possible «new metre» and the 1799 value is small, and that there is no need to change its value.

On the other hand, this campaign confirms Newton's results, but does not explain those found in Great Britain. After some new calculations, published by the Spanish Rodriguez (1776-1821) in 1813, several experiences were organised along meridian arcs in Great Britain. A repeating circle, used by Kater (1777-1835) in Arbury Hill, provides the same latitude value. Mudge came to Dunkerque with the Ramsden/Berge instrument, confirming Delambre's determination of latitude. It is too much, and something needs to be made, but 1812 is not a favourable year for Franco-British cooperation.

This occurrence will come in 1816, through the *Bureau des longitudes*, an institution created in 1795 more or less influenced by the British Board of Longitudes. Biot left France in May 1817 equipped with a repeating circle made by Fortin (1740-1817), most probably the one which is in the Paris Observatory Collections. He also has a pendulum he employed, with Arago, in the Balearic Islands. The whole campaign, in which Arago and Humboldt (1769-1859), the German scientist then in Paris, participated from November 1817, ended with their return to Paris in January 1818. The anomaly in the centre of Great Britain is confirmed. Thus, several people recalled the deviations of the vertical mentioned by Bouguer, one of the members of the Peru expedition, due to the mountains in Equator.

Travel along the longest European meridian arc measured by classical geodesy

This operation, in which were engaged people from Russia, Norway, Sweden,… along a meridian arc between the Baltic and the Black seas, is more or less a consequence of the discrepancies found between Maupertuis and Svanberg measurements. At the very beginning, Lindenau (1779-1854), director of the Seeberg Observatory, and W. Struve (1793-1864) were thinking about a meridian in the middle of Europe in longitude, which indeed was firstly an idea of J.-N. Delisle (1688-1768) after he had founded an observatory in Saint Petersburg, on the roof of the seat of the Russian Academy.

In 1737, thirteen years after his arrival in Russia, J.-N. Delisle had submitted the project of a meridian arc of 23°, aligned with the meridian of the newly installed observatory. To begin, Delisle measured a base, similarly to Maupertuis on the iced river in winter, between Doubky and Peterhoff. Delisle will not pursue the work, most probably because the expeditions to Lapland and to Peru had solved the problem raised concerning the shape of the Earth, favouring Newton.

The new meridian will have two more degrees than Delisle's project, 25 instead of 23. Similarly to Delambre and Méchain, measurements were made from a sea level to a sea level. The main actors of the subsequent voyages were a Russian general C. de Tenner (1783-1860), a Norvegian Ch. Hansteen (1784-1873), a Swedish N.H. Selander (1804-1870) and W. Struve, the founder of Pulkovo Observatory in 1839, Delisle's observatory having not been so much in use after his departure from Russia.

Seven arcs were measured, including 257 triangles, 10 bases, 13 latitude determinations along the meridian line of Dorpat (nowadays Tartu). This arc was, indeed, more or less in the middle of Europe in longitude as well as in latitude, a favourable position for a general geodetic map. With such length, the measurements along this meridian line, beginning in 1816 –after the end of the Napoleonic wars– ended in 1855. The publication was in two volumes (28 x 36 x 9 cm), plus an Atlas of 26 sheets for the drawings about the triangulation. The total length, measured by all these men and their collaborators, covers about 3000 km. They had all sorts of weather conditions, during the four decades they had to perform these various measurements.

With this European operation, it became clear that a general map of Europe was not far to be made. But a linkage between triangulations in the various countries was needed: Russia, Scandinavian countries, Poland, Prussia, Austria, France, Great Britain, … And even, W. Struve asked G. Everest (1790-1866) to check some linkage between Russia and India, in which geodetic measurements had been made from 1823 to 1843.

Toward the unification of weights and measures

During the second part of the 19th century, discrepancies began to appear, mostly after the measurements of the «longest meridian arc», in linkages between several parts of the triangulations.

As an example, clearly seen when linking Prussia to France, triangles could not be properly closed at the limit. After a very deep control, it was seen that this occurred because the metres employed for the Prussian operations were not exactly of the same length as the French reference, the *mètre des archives* defined as 3 *pieds* 11.296 *lignes* (1 ligne = 2.27

mm) of the *Toise du Pérou*. This *toise*, preserved in the collections of the Paris Observatory, was indeed the *Toise de l'Académie*.

The definitive *mètre* had been made by Lenoir, firstly in providing twelve pieces of iron and two in platinum prepared at the requested length, and measuring them accurately. They were, of course, not all of the same length. The reference taken for the *mètre*, the new French reference, was one of the irons ones, selected for being as close as possible of the mean obtained from the fourteen different lengths. On the other hand, despite the comparisons Angström (1814-1874) had made in Paris with his metre, more precise observations showed that its length was not exactly power minus ten of the French *mètre*.

Following all these remarks, it was decided –at the level of knowledge of the time– that it would be better to have an international reference. These considerations led to the creation in 1875/76 of the *Bureau international des poids et mesures* (BIPM), the International Bureau for Weigths and Measures. The reference for length would be the *Mètre des Archives*; for weight the reference would be the *kilogramme* under the form of a small cylinder which was, in 1799, deduced from the weight of one cube of water which sides were equal to one *décimètre*, a tenth of a *mètre*.

Among other consequences of all the measurements made in Europe through rather long voyages along meridian lines, was also the creation of the *Mittel-Europäische Gradmessung* (1861/62). As the name shows, it was a geodetic association related to Central Europe, but a few years later it became the *Europäische Gradmessung* (1864), the *Association géodésique internationale* (1866) and, after the inclusion of several other countries, the *Association internationale de géodésie* (1930). Very soon many other domains of cooperation would appear at the international level: solar watch, longitude determinations mostly after the discovery of wireless and its uses in astronomy and in geodesy.

All these developments, simultaneous to other scientific changes in different fields, both in science and in techniques during the 19th century, had several important consequences. To stay in the geodetic domain, the *Convention du Mètre* among seventeen countries was signed in 1875, leading to the adoption of the decimal metric system. To be noted, the most important character of this system of units is its decimal character, which became fifty years ago, in 1960, *Système international d'unités*, the SI, for International System of Units.

A few more comments at the end

At the time of Delambre and Méchain, and after addition of the meridian measurements in Great Britain, the length of the 1799 metre was recalculated; the differences were so small that it was decided not to change its length. Later, Biot and Arago examinated some anomalies in observations and found mistakes in their calculations. Again, it was decided not to modify its length. As noticed already by Lalande (1732-1807), the length of the meter, whatever its length was, is indeed a convention. Half a *toise* or the yard would have been as useful as all the measurements made, but such a decision was perhaps difficult in view of a new system not only for France but for the world.

As early as in 1799, Delambre had said that there was no need to modify the value of the length of the meter, many measurements having been made. It was also added that, in the future –after Delambre, Méchain, Arago, Biot, …-- nobody would do the same to verify this length.

This prediction has not been completely true, and the longest meridian arc was indeed measured by mid-19th century. But, as far as we know, it was not used to recalculate a length for the *mètre*. The only case known is most probably the subject of one of Jules Verne's books, whose title is *Aventures de trois Russes et de trois Anglais*. A meridian arc is measured in Africa, at a longitude appropriate to Russia in view of checking the length of the *mètre*. Let us say, when examining the part concerning the measurements, that almost word to word, the description comes from the volume about the Earth, one of the four volumes of Arago's book *Astronomie populaire*.

In Barcelona, to have an idea of the meridian arc, measured between Dunkerque and Barcelona, walk by the *Avinguda Meridiana*, up to the *Plaça de les Glòries Catalanes*. In a circular garden, there is a big piece of metal along one of its diameter. About its shape, it is said to represent, with some visibility, the central part of France, which caused some trouble to Delambre, similar to what Mudge had in Great Britain and Méchain in Catalonia, the scale, in altitude, was enlarged by a factor ten.

References

BIOT, Jean-Baptiste et ARAGO, François, *Recueil d'observations géodésiques, astronomiques et physiques […] prolongement du méridien de Paris*, Vve Courcier, Paris, 1821.

CASSINI Jacques, *De la grandeur et de la figure de la Terre,* Imprimerie Royale, Paris, 1720.

CASSINI (de Thury), César-François, La *Méridienne de l'Observatoire Royal de Paris, vérifiée dans toute l'étendue du Royaume*, Guérin, Paris, 1744.

CASSINI, Jean-Dominique, "De l'entreprise et de l'exécution de la carte générale de la France", In: *Mémoires pour servir à l'histoire des sciences*, Bleuet, Paris, 1810.

DELAMBRE, Jean-Baptiste et MÉCHAIN, Pierre-François-André, *Base du Système métrique décimal ou mesure de l'arc du méridien compris entre les parallèles de Dunkerque et Barcelone*, 3 vol., Baudoin, Paris, 1806, 1807, 1810.

LEVALLOIS Jean-Jacques, *Mesurer la Terre, 300 ans de géodésie française - De la toise du Châtelet au satellite*, Presses des Ponts et Chaussées/Ass. Franc. de Topographie, Paris, 1988.

OUTHIER Réginald, Journal d'un voyage au nord, en 1736 et 1737, Piget, Paris, 1744.

PICARD Jean, *Mesure de la Terre*, Imprimerie Royale, Paris, 1671.

TENNER, Carl, HANSTEEN, Christopher, SELANDER, Nils Haquin, et STRUVE, Friedrich Georg Wilhelm, *Arc du Méridien de 25°20' entre le Danube et la Mer Glaciale mesuré depuis 1816 jusqu'en 1855*, Acad. sci. de Saint-Petersburg, Saint-Petersbug, 1860.

Jean Picard et la Mesure de la Terre, Catalogue, Exposition Jean Picard, Obs. de Paris, Paris, 1982.

Jean Picard et les débuts de l'astronomie de précision au XVII^e siècle. Actes du coll. du tricentenaire. Ed. G. Picolet, Editions du CNRS, Paris, 1987.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 308.

CONTRIBUTION OF JAPANESE PRINTED DRAWINGS OF AQUATIC ANIMALS BROUGHT TO EUROPE BY HOLLAND MERCHANTS IN THE 18TH AND 19TH CENTURIES FOR BIOLOGICAL DEVELOPMENT

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Abstract

During Japan's isolation from the west, Linnaeus established the scientific classification using binominal nomenclature, which was promptly accepted as a universal system by Western scientists. Therefore, concerning fish, except for some specimens and materials brought back to Europe by voyage naturalists, there were only a few Japanese ichthyological resources available in Europe. Those materials included drawings called "Poisson du Japan" associated with Isaac Titsingh, and a book titled Umi no sachi *(Boon of the seas), which were brought to Holland by the Dutch East India Company, the sole western trader with Japan. They are now stored in the Muséum National d'Histoire Naturelle, Paris.*

Umi no sachi *is a haiku anthology book of wood engravings in colour, published in 1762, Edo Period, Japan. It mainly contains illustrations of fish and marine invertebrates, in and around Japan, whose characteristics are described to some extent with accuracy. Admitting to the limitation of the illustrations, which often lack accuracy in the number of the fin rays or spines, European biologists in the early 19th century did however utilize the book as a reliable source material for their scientific works. Cuvier and Valenciennes often cited it as "the Japanese printed copy" in their work,* Histoire Naturelle des Poissons *(1828-1849). Moreover, in the plates for* Histoire naturelle générale et particulière des Céphalopodes Acétabulifères *(1834-1848), Férussac and d'Orbigny exactly copied and reproduced the drawings of squids and octopuses from the book.*

Based on my research on Umi no sachi in the Bibliothèque Centrale du Muséum National d'Histoire Naturelle, Paris, I would like to evaluate its academic value in the progress of science. The role of the book is also reviewed as media, which contributed to the biological classification in the 19th century by providing ideas and description of aquatic animals living in the unknown country.

During Japan's isolation from the west, Linnaeus established the scientific classification using binominal nomenclature, which was promptly accepted as a universal system by Western scientists. Therefore, concerning fish, except for some specimens and materials brought back to Europe by voyager naturalists, there were only a few Japanese ichthyological resources available in Europe. Figure 1 shows the international relations between Japan and Europe in the 18th and 19th centuries. During the Edo period, when Japan had an isolationist policy, Holland was the sole western trader with Japan. Materials concerning Japanese fish were brought to Holland by merchants or voyager naturalists who worked for the Dutch East India Company.

In this paper, I would like to evaluate the roles of those materials and their academic values, especially fish drawings brought to European countries by Holland merchants in the 18th and 19th centuries. Contemporary Japanese also showed keen interests in natural history, and thus, many natural history drawings including fish illustrations were made. I want to focus on their contribution to ichthyological work by French biologists, Cuvier and Valenciennes. Based on my investigations, I would also like to discuss the roles and significance of the drawings, not only from their contemporary perspective, but also for their academic value in today's scientific areas.

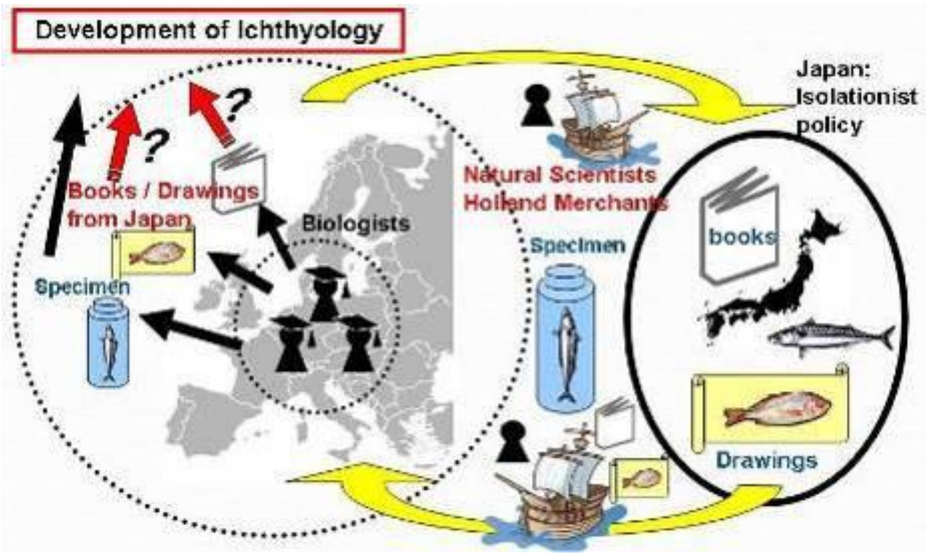

Fig. 1: Overview of this paper: the international relations and transfer of the academic source materials between Japan and Europe in the 18th and 19th centuries.

To understand the early period of ichthyology concerning Japanese fish, it is essential to analyze when fish in and around Japan were described and given their scientific names based on the Linnaean system of biological nomenclature. Historical background of this discipline gives us a good starting point, such as the western scientists who described the species, and the number of the new Japanese fish species they reported and described in their academic papers. As the ichthyological studies developed, the classification of species has changed accordingly. For example, species that once were regarded as different, now might belong to the same species. On the other hand, one species may have included different species. Both of the names of the scientists who described the species and the number of species which are still valid today are kindly given by the Japanese ichthyologist, Tetsuo Yoshino (University of the Ryukyus). Thunberg (1743-1828), who visited Japan from 1775 to 1776, was the Swedish physician who worked for the Dutch East India Company. Thunberg published 10 Japanese fish species, of which 6 species are still valid today. He sent his fish collection to J.C.M. Radermacher (1741-1783) in Batavia, who subsequently forwarded them to Houttuyn (1720-1798). Dealing with the Thunberg's Collection, Houttuyn was the first scientist who classified Japanese fishes and published reports on the 36 species of Japanese fishes in his work *Beschryving van eenige Japanese visschen, en andere zee-schepzelen* (Description of some Japanese fishes and other sea-creatures) in 1782 (Boeseman & Ligny, 2004: 71), in which 14 species are valid today. Temminck (1778-1858) and Schlegel (1804-1884) published *Fauna Japonica, Pisces* from 1842 to 1850, based on the enormous number of fish specimens collected by Phillipp Franz von Siebold (1796-1866). Siebold was the German physician who worked for the Dutch East India Company, who visited Japan from 1823 to 1829. Since Siebold collected both Japanese Fauna and Flora extensively, the number of the new fish species nominated by Temminck and Schlegel was, therefore, significant. However, ahead of Temminck and Schlegel, Cuvier (1769-1832) and Valenciennes (1794-1865) did publish as many as 32 Japanese fishes. This raises important issues of the French biologists Cuvier and Valenciennes. For example, how and why could they nominate Japanese fish species, and what were their source materials for the scientific descriptions.

Cuvier and Valenciennes were eminent biologists, who made great contribution to the development of ichthyology. From 1828 to 1850, they published their monumental work, *Histoire naturelle des poissons*, which consists of 22 volumes, 11,253 pages and 650 colour plates. *Histoire naturelle des poissons* contains 4,055 species, and among them, 2,311 were considered new species at that time (Bauchot et al., 1997: 27). Their work is still important and essential for ichthyologists today.

In the *Histoire Naturelle des Poissons*, Cuvier and Valenciennes clearly mentioned that two kinds of source information were available for them. One was fish specimens from Japan brought back by Langsdorff (1774-1852), who participated in the Russian Voyage under the command of Captain Krusenstern (1770-1846). To get a reply from Bakufu (Japan's feudal government), Russian missionaries had to stay in Nagasaki for half a year from 1804 to 1805. Although the Russian expedition failed to establish trade between Japan, those fish specimens, mainly from Nagasaki where the trading port with Dutch East India Company existed, were brought to Europe. Langsdorff donated 89 Japanese fish specimens to the Zoological Museum of Berlin (Paepke, 2001: 332), and they have been stored in the museum under the "Langsdorff Collection." The other source was Japanese books and drawings which had been brought to Paris from Japan.

The purpose of the present paper is to investigate the roles that these materials played for Cuvier and Valenciennes. For this reason, I would like to move on to details of these materials which are not biological.

The source materials which were used by Cuvier and Valenciennes consisted of three kinds of publications or drawings: first, *l'Encyclopédie japonaise* (in Japanese, *Wakan sansai zue*); second, "Poissons du Japon"; and third, Umi no sachi (Boon of the seas). The first two materials were brought back by Isaac Titsingh (1745-1812), who visited Japan three times from 1779 to 1784. As the chief officer of the Dutch East India Company, he stayed in Japan for about three years in total. Since he himself studied to be a physician and he was also interested in Japanese culture, he had scientific and cultural exchanges with many intellectuals throughout Japan. Those intellectuals included Rangakusha, some of whom were physicians and medical doctors who also studied foreign sciences. The Rangakusha had keen interests in natural history, and they collected many natural history materials, both specimens as well as well-drawn illustrations. Their collections also included natural history books, not only the domestics, but also foreign ones through Holland. Although the imported natural history books were very expensive, those intellectuals were eager to obtain them. Notables include: physicians Katsuragawa Hoshu (1751-1809) and Nakagawa Jun'an (1739-1786); Nagasaki interpreter, Yoshio Kogyu (1742-1800); and Feudal lords, Daimyos, Kutsuki Masatsuna (1750-1802) of Fukuchiyama, and Shimazu Shigehide (1745-1833) of Satsuma. Even after Titsingh left Japan, he kept in touch with those intellectuals by exchanging letters. Extensive research work by Frank Lequin has shown that the correspondences between Titsingh and his Japanese friends were extensive, including gifts, books and information. Thus, Titsingh became known as the Japanologist who was successful in possessing the first extensive Japanese Collection in Europe. In Paris, he deepened friendship with orientalists, Abel-Rémusat (1788-1832), who published Titsingh's works after he died.

Among the Titsingh's Collection in the French National Library (fig.2) is a set of illustrated Japanese encyclopedia called *Wakan sansai zue*, namely, *l'Encyclopédie japonaise*, consisting of 81 volumes, donated by Titsingh in 1803, which became the first Japanese printed books in the library (Lequin, 2003: 50). Cuvier and Valenciennes quoted the *l'Encyclopédie japonaise* in *Histoire Naturelle des Poissons*, with the aid of Abel-Rémusat, whose name is mentioned several times in the text to express their appreciation. The quality of the illustrations in the encyclopedia, however, is poor, so Cuvier and Valenciennes used it to grasp the characteristics and names of the fish.

The last remaining of the two materials is stored in the Bibliothèque Centrale du Muséum National d'Histoire Naturelle, Paris today (fig.2).

The second material referred to by Cuvier and Valenciennes was "Poissons du Japon," which is also associated with Isaac Titsingh. "Poissons du Japon" consists of 23 colour folios, which contains 47 drawings. The number of fish species amounts to 31, and the other species include Crustacea and a few turtles. Cuvier and Valenciennes highly valued the drawings in the "Poissons du Japon," and wrote as follows:

> The Duke of Rivoli possesses a superb manuscript, brought back from Japan by the late Titsingh, in which the Japanese names are added in European transliteration; … (Cuvier, 1995: 88)

As Cuvier and Valenciennes added to the text, the Duke of Rivoli was kind enough to show them the volume. It seems that they had not been able to use the "Poissons du Japon" as a referencial source, partly because it was in the private possession of the Duke of Rivoli. "Poissons du Japon" was referred to by Cuvier and Valenciennes only once in the *Histoire Naturelle des Poissons*, however, the quality of drawings impressed Cuvier and Valenciennes greatly enough to make them annotate it in the text. Through my investigation, it is getting clearer that the drawings of the "Poissons du Japon" were carefully copied and reproduced by hand, because there are almost the same drawings found in some libraries in Japan. The original drawings have not yet been found, but the handwritten notes attached to the existing drawings enable me to consider that the drawings were passed down among the famous physicians in Edo who had keen interests in natural history. Maybe one of them met Titsingh and offered to give him a copy.

Fig. 2: Photos of the Bibliothèque Nationale de France (left) and the Bibliothèque Centrale du Muséum National d'Histoire Naturelle, Paris (the right-most building).

The main source materials to describe Japanese fish for Cuvier and Valenciennes was the printed book titled *Umi no sachi*, which is a haiku anthology book of wood engravings in colour, published in 1762, Edo Period, Japan. It mainly contains illustrations of fish and marine invertebrates, in and around Japan, whose characteristics are described to some extent with accuracy. It was Katsuma Ryosui (1697-1773), a famous painter in Edo period, who drew the illustrations. Admitting to the limitation of the illustrations, which often lack accuracy in the number of the fin rays or spines, Cuvier and Valenciennes did however utilize the book as a reliable source material for their scientific works. The book is stored in the Bibliothèque Centrale du Muséum National d'Histoire Naturelle, Paris today. On the book, Cuvier wrote French names in the margins of the fish images by pencil.

Although the provenance of the book is uncertain, previous research has shown that it was stored as one of the items in the Stadtholder's Collection in The Hague. The Stadtholder's Collection was one of the most famous, so called "cabinet of curiosity," and many items such as paintings, prints, manuscripts, books, coins from all over the world were collected through the Dutch East India Company. Not only artefacts or human products, but also natural history collections, such as animal specimens were collected. After the French Revolution, the French Republican Army occupied Holland in 1795, and most parts of the Stadtholder's Collection were brought from The Hague to Paris by the French Army (Bauchot et al., 1996: 219). Together with other items in the collection, *Umi no sachi* was also taken and brought to Paris at that time, and has been stored in the Bibliothèque Centrale du Muséum National d'Histoire Naturelle, Paris since.

In the *Histoire Naturelle des Poissons*, Cuvier and Valenciennes referred to *Umi no sachi* as "l'imprimé japonais," "le recueil japonais," "l'ouvrage japonais," at least 39 times when they described the species. The number of the species they referred to in *Umi no sachi* amounts to 22 in total.

Here, the stonefish is shown as a case study. Cuvier and Valenciennes gave a new scientific name, *Pelor japonicum*, to this new species. It is possible to see Cuvier's handwriting by the stonefish drawing in *Umi no sachi*. He also mentioned its Japanese name, "Ogosse" in the text of the *Histoire Naturelle des Poissons*. Cuvier and Valenciennes described morphological characteristics of this species based on the examination of Japanese fish specimen brought back by Langsdorff in Berlin. Although the genus has been changed, the name of this species, *Inimicus japonicus* (Cuvier, 1829) is still valid today.

Interestingly enough, there is a case that Cuvier and Valenciennes described a new species based solely on the drawing in *Umi no sachi*, due to a lack of availability of the specimen of this species. Cuvier concluded that this drawing represents a special, different type of its neighboring species. Therefore, based on the characteristics of the drawing in *Umi no sachi*, Cuvier gave a new scientific name, *Cybium niphonium*, to this species. It is reasonable to consider that Cuvier named "niphonium" after this drawing from the Japanese book, because Japan can be called "niphon". The current scientific name of this species is *Scomberomorus niphonius* (Cuvier, 1832), therefore, although the genus has been changed, the specific name given by Cuvier is still valid. For this reason, this drawing in *Umi no sachi*, was actually used by Cuvier to describe the species when a specimen was not available.

It was not only Cuvier and Valenciennes who used the drawings in *Umi no sachi*. Here are the cases of Cephalopods, that is, the group of squids and octopuses. Monograph of Cephalopods, *Histoire naturelle générale et particulière des Céphalopodes Acétabuliferes* by Férussac and d'Orbigny, from 1834 to 1848, exactly copied drawings from

Umi no sachi. Two illustrations of squids (*Sepia*, Plate 9 and *Loligo*, Plate 18), as well as those of octopuses (Octopus, Plate 9), were copied from *Umi no sachi*.

Férussac and d'Orbigny monographs were usually drawn based on specimens as precise as possible. Seeing these figures, specimens from Far East countries were not available to them. This leads to me to conclude that Férussac and d'Orbigny did use *Umi no sachi* and exactly copied and reproduced the drawings. This proves that the Japanese drawings held enough academic value to be used to complete the monographs.

In conclusion, in the 18th and 19th centuries, even though Japan was isolated from other parts of the world both in politics and in science, merchants and natural scientists who worked for the Dutch East India Company brought back various materials concerning the natural history of Japan. Through Holland, those materials were eventually utilized by the European biologists. Among those materials, the illustrated drawing book called *Umi no sachi*, which contains fish and marine invertebrate drawings, played a significant role as a media, which provided important information and descriptions of the species living in and around the unknown country at that time. Cuvier and Valenciennes used *Umi no sachi* as source information in their ichthyological work, and some of their scientific names are still valid today. Férussac and d'Orbigny also used drawings from *Umi no sachi* for providing images of some squids and octopuses. European natural scientists in the early 19th century thus utilized the book brought back by the merchants as a reliable source material for their scientific works.

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References

BAUCHOT, M.L. and J. DAGET. (1996). Un Bicentenaire: Le Transfert a Paris du Cabinet du Stathouder. Son Importance pour l'Ichtyologie. *Cybium* 20(3): 219-250.

BAUCHOT M.-L., J. DAGET, & R. BAUCHOT. (1997). Ichthyology in France at the Beginning of the 19th Century: The Histoire Naturelle des Poissons of Cuvier (1769–1832) and Valenciennes (1794–1865). In PIETSCH T. W. & W. D. ANDERSON, eds. *Collection Building in Ichthyology and Herpetology*, American Society of Ichthyologists and Herpetologists, Special Pulication Number 3.

BOESEMAN, M. & W. DE LIGNY. (2004). Martinus Houttuyn (1720-1798) and his contributions to the natural sciences, with emphasis on zoology. *Zoologische Verhandelingen* (Leiden) No. 349: 1-222.

CUVIER, G. and A. VALENCIENNES. (1828–1850). *Histoire naturelle des poissons*. F. G. Levrault, Paris and Strasbourg, 22 vols.

CUVIER, G. (1995). *Historical Portrait of the Progress of Ichthyology: From Its Origins to Our Own Time*. Edited by T. W. PIETSCH, translated by A. J. SIMPSON. Johns Hopkins University Press.

FÉRUSSAC, A. E. and D'ORBIGNY A. (1834-1848) *Histoire naturelle générale et particulière des Céphalopodes Acétabulifères vivants et fossiles*. J.-B. Baillière Libraire, Paris. Tome Premier.-Texte: 1vi + 361p. Tome Second.-Atlas: 144 pls.

LEQUIN, F. (2003). *A la recherche du Cabinet Titsingh / its history, contents, and dispersal*

/ catalogue raisonné of the collection of the Founder of European Japanology. CANALETTO / REPRO-HOLLAND.

PAEPKE, H-J. (2001). Comments on the old Japanese fish collections in the Museum of Natural History of the Humboldt University of Berlin. *Ichthyological Research* 48(3): 329–334.

SEKIJUKAN, Shukoku. eds., illustrated by KATSUMA, Ryosui. (1762). *Umi no sachi*.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 313.

JAROSLAV PETRBOK AND FRANTIŠEK LEXA –ORIENT AND THE BALKAN PENINSULA TRAVELS[1]

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Abstract

The Balkan Peninsula was sometimes considered a gateway to the Orient. This impression lingered well into 20th century. In the same time, Balkans became a favourite holiday destination for Central Europeans. What happened if travellers-cum-scholars visited both places, the Balkans and the Orient?
Jaroslav Petrbok was a primary school teacher in everyday life. However, as a researcher he worked for the National Museum in Prague as geologist and paleoanthropologist. In the 1920s and 1930s he undertook scientific travels to the Balkan Peninsula and to the Orient. He organised research trips to the Orient and another trip to the seaside of Bulgaria and France (with detours to Istanbul or Tunisia) for teachers and their families. He gave talks about these countries and about his research, which were held for scholars as well as for general public.
František Lexa, studied Egyptology as an autodidact and mathematics at the University, first he was active as a grammar school teacher, later as an Egyptologist, and in 1925 he established the seminar of Egyptology at the Charles University in Prague. On his travels, including a trip to Egypt, he was devoted to research and spent considerable time obtaining study materials for his students. Lexa used his journey to Egypt in 1931 for improving his lectures for students of his Seminar. He took many photographs of monuments, their details, hieroglyphic script and objects from the Cairo museum. His travels however included also holiday journeys to Bulgaria with his family.
Both of them observed the "real" Orient, and the Balkans.

Jaroslav Petrbok

Jaroslav Petrbok (1881–1960) was born in Prague, obtained secondary school education for teachers and was employed as a teacher at primary schools in the area of Prague. He cooperated with the National Museum in Prague in

[1] This research was granted by GA ČR, č. 409/09/0295 project Českoslovenští vědci v Orientu 1918–1938 (Czechoslovak Scholars in the Orient).

capacities of geologist and paleoanthropologist. He became well known researcher in malacozoology, specialising in the Tertiary and Quaternary mollusks.[2]

He made scientific research and organised travels abroad during his specially funded paid holidays from September 1922 until August 1928. From September 1928 he worked again as a teacher, because the State School Council was no longer able to provide funds for him. Consequently he obtained only a few study days, and travelled during the summer holidays.[3]

Jaroslav Petrbok loved travelling and was on the road whenever he could. He wanted to know foreign countries, and study their cultural and natural wealth. He undertook his journeys with nearly no money, but he saw himself as different from just another of many globetrotters of his age, wandering penniless around the world for fun. He was, and perceived himself as being, a researcher working in Palestine, Egypt or Iraq, and in addition he organised travels for teachers to improve their knowledge about oriental countries, in other words he was a pioneer in specialised study trips.

The documents concerning the journeys are very rare. Papers of Jaroslav Petrbok are preserved in the National Museum archives. We can use his correspondence, scrap-archives (his own articles), and his book *Jewish ethics and work in Palestine*[4] His passports are preserved in the records of the Police Headquarters Prague, Section II, general records[5].

Travels to the Balkan Peninsula

Jaroslav Petrbok travelled very often to Bulgaria because of his geological and paleontological research. During 1914-1949 he undertook altogether 15 trips. During these mainly paleontological researches he found many Palaeolithic sites and further encouraged archaeological research in Bulgaria. No doubt friendly relations of the Balkan states with new Czechoslovak republic contributed to this activity.

He also organised summer school camps for secondary school teachers and their families in Yugoslavia and Bulgaria. From 1924 he tried to establish an organisation of teachers – scouts. In one of his articles from Czechoslovak *Teacher's magazine* he wrote:

> "Holidays at sea ceased to be a rich man's privilege, as camping and scouts organisations opened up possibilities for everyone including the least well off. Youth travellers can visit Asia Minor, Istanbul or Italy without difficulty, whereas not long ago these destinations were for privileged few, such as a professional traveller Kořenský. ... During my 1924 trip I observed carefully camping options and cheap eating out options (solid lunch for 4 to 5 piasters). In my experience it is best to compose the travelling group of people of approximately same social standing and education. Thus I would recommend teachers to organise their own scout inspired organisation, independent and not connected to existing groups, which are often in a state of litigation."[6]

Petrbok planned two types of trips. First series included camps near the sea in Bulgaria with a trip to Istanbul, or camps in France with a trip to Tunisia. Both of these summer travel series were prepared for teachers and their families. Trips to Istanbul and Tunisia were planned for several days to enable as much sightseeing as possible. We know from existing archived records that travels to Bulgaria took place for sure in 1925 and 1926. Second series of trips was planned only for adults and young adults as study trips through Middle East.

Travels to the Orient

In 1924 Jaroslav Petrbok undertook his first research journey to Palestine. During this time and during the next two expeditions he worked on Quaternary molluscs. In January 1926 Petrbok got a subvention of 5,000 CZ from the Ministry of Education to undertake a three month study journey to Palestine. Thanks to this financial support he added also research work in Syria and Egypt,[7] and finally, in 1936 he was also working in Iraq.

Petrbok used his knowledge of the countries he visited to organise trips for other teachers. A first travel was held in 1926[8], at an approximate cost of 3,000 CZ (compare it with the salary of Petrbok – 1820 Kč)[9] and took 6 weeks. They

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[2] The National Museum Archives, (ANM), papers of Jaroslav Petrbok, not registered.

[3] Viz ANM, part 23/8, personal papers of Jaroslav Petrbok.

[4] Jaroslav Petrbok, *Židovská práce a mravnost v Palestině (Jewish ethics and work in Palestine)*, Prague 1927.

[5] National Archive Prague, (NA), document collection Police Headquarters Prague, Section II, general records, box n. 8577, sign. P1541/15.

[6] Jaroslav Petrbok, *Za stavovským skautingem učitelským (Scouting for Teachers)*, in: Učitelské noviny (Teacher's Magazine), 34, 1924, pp. 73–74.

[7] ANM, papers of Jaroslav Petrbok, not registered. Ministry of Education, January 25, 1926, n. 762/26-IV.

[8] Viz *Krásný, báječný, nešťastný Egypt (Lovely, marvellous, unfortunate Egypt)*, Prague 2009, pp. 354–360.

[9] Viz NA, paper collection of Police Headquarters Prague, Section II, general records, box n. 8577, sign. P1541/15.

travelled by train to Constanta in Romania, where they boarded a steamship to Piraeus. They spent three weeks in Athens and its surroundings, and on Peloponnesus. The travel was supported also by the Greek Ministry of Culture; they had free accommodation, a guide and discounts for train tickets.[10] Then they continued by a steam-ship to Egypt to visit Alexandria and Cairo, and later to Palestine and Syria. We can consider this journey as a really educational journey including Greece, Syria, Palestine and Egypt. This journey was aimed at providing the teachers with an opportunity to meet ancient and modern monuments of these great civilisations and to transmit this first hand knowledge to their students.

There were also further travels in 1928 and 1929; these were opened for everybody, were more expensive (4,000 CZK), took less time (3 weeks) and included only Egypt and Palestine.[11]

These travels were a sort of predecessor to travel clubs that organised travels to the Orient from the early 1930s. Also club travels were sometimes organised to acquaint middle class public, including teachers, with the ancient civilisations but also the reality of life in Orient in the first half of the 20th century, including the Zionist movement in Palestine whose work was admired by Jaroslav Petrbok. He was also preparing talks illustrated with slides about visited regions and about his work. The talks were given to general public in Prague and in other places in Czechoslovakia. Petrbok actively promoted and spread the idea that Oriental travel is a part of education now accessible to most. However, when travelling, he also used some approaches close to package tours (discounted travel), albeit in more rudimentary conditions of camps, and with governmental support, as his trips were meant to emphasize the strong educational element.

His research activities were manifold and Petrbok's paleontological, prehistoric or ethnographic collections were deposited in the National Museum.

František Lexa

František Lexa (1876-1960), studied Mathematics and Physics at Charles University in Prague. He became interested in hieroglyphic script and he started to study Egyptology in private. For a long time he continued earning his living as a teacher at a grammar school. In 1907 and 1908 he studied in Berlin with the Egyptologist Adolf Erman and Coptologist Carl Schmidt; he spent the next year in Strasbourg studying demotic script with Wilhelm Spiegelberg to improve his linguistic skills.[12] In 1925 he founded a seminar at Faculty of Arts in Prague and began to teach Egyptology. In 1927 he became professor of Egyptology and in 1958 he co-founded the Czechoslovak Institute of Egyptology in Prague together with his pupil professor Zbyněk Žába. Lexa functioned in many institutions; he was a member of the Academy of Sciences, Oriental Institute, as well as Ski Club or Club of Czech Tourists. Clearly, Lexa's interest in travel went in many directions.

The seminar of Egyptology had in its beginnings only a very small library inherited from the Institute of History of Art and complemented with Lexa's own library. Lack of material for students and necessity to know ancient Egyptian monuments better were main reasons for Lexa's trip to Egypt in 1930/1931. He also tried to get a job there, because after the death of professor Spiegelberg there was a possibility to work for the Egyptian Museum in Cairo. Lexa had to ask many official institutions to grant his journey planned for four months. He obtained funding from the Oriental Institute (15,000 CZK), Ministry of Education (20,000 CZK) and President T. G. Masaryk (10,000 CZK)

Lexa was working very hard in Egypt. He had only a few months to travel through the entire country and he duly visited archaeological sites between Alexandria and Aswan. His research started in Cairo, in the Egyptian museum, where he spent plenty of time. For Lexa the museum was a rich mine of knowledge, as well as a source for wonder. In his letter from January, 1st he wrote to his wife:

> "Surely you remember the statues of Rahotep and Nofret. They have dark grey eyes, so real, so lively, that when I saw them for the first time my heart missed a beat. They make an impression of a living soul being concealed behind them. And the wonder went on. Djoser's wife carrying chair ..., jewellery of Senusrets and Amenemhets, ... golden falcon ... all these are so fine and exquisite that any modern gold work cannot but look crude next to them. My head reeled and after my first visit to the Museum I decided to spend at least a fortnight in Cairo only to study everything in detail. Archaeological objects must be seen; photographs fail to convey the right impression."[13]

Lexa made numerous glass slides for his students. He photographed well known statues and other antiquities as well as details with hieroglyphic text or ornament. He worked in libraries, photographing interesting texts and pictures in books

[10] ANM, papers of Jaroslav Petrbok, not registered. Embassy of the Czechoslovak Republic in Athens.

[11] ANM, papers of Jaroslav Petrbok, not registered. Preliminary programme of holiday journey to Palestine and Egypt in 1928 and 1929.

[12] The Archive of the Academy of Science, Prague (AAVČR), papers of František Lexa, box n. 1, f. n. 11and 12, documents from universtity studies.

[13] AAVČR, papers of František Lexa, box n. 2, f. n. 27, Letter from František Lexa to his wife Irena, Cairo January, 1st 1931, 3.

that he did not have at his disposal at the Prague University. In his collection of glass slides (approximately 1200 pieces) there are also pieces that were bought in photographic studios in Cairo; for example slides of objects from the tomb of King Tutankhamen.

He also visited the surroundings of Cairo; he was really excited to see the Pyramids in Giza:

> "The afternoon was spent with Mr Hais and his sister-in-law, seeing the Pyramids. New wonder; I imagined their greatness, and was afraid of being disappointed. The pyramids however, were exceeding expectations. They may fail to impress people who have no knowledge of mathematics, physics and geometry, but if you know something of the subjects, you are just wondering about what knowledge the builders must have possessed to be able to make the monument. It would have been a sin to go to Egypt unprepared – half the enjoyment would be lost.[14]

After visiting Cairo, Lexa continued further south to Luxor and Aswan, and he spent a couple of days in Deir el-Medina with his pupil and colleague Jaroslav Černý, who was then working for French Institute of Oriental Archaeology excavations. Lexa visited famous tombs and temples of Upper Egypt, made many photographs of these monuments and bought large amount of smaller antiquities.

> "I bought some antiquities, small and less so, in Luxor, and had a box made for them, which I sent together with my suitcase back to Cairo, to the embassy, where it would wait for me…"[15]

In one of his letters he comments on the gradual development of his skill in recognising and evaluating objects of Egyptian origin:

> "If I learnt to understand Egyptian antiquities in the Cairo Museum, then in Luxor I learnt to tell real antiquities from forgeries, that is, if it is at all possible, for there are pieces of craftsmanship so perfect that it is impossible to tell a fake from the real thing."[16]

František Lexa spent in Egypt nearly four months visiting ancient Egyptian monuments and collecting material for his students and for his own research. On the way back to the north he stopped at other archaeological sites and from Cairo he made a trip to Fayyum oasis together with Czechoslovak travellers František Foit and Jiří Baum. For a couple of weeks he became a member of the Czechoslovak community in Egypt, took part in the meetings in the salon of the Hais family in Cairo, and made lectures on Egyptology for them. Lexa used his new experience and material for his lectures at the university, e.g. he started with a lecture on archaeology and continued in his linguistic lectures using his own photographs and slides of ancient Egyptian inscriptions and many other details.

We can see many different reasons for Lexa's travels. His Egypt trip was professional, and aimed at improving his Egyptological knowledge. Furthermore, he was travelling to European countries to study Egyptian antiquities and inscriptions in museums. This travel pattern is not surprising in the 20th century; however, for a man who started his career as one of the grammar school teachers in Austro Hungarian monarchy for whom travel would be, if anything, part of leisure or family life, extensive professional travel was a new chapter.

Eventually, František Lexa was also travelling abroad to enjoy his holidays. He had many interests. He was an enthusiastic tourist and skier. He often made trips to the mountains and he loved especially Bulgarian mountains, which brought him to the Balkans. Lexa visited Bulgaria for holidays only. We can follow his tours to the seaside as well as to the mountains until the late 1950s, when he was climbing the Pirin range (Vichren 1958).

František Lexa made a great collection of materials from his journeys. He donated his Egyptian antiquities to the National Museum in Prague; in these days they form a part of the Egyptian collection of Náprstek Museum and part of the collection of the Prehistoric department of the National Museum. Photographs and postcards from his Egyptian journey are still in a possession of his family, that takes part on our project to preserve them for future generations of Egyptologists.

References

The National Museum Archives, (ANM), papers of Jaroslav Petrbok.

National Archive Prague, (NA), document collection Police Headquarters Prague, Section II, general records.

Archive of the Academy of Science (AAVČR), papers of František Lexa.

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[14] AAVČR, papers of František Lexa, box n. 2, f. n. 27, Letter from František Lexa to his wife Irena, Cairo January, 1st 1931, pp. 4–5.

[15] AAVČR, papers of František Lexa, box n. 2, f. n. 27, Letter from František Lexa to his wife Irena, Cairo January, 11th 1931, p 3.

[16] AAVČR, papers of František Lexa, box n. 2, f. n. 27, Letter from František Lexa to his wife Irena, Cairo January, 11th 1931, p 3.

PETRBOK, Jaroslav (1927), *Židovská práce a mravnost v Palestině* (*Jewish ethics and work in Palestine)*, Prague.

MACKOVÁ, Adéla Jůnová; NAVRÁTILOVÁ, Hana; HAVLŮJOVÁ, Hana; JŮN, Libor (2009). *Krásný, báječný, nešťastný Egypt (Lovely, marvellous*, *unfortunate Egypt*) Prague.

Učitelské noviny (magazine).

Jaroslav Petrbok

Fig. 1: Jaroslav Petrbok in his office in the National Museum, Prague.
ANM, papers of Jaroslav Petrbok, not registered.

Fig. 2: Travel to Bulgaria 1925 (Group of Jaroslav Petrbok in front of the train station at Varna).
ANM, papers of Jaroslav Petrbok, not registered.

Fig. 3:Travel to the Orient 1926 (Group of Jaroslav Petrbok at Athens, Acropolis).
ANM, papers of Jaroslav Petrbok, not registered.

Fig. 4: Advertisement of lectures about Palestine by Jaroslav Petrbok in 1936.
ANM, papers of Jaroslav Petrbok, not registered.

Fig. 5: Jaroslav Petrbok. ANM, papers of Jaroslav Petrbok, not registered.

František Lexa

389 16

(Form No. 1692, E.C.)

SERVICE DES ANTIQUITES No. 421

CARTE D'ENTREE GRATUITE
AU MUSEE EGYPTIEN & à la bibliothèque

Délivrée à Mr. le Dr. Fr. Lexa
Valable jusqu'au 31 Mai 1931
Le Caire, le 3 Janvier 1931

p. Le Directeur Général,
H. Gauthier

1

Cette carte est rigoureusement personnelle

Fig. 6: Card for free entry to the Egyptian museum in Cairo.
The Archive of the Academy of Science, Prague (AAVČR), papers of František Lexa, box n. 1.

(Form No. 1687, F.C.)

SERVICE DES ANTIQUITÉS

Nº 603

Carte délivrée le 3.1.31
à Mr le Dr. Fr. Lexa

Cette carte donne droit à l'entrée gratuite, pour une personne, dans tous les monuments de la Haute-Egypte et de Saqqarah.

Elle est valable pour une année, du 1er juin 1930 au 31 mai 1931.

Le Directeur Général,
H. Gauthier

Govt. Press 13,464-1929-500 ex.

رقم ٦٠٣

هذه التذكرة تبيح لحاملها دون غيره الدخول مجانا
فى المحلات الأثرية الكائنة بالوجه القبلى وسقاره
وذلك لمدة سنة واحدة من أول يونيه سنة ١٩٣٠
لغاية ٣١ مايو سنة ١٩٣١

This ticket gives free admittance, for one person, to all the monuments of Upper Egypt and Saqqarah. It is available for one year from June 1, 1930, to May 31, 1931.

Fig. 7: Card for free entry to all the monuments of Upper Egypt.
The Archive of the Academy of Science, Prague (AAVČR), papers of František Lexa, box n. 1.

Fig. 8: František Lexa with his wife Irena Lexová in the Bulgarian mountains, s. d.
Private archive, Lexa's family.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 322.

EUROPEAN VOYAGES AROUND THE LAPLAND EXPEDITION (1736 -1737)

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Abstract

The proposal focuses on the previous travels in different parts of Europe of some of the participating members of the concerned mission. This scientific travel was organized by the French Académie Royale des Ssciences, jointly with a similar mission to Peru, and was proposed in order to obtain a definite response to Newton's theory of gravitation after measurements made in France from the north to the south of the kingdom. Before their mission to Lapland, several participants visited England, Basel to be informed about the gravitation theory and implications. A new small English instrument was added to the equipment used in the measures in Lapland. Following this mission, further international cooperation and research settlement resulted for participants of the Lapland expedition, including other scientific travels in Europe.

The Lapland expedition is one of the two large expeditions ordered by the Académie Royale des Sciences in Paris in the 1730's to solve in a definite way the question of the figure of the Earth. This question was becoming very hot as there was some discrepancy between the flattening of the Earth deduced from measurements and experiments with the pendulum and the theoretical flattening deduced by Newton (1643-1742) and Huygens (1629-1695), both from different arguments. Newton used a result obtained by Jean Richer (1630-1696) in 1671 –the pendulum was slower at Cayenne than at Paris–: he inferred that the force of gravity was lower at Cayenne and explained this result, following his theory exposed in the *Principia Mathematica*, by the Earth being not a true sphere but a slightly oblate body (i.e. flattened at the poles). Huygens' argument was somehow different and implied all the bodies where there was a central force in $1/r^n$ –where r is the distance of a centre of force– situated at distance r from the figure of revolution –the Earth like an orange for both of them. These results contradicted those of Jacques Cassini (1677-1756) and his colleagues who, from local measured variations of degrees of latitude with latitude in France along the Paris Observatory Meridian line, deduced that the Earth was shaped like an elongated revolution ellipsoid; to explain theses discrepancies, some observers pointed out the possibility of observational errors, due to the precision of the instruments.

The Academy was very busy on that question and weekly debated both on the oblate or prolate "figure" of the Earth and the arguments and on what observations to complete to obtain a definite answer.

To be short, it is likely that in 1733 a young Academician, Charle Marie de La Condamine (1701-1774), while attending some meeting where the best ways to infer the actual shape of the Earth were being discussed, suggested a scientific expedition into a region far from France, where the variation of the length of a degree on a meridian line would be larger that in any measurement in France; the Academician Louis Godin (1704-1760) was ordered by King Louis XV and the Minister Comte de Maurepas (1701-1785) Secrétaire d'État à la Marine, to lead a measurement mission in Equator (now Peru); two other academicians were elected to participate in this voyage: La Condamine and Pierre Bouguer (1698-1758). This mission left La Rochelle and sailed for South America in May 1735.

Another Academician, Pierre-Louis Moreau de Maupertuis (1698-1759), following a visit to London in 1728, started a strong fight against the Cartesian mechanics and the ellipsoid Earth flattened at the equator inferred from the observations of the French astronomers, mainly Jacques Cassini (1677-1756) in position in the Observatoire Royal in Paris. Maupertuis attending the meetings of the Academy persuaded Minister Maurepas to plan a second scientific expedition, in the most distant place from the equator, in the polar regions; this second voyage was ordered in november 1735. With the help of a Swedish astronomer of his friends Anders Celsius (1701-1744), Maupertuis and the autorities, the Academy and the Royal Institution, elected a region near the Gulf of Bothnia in Lapland where the future measurements would be performed.

The participating persons to the Lapland expedition were Pierre Louis de Maupertuis as the leader; Alexis Clairaut (1713 -1765), an outstanding mathematician and a prominent French Newtonian whose calculations should be quite useful in the interpretation of the observational data; Charles-Etienne Camus (1699-1768), very skilled in experimental mechanics and time questions; Pierre-Charles Lemonnier (1715-1799), a very young Academician known as a very talented observer since he assisted his father Pierre Lemonnier (1675-1757); Abbot Reginald (or Regnauld) Outhier (1694-1774), elected correspondent of the Academy in 1731, considered too as a skilled observer –he had to write the log of the expedition. His book, the *Journal d'un voyage au nord en 1736 et 1737*, published in Paris in 1744, is a very pleasant and informative chronicle of the everyday expedition with many details about the astronomical observations and their organisation, about the people met by the travellers from Paris, about the cities and landscapes they went through.

One considers that this voyage definitively resolved the question about the figure of the Earth, the measurements completed in Lapland implying some flattening at the poles of the Earth.

Some voyages achieved by some participants to the Lapland expedition look like archetypes. The focus is on Celsius and Maupertuis' case, since they were the only ones who travelled abroad before the Lapland expedition (with the exception of a two months stay of Clairaut in the 1734 fall in Basel at Bernoulli's home following an invitation by Johan Bernoulli (1667-1748) possibly due to the influence of Maupertuis); another point is that their two voyages differed greatly in the aim and the way.

In a first part, the main abroad travels completed before the Lapland expedition will be presented; the travels following the northern mission will be discussed in second place.

Before the Lapland expedition

Pierre Louis Moreau de Maupertuis (1698-1759). Maupertuis was born in Saint-Malo (France). His family, thanks to his father's occupations in commerce and responsibilities in Brittany, acquired some influence and wealth. Maupertuis was first educated by his mother at home, then in Paris at the famous Collège de la Marche where he was taught mathematics by Guillaume LeBlond (1704-1781). Very young he showed a strong interest in mathematics and navigation; after a short stay in Holland, he came back to France as a volunteer for a musketeer company in Lille, he spent his free time working mathematics. As he was getting bored in the Army, he gave up this military career and came back to Paris. There he enjoyed some intellectual life with writers, mathematicians, musicians and carried on working on different mathematical questions with academicians as Guisnée (?-1718) and François Nicole (1683-1758). In December 1723, Maupertuis was appointed as adjoint- mécanicien member in the French Royal Académie des Sciences, to which, one year later, he submitted his first scientific paper: *«Sur la forme des instruments de musique»*; a second one was submitted in 1726: *«Sur une question de maximis et minimis»* ; he was 26 years old. These two papers show how extended his field of investigations was.

The following years, Maupertuis was abroad; he made two voyages of main importance to both his future scientific activity and his personal life. In 1728 he sailed to London where he met collaborators or scientists influenced by Isaac Newton (1643-1727, ns), who had just passed away. Maupertuis adopted quite immediately the Newtonian natural philosophy and held many meetings with prominent Newtonian figures such as Samuel Clarke (1675-1729), Henry Pemberton (1694-1771), and John Desaguliers (1683-1744). These mathematicians and philosophers either worked with Newton or taught and published books about the new Newtonian theory. Maupertuis became one among the first Newtonians in France and was very probably the most enthusiastic supporter of this new theory; all his future activity, in natural philosophy, mechanics and astronomy, was therefore changed. When he left London, since he had understood that studying the mechanics of Newton and the likely implications implied some improvement in his mathematical education, Maupertuis came into contact with Johann Bernoulli I (1667-1748), then a prominent senior teacher in the University of Basel. It followed a first stay in Basel with Bernoulli's family, during some months from 1729 to 1730. This first stay in Bernoulli's home was the beginning of both a fruitful collaboration and a very strong friendship. It is known that Maupertuis would die in 27th July 1759 in the arms of Johan Bernoulli (1710-1790), the third son of Johann Bernoulli I. More than ninety letters between Maupertuis and the Bernoulli familly are in the library of the Basel University, which show evidence of this strong and fruitful relations with the Bernoulli's family.

Quite different is the travel completed by ***Anders Celsius (1701-1744)***: a «grand tour» in Europe. Celsius was born in a scientist family; the Celsius «dynasty» was rich in astronomers and mathematicians. Celsius' grand-father Magnus Celsius (1621-1679) settled his observatory at home; he was professor of mathematics and astronomy in Uppsala University since 1668; during the 1664-1665 years he observed comets; his main published works are *Dissertatiuncula de cometis* (1665), *Tractacus de mundo visible* (1672), and *Dissertatio de cumparatione corporum ceolestium ad tellurem* (1672). Celsius' father Nils Celsius (1658-1724) had a tenure in Uppsala University, where he taught astronomy, mathematics, geography, poetry, and prose; his main published works are *De principis astronomicis propriis* (1679) and *Calculus astronomicus super theoria solis* (1685); Nils Celsius is known to have computed, in 1705, an Easter date a week too late. Anders Spole (1630-1699), the other grand-father of Anders Celsius, was too a prominent astronomer of his time: in 1688, he was appointed as professor in the newly created Lund University, where he equipped an astronomical observatory; later in 1679, he came to Uppsala as a professor, where he taught mainly about trigonometry, spherical and general astronomy, geography, optics, navigation, chronology, and ecclesiastic computes; Anders Spole founded a new observatory with new instruments (telescopes, quadrants), he observed the «Great» comet of 1680; he travelled abroad making his own «grand tour», from 1664 to 1667, and had meetings with the most renowned astronomers and philosophers of his time: Christian Huygens (1629-1695), Mercator, Robert Hooke (1635-1703), Robert Boyle (1627-1691), Jean-Dominique Cassini (1625-1712), and Giovanni Riccioli (1598-1671). Anders Spole presided over many dissertations and wrote textbooks, either in the *Acta* of the Swedish Academy or in the English *Philosophical Transactions*, the oldest scholarly journal in the modern sense. Most of these persons did travel from one university to another in Europe.

Anders Celsius showed very young some talent in mathematics and natural philosophy; in 1728 he was appointed professor in Uppsala, where he stood for Samuel Klingenstierna (1698-1765) then abroad. His first written work was published both in the Swedish academic *Acta* and the *Philosophical Transactions*; the following reference papers indicate the large field of his interest: 1724 publication: *Experimentum in argenti-fodina Salana circa ascensum Mercurii in barometro institum (Acta literaria Sveciae, 1724, p599-600) and (Phil. Tansactions,vol.33, 1724-25, 313-314)*; 1730 Publication: *Nova Methodus distantiam solis a terra determinandi* (resp. Marten Strõmer).

Celsius was granted by the Uppsala University autorities, to perfectionate his different skills, to meet European colleagues, to visit the most famous observatories and «scientific» universities and last, to acquire modern instruments; therefore he organised his European "Grand Tour" from 1732 to 1736, while carrying on his own astronomical observations and publishing on different subjects.

During this four year "Grand Tour" Celsius visited four countries: Germany, Italy, France and England; he departed from Uppsala in August 1732. From August 1732 to August 1733 Celsius was in Germany; in Berlin he attended some lessons by Christfried Kirch (1694-1740), director of the observatory where he completed some astronomical observations, mostly on the solar eclipse of 13 May 1733.

At the end of May he went to Nürnberg via Leipzig and Wittenberg, where he met Michael Adelbulner (1702-1779), a famous printer later teacher in mathematics and physics; both of them decided to create an astronomical newsletter, on which Celsius collaborated quite efficiently from 1733 to 1735. During his stay in Nürnberg in 1733, Celsius published a book related to auroral phenomena (northern lights) observations that he had collected: *CCCXVI observationes de lumine boreali ab a. MDCCXVI tad MDCCXXXII partim a se, partim ab aliis, in Suecia habitas,* collegit Andreas Celsius. While in Nürnberg, Celsius made astronomical observations with a new quadrant by the instrument maker Johan Ernst Esling from Berlin.

In August 1733 Celsius left to Italy. There, he visited Venice, Padova, Bologna, where he remained eight months, and finally Rome. He met Martin Folkes (1690-1754) Vice-president of the London Royal Society, and Giovanni Poleni (1683-1761), professor of astronomy, philosophy, and mathematics from Padova; Poleni was a talented scientist fellow of the London Royal Society and other academies. While in Bologna, Celsius worked and made some astronomical observations with Eustachio Manfredi (1674-1739) and Manfredi's assistant Eustachi Zanotti (1709-1782). Some of these observations were completed either in the observatory (here he used a mobile new quadrant and two mural ones) or in St Petronio Cathedral (where he used the big meridian line on the pavement in the Cathedral); some observations were photometric ones, and other related to the obliquity of the ecliptic. These observations were reported in the *Histoire de l'Académie Royale des Sciences*, 1735, by Dortous de Mairan (1678-1771) and in a publication of Bologna. In Rome, Celsius carried on his photometric observations, completed some verification of the Meridian Line in St Maria degli Angeli and observed the partial solar eclipse of 3rd May 1734 (the description of this observation is in the Philosophical Transactions 1738).

Celsius went to Paris in August 1734, being accompanied by Francesco Algarotti (1712-1764), a writer and philosopher who later, in 1737, published a very illuminating book about the Newtonian theory: Neutonianismo per le dame (this book had two translated editions: in French in 1738, *le Newtonianisme pour les Dames*; in English as *Theory of light and colors* in 1739). In France, Celsius had discussions with academicians and got in touch with some of them, specially Pierre Louis Moreau de Maupertuis, a strong support of the theory of Newton. He entered the group of Newtonians. He attended many heated sessions in the Académie Royale des Sciences where were debated the shape –the «figure»– of the Earth and the available observations to definitely solve the related question. Celsius participated in the final choice and organisation of the future astronomical and geodetic expedition, far from France in northern Europe that was to be led by Maupertuis.

During the summer 1735 Celsius sailed to London; there he got in touch with the prominent philosophers and Newtonians of the time: Hans Sloane (1660-1752), naturalist, collector, patron of science, the president of the London Royal Society after Newton; Cromwell Mortimer (1730-1752), Edmund Halley (1656-1742) and James Bradley (1692-1762). At that time, Celsius knew that he was going to participate in the Lapland expedition with Maupertuis, and tried to have the best instruments, as did the other expedition to Peru: The question «did Celsius obtain the new octant by John Hadley (1682-1744) which was acquired and used by Pierre Bouguer (1698-1758) in the Peru expedition, the related expedition ordered by the Académie Royale des Siences to solve the same question that the expedition to Lapland had to do?» has not a definite answer at the moment. During his stay in London, Celsius received letters fom Maupertuis worrying about the instruments which had been ordered to the most skilful English instrument makers, particularly a pendulum with the equipment to use it and a sector which had to be delivered before the departure to Lapland. In that aim Celsius met some of the Londonian instrument makers: George Graham (1675-1751), John Hadley (1682-1744) and John Ellicot (1706-1772). He made some astronomical observations and had some training in the use of the new instruments. The *Philosophical Transactions* report about observations of boreal auroras and of a lunar eclipse made by Celsius in Mr. Graham's house in London. Celsius left England and sailed to Dunkerque (France) in April 1736 to join his colleagues of the Lapland expedition in their way to the North.

After the Lapland expedition

A last mission was ordered to the four Academicians: a new measurement of the length of a degree of meridian between Paris and Amiens (this measurement had already been completed by Jean Picard in 1671) with the Lapland expedition instruments by using the same modus operandi: measurement of the distance between the two places, then measurement of the amplitude of the meridian arc.

It resulted that the degree measured in Lapland by the Academicians was 123 *toises* larger than the one measured by Picard. To be short, the question of the figure of the Earth was solved.

Maupertuis and Celsius followed different ways after the Lapland expedition. Whereas Celsius returned to Uppsala after his absence of five years, Maupertuis could not stay in France.

Celsius returned to Uppsala

There, in his university, he had to complete his many professor duties: teaching, thesis, lobby and success to establish the new and modern observatory in Uppsala; he was busy with its instrumental equipment, with the organization of the scientific activity of the Societas Scientiarum, with the completion of stellar catalogues; with the continuation of his own research on stellar photometry; with the definition of a new calendar as in other places in Europe (this question would lead to the new style calendar adoption in Sweden in 1753, after Celsius' death); with the definition of the thermometer temperature scale (the «Celsius scale»): 100° water freezing point, 0° water boiling point (this scale will be inverted later under the influence of Linnaeus (1707-1778). Coming back to his country, he had a very fruitful activity. He died quite young, aged 43, in 1744.

Maupertuis in Berlin

Before the Lapland expedition, Maupertuis had a very rich social life, enjoying, joking with a lot of friends, publishing many works; but due to his strong fight against Cartesianism and his strong support to Newtonianism, he made «enemies» very easily. When he came back from Lapland his relations with many other Academicians were still not pacified. The «older» Academicians in Paris, mainly friends of the Cassini, were still reluctant about the new theory of mechanics, and about the measurements obtained in the North during the Lapland expedition which meant a flattened Earth at the poles in opposition with the observational results of the Cassini Dynasty's measurements.

The return from Lapland was quite difficult, with many critics about the operations; Maupertuis, very early after the return to Paris, carried on a lengthy correspondence with Johann Bernoulli –a monthly letter, the postal car left Paris to Basel every Monday morning– and explained his operations, his results (a letter was sent from the Zuidersee as he sailed back to France), he visited his friends again in Basel to clarify his method and the results.

After some long discussions inside and outside the Academy, Maupertuis was quite disapointed, frustrated; the Lapland expedition results had been very questionable. In spite of all the explanations and verifications completed during the expedition, the «classical astronomers» , with Jacques Cassini (1677-1756) as a leader, went on in suspecting the quality of the measurements. As there were difficulties with the Académie Royale des Sciences, Maupertuis went on in different travels: to Saint- Malo, his youth town where he was happy to meet his family and he was confident of recovering a good health by breathing fresh air; in Cirey, near the independent region of Lorraine, where, in the château, lived the famous female scientist Gabrielle Émilie Le Tonnelier de Breteuil, Marquise du Châtelet (1706-1749) and, since 1734, his friend François Marie Arouet, known as Voltaire (1694-1778), who discovered Newtonism in London where he had been in 1728 to avoid a stay in the Bastille Royal jail. Both Mme du Châtelet and Voltaire needed a teacher in mathematics. Essentially, Mme du Châtelet did, as she was undertaking to make a French translation of the *Philosophiae Naturalis Principia mathematica* (1726 edition); her translation, *Principes mathématiques de philosophie naturelle*, published in Paris in 1756 (after Mme du Châtelet's death) permitted an easy diffusion of this work in Europe to a large public. Voltaire was among the first admirers of Maupertuis.

In the year 1739 three European kings would admit Maupertuis in their court or Academy to benefit of his knowledge and his renown: the kings of France, Russia and Prussia.

In 1746 King Frederick the Great (1712-1772) appointed Maupertuis as president of the new Académie Royale des Sciences et des Belles Lettres in Berlin; here, from 1746 to 1759, he could work with the most famous mathematicians and philosophers of the time. It seems that Voltaire (1694-1712) was the first to suggest to the future king of Prussia, in 1738, that his friend Maupertuis was the right person to run his new academy according to his wishes. Maupertuis met Frederick for the first time during the Prussian-Austrian war; Frederic, always in front of his army, went from one battlefield to another and Maupertuis followed trying to get meetings on the status of the Academy. In Berlin, as President of the Academy, Maupertuis had a quite tremendous activity: administration, organization, nomination of academicians, scientific research, publication of many works. The story and the results of this more stay of more than ten years in Berlin are not to be presented here.

In spite of his bad health (which, from some letters sent to Bernoulli in 1733, was already so before the expedition in the northern conditions), Maupertuis could stay and work in Berlin until May 1756. At that time he came back to Saint- Malo to try to recover from illness; it was the time of a European war and the two countries, Prussia and France, fought as enemies, which made Maupertuis quite unhappy.

After a long travel from Saint-Malo to Bordeaux, Lyon, and finally to Basel, he died in 1759, in Bernoulli's house, in the arms of his friend.

* * *

These two people, Maupertuis and Celsius, were very prolific, each of them in their own field. They were not the only ones to travel in Europe and to settle for some time in another country. Their travels were quite different: one of these two travels may be considered as a «grand Tour», since Celsius visited many places in Europe, met a lot of different persons – just a few names were indicated in this report–, used different instruments, and lastly, this acquired experience was to give advantage to the Uppsala Observatory. At the opposite, the other one, Maupertuis, had few very close friends –the Bernoulli family– who during his whole life would help him (with the exception of Voltaire, who later will be a terrible enemy); when Maupertuis left Paris to work in the new Academy in Berlin, a huge task was waiting for him.

Celsius became very famous as participating to the Academician mission to Lapland, which later made it easier for his attempts to modernize the Uppsala Observatory in his country, which he did not leave any more. At the opposite, after the success of the Lapland Mission, Maupertuis, very disappointed with some reactions in Paris, left his country and for about ten years he built a new life in another country, another family, another King. In Berlin he met many other mathematicians and philosophers from the whole Europe. The worries he wanted to leave in Paris might have found him in Berlin.

References

BRUNET, Pierre (1929). *Maupertuis, Étude biographique*, Paris, Pierre Blanchard.

NORDENMARK, N. V. E. (1936) *Anders Celsius*, Stockholm, Uppsala, Almqvist and Wiksells Boktryckeri

MOREAU, Pierre Louis. *Die Briefwechsel des mathematiker Bernoulli, Bernoulli Korrespondenz zwischen Johann I und Maupertuis*, University of Basel, library, www.ub.unibas.ch

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AND BEYOND EUROPE:
THE JESUITS AS
CIRCULATORS OF SCIENCE

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 328.

MATHEMATICS IN THE JESUIT COLLEGES IN SPAIN IN THE 17TH AND 18TH CENTURIES

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Abstract

Jesuits established a network of colleges in Spain which reached 126 in the 18th century. According to the Ratio Studiorum mathematics should be included in the philosophy programs of all Jesuit schools. In contrast to other European countries, Spanish Jesuits showed a certain resistance to fulfil this requirement. Explicit mention of mathematics professors are only recorded for the colleges of Madrid, Barcelona, Cadiz and Calatayud. The longest list is that of the Colegio Imperial of Madrid from 1627 to 1767, but of the 23 professors only 13 were Spaniards. In the other colleges, with the exception of Cadiz, teaching mathematics began in the middle of the 18th century. Teaching was centred in geometry during the first years, and later in algebra with the introduction of differential and integral calculus in the last years before the expulsion of Jesuits from Spain in 1767. The professors of the Colegio Imperial, where two chairs of mathematics were established in 1625, left a number of published books and unpublished manuscripts. Among the Spaniards excelled José Zaragoza and Tomás Cerdá, who proposed the publication of a series of mathematical textbooks. Some foreign professors were della Faille, Richard, Petrei, Wendlingen and Rieger. Mathematics were given a special importance in the Seminarios de Nobles of Madrid, Barcelona and Calatayud, founded in the middle of the 18th century. The contents of the teachings can be followed from the Conclusiones Mathematicas, the published programs of mathematical dissertations by students of the Seminario de Nobles and the Colegio Imperial of Madrid between 1704 and 1762. In them we can find when modern mathematics was introduced as well as the acceptance of the Copernican system and Newtonian physics.

Mathematics in Jesuit colleges

The most influential figure in introducing mathematics teaching in Jesuit colleges was Christopher Clavius (1537-1612), professor of mathematics in the *Collegio Romano* between 1567 and 1612. We must remember that, then, mathematics included also astronomy, optics, mechanics, music and any other science where mathematics was applied. During the years 1580 to 1593, Clavius insisted in the need of teaching mathematics in all Jesuit colleges in the programs of the three years dedicated to philosophy. He defended that mathematics was necessary for understanding natural philosophy, taking a position that was introduced by the proponents of modern science. Among his writings defending this position are: *Ordo servandus in addiscendis disciplinis mathematicis* and *Modus quo disciplinae mathematicae in scholis Societatis possent promoveri*. He took part in the composition of the norms that were to regulate the studies of all Jesuit colleges, known as the *Ratio studiorum*. Clavius contributed to the inclusion of mathematics in the three versions of the *Ratio*: in the first of 1586 in the chapter on mathematics (*De mathematicis*) and in the second of 1591, and the final of 1599 in the rules for the professors of mathematics (*Regulae professoris mathematicae*). In the final version of the *Ratio*, mathematics was included in the program of studies of the second year of philosophy. Professors of mathematics were to be in all schools which had programs of philosophy. At least, it was established that the programs included Euclid geometry, astronomy (*sphaera*) and geography. The *Ratio* recommended also the creation of an academy of mathematics (*Academia rerum mathematicarum)* for the most interested and able students, where more advanced topics were treated. These academies were to serve for the training of future professors (Lukacs, 1986). The norms of the *Ratio* concerning mathematics were, in general, well received and accepted in most colleges in Europe, and in the about 600 colleges 92 chairs of mathematics were established (Fischer, 1978, 1983).

The situation in Spain

During the composition of the *Ratio*, the norms about the teaching of mathematics got in Spain a rather negative reception. Some commentaries sent back to Rome on this subject were: "The teaching of mathematics is here neither necessary nor possible"; "One cannot find adequate professors"; "Other subjects are more necessary and of greater use in the Society"; "In Spain mathematics is of none or little use"; "For our students its utility is very small"; "It will be enough that the professor of philosophy explain a little of astronomy when teaching Aristotle's *De Coelo*". These comments give an idea of the little interest that Spanish Jesuits had about the subject and the little interest also in Spanish society. However, the answer from Rome to these comments was that in all colleges where there was a program of philosophy, teaching of mathematics was obligatory (Lukacs, 1992).

In Spain in the 18th century there were 126 colleges, most of them founded in the end of the 16th and beginning of the 17th century. With full programs in philosophy, were those of Alcalá de Henares, Barcelona, Calatayud, Gandia, Girona, Granada, Madrid, Medina del Campo, Monforte, Palencia, Palma, Pamplona, Salamanca, Sevilla, Valencia and Valladolid. According to the norms established in the *Ratio*, in these colleges there should have been professors of mathematics. However, in spite of the norms, very few colleges had specific professors of mathematics which figured with this title in their professor lists (Udías, 2010).

The only colleges with appointed professors of mathematics were, in fact:

- Madrid, *Colegio Imperial*, 1627-1767.
- Cadiz, *Colegio*, 1689-1702.
- Madrid, *Seminario de Nobles*, 1734-1767.
- Barcelona, *Seminario de Nobles*, 1756-1767.
- Calatayud, *Seminario de Nobles*, 1756-1767.

This doesn't mean that mathematics was not taught at all in the other colleges, but probably it was done at a very elementary level, so that no special professors were needed and classes of mathematics, if existed, were handled by professors of philosophy. The programs of philosophy were based on Aristotelian philosophy. It is possible that at least some elements of geometry and arithmetic and some fundamental notions of astronomy were taught to complement the teaching of natural philosophy, specially Aristotle's treatises of *De Coelo* and *Meteorologica.* We don't have evidence of this, since no written documents have been found of such lectures.

The *Colegio Imperial* of Madrid

The most important exception to this situation was the *Colegio Imperial* of Madrid (Simón Díaz, 1952). A small school existed in Madrid since 1572, which grew very fast so that in 1598 it had about 750 students. In 1609 it was newly founded with the name of *Colegio Imperial* under the patronage of the empress Mary of Austria, daughter of Charles V and wife of Maximilian II. The school was raised to the category of *Reales Estudios* (Royal Studies) by Phillip IV in 1625, with university level, though it could not confer academic degrees. Since 1627, it had two chairs of mathematics among a total of 17 chairs. This was more than any university in Spain had at that time. From 1627 to 1767, there were 23 professors, of whom 10 were not Spaniards. They came from France, Belgium, Scotland, Switzerland, Bohemia, Italy and Austria (Udías, 2005). Difficulty in finding professors of mathematics among Spanish Jesuits show the little interest they had for this subject. There was, however, a need to fill the two chairs created by the royal founding with experienced professors. The first four professors were all non-Spaniards: they came from Switzerland, Belgium, Scotland and France.

Among the non-Spanish professors, the following deserve a special mention (Udías, 2005). Jan Karel della Faille (1597-1652), born in Antwerp (Belgium), was disciple of Gregory of Saint-Vincent and had succeeded him as professor in the University of Louvain. He was professor in Madrid in 1629-1647. He was specially interested in the problem of the determination of the gravity centre, and published *Theoremata de centro gravitatis* (1632). Hugh Semple (1589-1654) was born in Scotland and was professor in 1630-1640. He published a general textbook about the different subjects included under the heading of mathematics, *De mathematicis disciplinis* (1635). In this book he defended that mathematics was a true science, in the Aristotelian sense of the term, against philosophers who denied it. Claude Richard (1589-1664), born in Omans (France), occupied the chair for the longest time, 1630-1664. He was specially interested in geometry and published a commentary to Euclid's books, *Euclidis elementorum geometricorum* (1645), and another to the books of Apollonius on conic sections, *Apollonii Pergaei conicorum* (1665). These were not textbooks for the students, but advanced treatises. Jakub Kresa (1648-1715) was born in Smrzice (Check Republic) and professor in Madrid in 1686-1701. He published a translation into Spanish of Euclid's books 1-6 and 11-12, *Elementos geométricos de Euclides* (1689), intended to be used as a textbook for students. Johann Wendlingen (1715-1790), born in Prague (Check Republic), was professor in 1750-1767. Aware of the lack of good textbooks in Spanish, he published *Elementos de matemáticas*, 4 vols. (1753-1756). They covered at an elementary level arithmetic, geometry, trigonometry and logarithms. He was also interested in astronomy and made observations with telescopes. Christian Rieger (1714-1780), born in Vienna (Austria) was professor in 1761-1765. He had been professor of architecture in the University of Vienna before coming to Madrid and had published there two books on civil and military architecture: *Universae architecturae civilis* (1756) and *Universae architecturae militaris* (1756). In Madrid he taught mathematics, architecture and experimental physics.

Of the thirteen Spanish professors, two excelled for their production of published textbooks and their importance in the development of mathematics in Spain: José Zaragoza and Tomás Cerdá (Udías, 2005).

José Zaragoza y Vilanova (1627-1679), from Alcalá de Chisvert (Castellón), moved to Madrid from the school in Valencia where he had privately studied mathematics as he taught theology (Cotarelo Valledor, 1935). He occupied the chair of mathematics in the *Colegio Imperial* from 1670 to 1679. During this time, in order to remedy the lack of books about mathematics he conceived the plan of publishing a complete set of books covering all related subjects from arithmetic to astronomy. He published 12 textbooks of mathematics and astronomy in Latin and Spanish, among them: *Aritmética universal* (1669), *Geometría especulativa y práctica* (1671) and *Trigonometría española* (1672). His most important work was *Geometria magna in minimis* (1674), a book about the determination of the centre of gravity for plane and solid figures solved on purely geometrical methods.

A different character has his book *Tratado de la esfera en común, celeste y terraquea* (1675), where he presents the principles of spherical geometry, astronomy and geography. He presents the Copernican system, but due to the ecclesiastical censure, he adds that it can only be accepted as a hypothesis. He shows to know about modern authors, and defends the geo-heliocentric system proposed by the Italian Jesuit Giovanni Battista Riccioli (1598-1671) in 1651, a modification of Tycho Brahe's system, in which the Earth is at the centre, the Sun moves around it with Mercury, Venus and Mars rotating around it. He made also astronomical observations, specially, of the comets of 1664, 1665 and 1677. He is considered to be one of the most important figures in science in the Spain of his time.

Tomás Cerdá (1715-1791), born in Tarragona, was professor in the *Seminario de Nobles* (Barcelona) in 1756-1764 and in the *Colegio Imperial* (Madrid) in 1765-1767 (Gassiot y Matas, 1995). He was trained between 1753 and 1756 in science and mathematics in Marseille by Esprit Pezenas (1692-1776), Jesuit director of the observatory. There, Cerdá got in contact with the modern Newtonian science and mathematics. Pezenas had translated into French several works of English scientists, among them the mathematical works of Colin Maclaurin (1698-1746). As he returned to Barcelona, he brought these new ideas. Aware of the backwardness in mathematics in Spain, he proposed to publish a series of books covering from elementary arithmetic and geometry to the new developments of differential and integral calculus. He could only publish the first two books. *Lecciones de matemáticas*: vol. 1, *Elementos generales de arithmética y álgebra*; vol. 2, *Sobre ecuaciones* (1758). His career was cut short by the Jesuits expulsion of Spain by order of King Charles III in 1767. He left manuscripts of his projected books on differential and integral calculus and Newtonian astronomy that he was not allowed to take with himself to his exile in Italy. He was one of the first in Spain to teach differential and integral calculus and Newtonian mechanics and astronomy.

The *Seminarios de Nobles*

A special type of Jesuit schools were the *Seminarios de Nobles* (Seminaries for the Nobility), founded in the 18th century with the specific aim of training the young from the Spanish nobility (Martínez Escalera, 1993). There were four Seminaries: in Madrid, Barcelona, Valencia and Calatayud. Their programs didn't follow those of normal colleges, but taught what was thought to be necessary for a nobleman. Their programs included modern languages, history, mathematics, astronomy, experimental physics and also military training, dance, horsemanship and fencing. Those of Madrid, Barcelona and Calatayud had specific mathematics professors. The most important one was that of Madrid, founded in 1725 by King Philip V, and which had chairs of mathematics and experimental physics. The chair of experimental physics was a novelty and only existed in Madrid from 1757 to 1767.

In the *Seminario de Nobles* of Madrid there were professors of mathematics between 1734 and 1767. Among them we can mention Esteban Terreros (1707-1782) and the Italian Esteban Bramieri (1720-?). Terreros is better known for his dictionary of the Spanish language. His scientific work was his translation into Spanish of the work by Noël Antonio Pluche (1688-1761), *Espectáculo de la Naturaleza*, an encyclopaedic work of 16 volumes, in which he introduced the doctrines of

Descartes and Newton. The *Seminario* of Barcelona created a chair of mathematics in 1757, that was first occupied by Cerdá from 1756-1764. He was succeeded by Ignacio Campcerver (1722-1798) until 1767, except for a year that he spent in the *Seminario* of Calatayud. In the Italian exile he published *Bibliotheca mathematica* (1789).

Introduction of modern science and mathematics

The introduction of modern science and mathematics was delayed in Spain with respect to the rest of Europe. Newtonian physics and differential and integral calculus, which were generally accepted in continental Europe in the beginning of the 18th century, were not introduced in Spain until nearly fifty years later in the middle of the century. Since Jesuits were expelled from Spain in 1767, they had very few years to introduce the modern ideas in their teaching. To find out how they managed to introduce the new ideas in their teaching, we have to rely on the documents which are preserved in manuscript form mainly of the professors of the *Colegio Imperial*. When Jesuits were banned into exile from Spain in 1767, they were not allowed to take with them any of their books or manuscript notes, so they have been preserved in different archives. Those belonging to the professors of the *Colegio Imperial* are kept at the library of the *Real Academia de la Historia* in Madrid.

A search in these manuscripts has shown the approximate years when modern science and mathematics were introduced in the teaching of the *Colegio Imperial* (Udias, 2005). A manuscript on differential calculus with references to Euler and Wolff, titled *In methodum fluxionum introductio*, has been assigned with great probability to Rieger and may correspond to the period 1761-1765. This may be one of the first references of differential calculus in Spain. Cerdá promised in the preface of his published textbook a volume on differential and integral calculus. A set of manuscripts on this subject are clearly his work. Some of the notes use both Newton's and Leibniz' notation and give the resolution of simple differential equations. If they correspond to his teaching years in Madrid they can be dated about 1765-1767. The acceptance of the heliocentric system was complicated by the ecclesiastic condemnation on the book of Copernicus in 1616 and of Galileo in 1633. The system could only be taught as a hypothesis, not as the real situation. This ban was lifted in 1757 by Pope Benedict XIV. A manuscript of Rieger dated on 1763 clearly presents the heliocentric system. Cerdá left in manuscript form a complete text, *Tratado de astronomía*, in which Newtonian astronomy is clearly presented. This text is dated in Barcelona, 1760 (Cerdá, 1760, 1999).

The acceptance between 1704 and 1762 at the *Colegio Imperial* and the *Seminario de Nobles* of Madrid of the new developments in mathematics, astronomy and physics can be obtained from the programs of the organized public disputations on mathematics, cosmography and experimental physics held at those schools and published under the title of *Conclusiones mathematicas* (Udias, 2011). They were presented by students and presided by a professor. Twelve published programs between 1704 and 1762 are preserved, which show the progress of the introduction of modern mathematics and science. In mathematics in the period 1704-1748, the main subjects were geometry and arithmetic with very little algebra. For 1751-1765, the preponderant topic was algebra with elements of analytic geometry, and with the introduction of elementary differential and integral calculus with simple differential equations. In astronomy in the period 1704- 1734, still the geocentric system was proposed and the motion of the Earth denied. The Copernican system, prohibited by the Church, was presented, but only accepted as a hypothesis. In the program of 1740, the Copernican system was presented and no mention was made of its prohibition. In the last programs of 1748 and 1760, planetary motion was presented according to Newtonian astronomy, but ecclesiastical censure was still mentioned. In physics, modern developments were slowly introduced, for example, in the period 1734-1748, topics of magnetism, propagation of light and sound, and weight of the air were presented. In 1760, Newtonian gravitation, existence of vacuum, light and sound waves, and properties of electricity were presented. Though these may seem to be late dates in comparison with the rest of Europe, Jesuits were actually pioneers in their introduction in Spain.

Conclusion

Of the 126 Jesuit schools in Spain during the 17th and 18th centuries, only six had regular professors of mathematics. The *Colegio Imperial* (Madrid) had two chairs and regular classes of mathematics from 1627 to 1767. Half of its professors were foreigners. Of the Spanish professors only Zaragoza and Cerdá excelled. In the 18th century the *Seminarios de Nobles* of Madrid, Barcelona, and Calatayud introduced in their programmes mathematics and experimental physics. Modern developments of mathematics, astronomy and physics were introduced in their teaching only about the middle of the 18th century. This situation can be explained by the lack of social interest in Spain for mathematical sciences in the 17th and 18th centuries. To remedy this situation Jesuits were forced to bring professors from outside Spain. In spite of the delay, Jesuits were among the first to introduce in Spain the modern developments of mathematics and science.

References

CERDÁ, T. (1760, 1999), *Tratado de astronomía. Curso dictado el año1760,(*Introduction and notes by Lluis Gassiot i Matas). Barcelona: Reial Acadèmia de Ciències i Arts de Barcelona.

COTARELO VALLEDOR, A. (1935), El P. José de Zaragoza y la astronomía de su tiempo. En: *Estudios sobre la Ciencia Española del Siglo XVII*. Madrid: Asociación Nacional de Historiadores de Ciencia Española.

FISCHER, K. A. F. (1978), Jesuiten-Mathematiker in der Deutschen Assistenz bis 1773 *Archivum Historicum Societatis Iesu,*. 47 (1978) 159-224.

FISCHER, K. A. F. (1983), Jesuiten-Mathematiker in der Französischen und Italienischen Assistenz bis 1762 bzw. 1773. *Archivum Historicum Societatis Iesu,* 52 (1983) 52- 92.

GASSIOT I MATAS, L. (1995), El P. Tomàs Cerdà i la introducció del pensament newtonià a Barcelona. *Actes III Trobades d'Història de la Ciència i de la Tècnica*, Barcelona 247-251.

LUKACS, L. (1986), *Monumenta Paedogica Societatis Iesu*, vol. V. Rome: Institutum Historicum Societatis Iesu.

LUKACS, L. (1992), *Monumenta Paedogica Societatis Iesu*, vol. VII. Rome: Institutum Historicum Societatis Iesu, 147-162.

MARTÍNEZ ESCALERA, J. (1993), Los seminarios de nobles" en Buenaventura DELGADO CRIADO, ed., *Historia de la educación en España y América. La educación en la España moderna (Siglos XVI-XVIII).*Madrid: Ediciones SM, 854-861.

SIMÓN-DÍAZ, J. (1952), *Historia del Colegio Imperial de Madrid*. Madrid: Consejo Superior de Investigaciones Científicas.

UDÍAS, A. (2005), Los libros y manuscritos de los profesores de matemáticas del Colegio Imperial de Madrid, 1627-1767. *Archivum Historicum Societatis Iesu.,* 74, 369-448.

UDÍAS, A. (2010), Profesores de matemáticas en los colegios de la Compañía en España. *Archivum Historicum Societatis Iesu*, 79 3-27.

UDÍAS, A. (2011), Conclusiones Mathematicas en los colegios jesuitas de Madrid, 1704-1762. *Archivum Historicum Societatis Iesu* (in press).

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 333.

OPTICAL INSTRUMENTS IN THE SERVICE OF GOD. LIGHT METAPHORS FOR THE CIRCULATION OF JESUIT KNOWLEDGE IN CHINA

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Abstract

The circulation of knowledge is accomplished by material means. Books and other written sources are often seen as privileged material bearers of knowledge, but other material objects are also suited for the distribution of practical as well as theoretical knowledge claims. In this paper, I will show how Jesuit authors were perceived to disseminate metaphysical and theological views by 'embodying' them in optical instruments. (I will understand this process of 'embodiment' in terms of the integration of scientific instruments, often accompanied by written inscriptions, in specific religious practices). Jesuits did not only promote their missionary work by preaching and by circulating theological volumes; they also did this by circulating material scientific objects through their vast missionary networks. In this paper, I will focus specifically on how the Jesuits' use of optical instruments was commented on by contemporaries (e.g. in Johann Christoph Wagners, Das mächtige Kayser-Reich Sina*, 1685). In the confrontation between the different parties in a religious controversy, otherwise tacit aspects of religious and technical practices become more explicit. This will give us a better understanding of the Jesuits' use of scientific instruments and their reception in a global context.*

Introduction

This paper is part of a larger project. I am interested in the relation between natural philosophy, mixed mathematics and religion in the early modern period. In particular, I want to look at how optical instruments were used in religious practices and how they acquired religious meanings. In this paper, I will look at *Das mächtige Kayser-Reich Sina und die Asiatische Tartaren*, a description of the Chinese empire, published by Johann Christoph Wagner in 1688. Wagner's book is an interesting case study, because the book is written by a protestant who based himself mainly on Jesuit sources. Furthermore, this book is published exactly at the moment of the Chinese rites controversy. This paper can therefore be considered a reception study of the Jesuit circulation of knowledge at a moment particularly fraught with confessional tensions.

In this paper, I will not only use the 'circulation of knowledge' as a historiographical concept. I also aim at studying the actor's own views on this circulation, and we should keep in mind this double use of 'circulation'. Wagner's book *Das* *mächtige Kayser-Reich Sina* is a description and an iconography of the Jesuit circulation of knowledge, from Europe to China.

This circulation is seen by Wagner through a specific lens, influenced by his protestant background and local context. At the same time, the book is itself the result of the circulation of knowledge back from China to Europe. It is a collection of data and narratives, based mainly on Jesuit descriptions, self-presentations and even propaganda, resulting in a very specific perspective on China.

Light metaphors and the frontispiece of *Das mächtige Kayser-Reich Sina*

Johann Christoph Wagner was born in 1640 in the German evangelical imperial city of Nürnberg. His father, Christoph Wagner, was cantor at the Lutheran Church of Saint Gilles (Sankt Egidien). From 1659 onwards, Johann Christoph Wagner studied at the famous University of Altdorf and in 1663 he held a disputation on his dissertation on astrological medicine, *De respectu corporum naturalium ad sidera in se agentia*, under supervision of Abdias Trew; and on the occult properties of seals, *De occultis qualitatibus et potissimum de sigilis*, under supervision of Johann Christoph Sturm. Later, Wagner would build a career as a popular writer in Augsburg. He wrote about other regions and cultures, about comets and he made calendars. Wagner, an evangelical Lutheran, moved around in a very difficult religious context, with Nürnberg and Augsburg located exactly at the changing frontline between Catholics and Protestants. *Das mächtige Kayser-Reich Sina* was a collection of the available knowledge on China presented in the vernacular. Wagner had never been to China, but his popular description was a compilation of diverse sources that he had consulted. He described the geography and the vegetation of the country, the cities, as well as the various dress habits and appearance of the inhabitants.

In this paper, I will focus on the frontispiece of the book, which represents a triumphal arch for the European arts in China. The Chinese, in this image, honoured the arts of Europe, brought to them by the Jesuit missionaries. The European arts depicted are part of mixed mathematics: gnomonics, statics, ballistics, hydraulics, optics, mechanics, music and geography. Mixed mathematics was generally seen as the focus of the teaching of the Jesuits. According to the Western reception stories, the Chinese admitted that they were behind in these fields and were eager to learn. The angel who places a cross on top of the triumphal arch symbolises that the study of these arts will in the end lead them to the true God.

For the purposes of this paper, I am interested in the upper part of the image. There, the four stages of knowledge are shown, as an adaptation of an older iconographical and philosophical tradition. William Ashworth's 'Light of Reason, Light of Nature: Catholic and Protestant Metaphors of Scientific Knowledge', published in *Science in Context* (1989), is a classic paper on this tradition. This paper is also a milestone in the study of scientific images as well as in the study of the use of early modern light metaphors. Note, however, that Ashworth claimed that he had found all copies of frontispieces that depicted such light metaphors, especially in relation to the light of nature and the light of reason (p. 104). Unfortunately, he missed Wagner's book on China. This is a pity, because I think the uses of light metaphors in this frontispiece give a very different view than those studied by Ashworth.

Let us start with the tradition as expounded by Ashworth. In *Retracing the Arts to Theology,* Bonaventura explains that there are four kinds of knowledge, which are hierarchically ordered, from lower to higher, as follows: (1) the light of mechanical art (external light), which include weaving, armour-making, agriculture, hunting, navigation, medicine and dramatic art; (2) the light of sense perception (lower light); (3) the light of philosophical knowledge (inner light); (4) the light of sacred Scripture (higher light). These ideas are visualised in a modified form in the frontispiece of the Jesuit mathematician and astronomer Christoph Scheiner's *Rosa Ursina sive Sol* (1626-30). One of the modifications that Scheiner makes is to change Bonaventura's 'light of mechanical art' into the light of the profane authorities. Athanasius Kircher also depicts the four sources of knowledge on the frontispiece of his *Ars Magna Lucis et Umbrae* (1646) as light effects. Again, Kircher makes some interesting modifications to the tradition. It should be clear that these works are likely sources for Wagner's frontispiece. There are interesting visual and iconographical similarities. It is not an accident that Wagner also refers to these four stages of knowledge. Furthermore, of course, Kircher's magnum opus on China was a dominant source to Wagner, from which he copied many images.

In contrast to the Catholic tradition of the four stages of knowledge, Ashworth argues that Protestants used light metaphors in an entirely different way. According to him, Protestants typically distinguish only between two kinds of light: the light of nature and the light of grace. Ashworth traces this use of light metaphors back to Paracelsus and Paracelsianism, and sees as an alternative source Francis Bacon. It is therefore, especially with protestant authors interested in the occult, that we find extensive use of light metaphors. Ashworth gives the example of Oswald Croll, who opposed in the frontispiece of his *Basilica Chymica* (1609) the two light orbs of nature and of grace. Another example Ashworth gives is Johann Daniel Mylius's *Opus Medico-Chymicum* (1618) and Johann Kunckel's *Ars Vitraria Experimentalis* (1679). Although Wagner is a protestant clearly interested in the occult (cf. his dissertation topics from 1663), in contrast to the sources mentioned by Ashworth, he distinguishes four lights of knowledge, and he is iconographically much closer to the Jesuits.

The conclusions that Ashworth draws from his analysis is that for the Jesuits, reason is the highest faculty, while for the protestants, the senses (as the 'light of nature') have priority in generating knowledge. There are some problems with

Ashworth's interpretation, however. Firstly, for many early modern authors, the light of nature did not refer to the senses, but to reason. Even if in some cases it might have referred to knowledge by sense perception, reason and/or intellect would still be involved. In many cases, it would therefore be wrongheaded to see the light of nature in *opposition* to reason. This is an anachronistic imposition of the historiographical construct of rationalists versus empiricists. Furthermore, for current historiographical sensibilities, Ashworth's argument is still too much framed in terms of the 'Scientific Revolution' or the Weber thesis. Lastly, Wagner provides an interesting counterexample to Ashworth's conclusions. Ashworth admitted to some difficult examples, e.g. the work of Hevelius and Scilla, but did not let them challenge his thesis. Wagner, however, is an interesting hybrid case that gives a very different view on the tradition of light metaphors and on the religious meaning of optical instruments.

The four emblems of knowledge

Let us look again at Wagner's frontispiece. The frontispiece was engraved by Melchior Haffner, an engraver from Ulm who worked mainly in Augsburg between 1670 and 1690. This engraver worked in both Catholic and Protestant visual traditions. Haffner engraved the frontispiece of the German edition of Kircher's *Phonurgia*, for instance, as well as the image of Ferdinand Verbiest's observatory in Peking, and many maps and travel journals in Asia. He was also the engraver of the allegorical frontispiece of Johannes Hartmann Medico-Chymica and of many portraits of Wittenberg theologians. The frontispiece of *Das mächtige Kayser-Reich Sina* is not chiefly the responsibility of the engraver, however, because Wagner explains in detail the important elements of the frontispiece in his text.

In the upper part of the frontispiece, we see the four cartouches that represent the four stages of knowledge. In this case, however, Wagner applies these four stages to the epistemic and religious development of the Chinese. In the first two figures, at the left, a lamp is depicted. The iconography is very similar to the lamp of profane knowledge in Scheiner and Kircher, with a hand coming out of the clouds holding a similar type of lamp. The novelty in Wagner's adaptation of this tradition is that the first lamp is closed, and that only the second one is opened. From the first, closed lamp, only a few dispersed light rays appear from the airing holes of the chimney. The caption of this image reads 'Es brennt zwar, aber leuchtet nicht' (it burns indeed, but it does not give light).

Wagner explains the first image in his text (Vorbericht, xxx^2/r) as follows. God has lighted the 'light of nature' (or the 'light of natural knowledge') in everyone, including the Chinese. It is clear that for Wagner, this 'light of nature' refers to knowledge that can be acquired by our natural means in general, and this term was not meant to divide between sense knowledge and reason. Until now, this 'light of nature' burned, but it did not give any light for the Chinese, because God blinded their unbelieving faculties. Their 'natural light' was closed by a thousand superstitions and vain doctrines. The truth remained inaccessible for this natural light, their leaders guided them blindly, and this is symbolised by the closed lantern.

The second cartouche depicts the same lantern, but a second hand opens the lantern. This is God's hand. Now, a shining light comes forth, so the Chinese can start searching for the Lord, and it becomes possible for them to feel and find him. It was God's grace to open the Chinese for the true religion. Evidence for this would be the recent missions of the Jesuits that have been successful in converting the Chinese. In the text, Wagner summarises this stage as 'So leitet Warheit das Gesicht' (In this way, truth leads our vision), but in the image, the caption refers more explicitly to the light metaphor: 'So leuchtet Warheit das Gesicht' (In this way, truth enlightens our vision).

At the right upper corner of the frontispiece, the engraver Melchior Haffner visualised the next two stages of knowledge. This iconography is striking. The third stage of knowledge depicts a magic lantern that projects a geometrical pattern. At this time, the magic lantern was still relatively new and unknown to the general public. Wagner was part of a local context in which the magic lantern played a significant role. When the engineer and instrument maker Johann Franz Griendel von Ach arrived in Nürnberg in 1670, he advertised his skills, including his aptitude in building a new instrument that 'shows everything that one desires'. There were regular demonstrations of Griendel's magic lantern in Nürnberg in the 1670s. Johann Christoph Sturm, who supervised Johann Christoph Wagner's studies in the 1660s, was fascinated by the magic lantern, and he demonstrated it in his lectures at the University of Altdorf in December 1672. He published a description and illustration of this 'curious recent invention' in his *Collegium experimentale* (Nürnberg, 1676, pp. 163-165). Wagner, who was still living in Nürenberg at this time, might well have been present at one of these occasions.

The instrument depicted in Sturm's text is a horizontal cylindrical lantern, a model introduced by Griendel. In contrast, the lantern on the frontispiece of Wagner's book is the traditional model with a vertical cylinder case. The design of the chimney and the case actually resemble most an illustration from Johannes Zahn's *Oculus artificialis teledioptricus sive telescopium* (1685). Zahn, who considered himself a pupil of Griendel as far as practical optics was concerned, described many magic lantern models in this book. The illustration in question can be found at the lower left corner on page 236 of Zahn's 1685 edition (p. 398 in the 1702 edition) and bears the caption 'Abscondita prodit et auget'. There is also a striking visual resemblance between the four emblematic shields with captions, in which Wagner depicted his light metaphors as optical instruments, and the four circular images with optical instruments, also with captions, set apart in Zahn's illustration.

Wagner calls the magic lantern the "Optical Lantern", after Griendel and Kohlhans, or also the "Art-Lantern". For him, this lantern symbolises the arts, and in particular the mathematical arts and sciences. Wagner writes that the Chinese are led to the knowledge of the true God by means of the mathematical arts and sciences, because these artful reflections will lead them from the created to the creator. This is what the magic lantern emblem, which bears the caption 'Die Kunst vergrössert dann das Liecht', shows. Wagner explains it in his *Vorbericht* as follows. A lens magnifies the light rays, like the arts magnify the natural light of reason. In the emblem, these light rays shine on a half circle, from which lines go to the centre, where a triangle is depicted. We can conjecture that this symbolises the following: the study of the wonders of nature leads to the origin of all things, i.e. God (the triangle stands for the trinity). In this way, Wagner propounds an interesting strand of natural theology, in which contemplation if the wonders of nature will lead us (and the Chinese) to knowledge of the true God. This is also the message of the frontispiece in general, with its depiction of all these mathematical arts and sciences, and the suggestion that they will lead the Chinese to the only true religion.

The fourth cartouche depicted at the upper part of the frontispiece is identified in the text as a camera obscura with a lens. Again, the iconography of this so called 'camera obscura' is striking. There is no dark room with a pinhole that might constitute a camera obscura. Rather, the whole scene is covered in darkness and it is unclear what the light source is. One might speculate that the image of the crucified Christ that stands on its head could be a light source of its own (a classical iconographic device, cf. Piero della Francesca's *Flagellation*), but there is no hint of such an interpretation in Wagner's text. Furthermore, the lens is far too big for a pinhole camera, even if we allow for some artistic exaggeration, as it has the same size as the represented crucifixion. This image seems to be inspired by several different images from Zahn's *Oculus artificialis.* The basic idea of the camera obscura that inverts a cross seems to be inspired by figure XVIII in Zahn, vol I, p. 160. Here we see an abstract representation of the basic elements of a camera obscura (object, pinhole and projection on a sheet) presented in front of a realistic landscape with a building, bridge and tree. The idea of the lens that reduces or magnifies a cross might be taken from an abstract representation of a cross that is inverted and reduced by a lens in Zahn, vol. II, p. 55. In Wagner's frontispiece, however, the cross is enlarged. Finally, the form of the lens in Wagner's cartouche looks very much like the lens represented by Zahn, vol. III, p. 238.

While the magic lantern represented the mathematical arts and sciences that the Jesuits introduced in China, the camera obscura stands for their religious teachings. Before, the Chinese despised Christianity, and Christ was only an executed convict. Now, thanks to the Jesuits, this perversity had been turned around. This process is symbolised by the camera obscura, according to Wagner, because this instrument turns around the cross, which was inverted, by means of an optical lens, and restores it. The caption of this cartouche reads 'und wird das Heil so auffgericht', 'and salvation (Christ) is erected in this way' (in the text, this variation is given: 'So wird das Heil auch aufgericht'). The inverted cross that is turned around by the camera obscura comes to represent the turned beliefs of the Chinese that are set right by the Jesuits.

Conclusion

The four cartouches on the frontispiece of Johann Christoph Wagner's *Das mächtige Kayser-Reich Sina*, depicting optical instruments, represent four stages of knowledge. The first stage is the natural light, represented by a closed lantern. The second stage is grace, which opens the natural light for the Truth, and is represented by a lantern that is opened by a divine hand. The third stage is the arts and sciences, which magnify the natural light. In this way, the knowledge of nature will lead the way to God. This stage, of natural theology, is represented by a magic lantern which shines on a geometrical figure. The fourth stage of knowledge is revealed theology, in principle embodied in scripture, but in this case the stress is more on the dissemination by Jesuit religious education. It is only this stage which will lead the Chinese to knowledge of the true religion.

These four stages are represented by the captions, which are reproduced with slight variations in the introduction of the book, and which represent a short poem. These four stages of knowledge are hierarchically ordered, as in the tradition of light metaphors that represent the four sources of knowledge. These optical instruments now also represent progressive stages in time, however. Providence and grace are described as a historical process, and these four stages depict historical events: the progress of the Jesuit missions in China and the conversion of the Chinese.

On the one hand, the four stages of knowledge are an iconographic depiction of the circulation of knowledge between Europe and China by the Jesuits. The Jesuits' central goals were education and missions. These are represented in stages three (magic lantern) and four (camera obscura) respectively. Just like the magic lantern and the camera obscura are optical instruments for the transmission of light, the Jesuits are God's instruments for the transmission of the true faith. On the other hand, the iconographic depiction on Wagner's frontispiece is also the result of this circulation: it is a reception story of the Jesuits' activities in China and their self-presentation towards the European public. Wagner's book represents a remarkably polished representation of what the Jesuits were doing in China. It is strikingly unpolemical, given the context in which the book appeared (Counter Reformation, Chinese rites controversy).

The frontispiece represents an interesting mix of Jesuit and protestant iconography. On the one hand, Wagner distinguishes four stages or sources of knowledge, in accordance with the Jesuit tradition. Also the visual imagery resembles Jesuit sources, and many illustrations in the book are directly taken from Athanasius Kircher's work. On the other hand, the first two stages represent one form of the 'Protestant' distinction between the light of nature and the light of grace. Furthermore, stage three can be interpreted as a natural theology, which was becoming very popular in protestant circles. The important role of the mathematical sciences, instead of observation and natural history, is again due to the Jesuit influence. Also, to avoid controversy, Wagner remains unclear on how and in how far we can obtain knowledge of God by pursuing the mathematical sciences. The stage of the magic lantern seems therefore ecumenical, which can be accepted by all parties. Stage four is the role of revealed religion, and although this of course refers to scripture, Wagner singles out the Jesuit teachings in China as the decisive factor in the conversion of the Chinese.

It should be noted that Wagner is not fixated by an opposition between the light of nature and reason. We should not interpret the light of nature as Ashworth suggests, i.e. as observation and sense knowledge. For Wagner, the natural light includes reason. Furthermore, stage three stands in particular for the mathematical sciences, and this stage is represented only as a magnification of the natural light of stage one. Stage three and four are the 'artificial' enhancement –by means of 'instruments' and education– of stage one and two, respectively. Wagner's representation of the four stages of knowledge by means of optical instruments innovates in the classical tradition of light metaphors of knowledge, and it is a fascinating example of the religious meaning attributed to optical instruments.

Maybe the most striking feature of these images, however, is that they fundamentally misrepresent the actual functioning of these instruments. These representations are plainly wrong. Firstly, the magic lantern is an instrument that projects and enlarges the image painted on a slide on a wall. In Wagner's text, however, the function of the magic lantern is only to magnify the light, like a spotlight. In the emblem on the frontispiece too, one does not see the typical rectangle with slides in the tube. Furthermore, the symbolic meaning of the magic lantern would change significantly if its projecting function would be taken into account. For Wagner, the sciences are supposed only to *magnify* the light of nature or reason. They are supposed to throw light on nature as it is. He would not want to add the projection of a painted slide, the projection of the philosopher's or mathematician's own imagination, to come into the picture. This, however, is the traditional metaphoric meaning of a magic lantern: the projection of one's own deranged fancies on the world in contrast to sober and truthful observation.

Secondly, the camera obscura without a lens gives an inverted projection. If one adds a lens, this is usually to get an erect presentation of an object. In the text, it seems that the function of the lens is misunderstood. Furthermore, the image in the frontispiece does not seem to be the image of a camera obscura. As I already argued, the image seems to be a compilation of different images from Zahn's book on optical instruments. If the illustration represents a camera obscura, the aperture is far too large, it should not contain a lens, and the projected image should be located in a dark room. Finally, it remains a mystery what the light source in this image might be –except for the inverted cross itself.

Is seems that both Wagner and the engraver of the frontispiece were not well informed about the technical aspects and working of optical instruments. These artefacts are not interesting in themselves to him. They only serve a representational function, and a way to extend the classical tradition of light metaphors for the sources of knowledge. Even if Wagner did not understand the details of optics, he wanted to expand on the iconographical and textual traditions of religious and epistemic meanings that were attributed to mathematical instruments. The religious meaning these instruments assumed in the seventeenth century is difficult to trace today, because the material artefacts do not speak and the textual traces are scarce. Nevertheless, these iconographical clues give us a glimpse of a very different religious world these mathematical instruments were part of.

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THE JESUIT HONORÉ FABRI AND THE THEORY OF PROJECTILES

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Abstract

The Jesuit Honoré Fabri (1608-1688) was a senior representative of Jesuit scientists during the period between Galileo's death (1642) and Newton's Principia mathematica *(1687). As I have shown in a paper published in 2008, Fabri managed to integrate into his impetus-based physics (and general Aristotelian framework) the principle of Linear Conservation of Motion, generally (and inaccurately) referred to as "inertia". Furthermore, Fabri also accepted Galileo's law of falling bodies, as long as perceptible times and spaces are concerned; he indeed formulated a different rule for "infinitesimal" moments, but he took pains to (successfully) show that for measurable spatial or temporal units his own law converged to Galileo's rule, i.e. the "odd numbers law". However, while thus accepting two key concepts of classical (or Pre-Classical) physics, Fabri flatly rejected Galileo's analysis of projectiles and dismissed the Pisan parabola as the solution for the projectile's trajectory; instead, Fabri employed an Aristotelian-flavoured principle –that nothing exists "in vain" (*frustra*)– and developed a different curve, which albeit being totally erroneous was closer to the trajectory actually observed than Galileo's parabola. My lecture will thus explore this unique case of scientific-theory dissemination, in which a member of the Society of Jesus reveals himself as keen on assimilating important "New Science" insights, but in his own terms: preserving an Aristotelian (or Neo-Aristotelian) spirit that demands theory to stay as close to observed facts, and cannot accept a mathematical abstraction which does not correspond to observed projectiles (i.e. the parabola, which as we know today is different from observed projectiles because of air resistance).*

The French Jesuit Honoré Fabri was born in 1608 and between 1640 and 1646 taught natural philosophy and mathematics in the Jesuit *Collège de la Trinité* at Lyon. This period is considered the most fruitful and brilliant in his career, and his writings were quite successful even outside the Jesuit order. In 1646 Fabri's career as a Jesuit College lecturer came abruptly to an end, when he was removed from teaching and transferred to a bureaucratic post in Rome. According to contemporary sources, Fabri was at odds with his Jesuit colleagues and superiors, who did not welcome his fondness for novelties (Lukens 1979, pp. 10-16; Heilbron 1979, p. 113). The removal of audacious Jesuits from their teaching posts was a common measure against those charged with introducing dangerously new ideas into the Jesuit classroom (Feingold 2003, p. 31); Fabri was indeed introducing Cartesian and Galilean principles "through the back door", so to say, while using the old and "legitimate" (so-to-speak) concept of impetus. However, as we shall see today, such assimilation of novel ideas did not imply unconditional assent. Fabri was deeply influenced by Galileo's and Descartes' inertial (or pre-inertial) thinking, and was not afraid to harshly attack Aristotle on this subject (a fact which must have irritated his superiors),[1] but he nevertheless strongly rejected (like most contemporaries) Galileo's analysis of projectiles, including the parabolic trajectories.

Fabri's physical system was centered around impetus, a concept introduced into scholastic physics in the 14th century, whose main purpose was to modify Aristotle's account of projectile motion: while Aristotle resorted to the medium as the factor explaining the continuation of motion of a projected object, Jean Buridan and his followers rejected this explanation and opted for a *vis*, or *qualitas* called impetus, which is impressed in a body and causes it to move (Clagett 1959, pp. 534-537). Fabri, in line with this scholastic tradition, defined impetus as a "quality" which causes motion in the subject in which it inheres, and like Buridan claimed that impetus is a "permanent" quality, and thus able to endure. However, contrary to the scholastic tradition, Fabri integrated his belief in the innate permanence of impetus, together with the uncontested possibility of motion in vacuum and the exclusive linearity of impetus, to explicitly formulate the principle of Conservation of Linear Motion. The scholastic protagonists of impetus still followed in Aristotle's footsteps by rejecting the possibility of terrestrial motion devoid of any resistance, and by retaining Aristotle's common dicta: that violent motion is inherently brief, and thus can never be imagined to be eternal, and that straight motion – whether natural or violent – can never be, by definition, *ad infinitum*: it must have both a *terminus a quo* and a *terminus ad quem* (Damerow et al. 2004, p. 264; Baldini 2004, p. 135). Even the young Galileo, in his *De motu*, claimed (like many scholastics philosophers) that impetus is a self dissipating entity, thus avoiding the conclusion that violent motion in vacuum might be eternal.

Fabri started by transforming Buridan's "permanence" of impetus to a genuine principle of conservation, insisting that impetus is conserved as long as nothing causes its destruction (Fabri 1646b, pp. 68-69). Furthermore, Fabri was highly influenced by those whom we now regard as the pioneers of modern science. Contrary to Buridan's impetus, which can be directed, either linearly or circularly, Fabri adopted from Giovanni Benedetti an exclusive linear impetus, and stated that circular motion is nothing but an impeded straight motion, which explains why a stone released from a sling tends to move along a straight line (Fabri 1646b, "Synopsis amplior", para. 1).

Fabri utilizes these two principles –the conservation of motion and the exclusive linearity of motion– in his analysis of motion in the void, which appears in an appendix to his book *Metaphysica demonstrativa* (Fabri 1648), an appendix entitled *De vacuo*. In this analysis Fabri exhibits a deep Galilean influence, and he is no doubt also inspired by Descartes and Gassendi. Fabri not only accepts there the possibility of motion in the void, but explicitly determines that violent motion in vacuum must be eternal, linear and with constant velocity. In order to illustrate this fact, he suggests some kind of a "thought experiment", resulting in what we would call inertial motion:

> If, while a stone moves upwards with a decelerated violent motion, everything else were to be destroyed, it would then never stop, but would move [upwards] with constant motion, namely retaining the same level of velocity which it had at that very instant in which the rest of the universe was destroyed.[2] (Fabri 1648, pp. 563-564)

Fabri now mentions Aristotle's four famous objections to the possibility of vacuum –presented, in *Physics* book IV, as so called "paradoxes" which allegedly result from the assumption of void. According to one of these objections, the assumption of void entails zero resistance, and therefore infinite velocity, and thus absurd instantaneous motion; according to a second objection, the same assumption would entail the ridiculous conclusion that bodies differing in weight and shape would move in the same velocity. Fabri flatly rejects these objections by showing that rather than consisting "absurdities" they accurately depict the behavior of bodies existing in vacuum; Fabri explicitly relies on Galileo's *Discorsi*, and adopts the proportion Galileo formulated there, which indeed allows for motion in zero resistance and determines that bodies fall exactly in the same rate in vacuum, no matter their weight or shape (Elazar 2008, pp. 15-19). Aristotle's last objection claims that if we permitted vacuum, then a body once moved would have no reason to stop moving (that is, an eternal violent motion would result; *Physics* [Aristotle 1930], 215a19). Fabri's reply to this objection is straightforward and decisive:

> I concede, of my own accord, that in a total void a stone once moved, would move forever, unless it is restrained extrinsically.[3] (Fabri 1648, p. 564)

Fabri regards the motion of a horizontal projectile as a "mixed motion" resulting from two impetuses: "natural impetus" directed downwards, caused by gravity, and an impressed horizontal impetus. In line with linear conservation of motion, he speculates what would happen to the projectile if it were weightless:

> If natural impetus did not participate in this motion, the projectile would obviously move along a straight line with constant motion.[4] (Fabri 1646b, p. 162)

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[1] This issue is discussed in detail in Elazar 2008.

[2] "Si tamen dum lapis movetur sursum, motu violento retardato caetera omnia destruerentur, nunquam deinde sisteret, sed moveretur motu aequabili, retento scilicet eo gradu velocitatis, quem habebat eo instanti, quo reliquum orbis destructum est".

[3] "Concedo ultro fore, ut in totali vacuo lapis semel motus, semper moveatur, nisi ab extrinseco retineatur".

As already Alexandre Koyré noted, Galileo himself was never able to totally abstract an object from its weight, and only in the writings of his disciples can we find comparable statements, namely unequivocal announcements of linear conservation of motion (Koyré 1978, pp. 240-241). Furthermore, Fabri adopted Descartes' opinion concerning a projectile, that contrary to those who Descartes called the *docti* (namely, scholastic philosophers), the phenomenon which must be explained is rather the decay of its motion, not its continuity (Koyré 1968, pp. 72-73). However, the *answer* that Fabri gave to the question "why does a projectile stop moving", is entirely non-classical: it consists of a peculiar method of "patching up" again natural impetus in a non-inertial way that reveals strong Aristotelian (or scholastic) restrains. It is well known that Galileo assumed "superposition", i.e. the mutual independence of the two components, and easily proved that the inevitable outcome of compounding constantly accelerated fall with constant horizontal velocity would be a simple parabola. Fabri rejected Galileo's elegant solution, and replaced it with an original explanation which abolishes superposition, and assumes that these two components are not mutually independent. The explanation Fabri suggests derives from Aristotle's dictum that nothing in nature exists "in vain" (*frustra*), and as I will show, it serves Fabri to qualitatively define a curve which is similar to the parabola, but not identical with it, and describes the real motion of a projectile (so Fabri claims) somewhat better than Galileo's parabola; essentially, the explanation which Fabri posited replaced the reason given by the Galileans (and by modern science) to the conspicuous difference between the parabola and actual projectiles: I mean of course air resistance.

Why does Fabri reject Galileo's parabola? Essentially, for the same reason that the parabola as a projectile's trajectory was very unpopular among Galileo's immediate contemporaries, especially practitioners: it did not fit observed projectiles, and the discrepancy became worse as the projectile was faster. In Fabri's words, even a child playing with a discus can see that in projectile motion the arc of descent is much smaller than the arc of ascent, contrary to the symmetric parabola (Fabri 1646b, p. 169). But still, Fabri's main argument against Galileo might strike the modern reader as odd. Adopting Galileo's parabola, Fabri explains, would violate what he defines as "hypothesis 3". This is hypothesis 3:

> A horizontal projectile close to the end of its motion strikes less hard than at the beginning, indeed also a projectile thrown downwards at an angle. This hypothesis has been proved a hundred times, and cannot be cast in doubt.[5] (Fabri 1646b, p. 154)

Now, a "hypothesis" –according to Fabri– is not a tentative assumption, open to subsequent confirmation or rejection; rather, it is a qualitative statement drawn from sensible experience which is considered by Fabri entirely valid and suitable to serve as a premise for the subsequent deduction of theorems. According to Fabri, a "hypothesis" is always drawn from "the most certain experiments", and other examples for such hypotheses are "fire heats" and also "a heavy body falls with accelerated motion" (Fabri 1646a, pp. 86-90; Dear 1995, pp. 138-140).

Fabri's third hypothesis seems strange to the modern reader, who following Galileo's analysis knows that (under ideal conditions) even a horizontal projectile (let alone one thrown in a downward inclination) necessarily increases its velocity as it falls, and therefore strikes harder, not less hard, as the motion advances. However, it must be noted that the influence of air resistance is indeed strongly felt –even with relatively slow projectiles– so Fabri's hypothesis is less unreasonable than it might seem at first glance; it was actually rather common in the 1630's and 40's, and was expressed for example by Diego Ufano and Marin Mersenne, influenced by the Aristotelian dictum that violent motion always decreases with time. Fabri simply claims that adopting Galileo's solution –which entails a constant horizontal velocity and a constantly accelerating downward motion– would violate hypothesis 3 (Fabri 1646b, p. 160).

Here is the trajectory Fabri proposes for a horizontal projectile (see figure 43 inside figure 1). The lack of superposition is obvious in the gradual decrease of the horizontal distance acquired in consecutive instants (HI<GH<FG<EF). Furthermore, in order to make sure that hypothesis 3 is not violated, Fabri remarks that not only is the horizontal motion impeded, but also the acceleration downwards is not constant, but behaves like an inclined plane: it starts with zero, and gradually increases until it reaches the regular free fall acceleration (Fabri 1646b, p. 161). Accordingly, the perpendicular additions, PQ, RS and TV keep increasing with time. Fabri insists that this mutual inhibiting is necessary in order to prevent the resultant final motion from becoming higher than the initial velocity, thus violating hypothesis 3.

As we can see, the horizontal component of the projectile is gradually diminished; in his analysis of oblique projectiles (figures 47-49 inside figure 2), Fabri provides an explanation for the way in which this diminution occurs, based on the Aristotelian dictum that nature does nothing in vain. Look at the parallelogram in figure 48; it depicts oblique projectiles, but the mechanism is the same for horizontal projectiles. AD is the initial violent impetus, and AB is what Fabri calls "innate impetus", namely gravity. In order to see what happens to the violent impetus, we need to compound these two impetuses. Now impetus, according to Fabri, is inherently a scalar magnitude, so these vectors are supposed to add up to the scalar sum, AB + AD: this would be the case if they were in the same direction. However, according to the old parallelogram rule (from the pseudo-Aristotelian *Mechanical Problems*), the actual sum of these two impetus is the diagonal AC. Fabri therefore

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[4] "Si impetus naturalis non concurreret ad hunc motum, proiectum moveretur per lineam horizontalem rectam, ut constat, motu aequabili".

[5] "Proiectum per horizontalem sub finem motus minus ferit quam initio, imo & proiectum per inclinatam deorsum; haec hypothesis centies probata fuit; nec in dubium revocari potest".

claims that the difference between the scalar sum and this diagonal is in vain, *frustra*, so this is the amount to be subtracted from the initial violent impetus: AB = DN in figure 48, so AN = AD + AB (the scalar sum); AC = AE, so EN = (AD+BC) – AC; FD = EN, which is the amount *frustra*, so AF (i.e. the violent impetus in the next iteration) = AD – FD (Fabri 1646b, p. 168). In the descending part of the motion the natural component – Fabri explains – is not constant but gradually increasing, therefore the overall traversed horizontal distance will be smaller than in the ascending part.

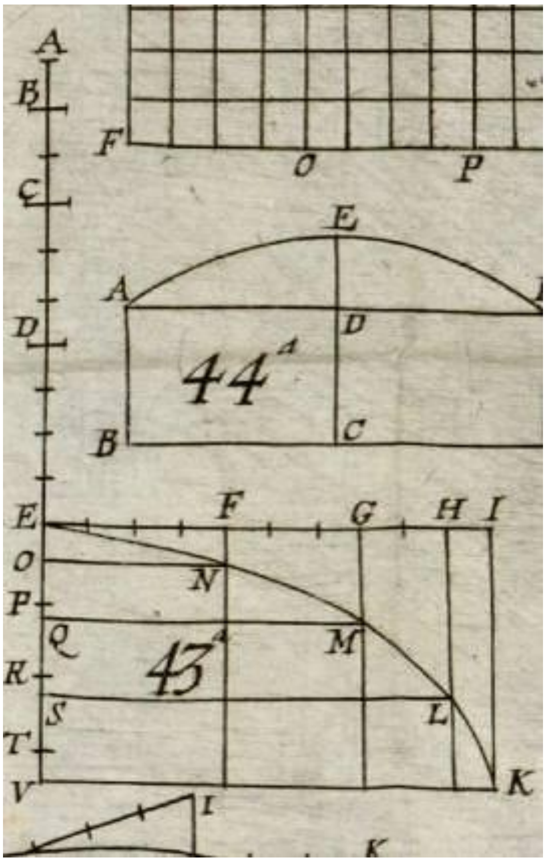

Fig. 1: Fabri's trajectory for a horizontal projectile
[File Fabri-figure-1.jpg; taken from Fabri 1646b]

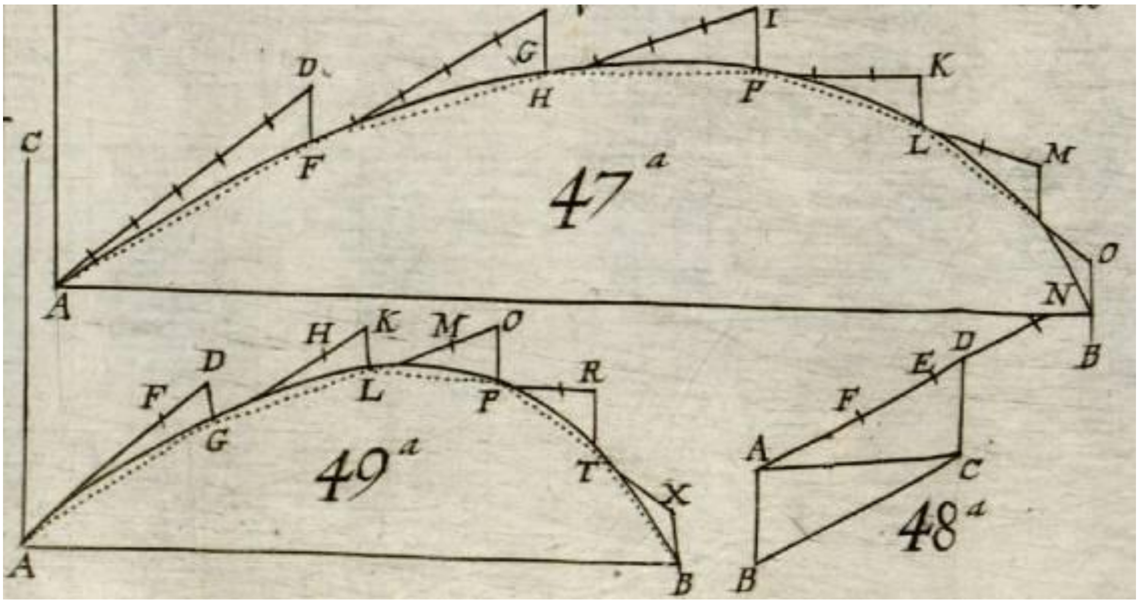

Fig. 2: Fabri's analysis of an oblique projectile
[File Fabri-figure-2.jpg; taken from Fabri 1646b]

Fabri rejects the Galilean explanation that it is air resistance which is responsible for the difference between the theoretical parabola and the observed trajectory, claiming (wrongly) that we hardly see any effect on the downward natural motion, while the violent motion is indeed affected; therefore, it cannot be the case –claims Fabri– that air resistance causes the projectile's retardation (Fabri 1646b, p. 160).

Fabri's rejection of Galileo's parabola is perhaps better understood in light of his general conception concerning the manner in which physics should be pursued. Here's Fabri's opinion on Galileo's achievements, pertaining especially to his theory of falling bodies, but also relevant to projectiles:

Indeed the Great Galileo, who before any one else wonderfully and with almost divine sharpness of genius led local motion to where no mortal had led it before (*Quidem prae caeteris magnus ille Galileus, qui mirifica, & fere divina ingeni iacie, motum localem eo perduxit, quo mortalium nemo perduxerat*); nevertheless because he omitted many things which relate to motion, as everyone knows, and did not prove those marvellous effects from physical principles, but only assented to some proportions from geometrical [principles]; in order to have regard for physics, we undertake another way: we do consult geometry, to explain and set forth those aforementioned proportions which belong to motions; but we reduce the effects connected to those proportions to physical principles; in other words, while we suppose what there is (*quod sint*), we prove why it is (*propter quid sint*). (Fabri 1646b, p. 5).

Elsewhere, in a letter to Mersenne, Fabri states that physics must employ mathematics, and stresses that its purpose is to reduce "sensible effects" (that is, effects perceived by the senses) to their causes; this time he does not attack the New Physics, but rather standard scholastic physics, deeming it a discipline "full of disputes", worthy of being called only "elementary metaphysics" or "superior grammar" (Tannery et al. 1945-1988, vol. XII, p. 276). Galileo, in other words, indeed merits praise for amply "consulting geometry", but he made the mistake of treating the geometric figure of a parabola –which does not depict an actual projectile– as a "sensible effect". Fabri, therefore, loyal to the new principles he adopted from the New Science (thus opting for an analysis in a vacuum, and clearly asserting Linear Conservation of motion), exhibits also loyalty to the Aristotelian ideal of *scientia*, which must involve a causal explanation of observable phenomena, namely reducing natural effects to their causes. Fabri is clearly trying to limit the scope of physics to "sensible", i.e. observable effects, and thus avoid Galileo's way of pursuing physics, which was conceived by Fabri and his contemporaries as nothing more than abstract mathematics, which does not pertain to real ("sensible") nature.

References

ARISTOTLE. (1930). *Physics*, tr. R. P. Hardie and R. K. Gaye. In *The Works of Aristotle*, 12 vols, ed. W. D. Ross and J. A. Smith. Oxford: Oxford University Press.

BALDINI, Ugo. (2004). Ontology and Mechanics in Jesuit Scholasticism: The Case of Gabriel Vasquez. In *Scientiae et Artes: Die Vermittlung alten und neuen wissens in Literatur*, *Kunst und Music*, ed. Barbara Mahlmann-Bauer, 99-142. Wiesbaden: Harrassowitz in Kommission.

CLAGETT, Marshall. (1959). *Science of Mechanics in the Middle Ages*. Madison: University of Wisconsin Press.

DAMEROW, Peter, Gideon FREUDENTHAL, Peter MCLLAUGHLIN and Jürgen RENN. (2004). *Exploring the Limits of Preclassical Mechanics*. 2nd ed. New York: Springer.

DEAR, Peter. (1995). *Discipline & Experience: The Mathematical Way in the Scientific Revolution*. Chicago: University of Chicago Press.

ELAZAR, Michael. (2008). Honoré Fabri and the Trojan Horse of Inertia. *Science in Context* 21(1):1-38.

FABRI, Honoré. (1646a). *Philosophiae tomus primus, qui complectitur scientiarum methodum sex libris explicatam, logicam analyticam, duodecim libris demonstratam, & aliquot controversias logicas, breviter disputatas; auctore Petro Mosnerio,.. Cuncta excerpta ex praelectionibus R.P. Hon. Fabry*. Lyon.

FABRI, Honoré. (1646b). *Tractatus physicus de motu locali, in quo effectus omnes, qui ad impetum, motum naturalem, violentum, & mixtum pertinent, explicantur, & ex principiis physicis demonstrantur; auctore Petro Mousnerio Doctore Medico; cuncta excerpta ex praelectionibus R. P. Honorati Fabry, Societatis Iesu*. Lyon.

FABRI, Honoré. (1648). *Metaphysica demonstrativa, sive scientia rationum universalium; auctore Petro Mousnerio Doctore Medico; cuncta excerpta ex praelectionibus R. P. Hon. Fabry soc. Iesu*. Lyon.

FEINGOLD, Mordechai. (2003). Savants. In *Jesuit Science and the Republic of Letters*, ed. M. Feingold, 1-46. Cambridge, Mass.: MIT Press.

HEILBRON, John L. (1979). *Electricity in the 17th & 18th Centuries: A Study of Early Modern Physics*. Berkeley: University of California Press.

KOYRÉ, Alexandre. (1968). *Newtonian Studies*. Chicago: University of Chicago Press.

KOYRÉ, Alexandre. (1978). *Galileo Studies*, tr. John Mepham. Hassocks: The Harvester Press.

LUKENS, David C. (1979). *An Aristotelian Response to Galileo: Honoré Fabri, S.J. (1608-1688) on the Causal Analysis of Motion*. Ph.D. Thesis, University of Toronto.

TANNERY, Paul, Cornélis DE WAARD and Bernard ROCHOT (eds.). (1945-1988). *Correspondance du P. Marin Mersenne religieux Minime*, 17 vols. Paris: G. Beauchesne.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 344.

THE FIRST PROTESTANT MISSIONARIES AS EUROPEAN NATURALISTS IN INDIA –COMPETITORS OF THE JESUITS IN THE 18TH CENTURY

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Abstract

At the beginning of the 18th century (1706) the first protestant missionaries were sent to South India with the aid of a contract between the 'Waisenhaus'-foundation in Halle (Saale) and the King of Denmark. They had different contacts with Jesuits, the religious competitors but also supporters of activities with the same aim. Priests of both groups, trained at European universities, and in particular the protestant missionaries supported by missionary physicians sometimes, were scholars in many fields. They published Tamil translations of Christian religious texts and comparative textbooks on linguistics.

In one field the first protestant missionaries in India surpassed the Jesuits: Some of them became specialists in natural history and made extensive collections of Indian natural objects, which activities remained nearly unnoticed by the history of science until our days. The 18th century missionaries in India remained in correspondence with eminent European naturalists, who highly estimated the natural objects of the overseas regions, sending recent European publications on natural history to India, in order to support the self-training of the scholars, who trained also their disciples in Natural History in the overseas country. In the counter-move these sent their collections and observations to European partners who became sponsors for the missionary work. The historian states many ways of knowledge exchange between Europe and India from the 18th to 19th centuries and find traces of the spread of Indian natural history collections and manuscripts through whole Europe. In this way, these activities furthered much the knowledge of Indian civilization and the development of natural history in Europe from the 18th to the 19th centuries, during decades before Indian countries became members of the British Empire.

First steps of Jesuits into India

This paper will focus on missionary activities which remained neglected by the history of science. At first it remembers some well-known events of the beginning of Christian missions at the former East-Indian continent. After the fleet of Vasco da Gama (c. 1469–1524) had reached the West coast of India in 1498, the first catholic priests came together with the traders from Portugal to Goa. An intense colonial mission was initiated by the Jesuit Francisco de Xavier (1506–1552) since his arrival in India in 1542. He even brought the inquisition to Goa, which worked there from 1554 to 1774. This example was followed by several Portuguese Jesuits. They tried to convert people of the Middle Class in favour of Catholicism with the aid of privileges, but many 'rice Christians', not deeply convinced adherents, hated finally the Christian priests, trying to change Hindu religious practices such as burning the widows. Another problem arose during the 17th century, after the Jesuit Roberto de Nobili (1577–1656, in India since 1606) achieved to establish new mission stations in internal regions of South India, since he initiated an adaptation to the Hindu civilization. He hoped to diminish the European image of Christian religion and to win people of the lower casts with the help of Brahmans, but this strategy led to conflicts between missionaries and the Pope; thus, the 'controversy of rites' burdened the missionary activities, lasting until 1744. Nevertheless, many Jesuits worked in South India. One eminent representative was the Italian Costanzo Giuseppe Beschi, S. J., (1680–1747) who arrived in India in 1710. He followed the missionary objectives as a learned theologian and an eminent linguistic expert in Sanskrit, Tamil, and Telugu. He published grammars and a dictionary of the vernacular languages and wrote religious poetry, e.g. the poem *Têmbâvani*.[1]

Religious, linguistic, and literary activities of the first protestant mission founded in India

During the lifetime of this Jesuit similar aims, such as founding new parishes and Christian churches, educating converted persons to new priests and catechists, learning the regional vernacular languages, and writing religious texts in these languages, were followed by the first Protestant mission established in 1706 in India. It started its work at the East coast of South India at Tranquebar (now Tara(n)gambadi) in Tamil Nadu at the Coromandel coast.[2] This Danish trade place was situated in the colony of the only European Lutheran King, the pious Frederik IV (1671–1730). Since Danish pastors were not disposed to that mission, he appointed to work for the 'Danish-Halle-Mission-Society' German theologians trained by the 'Waisenhaus' (Orphan-House) in Halle (Saale) and ordained in Copenhagen, shortly before leaving Europe. Thus, the religious basis of that mission society was a group of strictly pious Protestants called the Pietists. They propagated the biblical fundament of Christianity and its practical realization by means of social works of charity and teaching in a world-wide perspective which they called "*Pietas universalis*".[3]

The first protestant missionaries, Bartholomaeus Ziegenbalg (1682–1719) and Heinrich Pluetschau (1677–1752, returned to Germany in 1711) arrived in 1706 and were followed by three colleagues in 1709, among others Johann Ernst Gruendler (1677–1720). They had to surmount many difficulties during the first years in India. Since their missionary objectives were much different from those of colonial politics, the trade authorities entertained distrust against the activities of charity including poor people of lower casts for inspiring trust to the Europeans. Nevertheless, after eight years the young priest Ziegenbalg achieved to finish the conflicts.[4] He attained toleration of the missionary activities for the next decades; thus, a very fruitful work was continued with the help of many followers.[5]

The priests were supported in the field of medical care by an indigenous medical man since 1712. Since he knew the health problems in the tropical region and the regional therapeutics and cures, his practice was relatively successful, during

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[1] MUNDADAN, Anthony Mathias [et al.] (eds.), *History of Christianity in India*, vols. 1–2. Bangalore: Church History Association of India, 1988–1989.
O'MALLEY, John W. (ed.), *The Jesuits: Cultures, Sciences, and Arts, 1540–1773*. Toronto [et al.]: University of Toronto Press, 1999, repr. 2000.
HARRIS, Steven J., Jesuit Scientific Activity in the Overseas Missions, 1540–1773, in: *Isis* 96, 2005, 71–79.
LEDERLE, Julia, *Mission und Oekonomie der Jesuiten in Indien (Studien zur außereuropaeischen Christentumsgeschichte, 9)*. Wiesbaden: Harrassowitz, 2009.

[2] LEHMANN, Arno, *Es begann in Tranquebar*, 2nd edition. Berlin: Evangelische Verlagsanstalt, 1956.
Halbfass, Wilhelm, *India and Europe: an Essay in Understanding*. New York: State University of New York Press, 1988.
GRAFE, Hugald (ed.), *The history of Christianity in Tamilnadu from 1800 to 1975 (History of Christianity in India, vol. 3)*. Bangalore: Church History Association of India, 1990.
LIEBAU, Heike (ed.), *Geliebtes Europa / Ostindische Welt. 300 Jahre interkultureller Dialog im Spiegel der Daenisch-Halleschen Mission (Kataloge der Franckeschen Stiftungen, 16)*. Halle (Saale): Franckesche Stiftungen, 2006.
GROSS, Andreas [et al.] (eds.), *Halle and the Beginning of Protestant Christianity in India*, vols. 1–3. Halle (Saale): Franckesche Stiftungen, 2006.

[3] BRECHT, Martin and Lehmann, Hartmut (eds.), *Geschichte des Pietismus*, vols. 1–2. Goettingen: Vandenhoeck & Ruprecht, 1993–1995.
RAABE, Paul et al. (eds.), *Pietas Hallensis Universalis (Kataloge der Franckeschen Stiftungen, 2)*. Halle (Saale): Franckesche Stiftungen, 1995.
LEHMANN, Hartmut (ed.), *Glaubenswelt und Lebenswelten (Geschichte des Pietismus, 4)*. Goettingen: Vandenhoeck & Ruprecht, 2004.

[4] NØRGAARD, Anders, Mission und Obrigkeit. Die Daenisch-hallische Mission in Tranquebar 1706–1845. Guetersloh: Verlagshaus Mohn 1988.

[5] JEYARAJ, Daniel, Inkulturation in Tranquebar: der Beitrag der fruehen daenisch-halleschen Mission zum Werden einer indisch-einheimischen Kirche (1706–1730). Erlangen: Verlag der Ev.-Luth. Mission, 1996.

the first years. Thereupon, for the health care of the increasing parish, other Europeans and indigenous people, a European physician was sent to the mission from the 1730s on. In 1732 Samuel Benjamin Cnoll (1705–1767), trained in Halle, launched into a successful medical practice, while he applied the European healing methods, and established a chemical laboratory where he produced the pharmaceuticals himself. This practitioner much engaged in empirical work did not write long medical treatises.[6] However, the manuscript *Der Malabarische Medicus*, a translation of a Tamil medical book, finished by the missionary J. E. Gruendler in 1711 and sent to Halle, reported on the indigenous medical practices, the principles and therapeutics. The missionary intended to let publish the experiences unknown to the Europeans as a document of the foreign civilization.[7]

Similar literary products on diverse subjects accompanied the mission work. Shortly after the arrival in India the missionaries began to study the native languages of the region, mainly with the help of an indigenous teacher. For communicating with other Europeans and also for teaching in the mission schools, later on, they used the Portuguese language. In Tamil, Telugu, and Sanskrit they produced translations of the basic religious texts –often using earlier writings produced by Jesuits– such as the Lord's Prayer, chapters of the Gospel (a first edition was finished by Ziegenbalg and Gruendler in 1714; a second edition was published by Ziegenbalg and Benjamin Schultze (1689–1760) in 1723), and hymns (Gruendler's 48 hymns in 1714). Moreover, the learned missionaries wrote several types of dictionaries and grammars, sometimes comparing expressions of different languages;[8] such writings should be helpful for training the future religious teachers, disciples of different types of schools established since 1707,[9] and also for European scholars. Such useful learned linguistic studies, supplemented by poetry, were continued by several missionaries until the 19th century, for example by B. Schultze, Christoph Theodosius Walther (1699–1741), and August Friedrich Caemmerer (1767–1837), the last 'Danish-Halle' missionary.[10] They furthered linguistics in Europe and stimulated studies of literature and native languages in India itself.

While the contrast between the Catholic Church personified by the Jesuits and the Lutheran confession produced the competition between both sides, the same scholarly interests became the basis for a kind of collaboration. Missionaries of both confessions compared their religious writings, in order to learn from one another, how to translate principal theological notions, such as 'God', 'heaven', 'Saviour'. The Protestants even helped the Jesuits to resolve a practical problem. They realized soon that the best way to spread their ideas was not only by preaching, but also by supporting it with the aid of printed booklets given to the people. Remembering the successful manufacture of book production at the Orphan-House in Halle, they established such a manufacture at the mission station.[11] This very rare equipment in former India missed the Jesuits at that time. Therefore, the learned brother C. G. Beschi asked the Protestants to print his new Latin-Tamil grammar in Tranquebar (1738). That was realized, but not without including some corrections annotated by the missionary C. Th. Walther.[12]

Protestant Missionaries as naturalists in India

In one field the Danish-Halle Missionaries surpassed the Jesuits: they contributed to the development of a scientific natural history of India in Early Modern Times; that area was unknown in Europe and carried out mainly with regard to useful applications in India during earlier times. They were active with respect to both, to gain knowledge on useful natural products and also to observe natural phenomena by collecting natural objects, unknown to European scientists. In contrast to the Jesuits they did not only pay attention by preference to natural products helpful as medicaments or useful as foodstuffs,[13] but based on the broad education in the Orphan-House in Halle, where disciples had been trained in some practical work and gardening, the Danish-Halle missionaries were interested also in natural phenomena and improved their knowledge by self-instruction with the aid of special books. In addition, they were stimulated by an external impulse, namely by the directors of the Orphan-House in Halle, demanding to send reports and documents of all kind of remarkable objects and phenomena different from European ones, but typical of the foreign nature and civilization. From the beginning on, the missionaries fulfilled this task, already starting on board of the ship, during the long shipping through the Cape of Good Hope. After having received the reports, the directors in Halle published them in the newly founded journal *Merckwuerdige Nachricht aus Ost-*

[6] NEUMANN, Josef N., Medizinische Forschungen, in: LIEBAU, Heike (ed.) (note 2), 2006, pp. 180-193.

[7] NEUMANN, Josef N., Malabarischer Medicus – eine ethnomedizinisch-historische Quelle des fruehen 18. Jahrhunderts, in: LIEBAU, Heike [et al.] (ed.), *Mission und Forschung (Hallesche Forschungen, 29)*. Halle (Saale): Franckesche Stiftungen, 2010, pp. 195-203.

[8] JUERGENS, Hanco, Forschungen zu Sprachen und Religion, in: LIEBAU, Heike (ed.) (note 2), 2006, pp. 112, 115 squ., 119, 121-133.

[9] LIEBAU, Heike, Das Schulwesen, in: LIEBAU, Heike (ed.) (note 2), 2006, pp. 136-159.

[10] JUERGENS, Hanco (note 8), 2006, 123-132.

[11] LIEBAU, Kurt, Buchdruck und Missionsbibliothek, in: LIEBAU, Heike (ed.) (note 2), 2006, pp. 105-120.

[12] JUERGENS, Hanco (note 8), 2006, p. 129.

[13] HARRIS, Steven J. (note 1), 2005, p. 75.

Indien (entitled *Hallesche Berichte*, later on), since 1706. With the aid of that journal accounts on India were spread through Europe, immediately. One effect was that many European citizens wanted to support the mission work and spent money to the Orphan-House.[14] By continuing that procedure of fund raising, the mission could work in India with the support of sponsors until the beginning of the 19th century, when the Napoleonic Wars shocked Europe.[15] Another effect of the publications was the reaction of interested scholars. Since trustworthy information from India sent by educated or even learned persons was extremely rare in Europe in the 18th century, it was much welcome by scholars. Some of them asked special questions directly to the missionaries.[16] Therefore, these intensified their efforts in the field of natural history, too. Several pastors became eminent specialists in this area. Some of their most important accomplishments, which are described in detail in my recent publications,[17] are to be appreciated by summarizing the details as follows.

The missionaries conveyed their observations by means of several ways: they sent reports continuously, often in the form of long letters, to the directors of the Orphan-House in Halle or to the superiors in Copenhagen or to learned personalities or personal friends in Europe; moreover, they communicated objects or special collections of natural objects, and manuscripts of treatises for publication to scholars in Europe. All kind of documents on meteorological, geographical, mineralogical, zoological, botanical, and, last but not least, ethnological phenomena are preserved now in archives, libraries, museums, and public collections spread through Europe. Many documents of correspondences, manuscripts, and sketches of objects besides some objects themselves are to be found in the archives of the Franckesche Stiftungen at Halle (Saale), of course. In addition, we found letters, manuscripts and in particular collections of herbarium plants from Copenhagen, Lund in Scandinavia, London and Liverpool in England, through Paris and the private Herbarium of E. v. Lindemann in 'Jelisabethgrad' (now Kirovograd in Russia), to several places in Germany, from Berlin, Goettingen, Jena to Munich (coming from the Herbarium of Johann Daniel Christian Schreber, 1739–1810, Professor at the University of Erlangen) and Regensburg.[18] Besides the herbarium parts in Berlin we have to mention the extraordinary special collection of preparations of Indian fishes in the Natural History Museum of Berlin, which was performed by Christoph Samuel John (1747–1813) and sent to the Berlin physician and zoologist Marcus Elieser Bloch (1723–1799), who included it in his publication (*Naturgeschichte der auslaendischen Fische*, 8 vols., 1785–1795).[19]

Some of the natural history observers excelled as research workers; they relied on a good school and university training in Germany, which they completed by means of self-instruction. As early as in the 1730s C. Th. Walther collected and compiled a herbarium of more than 700 Indian specimens, mainly spermatophytes (*spermatophyta*), by request of a physician and botanist at Hannover (Germany). Now, the sheets with pressed plants and plant parts are bound in 12 volumes entitled *Plantae Malabaricae*, forming a special collection of the actual Herbarium at the University of Goettingen (Germany).[20] Many handwritten labels of the specimens are kept; some original ones are palm leaf pieces with an ink inscription, showing the broad interest of Walther in the Indian civilization by mentioning the former Indian plant name in Tamil characters and Walther's Latin transcription; in addition, a paper label presents often pharmaceutical and economic applications in Latin and sometimes in German language.[21]

In the second half of the 18th century the scientific style of botanists switched from the pre-Linnaean methods to the rules of Linnaean plant research. That arrived relatively early in Tranquebar, since a former student of Linnaeus joined the mission. From 1768–1775 the physician and botanist Johann Gerhard Koenig (1728–1785) worked in the mission station before he was sent by European and Indian sponsors to research expeditions at different places in East Asia. Between his sojourns outside he visited the colleagues in Tranquebar several times, discussing with them botanical problems.[22] Those

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[14] LIEBAU, Heike, Die Halleschen Berichte, in: LIEBAU, Heike (ed.) (note 2), 2006, pp. 97-101.
GLEIXNER, Ulrike, Expansive Froemmigkeit. Das hallische Netzwerk der Indienmission im 18. Jahrhundert, in: LIEBAU, Heike [et al.] (eds.), *Mission und Forschung (Hallesche Forschungen, 29)*. Halle (Saale): Franckesche Stiftungen, 2010, pp. 57-66.

[15] NEHRING, Andreas, Das Erbe der Mission, in: LIEBAU, Heike (ed.) (note 2), 2006, pp. 196-207, see 196-198, cf. 216-217.

[16] GROSS, Andreas [et al.] (eds.) (note 2), vol. 3: Communication between India and Europe, 2006.

[17] HOPPE, Brigitte, Kulturaustausch zwischen Europa und Indien auf wissenschaftlicher Grundlage im fruehen pietistischen Missionswerk, in: HUELSENBERG, Dagmar (ed.), *Der Bologna-Prozess und Beitraege aus seinem Umfeld (Abhandlungen der Humboldt-Gesellschaft für Wissenschaft, Kunst und Bildung e. V., 23)*. Rossdorf: TZ-Verlag, 2009, pp. 133-173.
HOPPE, Brigitte, Von der Naturgeschichte zu den Naturwissenschaften – die Daenisch-Halleschen Missionare als Naturforscher in Indien vom 18. bis 19. Jahrhundert, in: LIEBAU, Heike [et al.] (eds.), *Mission und Forschung (Hallesche Forschungen, 29)*. Halle (Saale): Franckesche Stiftungen, 2010, pp. 141-166.

[18] HOPPE, Brigitte (note 17), 2010, pp. 162-164.

[19] LANDSBERG, Hannelore, Eine Fischsammlung aus Tranquebar, die Berliner Gesellschaft Naturforschender Freunde und deren Mitglied Marcus Elieser Bloch, in: LIEBAU, Heike [et al.] (eds.) (note 17), 2010, pp. 167-179.

[20] [WALTHER, Christoph Theodosius], *Plantae Malabaricae*, 12 vols., special collection with handwritten labels, now in the Herbarium at the University of Goettingen (Germany).

[21] HOPPE, Brigitte (note 17), 2010, pp. 151-157.

[22] HOPPE, Brigitte (note 17), 2010, pp. 153-158.

arose during the collecting and classifying studies which the missionaries of the central station in Tranquebar carried out in the style of teamwork, unifying two generations of missionaries and their native disciples, during the period from c. 1780 to c. 1810. That very intense research work, stimulated by physico-theological ideas of the European Enlightenment,[23] was developed and improved by Christoph Samuel John (1747–1813), living in Tranquebar from 1771 on, and Johann Peter Rottler (1749–1836), who worked 1776–1803 in Tranquebar and subsequently in Madras. The team of naturalists was completed by the younger missionaries Johann Gottfried Klein (1766–1821), since 1791 the mission physician, and Benjamin Heyne (written also Hayne, Heine, 1770–1819), who lived as a physician employed by an Anglican mission society 1793–1819 in India, working in particular as a botanist 1802–1808; after staying only a few years in Tranquebar, he remained in contact with the missionaries. The connections between these naturalists are to be inferred from remarks in the correspondence and from the handwritten labels of herbarium specimens, which show for example a text handwritten by Klein and corrected by Rottler.[24] The scholarly partners in Europe of the research workers in India were the most eminent naturalists of that time. Koenig corresponded with Linnaeus and sent longer manuscripts and many plant collections to Anders Johan Retzius (1742–1821), Professor of Botany at the University in Lund, who let publish several manuscripts of the travelling research worker. John and Rottler were in touch with the superiors in Copenhagen and with the naturalists of the *Gesellschaft Naturforschender Freunde zu Berlin*, contacting especially the botanist Carl Ludwig Willdenow (1765–1812), moreover, with the *Naturforschende Gesellschaft zu Jena* and the *Regensburger Botanische Gesellschaft*. After having published papers in the journals, edited by these learned societies, and having sent natural objects to their members, both missionaries were honoured by being appointed members. Similar personal honours attained J. G. Klein and B. Heyne, too. Moreover, the research results of these missionaries were included into many publications produced by specialists in Europe from the end of the 18th to the middle of the 19th century. In addition, their collections became the basis of the most important publications on the flora and fauna of India worked out by British naturalists from around 1800 on. The names of the missionary collectors are mentioned in the entries of many specimens in the works published by William Roxburgh (1751–1815) (*Flora Indica*, 3 vols., 1795–1832) and Robert Wight (1796–1872) in his *Contributions to the Botany in India* (1834).[25] Finally, the collections preserved in European and Indian (in Chennai) herbariums have a special value because of containing many types or syntypes of Indian species; therefore, they are much esteemed until our days.

Conclusions

The mission in India achieved its principal religious objectives, while it contributed to the foundation of a Christian Church in India. Although the European competition between the confessions was continued in the foreign country, Jesuits and Protestants practiced some collaboration in secular fields. The inhabitants of the subcontinent, where many people of different origin were assembled, had to learn some tolerance against different religions and cultural traditions, in order to mastering the daily life.

The methodical basis of all missionary activities, religious and secular ones, was constituted by networks. One network was established between the missionaries and some European organizations and scholars; it was maintained by correspondences and exchanges of manuscripts, books and other objects. Another one was founded by the missionaries and natives of India, such as scholars, theologians of other religions and confessions, the rulers, but also pariahs. Special tasks, in particular natural history research, were accomplished by working as a team. Missionaries worked together with their colleagues having a university education; their collaborators were indigenous students and disciples, and they learned from Indian informants.

Through the channel of networking the Protestant missionaries contributed to a circulation of knowledge between Europe and India from the 18th to the 19th centuries. They exchanged knowledge on languages, literature, and all areas of civilization. In the field of natural history they brought not only European scientific methods and knowledge to India, but they stimulated changes of the European scientific knowledge by including natural objects and phenomena, appearing in a tropical region, into the international science of Modern Times.

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23 HOMMEL, Karsten, »Fuer solche [Theologen] wolle Gott seine Ost-Indische Kirche in Gnaden bewahren!« Physikotheologie und Daenisch-Englisch-Hallesche Mission, in: LIEBAU, Heike [et al.] (eds.) (note 17), 2010, pp. 181-194.

24 Herbarium specimen, dated "Tranquebar, […], 1799" of the collection "Ex Herbarii Schreberi", now kept by the Herbarium of the Botanische Staatssammlung, Muenchen.

25 HOPPE, Brigitte (note 17), 2010, pp. 158-160.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 349.

"IN PARTE PHYSICAE THEORETICA NEWTONUM EIUSQUE COMMENTATORES SECUTUS SUM" –LEOPOLD GOTTLIEB BIWALD'S *PHYSICA GENERALIS* AS A COMPENDIUM PROPAGATING NEWTONIAN PHYSICS IN EUROPE

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Abstract

Leopold Gottlieb Biwald's physics textbook in two volumes, Physica Generalis *and* Physica Particularis *(Graz 1767/1768), was very important in the 18th century. Widespread in whole Europe and officially designated for use at the universities and lyceums throughout the Habsburg monarchy by an imperial decree of 1779, it played an important role as a very up-to-date compendium propagating Newtonian physics. In his textbook Biwald, Jesuit and Professor in Graz (Austria) for decades, does not only refer to elements of Newton's theory itself but also to several Newton-commentaries and uses various Newtonian textbooks. Biwald's principal sources on the subject are compendia written by 'sGravesande, Keill, Maclaurin, Mako, Musschenbroek, Pemberton, and Scherffer. In this paper I analyze the Physica Generalis into which the most important elements of Newton's theory found their way as an example of the propagation of Newtonian physics in Europe. Attention will be given to the following questions: Which sources are the most important for Biwald: Newton's* Principia*, Newton-commentaries or textbooks? Does Biwald focus on Jesuit sources when explaining Newtonian physics? Which is the geographical and cultural background of the authors of the Newtonian sources of the* Physica Generalis*? What does this aspect tell us about the (international) scientific (Jesuit) network in the 18th century and which new facets concerning the form of dissemination of the Newtonian theory in Europe result from that? In which aspects is Biwald's textbook outstanding in comparison to its Newtonian sources, what are the author's own specific achievements? Hence, how can the success of the* Physica Generalis *be judged in the pan-European context?*

Leopold Gottlieb Biwald and his achievements in physics

Today Leopold Gottlieb Biwald is primarily known for his physics textbook, which played an important role in 18th century physics teaching. Biwald was born on February 26th, 1731 in Vienna (Austria).[1] On October 17th, 1747 he entered the Society of Jesus[2] and studied in Raab, Trnava, Vienna, Ljubljana, and Graz. After his ordination to the priesthood Biwald was employed as a Professor of Logic and Metaphysics at the University of Graz in 1763. From 1764 onwards he officiated as a Professor of Physics,[3] additionally he taught Natural History at Graz from some time later on.[4] For two times –namely in 1786/1787 and in 1798/1799– Biwald was the principal of the Lyceum of Graz,[5] into which the University had been changed.[6] When Biwald was 74 years old, Emperor Franz I. awarded him a necklace with a gold medal, which was an honor especially for Biwald's merits during his 42 year long teaching activity at Graz.[7] Shortly afterwards Biwald died on September 8th, 1805 in Graz (Styria, Austria).[8]

Considering that Biwald was the first Professor of Graz who was ever awarded such a medal of honor and that a bust portraying him with this medal was dedicated to him,[9] one can say that he was eminently important for the University and Lyceum of Graz respectively in the 18th and early 19th centuries. When it comes to the question of the highest significance of Biwald's works, his physics textbook in two volumes, entitled *Physica Generalis* and *Physica Particularis*, plays the most important role. This compendium, written in Latin and published in Graz in 1767/1768,[10] was published several times in quick

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1 Constant von WURZBACH, s.v. Biwald, *Biographisches Lexikon des Kaiserthums Oesterreich, enthaltend die Lebensskizzen der denkwürdigen Personen, welche 1750 bis 1850 im Kaiserstaate und in seinen Kronländern gelebt haben*. Erster Theil, Wien: Verlag der Universitäts-Buchdruckerei von L. C. Zamarski 1856, 415.

2 *Bibliothèque de la Compagnie de Jesus. Bibliographie par les Pères Augustin et Aloys de Backer*. Nouvelle édition par Carlos SOMMERVOGEL, tom. 1, Bruxelles-Paris: Schepens & Picard 1890, s.v. Biwald, 1528.
WURZBACH, Biwald (cf. n. 1), 415.

3 Ladislaus LUKÁCS, *Catalogi Personarum et Officiorum Provinciae Austriae S.I.*, Roma: Institutum Historicum S.I. 1995, IX, 47, 111, 148, 237, 305, 372, 448, 517, 562, 632, 697, 768, 840; X, 14, 160, 232, 305, 378, 450, 522, 595; XI, 15, 85, 157, 229.

4 *Biographie des Herrn Leopold Gottlieb Biwald, der Weltweisheit und Gottesgelehrtheit Doctor, ehemaliges Mitglied des aufgelösten Jesuitenordens, ordentl. und öffentlicher Professor der Physik, Senior und Director der philosophischen Facultät, und gewesener Rector Magnificus an dem k. k. Lycäum zu Grätz*. Von Michael KUNITSCH, jubilirten Lehrer der k. k. Hauptnormalschule zu Grätz. Grätz 1808, gedruckt bey den Gebrüdern Tanzer, 23.

5 Franz von KRONES, *Geschichte der Karl Franzens-Universität in Graz. Festgabe zur Feier ihres dreihundertjährigen Bestandes*, Graz: Verlag der Karl Franzens-Universität 1886, 581.

6 This degradation of the University of Graz happened in 1782 (KRONES, *Geschichte der Karl Franzens-Universität in Graz* (cf. n. 5), 465).

7 De BACKER, De BACKER & SOMMERVOGEL, Biwald (cf. n. 2), 1528.
KUNITSCH, *Biographie des Herrn Leopold Gottlieb Biwald* (cf. n. 4), 23f.
WURZBACH, Biwald (cf. n. 1), 416.
Ausgezeichnete Belohnung des Leopold Biwald, Professors der Physik am k. k. Lycäum zu Grätz. Den 9ten des Brachmonaths 1805. Grätz, bey Alois Tusch Buchhändler.
For further information on Biwald's merits concerning physics teaching cf. Cornelia FAUSTMANN, The Roots of Modern Physics Teaching at the University of Graz – Leopold Gottlieb Biwald's Merits, *Proceedings of the First Joint European Symposium on the History of Physics: The Roots of Physics in Europe*, Pöllau, Austria, May 28–29, 2010 [forthcoming].

8 De BACKER, De BACKER & SOMMERVOGEL, Biwald (cf. n. 2), 1528.
KUNITSCH, *Biographie des Herrn Leopold Gottlieb Biwald* (cf. n. 4), 24.
WURZBACH, Biwald (cf. n. 1), 415.
Further biographical information on Biwald is presented in KUNITSCH, *Biographie des Herrn Leopold Gottlieb Biwald* (cf. n. 4), for the most concise summary of Biwald's career cf. Cornelia FAUSTMANN, Tradition und Fortschritt in der Astronomie des 18. Jahrhunderts. Eine Fallstudie am Beispiel von Leopold Gottlieb Biwalds Physica Generalis, in: *Multiple kulturelle Referenzen in der Habsburgermonarchie des 18. Jahrhunderts* (ed. by W. SCHMALE), Wien 2010 (Jahrbuch der Österreichischen Gesellschaft zur Erforschung des 18. Jahrhunderts 2009/24), Bochum: Verlag Dr. Dieter Winkler 2010, 316-318.

9 KUNITSCH, *Biographie des Herrn Leopold Gottlieb Biwald* (cf. n. 4), 35.
WURZBACH, Biwald (cf. n. 1), 416.
Cf. also Cod. 157 of the Library of the University of Graz.
Cf. *Ausgezeichnete Belohnung des Leopold Biwald* (cf. n. 7).

10 In the following, quotations are taken from the more influencial second edition of Biwald's textbook:
Physica Generalis, quam auditorum philosophiae usibus accomodavit Leopoldus Biwald e Societate Iesu, Physicae in Universitate Graecensi Professor Publicus. et Ordinarius. Editio Secunda, ab authore recognita. Cum speciali privilegio S. C. R. Maiestatis. Graecii, Sumptibus Iosephi Mauritii Lechner, Bibliopolae Academici. Typis Haeredum Widmanstadii, 1769.
Physica Particularis, quam auditorum philosophiae usibus accomodavit Leopoldus Biwald e Societate Iesu, Physicae in Universitate Graecensi Professor Publicus, et Ordinarius. Editio secunda, ab authore recognita. Cum speciali privilegio S. C. R. Maiestatis. Graecii, Sumptibus Iosephi Mauritii Lechner, Bibliopolae Academici. 1769.

succession, widespread in whole Europe,[11] and officially designated for use at the universities and lyceums throughout the Habsburg monarchy by an imperial decree of September 1779.[12] Biwald's textbook is especially characterized by an adequate composition of the material, by a presentation of the information in a didactically well deliberate manner, and by a very clear phrasing.[13]

Newtonian Physics in the *Physica Generalis* and Biwald's Newtonian sources

Regarding the reception of Newtonian Physics in the *Physica Generalis* there are a lot of chapters explaining Newtonian findings. This is not astonishing as Newton's authority in physics was beyond dispute at that time and as a major part of the *Physica Generalis* is devoted to classical mechanics (according to the usual practice of 18th century physics textbooks). So Biwald explains rectilinear and curvilinear motion, equilibrium of forces, collisions of bodies, gravitation, and celestial mechanics on the basis of the Newtonian theory.[14]

Apart from Newton's *Philosophiae Naturalis Principia Mathematica* Biwald also uses many other Newtonian sources in order to gain a preferably concise description in the *Physica Generalis*. He mentions several of those texts in the praefatio at the beginning of the *Physica Generalis*: so –except of the *Principia* – the commentaries on Newton written by 'sGravesande, Keill, Maclaurin, Musschenbroek, Pemberton, Sigorgne, and Paulian.[15] Additionally, Biwald uses further sources which he does not mention in the praefatio or even not throughout the whole textbook. As far as Newtonian Physics is concerned, two physics textbooks have to be listed: Paul Mako de Kerck-Gede's *Compendiaria Physicae Institutio* and Karl Scherffer's *Institutiones Physicae*.[16]

When the geographical circumstances of Biwald's Newtonian sources are considered, we see that most of them were published in Great Britain, namely Newton's *Principia*, of course, as well as Keill's, Maclaurin's, and Pemberton's

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[11] KUNITSCH, *Biographie des Herrn Leopold Gottlieb Biwald* (cf. n. 4), 14f.

[12] *Verwendung des Physiklehrbuchs des P. Biwald, 1779*. (Steiermärkisches Landesarchiv K. 216, Fasz. 62 bzw. Archive of the University of Vienna CA 1.2.206 [formerly: Kons.Akt. Fasz. I/2, Reg. Nr. 205]).

[13] Concerning these aspects cf. especially Cornelia FAUSTMANN, *Physik des 18. Jahrhunderts im Spiegel der Quellen. Komparatistische Studien und Quellenanalysen zu Leopold Gottlieb Biwalds* Physica Generalis, Diss. Wien 2010.

[14] For a further discussion of the contents of the *Physica Generalis* and the *Physica Particularis* cf. Cornelia FAUSTMANN, *Das astronomische Begriffssystem in Leopold Gottlieb Biwalds Physica Generalis*, Sitzungsberichte der Österreichischen Akademie der Wissenschaften 2009, math.-nat. Klasse, Wien 2011 [forthcoming].

[15] Isaac NEWTON, *Philosophiae Naturalis Principia Mathematica. Auctore Isaaco Newtono*, Eq. Aurato. Perpetuis Commentariis illustrata, communi studio PP. Thomae Le Seur & Francisci Jacquier Ex Gallicanâ Minimorum Familiâ, Matheseos Professorum. Tomus Primus – Tomus Tertius Genevae, Typis Barrillot & Filii Bibliop. & Typogr. MDCCXXXIX – MDCCXLII.
Willem Jacob 'SGRAVESANDE, *Physices Elementa Mathematica, experimentis confirmata. Sive Introductio ad Philosophiam Newtonianam. Auctore Gulielmo Jacobo 'sGravesande*. Tomus Primus – Tomus Secundus. Editio Tertia duplo auctior. Leidae. Apud Johannem Arnoldum Langerak, Johannem et Hermannum Verbeek. Bibliop. MDCCXLII.
John KEILL, *Joannis Keill, M. D. Regiae Soc. Lond. Socii, In Acad. Oxon. Astronomiae Professoris Saviliani Introductiones Ad Veram Physicam Et Veram Astronomiam. Quibus accedunt Trigonometria. De Viribus Centralibus De Legibus Attractionis*. Mediolani, Excudit Franciscus Agnelli anno MDCCXLII. Publica auctoritate, ac privilegio.
Colin MACLAURIN, *Colini Mac-Laurini Expositio Philosophiae Newtonianae in Latinum conversa a Gregorio Falck Soc. Jesu, et Regiae Celsitudini Joseph I Archiducis Austriae, Principis Haereditarii dicata cum sub serenissimis ejusdem auspiciis Stephanus Oliverius S. R. Imp. Comes de Wallis Ex Philosophicis, Historicis, & Mathematicis Disciplinis in Collegio Regio Theresiano publicum tentamen subiret*. Anno M DCC LXI. [vacat] Mense Viennae Austriae, Typis Joannis Thomae Trattner, Caesar. Reg. Apost. Majest. Aulae Typogr. et Bibliop.
Pieter van MUSSCHENBROEK, *Introductio Ad Philosophiam Naturalem auctore Petro van Musschenbroek*. Tomus I – Tomus II. Lugduni Batavorum, apud Sam. et Joh. Luchtmans, MDCCLXII.
Henry PEMBERTON, *A View of Sir Isaac Newton's Philosophy. By Dr. Pemberton. With a Poem on Sir Isaac. By Mr. Glover*. Illustrated with Copper Plates. Dublin: Printed for William Williamson, Bookseller, at Mecaenas's-Head in Bride-street, MDCCLVIII.
Paul SIGORGNE, *Institutions Newtoniennes, ou introduction a la philosophie de M. Newton. Par M. Sigorgne, de la Maison & Société de Sorbonne, Professeur de Philosophie en l'Université de Paris*. A Paris, Chez Jacques-François Quillau, fils, Libraire, rue saint Jacques, vis-à-vis celle des Mathurins, aux Armes de l'Université. MDCCXLVII. Avec Approbation & Privilege du Roi.
Aimé-Henry PAULIAN, *Dictionnaire de Physique, dédié à Monseigneur Le Duc de Berry. Par le P. Aimé-Henri Paulian Prêtre de la Compagnie de Jesus, Professeur de Physique au Collège d'Avignon*. Tome Premier – Tome Troisiéme. A Avignon, Chez Louis Chambeau, Imprimeur-Libraire, près les RR. PP. Jésuites. M. DCC. LXI.

[16] Paul MAKO DE KERCK-GEDE, *Compendiaria Physicae Institutio quam in usum auditorum philosophiae elucubratus est P. Mako e S. I.* Pars I. Vindobonae, Typis Ioannis Thomae Trattner, Caes. Reg. Aulae Typogr. et Bibliop. MDCCLXII.
Karl SCHERFFER, *Institutionum Physicae Pars Prima, seu Physica Generalis, conscripta in usum tironum philosophiae a Carolo Scherffer e S. J.* Editio Altera. Vindobonae, Typis Joannis Thomae Trattner, Caes. Reg. Maj. Aulae Typogr. et Bibliop. MDCCLXIII.
Karl SCHERFFER, *Institutionum Physicae Pars Secunda, seu Physica Particularis, conscripta in usum tironum philosophiae a Carolo Scherffer e S. J.* Editio Altera Vindobonae, Typis Joannis Thomae Trattner, Caes. Reg. Majest. Aulae Typogr. Et Bibliop. MDCCLXIII.

books. The main importance of commentaries on Newton published in Great Britain does not seem very astonishing as Newton's *Principia* were also published in Great Britain. However, there are also important works explaining Newton's theory which originated in other countries – in this connection especially the Netherlands have to be mentioned with 'sGravesande's and Musschenbroek's publications. Furthermore, two of the mentioned sources published in France have to be considered – namely Paulian's *Dictionnaire de Physique* and Sigorgne's *Institutions Newtoniennes*. Last but not least, further sources used by Biwald were published in his own country, Austria – regarding the reception and propagation of Newtonian Physics especially Mako's and Scherffer's publications have to be mentioned. Biwald's use of Mako's and Scherffer's textbooks is absolutely not surprising because both of them were Jesuits, contemporaries of him, and published their works quite shortly before the *Physica Generalis* in Vienna, not far away from Biwald's workplace in Graz. So the geographical distribution of these sources of the *Physica Generalis* shows a well working (Jesuit) scientific network in the 18th century. Within Europe there did not seem to exist any barriers which could constrain the reception of Newtonian Physics respectively of certain commentaries on Newton. On the contrary, when considering the dates of publication of Biwald's sources, it becomes clear that their reception happened proportionally quickly.

Within the scope of analyzing these sources the question of the languages also has to be taken into account. Six of the texts presented above are written in Latin (Newton, 'sGravesande, Keill, Mako, Musschenbroek, Scherffer), one Latin translation of an English publication is available (Maclaurin), one further publication is originally written in English (Pemberton), and finally Biwald uses two French sources when explaining Newtonian Physics in the *Physica Generalis* (Paulian, Sigorgne).[17] The predominance of Latin texts as Biwald's sources is not surprising as his compendium is also written in Latin. A more substantial question in this connection would be why Biwald used the Latin language for his textbook, the more so as vernacular languages experienced a significant upturn at that time. This issue can be answered by the context in which the *Physica Generalis* has to be classified – namely the Jesuit educational field, in which the use of the Latin language still was absolutely common.[18]

Another important aspect in this regard is the fact that Biwald did not only stick to Jesuit sources. Of course, there are certain obvious tendencies – such as the great significance of Jesuit physics textbooks and other Jesuit publications, e.g. Boscovich's *Theoria Philosophiae Naturalis*,[19] for Biwald –but we also have to note that Biwald equally uses non-Jesuit commentaries on Newton to a large extent– namely most of the sources quoted above originate from a non-Jesuit context (these are 'sGravesande, Keill, Maclaurin, Musschenbroek, Pemberton, Sigorgne, and Paulian).[20] So we can say that there are no confessional limitations as far as intertextual relations between the sources are concerned. Generally speaking, also when it comes to terms of content, Biwald presents the information in the *Physica Generalis* in an objective scientific way, which can be regarded as a modern method likewise from today's point of view.

The importance of different types of sources for the *Physica Generalis*

For analyzing the importance of the different types of sources for Biwald's textbook firstly a good impression is given by the total number of the authors and scientists mentioned in the *Physica Generalis*: Newton absolutely predominates with 58 quotations, followed by Boscovich with 38 quotations. Afterwards the Cassini family and Lalande (especially important for Biwald's astronomy) follow with 20 quotations each. Then Lacaille succeeds with 14 quotations. However, the most important Newton commentary for Biwald's *Physica Generalis* follows afterwards with 11 quotations (cf. Figure 1).

When considering the total number of literal quotations in the *Physica Generalis*, we get another impression: In this connection too, Boscovich plays an eminent important role, as Biwald quotes him 23 times. But the second place is held by Musschenbroek with 20 quotations. Newton only follows after Lacaille (6 quotations) with 5 quotations and after Kepler (4 quotations) the next Newton commentary –Maclaurin– follows with 3 quotations (cf. Figure 2).

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[17] Publications characterized by a mere astronomical focus (such as Lalande's works) are excluded from the considerations in this connection because of their specific target.

[18] For a more extensive analysis of this question (including further bibliographical references) cf. Cornelia FAUSTMANN, Zur Bedeutung des Lateinischen als Wissenschaftssprache im 18. Jahrhundert. Ein Beitrag anhand der Physica Generalis des Leopold Gottlieb Biwald, S. J., in: *Sprachen der Wissenschaften 1600-1850* (ed. by D. Ulbrich), 2010 (Themenheft Jahrbuch für Europäische Wissenschaftskultur) [forthcoming].

[19] *Theoria Philosophiae Naturalis redacta ad unicam legem virium in natura existentium, auctore P. Rogerio Josepho Boscovich Societatis Jesu, nunc ab ipso perpolita, et aucta, Ac a plurimis praecedentium editionum mendis expurgata*. Editio Veneta Prima ipso auctore praesente, et corrigente. Venetiis, MDCCLXIII. Ex Typographia Remondiniana. Superiorum permissu, ac privilegio.

[20] If there are certain reasons for the predominance of non-Jesuit commentaries on Newton quoted by Biwald as sources for his explications of Newtonian physics, cannot be answered in this connection.

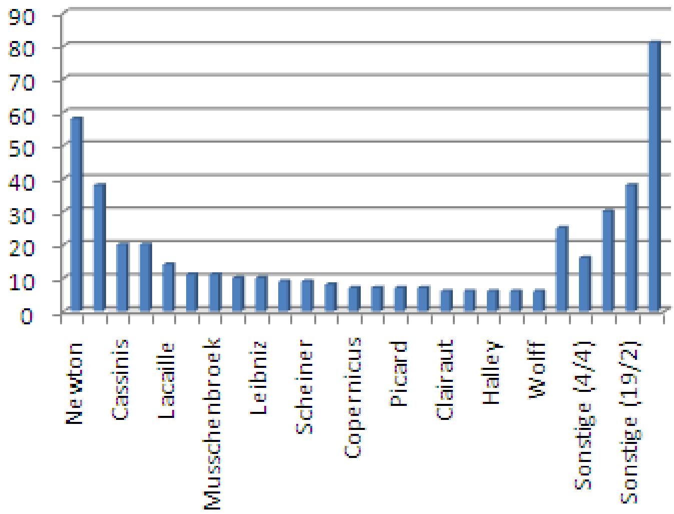

Fig. 1: Number of scientists mentioned in Biwald's *Physica Generalis*.[21]

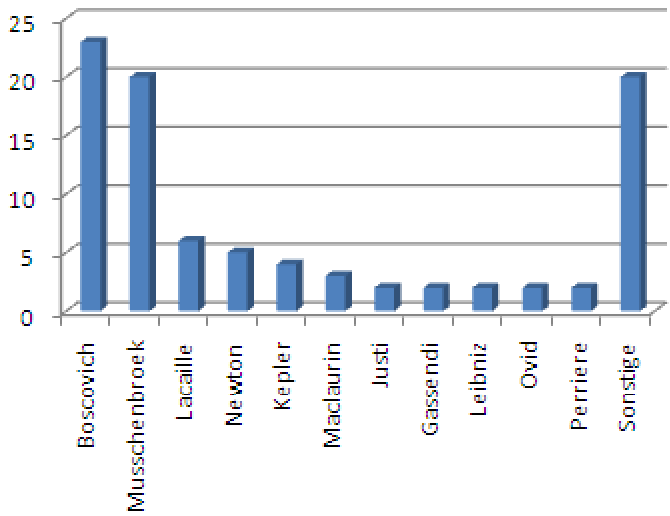

Fig. 2: Number of literally quoted scientists/authors in the *Physica Generalis*.[22]

The number of literal quotations of different scientists respectively authors can only be considered an indicator for Biwald's very near orientation on certain sources, therefore the whole text of the *Physica Generalis* has to be analyzed in order to approve or refute the impression received by the quotations. As far as the importance of Newton's *Principia* respectively commentaries on Newton as sources for Biwald is concerned, the results of the examination of the quotations in the *Physica Generalis* can be verified for the whole textbook – so Biwald uses commentaries on Newton, above all the publication written by Musschenbroek, more likely than Newton's *Principia* themselves.[23] And yet the importance of Newton himself as the most prominent scientist besides Boscovich in the *Physica Generalis* is clearly given because of the numerous remarks about him throughout Biwald's textbook.

[21] FAUSTMANN, *Physik des 18. Jahrhunderts im Spiegel der Quellen* (cf. n. 13), 237.

[22] FAUSTMANN, *Physik des 18. Jahrhunderts im Spiegel der Quellen* (cf. n. 13), 237.

[23] FAUSTMANN, *Physik des 18. Jahrhunderts im Spiegel der Quellen* (cf. n. 13), 237.

However, additional other sources have to be taken into account when analyzing intertextual approaches of this kind in the *Physica Generalis* – namely physics textbooks, which Biwald does hardly ever or even never quote in his compendium. When considering the wording of Newton's today very prominent axioms of motion as example and comparing their wording in the edition of the *Principia* used by Biwald and the wording in the *Physica Generalis*, there are apparent differences.[24] However, if the wording of Newton's axioms in Karl Scherffer's physics textbook *Institutiones Physicae* is taken into account, it becomes clear that Biwald copied Scherffer's wording – because it is exactly the same as in the *Physica Generalis*.[25] So this and many other examples show that Biwald's reception of Newton is carried out via certain physics textbooks and commentaries of Newton to a large extent, which thoroughly is an adequate method for a physics textbook of the 18th century like Biwald's *Physica Generalis*. However, Biwald undoubtedly was expert enough in Newtonian Physics – this aspect is shown in the author's selection and combination of different relevant source texts in order to create very well understandable explications as well as in deviations from source texts and totally independently written passages, which also is a quality criterion of the *Physica Generalis*.[26]

Conclusion

Considered as a whole, it is clear that Biwald could stick to a lot of 18th century sources. Within the scope of the analyses above it became manifest that commentaries on Newton's *Principia* and Newtonian textbooks are more important as sources for the *Physica Generalis* than the *Principia*. Biwald does not focus on Jesuit sources when explaining Newtonian physics, so there were no confessional boundaries in this connection in the 18th century. Additionally, the geographical distribution of the texts used by Biwald shows a very well working Jesuit network at that time, which is also illustrated by the fact that the reception of the sources happened proportionally quickly. As far as Biwald's specific achievements regarding intertextuality are concerned, the adequate selection of sources, the combination respectively compilation of different texts in order to make the descriptions as understandable as possible, and deviations from the methods of explications of the sources as well as independently written passages are the most important aspects. Additionally, Biwald's very clear style and an especially for his target audience useful structure of the material have to be seen as criteria for the success of Biwald's compendium at its time in whole Europe.

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[24] BIWALD, *Physica Generalis* (cf. n. 10), pp. 98f: LEX 1. Corpus omne perſeverat in ſtatu quietis, vel motus uniformis in directum, niſi quando ab aliis cauſis ſtatum mutare cogitur. LEX 2. Mutatio motus proportionalis eſt vi impreſſae, & fit ſecundum lineam rectam, qua vis illa imprimitur. LEX 3. Actioni contraria ſemper, & aequalis eſt reactio.
NEWTON, *Principia* (cf. n. 15), tom. 1, pp. 20f and p. 23: LEX I. Corpus omne perſeverare in ſtatu suo quiescendi vel movendi uniformiter in directum, niſi quâtenus à viribus impreſſis cogitur ſtatum illum mutare. LEX II. Mutationem motus proportionalem eſſe vi motrici impreſſæ, & fieri ſecundùm lineam rectam quâ vis illa imprimitur. LEX III. Actioni contrariam ſemper & æqualem eſſe reactionem: ſive corporum duorum actiones in ſe mutuo ſemper eſſe æquales & in partes contrarias dirigi.
FAUSTMANN, *Physik des 18. Jahrhunderts im Spiegel der Quellen* (cf. n. 13), 42.

[25] SCHERFFER, *Institutionum Physicae Pars Prima* (cf. n. 16), p. 103.
Cornelia FAUSTMANN, „Ignorato motu naturam ignorari necesse est." – Zur Mechanik in Leopold Gottlieb Biwalds Werk. Bericht zum Vortrag beim Driburger Kreis 2009 an der Medizinischen Hochschule Hannover, *Nachrichtenblatt der DGGMNT* 2009.
FAUSTMANN, *Physik des 18. Jahrhunderts im Spiegel der Quellen* (cf. n. 13), 43.

[26] FAUSTMANN, *Physik des 18. Jahrhunderts im Spiegel der Quellen* (cf. n. 13), 235.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 355.

HALLERSTEIN AND GRUBER'S SCIENTIFIC HERITAGE

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Abstract

Augustin Hallerstein's (1703 Ljubljana; † 1774 Beijing) role in the Beijing Jesuits' electrical and vacuum research was not that clear as his publications in astronomic, cartographic, Aurora Borealis, or China demographic research. The Jesuits were not always useful in all aspects but they were also not evil or mean all the time. The same goes for Hallerstein's younger Slovenian compatriot Gabrijel Gruber's (* 1740; † 1805) Jesuits in Russia who brought the western education, technology, and science to Petersburg. The Jesuits tried to sell their superior scientific and technological know-how to their hosts as the keys unlocking hosts' hart and most of all their souls. They especially favored the souls of domestic high society including the Russian or Chinese Emperor because the Jesuits hoped to repeat the Roman Emperor Constantine's example. General Gruber eventually nearly won Tsar Paul's soul for the Catholics, but the Chinese Jesuits were never that close. Hallerstein happened to be among the first to figure out that all tools will never be enough to conquer the Tsar's soul for Christianity, and therefore somewhat disappointed Hallerstein devoted most of his strength to the science on both opposite ends of Euro-Asia with numerous European Publications. The science Hallerstein offered to the Chinese was not always up-to-date because the Lisbon-Canton ship connection returned his mail only once in two years, which is far the from modern internet capabilities. Hallerstein was extremely proud of his observatory, and didn't hide it from the visitors. Even the military oriented experiments, which would be today considered as top-secret, were freely published in European scientific journals before French Revolution, and there was no reason for the Jesuits to hide the European knowledge from the Chinese. With the Chinese Jesuits still on board, the Chinese technological gap would not broaden so deep as it did after Hallerstein's death. With Gruber's Jesuits still on board, the Russian educational system could have produced even more pro-Western literati with* Decabrists' *souls to fulfill Peter the Great's goal of Westernization.*

Introduction

The best known Slovenian Jesuits, Augustin Hallerstein and Gabrijel Gruber, accomplished their scientific-political success in the greatest Eastern metropolis, Beijing and Saint Petersburg. Edo-Tokyo was beyond their reach in those times. They followed the Mattheo Ricci's plan: amaze the domestic nobles with superior Western science&technology to win their souls for Catholicism. The science&technology was their tool and the political-theological success their goal. Hallerstein soon figured out that his goal, namely the conversion of the Emperor, is beyond any real expectations. He decided to cultivate just the first part of the plan, the sciences. His younger compatriot, Gruber, had stronger political position in Saint Petersburg and never abandoned the second part of the plan, even if his Tsar Paul the 1st was killed just in the time Tsar was about to became the Catholic. While the early Ljubljana Gruber's work was dedicated to education&technology, his later Russian life was predominantly occupied with politics including the attempts to incorporate Hallerstein's Chinese successors into new Jesuit Society. Hallerstein never reached that second part of his projected achievements because of the unfavorable political climate in Beijing Court, and researched in sciences with the most of his strength. Therefore the history of science&technology values Hallerstein's work higher although the young engineer Gruber expertly dug water canals or designed buildings in engineering manner highly above Hallerstein's range. On the other hand Gruber published just some magnetic measurements besides his optical and hydro-mechanical work, while pure science dominated all Hallerstein's life. Hallerstein and Gruber expressed extraordinary mathematical-scientific, linguistic, and diplomatic-political abilities. While Beijing circumstances enabled Hallerstein to fulfill only the first group of his talents, the Tsar Paul 1st politics favored the last group of Gruber's capabilities.

Hallerstein's Electricity

Hallerstein was able to study Pascal,[1] Schott,[2] Newton, Lana Terzi, Musschenbroek,[3] or Laurentio Gobart's (* 1656 Liége; † 1750 Liége) 1716 and 1746 vacuum experiments in his uncle Franc Mihael Baron Erberg's (* 1679; † 1760) library or in Franciscan Ljubljana Library. He continued to use the same books in the Beijing Jesuit libraries. The magnetism seems to be Hallerstein's first love. He measured the declinations of the magnetic needle while sailing to China. Later, he made similar observations in Beijing,[4] where Nicola Cabeo's (* 1586; † 1650) *Philosophia magnetica*[5] and Ferdinand Verbiest's magnetic theory still prevailed over the more modern William Gilbert's followers,[6] although already Schreck's collaborator Nicolas Trigaut (* 1577; † 1628) acquired Gilbert's *De magnete* (1600) for his Macao and after 1623 Beijing Library.[7] It is also possible to determine the books, which other Far East missionaries used, but no bookplate bears Hallerstein's name[8] because he preferred to use the bookplate of his Beijing College. Modern catalogues probably do not show all Beijing missionary books because some of them were probably lost or borrowed.[9] Before he put anchor in Canton in September 1738, Hallerstein studied the electricity research of Stephen Gray (1729), Charles François de Cisternay Du Fay (1732), and Du Fay's student Abbé Jean Antoine Nollet.

In 1750, the Jesuits of Hallerstein's Portuguese college of St. Joseph in Beijing received the electrical machine and the instrument for the observation of the eclipses. Sanchez[10] provided the instruments from his friends in London and

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[1] Pascal's *Provincials* (Verhaeren, 1969, 150), in first French edition or more probably in later Latin translations.

[2] Verhaeren, 1969, 803-805.

[3] Verhaeren, 1969, 669-670.

[4] Amiot, October 2, 1784 *Mémoires*, 11: 563; Pfister, 1934, 760.

[5] The Beijing copy had no bookplate but both copies of Cabeo's 1646 book had bookplates of the Chinese Jesuits Francisco Pereira (* 1607 Lisbon; † China) and Martin Martini (* 1614; † 1661 China): *Applicado a Missao da China ou ao Collegio de Macao Fr. Francisco Pereira. Coimbra 1656* and *Ad usum p. Martini. – aplicados a Igreja de Hâm Cheu* (Verhaeren, 1969, 336; Pfister, 1932, 220, 256).

[6] Guan, 2005, 143.

[7] Bookplate *Bibl. Trig.* (Verhaeren, 1969, 494-495). Schreck and Trigaut took with them to the books with bookplate *Bibl. Trig.*: Giovanni Botero's (* 1540; † 1617) 1607 book bound with F. Barocci's 1572 item, S. Velli's 1573 publication, G. Mei's 1602 book, and S. Beham's publication of 1605 (Verhaeren, 1969, 939 (No. 3209)), Kepler's 1611 publication containing Galileo's letter was bound with Galileo's 1612 book (Verhaeren, 1969, 558). Trigaut also had Andriaen van Scrieck's (* 1560) 1614 publication with author dedication: *Bibl. Trig. – Rdo in Christo Patri ac D. Patri Nicolao Trigautio, Rectori, et Patruibus Collegij Socs. JESU in Sina, sacris Orientis sideribus: Auctor. D.D* (Verhaeren, 1969, 1194-1195), Levinus Hulsius' 1603-1605 book bound with J. Faulhaber two items published in 1610, Benjamin Bramer's (* 1588; † after 1648) 1616 book, Georg Brentel's 1615 publication, and adorned the book with bookplate *Bibl. Trig.*, as he did in Simon Hacob († 1564) in 1600 (Verhaeren, 1969, 1140-1141), Johann Cuba's (flourished 1484) 1546 book (Verhaeren, 1969, 1130), and Friederich Helbach in his wine-book (1604) bound with Heinrich Knaust's (16th century) 1614 publication of 1575 (Verhaeren, 1969, 1138, 1143), and Bartolomeo Crescentio Romano's († 1602) 1601 book. The donator, Schreck's friend Joannes Faber, wrote much longer versed bookplate in May 1616: … *Rndo adm P. Nicolao Trigavtio Societatis Jesv,&Joanni Terrentio eiusd: Societ: Amicis dulcis: pri Sinensi itinere insui memoriam librum hinc donabat Joannes Faber, Medicus,&Simplicarius Pontificius. – Misionis Sinensis.* (Verhaeren, 1969, 954-955 (No. 3254)). Golvers, 2010, 51; Golvers, 2006, 12, 18.

[8] Bernard-Kilian Stumpf (* 1655 Wurzburg; † 1720 Beijing) wrote his bookplate *Anno Kam Hi 56 luna 1a, die 4a Impr hunc librorum cum 10 aliis dono dedit P. Kiliano* in Valeriano Bonvicini's 1666 book (Verhaeren, 1969, 938 (No. 3206)) and *P. Kiliano Stumf. – Ex dono Pris Amiani ijn itinere Maccaensi. – Vice prov. Sinensis S. J. –Ad usum P.K.S.* to the one of three Beijing examples of the Jesuit Paolo Casati's (* 1617; † 1707) 1685 book (Verhaeren, 1969, 944 (no. 3228)). Stumf sometimes just used bookplate *P.K.S*.

[9] Walravens, 2001, 188.

[10] Gaubil, 1970, 617.

Holland. The bishop of Beijing after 1740, named de Souza,[11] saw over the shipping.[12] No document mentioned the nature of Hallerstein's electrical machine. It could be electrostatic frictional device or Musschenbroek's Leyden jar invented in November 1745 and announced in January 1746. The former Leyden student, Sanchez, knew Musschenbroek's Leyden jar experiments. At the same time, Peter Collinson[13] of the Royal Society of London shipped the Leyden jar to Benjamin Franklin across the Atlantic. Sanchez corresponded with Collinson and sent him the rhubarb plant obtained from the Beijing Jesuits.[14] On October 25, 1753 Collinson sent the mimosa flower to the Hallerstein's friend, glassmaker and Emperor's Beijing gardener Pierre Noël Chéron d'Incarville (* August 21, 1706; † June 12, 1757).[15] On November 2, 1747 d'Incarville reported to Parisian Bernard de Jussieu (* 1699 Lyon; † 1777 Paris) how Hallerstein gave him Johann Schreck's herbarium *Plinius Indicus* with the plants minerals and animals picked up in nature and collected in Hernandez' Mexican way. J. Gruber described Schreck's herbarium to Kircher. Schreck was Galileo's former collaborator in the Academy *dei Lincei*, and he coauthored Federico Cesi's (* 1585; † 1630) Roman publication of Hernandez' notes on Mexico plants *Thesaurus Mexicanus* in 1651. Under the Ming Emperor Schreck was able to travel around in search for plants or minerals. Kepler published Schreck's letter in *Ad Vitellionem* (1604) which Hallerstein inherited from the missionary Alexandre de Rhodes (* 1591; † 1660 Ispahan in Persia).[16] Hallerstein allowed d'Incarville to copy the Schreck's herbarium for his and Jussieu's use, so there were just three exemplars of the unpublished item, which Kögler have to Hallerstein before he died. D'Incarville sent the copy to Jussieu in three letters dated 1747, 1748, 1751 and to Claude-Joseph Geffroy (* 1685 Paris; † 1752 Paris), who died before finishing the comparative studies.[17] In February 1746, the Royal Society secretary Cromwell Mortimer asked the Chinese Jesuits to mail him the butterflies and their larvae. Hallerstein hired d'Incarville to do the job because he was the most leaned botanist among the monks as he reported in his letter on September 18, 1750.[18]

As the leading scientist among the Beijing Jesuits, Hallerstein experimented with the electricity. His collaborator from French college Amiot was especially interested. French college of Beijing probably had no electrical instruments. Therefore, Amiot had to borrow Hallerstein's equipment from the nearby Portuguese college. Hallerstein had much better equipment according to his friend, the French Beijing Jesuit Gaubil.[19] At that time, Amiot, Hallerstein, and Gogeisl cooperated on the measurement of the height of the star Gamma in Andromeda to match the order of Pezenas[20] and two other Jesuits, who made similar measurement in Marseilles.[21]

The Beijing Jesuit electrophorus experiment under Hallerstein's supervision, described in Jesuits' letter mailed to Petersburg, was a corporate Jesuit work highly praised in the Petersburg academic circles. In 1779 letter addressed to the Emperor's gardener named after d'Incarville's death, Peter Cibot, the Petersburg academicians expected the continuation of Beijing Jesuits' research in electricity[22] which eventual did not appear after 1755. In 1755, the Beijing Jesuits electrified a thin glass plate by friction and put it on the glass coverage of the magnetic needle. They probably used the "electric machine" which Hallerstein received in 1750. The needle suddenly raised and adhered to the inner side of the glass wall for few hours. Later it returned to its normal position. When the Jesuits removed the previously electrified glass plate, the needle rose again and remained in touch with the glass cover. When they returned the plate, the needle fell down again, and the Jesuits repeated the experiment many times.[23]

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[11] Polycarpe de Souza (Sou Tche-Neng Joei-Kong, * January 26, 1697 Coimbra; SJ October 31, 1712 Portugal; † May 26, 1757 Beijing (Pfister, 1934, 701)).

[12] Gaubil, 1970, 703.

[13] Peter Collinson (* January 14, 1694 Hugal Hall; † August 11, 1768 London).

[14] Chalmers, 1816, 27: 88; Gaubil, 1970, 37.

[15] Rinaldi, 2006, 153, 159.

[16] The Beijing Jesuits had two heavily annotated copies, one with the Rhodes' Macao bookplate *Alex^{ri} Rhaud^{s} J.C. D.M.Ch.* (Verhaeren, 1969, 557; Pfister, 1932, 184-185), and the same bookplate has Heron's Venetian edition of *De gli avtomati* (1601) (Verhaeren, 1969, 968). In late 1646 in Macao, the Pole Michel Boym (* 1612; † 1659) called "*Miguel Polaco"* adorned Kepler's *Tabulae Rudolphinae* (1627) with manuscript marginal notes for ecliptic predictions and bookplate *Da Livreria de Nant'am. – Memoriale ao p^{e} Procurador do Japon. Pede muyto o P Miguel Polaco que ste liuro Tabulas Rudolphinas V R detenia en Procuradoria e si o mesmo P^{e} Miguel Polaco ditto liuro pedira a P^{e} Joan Nicolao de China que o mande lae si ambos merveran que a appliqué pera Pekin porque he unico a optimo pera calcular as Ecclipses Sol et Luna e movimentos dolle 1646. Decemb 2. Maccaj* (Verhaeren, 1969, 559).

[17] Bernard-Maitre, 1949, 25-27, 28; Pfister, 1932, 154, 157; Iannaccone, 1998, 84-85; Kircher, 1667, 110-111.

[18] Bernard-Maitre, 1949, 29; Pray, 1781; Pinto, 2010, 51.

[19] Hsia, 2009, 4, 172.

[20] Espirit Pezenas (* November 28, 1692 Avigon; SJ; † February 4, 1776 Avignon).

[21] Gaubil, 1970, 840, 843, 850.

[22] Pray, 1781, 270; Pfister, 1934, 891; Bernard-Maitre, 1948, 175; *Nova acta academ. Scient. Petropoli* 1779, 7: 22.

[23] Aepinus, 1979, 130.

On January 12, 1755, Gaubil received the undated letter of Richman[24] and Kratzenstein's letter dated April 12, 1753 addressed to the Jesuits of Beijing. Gaubil wrote back to both Petersburg academicians on April 30, 1755, and mentioned Amiot's experiments "that should make you happy." Gaubil complained that the Chinese were not very interested in electrical experiments[25] compared to the European and American euphoria of that time soon after the Musschenbroek's invention of Leyden jar. Even the medical use of electricity was not Chinese match, although Kangxi (康熙帝, ruled 1662-1722) during his illness highly praised European anatomical knowledge[26] of Harvey and Vesalius[27] borrowed via the French Beijing Jesuits Parrenin (* 1665; † 1741) and Joachim Bouvet (* 1656; † 1730).

On November 25, 1753, the Russian Academy of Science proposed the award for the best explanation of the true causes of electricity including their theory, with a deadline June 1, 1755.[28] They were interested in chemical and physical aspects.[29] For September 6, 1751 the Empress Elisabeth and the Academy president Count Kiril Grigorjevič Razumovskii (* 1728; † 1803) proposed a Lunar theory competition, and on November 26, 1753 dissolution of silver in *aqua fortis*. For the year 1756, they offered award for corpuscular theory, and for 1757 competition they put into limelight the diurnal planetary motion. On September 6, 1755, the Empress announced the awards without mentioning Beijing contribution, which was probably the part of competition. The first award (*praemio coronato*) went to Leonhard Euler's son Johan Albert Euler for his *Disquisitio de causa physica electricitatis*. Other two elected researchers were professor in Pisa University Paul Frisi (* 1727; † 1784) from Paul's clerical order[30] who discussed the causes of electricity, and Lalande's Jesuit teacher and director of Lyon observatory after 1740, Laurent Béraud (* 1703 Lyon; † 1777), who put forward his theory for the award of Petersburg academy. Euler tried to calculate the speed of electricity from the elasticity of ether, and experimented with the conductivity of the vacuum above quicksilver in barometer. Euler used Mussenbroek and Kleist's Leyden jar,[31] and discussed Martin Frobenius Lederemüller's (* 1719; † 1769) experiments with the spirit of wine. Frisi developed Kratzenstein's experiments[32] to please the academicians, and used Mairan Parisian theory, Bernoulli's vibrations, Bose, Beccaria, and Jean T. F. Jallabert's (* 1712 Geneva; † 1768 Geneva) experiments.[33] In the last, 5th, experiment Frisi repeated Franklin's glass jar conclusions, and moved to the theory of John Théophile Désaguliers (* 1683; † 1744). He also relied on the ideas of Monnier, Franklin's antagonist Nollet, Martin Frobenius Lederemüller, and Winkler. Frisi added some simple calculations with corresponding figures attached to the end of the book. Béraud used L. Euler theory of force for Boyle and Hale's molecular theory and heavily relied on his compatriot Nollet's experiments, while briefly mentioning Franklin.[34]

The copy of academic publication of electricity research of Euler, Frisi, and Béraud (1757) passed to the tertiary Franciscan Alexander de Gouvea (* 1731 Evora in Portugal; † 1808 Beijing), the appointed Beijing Bishop in 1782, who used his stamp as a bookplate.[35] In August 1755, Gaubil sent to Razumovskii two Amiot's packets and other presents.[36] Gaubil knew that Richman and Kratzenstein widely published on electricity, but he was not aware of Richman's accident on July 26/August 3, 1753. Soon afterwards, on August 13, 1753, Kratzenstein left the academy of Petersburg and became the professor of medicine and physics in the University of Copenhagen.[37]

Zeiher[38] replaced Kratzenstein in 1756 and reported to Gaubil how Richman's unfortunate death[39] shocked the scientists worldwide. A.Hallerstein's first cousin Baron Erberg bought Kratzenstein's research of vapors republished in Trnava in 1763. After working in Berlin for a short time, Aepinus inherited Richter's membership in the Petersburg academy. Aepinus was born in Rostock in Prussia where he completed his studies and taught. He collaborated with his Swedish student Johann Karl Wilcke (* 1732; † 1796) on the early type of condenser which Volta later developed into electrophorus in

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24 Georg Wilhelm Richman (Richmann, * July 11, 1711 Pernau; † July 26, 1753 Petersburg).

25 Gaubil, 1970, 803, 810-811; Heilbron, 1979, 405; Kloss, 1987, 41; Koplevič, Cverava, 1989, 55; Cverava, 1986, 58.

26 Asen, 2009, 33-34.

27 Niu, 2006, 65.

28 in veram electricitatis caussam, veramque ejus condatur theoria (Euler, Frisi, Béraud, 1757, 1: 3-4, 10).

29 Euler, Frisi, Béraud, 1757, 1: 8.

30 Frisi, 1755. Later Beijing missionaries obtained Frisi's anthology published in 1781 (Verhaeren, 1969, 958).

31 Euler, Frisi, Béraud, 1757, 2: 8, 10, 12, 35, 84, 103.

32 Euler, Frisi, Béraud, 1757, 2: 36, 57, 68, 101.

33 Euler, Frisi, Béraud, 1757, 2: 49, 54, 59, 63, 77, 88, 106, 121, 131.

34 Euler, Frisi, Béraud, 1757, 2: 61, 64, 78, 80, 90, 96-97, 140, 143, 150, 153, 173, 187, 194, 198, 202.

35 Verhaeren, 1969, XXII, XXIII, 199; Pfister, 1934, 942, 1034.

36 Gaubil, 1970, 818.

37 Koplevič, Cverava, 1989, 80.

38 Johan Ernst Zeiher (* 1720 Weissenfels; † January 7, 1784 Wittenberg).

39 Cverava, 1986, 58.

the gymnasium of Como in 1775 where he taught, before he got a chair in Pavia University. On May 10, 1757,[40] Aepinus arrived to Petersburg and remained professor of physics in the academy until 1798.

The new professor of physics Aepinus[41] immediately analyzed the Beijing experiments to please the Russian authorities. Just few months later he reported on the Beijing experiments to the Petersburg academy[42] and described his research on March 9, 1758. Aepinus explained the Jesuits' Beijing experiment with the small conductivity of glass, which allowed the induced charge on the compass' glass coverage, slow movement of the charge into the attached compass' needle during the experiment, and equally slow returning of the charge after the induced charge removal. Aepinus successfully repeated Beijing experiment and figured twelve similar experiments of his own. He believed that Beijing experiment fully confirmed Franklin's theory.[43] It was certainly also a political question in the middle of the Franklin and Count Buffon's quarrels against Nollet and d'Alembert in France. The Beijing Jesuits' support of Franklin's ideas mirrored the Jesuit physicist Rudjer Boskovic's close friendship with Franklin. Hallerstein certainly knew Boskovic's work pretty well. Boskovic was few years his younger, but he used Boskovic's work, and A.Hallerstein's brother Vajkard Hallerstein was the confessor of Emperors brother in Brussels where Boskovic frequently visited his friend, the Habsburg Netherlands' Governor Count Janez Karl Filip Kobencl (* 1712 Ljubljana; † January 27, 1770 Brussels) from Hallerstein's native Ljubljana. We could consider Aepinus' mathematical theory of electricity as a part of the mainstream to which Beijing Jesuits' electricity research also belonged. Hallerstein and his Beijing collaborators could have read Franklin's letters to the wealthy English Quaker Collinson about electricity of Leyden jar published in English (1751) or French (1752), or Beccaria's Latin compilation (1751) before they prepared their electrical experiments, which were drastically different from Franklin's achievements. Sanchez and Collinson provided the necessary equipment for both research groups, Franklin's group in America and Jesuits' college in Beijing. Both target groups proved extremely successful, and returned the gift with unexpected extraordinary new discoveries. Sanchez and Collinson both collaborated with the London Royal Society, but Franklin did not cooperate with Beijing Jesuits although the leading European Jesuit Boskovic became his close friend. Both target groups in America and China communicated just through the good old Europe. Franklin himself probably did not bother to repeat Beijing Jesuit's experiment because he switched to diplomatic-political waters son after he gained his fame in the field of electricity. It is also not widely known if the Beijing Jesuits repeated Franklin's experiments, or even introduced the Lightning Rods into China, but technically speaking their experiments brought even more fruit through Volta's research even if they received far less fame compared to the clever diplomat Franklin. Hallerstein or other Jesuits in China did not have any Franklin, Beccaria, or Boskovic followers' books[44] at least they are not preserved.

Aepinus discussed the electrical and magnetic forces for the Academy and dedicated his research to the Empress on September 7, 1758. He used the experiments with Leyden jar to explain the analogy between electricity and magnetism but he did not mention the Beijing report although he used the Jesuits' Beijing data.

In autumn 1758, Aepinus developed his mathematical theory of the electric and magnetic effects. On June 4, 1759, Aepinus presented his book to the academy. In late November,[45] he dedicated it to Razumovskii. Aepinus used his favorite Franklin's theory of one fluid without mentioning the Beijing experiments.[46] But Aepinus' explanation did not please everyone. He had to excuse from his readers because he used to sophisticated mathematic in the field of electricity which was

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[40] Novik, 1999, 10.

[41] Franz Maria Ulrich Theodosius Aepinus (* December 13, 1724 Rostock; † August 10/22, 1802 Dorpath).

[42] On November 17, 1757 (Novik, 1999, 11) or on December 1, 1757 (Aepinus, 1979, 492).

[43] Aepinus, 1761, 23-24; Aepinus, 1758.

[44] Hallerstein used Paris academic publications, one of them with d'Incarville's article published in 1755 (Verhaeren, 1969, 3-4). Hallerstein had Musschenbroek's textbook *Elementa Physicae* in three editions with the older ones bearing Sousa-Sanchez and Gouvea bookplates (Verhaeren, 1969, 669-670). He used Boyle's works on vacuum techniques (Verhaeren, 1969, 312, 315), Hooke's *Micrographia* in the first English 1665 edition with the bookplate of China vice-provincial appointed in 1727, Joao de Saà (* 1672 Sambujat in Portugal; † 1731 Macao) (*Aplicado à Resid^{a} de Chinkiam por ordent Publicada pello P. VProv^{ai} Joao de Saà* (Verhaeren, 1969, 1198; Pfister, 1932, 484). Saà also had Gemma Frisius *De Principi Astronomiae&Cosmographiae* (1553. Antverpiae: Ioannis Steesii (Verhaeren, 1969, 487), and his successor vice-provincial, superior, and Emperor's mathematician Karl de Rezende (* 1664 Lisbon; † 1746 Beijing) wrote his bookplate *Comprado p^{a} Chim tim fu pello P. Carlos de Rezende em 1734* in Carlo Fontana's (* 1634; † 1714) *Utilissimo trattato dell' acque correnti* (1696. Roma: Gio. Francesco Buagni) (Pfister, 1932, 483-484; Verhaeren, 1969, 956 (No. 3271)). The Beijing missionaries also had Newton's books including his English Theology (Verhaeren, 1969, 1099-1200), the Viennese Marinoni (Verhaeren, 1969, 637), Hallerstein's favorite astronomers, the Bolognese Manfredi and Zanotti's four copies of 1750 edition, two of them with bookplates (*Jam ab aliquot annis, anni 1726 tomum lipsiensem acceperamus. Hunc itaque R^{do} P^{i} Andre. Pereyra D D. Servus in X^{o} humillium. C Gaubil SJ.* and *Dono De Lisle* (Verhaeren, 1969, 632-633)), Zanotti's ephemerides (1750) (Verhaeren, 1969, 912), and Johann Leonhard Rost (* 1688; † 1727) *Der Aufrichtige Astronomus* (Nurnberg 1727) (Verhaeren, 1969, 1151).

[45] Novik, 1999, 12-13.

[46] Aepinus, 1979, 130-131.

traditionally (ab)used by philosophers or experimentalists without any love for higher mathematics. Symmer[47] conducted two sets of experiments similar to the Beijing ones which he apparently never heard of. Symmer presented his results to the Royal Society of London between February 1 and December 20, 1759. He concluded with an essay about two distinct powers in electricity[48] which pleased Franklin's opponents.

Cigna[49] was the first to comment on Aepinus' analysis of Jesuits' experiment,[50] and Cigna's uncle Beccaria[51] was interested in Beijing Jesuits' results too. In 1747, Beccaria became professor of experimental physics at the University of Turin. Although he was member of different religious order called Scolopi or Piaristen, he was a close Boskovic's friend and an admirer of Franklin's achievements. In 1767, he added a previously unknown similar effect to the Beijing Jesuits' experiment. He charged the coated glass plates. He removed the coating from the negative plate and put other neutral uncoated glass plate nearby. He coated uncharged plate and used the conductor to connect its coating with the coating of charged plate. The plates touched and leaned on each other with the whole area. If he separated the plates after they were in touch for some time but before the fusion, the charged plate got the positive charge on both sides and the uncharged plate got the negative charge on both sides. If he separated the plates after the fusion, the charged plate became negative on both sides, and the uncharged became positively charged on both sides. If after the fusion he separated and again joined the plates, the small circled paper under the uncharged plate fused to it after each separation and got repelled after each touch. Beccaria was able to repeat the experiment for as many as 500 times after the single charging of the plate.[52] It was the great advancement compared to the Leyden jar experiment where the recharge of the jar was necessary after each experiment. The era of the steady electricity current was on board thanks to the Beijing experiments.

In 1769, Beccaria reprinted the Beijing report, mentioned Aepinus mathematical explanation as incomplete, and put forward the explanation of his own. He offered the idea of the special "*electricitas vindex*".[53] Beccaria was an experienced researcher, but even he had his shortcomings. Alessandro Volta worked in Como and later in the Habsburg University of Pavia where the Jesuit Boskovic and former Carniolan naturalist Anton Scopoli used to lecture before him. Volta was not satisfied with Beccaria's explanation and the urgent search for other possible explanations led Volta to the invention of electrophorus several years later described in a letter to Priestley dated June 10, 1775.[54] Volta was a Habsburg North Italian employee and his electrophorus was quickly introduced in the Habsburg Monarchy, especially by the Empress Maria Therese's personal physician Dutch Jan Ingenhousz (Ingen Housz, * 1730 Breda in Southern part of the Dutch Republics; † September 6, 1799 Wiltshire) who published his own contributions. The German Molitor's Viennese translation of Ingenhousz's work was published in 1781, just before Ingenhousz returned to England. It was already too late for Hallerstein to learn more about Volta and Ingenhousz from his Viennese collaborators, because Hallerstein passed away in 1774.

Gruber's Technology for Politics

As a young Jesuit Gabriel Gruber wished to follow Hallerstein's example in the China Jesuit missions, but he was never accepted. Authorities needed his mathematical, technical, or diplomatic abilities in Europe. The Viennese born professor of Slovene origin Gruber made successful engineering career in Ljubljana, White Russian Polotsk, and Petersburg. As director of the first of the two Habsburg Monarchy navigational Divisions he worked most of all on Mur River between Graz and Radgona, Drau River around Ptuj, Sava River, and Hungarian-Rumanian rivers. Gruber and his staff built two models of the "machine ship" for upstream navigation in 1779/1780, and successfully trued them on Mur River. Gruber's model of "machine ship" was comparable with the contemporary achievements in France, England, and even in the recently established USA where the Fulton's steamboats soon brought the success.

Gruber build the Ljubljana canal, and for many decades designed canal connecting the Adriatic with Black Sea. He published new data about that *Argonaut* road in Prague in 1802 when he was already a Jesuit General. As pioneer of the karst research Gruber examined underground water connections allover today Slovenian Karst to prepare the Ljubljana Canal Design. As physicist he researched the *fata morgana* optical illusions above the water of the Cerknica Lake. With the help of his brother Tobias Gruber, the president of Bohemian Scientific Society, Gabrijel Gruber accomplished pioneering *fata morgana* laboratory experiments.

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47 Robert Symmer (* about 1707 Galloway; † June 19, 1763 London).

48 Symmer, 1759, 380; Aepinus, 1979, 406.

49 Gian Francesco Cigna (* July 2, 1734 Mondovi; † July 16, 1790 Turin).

50 Aepinus, 1979, 200.

51 Giacomo Battista Beccaria (Beccheria, * October 31, 1716 Mondovi; † May 27, 1781 Turin).

52 Beccaria, 1767, 297-298. Beccaria (1767, 297) noted the wrong volume citation of the Aepinus' comment of Beijing experiment (*Phil.Trans*. 8: 276). Priestley (1775, 1: 316) later repeated the error.

53 Beccaria, 1769, 44-47; Heilbron, 1979, 405-410.

54 Aepinus, 1979, 131; Volta, 1816, IV, 108. Joseph Priestley (* March 13, 1733 Fieldhead; † February 6, 1804 Northumberland).

In Polotsk Gruber researched connections between earthquakes and terrestrial magnetism, and published his experimental measurements in Jena. He combined his scientific, artistic and engineering practice with exceptionally successful political and diplomatic career eventually becoming Jesuit General and influential unofficial political advisor of the Tsar Paul I. Gruber connected east and west technologies. He designed architectural novelties as the first professor of architecture in Russia. Gruber used his technological, artistic, and scientific skills to navigate between Scylla and Charybdis of Napoleonic politics eventually preparing the Conclave for Pius VII election, or even the Tsar Paul's association with Napoleonic forces. Gruber was the most successful follower of the doctrine *for the greater glory of God* and used his technical-scientific knowledge for that purpose. The 18th century Jesuits lived as priests and scientists in the way never achieved before or after, although few Franciscans and other friars certainly were their match. Because of the rite controversy Chinese Jesuits with Hallerstein as their leader abandoned the idea of converting the Emperor. The Hallerstein's Jesuits predecessors in Beijing believed in the success, but Hallerstein himself already had considerable doubts expressed in several letters mailed to his brother in Brussels. The extra time gained enabled Hallerstein's full-time scientific research.

The Jesuit idea of converting the ruler as a prelude for his country's Catholic conversion was rather old Constantine's[55] example. We claim that Hallerstein abandoned the politics for the attempted mass conversion from the top and rather tried to win Chinese for the European Science. The Jesuits always used knowledge as propaganda showing that Catholics with better science-technology should also provide a better faith and church. Hallerstein published as much as possible of his Chinese measurements in Centers like London, Paris,[56] Lisbon, Petersburg, or Vienna. He eventually took the trouble to publish his best scientific results in Europe as far the most successful of all Jesuit Beijing missionaries considering his European publications.

The idea of conversion from the top downwards was not buried at all with the Ljubljana Jesuit Hallerstein's soft politics in Beijing. His younger countryman Jesuit General Gruber was the most successful follower of the "top to bottom" conversion idea of all times. Gruber in Russia did not accept Hallerstein's kind of defensive political role and nearly made the Emperor Paul I his Emperor Constantine.

Russian court just occasionally took some interests for the Jesuits' connections with the Far East which could assist Russian invasion of the East. The Tsarina Catharina the Great used the exceptional Gruber's diplomatic abilities immediately after Gruber's arrival. In 1785 the White Russian gubernator Passek passed to the Russian Jesuits the order of the court asking them to send two capable Jesuits to the Russian capital. The Jesuits named Manswet Skokowski (Mansuetus, * February 20, 1751 White Russia; SJ August 13, 1770 Nieświež; † February 8, 1798 Mscislaw) and Gruber for the secret mission, immediately after Gruber's arrival. Gruber was asked to obstruct the English influence in China which began to grow after the year 1785 and especially after the Chinese Emperor talked to the English representative, Lord James McCartney, in 1792.[57] The task was not easy and Gruber got things done very slowly. He and his helper returned to Polock to wait for the new invitation from Petersburg court.

During his June 1799 visit of Petersburg Academy Gruber wrote a French letter to the Tsar,[58] asking him again to write to the Pope for official recognition of Gruber's Jesuits. In the second paragraph Gruber remembered the Papal spoken support of the legacy of Jesuits in (White) Russia. In the third paragraph he limited the legacy just on Russia without mentioning the world-wide expansion of the former Jesuits. Gruber underlined the success of Jesuits' juvenile educational program in newly conquered Russian lands with no note on the Catholics of former Polish territories. In the fourth paragraph Gruber put in the limelight the diplomatic advantages of the broad legitimism of Jesuits which could enable the official support if the former Jesuits in Beijing. The Tsar certainly had a lot of diplomatic or trade interest in China, and Gruber put them forward ingeniously. After the suppression the status of the Jesuits in China was certainly unstable and Gruber therefore asked the Petersburg Court to submit the letter for the Pope about the legitimization of Jesuits. He wrote about China »*where it is necessary for the success of all the diplomacy that I travel and (stop) the rumors against the Jesuits who cannot work in (those) areas without the written permission*«. In his 5th paragraph Gruber stated that the Duke of Parma and many other rulers support his plans. Many peculiarities of Gruber's courting the Chinese Jesuits were destroyed in fire which caused Gruber's death in 1805.[59]

The Tsar Paul suggested to Gruber that he should send two Jesuits for China. That was one of the rare ideas which Paul inherited from his mother (1792), and even his son Alexander liked it. In May 1799 Gruber and Skokowski were invited

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55 Flavius Valerius Aurelius Constantinus the Great (* 280/288 Niš (Naissus); Emperor 306; † 337).

56 Iannaccone, 2005, 384.

57 Peyrefitte, 1991, LVII.

58 ARSI, Russia, »Acta Congreg. Gen. Russia«, box 1027, folios 148r-149r; Inglot, 2002, 283.

59 James, 1977, 4.

to Carskoje selo without any definitive success.[60] On January 16, 1800 Gruber and Kamienski left Polock again to meet the Tsar, but they had a lot of problems.[61]

Gruber got the official patronage and even free entrances into the rooms of the Russian Tsar Paul I,[62] who usually accepted him with the Jesuit's slogan "*Ad majorem dei gloriam*!"[63] The Tsar even publicly declared himself as "the Catholic in hart",[64] which was certainly a strange position for the nominal leader of the Russian Orthodox. On August 11, 1800 in Gachin the Tsar suggested to the Turkish Authorities and to the Swedish king that they should recognize Gruber's Jesuits in their states. On the same day the Tsar finally mailed a letter to the Pope asking him to legitimize the Jesuits.[65] Paul's days were nearly over but his letter finally gave Gruber all he wanted. However, on the March 11/23, 1801 evening Paul was murdered in his bedroom of St. Michael palace in Petersburg.

Once in the saddle, Gruber made huge efforts to re-establish the Jesuit hierarchy in China. In that way he continued the work of his compatriot Hallerstein a quarter of century after Hallerstein's death. Gruber certainly followed Hallerstein's ideas and had considerable success in courting Hallerstein's Chinese successors. Gruber certainly knew how to select the useful helpers in China. His main connection in Beijing was the former Jesuit Mandarin, Louis de Poirot.[66] On March 20, 1770 Poirot and Luigi Cipolla[67] boarded the Portuguese ship, and on October 20, 1770 they put their anchor in Canton. Poirot arrived to Beijing on August 14, 1771. The painter Poirot got his Mandarin after his excellent translations during the McCartney's visit, and he grew famous for his translation of the Bible in Chinese and Manchu. In September 1802 he asked to join Gruber's Russian Jesuits.[68] Cipolla finished his medical studies because the Chinese court of those times was short for physicians and painters. Cipolla asked for a permission to change his Beijing French Mission with the Portuguese one because he wished to work in Hallerstein's Astronomical Bureau. After the suppression of the Jesuits Cipolla became the propagator in Beijing in 1776. He asked to join Gruber's Jesuits (1805) after Gruber added to his letter of invitation the most suggestive advice of Cipolla's juvenile friend, Eduard Desperamus.[69] Gruber knew how to invite his former Jesuit brothers to rebuild the Jesuit Society.

Among others, the Slovakian-Viennese astronomer Maximilian Hell and the Bishop of Nanking Laimbeckhoven asked to join Gruber's Jesuits.[70] In that way Gruber used the high politics to attract the Viennese editor of Hallerstein's scientific works, Hell. In the same time he talked over Hallerstein's best friend, Laimbeckhoven.

The Tsar Alexander was even more willing to support the Jesuit work on the Far East compared with his farther or grandmother. Therefore Alexander and Gruber easily found their mutual course for the Jesuit work in Siberia near Chinese border. In 1802 the new interior minister, the Prince Viktor Pavlovic Kocubej (Kocubei, * 1768; † 1834) supported Jesuits' missionary work.[71]

Gruber learned about the Siberian and American circumstances already in Ljubljana after reading Ignaz Born's[72] expert Natural History freemason Journal *Physikalische Arbeiten der einträchtigen Freunde in Wien* for the years 1783, 1784, and 1785. In March 1802, the Tsar Alexander suggested that three Gruber's Jesuits should join the Chinese deputy of Russian Embassy. One of them should be Pole, other German, and an Italian. In that way Alexander fulfilled a decade older wish of his grandmother Catharine. Gruber did not allow to be asked twice and in September 1802 he selected the Polish Father-mathematician Norbert Korsak (Corsak),[73] Italian mathematician Giovanni Grassi, and German lay-brother Johann Sturmer. Gruber gave them a letter for the Chinese together with the Russian Imperial addenda. In 1805 they sailed across

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60 James, 1977, 160.

61 James, 1977, 171-172.

62 Paul I (* 1754; † 1801).

63 Mihnevič, 1955, 304; Moroškin, 1888, 1: 375; Zaleski, 1886, 2: 56.

64 Rouët de Journel, 1922, 73.

65 James, 1977, 179-181.

66 Louis de Poirot (Ho Ts'ing-T'ai, He Qing Tai, * October 23, 1735 Loraine; SJ July 9, 1756 Rome; † October 13, 1813 Beijing). Pfister, 1932, 173, 965-966; Dehergne, 1973, 207; Moroškin, 1870, 1: 225-226; Moroškin, 1888, 2:318; Zaleski, 1905, 2: 136.

67 Luigi Cipolla (Aloys Cibolla, Louis, * Caltavuturo near Palermo; SJ November 5, 1757 Sicily; † after 1805 (Dehergne, 1973, 56)).

68 Rinaldi, 2006, 263; *Woodstock Letters*, 1886, 15: 116. Gruber's letter mailed on March 12, 1804 to Carroll mentioned »last year« (that means 1803) when the Apostolic missionary Poirot in Beijing asked to join Gruber's Jesuits.

69 Inglot, 2002, 358; Moroškin, 1888, 2: 333. Eduardo Desperamus (* September 27, 1737 Chios; SJ November 6, 1751 Province Sicily, October 27, 1803 Petersburg; † November 26, 1812 (Inglot, 1997, 214)).

70 Gottfried-Xavier Laimbeckhoven (Nan Hoai-Jen Ngo-Te, * January 9, 1702 Vienna; SJ January 27, 1722 Vienna; † May 22, 1787 T'ang-kia-hiang near Su-choua in China). Zaleski, 1886, 413, 427; Inglot, 2002, 358; Moroškin, 1888, 1: 459.

71 Kočubej was ambassador in Istanbul (1793-1798) and the vice-chancellor in 1798 (De Maistre, 1995, 44).

72 Košir, 1998, 43.

73 Moroškin, 1870, 2: 311.

Denmark to Portugal and stayed for a while in Coimbra. But the Cardinal Consalvi[74] and the Roman congregation for the propagation of Faith (*Propaganda Fide*) stopped the procedure.[75] The Russian Tsar finally forbade the Chinese journey of the Jesuits. Korsak went to England, Grassi became the 5th president of the Georgetown College (1812-1817),[76] and Sturmer left the Jesuit Society in Petersburg. Gruber died in between and did not witness how his carefully planned renewal of the Beijing Jesuit Mission failed.[77]

A year after Gruber's death on June 29, 1806 the Tsar allowed the Jesuits to enter Siberia, although Gruber's antagonist Stanislav Jan Siestrzencewicz-Bohusz (Sestrencevič, * September 3, 1731; † December 1, 1826) preferred the Dominicans. Two Jesuits lived in East-Siberian Irkutsk, and other two in West Siberia. In December 1811 two Jesuit Fathers left Polock for Irkutsk and arrived there on April 1, 1812. The huge city and the landscape pleased them in spite of the expensiveness. Later one of the fathers went to West-Siberian Tomsk. The Siberian mission was a part of the Archbishopric of Mogilev, and Siberians called around thousand Catholics in Irkutsk with their ancient name *Poles*. The connection between the faith and nationality was similar as among the Serbians and Croatians but it was abandoned earlier because of the quick separation between Russian and Polish languages with much more numerous speakers. The similarity between being Catholic and being Pole was lost in European part of Russia already in Gruber's time.[78]

The leader of the Russian Uniats, the Archbishop Krassowski, lived in Polock as did the majority of Russian Jesuits. He soon began to complain because the Jesuits baptized the children of the Uniats in Siberia. The Russians planned to make their way through Siberia to China, but they did not allow the Jesuits to cross the Chinese border. The only Jesuits allowed to do so were the Chinese Emperor's deputies during the signing of the peace in Nerchinsk on August 27, 1689. More than a century later the situation was completely reversed because the Russian Jesuits were allowed to visit Nerchinsk east of Irkutsk, but they were not allowed to enter China which they desired urgently.

After the Jesuits had to live Russia in 1820, the Siberian Jesuits went to Galicia because Chinese or Russian did not need their mediation to settle their mutual quarrels on the border. The Jesuits became the intolerable elements for both sides, and Russians tried to replace them with some more obedient Catholic Order.[79] As the result, Moscow, Saratov, South-Russia, Siberia, Riga, and other Russian missions did not prosper in a way which Gruber planned and wished.

Gruber was the most successful follower of the Ricci's method of combining the successful Western science&technology with the Catholic propaganda. Gruber decisively influenced Bishop Carroll's development of the catholic hierarchy and Jesuits Schools in USA. The USA situation was different from Europe or China in many aspects because the new world settlers' cultural-scientific level was comparatively low in Gruber's time. In spite of the Jesuit Boskovic's friendly relations with the American Benjamin Franklin, the USA was not so promising as China or even South America before the end of Napoleonic wars. Gruber studied with Boskovic's Viennese collaborators and helped to glorify the Franklin's fame all over Europe. Jurij Vega and other Gruber's students discussed Franklin's electricity ideas. Gruber also studied Humboldt's American measurements. Humboldt described in detail the connections between electricity and magnetism without mentioning Gruber's work, but he eventually criticized Gruber's measurements of *fata morgana* above Cerknica Lake (1793),[80] because Gruber didn't precise the volume, height, or site of his *fata morgana*.

Gruber devoted a lot of attention to the USA Jesuits and helped Carroll to rebuild the USA Jesuit hierarchy with the Jesuits' Schools. On April 25, 1803, the former USA Jesuits wrote to the first USA Bishop Carroll from the Counties Charles and St. Mary's about Papal approval of Gruber's Jesuits. They knew that Gruber is rebuilding the Jesuit Society worldwide and they wished to join him.[81] On May 25, 1803 Carroll[82] and the Bishop Charles Neale († April 1823) of Baltimore told Gruber that 13 former Maryland Jesuits asked to join Gruber's Society. They were descendants of 5 Jesuits who entered Maryland with their leader Father Andrew White in 1634. They were prepared for Papal suppression in 1773 when 21 Jesuits of Maryland and nearby lands joined their former Jesuit property into a trust while they awaited the renewal of the Society.[83]

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[74] Ercolo Consalvi (* June 8, 1757; † January 24, 1824).

[75] Inglot, 2002, 358, 363.

[76] Giovanni Grassi (John Antony, * November 10, 1775 Schilpario near Bergamo; SJ November 21, 1799 Colorno; † December 12, 1849 Rome (Grzebien, 1996, 543; Schlafly, 1999, 270; Inglot, 1997, 235; Devitt, 1909, 29, 31, 32; Dixon, 1974, 73; Easby-Smith, 1907, 1: 48)).

[77] Zaleski, 1870, 2: 136; Peck, 2001, 19.

[78] Peck, 2001, 20-23.

[79] Peck, 2001, 31, 33.

[80] Humboldt, 1970, 1: 625.

[81] Hughes, 1910, 1/2: 816.

[82] John Carroll (* January 8, 1735 Upper Marlboro in Maryland; SJ September 7, 1753 Watten; † December 3, 1815 Baltimore).

[83] Kuzniewski, 1992, 57.

The New World followed the modern capitalistic views of property while the goods of the helpless European Jesuits mostly passed to the local governments.

Gruber received Carroll's letters dated on March 10, 1803 and May 25, 1803 in the USA, together with a shorter one dated on September 21, 1803. He warmly accepted Carroll's suggestions and on March 12, 1804[84] answered that he will send his Jesuits from Russia and even from England to help the USA Jesuits.[85] Gruber certainly had enough men among the English Jesuits, but his Russian Jesuits did not know the English language well enough. In Baltimore Carroll used Gruber's authority to appoint Robert Molyneux as the superior of the USA Jesuits on June 10, 1805.[86] For his negotiations with Molyneux in his letters dated on May 9/10, 1805, Carrol cited Gruber's authority as we could read in Gruber's handwritten copy preserved in Georgetown Archive.[87] Between June and October 1805 the new superior Molyneux renewed the oaths of the USA Jesuits, but Carroll did not rejoin them. In 1813 there were just 50 Jesuits in the USA,[88] but in the mid-20th century a quarter of all Jesuits lived in USA,[89] in 1965 as much as 8393, and in 2000 only 3635.[90] The USA Jesuits had great influence on American history of science overestimated opinion about the congeniality of Kircher and other Jesuit scientists. New York Jesuit schools founded in 1684 and 1808 worked just for few weeks, but others in Fordham, Saint Louis, or Georgetown were much more successful.

The English laws prohibited studies in the Jesuit or other Catholic schools in 1581 and 1603;[91] St. Omers was established to train Catholics and Jesuits who were brave enough to enter England illegally. There are no straightforward data about the philosophy or mathematics curriculum in 17th Century Belgium Jesuit Colleges, but they probably followed the Mathematician Clavius' Roman reforms,[92] and did not permit the use and possession of the new fashion watches among students, although they were pretty inexpensive.[93] In Gruber's time the English ceased the persecution of the Jesuits. Thomas Weld gave to the Jesuits a nice castle Stonyhurst in Lancashire. Gruber negotiated with the elderly Father William Strickland[94] to appoint Marmaduke Stone as the provincial of England with all Scottish and Irish missions included on March 1, 1803. The English Jesuits joined the Gruber's Society on May 22, 1803.[95] On August 19, 1803 Gruber described to Stone the influences of the former novitiates in the English Jesuit Province. The Novitiate in Watten, 5 miles from St. Omers,[96] worked until 1762, and the other novitiate in Ghent worked until 1773. John Carroll entered the Watten Novitiate on September 8, 1753 after he finished his studies of humanistic in St. Omers. His leader in the novitiate was Henry Corbie (Clorby, * April 14, 1700 Sussex; SJ September 7, 1722 Watten; † June 14, 1765 Ghent) who later became the provincial of England.[97] With historic examples Gruber put forward the great importance of the feast performed on the opening of the novitiate for 14 novices in Hodder Place near Stonyhurst on September 26, 1803. The preacher with 11 scholastics headed the introductory section, and the leader of the novitiate became the Father Charles Plowden (Simons, Simeon, * May 1, 1743; SJ September 7, 1759 Watten; † June 13, 1821 Jougne in France), later rector of Stonyhurst (1817-1819) and provincial between the years 1817-1821. Gruber highly praised Stone's work for the spiritual growth of the English novices on February 21, 1804 and on April 21, 1804,[98] because he expected much from his English brothers Jesuits. The Stonyhurst lecturer of physics and Natural Philosophy was Walter Clifford (* March 13, 1773; SJ November 2, 1803 Stonyhurst; † July 23, 1806 Palermo) who wrote his lectures on July 1, 1822.[99]

The Georgetown College was founded in 1789 as the first educational establishment in the District of Columbia, before the formation of the federal district and the birth of the City of Washington. The first Carroll's proposals of 1788

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[84] ARSI, Russia: 4A, Epist Vic. Gen. In Russia, 1802-1808; *Woodstock Letters*, 1886, 15: 118; Hughes, 1910, 1/2: 817; Maryland-New York Province Archives, May 12, 1804 with somewhat different registered letter analogue in contents (Deneef et all, 1992, 164).

[85] *Woodstock Letters*, 1886, 15: 117; Hughes, 1910, 1/2: 817-819.

[86] Hughes, 1910, 1/1: 387-388, 394, 397, 1/2: 820, 873.

[87] http://www.library.georgetown.edu/dept/speccoll/cl269.htm box 23, folder 38, John Carroll's correspondence 1804.

[88] Schroth, 2008, 8.

[89] Dixon, 1974, 94.

[90] McDonough, Bianchi, 2002, 307.

[91] Chadwick, 1962, 73.

[92] Chadwick, 1962, 77.

[93] Chadwick, 1962, 267.

[94] Chadwick, 1962, 371.

[95] Rule, 1853, 291, 361; Steska, 1905, 46; Spalding, Thomas, 2000, 77.

[96] Chadwick, 1962, 163.

[97] Guilday, 1954, 22; Holt, 1984, 69; Spalding, 2000, XIX.

[98] Holt, 2003, 105, 114, 117-120.

[99] Clifford, 1822; Holt, 1984, 60, 195; Foley, 1879, 5: 981-985.

mentioned the lecturing on »*Arithmetic's, the Branches of Mathematics*«, but not explicitly physics.[100] The friend of 3rd Georgetown College president William DuBourg, Lady Joustine Douat, gave 15 shillings for the Georgetown College visiting young students on May 11, 1798.[101] The Old North Building was erected between 1791 and 1808 and served until 1905. The Observatory was erected in 1843.[102] Curley taught in the Washington Seminary, the future Gonzaga College, before he joined the Jesuits in 1826. He became professor in Georgetown in 1831 and in 1834 began to teach natural philosophy and chemistry. After the observatory was built in 1843 he determined the true meridian of Washington City there and published about the *Electric Rheometry* with the Smithsonian Institute.[103] Between the times of Franklin and Joseph Henry during the first century of USA, the Americans did not produce first rate scientists.[104]

Gruber's student Dzierożynski[105] was the most successful among the Americans. He began as the professor of mathematics, physics, philosophy,[106] and (dogmatic) theology in Polock Academy. Between the years 1794-1809 he was Gruber's student in Polock. After finishing his philosophical studies he lectured on French language, physics, music, and grammar in Gruber's *Collegium Nobilium* in Petersburg,[107] and after finishing theological studies Dzierożynski lectured on Natural History, Geology, and Mathematics in Mogilev. According to de Maistre's suggestions the Tsar Alexander made Polock Academy the chief Jesuit school in Russia on January 12, 1812. In 1819/20 the Polock Jesuits lectured on the mathematical solving of chemical problems and the trigonometric problems in physics,[108] just before the Tsar exiled Russian Jesuits.

Conclusion

Slovenia is hundreds times smaller compared to USA, Russia, or China. Nevertheless, with Hallerstein and Gruber, Slovenians made their way into Chinese and Russian history. They even wrote some of the most important pages of it because Beijing astronomer Hallerstein published the greatest number of scientific works in Europe. Gruber enabled the continuous development of Jesuit Order in Russia, and eventually tried to reestablish the Chinese and USA Jesuit organization.

The Baron Hallerstein belonged to the nobility on his father side and to the new Barons on his mother's Erbergs side. Gruber was armory's son and therefore a member of the 3rd class. Those differences in origin anticipated their different orientation towards technology&science although as the Jesuit priests they belonged to the same 2nd class. Their achievements are comparable, even if Gruber was an engineer and diplomat, while Hallerstein was astronomic observer and scientist. In his primary scientific orientation Hallerstein was experimentalist, while Gruber mostly relied on technology and inventions. Their birth dates are nearly forty years apart. Hallerstein never met Gruber because he left Europe before Gruber's birth and never returned. As a priest Hallerstein would welcome Gruber's Russian political opportunities, but as a scientist he certainly preferred his privileged position on the top of Beijing Mathematical Bureau. On the other hand, the young Gruber would welcome more influential position in Ljubljana to support his planned building of the Argonaut connection between the Adriatic Sea and Sava River during the early regime of the Emperor Josef the 2nd.

Comparative studies are rare in the history of science especially in our way of parallel research of two different personalities working in different metropolis on non-similar research topics. The only things Hallerstein and Gruber had in common were their mutual Slovene nationality, Jesuit priesthood, 18th century, and success in the East. They eventually had pretty comparable starting positions with similar linguistic-mathematical talents, but varied circumstances decided their different fates and destinies. Gruber wanted to work in China without ever winning a chance to do so, but we could imagine him in Hallerstein's Mathematical Bureau if Jesuit Order would not get suppressed in 1773. Just before the suppression, Gruber accomplished his final wows for the Jesuit order which was just about to collapse. He was predestined to carry the torch of science&technology to the East during the worst times his order ever had. Science&technology for *the greater glory of the Lord* was the motto of Hallerstein and Gruber, and both got their personal glory out of it as the best Mid-European literati ever working in the East.

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[100] Guilday, 1954, 558.

[101] Murphy, 1998, 26.

[102] Devitt, 1909, 21, 29; Jackson, 1878, 218, 223.

[103] Easby-Smith, 1907, 2: 67; McLaughlin, 1899, 99.

[104] Cohen, 1961, 1.

[105] Frančišek Dzeržiniski (Dzierožyniski, * January 3, 1779 Orsha in White Russia; SJ August 13, 1794 Polock; † September 22, 1850 Frederick (Grzebien, 1996, 141; Blinova, 2002, 159, 244)).

[106] Filipowicz, 2004, 16.

[107] Pavone-Tavani, 1995, 170.

[108] Pavone-Tavani, 1995, 180, 190, 193.

References

AEPINUS, Franz Maria Ulrich Theodosius. (1758). *Sermo academicus de similitudine vis electricae atque magneticae.* Petersburg: Typis Academiae.

AEPINUS, Franz Maria Ulrich Theodosius. (1761). Descriptio ac explicatio novorum quorundam experimentorum electricorum. *Novi Commentarii Academiae Scientiarum Imperialis Petropolitanae. 1758-1759.* 7: 277-302. Summary 22-24.

AEPINUS, Franz Maria Ulrich Theodosius. Comment of Beijing experiment. *Phil.Trans*. 8: 276.

AEPINUS, Franz Maria Ulrich Theodosius. (1979). *Aepinus's Essay on the Theory of Electricity and Magnetism*. Princeton: University Press.

AMIOT, Jean-Joseph-Maria. (October 2, 1784). *Mémoires, concernant l'histoire, les sciences, les arts, les moeurs, les usages etc. des Chinois, par les missionnaires de Pe-kin*. Paris: Nyon. 11: 563.

ASEN, Daniel. (2009). 'Manchu Anatomy': Anatomical Knowledge and the Jesuits in Seventeenth- and Eighteenth-Century China. *Social History of Medicine: the Journal of the Society for the Social History of Medicine*. 22/1: 23-44.

BOTERO, Giovanni (* 1540; † 1617). (1607). *La primavera*. Torino: Tarino.

BAROCCI, F. (1572). *Rythmomachia*. Venetia.

BECCARIA, Giacomo Battista. (1767). Novorum quorundam in re electrica experimentorum Specimen, quod Regiae Londinensi Societati mittebat die 26 Aprilis 1766 Joannes Baptista Beccaria, ex Scholis Piis, R.S. Soc. Communicated by M. Maty, Sec. R.S. *Phil. Trans.* 1768. 57/1: 297-311.

BECCARIA, Giacomo Battista. (1769). *Experimenta, atque observationes, quibus electricitas vindex late constituitur, atque explicatur*. Torino: Ex Typographia Regia.

BEHAM, S. (1605). *Wahrhaftige beschreibung alter fürneme künsten*. Frankfurt.

Bernard-Maitre, Henri. (1949). *Un correspondant de Bernard de Jussieu en Cline. Le Père Le Chéron d'Incarville missionaire français de Pékin d'après de nombreux documents inédits (Un correspondent de Bernard de Jussieu en Chine)*. Paris: J. Peyronnet (Cathasia: Série culturelle des Hautes Études de Tientsin). Reprint from October 1948. Archives internationales d'histoire des Sciences. Tome 28: 333-362, 692-717.

BERNARD-MAITRE, Henri. (1948). Catalogue des objects envoyés de Chine par les missionaires de 1765 a 1786. *Bulletin de l'Université Aurore*. Série III, Tome 9: 119-206.

BLINOVA, Tamara Borisovna. (2002). *Iezuiti v Belarusi. Rol iezuitov v organizacii obrazovanija i prosveščenija*. Grodno: Grodnenskii gosudarstvenni universitet.

BRAMER, Benjamin (* 1588; † after 1648). (1616). *Instrument zum rundtlegen*. Marburg: Paul Eenolff.

BRENTEL, Georg. (1615). *Cylinder Solaris Concavum*. Lauingen: Iacob Winter.

CABEO, Nicolo. (1646). *Nicolai Cabei Ferrariensis Societatis Jesu In quatour libros meteorologicorum Aristotelis commentaria, et quaestiones*. Romae: Francisci Corbelletti.

CASATI, Paolo (* 1617; † 1707). (1685). *Fabrica et uso del Compasso di Proportione*. Bologna: Gioseffo Longhi.

CHADWICK, Hubert. (1962). *St Omers to Stonyhurst. A History of two Centuries St Omers, 1593 Bruges, 1762 Liege, 1773 Stonyhurst, 1794*. London: Burns and Oates.

CHALMERS, Alexander (ed.). (1816). *The General Biographical Dictionary*. London: Nichols and Son.

CLIFFORD, Walter. (1822). *Select Propositions of Natural Philosophy, to be Explained and demonstrated by Hon. Walter Cifford... (et al.) Stonyhurst College, July 1, 1822*. Blackburn/Lancashire: T. Rogerson.

COHEN, Bernard. (1961). *Science and American Society in the first Century of the Republic*. Columbus: Ohio State University.

CRESCENTIO ROMANO, Bartolomeo († 1602). (1601). *Navtica Mediterranea*. Roma: Bartolomeo Bonfadino).

CUBA, Johann (flourished 1484). (1546). *Kreutenbuch, vionn aller Kreuter, Bäum, Gestreud vñ Frücht Deszgleichen der Gethier, edlen, gesteyn, Metal und andreren Simplicien unnd Stucken der Artzenei, Natürlichem nutz und gebrauch. Mit aller deren fleissiger Beschreibung, und leblichen Abconterfeytungen. Distilierzeug und Bericht, Kostbarliche Wasser zubrennen, halten und gebrauchen*. Frankfurt: Egenolph.

CVERAVA, G. K. (1986). Dopolnitelni stranici k biografii G. V. Rihmana. *Priroda.* No. 7: 58.

DEHERGNE, Joseph. (1973). *Répertoire des Jésuites de Chine de 1552 a 1800.* Rome: Institutum Historicum S.I.

DENEEF, Alain et all. (1992). *Les jésuites Belges 1542-1992. 450 ans de Compagnie de Jésus dans les Provinces belgiques*. Bruxelles: Association Royale des Ancients Elèves du collèges Sainte-Michel.

DEVITT, Edward I. (1909). Georgetown College in the Early Days. *Records of the Columbia Historical Society* (Washington D.C.). 21-37.

DIXON, Henry William. (1974). *An Historical Survey of Jesuit Higher Education in the United States with Particular Reference to the Objectives of Education*. Dissertation Arizona State University.

EASBY-SMITH, James S. (1907). *Georgetown University in the Disctict of Columbia 1789-1907 its Founders, Benefactors, Officers, Instructors and Alumni*. 1-2. New York/Chicago: Lewis.

EULER, Johan Albert; FRISI, Paolo; BÉRAUD, Laurent (SJ). (1755). Dissertationes selectae Jo. Alberti Euleri, Paulli Frisii, et Laurentii Beraud, qua ad Imperialium Scientiarum Petropolitanam Academiam an. 1755 missae sunt…De causa electricitatis, De causa electricitatis dissertation. Theoria electricitatis. *Academia Imperialis Petropolitana*. Petropoli/Lucae: Vincent Junctin.

FAULHABER, J. (1610). *Newe geometrische Inventiones*. Frankfurt.

FAULHABER, J. (1610). *Ein Niderländische Instrument zum abmessen und Grundtlegen*. Frankfurt.

FILIPOWICZ, Stefan. (2004). *Saga Polskich Jezuitów w USA*. Krakow: Ignatianum.

FOLEY, Henry. (1879). *Records of the English Province of the Society of Jesus*. 5. London: Burns and Oates.

FONTANA, Carlo (* 1634; † 1714). (1696). *Utilissimo trattato dell' acque correnti*. Roma: Gio. Francesco Buagni.

FRISI, Paolo. (1755). *De existentia et motv aetheris sev de theoria electricitatis ignis et lvcis dissertation*.

FRISI, Paolo. (1781). *Opuscoli filosofici* with the second article *Dei conduttori elettrici*. Milano: Giuseppe Galeazzi.

GEMMA Frisius. (1553). *De Principi Astronomiae&Cosmographiae*. Antverpiae: Ioannis Steesii.

GALILEO. (1612). *De proportione instrument*. Argentorati.

GAUBIL, Antoine. (1970). *Correspondance de Pékin 1722-1759, publiée par Renée Simon*. Études de Philologie et histoire. Genève: Libraire Droz.

GOLVERS, Noël. (2006). The Library Catalogue of Diego Valente's Book Collection in Macao (1633). *Bulletin of Portuguese/Japanese Studies (Lisbon)*. December, 13: 7-43.

GOLVERS, Noël. (2010). The Jesuit and the Circulation of Books in the Sciences from Europe to China (17th-18th Cent.). *Book of Abstracts, Congrés internacional ECHS, Barcelona.* November, 47.

GRZEBIEN, Ludwig. (1996). *Encyklopedia wiedzy o jezuitach na zieniach Polski i Litwy 1564-1995*. Kraków: Wydzial filozoficzny towarzystwa jezusowego.

GUAN, Zengjian. (2005). The Historical Evolution of the Theory of Compass in China. *Studies in the History of Natural Sciences (Zi ran ke xue shi yan jiu)*. 24/2: 128-143.

GUILDAY, Peter. (1954). *The Life and Times of John Carroll, Archbishop of Baltimore, 1735-1815*. New York: Newman Press.

HACOB, Simon († 1564). (1600). *Rechenbuch*. Frankfurt: Egenolph.

HEILBRON, John L. (1979). *Electricity in the 17th and 18th Centuries.* Berkeley: University of California Press.

HELBACH, Friederich. (1604). *Oenographia oder Kunstbuch von Wien*. Frankfurt: Beckern.

HOLT, Geoffrey. (1984). *The English Jesuits 1650-1829. A Biographical Dictionary.* Catholic Record Society.

HOLT, Geoffrey. (2003). A Bicentary: 1803-2003. *AHSI*. Januar-Junij. 72/143: 105-126.

HOOKE, Robert. (1665). *Micrographia: or some physiological descriptions of minute bodies made by magnifying glasses with observations and inquiries thereupon*…London: John Martyn.

HSIA, Florence C. (2009). *Sojourneus in a strange land: Jesuits and their scientific missions in late imperial China*. Chicago: University Press.

HUGHES, Thomas. (1910). *History of the Society of Jesus in North America Colonial and Federal. Documents Volume I Part II Nos. 141-224 (1605-1836)*. Cleveland: The Burrows Brothers Company.

HULSIUS, Levinus. (1603-1605). *Der mechanischen Instrumenten.* Franckfurt: Richter.

HUMBOLDT, Friedrich Wilhelm Heinrich Alexander baron. (1814-1825). *Relation historique du Voyage aux Régions équinoxiales du Nouveau Continent*. Paris. Reprint: 1970. Stuttgart: Brockhaus.

IANNACCONE, Isaia. (1998). *Johann Schreck Terrentius*. Napoli: Instituto Universario Orientale.

IANNACCONE, Isaia. (2005). Documents relatifs à l'histoire de l'astronomie Chinoise et aux raports scientifiques entre l'Europe et la Chine (XVII^{e}-XIX^{e} siècles) conservés à la bibilothèque de l'observatoire de Paris. Second part. *Nuncius*. 20/2: 371-399.

INGLOT, Marek. (1997). *La Compagnia di Gesù nell'impero Russo (1772-1820) e la sua parte nella restaurazione generale della Compagnia*. Roma: Editrice Pontificia Università Gregoriana.

INGLOT, Marek. (2002). Gabriel Gruber, S.J.(1740-1805): nel bicentenario della sua elezione a generale della Compagnia di Gesù. *Archivum historicum Societatis Iesu*. 71/142: 353-368.

JACKSON, Richard Plummer. (1878). *The Chronicles of Georgetown, D.C. from 1751 to 1878*. Washington: Polkinhorn.

JAMES, William Alexander. (1977). *Paul I and the Jesuits in Russia*. Univesity of Washington: Dissertation.

KEPLER, Johannes. (1611). *Dioptrice, seu, Demonstratio eorum quæ visui&visibilibus propter Conspicilla non ita pridem inventa accident: praemisae epistolae Galilaei de ijs*… Augustae Vindelicorum: David Franci.

KEPLER, J. (1627). *Tabulae Rudolphinae, quibus astronomicae scientiae temporum longinquitate collapsae Restauratio continetur; A Phieniceillo Astronomorum Tychone Braheorum... familia... primum animo concepta et destinata... ; tabulas ipsas... perfecit, absolvit... traduxit Joannes Keplerus*. Ulmae: Jonas Sauris.

KIRCHER, A. (1667). *Athanasius Kircheri e Soc Iesu, Magneticum Naturae regnum, sive disceptatio Physiologica De triplici in Naturam rerum Magnete, iuxta triplicem eiusdem Naturae gradum digesto Inanimato Animato Sensitivo Quae Occultae prodigiosarum quarundam motionem vires et proprietates, quae in triplici Naturae Oeconomia nonnullus in corporibus noviter detectis observantur, in apertam lucem eruuntur, et luculentis argumentis, experientia duce, demonstratur. Ad Inclytum, et Eximium Virum Alexandrum Fabianum Novi orbis Indigenam*. Romae: Ignatii de Lazaris.

KLOSS, Albert. (1987). *Von der Electricitaet zur Elektrizität*. Basel-Boston: Birkhäuser.

KNAUST, Heinrich (16th century). (1614). *Fünff Bücher, Von der Göttlichen und Edlenn Gabe, der Philosophischen, hochthewren und wunderbaren Kunst, Bier zu breawen. Auch von Namen der vornemesten Biere, in gantz Teutschlanden, und von deren Naturen, Temperamenten, Qualitaten, Art und Eigenschafft…* Erfurt: Nicolaus Schmuck (the first edition 1575. Erfurt)

KOPLEVIČ, Judifa Haimovna, CVERAVA, G. K. (1989). *Hristian Gotlib Kratzenstein 1723-1795*. Leningrad: Nauka.

KOŠIR, Matevž. (1998). Prostozidarstvo v habsburški monarhiji v 18. stoletju ter prostozidarski loži "Združena srca" v Mariboru in "Dobrodelnost in stanovitnost" v Ljubljani. *Kronika.* 46: 41- 65.

KUZNIEWSKI, Anthony J. (1992). *Francis Dzierozynski and the Jesuit Restoration at the United States.* Connecticut: The Fiedorczyk Lecture in Polish American Studies April 5, 1988. 1-22; *The Catholic Historical Review*. 78/1: 51-73.

De MAISTRE, Joseph. (1995). *Peterburgskie pisma 1803-1817*. S.-Peterburg: Inapress.

MANFREDI, Eustachio (* 1674; † 1739); ZANOTTI, Eustachio. (1750). *Ephemerides Motuum Coelestium, ex anno 1715 ad annum 1725*. 1-3. Bononiae.

MCDONOUGH, Peter; BIANCHI, Eugene C. (2002). *Passionate Uncertainty. Inside the American Jesuits*. Berkeley: University of California Press.

McLaughlin, J. Fairfax. (1899). *College Days at Georgetown*. Philadelphia: Lippincott Company.

Mei, G. (1602). *La Musica antica e moderna*. Venetia.

Mihnevič, D.E. (1955). *Očerki iz istorii katoličeskoi reakcii (iezuiti).* Moskva: Izdajateljstvo Akademii nauk SSSR.

Moroškin, Mihail Jakovlevič (* 1820; † 1870). (1867). *Iezuiti v Rossii: s carstvovanija Ekaterini II-i i do našego vremeni. Čast 1.* Sankt-Peterburg: E. I. V. Kancelari. Reprint: 1888. Peterburg: Tipografija tovariščestva "Obščestvennaja Poljza".

Moroškin, Mihail Jakovlevič. (1870). *Iezuiti v Rossii: s carstvovanija Ekaterini II-i i do našego vremeni. Čast vtoraja, obnimajuščaja istoriju iezuitov v carstvovanije Aleksandra I-go*. Sankt Peterburg: Vtoro Otdelenije Sobstvenoi Ego Imperatorskog Veličanstva Kancelarii.

Murphy, G. Ronald. (1998). Georgetown's Schield: Utraque Unum. *Splendor and Wonder Jesuit Character, Georgetown Spirit, and Liberal Education* (ed. O'Brien, William J.). Washington: Georgetown University. 23-38.

Musschenbroek, Pieter. (1779). *Elementa Physicae.* in three editions with the older ones bearing *Nova acta academ. Scient. Petropoli* 1779, 7: 22.

Niu, Yahua. (2006). A Study on Two Earliest Translations of Western Anatomy. *Studies in the History of Natural Sciences (Zi ran ke xue shi yan jiu)*. 25/1: 50-65.

Novik, V. K. (1999). Akademik Frants Epinus (1724-1802): Kratkaja biograficheskaja kronika. *VIET.* 4: 4-35.

Pascal, Blaise (Louis de Montalte). (1656-1657). *Les Provinciales, ou letters écrites par Louis de Montalte à un provincial de ses amis*…Köln.

Pavone-Tavani, Sabina. (1995). Accademia di Polock, collegi gesuiti e riforme statali in Russia all'inizio del XIX secolo. *Pontificum Institutum Orientalum Studiorum*. 61/1: 163-193.

Peck (Zamek-Gliszcznska), Ana. (2001). Between Russian Reality and Chinese Dream: the Jesuit Mission in Siberia 1812-1820. *The Catholic Historical Review*. 87-1: 17-30.

Peyrefitte, Alan. (1991). *Un choc de cultures. La vision Chinoise*. Paris: Fayard.

Pfister, Louis. (1732, 1734). *Notices biographiques et bibliographiques sur les Jésuites de l'ancienne mission de Chine*. I-II. Chang-hai: Imprimerie de la Mission Catholique.

Priestley, Joseph. (1775). *The History and Present State of Electricity, With Original Experiments*. London: Bathurst&all.

Pray, Georgius. (1781). *Imposturae CCXVIII in dissertatione r. p. Benedicti Cetto, Clerici Regularis e Scholis Piis de Sinensium imposturis detectae et convulsae. Accedunt Epistolae anecdotae r. p. Augustini e comitibus Hallerstein ex China scriptae*. Budae: Typis Regiae Universitatis.

Rinaldi, Bianca Maria. (2006). *The "Chinese Garden in Good Taste" Jesuits and Europe's Flora and Art of the Garden in the 17th and 18th Centuries*. München: Martin Meidenbauer.

Rost, Johann Leonhard (* 1688; † 1727). (1727). *Der Aufrichtige Astronomus*. Nurnberg.

Rouët de Journel, Marie Joseph. (1922). *La Compagnie de Jésus en Russie: un college de Jésuites a Saint-Pétersbourg, 1800-1816*. Paris: Librarie Académique Didier.

Rule, William H. (1853). *Celebrated Jesuits. Vol. II A Cardinal, a Mandarin, and a Refugee*. London: John Mason.

Schlafly, Daniel L. (1999). The Ratio Studiorum on alien shores: Jesuit Colleges in St. Petersburg and Georgetown. *Revista Portuguesa de filosofia.* 55/3: 253-274.

Schroth, Raymond A. (2008). *Fordham: a History and Memory*. New York: Fordham University Press.

van Scrieck, Andriaen (* 1560). (1614). *Vant t'beghin der eerster volcken van Evropen*. Ypres.

Spalding, Thomas W.; Thomas, Paul K. (2000). *John Carroll Recovered Abstracts of Letters and Other Documents not Found in the John Carroll Papers*. Baltimore: Cathedral Foundation Press.

Steska, Viktor. (1905). P.Gabriel Gruber. *MMK.* 43-46.

SYMMER, Robert. (1759). New Experiments and Observations Concerning Electricity. *Phil. Trans.* 1760. 51/1: 340-389.

VERHAEREN, Hubert. (1969). *Catalogue de la Bibliotheque du Pe-t'ang*. Paris: Societé d'Édition Les Belles Lettres.

VELLI, S. (1573). *Della proportione e proportionalita*. Venetia.

VOLTA, Alessandro. (1816). *Collezione dell'opere del Cavaliere Conte Alessandro Volta*. Tomo I. Parte I. Firenze: Guglielmo Piatti.

WALRAVENS, Hartmut. (2001). The Qiqi Tushuo revisted. *Missionary approaches and linguistic in mainland China and Taiwan* (ed. Ku Wei-ying). Lueven: University Press. 183-198.

Woodstock Letters, (1886), 15: 117.

ZALESKI, Stanislaw. (1886). *Les Jésuites de la Russie-Blanche*. Paris: Letouzeu et Ané. I-II.

ZALESKI, Stanislaw. (1900-1905). *Jezuici w Polsce*. Lwów: Drukarna Ludowej. Reprint: 1905. Krakow: W.L. Anczyca i Sp. I-IV.

ZANOTTI. (1750). Ephemerides.

Archival and Web Materials

ARSI (Archivum Romanum Societatis Jesu), Russia, »Acta Congreg. Gen. Russia«, box 1027, folio 148^{r}-149^{r}.

ARSI, Russia: 4A, Epist Vic. Gen. In Russia, 1802-1808.

Maryland-New York Province Archives, May 12, 1804.

http://www.library.georgetown.edu/dept/speccoll/cl269.htm box 23, folder 38.

CARTESIAN PHYSICS (AS EXPERIMENTAL PHILOSOPHY) AND ITS UNIVERSITY RECEPTION

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AN EPISTOLARY LAB: THE CASE OF PARHELIA AND HALOS IN DESCARTES' CORRESPONDENCE (1629-1630)[1]

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Abstract

The aim of this paper is to provide insight on Descartes' experimental philosophy as it is recorded in his correspondence. Because Descartes articulates very precisely experience with theory, he excludes any random collection of experimental data if they cannot be inserted into a theoretical framework. From the record of the apparition of parhelia in 1629 in Rome, the epistolary communication between Mersenne, Descartes and Gassendi presents us three different attitudes toward observation, its interpretation and the possibility to relate it to other phenomena.

Descartes' physics has often been seen as a mere theoretical work or, even worse, as an imaginary production, far from being in touch with experimental reality.[2] Nevertheless, if we pay more attention to Descartes' own words on the topic, we may draw quite a different picture of his natural philosophy. In several of his works, experience is theorized as a way to choose between several competing explanations of physical phenomena, all being coherent with physical principles and equally possible from a theoretical point of view. But, useful as they might be to understand the role experience can play from a theoretical point of view in Descartes' physics, these texts do not always make fully explicit what precise role experience plays in the concrete making of Cartesian natural philosophy, nor what Descartes considers to be a valid experiment. To have a more accurate view on this, we must turn to Descartes' correspondence. There we discover him commenting on phenomena, discussing of several experimental devices or fully realized experiments, some of them being notably performed by others. This considerably challenges the picture Descartes built of himself as an independent thinker[3] who would have isolated himself from the world to think with his own intellectual means.

[1] Research for this paper was made possible by NWO, the Dutch Organisation for Scientific Research. I would like to thank Mihnea Dobre and Tammy Nyden for their useful suggestions on an earlier draft of this paper.

[2] Cf. Christiaan Huygens, *Œuvres complètes*, The Hague, Nijhoff, 1905, X, p. 403; Voltaire, *Le Siècle de Louis XIV*, Frankfurt, Knoch & Eslinger, 1753, III, pp. 53-54.

[3] Peter Dear depicts Descartes as "the apostle of the individual knower" (*Discipline and Experience. The Mathematical Way in the Scientific Revolution*, Chicago and London, The University of Chicago Press, 1995, p. 94).

The collection of experimental data in relation to Descartes' epistemology

It is difficult to establish a clear demarcation between experiment and passive observation in Descartes' texts.[4] From a linguistic point of view, Descartes establishes no difference between "experience" and "experiment". Whereas he could have used, at the time, the French word "experiment",[5] he does not so. "Expérience" therefore refers, for him, to simple observations as well as to contrived experiments. This can be accounted for, as we are going to see, by Descartes' double appeal to experience.

Indeed, experience and experiments play a central role, in Descartes' natural philosophy, because an *a priori* natural philosophy cannot be deductively established in all its details and because, as a consequence, Descartes has to rely on a methodical dialectics of effects and causes. From the general principles of matter and motion established by metaphysics, it is possible to elaborate several possible explanations which account for the phenomena. But to decide what explanation is the one really working in nature, one has to appeal to experience.[6]

In a letter to Morin, Descartes indicates: "I should add also that there is nothing circular in proving a cause by several effects which are independently known, and then proving certain *other* effects from this cause."[7] Experience plays then a role at two levels: first to collect facts produced in nature to go from them to the explanatory structure of bodies, and second to conceive other effects which can be produced by such a set of mechanical properties. The mechanical structure which has gained plausibility from its compatibility with the phenomena from which it was designed, will gain further confirmation in its capacity to agree with other phenomena which are different from the first ones.

As a consequence, experience has no value independent of the possibility that it be linked to all the phenomenal aspects of the world through a kind of demonstration or series of demonstrations.[8] Descartes defends a holistic account of natural philosophy in which experience serves to complete the whole of natural philosophy. In the Lettre-préface to the *Principles of Philosophy*, he writes:

> "I am also very well aware that many centuries may pass before all the truths that can be deduced from these principles are actually so deduced. For the majority of truths remaining to be discovered depend on various particular observations *which we never happen on by chance* but which must be sought out with care and expense by very intelligent people."[9]

In that case (which corresponds to the second level of experience), Descartes does not consider experiments as what one can find by chance when "looking around" but experiments are supposed to answer specific purposes in the search for truth in natural philosophy. They must therefore be designed according to theories and hypotheses already conceived.

The corollary of this valorisation of experience depending on theoretical prerequisites is the critics of random experiences. It is not legitimate, for Descartes, to rely on approximate experimental results to invalidate a law. This is for example the case with the law of refraction: in 1639, he criticizes, in a letter to Mersenne, an opponent for trying to refute his law through experimental measurements of refraction by saying that this endeavour is not better than "to show, with a poorly-constructed set-square, that the three angles of a triangle are not equal to two right angles."[10] Demonstrations in optics are comparable to those in mathematics and, for this reason, are not liable to mere empirical refutation. But, on the reverse, it is not absolutely pointless to get experimental confirmation of the law of refraction, once it has been theoretically demonstrated. This is the reason why Descartes thanks Debeaune for his exact measures of refraction.[11] In this case, the results confirming Descartes' law of refraction are a proof that the experiment was correctly conducted. On the other hand, in 1632,[12] Descartes himself devises, for Golius, such an experiment to verify his sine law. He details to his correspondent an experiment to measure refractions from air into water. Note that this is not an experiment he seems to have performed himself. But Descartes imagines a specified use for this device, in agreement with his own law of refraction. The procedure does not

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[4] Cf. Desmond Clarke, *Descartes' Philosophy of Science*, Manchester, Manchester University Press, 1982, pp. 19-24.

[5] Mydorge uses this word in a letter to Mersenne (13 November 1638) to refer to tests of Galileo's work: *Correspondance du Père Marin Mersenne*, Paris, CNRS, 1932-1988, V, p. 527 (abridged hereafter CM).

[6] *Discours de la méthode*, in *The Philosophical Writings of Descartes*, Cambridge, Cambridge University Press, 1985-1991, I, p. 144 (abridged hereafter CSM or CSMK for the third volume): "seeking further observations whose outcomes vary according to which of these ways provide the correct explanation". See also *Principia philosophiae*, III, 46, in *Œuvres de Descartes*, Paris, Vrin, 1996, VIII-1, pp. 100-101 (abridged hereafter AT).

[7] Letter to Morin [13 July 1638], CSMK, p. 106 (our emphasis).

[8] Cf. Descartes' letter to Regius from July 1645 in which Descartes insists on the need of "*probationes*" (AT IV, p. 245).

[9] CSM, I, p. 189; AT IX-2, p. 20.

[10] AT II, p. 497.

[11] Descartes to Debeaune, 30 April 1639, AT II, p. 542.

[12] Cf. Descartes to Golius, 2 February 1632, AT I, p. 237-240.

consist in listing in tables some measurements obtained directly with the instrument through an inductive process,[13] but to anticipate by a geometrical construction what is to be seen on a ruler immersed in water. He advises Golius to draw first on a ruler the marks according to which refraction is supposed to happen according to a geometrical construction elaborated from Descartes' law of refraction. Then the ruler is immersed into water so that the experimenter can observe the coincidence between the geometrical construction and the physical phenomenon. The experimental device is therefore only meant to read into experience what has already been deduced and drawn by optical theory and geometry.

Descartes in his experimental network

As we can see with the examples of Debeaune and Golius, Descartes is also interested in experiences he does not perform himself. Among the experiences discussed in Descartes' correspondence, one finds comments on experimental records as much as on experiments really performed by Descartes himself. This is made possible mainly through Mersenne's large epistolary network which allows Descartes to get more experimental data than the experiences he can perform by himself could provide him with.

Apart from the well-known role Mersenne played in scientific communication across Europe and from his support to experimental philosophy, Descartes' correspondence can give us an accurate insight on what the philosopher considers to be important in experience by comparison with what other experimenters do and with what they find relevant for natural philosophy. In these epistolary exchanges, this is not only the way scholars communicate on experimental results which is to be seen, but also several divergent aspects of the status of experience in natural philosophy in the first half of the seventeenth century.

For Descartes, as we have seen, only experiences that could be integrated into a coherent study of nature relying on theoretical assumptions could be relevant, regardless of any attraction for curiosities. Therefore, the stake was to integrate discrete events into a more general "system of the world,"[14] so as to access a universal knowledge of nature. Particular phenomena are the occasion for natural philosophers to enrich their general framework and to apply it to specific aspects of nature. But from a traditional, that is to say Aristotelian, point of view, experience was related to the way things happen in nature in general and not to the way a particular event had occurred. This conception of experience changes during the 17th century, but most of the time we can find a mix of these two conceptions of experience, even often in the same author's writings. When they are not part of an every-day experience which can be easily recognized by everyone, experiments are not evident data which, by themselves, can confirm general theories on nature. Observations are not always unproblematic, even if they do not imply any specific technical apparatus or any determinate experimental procedure. This is eminently the case when observations are actually *reports* of observations. Reports always mean a selection of what is considered to be the set of appropriate data by opposition with superfluous or contingent or inessential data. In that case, the natural philosopher has to distinguish between important elements and irrelevant details which do not allow elaborating a scientific explanation.

Moreover, the reported observations first have to be established as facts, that is to say as serious pretenders to integrate the realm of experience, before they can be dealt with in a scientific manner. Correspondence is particularly suited to record and report discrete events, detailing their appearances and the circumstances under which they occur. But, at the same time, a letter is only a substitute for a direct contact with the experience in question. Therefore, what is at stake is to pick out what the correspondents consider to be the appropriate elements to be communicated about an experience. To give more plausibility to observations, reports of experience could also be constructed as narrative, and not as mere description of facts.[15]

From this point of view, the phenomenon of parhelia which we will examine more in detail is very revealing, because it was reported as a spectacular and rare event, nearly an historical one (the title of Gassendi's explanation published in Amsterdam indicated for example on what date it occurred).[16] But, on the other hand, it was integrated into a general explanation relying on optical theories, on matter theories and these approaches were tentatively extended to other so-claimed related phenomena (like halos, rainbows and candles).

[13] I do not intend to claim that Witelo or Alhazen followed such an inductive procedure themselves. On optical tables which were not the resultant of raw observation, but more often of a logarithmic formula, cf. Dear, *Discipline and Experience*, *op. cit*., p. 81.

[14] Descartes to Mersenne, 12 September 1638, AT II, p. 355.

[15] Cf. Peter Dear, *Discipline and Experience, op. cit*., p. 75.

[16] *Phaenomenon rarum et illustre Romae observatum 20 Martij Anno 1629*, etc., Amsterdam, Gerard, 1629.

The example of halos and parhelia: from the apparition in Rome in 1629 to the *Météores*

I would like now to turn to a case study at the crossroads of optics and meteorology in Descartes' correspondence: the explanation of the parhelia of 1629. Whereas optics was considered by scholastic philosophy as mixed mathematics (a branch of geometry), meteorology belonged to a more physical and uncertain realm. The conjunction of both domains in the study of this phenomenon testifies to the change occurring in scientific boundaries at the time. Noteworthy is then the way experience is articulated with theoretical analysis in the explanation of such a phenomenon. One also has to underline that optics itself is then evolving from the domain of geometry to one of a mix between mathematics and natural philosophy.[17] And Descartes himself presents his *Dioptrique* as a mix of mathematics and (natural) philosophy.[18]

What is most striking is that the 1629 parhelia are studied by numerous scholars throughout Europe, whereas most of them did not see the phenomenon. All the explanations therefore develop from reports of observation, exchanges of letters, and reconstructions based on comparisons with other observations actually performed.

On March, 20th, 1629, five suns shone in Rome. Cardinal Barberini sent a drawing and description of the display to Peiresc[19] who in turn sent some copies to the brothers Dupuy[20] and Gassendi.[21] On 14 July 1629 Gassendi had given an explanation and description of the phenomenon of the parhelia to Reneri, one of Descartes' friends. This letter was published first by Reneri,[22] and then by Gassendi himself.[23] The description of the phenomenon might be based on the observation made by the Jesuit Christoph Scheiner.[24] Descartes was intrigued by the phenomenon and decided to give his own explanation. Perceiving a similarity between the appearance of the parhelia and that of a rainbow, he was then led to study all sublunar phenomena.[25] His explanation of parhelia will later be published in the *Météores*.[26]

Let us now turn to the subsequent epistolary exchanges it gave rise to between Descartes and Mersenne. On 18 December 1629 and in January 1630, Descartes answers to letters from Mersenne in which the Minim reports the observation of coloured circles around a candle.[27] At first, Descartes seems sceptical about those observations. Mersenne had apparently tried to connect this phenomenon to the luminous circles appearing around the sun, like in the case of halos appearing together with parhelia. But Descartes rejects this analogy on the ground that the order of the colours is different in the two cases.

The rejection of Mersenne's analogy seems nevertheless to conflict with Descartes' later claim in a letter to Morin: "I maintain, therefore, that analogies of this sort are the most appropriate means available to the human mind for laying bare the truth in problems of physics."[28] Descartes requires that any explanation in physics be based on one analogy. But this methodological requirement is rooted in his metaphysical conception of matter as being one and the same everywhere and at every scale. What is therefore at stake is to find the right analogy. This shows that, for Descartes, one should not make a too widespread and thoughtless use of analogy when comparing our visual data, but a moderate and reasoned one. This reasoned use of our senses to collect data gives rise to a certain attitude of the natural philosopher. He is the one who has learnt to see, as is manifest from what Descartes declares in the *Sixth Replies*:

> "[W]hen we say 'The reliability of the intellect is much greater than that of the senses', this means merely that when we are grown up the judgements which we make as a result of various new observations are more reliable than those which we formed without any reflection in our early childhood [...]."[29]

[17] This is also true among Aristotelian philosophers: cf. Aguilonus, *Opticorum libri sex*, Antwerp, Plantin, 1613, preface, s.p.

[18] Descartes to unknown, [27 April 1637?], AT I, 370.

[19] Cf. Peiresc to Dupuy, 16 April 1629, in *Lettres de Peiresc publiées par Philippe Tamizey de Laroque*, Paris, Imprimerie Nationale, 1888-1898, II, pp. 72-73.

[20] Cf. Peiresc to Dupuy, 20 April 1629, in *ibid.*, II, p. 76.

[21] Cf. Gassendi to Peiresc, 21 July 1629, in *ibid.*, IV, p. 201.

[22] Letter of [14] July 1629, published as *Phaenomenon rarum et illustre Romae observatum 20 Martij Anno 1629*, etc., *op. cit.*

[23] *Parhelia, sive soles quatuor qui circa verum apparuerunt Romae die XX mensis martis, anno 1629, et de eisdem Petri Gassendi ad Henricum Renerium epistola*, Paris, Vitray, 1630.

[24] But there is no definite textual evidence about Scheiner as being the source of the observation: cf. Walter Tape, Eva Seidenfaden, Gunther P. Können, "The Legendary Rome Halo Displays", *Applied Optics*, Vol. 47, No. 34, 2008, H72-84.

[25] Descartes to Mersenne, 8 October 1629, AT I, 23; Descartes to Mersenne, 13 November 1629, AT I, p. 70.

[26] AT VI, p. 355.

[27] AT I, pp. 83-84, 97-99, 106.

[28] Descartes to Morin, 12 September 1638, CSMK, p. 122.

[29] CSM II, p. 295.

From this point of view, so-far unseen observations play a privileged role in the constitution of a natural philosophy, since they are not liable to the same precipitation of judgement, because of their lack of familiarity to the observer. On the contrary, the false evidence of an experience which has not enough been analyzed by reason is a dangerous trap.[30]

So why is Descartes sceptical about Mersenne's observation in that case? Mersenne apparently tried to sustain his claim by referring to the testimony of an observation made by Gassendi, which indicates that the Minim appeals to a consensual and collaborative conception of experience. Mersenne apparently also tried to insist on Gassendi's talents as an observer who claims to have observed several times that, in the case of parhelia, the apparent diameter of the corona is of 45°. That is indeed what Gassendi had conjectured in his *Phaenomenon rarum*[31] and Descartes himself is ready to agree with that specific value, without excluding others. Kepler had already noticed that the diameter of the coronas appearing around the sun was of 45°.[32] That Descartes and Gassendi take up this value is certainly due to their reading of Kepler as much as to real observations. While rejecting as impossible Mersenne's observation of the colours around a candle, Descartes manages to account for Gassendi's by assuming that he took for one two coronas which were partly confused.

As I noted, Mersenne must have alluded to an experience attributed to Gassendi to sustain his analogy between the corona around a candle and solar halos. This "experience" can indeed be related to Gassendi's description of coronas around the sun associated to the phenomenon of parhelia. In his letter to Reneri,[33] Gassendi reports that the sun was circled by two incomplete rainbows of diverse colours. Descartes, to whom this text was transmitted by Reneri, here claims that, in agreement, as it seems, with Gassendi's experience, every corona or iris displays more or less the colours of the rainbow, and not the colours Mersenne claims to have seen around the candle.[34] The colours listed by Descartes in his letter to Mersenne are indeed close to the ones Gassendi identifies in a solar halo observed in 1623: red in the inner part, then yellow, and green in the outer part.[35] This observation is certainly the basis for Gassendi's conjecture on the types and order of colours of the coronas observed together with parhelia in his explanation of a phenomenon he has not himself directly observed.[36] Mersenne seems to have had access to some of these so far unpublished reports of observations made by Gassendi[37] and in May 1630 he will send Gassendi's description of a corona to Descartes.[38] The information that was available to Gassendi lacks precision as to the diameters and thickness of halos, the distance between the suns and their size, as Gassendi himself recognizes.[39] One therefore understands that he relied on his collection of former, related observations to enrich the description of the 1629 parhelia. Gassendi thus appealed to analogy between real and reported observations to build his explanation, whereas Descartes argues from a more theoretical basis, postulating a homogeneity between determinate optical phenomena —this postulated link being the source of *Les Météores*— and *afterwards* using Gassendi's supposed observations to reinforce his position.

Descartes eventually accounts for coronas around a candle by a partial dispersion of light inside the eye (what he calls *lumen secundarium*).[40] The source of the parallel Descartes draws between his conception of secondary light rays inside the eye and the light dispersed around the main ray when entering a dark room through a small hole certainly is Witelo's definition of secondary light in his *Perspectiva*.[41] But here Descartes modifies the meaning of the term to apply it to light rays inside the eye. Deviating from the straight line along which the main light ray is refracted in the crystalline, some secondary light rays coming from the same source can be spread and impressed on the retina around the image-point created by the main light ray, giving rise to the optical illusion of coronas around the candle.[42] According to Descartes, this phenomenon is only due to a specific modification of some internal parts of the eye (provoked for example by a pressure exerted on it which could modify the crystalline's configuration).[43] Descartes appears close to Galileo who also relies on the eye's configuration and on the refraction of light rays inside the eye to account for coronas seen around candles.[44]

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[30] Cf. Annotationes to the *Principia philosophiae*, AT XI, p. 654; see also *Description du corps humain*, AT XI, p. 242.

[31] *Opera omnia*, Lyon, Anisson & Devenet, 1658, III, p. 653.

[32] Cf. *Ad Vitellionem Paralipomena*, in *Gesammelte Werke*, C.H. Beck'sche Verlagsbuchhandlung, Munich, 1939, II, p. 142

[33] *Opera omnia*, *op. cit.*, III, p. 651.

[34] Cf. also *Météores*, AT VI, pp. 333-334, 350.

[35] Cf. *Commentarii de rebus caelestibus, in Opera omnia, op. cit.*, IV, p. 92.

[36] Cf. *Opera omnia*, *op. cit.*, III, p. 655.

[37] They will be published as an appendix to the *Epistolica exercitatio* later in 1630.

[38] See Descartes to Mersenne, 6 May 1630, AT I, p. 148

[39] Gassendi to Peiresc, 15 June 1629, Carpentras, Bibliothèque Inguimbertine, ms. 1832, fol. 15 r.v.-16 r.v.

[40] Descartes to Mersenne, January 1630, AT I, p. 106.

[41] *Witelonis Perspectivae Liber secundus et Liber tertius*, transl. S. Unguru, Wroclaw, Warsaw, Cracow, The Polish Academy of Science Press, 1991, p. 39.

[42] Cf. *Météores*, AT VI, pp. 351-354.

[43] Cf. *Météores*, AT VI, p. 352.

[44] *Saggiatore*, in *Opere*, Edizione nazionale, Florence, Barbèra, 1896, VI, p. 357.

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Descartes' correspondence thus appears as a laboratory where he does not only elaborate his concepts and theories, but also where he "experiments" with his correspondents on matters of natural philosophy through the analysis of observations. But several figures of the observer appear throughout that correspondence. Mersenne, the eclectic incautious observer, reports as much as he can what he has seen, without being really able to analyze his observations. Gassendi is a systematic collector of singular observations, but he is also able to compare them to elaborate explanations. Descartes is the most theoretical experimental philosopher of the group: he needs to gather experimental data (which corresponds to the first level in his methodology) but accepts observations when they are at least coherent with some theoretical positions (his own as well as others' like Witelo and Galileo).

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THE EMPIRICAL ELEMENT IN DESCARTES' PHYSICS AND ITS RECEPTION BY SPINOZA

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Abstract

Cartesian physics adopts an epistemologically realistic worldview based on the doctrine of the truthfulness of clear and distinct ideas. But the application of this doctrine on physics is closely related to the demonstration of the existence of material objects and thus implies an allusion to experience. Thus, Descartes' theory concerning the principle of material objects, although claiming to be a mathematical theory, seems to allow experience to play a crucial role. In his first published book, Spinoza gives a "more geometrical" transcription of Descartes' metaphysics and physics. Spinoza follows a rigid deductive method leading in a deterministic way from the first principle to their consequences and his exposition of Cartesian physics relies strongly on a purely intellectual grasp of the nature of corporeal substance that our senses cannot reveal. But things become more complicated once we realize that Spinoza's text contains some elements that are not of a purely rational origin.

Introduction

One of the main features of the Cartesian natural philosophy is reputed to be the absence of a rigorous experimental methodology and of facts originating from experience. There are, of course, several points in Descartes' work that contradict such a generalization. I think that the role of experience has indeed often been underestimated in the secondary Cartesian literature, as the image usually found in this literature is the grotesque image of Descartes constructing his physical science with nothing more than elements of a purely rational origin. I do not intend to reproduce this conception of Cartesian science, and anyway my paper will not focus on the general features of Cartesian science. My aim is a much more limited one, as I intend to examine the impact of some aspects of Cartesian science on the work of Spinoza and to show that at least in some cases the absence of the empirical element in Cartesian science has led Spinoza to results that can be taken to be the ultimate consequences of the absence of an elaborated experimental attitude.

General remarks

It is a well-known fact that Descartes is the philosopher who proposed and organized in a rigorous manner the mechanical worldview. He is the philosopher who deliberately worked on a theory providing the metaphysical basis for the conception of matter as pure geometrical extension. For Descartes matter is identified with an extension that does not possess any qualitative differentiations. Such a reduction of matter to geometrical extension and of all the qualities of the former to properties geometrically defined is attained at the cost of a strict dualist distinction between body and mind.

The material world, thus, consists according to the Cartesian mechanical worldview in nothing more and nothing less than geometrically conceived extension. Descartes cleanses matter of any property that cannot be translated into the vocabulary of geometry. In other words, bodies do not have the substantial forms that according to medieval scholastic philosophy constitute the element of individualization. Also, bodies do not possess any internal tendency to accomplish a task, a goal. Cartesian physical philosophy excludes any kind of teleological explanation since bodies obey only mechanical necessity, the necessity of extension, motion, and figure. Motion and rest thus become, ontologically speaking, mere modes of a substance, that is states of bodies moving in a homogeneous Euclidean space. But such a conception leads inevitably to the problem of the cause of motion as long as geometrical entities do not possess an internal principle or a tendency which puts them in motion. Given this, it can be inferred that matter is by its nature in rest and must be put in motion by an external cause[1].

The cause that sets matter in motion according to Cartesian theory is of a transcendent nature. It is God who bestows motion to matter, and He does it as He recreates at every instant all that there is in the world. As Descartes says in his work *The Principles of Philosophy*, "from the fact that we now exist, it does not follow that we shall also exist a moment from now, unless some cause (that is, the same one as that which first produced us) continually produces us, as it were, anew; that is, conserves us."[2]. What is at stake here is Descartes' theory of continuous creation, a theory playing a significant role in his physics[3]. Because God does not only recreate at every instant all that exists but also "in now maintaining the world by the same action and with the same laws with which He created it, He conserves motion"[4]. Such a statement proceeding from Descartes' pen has multiple effects on his conception of motion, but it will be enough to note here that for the Cartesian physical philosophy the action through which God maintains the world is at the origin of the laws that govern the distribution of motion and rest between bodies.

The whole structure of Descartes' natural philosophy finds its basis in a metaphysical theory. This theory sets the traditional problems of the theory of knowledge on new ground. Descartes' theory of knowledge consists above all of a new way of defining the thinking substance and man's ability to grasp certain knowledge. The completion of the enterprise is described in a text emblematic for seventeenth century metaphysics, the *Meditations on first philosophy*. This text, divided into six meditations that each mark a step towards the conception of indubitable knowledge, has as its starting point a systematic and well-organized leap into total uncertainty. This leap is based on some arguments that in Descartes' philosophy are supposed to show that we cannot be sure even of the truth of simple mathematical propositions such as the sum of two and three.

By contrast with what takes place during the first meditation of the *Meditations on first philosophy*, the second meditation introduces a new issue. Descartes, or the unidentified narrator in Descartes' text, starts to ask himself if the universal doubt leads to some kind of knowledge, if he knows anything at all. This questioning will lead him to the famous "*ego sum, ego existo*".

In such a metaphysical theory that establishes the mind's ability to grasp clear and distinct ideas without any reference to the outside world there is no place for experience, or, if there is, this place is well determined. Thus, in the *Principles of Philosophy*, experience is given a significant role in the first section of the second part of the book. In this section Descartes refers to experience in order to prove that "we know with certainty that material objects exist."[5] The existence of bodies is thus closely related to what we learn through our senses. According to Descartes' demonstration, the existence of material substance is obvious as long as bodies in motion provoke in us multiple sensations. Against this assertion an objection can easily be raised: are we sure that bodies and their motion is what causes our sensations? Because one might argue that God produces in our minds the idea of the material substance without even having to create this substance. But, as Descartes replies to his potential objector, God is not a deceiver but a guarantor of the truthfulness of what we clearly and distinctly perceive.

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[1] This is exactly what Descartes has to admit in a letter (August 1649) to More: "I agree that 'if matter is left to itself and receives no impulse from anywhere' it will remain entirely still." (Descartes, 1991a, 381).

[2] *Principles of Philosophy*, I, section 21 (Descartes 1991b, 11).

[3] See R. Hatfield (1979).

[4] *Principles of Philosophy*, II, section 42, (Descartes, 1991b, 62).

[5] *Principles of Philosophy*, II, section 1 (Descartes, 1991b, 39).

Spinoza and the problem of the existence of matter

The importance of the problem of matter's existence is emphasized in the first published work of Spinoza, a presentation of Descartes' metaphysics and physics according to the geometrical method, published under the title *Principles of Descartes' Philosophy*. We should not forget that Spinoza's philosophy occupies a central position in the movement of early modern philosophy which deals with the foundations and the ultimate consequences of the mathematical conception of nature. Besides, it is a well-known fact that Spinoza was in contact with some eminent scientists of his time like Oldenburg and Boyle, and closely followed the achievements of contemporary science.[6] His correspondence also manifests his interest in physical problems. Of course, there is no evidence that Spinoza read any of Galileo's texts, or at least no evidence drawn directly from the extant sources of Spinoza's life and work.[7] The lack of any reference to Galilean physics in Spinoza's writings cannot, however, negate the assumption that he was well aware of its fundamental concepts.

In order to return to the problem of the role of experience in Spinoza's first published work, let us examine the relation between physics and metaphysics as presented in the *Principles of Descartes' Philosophy*. The problem we are facing is one found between the first and the second part of this work. It is a problem concerning the association of the demonstration of the material substance's existence to the knowledge of matter's essence. Is there any place in this passage for experience? And if there is, as Descartes has already tried to show, how does Spinoza treat experience?

In Spinoza's presentation of the Cartesian theory, it is clear that what pertains to the problem of God's veracity and what is part of a rational physical theory belong to two totally different levels. Thus, Spinoza separates what is intrinsically woven together in Descartes' philosophy. In his work the demonstration of the existence of bodies is presented at the end of the first part of his book, in the part dedicated to metaphysics. Despite the fact that according to Descartes a rational physics must find its object only by means of a metaphysical demonstration, Spinoza does not accept as a part of a rational physics what is of a metaphysical origin and what necessitates a reference to our experience. Physics is for Spinoza the rational study of the properties of material substance.

Evidently, such an approach transforms Cartesian physics into a theory defined in purely geometrical terms and ordered according to the rules of deductive structure. This is exactly the goal that Spinoza tries to achieve in his *Principles of Descartes' Philosophy.* It is true, of course, that as long as Spinoza follows the Cartesian doctrine, he has to give to experience a role in the demonstration of the existence of bodies. But this role is not one that can find a place in the context of a rational physics. Spinoza reserves the second part of his work for a physical theory of a geometrical origin and does not want to combine an element not rigorously deductive with his systematic presentation of a theory identifying matter with geometrical extension.

Spinoza's presentation opens with a set of definitions and axioms. In this set, there is no mention of elements of an empirical origin, with only one significant exception in the twelfth axiom. This axiom will later be used by Spinoza in the demonstration of a proposition treating the motion of bodies submerged in a fluid. Even if this theory is basically of an auxiliary character, we must confess that the theory of motion in a fluid is the part of Descartes' physics which tries to reconcile the predicted results of collisions between bodies with the empirical lessons on that matter.

The problem of inertial motion

If we wish to examine in a more subtle way the use of experience in Spinoza's early work, we shall have to search for those propositions which, although drawn from Descartes' text, have in Spinoza's work demonstrations that differ from the ones given by the French philosopher. These demonstrations are usually proposed by Spinoza in an attempt to render the physical theory he is exposing more rigorous from a geometrical point of view. Such is the case, for example, of the two demonstrations he gives when presenting a crucial proposition for Cartesian physics, a proposition which forms a part of Descartes' second law of nature. Spinoza writes: "Every body which moves in a circle, as for example, a stone in a sling, is continuously determined to go on moving along a tangent."[8]

In Spinoza's formulation of this proposition a couple of points merit our attention. First of all, we have to notice that this theorem is followed by two demonstrations, each of them containing an allusion to a different geometrical theory. The first one refers the reader to two propositions of the third Book of Euclid's *Elements*, while the second one proceeds in a way supposedly borrowed from Archimedean geometry. Secondly, we must underline the fact that these two demonstrations do not have any equivalent in Descartes' text. Descartes simply takes for granted the fact that a stone rotated in a sling and describing a circular path, is in fact inclined to move along the tangent of the circle. So, given that Descartes never really

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[6] See A. R. Hall & M. Boas Hall, "Le monde scientifique à l'époque de Spinoza", *Revue de Synthèse*, 89-90 (1978), pp. 19-29.

[7] S. Nadler (1999, 111) conjectures that Spinoza had lessons in the new science and Galileo's physics in Van den Enden's school.

[8] *Principles of Descartes' Philosophy*, II, proposition 16 (Spinoza, 1988, 278).

tries to demonstrate this proposition, these demonstrations can be seen as Spinoza's contribution to the theory of inertial motion.

Spinoza's first demonstration of the theorem is of a special interest for the scope of our study. It is based on an analysis of the example of a stone rotating in a sling, an example taken from a section of the second part of Descartes' book where the French philosopher writes: "when the stone A is rotated in the sling EA and describes the circle ABF, at the instant at which it is at point A, it is inclined to move along the tangent of the circle toward C. We cannot conceive that it is inclined to any circular movement: for although it will have previously come from L to A along a curved line, none of this circular movement can be understood to remain in it when it is at point A. Moreover, this is confirmed by experience, because if the stone then leaves the sling, it will continue to move, not toward B, but toward C. From this it follows that any body which is moving in a circle constantly tends to move away from the center of the circle which it is describing."[9]

Spinoza in his demonstration witnesses his knowledge of the geometrical method. According to this method every proposition must be demonstrated, unless it constitutes an axiom. So, Spinoza decides to demonstrate the proposition left by Descartes without demonstration. Spinoza wants to show that the path followed by the stone is well determined and that the tangent is the only path compatible with the geometrical principles of a rational physical theory.

Spinoza's demonstration is composed of two steps. In the first one, he makes it clear that any given moving object continues to move in a straight line and if it does not, it "is continuously prevented by an external force from continuing to move in a straight line. If this force ceases, the body of itself will continue to move in a straight line"[10].

After these general remarks, Spinoza passes to the study of the specific case of a body moving in a circle: a body rotating in a sling "is determined by an external cause to continue to move along a tangent."[11] But Spinoza has told us a few lines earlier that it is an external force which *prevents* the mobile from moving in a straight line. How can we explain the fact that Spinoza attributes to an external cause the two different and even opposed tasks just described? On the one hand, he holds that it is by an external cause that a body is prevented "from continuing to move in a straight line", and then he contradicts himself when he argues that an external cause is what determines a stone in a sling to continue to move along a tangent.

Let us reiterate the basic components of our problem. The stone does not continue its motion in a straight line because of a force other than the force of its own movement, this is clear for Spinoza who states this principle unambiguously. A moving object in order to continue its motion does not require anything else because motion perseveres in its being without the need of an external force. So, the continuation of a motion in a straight line must be conceived of as nothing more than the mere fact of the conservation of a mode of the body.

It seems, though, that Spinoza misunderstands the fundamental law of inertia. The source of this misunderstanding is probably the fact that he indistinctly makes use of elements found in the demonstrations of the two parts of the Cartesian version of this law. Descartes' demonstration of the first half of the law of inertia, that is of the part that states that "when a thing is once moved, it always continues to move"[12], makes use of the principle of the conservation by God of the quantity of motion placed in matter. This sounds as if God acts as an external cause which determines matter's motion. And this is exactly the point which Spinoza transfers to the second part of the Cartesian law of inertia, when he takes the rotating stone as being determined to move on a straight line by an external cause. Spinoza holds that a rectilinear motion must depend on a cause that determines the kind of motion that will be produced.

One might conjecture here that what Spinoza means is that God, with his permanent concurrence, recreates everything at every instant and by doing so He brings about a motion that is simple, thus rectilinear. But it is easy to see that this is not the case and that Spinoza had the means to avoid any kind of confusion on this subject. It is not an external cause which determines the stone to continue to move along a tangent. Even if we accept that it is God who is the principal cause of motion, God from this point of view is not an external cause as long as He imparts to every one of the bodies in the world a certain quantity of motion. Even worse, the rest of Spinoza's demonstration shows clearly that it is not to God that Spinoza is referring when he speaks of an external cause, but to the sling. To say it more clearly, Spinoza considers the sling as a cause which determines the stone to move along a tangent.

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[9] *Principles of Philosophy*, II, section 39 (Descartes 1991b, 61).

[10] *Principles of Descartes' Philosophy*, II, demonstration of proposition 16 (Spinoza, 1985, 278).

[11] *Ibid* (Spinoza, 1985, 278-279).

[12] *Principles of Philosophy*, II, section 37 (Descartes, 1991b, 59).

The misuse of experience

We can now pass to the search of the cause that led Spinoza to such a remarkable misunderstanding. A lot of conjectures can be put forward, but I shall focus on one which stresses the role of experience in Spinoza's analysis. I think that it is because of a misuse of experience that Spinoza makes a mistake that normally would be obvious to any careful reader, and of course to Spinoza himself. But before taking a look at the real cause of Spinoza's mistake, we have to go back to Descartes' text. Descartes discusses the same example of the tendency to move away from the center of the circle detected in the case of a rotating stone. We can see that there is an interesting allusion to experience in the Cartesian text: "This is confirmed by experience, because if the stone then leaves the sling, it will continue to move, not toward B, but toward C."[13] Thus, it is in experience that Descartes finds a confirmation of the rectilinear trajectory that the stone will follow if there is no impediment. Leaving aside the fact that experience does not corroborate such an assumption, it is interesting to observe the way both philosophers are misled because of the fact that our sensation seems to imply that the stone rotating in a sling is at every instant determined to continue its motion on the tangent.

Both Descartes and Spinoza approach the example at hand through the lens of everyday unanalyzed experience. In the case of Descartes we are in front of textual evidence, whereas in the case of Spinoza no such evidence can be found. But Spinoza declares that the stone is determined by an external cause to continue to move along a tangent, and that this cause is the sling itself. This may plausibly be attributed to the impression that we have when we hold a sling in our hand and rotate it. We have the feeling of the sling forcing the stone to move away from the center of rotation. But this is just a false impression, because what is really happening is that the rope of the sling prevents the stone from moving upon a straight line and keeps it moving circularly.

Conclusion

It is now clear that when Spinoza is describing the sling as an external cause determining the stone to move along the tangent he is alluding to empirical evidence which is nothing more than a confused feeling. But Descartes' text which Spinoza had in his hands when he wrote his first published work, did not provide him with the indispensable skill of analyzing with a mathematical rigor what is given through experience. Spinoza's method is rigorous, of course, but it is a method of geometry, not of experimental physics.

This is why we find in Spinoza an ambivalent attitude toward experience. Obviously, he cares about what experience has to tell us, but what is lacking from his early work is an ability to translate the language of experience into geometrical terms. We have seen that he does not banish experience from the field of natural philosophy, but tries to delimitate its possible uses. One of these possible uses concerns the agreement between the previsions of a geometrical physics and the observations drawn from the real world. But when it comes to the validity of a proposition, Spinoza seems incapable of using techniques that might lead to a refined evaluation of the lessons taught by experience.

I think that one can find the same ambivalent attitude in Descartes' work. There, the empirical element is not absent, but it does not constitute a substantial part of the theory, this theory being essentially based on geometrical principles. What is more, this geometrically-based physical theory depends heavily on a theory of knowledge which is woven together with a metaphysical system rationally orientated. The whole structure of such a theory on the one hand minimizes the role of the empirical element and on the other leads to an acceptance of some elements of empirical origin without their necessary analysis.

References

DESCARTES, R. (1991a), *The Philosophical Writings of Descartes*, vol. 3: The Correspondence, translated by Cottingham, J., Stoothoff, R., Murdoch, D. and Kenny, A., Cambridge: Cambridge University Press.

DESCARTES, R. (1991b), *Principles of Philosophy*, translated by Miller, V. R. and Miller, R. P., Dordrecht: Kluwer Academic Publishers.

GUEROULT, M. (1980), "The metaphysics and physics of force in Descartes", in GAUKROGER, S. (ed.), *Descartes: Philosophy, Mathematics and Physics*. Brighton: Barnes and Noble, 196-229.

HALL, A. R. & BOAS HALL, M. (1978), "Le monde scientifique à l'époque de Spinoza", *Revue de Synthèse*, 89-90, 19-29.

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[13] *Principles of Philosophy*, II, section 39 (Descartes, 1991b, 61).

HATFIELD, G. (1979), "Force (God) in Descartes' physics", *Studies in History and Philosophy of Science* 10, 113-140.

NADLER, S. (1999), *Spinoza: A Life*, Cambridge: Cambridge University Press.

SPINOZA, B. (1985), *The Collected Works of Spinoza*, vol. 1, translated by Curley, E., Princeton: Princeton University Press.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 384.

T. NYDEN: EXPERIMENT IN CARTESIAN COURSES: THE CASE OF PROFESSOR BUCHARD DE VOLDER[1]

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Abstract

In 1675, Burchard de Volder became the first university physics professor to introduce the demonstration of experiments into his lectures and to create a special university classroom, The Leiden Physics Theatre, for this specific purpose. This is surprising for two reasons: first, early pre-Newtonian experiment is commonly associated with Italy and England, and second, de Volder is committed to Cartesian philosophy, including the view that knowledge gathered through the senses is subject to doubt, while that deducted from first principles is certain. On the surface, it is curious that such a man would bring experiments to university physics. After all, in demonstrating experiments to his students, he was teaching them through their senses, not reason. However, if we consider the institutional context of de Volder's teaching, we come to see de Volder's Cartesian experimentalism as representative of a certain form of pre-Newtonian Dutch Cartesian science, as well as part of a larger tradition of teaching through observation at the University of Leiden. Considering de Volder's institutional context will help us see that de Volder's experimentalism was not in spite of his Cartesianism, but motivated by it.

This paper will describe de Volder's physics curriculum, the Theatre in which it was taught and how his work predates similar efforts in other European Universities. The second part will consider three aspects of the culture at the University of Leiden: a tradition of teaching through observation, a particular reception of Cartesianism, and an eclectic approach to the New Philosophy. The concluding portion shows how de Volder would have understood his enterprise in Cartesian terms.

De Volder's Teaching

Very few discuss de Volder today and most of those discussions are in relation to his eight-year correspondence with Leibniz. However, during his own lifetime, he was well known,[2] largely due to the form and content of his teaching. De Volder

[1] I would like to thank Esther Howe for her assistance in proofreading this article.

[2] Not only were his ideas sought out by Leibniz, his innovative teaching was emulated by Johann Bernoulli and Christiaan Huygens held him in such high esteem as to entrust him with overseeing the publication of his posthumous works. Clerq, P.d. (1997), *At the Sign of*

was hired to teach logic at the University of Leiden, his *alma mater*, in 1670, and soon thereafter began to teach physics as well. In 1674, he traveled to England, where he met Boyle and Newton, attended meetings of the Royal Society, and witnessed several experiments, particularly air pump demonstrations. Immediately after his return, de Volder requested the Leiden curators to fund the creation of a Physics Theatre in which he would demonstrate experiments during physics lectures.[3] The curators enthusiastically agreed and in 1675 de Volder taught the first experimental physics course offered by a European university.[4] He bought a house near the main academic building and botanical gardens to house the theatre and began to furnish it with the latest equipment. He was able to secure funding amounting to one-eighth of the university's annual budget for this purpose.[5] De Volder's teaching became famous and within the next thirty years his pedagogy spread from Leiden to other institutions.

By 1705, professors at the Universities of Utrecht, Harderwijke, Franeker, and Groningen were teaching physics through experiments.[6] In the first three cases the professors were graduates from Leiden and most likely had been students of de Volder himself. At the University of Groningen, Johann Bernoulli (1667−1748) introduced the experimental approach and though he was not an alum of Leiden, we know from his January 8, 1698 letter to Leibniz that his pedagogy was directly influenced by de Volder:

> The governors have set me a new teaching task, and to this end have appropriated a certain sum to buy experimental instruments, so that, after the example of de Volder in Leiden, I shall occupy and amuse our students with mathematic-physical experiments.[7]

This experimental pedagogy spread most quickly within Dutch universities. Eventually, it made its way to other Northern European universities, however, just after 1700 there were still only a few cases: J.C. Sturm (1635−1703) at Altdorf, G.A. Hamberger (1662-1716) at Jena and Pierre Varignon (1654−1722) at the *Collège Mazarin*.[8] Interestingly, each of these cases has links to Leiden, though some are certainly more direct than others. Sturm studied at Leiden and it is likely that the spirit of observation he learned there influenced his own demonstration of experiments in his *Collegium Experimentale sive Curiosum* at the University of Altorf. Sturm began the *Collegium* in 1679 and based his experiments on those from the *Accademica del Cimento*.[9] It is in the *Collegium* at Altorf that Sturm's student, G.A. Hamberger, witnessed this new pedagogy, which he went on to use in his own teaching at Jena. The third professor, Pierre Varignon, has less clear links to Leiden. Varignon began teaching at the *Collège Mazarin* thirteen years after De Volder's first experimental course. What he knew of De Volder's activities is not clear, after all, he trained and worked in Catholic institutions, which were less likely to take the Protestant Leiden University as a model for excellence. However, it is noteworthy that he too was a longtime correspondent of Johann Bernoulli and Leibniz. It is possible that he knew of De Volder's work through them.

It is noteworthy that while much of the demonstrations taking place in the Dutch universities, Altorf, Hamberger, and Jena were based on those of the famous *Accademica del Cimento*, and while there was a rich experimental history in Italy, including some demonstrations at Italian universities before 1690, it was not until 1690 that the first university laboratory opened in Italy at the University of Bologna.[10] This is fifteen years after the founding of Leiden's Physics Theatre.

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the Oriental Lamp: The Musschenbroek Workshop in Leiden, 1660-1750. Rotterdam: Erasmus Publishing, p.143; Ruestow, E.G., (1997), *Physics and 17th and 18th Century Leiden*. The Hague: Martinus Nijhoff, p. 110-111.

3 Wiesenfeldt, G. (2000), "The Virtues of New Philosophies Or: How the Leiden Philosophical faculty survived the crises of 1676," 19th International Congress of Historical Sciences Oslo, Sweden, (*http://www.oslo2000.uio.no/AIO/AIO16/group%203/Wiesenfeldt.pdf*) p. 5; Rupert Hall ed. (1982), "Further Newton Correspondence," (Burchard de Volder to Newton November 1684), Notes and Records of the Royal Society of London, 37 (1): p. 27.

4 Turner makes the loose remark that Cambridge, Oxford, and the University of Leiden were all teaching with experimental apparatus at the same time as the formation of the Royal Society (1660), but this account is contradicted by the bulk of the literature. Turner, Gerard L'E., (1998), *Scientific Instruments: 1500-1900*, Berkeley: University of California Press, p. 103; Knoeff, R., (2002) *Herman Boerhaave (1688-1738): Calvinist Chemist and Physician.* Koninklijke Nederlandse Akademie van Wetenschappen: Amsterdam, p. 23; de Clerq, 42-43; Klever, W. (1998), "A Cryptospinozist on a Leiden Cathedra," *LIAS* XV (2) p.199; Otterspeer, Willem (2001), "The University of Leiden: An Eclectic Institution," *Early Science and Medicine*. 6 (4), p, 325; C.A. Crommelin, (1951), *Descriptive Catalogue of the Physical Instruments of the 18th Century.* Leiden: National Museum of the History of Science, p. 2; C.A. Crommelin (1926), "Physics and the Art of Instrument Making at Leyden in the 17th and 18th Centuries," Lectures on Physics and Physiology Delivered in the University of Leyden During the Second Netherlands Week for American Students, July 5-10, 1926. Lytoff, Leiden, p. 1-2.

5 Wiesenfeldt, 3.

6 De Clerq, p. 134.

7 Bernoulli-Leibniz January 8, 1698, translation from de Clerq, p. 143.

8 J.L. Heilbron. (1982), *Elements of Early Modern Physics*. Los Angeles: University of California Press, p. 132-3.

9 Thomas Ahnert, (2002), 'The Culture of Experimentalism in the Holy Roman Empire: Johann Christoph Sturm (1635- 1703) and the Collegium Experimentale' (unpublished paper, *http://sammelpunkt.philo.at:8080/308/*).

10 Paul Schuurman, (2004), *Ideas, Mental Faculties and Method: The Logic of Ideas of Descartes and Locke and its Reception in the Dutch Republic*, 1630-1750. Leiden: Brill, p. 66.

It was even longer, over 31 years, before non-Dutch universities demonstrated experiments within their *physics* courses. The first English *public* physics course included experiment and was taught by Francis Hauksbee (1666–1713) in London in 1704, but it was not until 1706 that an English *university* taught experimental physics in a course by William Whiston and Roger Cotes at Cambridge.[11] Christian Wolff continued in the tradition began in Germany by Sturm and Hamberger by teaching a course in experimental physics at Halle in 1706 and experiment did not enter the university physics classroom in Italy or Switzerland until 1737 (at Bologna and Geneva).[12]

De Volder's courses were most famous for his demonstration of the air pump, but university inventories and student notes indicate that in addition to pneumatics, he demonstrated experiments in mechanics, hydraulics, optics and magnetism. His Physical Theatre contained 64 scientific instruments, including many apparatus for his air pump, a collision machine, microscopes, fountains, a magic lantern and Magdeburg hemispheres.[13] As the lecture notes of Carolus Vinson, one of de Volder's physics students in 1676 year attests, de Volder's lectures centered on the demonstration of a variety of experiments, many taken from Boyle's *New Experiments PhysicoMechanicall Touching the Spring of the Air and its Effects* (1660).[14]

The University of Leiden

Leiden's Empirical Tradition

From its beginnings in 1575, the University of Leiden was committed to a balance of theory and practical experience.[15] William of Orange expresses this commitment in his December 1574 letter to the States of Holland and Zeeland, in which he claims that it is necessary to establish a university as:

> ...a firm support and sustenance of freedom and good legal administration of the country not only in matters of religion, but also with regard to the general welfare of the people.[16]

He goes on to say that students are to be trained

> …in both the right knowledge of God and all sorts of good, honourable, liberal arts and sciences, serving the legal administration of countries.[17]

Leiden made a point of teaching through practice in order to prepare students to meet the needs of the country.[18] The essence of this approach was providing students opportunities to observe and interact with the natural world. For example, in 1593 the university founded its famous botanical gardens and anatomy theatre and 1669 added a Chemistry Theatre, each of which became an integral part of lecture. As a medical student at Leiden, de Volder directly witnessed an additional form of teaching through observation. His professor, Franciscus Sylvius pioneered the practice of teaching medicine through clinical rounds in 1636 (a practice that continues to this day).[19] When viewed in this institutional context, de Volder's Physics Theatre, directly modeled on the Anatomy and Chemistry theatres of his institution, seem the next obvious step for a professor recently exposed to the physical experiments of the Royal Society.

Cartesianism at Leiden

De Volder was not the first to combine aspects of Descartes' philosophy with empirical pedagogy at Leiden. Sylvius provides an earlier example, one that is thought to have influenced de Volder's own abandoning of Aristotelian philosophy for that of Descartes' during his medical training.[20] This is significant, for, as Pamela Smith notes, Sylvius' approach involved:

> ...affirming observation and experiment and, at the same time, attempting to introduce mathematics into medicine….he insisted in his lecture that for natural knowledge to be truly certain, it must be *demonstrated mathematically*…[21]

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[11] Heilbron, p.134 and 153.

[12] Ibid., p. 133-9.

[13] De Clercq, Peter (1997), *The Leiden Cabinet of Physics: A Descriptive Catalogue*. Leiden: Museum Boerhaave, p.10.

[14] Vinson, C. (1676-1677), "Experimenta Anatomica et Philosophica" in *Sloane* 1676-1677, The British Library: London.

[15] Otterspeer, p. 333.

[16] Quote from Otterspeer, p. 324.

[17] Quote from Otterspeer, p. 325.

[18] Smith, Pamela (2005), *The Body of the Artisan: Art and Experience in the Scientific Revolution*. Chicago: University of Chicago Press, p. 221.

[19] Ibid.

[20] Knoeff, p. 24.

Sylvius was influenced by Descartes in two important ways. As just mentioned, he too wanted certainty for the New Science and thought this was to be found in mathematical demonstration, that is, the geometrical deduction of the rationalists, though he himself did not make much progress in this regard. Second, he was influenced by Descartes' account of the passions. Sylvius held that the passions were excited by external objects and therefore connected to the senses.[22] This, and lingering concerns from his Aristotelian roots, made him worry about the reliability of information that comes to us from our senses. Further, he worried about the dangers posed to individual scientists who, through empirical observation and experiment, are immersed in the senses. For example, to protect themselves, natural philosophers must have moderate, if not chaste, bodily habits, so as to prevent the distortion of knowledge by desire.[23] Sylvius did not agree with Descartes on all things, however. For instance, he understood the body, and nature in general, in terms of chemical processes, rather than mechanical ones. What is important to us here is that through Sylvius' teaching, de Volder would have been exposed to methodologies that involve experiment and observation and at the same time, extol mathematical certainty. Further, Sylvius would have modeled for de Volder the effectiveness of teaching through empirical means, not only through anatomies, dissections, and experimentation, but through the daily clinical rounds in which students observed patients. [24] Sylvius gave de Volder an appreciation for the effectiveness of appealing to sensory knowledge in affecting their student's passions, preparing them to accept certain ideas that eventually can be proven with mathematical certainty.

Eclecticism at Leiden University

Cartesianism had an early reception at Leiden, largely as a result of the university's eclectic tradition. This tradition goes back to Franco Burgersdijk (1590-1635), who combined Aristotle with the ideas of humanists, particularly those of Pierre de la Ramée who advocated for a strong separation of the disciplines. In other words, Burgersdijk used contemporary philosophy to inform his re-interpretation of Aristotle and took it as a given that the ancient and new philosophies could be made compatible. This eclectic approach continued in the first Cartesian professors at Leiden: Adriaan Heereboord (1613-1671) and Johannes De Raey (1602-1702) who combined Cartesianism with other modern philosophies, such as those of Bacon and Gassendi, and presented these new ideas as consistent with a 'true' reading of Aristotle. This eclecticism had two important elements that encouraged a positive reception of Cartesianism in Holland. First, because the New Philosophy was not presented as a break with Aristotle, it was not considered as threatening to the university *status quo* as it was in other countries, such as France and England.[25] Second, this eclecticism, in accepting and combining the mathematical tendencies of Cartesianism and the empirical methodologies of observation and experiment, appealed to the practical orientation of the Dutch, who had a deep appreciation for the practical value of mathematics (for example, to navigation) and a growing appreciation for the value of cooperation between scientists and artisans – a trend that Wiep van Bunge says "…started to blur the distinction between practice and theory."[26] Within a few years of teaching at Leiden, de Volder adopted the eclectic approach of using logic courses to defend Descartes' philosophy by demonstrating its consistency with ancient philosophy. In doing so, he made particular use of a Burgersdijk's reading of Aristotle that called for a clear separation of the disciplines.[27] This separation was useful to the Cartesian cause in that it separated philosophy from theology. This eclecticism was appealed to when de Volder's request to the Regents suggested that the experiments in the Physical Theatre would provide a non-violent, non-partisan way to calm controversies that were then raging at the University between the Scholastic and Cartesian members of the university community. Both he and his Aristotelian colleague Wolferd Senguard (1646-1724) shared the theatre, both performing experiments from Boyle's book and yet both understanding the use of those experiments within their own epistemological framework.

De Volder's Cartesian Experimentalism

Neither Senguard or de Volder subscribed to Bacon's methodology. Both professors used experiment to verify conclusions already held, the same way they used mathematical and philosophical reasoning. They did not promote induction or try to gather new information about the world through sensory perception or experiments.[28]

De Volder's commitment to Cartesian, *a priori* methodology is found throughout his career. For example, in his earliest writing, *Consideratien over de Resolutie,* he defends Cartesian methodology. De Volder wrote the *Consideratien* with two Cartesian theology professors at Leiden. They were responding to a resolution forbidding the teaching of certain

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21 Smith, p. 223.

22 Ibid, 226.

23 Ibid., 228.

24 Ibid., 221.

25 Wiesenfeldt, p. 6.

26 Wiep van Bunge, *From Stevin to Spinoza: An Essay on Philosophy in the Seventeenth-Century Dutch Republic*. (Leiden: Brill, 2001) pages 6-9.

27 Tammy Nyden-Bullock (2007), *Spinoza's Radical Cartesian Mind*. London: Continuum, p. 10; Klever, 129.

28 De Clerq 17; de Clercq, p.10.

Cartesian tenets at the University that resulted from the latest wave of Cartesian controversies.[29] The *Consideratien* was written only a year after the Physics Theatre opened, and so we can conclude that the teaching innovation of bringing experiment into the university Physics classroom was indeed by a self-identified 'Cartesian'. Further, de Volder's deep commitment to Cartesian methodology continues through his correspondence with Leibniz, which ends three years before de Volder's death, well after his thirty-year tenure of teaching students through experiments in the physics theatre. While de Volder did come to distance himself from some of Descartes' doctrines, he maintained a commitment to Cartesian methodology throughout his entire career.

So what did experiment mean to such a Cartesian? De Volder's use of experiment in the classroom was meant to awaken a desire in his students to study 'true science', that is to undertake the difficult work of deductive science. Experiment for him was not part of 'true science' itself, but an effective motivation for young boys operating mostly at the level of imagination and sensory knowledge. De Volder saw experiments as a means to prepare the student's mind for 'true science', which is a deductive understanding from first principles. Demonstrations of experiments were a way to get the students attention, to persuade them of certain possibilities through their senses and imagination, creating, if you a will, a certain disposition of belief, that would make the real work of science, reasoning from innate principles, more easily received by his young charges. Observation itself does not provide us certain metaphysical knowledge about the world, the type of knowledge that de Volder sought as a Cartesian. However, observation can provide us with a moral certainty that can give us the faith necessary to take the initiative to engage in the requisite metaphysical search for truth. This approach is importantly *pre*-Newtonian. It is true that de Volder met Newton and was later one of the first on the Continent to read the *Principia*, receiving a copy from Newton himself in 1687. However, this occurred three years before his correspondence with Leibniz, in which De Volder never asserts or defends Newtonian physics, but on the contrary, acts as an apologist for *Cartesian* physics and methodology. Further, de Volder does not mention Newton in his own writing until 1697, after twenty-two years of experimentation in the Leiden Physics Theatre. This occurs in *De rationis viribus et usu in scientis*, written while de Volder was serving as Rector of the university, and even in that work, he merely mentions Newtonianism and in no way appears to advocate it.[30]

Conclusion

In conclusion, experiment was introduced to university physics by a Dutch Cartesian, not an English Newtonian or Baconian, nor one of the members of the great Italian academies. De Volder's innovation can be understood within the context of Leiden's deep traditions of eclecticism and teaching through observation. De Volder's motivations were pedagogical (and as I argue elsewhere, political[31]). His story provides us with an interesting example of a Dutch Cartesian experimentalism, one that views observation and experiment as useful, particularly in affecting the passions, but still subservient to deduction from first principles.

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[29] *Consideratien over de Resolutie van de Ed. Achtbare Heeren Curateuren der Universiteyt binnin Leyden, en Borgermeestern de selver Stede, &c.* Molhuysen, P.C., (1918), *Bronnen tot de Geschiednis van de Leidsche Universiteit*. Vol. III. The Hague: Martinus Nijhoff, p. 320; Knoeff, 25-6.

[30] Knoeff, 27.

[31] Tammy Nyden (2010), "De Volder's Introduction of Experimental Physics to the University." Conference paper at the 2010 Conference for the International Society of the Study of European Ideas Ankara, Turkey, August 2010.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 389.

ROHAULT'S *TRAITÉ DE PHYSIQUE* AND ITS NEWTONIAN RECEPTION

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Abstract

Jacques Rohault (1618-1672) was one of the most important Cartesians in seventeenth-century France. He became famous in Paris, during the 1660s, when he hosted some very popular public conferences. Unlike his contemporary Cartesian fellows, Rohault was well concerned with the problem of experiment and he designed a number of instruments, which were used in his observations. The results of his experimental research were printed in the Traité de physique *(1671), which was quickly translated into Latin and published in Geneva, Amsterdam, London, and Louvain among other places.*
In this paper, I shall propose a new reading for the problem of the reception of Rohault's textbook in England. Translated and annotated by the celebrated Newtonian, Samuel Clarke, this book represents a combination of Cartesianism and Newtonianism. While Descartes' influence upon Newton's philosophy has been discussed by various scholars, the relation between Cartesianism and Newtonianism is still a topic in need of further exploration for the historians of science. Clarke's various editions of Rohault's Traité *provide a good example of the diffusion of Cartesianism at the end of the seventeenth century, making an interesting case study for the dialogue between two competing paradigms during the "scientific revolution" and the transformation of natural philosophy into physics.*

Introduction: Descartes' system of natural philosophy

In 1647, when he was preparing the French edition of the *Principia philosophiae*, Descartes wrote in the preface-letter that:

> I will here point out the fruits which I am sure can be derived from my principles. The first is the satisfaction which will be felt in using them to discover many truths which have been unknown up till now. (...) The second benefit is that the study of these principles will accustom people little by little to form better judgements about all the things they come across, and hence will make them wiser. (...) The third benefit is that the truths contained in these principles, because they are very clear and very certain, will eliminate all ground for dispute, and so will dispose people's minds to gentleness and harmony. (…) The last and greatest fruit of these principles is that they will enable those who develop them to discover many truths which I have not explained at all (AT IXb 17; CSM I 188).

One can nevertheless notice Descartes' optimism about the reception of his philosophy. In this programmatic preface to his textbook on philosophy, Descartes expresses his confidence in changing the *old* philosophy in its entirety. The second and the third benefits listed above are, in fact, direct attacks towards the traditional university teaching. They seem to suggest that Descartes' first principles are so clear that no disputes about them would be possible anymore –at least for those making a good use of their reason– which will subsequently lead to discover new things, as the first and the fourth listed fruits suggest.

As many scholars have noticed, Descartes' entire project of reforming philosophy takes the way of metaphysics. He is very specific about the right way of philosophizing, claiming in the same preface-letter that only one order should be followed. Not surprisingly, this is the same with the structure of his *Principia*, where the aim goes from metaphysics to physics, and finally to special sciences:

> Thus the whole philosophy is like a tree. The roots are metaphysics, the trunk is physics, and the branches emerging from the trunk are all the other sciences, which may be reduced to the three principal ones, namely medicine, mechanics and morals (AT IXb 14; CSM I 186).

It is interesting that Descartes acknowledges with this tree-metaphor the preparatory role of the first two parts of his book in reaching what seems to be the goal of enquiry ("to gather the fruits"), yet he does not seem to allow for possible developments in the direction of special sciences. I am referring here to his open critique addressed to Henricus Regius, a committed Cartesian from the University of Utrecht, whose *Fundamenta physices* (1646) upset Descartes.

Besides the personal note of the attack, Descartes makes also a general comment about how a philosophical work should be evaluated. He claims that readers "should not accept any opinion as true –whether in my writings or elsewhere– unless they see it to be very clearly deduced from true principles" (AT IXb 19; CSM I 189). However, what is odd with this reaction to Regius is that Descartes begins to build up something that scholars have called "Cartesian scholasticism."[1] Not only does he reduce natural philosophy to metaphysics, but he also claims that only his principles are true. Metaphysics and physics are thus linked, creating a system of intertwined principles meant to explain all phenomena.

This is quite a traditional reading of Descartes' philosophical aims and method.[2] I would not want to challenge it here, but rather to address the problem of the proposed structure of a system of natural philosophy in the context of the late seventeenth-century Cartesianism. In doing this, I shall focus on the most important Cartesian treatise on physics –*Traité de physique* (1671)– of Jacques Rohault and on its reception in England.

Jacques Rohault: Life and Works

Jacques Rohault was one of the leading seventeenth-century French Cartesians. Not only philosophically, but also socially connected to Descartes' philosophy, his views were respected and debated all over Europe. From the social point of view, Rohault was the son-in-law of Claude Clerselier, the editor of Descartes' unpublished writings.[3] He could thus become one of the focal points of the Cartesian movement in France. His house was used to host the famous "Wednesday" meetings, where Descartes' philosophy was discussed.

As far as Clair presents him, Rohault's education seems to have been a mixture of traditional university training and self-study.[4] After a first period of learning at the Jesuit college of Amiens, he moved to Paris, where he became known as a "professeur des mathématiques." With a more extensive knowledge of the sciences than any other Cartesian, Jacques Rohault has been described by Paul Mouy as working to experimentally confirm Descartes' physics.[5] Unlike other Cartesians, Rohault was also interested in the development of scientific instruments, as is confirmed by his invention of a new type of air-pump.[6]

He published little during his lifetime. A small treatise of his was however inserted into the first French edition of Descartes' *Le Monde* (1664). Clerselier, the editor of this book, presented him as "un Philosophe & Mathematicien."[7] Probably based on his public lectures and written a couple of years earlier, Rohault published a first book in 1671: it was his

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[1] See Bohatec (1966), p. 145.

[2] Both general histories of philosophy and more specialized studies on Descartes and Cartesian philosophy are stressing the importance of metaphysics in Descartes. See for example, Mouy (1934), Copleston (1985), Garber (1992), etc.

[3] For details of the connection between Rohault and Clerselier, see Clair (1978), p. 28. A good analysis of the social roots of Rohault's experimentalism is also provided in McClaughlin (1996).

[4] See Clair (1978), pp. 23-26.

[5] Mouy (1934), p. 108: "S'efforçait de constituer, sur les bases cartésiennes, une physique expérimentale. A ses 'mercredis', tout Paris, la province, l'étranger même se pressaient pour assister aux belles expériences par lesquelles il confirmait la physique de Descartes."

[6] Mouy (1934), pp. 128-130.

[7] Cf. Thijssen-Schoute (1989), p. 269.

Traité de physique. During the same year, a second publication came out, the *Entretiens sur la philosophie*.[8] The rest of his writings, mainly on mathematics and mechanical problems, were published only after his death (1672) by Clerselier, under the title of *Œuvres posthumes* (1682).

Of particular interest for this paper is his treatise on physics, which soon after its publication became very well known in France and elsewhere. For a number of years, it was “the standing text for *Lectures* at Cambridge” (Des Chene, 2002) and “the leading textbook of its time, and it was used at the universities of Utrecht, Frankfurt, Groningen, Louvain and Leyden” (Schüller, 2001). Translated by Samuel Clarke, the book was very successful in England, where it was used in various universities. The Newtonian character of Clarke’s comments is obvious, as he makes use of Newton’s arguments and experiments to refute the Cartesian theory of matter presented by Rohault.

Before moving forward, I would like to stress that Clarke’s comments appear in various places where Rohault is presenting his theory of matter and the consequences deduced from it. Yet, Clarke leaves without comments the part of the treatise dedicated to methodological issues. As I shall argue below, this is, in my view, the main reason for this very good reception of a Cartesian textbook on physics in the context of the birth and spread of Newtonian physics, as both share a common methodology.

Cartesian Empiricism

Roger Ariew has convincingly argued for the existence of a large variety of “Cartesianisms,” pointing out both the novelty of some ideas developed by Descartes’ followers and the new methods embraced by such philosophers in their writings.[9] Thus, perhaps one of the most important –yet, puzzling– category is that of “Cartesian Empiricists” from the second half of the seventeenth century.

Jacques Rohault is the main character in this empirical movement developed within Cartesian philosophy. Starting from his opening observation in the preface to the treatise on physics –“When I came to consider Philosophy, particularly Natural Philosophy, I was very much surprised to see it so barren as not to have produced any Fruit; in so much that twenty Ages have passed, without any new Discovery made in it” (Rohault, 1987, unpaginated preface)– Rohault traces the impediments in the evolution of natural philosophy, which, in his view, was hindered by five difficulties.[10]

First, there was too much emphasis on authority. Rohault makes similar accusations as Descartes: too many people have “such a blind Submission to the Opinions of Antiquity.”[11]

Second, natural philosophy is too metaphysical. While many philosophers from the second half of the seventeenth century made similar comments –especially in the English context– the reader seems to be left wondering whether a Cartesian natural philosophy is possible, if metaphysics is eliminated from the construction of physics.

The third difficulty concerns method, as Rohault denounces the two “extremes” of natural philosophy, which either put too much emphasis on experiment or on reason, despite the fact that taken separately they cannot give us good explanations. Instead, Rohault claims that we should join them together: “it cannot but be very advantageous to mix Experiments and Arguments together.”[12]

The fourth reproach addressed to natural philosophy is once more about method. Rohault finds it strange that mathematics, as a traditional discipline, has been neglected by the Scholastics when it came to the study of natural philosophy.

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[8] For a detailed list of Rohault’s writings and their subsequent editions, see Clair (1978), pp. 5-9. The *Traité de physique* had been translated into Latin and published in Geneva (1674), London (1682, 1702, 1708, 1710, 1713, 1718), Amsterdam (1691, 1700), Leiden (1729, 1739). In England, the second edition of the Latin translation of Rohault’s treatise (1702) was prepared by the celebrated Newtonian, Samuel Clarke (the first edition of 1682 was made by the famous Cartesian Antoine Le Grand). It was issued together with his annotations to the text, which were extended in the subsequent editions, including also four large technical footnotes written by the Jesuit Charles Morgan. While the *Traité* became more popular and its Latin edition was used as a school textbook in Cambridge and elsewhere, an English version was prepared by John Clarke, Samuel Clarke’s brother, who translated both Rohault’s text and the annotations of his brother’s Latin edition. This very influential translation was published in 1723, followed by a second edition in 1728-1729.

[9] See Ariew (2006).

[10] For more on Rohault and his preface, see Dobre (2010).

[11] Rohault (1987), vol. I, unpaginated preface, p. iv.

[12] Rohault (1987), vol. I, unpaginated preface, p. ix.

The "fifth Defect" noticed by Rohault is "not in the *Method* of those who study Philosophy, but in *that* of a great many who read their works."[13] Apparently, an author cannot control this, which is precisely what Rohault criticizes here, because he seems to have feared being misinterpreted. But his critique opens another direction of his arguments from the preface, namely to create a connection with Aristotle's philosophy. He blends up Aristotle and Descartes in a new way, because he announces that the Aristotle he will use in the treatise is a rather different than the one of scholastics.

Rohault's treatise starts with a definition of physics: "we here use it to signify the Knowledge of natural Things, that is, that Knowledge which leads us to the Reasons and Causes of every Effect which Nature produces."[14] Following Descartes, he appeals to the re-evaluation of all our received knowledge: "Minds are filled with a variety of Notions," and "we must lay aside all our old Prejudices, and reject them as false."[15] By the start of the second chapter, Rohault turns to the foundations of his natural philosophy: "we know that there are *Things* really *existing* in the World; and from hence we think we know, at least in part, *what* they are."[16]

Although Rohault does not have a metaphysical foundation as Descartes, one can still find traces of Cartesian metaphysics spread all over this preliminary part of his physics. However, in order to establish his empirical approach to natural philosophy, Rohault develops first a theory of sensation. He defines the four faculties of perception, judgement, reason, and sensation, emphasizing the role of the latter. Not surprisingly, Rohault describes sensation in terms of one's capacity of being affected by an external object; but, at the same time, he presents it in alleged correspondence with the Aristotelian philosophy: "wherefore we may say with Aristotle, that all Sensation is a kind of Passion, and when we have any Sensation, whatever sort it be, we know very well what the Objects raise in us, but we don't know what they are in themselves."[17] It follows that one can expect to know only the causes of particular perceptions, but not essences, which leads to the argument for the existence of external things. Overlapping with Descartes' famous argument from the *Principia* II 1, Rohault presents his conclusion:

> Whoever acknowledges this Truth, must confess, that he has been in an Error so long as he thought that the Existence of Things without him was proved by his Senses; for all that these can do, is only to be the Occasion of knowing them; and it is chiefly from Reasoning that we are assured of their Existence.[18]

Again, he is very Cartesian, grounding the existence of bodies on our *reason*, not on our *senses*. But his explanation makes sensation (and the senses) more important, because without all the perceptions they cause, reason does not have (or, it will miss) "the occasion" to exert upon natural philosophy.

From all this, it follows what we may consider to be Rohault's method of inquiry into natural philosophy: "in order to find out what the Nature of any Thing is, we are to search for some one Particular in it, that will account for all the Effects which Experience shows us it is capable of producing."[19] Just like he argued in the preface, neither reason alone nor experience taken by itself can provide us with the needed tools for the study of natural world; instead, a permanent collaboration of the two is suggested as the right method. This insight leads to the next step in the process of enquiry:

> If that which we fix upon, to explain the particular Nature of any Thing, do not account clearly and plainly for every Property of that Thing, or if it be evidently contradicted by any one Experiment; then we are to look upon our Conjecture as false; but if it perfectly agrees with all the Properties of the Thing, then we may esteem it well grounded, and it may pass for very probable.[20]

This surprisingly Popperian program of "conjecture and refutation" entails a probabilistic account of knowledge. Indeed, Rohault draws the parallel between natural philosopher and someone decrypting a coded letter. A traditional example, which became widely used by Cartesians, starting with Descartes himself, the metaphor of the code cracker is now turned into an argument in favour of a probabilistic account of knowledge. Rohault states:

> Thus we must content our selves for the most part, to find out how Things may be; without pretending to come to a certain Knowledge and Determination of what they really are; for there may

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[13] Rohault (1987), vol. I, unpaginated preface, p. xviii.

[14] Rohault (1987), vol. I, Part I, Ch. I, 1, p. 1.

[15] Rohault (1987), vol. I, Part I, Ch. I, 4, p. 2.

[16] Rohault (1987), vol. I, Part I, Ch. II, 1, p. 3.

[17] Rohault (1987), vol. I, Part I, Ch. II, 19, p. 6.

[18] Rohault (1987), vol. I, Part I, Ch. II, 36, pp. 10-11.

[19] Rohault (1987), vol. I, Part I, Ch. III, 1, p. 13.

[20] Rohault (1987), vol. I, Part I, Ch. III, 2, pp. 13-14.

> possibly be different Causes capable of producing the same Effect, which we have no Means of explaining.[21]

While this can be linked to Descartes' example, given at the end of the *Principia*, where two clocks with completely different mechanisms that are perfectly obscured to our senses still function in the same visible way, showing us the same time, Rohault's claim expresses something stronger.[22] Rohault deals with the epistemic status of our knowledge in physics and finds it to be always perfectible. At the same time, his words express a strong confidence in the results that have been confirmed through observation: "our Conjecture is not only to be looked upon as highly probable, but we have Reasons to believe it to be the very Truth."[23] Nevertheless, Rohault refrains from Descartes' claim that his physics is certain (recall Descartes' "more than moral certainty"). On the contrary, he makes a strong case for the permanent possibility of finding something that contradicts his conjectures. He gives the example of the new astronomical discoveries, where telescopes "have confirmed the Hypothesis of *Copernicus*." However, he warns us that even "if our Conjecture be otherwise well grounded, it does not lose its Probability, because we cannot upon the Spot explain by it a Property, which appears from some new Experiment, or which we did not before think of."[24] Hence, our knowledge in natural philosophy is perfectible and perfection is mainly achieved by the use of additional experience. Rohault's own history as experimenter testifies to this conviction.

Concluding section: the reception of the treatise in England

As mentioned above, this treatise of Cartesian physics knew its glory in the context of the birth and spread of Newtonianism. However strange that may seem to the modern reader, what I have presented in the previous section of my paper can offer a more nuanced understanding of the clash between Cartesianism and Newtonianism in the first years of the eighteenth century.

Rohault performs countless observations and engages in experiments even in those areas where theory provides a certain answer. In this sense, he is a more modern, empirical investigator than Descartes, as his experimental attempts appear to be trials performed in order to test a theory, which as a consequence is not deemed certain. Theory is then relegated to the status of a hypothesis in need of confirmation. Seen in this light, Rohault's system of philosophy becomes more consistent, but loses the certainty claimed by Descartes for Cartesian natural philosophy. Yet, when introducing the foundational elements of his physics, he is forced to deal with some of the consequences of Descartes' metaphysically rooted physics, notably with the principles of science, the essence of matter, and the role of God in the whole enterprise of creating and maintaining a law-governed universe.[25] Hence, his physics is not "metaphysically free" –despite his comments in the preface of the *Traité*.

The metaphysical certainties smuggled into Rohault's system from Descartes' *Principia* are, however, brought into a precarious situation because of Rohault's decision to subject them to (potentially falsifying) experimental testing.

Translated and annotated by the famous Newtonian, Samuel Clarke, Rohault's *Traité de physique* represents the textbook on physics for its time. Newtonian textbooks have not yet been written and Rohault fills in the missing gap with his empirically-oriented theory. However, what is important to notice here is that Clarke's footnotes are usually added whenever Rohault's starting hypotheses clashes with the Newtonian assumptions sitting at the basis of natural philosophy. Such is the nature of matter and gravity, but not the preliminary section devoted to the methodological structure of physics. In this respect, we can conclude that Rohault's system of natural philosophy makes it easier for Newtonianism to enter into the system of university teaching. Both shared the same methodological focus on experiment, which I have discussed in this paper. Of course, more can be said about the text of the treatise as well as about Clarke's remarkable footnotes, but this glimpse into the reception of Cartesianism in England is just a first step in providing a better picture of the intricate discussions about the formation of modern science.

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[21] Rohault (1987), vol. I, Part I, Ch. III, 3, p. 14.

[22] For Descartes's example, see AT IXb 322; CSM I 289.

[23] Rohault (1987), vol. I, Part I, Ch. III, 5, p. 14.

[24] Rohault (1987), vol. I, Part I, Ch. III, 6, p. 14.

[25] Due to space constrains, I have not discussed these here. For more on Rohault's Cartesian natural philosophy, see Dobre (2010).

References

ARIEW, Roger (2006), "Cartesian Empiricism", *Revue Roumaine de Philosophie*, vol. 50, pp. 71-85.

BOHATEC, Josef (1966), *Die cartesische Scholastik in der Philosophie und reformierten Dogmatik des 17. Jahrhundrets. I: Entstehung, Eigenart, Geschichte und philosophische Ausprägung der cartesianischen Scholastik* [1912], rep. Hildesheim: Olms.

CLAIR, Pierre (1978), *Jacques Rohault (1618-1672). Bio-Bibliographie*, Paris: Centre National de la Recherche Scientifique.

COPLESTON, Frederick (1985), *A history of philosophy*, Garden City, New York: Image Books.

DESCARTES, René (1964-1974), *Œuvres de Descartes*, 11 vols., eds. Adam, Charles & Tannery, Paul, Paris: J. Vrin.

DESCARTES, René (1984-1991), *The Philosophical Writings* (translated by J. Cottingham, R. Stoothof and D. Murdoch), Cambridge: Cambridge University Press, 2 vols. With a third volume on Descartes' correspondence, translated by John Cottingham, Robert Stoothoff, Dugald Murdoch, and Anthony Kenny (1991).

DES CHENE, Dennis (2002), "Cartesian Science: Régis and Rohault" in Nadler, Steven (ed.), *Blackwell Companions to Philosophy: A Companion to Early Modern Philosophy*, Blackwell, pp. 183-196.

DOBRE, Mihnea (2010), *Metaphysics and physics in Cartesian natural philosophy: Descartes and early French Cartesians on the foundation of natural philosophy*, Ph.D. dissertation, Proefschrift Radboud Universiteit Nijmegen.

GARBER, Daniel (1992), *Descartes' Metaphysical Physics*, Chicago & London: The University of Chicago Press.

MCCLAUGHLIN, Trevor (1996), "Descartes, experiments, and a first generation Cartesian, Jacques Rohault" in GAUKROGER, Stephen & SCHUSTER, John & SUTTON, John (eds.), *Descartes' Natural Philosophy*, Routledge, pp. 330-346.

MOUY, Paul (1934), *Le Développement de la physique cartésienne*: 1646-1712, Paris: J. Vrin.

ROHAULT, Jaques (1987), *System of Natural Philosophy, Illustrated with Dr. Samuel Clarke's Notes, taken mostly out of Sir Isaac Newton's philosophy*, Garland Publishing, New York, London.

SCHÜLER, Volkmar (2001), "Samuel Clarke's Annotations in Jacques Rohault's *Traité de Physique*, and how they contributed to popularising Newton's physics" in LEFÈVRE, Wolfgang (ed.), *Between Leibniz, Newton, and Kant –philosophy and science in the 18th century*, Springer, pp. 95-110.

THIJSSEN-SCHOUTE, C. Louise (1989), *Nederlands Cartesianisme (Avec sommaire et table des matières en français)*, with additions and a new bibliographical list by Th. Verbeek for the 2nd ed., HES Uitgevers Bv, Utrecht.

SOME ASPECTS OF THE CIRCULATION OF SYMBOLIC LANGUAGE

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THE CIRCULATION OF ALGEBRAIC SYMBOLISM RELATED TO THE FIRST ALGEBRAIC WORKS IN THE IBERIAN PENINSULA

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Abstract

In the 16th century mathematics underwent in Europe deep changes whose diffusion was favoured by the invention of printing in the previous century, that changed completely the way of transmitting culture. One of the main changes was the progressive development of algebra from practical arithmetics, in which the symbolism played a relevant role.
In this paper we will analyse the circulation of symbolic language in the algebraic works of the second half of the 16th century from other countries to the Iberian Peninsula, and also this circulation in the Iberian Peninsula itself. We will give some examples to show that in some cases the use of specific symbols was due to the constrictions of typography, as Juan Pérez de Moya (ca. 1513-ca.1597) stated in his Arithmetica practica y speculativa *(1562).*
We will also analyse when symbolic language is used only as a simplification to contribute to a better understanding of the rhetoric reasoning and when it is used as a part of the symbolic reasoning, thus stepping forward from the operations with numbers to the operations with new objects of algebra. These analysis will contribute to the better understanding of the process of algebraization of mathematics in the Iberian Peninsula.

Introduction

In 16th century Europe, mathematics underwent deep changes whose diffusion was favoured by the invention in the previous century of printing, which completely changed the way of that culture was transmitted. One of the main changes was the progressive development of algebra from practical arithmetic, in which the symbolism played a relevant role.

In this paper we address the circulation of symbolic language in the algebraic works of the second half of 16th century, from other countries to Iberian Peninsula, and also this circulation in the Iberian Peninsula itself.

We give some examples to show that in some cases the use of specific symbols was due to typographical constrictions, as Juan Pérez de Moya (ca. 1513-ca.1597) stated in his *Compendio de la Regla de la Cosa, o Arte Mayor* (1558)[1].
We also show some cases where the symbolic language is used only as a simplification to contribute to a better understanding of the rhetoric reasoning, and other cases where it is used as a part of symbolic reasoning, thereby moving forward from operations with numbers to operations with new objects of algebra. This analysis will contribute to a better understanding of the process of algebraization of mathematics in the Iberian Peninsula.

In this paper we refer to the works of the following authors: Marco Aurel, Juan Pérez de Moya, Antic Roca, Pedro Núñez and Diego Pérez de Mesa. We focus on the symbolism for the unknowns as well as on the symbols to indicate the operations and on the way the authors solved the systems of equations.

Context situation

Mathematics is the least studied discipline of the 16th century in Spain. This is partly due to what is called the polemic of Spanish science: after the unjustified praises from authors like Picatoste[2], Fernández Vallín[3] and Menéndez Pelayo[4], Rey Pastor[5] demonstrated that some of this praise was unjustified and also analyzed some texts. His research is aimed at finding reasons to sadden the Spanish people or fill with them pride about their country, as well as seeking a great national figure. For this reason, Rey Pastor does not take the processes of diffusion and transmission of the knowledge into account; neither does he see scientific activity as part of a social reality. His work "Spanish Mathematicians of the 16th century" has for many years been the only source consulted on this topic, and regard for him as an authority has been a barrier to knowledge for the few researches into this subject. We must also take into account that Spain's communication with the rest of Europe almost ceased from 1557, when groups of protesters were arrested in Seville and Valladolid. One year later, King Philip II presided over the first of a series of autos-da-fe that culminated in the burning of the Spanish Protestants. This ideological repression amounted to a strict control by royal power and the Inquisition on intellectual activity, limited in principle to theology but soon spreading to other fields. These circumstances made the availability of European reference works more difficult in Spain.

Marco Aurel

The first printed text to include algebra is Luca Pacioli's (1445-1517) *Summa de arithmetica geometria proportione e proportionalita* published in Venice in 1494. The first book to be printed in the Iberian Peninsula that could be regarded as an algebraic treatise is[6]: *Despertador de ingenios. Libro Primero de Arithmetica Algebratica,* which was published in Valencia in 1552. Its author, Marco Aurel, was born in Germany and settled in Valencia, where he taught practical mathematics.

This book consists of 24 chapters. In the six first chapters the author exposes the properties of whole numbers, fractions and proportions, the rule of three, currency exchanges and progressions. From the 7th to the 12th chapter, Marco Aurel deals with square roots and cubic roots, binomials and apotomes and their square and cubic roots. In the 13th chapter he states the characters that he will subsequently use. From the 14th to the 21st, and in the 23rd and 24th chapters, he deals with the rules for solving the different types of equalities, while in the 22nd he addresses the binomials and apotomes.

Marco Aurel declares the characters he intends to use as follows:

> I want to put 10 characters in continuous proportion and call each of them by their name… the first one is called dragma or number; the second one root or thing…

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[1] [Pérez de Moya, 1558, 4]

[2] Felipe Picatoste (Madrid 1834- Madrid 1892) is the author of *Apuntes para una biblioteca científica española del siglo XVI: estudios biográficos y bibliográficos de ciencias exactas físicas y naturales y sus inmediatas aplicaciones en dicho siglo* (1891), in which he collects the biographies of Spanish scientists who were important in their own fields during the 16th century. He wrote this book as a reaction to Echegaray's speech on his entry in the Academy of Sciences and in which he reviled the Spanish scientific tradition.

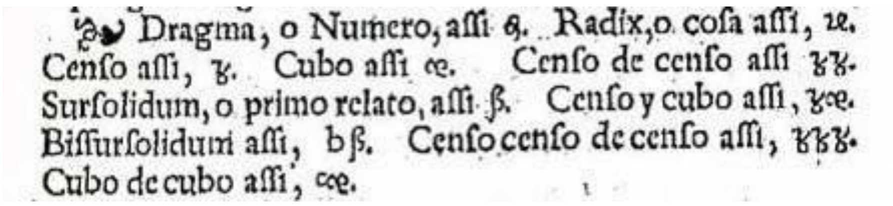
[3] Acisclo Fernández Vallín (Gijón 1825 – Madrid 1896) is the author of *Cultura científica de España en el siglo XVI*.

[4] Marcelino Menéndez Pelayo (Santander 1856 – Santander 1812), best known as a literary critic, wrote *The Spanish Science* (1876) as a claim of an existence of a scientific Spanish tradition.

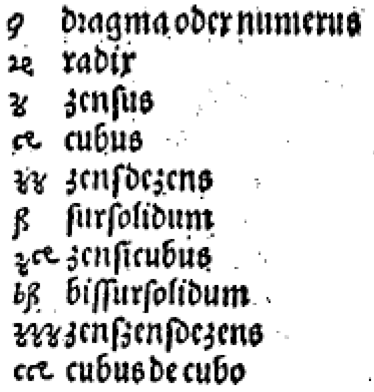
[5] Julio Rey Pastor (Logroño 1888 – Buenos Aires 1962) was a mathematician who made some incursions into the history of mathematics. In his inaugural speech to the academic year at Oviedo University in 1913, he followed Echegaray's line by not giving importance to Spanish mathematicians.

[6] This was the first algebraic treatise to appear in print, but not the first treatise in the Iberian Peninsula containing algebra, as Docampo has shown (Docampo, 2008).

> One writes the characters because they are short and to avoid writing them with all the letters …everyone can put the characters he wants…I put the following ones[7]
>
> [Marco Aurel, 1552, 69r]


Dragma, o Numero, assi ᵹ. Radix, o cosa assi, ℞. Censo assi, ʒ. Cubo assi ce. Censo de censo assi ʒʒ. Sursolidum, o primo relato, assi ß. Censo y cubo assi, ʒce. Bissursolidum assi, bß. Censo censo de censo assi, ʒʒʒ. Cubo de cubo assi, cce.

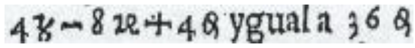
This notation shows a clear German influence, due probably to the origin of the author. In particular, the influence of Rudolff's work can be seen in the wording of several problems as well as in the notation, which Rudolff expresses as follows:


ᵹ dragma oder numerus
℞ radix
ʒ zensus
ce cubus
ʒʒ zensdezens
ß sursolidum
ʒce zensicubus
bß bissursolidum
ʒʒʒ zenszensdezens
cce cubus de cubo

[Rudolff, 1525, 174]

Aurel assigned to algebra the same names as Pacioli: *la regla de la cosa*, or *arte mayor*, or *Algebra e Almucabala*, which he translated as *restauratio e oppositio*[8] as Pacioli had translated.

To indicate addition and subtraction, he uses the signs + and –. To indicate the equality he writes the word "ygual"[9] and puts a symbol with the independent term.


4ʒ – 8℞ + 4ᵹ ygual a 36 ᵹ

[Aurel, 1552, 77r]

Rudolff uses the same symbols for addition and subtraction, and the word "gleich"[10] to indicate the equality.

Marco Aurel deals with the second quantity (systems of equations) in the 16th chapter of his algebra: «It deals with the rule of quantity and with some rules and requirements that can be done with it, and which one can refer to as a rule of second quantity» He solves 8 problems with very different wordings.

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[7] Quiero poner 10 caracteres en una continua proporción, y nombrar a cada uno por si, por su propio nombre que le conviene... el primero se llama dragma, o numero; el segundo rayz, o cosa…
Ponense los caracteres, porque son breves, y por evitar la prolixidad de escribir tales nombres a la larga…cada uno puede poner los que a el plazeran…Yo al presente pongo los siguientes.

[8] [Aurel, 1552, 68v]

[9] equal.

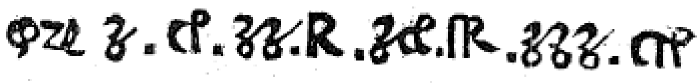
[10] equal.

The method to solve these kinds of problems is similar to the method by Luca Pacioli in his *Summa* and consists in putting the second unknown in terms of the first one, the same for the third unknown and so on. *Co*. being the first unknown, he puts *q* for the second, and when the second is expressed in terms of the first he also puts *q* for the third, and so on. This method does not allow these authors to raise the system of equations; they are obliged to work with every unknown rather than all the equations at the same time.

Juan Pérez de Moya

Aurel's book is probably one of the main sources for the most popular book written in the Iberian Peninsula during the 16th century, *Arithmetica practica y speculativa* (Salamanca, 1562) by Juan Pérez de Moya (Santisteban del Puerto, ca. 1513 - Granada, ca. 1597), which ran to 30 editions after the first publication in 1562, up to the last in 1798. In 1558 this author published the *Compendio de la Regla de la Cosa, o Arte Mayor,* which four years later was to form the seventh book of his *Arithmetica practica y speculativa.*

This work consists of nine books dealing with the properties of whole numbers, fractions, the rule of three, currency exchanges and also some practical rules of geometry. The last book is very different from the rest. It does not contain rules and is written as a dialogue between students about the importance of arithmetic, which the author wished to highlight. The rule of the thing is the seventh book, which is divided into fifteen chapters. In the second chapter Pérez de Moya provides some characters, which he says are invented for their brevity. These are:



Although Pérez de Moya gives the same characters than Aurel, he does not quote him as a source, and in the third chapter he states other characters he intends to use, because, as he says, there are no others in the print[11]. These characters are as follows: *n* for the number, *co*. for the thing, *ce*. for the census, *cce*. for the census of census, and so on, all of them belonging to the Italian tradition.

He uses no special symbol to indicate addition and subtraction, but the letters *p* and *m*, abbreviations of the Italian words "più" and "meno", which were used by Pacioli in his *Summa.* To indicate the equality he abbreviates the Spanish word "ygual" by putting "yg", and also puts a symbol with the independent term.

18. ço.m. 22. n.yg.à.10.co.p. 8. n.

[Pérez de Moya, 1562, 563]

The ninth article of the 12th chapter in the 7th book of the *Arithmética práctica y speculativa* (1562 edition) by Juan Pérez de Moya concerns the rule of second thing or quantity. He solves three problems, and the way to solve them is the same as that employed by Marco Aurel. In the 1573 *Tratado de Mathematicas* the author extended his work, particularly as regards the second quantity, which is the sixth article of the 51st chapter. An important novelty is the assignation of a letter to the third unknown, which is different from the letter assigned to the second one. In this work he quotes Cardano and Núñez thus:

> There are some other equalities which I do not include, because the wise have no need of them and beginners will not understand them. Whoever wishes to learn more should read Cardano's *Tenth Arithmetica*. And for the equalities in which the cube and the thing equal a number, they should read the end of the treatise on Algebra by doctor Pedro Nuñez, where he addresses the question with greater discretion than any of its former inventors.[12]

[11] Capítulo tercero. En el qual se declaran algunos caracteres que yo uso, por no aver en la stampa otros.

[12] Otras varias, y diversas ygualaciones ay que dexo de poner, porque para sabios no son menester, y para principiantes no se entenderan. Quien quisiere ver algo, lea el decimo d Arithmetica de Cardano. Y en las ygualaciones que el cubo y cosa, se ygyualaren a numero. Lea al doctor Pedro Nuñez, al fin del tratado de Algebra que lo trata mas discretamente, que ninguno de los que antes del lo inventaron (Pérez de Moya, 1573, 589).

Antic Roca

Antic Roca (c.1530-1580) was born in Girona and graduated in arts and philosophy in 1555 at the University of Barcelona, where in 1559 was elected a professor in arts[13].

Arithmetica (1565 edition, the first appeared in 1564) consists of 268 folios divided into two parts, each of which is made up of four books. Inside the cover of his *Arithmetica* the author says that it is a compilation of the other arithmetical works had been published up to that moment.

The first part of the book is basically a practical arithmetic, while the second deals with mercantile problems. The book belonging to this part contains the "rule of the thing".

In the XIII chapter, "On the characters that are useful for the continuous proportions and their names and significances", Antic Roca presents the characters. He says that they can be named in different ways and can also be stated in a different number.

He also notes that the quantity of characters described is not essential for the use of the rule. Nevertheless, he claims that the number of characters can be extended to infinity like the numbers themselves.

0.	1.	2	3.	4.	5.	6.	7.	8.	9.
N.	co.	ce.	cu.	cce.	r.	cecu.	rr.	ccce.	ccu.

[Roca, 1564, 258r]

He writes *mas*[14] to indicate addition and *me*[15] to indicate subtraction, and like Aurel and Pérez de Moya he puts a symbol with the independent term. Before writing this operation, he states that when the quantities have a different sign, then one must subtract the lesser from the greater, but fails to say which sign should be used. In the following operation the addition to the "*ce."* is incorrect.

3. cce. me. 8. cu. mas. 4. ce. mas. 7. co. ma. 5. n.
4. cce. me. 3. cu. me. 7. ce. mas. 5. co. me. 5. n.

7. cce. me. 11. cu. mas. 3. ce. mas. 12.

As regards the classification of the equalities, Roca says that he has decided to follow Marco Aurel and puts 4 simple and 4 compound ones.

They all correspond to those from Marco Aurel, while other expressions reproduce Marco Aurel's literally.

In the following equation we can see that Roca writes for the equality the Spanish word "yguales":

3.co.mas.2.ce.ſon yguales a.3000. nu.

[Roca, 1564, 263v]

In the fourth book of Roca's *Arithmetica*, in which he deals with the Major Art, there are no references to the rule of quantity.

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[13] The Arts courses were preparatory studies for joining higher faculties of Theology and Medicine.

[14] *Mas* is the whole Spanish word to express the addition.

[15] *Me* is an abbreviation of the Spanish word "menos, which expresses subtraction.

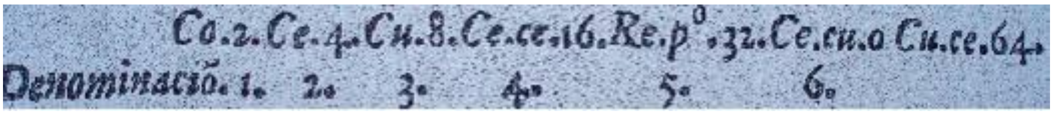
Pedro Núñez

The most remarkable mathematician in the Iberian Peninsula in the 16th century was probably the Portuguese Pedro Núñez (Alcácer do Sal, 1502- Coimbra, 1578), who studied medicine and mathematics and was a professor at the University of Salamanca as well as in Coimbra.

His book *De algebra en arithmetica y geometria,* which Núñez wrote in Portuguese during the 1530s, was not published until 1567 and appeared in Spanish. In the preface of this work, Núñez refers to the importance of Euclid's *Elements* to the foundations of algebra; he also refers to a the *Summa* from Pacioli[16] as the first printed algebra treatise, known in Spain only by a few people during the 60 years after its publication because of the obscure way in which it was written.

The algebra in Núñez's work is divided in three parts, although it does in fact consist of 5 parts, because the second one is divided into three other independent parts. In the first part, consisting of 6 chapters, Núñez gives the resolution to the first and second degree equations with one unknown. In the first chapter he deals with the purpose of algebra, its conjugations[17] and its rules. He says that the purpose of algebra is to highlight the unknown quantity and the way he will use for it is the equality. The quantities are as follows: *number*, *thing* and *census*.

He refers to them as "dignidades", and in the first chapter of the second part he gives the names for the first six quantities and explains how to obtain the others.


Co.2.Ce.4.Cu.8.Ce.ce.16.Re.p°.32.Ce.cu.o Cu.ce.64.
Denominació. 1. 2. 3. 4. 5. 6.

[Núñez, 1567, 24v]

He uses the symbol *p* to indicate addition and *m* to indicate subtraction. For the equality he writes the whole Spanish word and puts no symbol with the independent term.


20.co. p̃.7.ce. yguales a 30.co. p̃ 4.ce.m.3

[Núñez, 1567, 124r]

In the 5th and 6th chapters of the third main part, Núñez solves some problems that nowadays we would solve as systems of equations. In the 5th: *De la Practica de las Reglas de Algebra en los casos de Arithmetica,* Núñez solves 110 problems than can be solved using systems of equations, but in this chapter he makes no reference to a rule of quantity.

In the 6th chapter, Núñez deals with the rule of quantity: *About the rule of quantity simple or absolute,* and solves three problems. One of which is a problem with three unknowns; here he puts *co*. for the first and «quantity» for the second, without using any abbreviation. Using a reasoning similar to that by Aurel, Núñez puts the «quantity» in terms of the «*co*.». He works with the two first equations and also puts the third unknown in terms of the first, but without giving any name to the third unknown.

Diego Pérez de Mesa

Diego Pérez de Mesa was born in Ronda (Málaga) in 1563 and studied Arts and Theology at the University of Salamanca, where he took the courses taught by Jerónimo Muñoz (València 1520- Salamanca 1592) from his chair of astronomy and mathematics[18].

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16 He refers to Pacioli as Fra Luca de Burgo.

17 Conjugations is the name that Núñez uses for the equations.

18 Jerónimo Muñoz was born in València, where he studied Arts. After travelling in different countries to complete his education, he held the chair of Hebrew at the University of Ancona. He returned to València where he served as professor of Hebrew and mathematics between 1563 and 1578. From 1579 he held the chair of astronomy and mathematics at Salamanca.

Pérez de Mesa wrote several mathematic works but published none of them. Manuscript 2294 at the Library of the Salamanca University is one of his works. It is a double faced 100-page treatise dated 1598. The first part deals with arithmetics and the second, which deals with algebra, begins on page 60 and consists of an introduction and 23 chapters.

The last six chapters concern the resolution of equations and systems of equations. In this manuscript Pérez de Mesa states that the algebra is called "regla de la cosa" by some authors, since the "Italians" call "cosa" the "root" or "number".

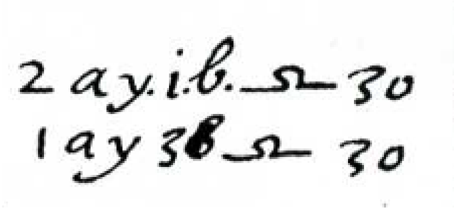
He quotes "Triputeon" when referring to the different names that algebra is called, and says that this author refers to it as "ciencia de la cuadratura". This author is Buteo, the latinized name of Jean Borrel[19], who in 1559 changed the name of algebra to the *Logistica* or, more specifically, *quadratura* and whom Pérez de Mesa also refers to in another manuscript dated 1592. He also quotes Diophantus, Pacioli, Stifel, Cardano, Tartaglia, Núñez, and Gosselin, and also refers to other "inferior" authors such as Roca and Moya.

In the twentieth chapter Pérez de Mesa studies the compound equalities, that is, the equalities that "after removing the superfluous quantities, which are those that have the character plus, and adding the tiny quantities, up to three dimensions remain". He says that it does not matter which dimensions they are, although the "writers" use the dimensions: *nº*, *£* and *q*, which can be placed in three ways.

The title of the last chapter of algebra in manuscript 2294 by Pérez de Mesa is "*de la Regla de la quantidad"* and deals with systems of equations.

For solving the systems of equations he takes no auxiliary unknown, but rather puts «a» for the first unknown, «b» for the second, and so on.

Then he sets out the system as follows:



and then solves it by linear combinations of equations. It is important to point out that the symbol for the "equality" is not introduced until the moment when he must operate with equations.

Concluding remarks

Not all the studied authors use the same symbols to indicate the unknown and its powers.

Neither Núñez nor Pérez de Mesa used any symbol to refer to the independent term. However, Aurel, Pérez de Moya and Roca did use a symbol.

The symbols used by Aurel are clearly from the German tradition and Rudolff is probably his main source.

Pérez de Moya's main source is Aurel, although he does not quote him and takes his symbolism from the Italian tradition.

Roca quotes many authors, but mainly follows Aurel.

Núñez is the most original author among these mathematicians. He says that the first book of algebra to appear in Spain was the *Summa* by *Fra Luca,* which is a rather untidy work. He also quotes Cardano and Tartaglia and critizes them for failing to be clear in their explanations.

Pérez de Mesa's main sources are probably the French authors. The way to solve systems of equations seems to be taken from Buteo, while his notation is similar to that used by Gosselin. On the first page of the manuscript by Pérez de Mesa devoted to algebra, he quotes Buteo, first calling him "Triputeon"[20] and later "Puteon". Buteo makes the system of equations

[19] French mathematician (ca. 1492-ca. 1570).

[20] Pérez de Mesa, 1598, 61.

explicit thus: [21]

3 A. 1 B. 1 C [42 | 1ª
1 A. 4 B. 1 C [32 | 2ª
1 A. 1 B. 5 C [40 | 3ª

Pérez de Mesa operates in the same way as Buteo.

In all the studied authors, the resolution of the quadratic equations is rhetorical, and although they present the symbols that they intend to use, these symbols do not modify the type of reasoning employed.

There is therefore no thread linking together the different algebras in the Iberian Peninsula during the 16th century. They otherwise draw their inspiration from different relevant works that appeared in Europe during the same period.

References

AUREL, Marco (1552). *Libro primero de Arithmetica Algebratica*, Valencia.

BORREL, Jean (1559). *Joan. Buteonis Logistica, quæ et arithmetica vulgo dicitur, in libros quinque digesta […]* Lyon, G. Roville.

DOCAMPO REY, Javier (2010). "Vernacular algebra in the Iberian Peninsula before Marco Aurel: Notations and terminology", in Hermann HUNGER, Felicitas SEEBACHER, Gerhard HOLZER (ed.) *Proceedings of the 3rd International Conference of the European Society for the History of Science,* Vienna, 2008, p. 85-92.

MASSA-ESTEVE, M. Rosa (2010). "The treatment of equations in the Iberian Peninsula after Marco Aurel (1552): the Great Art of Antic Roca" in Hermann HUNGER, Felicitas SEEBACHER, Gerhard HOLZER (ed.), *Proceedings of the 3rd International Conference of the European Society for the History of Science,* Vienna, 2008, p. 103-111.

NÚÑEZ SALACIENSE, Pedro (1567). *Libro de algebra en arithmetica y geometria*, Amberes, Herederos dí Arnoldo Birckman.

PACIOLI, Luca (1494). *Summa de Arithmetica, Geometria, Proportioni et Proportionalità*, Venecia, Paganino de Paganini, Venecia.

PÉREZ DE MESA, Diego (1598). *Libro y tratado del arismetica y arte mayor y algunas partes de astrología y matematicas*. Manuscript 2294 of the Library of Salamanca University.

PÉREZ DE MOYA, Juan (1558). *Compendio de la Regla de la Cosa, o Arte Mayor*. Burgos.

PÉREZ DE MOYA, Juan (1562). *Arithmetica práctica y speculativa,* Salamanca, 1562.

PÉREZ DE MOYA, Juan (1573). *Tratado de Mathematicas en que se contienen cosas de Arithmetica, Geometria, Cosmographia y Philosophia natural*, Alcalá de Henares, Juan Gracian.

REY PASTOR, Julio (1934). *Los matemáticos españoles del siglo XVI,* Madrid, Junta de Investigaciones Histórico-Bibliográficas, 1934.

ROCA, Antic (1564). *Arithmetica recopilación de todas las otras que se han publicado hasta agora*, Barcelona, Claudio Bornat.

ROMERO, Fàtima (2007). Una aproximació al pensament algebraic a l'Espanya del segle XVI. Estudi del manuscrit 2294 de la Biblioteca de la Universitat de Salamanca. (research work that can be consulted following the link: http://www.recercat.net/bitstream/2072/5158/1/TR+F.+Romero.pdf)

[21] Buteo, 1559, 190.

ROMERO, Fàtima (2010a). "The equalities and the rule of quantity in Pérez de Mesa's work" in Hermann HUNGER, Felicitas SEEBACHER, Gerhard HOLZER (ed.), *Proceedings of the 3rd International Conference of the European Society for the History of Science*, Vienna, 2008, p. 122-130.

ROMERO, Fàtima (2010b). "Les quantitats irracionals a l'àlgebra de Pedro Núñez", *Quaderns d'Història de l'Enginyeria,* 11.

RUDOLFF, Christoff (1525). *Behend und Hübsch Rechnung Durch die Kunstreichen Regeln Algebra, so Gemeincklich die Coss Genennt Werden*. Straßburg.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 405.

THE CIRCULATION OF SYMBOLIC LANGUAGE IN THE SEVENTEENTH CENTURY

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Abstract

This research on the symbolic language is framed within the context of a more extensive research concerning the transformations of mathematics and natural philosophy and their relationship between the fifteenth century and the seventeenth century.
The publication in 1591 of In artem analyticen isagoge *by François Viète (1540–1603) constituted an important step forward in the development of a symbolic language. As his work came to prominence at the beginning of the 17th century, other authors also began to consider the utility of symbolic language and algebraic procedures for solving all kinds of problems. Thus, symbolic language and its use in different forms became more widely disseminated, though this fact did not imply that it was generally accepted.*
In this paper, after presenting some examples of different reactions to the acceptance of symbolic language and algebraic procedures, such as those of Cavalieri, Barrow and Hobbes, I would like to analyze some examples of the connection between the transmission and reception of symbolic language and its appropriation by other scientists such as Hérigone and Mengoli or Mengoli and Leibniz. These analyses can provide new ideas for a better understanding of the algebraization of mathematics.

Introduction

This research into symbolic language is framed within the context of more extensive research into the relations between the transformations of mathematics and natural philosophy that took place from the fifteenth century to the end of the seventeenth century[1].

We analyze the relations between the following three works referred to in chronological order: *In Artem analyticen Isagoge* (1591) by François Viète (1540-1603); *Cursus Mathematicus* (1634-1637-1642) by Pierre Hérigone (1580-1643) and *Geometriae Speciosae Elementa* (1659) by Pietro Mengoli (1626/7-1686).

First of all, in order to address the circulation of symbolic language, one should consider the notation of the works under study. Certain differences exist between Hérigone's and Mengoli's signs and those used by Viète, in whose case it is

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[1] See Massa, 2006a.

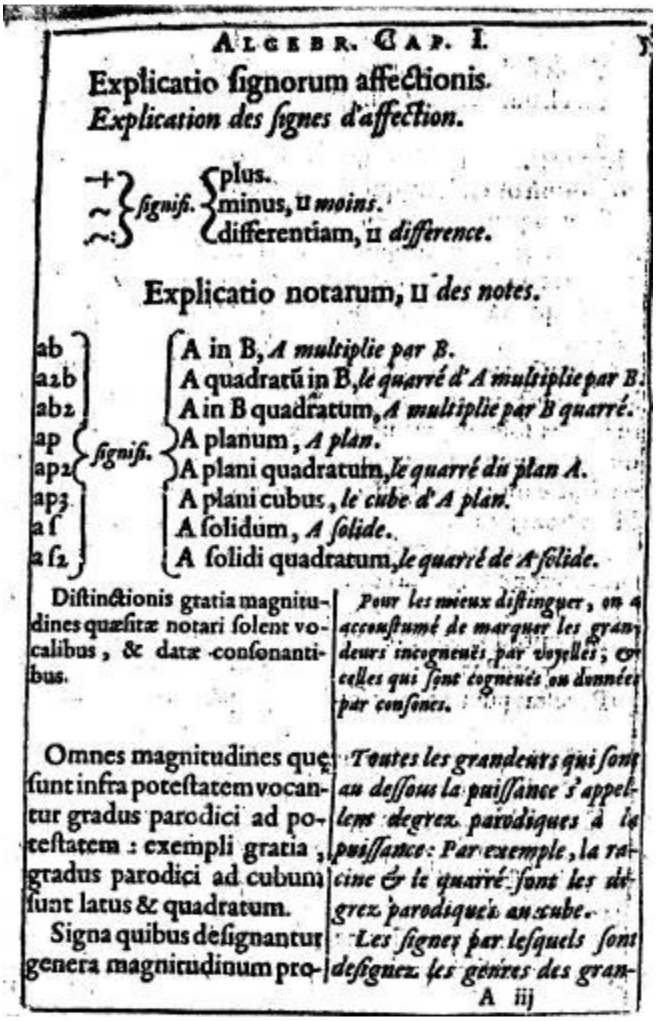
not the notation explicitly that circulates. There are in fact only a few coincidences between Hérigone's and Mengoli's notation. So the question to be posed is: In what sense can we speak about reception or appropriation between these authors? The aim of this article is to answer this question by discussing some connections between the reception of symbolic language and its singular appropriation by these authors in the seventeenth century (1591-1659).

The appropriation of Viète's algebra by Hérigone

The publication in 1591 of *In artem analyticen isagoge* by François Viète constituted an important step forward in the development of a symbolic language.

Viète used symbols to represent both known and unknown quantities, and was thus able to investigate equations in a completely general form[2]. However, if we observe Viète's equation, we can appreciate the rhetorical form. We provide one example to show how Viète writes an equation; this letter B represents the known quantity: "B in A – Aquad. Aequatur Zquad", in modern notation, $Bx - x^2 = Z^2$.

Viète showed the usefulness of algebraic procedures for analyzing and solving problems in arithmetic, geometry and trigonometry. As his work came to prominence at the beginning of the 17th century, other authors also began to consider the utility of algebraic procedures for solving all kinds of problems. Viète's work was diffused through many other algebra texts, such as the section entitled Algebra in the second volume of *Cursus Mathematicus* by Pierre Hérigone in 1634.



ALGEBR. CAP. I.

Explicatio ſignorum affectionis.
Explication des ſignes d'affection.

+ } *ſignif.* { plus.
~ } *ſignif.* { minus, ū *moins.*
~: } *ſignif.* { differentiam, ū *difference.*

Explicatio notarum, ū *des notes.*

ab — A in B, *A multiplie par B.*
a2b — A quadratū in B, *le quarré d'A multiplie par B.*
ab2 — A in B quadratum, *A multiplie par B quarré.*
ap — A planum, *A plan.*
ap2 — A plani quadratum, *le quarré du plan A.*
ap3 — A plani cubus, *le cube d'A plan.*
a ſ — A ſolidum, *A ſolide.*
a ſ2 — A ſolidi quadratum, *le quarré de A ſolide.*
(*ſignif.*)

Diſtinctionis gratia magnitudines quæſitæ notari ſolent vocalibus, & datæ conſonantibus.

Pour les mieux diſtinguer, on a accouſtumé de marquer les grandeurs incogneuës par voyelles; & celles qui ſont cogneuës ou données par conſones.

Omnes magnitudines quę ſunt infra poteſtatem vocantur gradus parodici ad poteſtatem: exempli gratia, gradus parodici ad cubum ſunt latus & quadratum.

Toutes les grandeurs qui ſont au deſſous la puiſſance s'appellent degrez parodiques à la puiſſance: Par exemple, la racine & le quarré ſont les degrez parodiques au cube.

Signa quibus deſignantur genera magnitudinum pro-

Les ſignes par leſquels ſont deſignez les genres des gran-

A iij

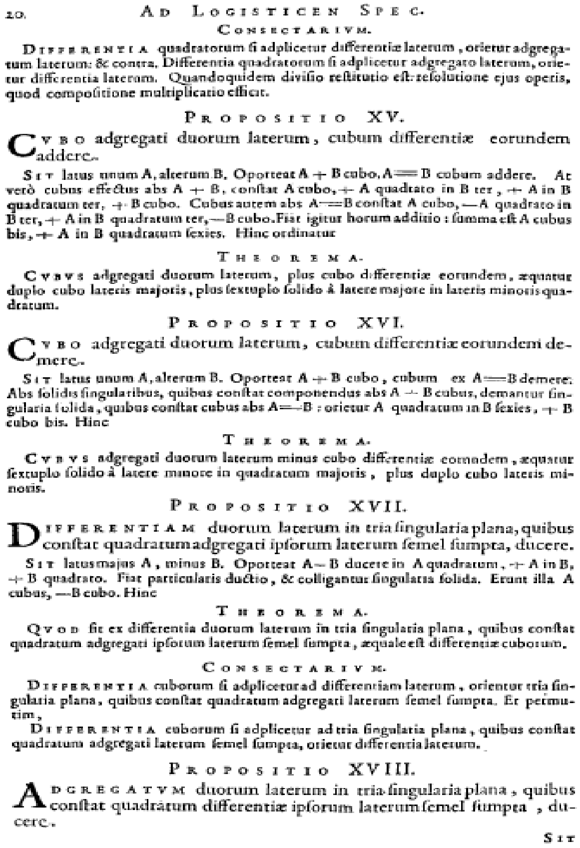
Fig. 1.

[2]The purpose of Viète's analytical art was to solve all kinds of problems. For more information see Viète, 1970 and 1983.

Hérigone wrote an encyclopedic textbook consisting of five volumes (six volumes in the second edition) entitled *Cursus Mathematicus, nova, brevi et clara methodo demonstratus, per notas reales & universales, Citra usum cuiuscunque idiomatis, intellectu faciles*. Published in parallel French and Latin columns arranged on the same page, the first four volumes appeared in 1634, the fifth in 1637, and a sixth in 1642 as a supplement to the second edition. The first and second volumes of *Cursus* deal with pure mathematics. The first volume contains Euclid's Elements and Data, and Apollonius's Coniques. The second volume is devoted to arithmetic and algebra. The third and fourth volumes deal with mixed mathematics, that is to say, with the mathematics required for practical geometry, military or mechanical uses, geography, and navigation. The fifth and last volume of the first edition, published in 1637, includes spherical trigonometry and music. Later, in the second edition (1642), he adds the sixth and final volume, which contains two parts dealing with algebra; it also deals with perspective and astronomy.

Algebra is a section of volume 2, which consists of 20 chapters[3]. From the description of Algebra we realize that Hérigone presents the same parts as Viète's works, and although his notation, presentation and procedures in his algebraic proofs were quite different from Viète's, Hérigone generally used Viète's statements[4].

As an example, we can see the explanation of the notation where Hérigone tries to change Viète's notation. See Figure 1.



20. AD LOGISTICEN SPEC.

CONSECTARIVM.

DIFFERENTIA quadratorum si adplicetur differentiæ laterum, orietur adgregatum laterum: & contra, Differentia quadratorum si adplicetur adgregato laterum, orietur differentia laterum. Quandoquidem divisio restitutio est resolutione ejus operis, quod compositione multiplicatio efficit.

PROPOSITIO XV.

CVBO adgregati duorum laterum, cubum differentiæ eorundem addere.

SIT latus unum A, alterum B. Oporteat A + B cubo, A == B cubum addere. At verò cubus effectus abs A + B, constat A cubo, + A quadrato in B ter, + A in B quadratum ter, + B cubo. Cubus autem abs A == B constat A cubo, — A quadrato in B ter, + A in B quadratum ter, — B cubo. Fiat igitur horum additio: summa est A cubus bis, + A in B quadratum sexies. Hinc ordinatur

THEOREMA.

CVBVS adgregati duorum laterum, plus cubo differentiæ eorundem, æquatur duplo cubo lateris majoris, plus sextuplo solido à latere majore in lateris minoris quadratum.

PROPOSITIO XVI.

CVBO adgregati duorum laterum, cubum differentiæ eorundem demere.

SIT latus unum A, alterum B. Oporteat A + B cubo, cubum ex A == B demere. Abs solidis singularibus, quibus constat componendus abs A + B cubus, demantur singularia solida, quibus constat cubus abs A == B: orietur A quadratum in B sexies, + B cubo bis. Hinc

THEOREMA.

CVBVS adgregati duorum laterum minus cubo differentiæ eorundem, æquatur sextuplo solido à latere minore in quadratum majoris, plus duplo cubo lateris minoris.

PROPOSITIO XVII.

DIFFERENTIAM duorum laterum in tria singularia plana, quibus constat quadratum adgregati ipsorum laterum semel sumpta, ducere.

SIT latus majus A, minus B. Oporteat A — B ducere in A quadratum, + A in B, + B quadrato. Fiat particularis ductio, & colligantur singularia solida. Erunt illa A cubus, — B cubo. Hinc

THEOREMA.

QVOD fit ex differentia duorum laterum in tria singularia plana, quibus constat quadratum adgregati ipsorum laterum semel sumpta, æquale est differentiæ cuborum.

CONSECTARIVM.

DIFFERENTIA cuborum si adplicetur ad differentiam laterum, orientur tria singularia plana, quibus constat quadratum adgregati laterum semel sumpta. Et permutim,

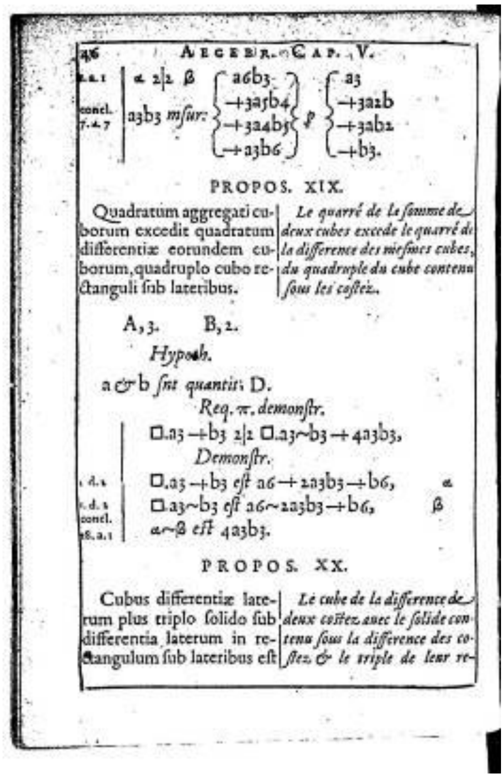
DIFFERENTIA cuborum si adplicetur ad tria singularia plana, quibus constat quadratum adgregati laterum semel sumpta, orietur differentia laterum.

PROPOSITIO XVIII.

ADGREGATVM duorum laterum in tria singularia plana, quibus constat quadratum differentiæ ipsorum laterum semel sumpta, ducere.

SIT

Fig. 2.



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α 2|2 β

a3b3 msur: { a6b3, -+3a5b4, -+3a4b5, -+a3b6 } β { a3, -+3a2b, -+3ab2, -+b3.

PROPOS. XIX.

Quadratum aggregati cuborum excedit quadratum differentiæ eorundem cuborum, quadruplo cubo rectanguli sub lateribus.	*Le quarré de la somme de deux cubes excede le quarré de la difference des mesmes cubes, du quadruple du cube contenu sous les costez.*

A, 3. B, 2.

Hypoth.

a & b *snt quantit.* D.

Req. π. demonstr.

□.a3 -+b3 2|2 □.a3~b3 -+ 4a3b3,

Demonstr.

□.a3 -+b3 *est* a6 -+ 2a3b3 -+ b6, α

□.a3~b3 *est* a6~2a3b3 -+ b6, β

α~β *est* 4a3b3.

PROPOS. XX.

Cubus differentiæ laterum plus triplo solido sub differentia laterum in rectangulum sub lateribus est	*Le cube de la difference de deux costez avec le solide contenu sous la difference des costez & le triple de leur re-*

Fig. 3.

[3] Algebra includes: 1: Several definitions and notations. 2, 3: Operations involving simple and compound algebraic expressions. 4: Operations involving ratios. 5: Proofs of several theorems. 6, 7: Rules for dealing with equations, which are the same as those in Viète's work [These rules were: the reduction of fractions to the same denominator ("isomerie"), the reduction of the coefficient of the highest degree ("parabolisme"), the depression of the degree ("hypobibasme") and the transposition of terms ("antithese")]. 8: An examination of theorems by "poristics". 9: Rules of the "rhetique" or exegetic in equations up to the second degree. 10–13: Solutions of several problems and geometric questions using proofs (determined by means of analysis). 14: Solutions of several "ambiguous" equations. 15: Solutions of problems concerning squares and cubes, referred to as Diophantus' problems. 16–19: Calculation of irrational numbers. 20: Several solutions of "affected" (negative sign) powers.

[4] On Hérigone see Massa, 2008 and 2010.

If we compare Hérigone's presentation of an identity proposition with Viète's similar identity, we can see that Viète gives rhetorical explanations and verbal descriptions, uses few symbols, employs capital letters to represent quantities, leaves no margins and writes the words "cubus", "quadratus", etc. to express powers. In contrast, Hérigone formulates all identities and properties, and even some statements, in symbols, with no rhetorical explanations or verbal descriptions. Compare Figure 2 and Figure 3.

The most important point therefore is how Hérigone appropriates Viète's proofs and transforms this rhetorical explanation into symbolic lines. In fact, Hérigone devises a new method to make the proofs. So, in the title he says: "A Course of Mathematics demonstrated by a brief and clear new method through real and universal symbols, which are easily understood without the use of any language". Hérigone also claims that he had invented a brief and intelligible new method for making proofs in the preface, which bore the dedication "Au lecteur" [To the reader]

> There is no doubt at all that the best method for teaching the sciences is that in which brevity is combined with ease. But it is not always easy to attain both, particularly in mathematics, which, as Cicero pointed out, is highly obscure. Having considered this myself, and seeing that the greatest difficulties are in the demonstrations, understanding of which depends on a knowledge of all parts of mathematics, I have devised a new method, both brief and clear, of making demonstrations, without the use of any language[5].

Indeed, Hérigone's stated aim in the *Cursus* was to introduce a symbolic language as a universal language for dealing with both pure and mixed mathematics by means of a new method. Moreover, Hérigone stressed the importance of knowing the symbols and understanding the proofs performed with this notation.

Hérigone's new method

We can distinguish three features in Hérigone's new method: the original system of notation, the axiomatic-deductive reasoning and the presentation of the propositions.

As regards the first feature, Hérigone uses an original system of notation with new symbols and abbreviations to represent algebraic expressions, numbers and signs. He provides alphabetically ordered tables of abbreviations (which he calls « explicatio notarum »); see Table 1.

He also gives tables of explanations of the citations ("explicatio citationum") at the beginning of each of the volumes that make up the Cursus; see Table 2. The citations always refer either to propositions in Euclid's Elements or to the Cursus itself. Thus, for example, "C.17.I" means "Corollarium decimae septimae primi. Corollaire de la dix-septiesme du premier".

The second feature refers to Hérigone's use of an axiomatic-deductive reasoning:

> In this method there is nothing that has not already been explained and conceded in the premises…And as each consequence depends immediately on the proposition cited, the demonstration follows from beginning to end by a continue series of legitimate, necessary and immediate consequences [6].

The third feature addresses the presentation of the propositions. Hérigone divides his propositions into separate sections: hypothesis (known and unknown quantities), explanation or requirement, proof, and conclusion. In the margin he writes the number of propositions of Euclid's Elements that he is using. Occasionally, he gives the numerical solution (for example in an equation) in a section headed "Determinatio". In geometric constructions, he provides the instructions needed to make the drawing in a paragraph named "Constructio". Hérigone claims:

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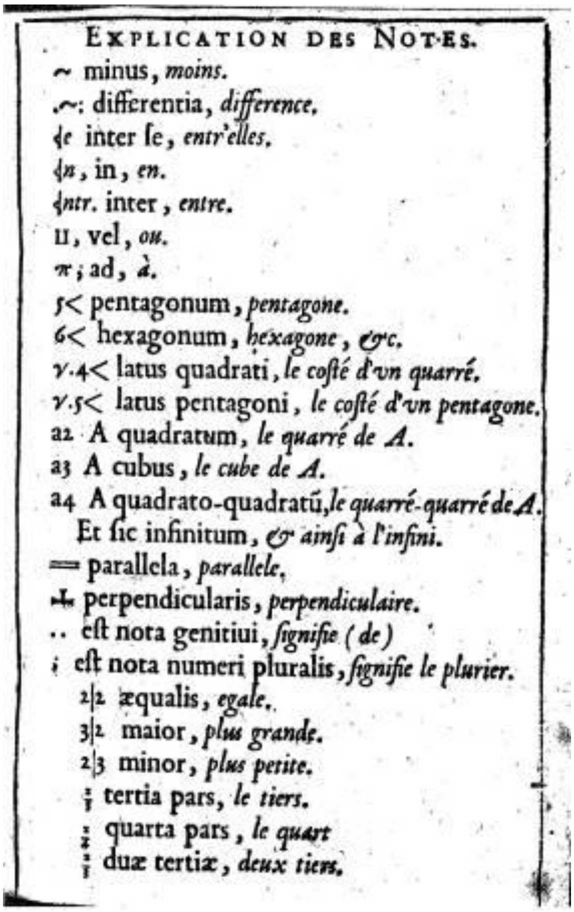
[5] « Car on ne doute point, que la meilleure méthode d'enseigner les sciences est celle, en laquelle la brièveté se trouve conjointe avec la facilité : mais il n'est pas aisé de pouvoir obtenir l'une & l'autre, principalement aux Mathématiques, lesquelles comme témoigne Ciceron, sont grandement obscures. Ce que considérant en moi-même, & voyant que les plus grandes difficultés estoites aux démonstrations, de l'intelligence desquelles dépend la connaissance de toutes les parties des Mathématiques, j'ai inventé une nouvelle méthode de faire les démonstrations, briefe & intelligible, sans l'usage d'aucune langue. » "Nam extra controversiam est, optimam methodum tradendi scientias, esse eam, in qua brevitas perspicuitati coniungitur, sed utramque assequi hoc opus hic labor est, praesertim in Mathematis disciplinis, quae teste Cicerone, in maxima versantur difficultae. Quae cum animo perpenderem, perspectum que haberem, difficultates quae in erudito Mathematicorum pulvere plus negotijs facessunt, consistere in demonstrationibus, ex quarum intelligentia Mathematicarum disciplinarum omnis omnino pendet cognitio: excogitavi novam methodum demonstrandi brevem, & citra ullius idiomatis usum intellectu facilem." [Hérigone, 1634, I, unpaginated].

[6] En cette methode on ne dit rien qui n'aye esté expliqué & concedé aux premises... Et parce que chaque consequence depend immediatament de la proposition citée, la demonstration s'entretien depuis son commencement jusques à la conclusion, par une suite continue de consequences legitimes, necessaires & immediates »/ In hac methodo nihil adfertur, nisi fuerit in praemissis explicatum & concessum...Et quoniam singulae consequentiae ex propositionibus allegatis immediate pendent, demonstratio ab initio ad finem, serie continua, legitimaru, necessariarum que consecutionum immediatarum. Hérigone, 1634, I, *Ad Lectorem*, To the reader.

> The distinction of the proposition in its members, that is, the part in which the hypothesis is advanced, the explanation of the requirement, the construction or preparation and the demonstration, likewise relieves the memory and makes it very helpful for understanding the demonstration[7].



EXPLICATION DES NOTES.
~ minus, *moins.*
∽: differentia, *difference.*
4e inter ſe, *entr'elles.*
4n, in, *en.*
4ntr. inter, *entre.*
ıı, vel, *ou.*
π; ad, *à.*
5< pentagonum, *pentagone.*
6< hexagonum, *hexagone, &c.*
ν.4< latus quadrati, *le coſté d'vn quarré.*
ν.5< latus pentagoni, *le coſté d'vn pentagone.*
a2 A quadratum, *le quarré de A.*
a3 A cubus, *le cube de A.*
a4 A quadrato-quadratū, *le quarré-quarré de A.*
Et ſic infinitum, *& ainſi à l'infini.*
= parallela, *parallele.*
⊥ perpendicularis, *perpendiculaire.*
.. eſt nota genitiui, *ſignifie (de)*
; eſt nota numeri pluralis, *ſignifie le plurier.*
2|2 æqualis, *egale.*
3|2 maior, *plus grande.*
2|3 minor, *plus petite.*
1/3 tertia pars, *le tiers.*
1/4 quarta pars, *le quart*
2/3 duæ tertiæ, *deux tiers.*

Table 1.

EXPLICATIO CITATIONVM.	
c.l.60.10	Corollarium lemmatis ſexageſimæ decimi. / *Corollaire du lemme de la ſoixantiéme du dixiéme.*
2.ſe.det.	Secunda de ſectione determinata. / *Seconde de la ſection determinée.*
1ſec.ſpa.	Prima de ſectione ſpatij. / *Premiere de la ſection de l'eſpace.*
1.ſec.pr.	Prima de ſectione proportionis. / *Premiere de la ſection de la proportion.*
2.incli.	Secunda de inclinatione. / *La ſeconde d'inclination*
8.t.	Octaua tactionum. / *Huictieſme des attouchements.*
l.9.t.	Lemma nonæ tactionum. / *Lemme de la neufieſme des attouchements.*
2.ang.	Secunda angularium ſectionum. / *Seconde de la ſection des angles.*
conſtr.	Conſtructio, id eſt, per conſtructionem. / *Conſtruction, c'eſt à dire, par la conſtruction.*
ſymp.	Symperaſma, *ſymperaſme.*

Symperaſma eſt finis conſtructionis, qua peracta, aſſerimus conſtructum aut inuentum eſſe quod iubet problema, itaque in ſymperaſmate loquimur ſic.	*Symperaſme eſt la fin de la conſtruction, laquelle eſtant acheuée, on affirme qu'on a conſtruict ou inuenté ce que demande le probleme, partant au ſymperaſme on parle ainſi.*

Propoſ. 1. libr. 1.

ſymp. \| △abc *eſt æquilat.*

Dico triangulum ABC eſſe æquilaterum.	*Ie dis que le triangle ABC eſt equilateral.*

Table 2.

Hérigone's method in the demonstrations

Hérigone's originality resides not only in the explicit explanation of axiomatic-deductive reasoning, but also for using syllogisms, because in the demonstrations one can find in one symbolic line the major premise and the conclusion, using the former symbolic line as the minor premise. Hérigone's claims:

> The demonstration follows from beginning to end by a continue series of legitimate, necessary and immediate consequences, each one included in a short line, which can be solved by syllogisms, because in the proposition cited as well as in that which corresponds to the citation one can find all parts of the syllogism[8].

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[7] « La distinction de la proposition en ses membres, savoir en l'hypothese, l'explication du requis, la construction, ou preparation, & la demonstration, soulage aussi la memoire, & sert grandement à l'intelligence de la demonstration. » / Praeterea distinctio propositionis in sua membra, scilicet in hypothesin, explicationem quaesiti, constructionem, vel praeparationem, & demonstrationem non parum iuvat quoque memoriam, & ad intelligendam demonstrationem multùm prodest. Hérigone, 1634, I, *Ad lectorem,* To the reader.

[8] La demonstration s'entretien depuis son commencement jusques à la conclusion, par une suite continue de consequences legitimes, necessaires & immediates, contenues chacune en une petite ligne, lesquelles se peuvent resoudre facilement en syllogismes, à cause qu'en la proposition citée, & en celle qui correspond à la citation, se trouvent toutes les parties du syllogisme: comme on peut voir en la premiere demonstration du premier livre, qui a esté reduite en syllogismes./demonstratio ab initio ad finem, serie continua, legitimarum, necessariarumque consecutionum immediatarum, singulis lineolis comprensarum aptè cohaeret: quarum unaquaeque

We analyze the demonstration of Proposition I in the first book by syllogisms and the identification of the premises in the demonstration.

At the beginning, Hérigone states: "This demonstration is made by four syllogisms, as one can perceive from the number of citations".

Then he explains all the syllogisms.

"III SYLLOGISM.

- Those things that are equal to the same are equal to each other.
- But the straight lines AC & CB are equal to the same straight line.
- Therefore the straight lines AC & CB are equal to each other"[9].

So, in the III syllogism, Hérigone writes: "I. axiom. 1. AC = BC", the major premise is the first axiom of Euclid's book I, while the minor premise is deduced from the conclusions of the first and second syllogisms: AC= AB and BC=BA, and the conclusion of this third syllogism is AC=BC. These conclusions enable the minor premise in the last syllogism to be deduced.

"IV SYLLOGISM.

- All triangles that have three equal sides are equilateral.
- But the triangle ABC has three equal sides.
- Therefore the triangle ABC is equilateral"[10].

In this case, Hérigone writes:" I. definition. 23. ABC is an equilateral triangle", the major premise is I.d.23, while the minor premise is deduced from the former conclusions AC= AB, BC=BA and AC=BC, and the conclusion of the third syllogism is that "the triangle ABC is equilateral", which concludes the demonstration.

Hérigone's originality resides not in demonstrating by syllogisms, but rather in that it is possible to identify all parts of the syllogism in symbolic lines, which transforms the demonstration by syllogisms into another one that is shorter and easier. Indeed, it is important to point out that Hérigone sought to introduce a new, briefer and more intelligible method for making demonstrations.

The influence of Hérigone's method on Mengoli's *Geometriae*

In this section we wish to show how Hérigone's method, devised for a better understanding of Mathematics, was used by Pietro Mengoli for obtaining new results in his *Geometriae* 30 years later.

Mengoli's *Geometriae Speciosae Elementa* (Bologna, 1659) is a text in pure mathematics consisting of 472 pages with six *Elementa* whose title: "Elements of Specious Geometry" already indicates the singular use of symbolic language in this work, and particularly in Geometry. Mengoli unintentionally created a new field, a "specious geometry" modeled on Viète's "specious algebra", since he worked with "specious" language, that is to say, symbols used to represent not just numbers, but also values of any abstract magnitudes. He deals with limits, logarithms and quadratures[11].

In the introductory letter to Fernando Riario, Mengoli reveals his sources in a reference to Viète's algebra:

> Both geometries, the old form as found in Archimedes, and the new form of indivisibles given by my tutor, Bonaventura Cavalieri, as well as Viète's algebra, are regarded as pleasurable by the

nullo negotio in syllogismum potest converti, quòd in propositione citata, & in ea quae citationi respondet, omnes syllogismi partes reperiatur: ut videre est in prima libri primi demonstratione, quae in syllogismos est conversa [Hérigone, 1634, I, *Ad Lectorem*].

[9] III. SYLLOGISME. Les choses égales à une mesme, sont égales entr'elles. Mais les lignes droites AC & CB sont égales à une mesme ligne droite. Donc les lignes droites AC & BC sont égales entr'elles./III. SYLLOGISMUS. Quae eidem aequalia sunt, inter se sunt aequalia. Sed rectae AC & BC sunt eidem rectae aequales. Igitur rectae AC & BC sunt inter se aequales. [Hérigone, 1634, I, p. 2].

[10] IV. SYLLOGISME. Tout triangle qui a trois costez égaux, est equilateral. Mais le triangle ABC a trois costez égaux. Donc le triangle ABC est equilateral. /IV. SYLLOGISMUS. Omne triangulum habens tria latera aequalia, est aequilaterum. Sed triangulum ABC tria habet aequalia latera. Igitur triangulum ABC est aequilaterum. [Hérigone, 1634, I, p.2].

[11] On Mengoli's quadratures see Massa, 2006a and 2009a.

learned. Neither through their confusion nor through their perfect conjunction, a somewhat new form [of geometry will arise] — our own— which cannot displease anyone[12].

Mengoli also claims as a source Hérigone's algebra: "To those symbols that Viète, Hérigone, Beaugrand…"[13].

Like Hérigone, at the beginning of the Geometria and on a separate page under the title *Explicationes quarundam notarum*, he explains the basic notation he intends to use. Note that for representing the powers, Mengoli, like Hérigone, wrote the exponent on the right of the letter. See Table 3.

Explicationes quarumdam notarum.

Additio ſignificabitur, charactere crucis: vt ex *a*, & *r*, collecta ſumma, *a* + *r*.

Subtractio, charactere lineolæ: vt ex *t*, dempta *a*, relinquit differentiam, *t* — *a*.

Æqualitas, ea interpunctione ſignificabitur, qua partes principes periodi ſolent diſtingui. vt quod *a* + *r*, eſt æqualis ipſi *t*,

a + *r* : *t*.

Ratio ſignificabitur interpunctione, qua maximæ partes periodi ſubdiſtinguuntur; ſcilicet puncto, & commate. vt ratio *a* ad *r*, ſcribendo,

a ; *r*.

Itaque proportio *a* ad *r*, ſicut *a*2 ad *ar*, ſignificabitur, ſcribendo,

a ; *r* : *a*2 ; *ar*.

Et compoſita ratio ex rationibus. velut ex *u* ad *a*2, & *u* ad *r*3, compoſita *u* ad *a*2*r*3, ſcribendo,

u ; *a*2, + *u* ; *r*3 : *u* ; *a*2*r*3.

vbi comma, inter *a*2, & crucem, vtiliter diſtinguit, ad ſignificandum, non quantitatum *a*2, & *u*, ſummam *a*2 + *u*, ſed rationum.

Multiplicata quoque ratio, ſignificabitur. velut *a*3 ad *r*3, triplicata rationis *a* ad *r*, ſcribendo,

*a*3 ; *r*3 : triplicata *a*; *r*.

Theo.

Table 3.

The role of symbolic language in Mengoli's *Geometriae* is both significant and original. In fact, the arithmetic manipulation of algebraic expressions helped Mengoli to obtain new results and new procedures. For instance, in the *Elementum secundum* of his *Geometriae* he invented a new way of writing and calculating finite summations of powers and products of powers. He did not give them values or wrote them using the sign + and suspension points (…), but rather created an innovative and useful symbolic construction that would allow him to calculate these summations (which he calls species), which he regarded as new algebraic expressions.

In order to compute the value of these summations Mengoli displays them in a triangular table. Indeed, throughout the book he uses triangular tables as useful algebraic tools for calculations. In the *Elementum primum*, the terms of the triangular tables are numbers and are used to obtain the development of any binomial power. In the *Elementum secundum*, the terms are summations and are used to obtain their values. Finally, in the *Elementum sextum*, the terms are geometric figures or forms and are used to obtain the quadratures of these geometric figures. It is our contention that Mengoli's originality stemmed not from the presentation of these tables but rather from his treatment of them. On the one hand, he used the combinatorial triangle and symbolic language to create other tables with algebraic expressions, clearly stating their

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[12] Ipsae satis amabiles litterarum cultoribus visae sunt utraque Geometria, Archimedis antique, & Indivisibilium nova Bonaventura Cavallerij Praeceptoris mei, necnon & Viettae Algebra: quarum non ex confusione, aut mixione, sed coniuntis perfectionibus, nova quaedam, & propria laboris nostri species, nemini poterir displicere [Mengoli, 1659, 2-3].

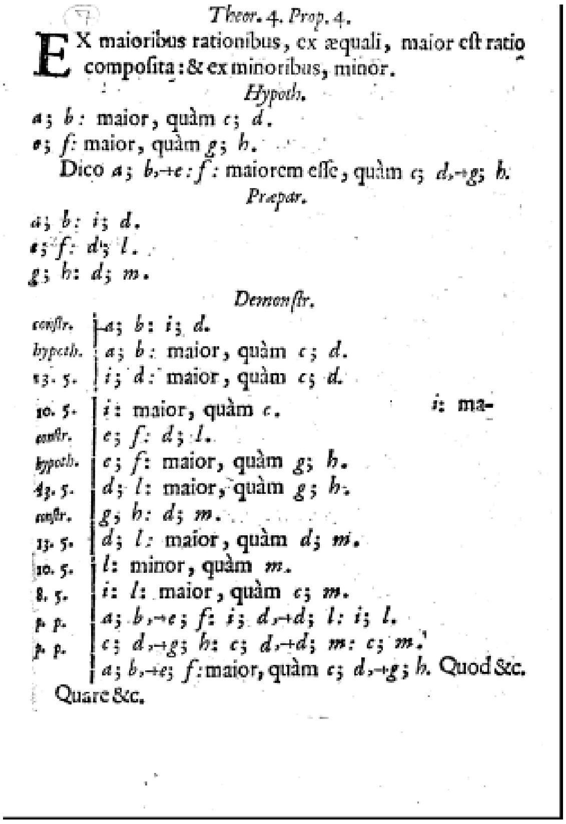
[13] Quibus characteribus à Vietta, Herigonio, Beaugrand…[Mengoli, 1659, 12].

laws of formation; on the other hand, he employed the relations between these expressions and the binomial coefficients to prove results such as the sum of the pth-powers of the first t-1 integers[14].

An important point therefore is to be found in Mengoli's triangular tables, the principal source for which is found in Hérigone's *Cursus*. In the *Elementum primum*, Mengoli defines the *Tabula proportionalium*, which he says he has constructed similarly to others in Euclid's Elements (Prop. 2 in Book 8). If you read Euclid's Elements, you will not find this triangle, but if you read Hérigone's Elements (Tome 1 of *Cursus* I) you will. We therefore conclude that Mengoli must have read about Viète's specious algebra in Hérigone's *Cursus*, and in this book found one of these principal tools, that is to say, triangular tables.

The most important relation, however, lies in the proofs and in the presentation of the propositions. Mengoli's propositions, like those by Hérigone, are divided into parts, such a "Hypothesis", "Praeparatio" and "Demonstratio". Mengoli, who was influenced by Hérigone's idea of symbolic language as a powerful tool, also absorbed his method of presenting demonstrations.

Like those by Hérigone, Mengoli's demonstrations are expressed in symbolic language with logical statements consisting of a few lines. We can identify the syllogisms in the lines of this proposition: See Figure 4.



Theor. 4. Prop. 4.

EX maioribus rationibus, ex æquali, maior est ratio composita: & ex minoribus, minor.

Hypoth.

a; *b*: maior, quàm *c*; *d*.
e; *f*: maior, quàm *g*; *h*.
Dico *a*; *b*,→*e*: *f*: maiorem esse, quàm *c*; *d*,→*g*; *h*.

Præpar.

a; *b*: *i*; *d*.
e; *f*: *d*; *l*.
g; *h*: *d*; *m*.

Demonstr.

constr.	*a*; *b*: *i*; *d*.
hypoth.	*a*; *b*: maior, quàm *c*; *d*.
13. 5.	*i*; *d*: maior, quàm *c*; *d*.
10. 5.	*i*: maior, quàm *c*.
constr.	*e*; *f*: *d*; *l*.
hypoth.	*e*; *f*: maior, quàm *g*; *h*.
13. 5.	*d*; *l*: maior, quàm *g*; *h*.
constr.	*g*; *h*: *d*; *m*.
13. 5.	*d*; *l*: maior, quàm *d*; *m*.
10. 5.	*l*: minor, quàm *m*.
8. 5.	*i*: *l*: maior, quàm *c*; *m*.
p. p.	*a*; *b*,→*e*; *f*: *i*; *d*,→*d*; *l*: *i*; *l*.
p. p.	*c*; *d*,→*g*; *h*: *c*; *d*,→*d*; *m*: *c*; *m*.
	a; *b*,→*e*; *f*: maior, quàm *c*; *d*,→*g*; *h*. Quod &c.

i: ma-

Quare &c.

Fig. 4.

The circulation of ideas on symbolic language

Finally, we provide an example of the transformation of the concepts as a result of this circulation of symbolic language in the work of these three authors.

[14] In fact, the formulae for sums of squares, cubes, and higher powers of integers were crucial to the development of integration in the 17th century. Mengoli arrived at these results independently of Fermat [1891-1922] and Pascal [1954], by using symbolic language to express the summations, which enabled him to achieve a certain level of generalisation. Like Fermat and Pascal, he found a rule in which the value of the sum of the *p*th powers is obtained. However, in addition to stating the rule, Mengoli also proved it and used it to perform these values expressing all calculations in symbolic language. On Mengoli's summations, see Massa, 1997.

According to Serfati (2010, p. 108-109), the symbolic representation of the “given” was a major innovation in the late sixteenth century. In 1591, Viète introduced the letter B for the known, namely the “given”; although fixed; its value can be arbitrarily selected. One may then speak about a quantity being arbitrary but fixed. In 1634, Hérigone clearly realized and explained this advantage.

Let us show Hérigone’s view of the status of the letter “given”:

> Specious algebra is so-named from letters of the alphabet which have no particular meaning, either as discrete quantities, which are numbers, or as continuous quantities, except what one attributes to them. For example, if we attribute a value of 12 to the letter B, the reasoning applied to this letter B, without thinking of the number 12, applies to any other number, such as 15, 20, etc., and thus the letter B will represent these numbers as a kind, not as individuals or particulars. This must also be understood for continuous quantities, whether they be lines, surfaces or any other quantities one wishes. By means of these letters, one can discover universal theorems for both continuous and discrete quantities[15].

Thirty years later, Mengoli’s introduced a new concept: the determinable indeterminate quantities. Mengoli’s idea is that letters could represent not only given numbers or unknown quantities, but variables as well: that is, determinable [but] indeterminate quantities. The summations that he obtains are indeterminate numbers, but they are determinate when we know the value of t. Mengoli claims:

> When I write O.a.,...you have the summation [massa] of all the abscissae: but what value this summation is you do not yet know if I do not write what number the summation is. But if I assign O.a. to the summation of the number t, you do not know either how much it is if at the same time I do not assign the value of the letter t[16].

By assigning different values to t, Mengoli explicitly introduces the concept of the “variable”, a notion that was quite new at the time. To clarify this idea, Mengoli adds:

> But when I allow you to fix a value for the letter t, and you, using this license, say that t is equal to 5, immediately you will accurately assign O.a. equal to 10, t^2 equal to 25, t^3 equal to 125, and O.r. equal to 10, and if the letters t are determinate, the quantities O.a., O.r. , t^2 , t^3 , will be determinate. Thus, before you have used the license given, you actually had O.a., O.r. , t^2 , t^3 ,[which are] determinable [but] indeterminate quantities[17].

It should be pointed out that Mengoli uses “specious” language both as a means of expression and as an analytic tool. Therefore, Mengoli applies his idea of “variable” to calculate the "quasi ratios" (nowadays, the limit) of these summations. The ratio between summations is also indeterminate, but is determinable by increasing the value of t. From this idea of quasi ratio, he constructs the theory of “quasi proportions” taking the Euclidean theory of proportions as a model, which enabled him to calculate the value of the limits of these summations. This theory constitutes an essential episode in the use of the infinite and would prove to be a very successful tool in the study of Mengoli’s quadratures and logarithms.

Some remarks

Algebrization is a process that took place slowly and in a very different way in every country. We have presented an example on the perception for the letter representing the known by these authors, which in Mengoli’s hands allows him to introduce the idea of variable and the concept of limit. This provides us with a valuable example of the evolution of the understanding of symbolic language through this circulation in the seventeenth-century.

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[15] “L’Algèbre Spécieuse se nomme ainsi des lettres de l’alphabet, qui n’ont aucune signification particulier, ny en la quantité discrète, qui soit les nombres, ny en la continue, sinon celle qu’on leur attribue. Par exemple, si on attribue à la lettre B12 pour sa valeur, le raisonnement qu’on fera avec icelle lettre B, sans considérer le nombre 12, conviendra aussi à tout autre nombre comme à 15, 20, etc & par ainsi la lettre B signifiera l’espèce des nombres & non les individus & particuliers. Ce qu’il faut aussi entendre en la quantité continue, pouvant signifier une ligne, une superficie, ou autre quantité telle qu’on voudra, par le moyen des quelles lettres on invente des théorèmes universels tant en la quantité continue que discrète.” [Hérigone, 1642, VI, 76]

[16] Cum scripsero *O.a.* habes massam ex omnibus abscissi: sed quota sic haec massa, nondum habes, nisi scripsero cuius numeri sit massa. Quod si assignavero *O.a.*, numeri *t* massam esse; neque sic habes, quota sit, nisi simul assignavero, quotus est numerus, valor litterae *t* …[Mengoli, 1659, 61].

[17] Cum verò licentiam dedero, ut quotum quemque litterae *t* valorem taxes; tuque huiusmodi usus licentia dixeris, *t* valere quinario : statim profecto assignabis & *O.a.*, valere 10 ; & *t*2, valere 25 ;& *t*3, valere 125 ;& *O.r.*, valere 10 ;& determinatae litterae *t*, determinatas esse quantitates *O.a., O.r.,t*2, *t*3. Quare data licentia antequam usus fueris, habebas profecto *O.a., O.r., t*2, *t*3, quantitates indeterminatas determinabiles [Mengoli, 1659, 61].

As regards the circulation of these ideas, it is not only important if one author or another uses the same symbols to represent quantities (powers); rather the significance of these symbols is more important for the process of reasoning in the demonstrations or for the resolution of the problems.

From the reception of Viète's statements and rhetorical demonstrations, Hérigone introduces a method consisting of a new symbolic notation and a new axiomatic-deductive reasoning for improving the understanding of Viète`s rhetorical demonstrations.

The symbolic language is expressed in short lines and can be identified by syllogisms. Moreover, in Hérigone's view is clearer and more concise than the rhetorical demonstrations in Viète's works. Hérigone wanted to diffuse Viète's analytic art introduced this new method for understanding better the results in the mathematical demonstrations. Therefore, the justification of the use of this method, which involves the symbolic language, lies on a didactical aim.

Later, Mengoli, who read (reception) Hérigone's *Cursus*, took (appropriate) his idea of a triangular table related to finding proportional numbers in Euclid's Elements, and developed this idea for constructing an original theory of quasi proportions.

Mengoli also absorbed (appropriated) Hérigone's ideas in his Geometriae, and presented his demonstrations using Hérigone's procedure, thereby enabling him to find new results.

Finally, this new method of demonstration using a universal language and logical sentences through an axiomatic-deductive reasoning is absolutely original and offers us the logical and clear structure of Hérigone's and Mengoli's thinking.

References

FERMAT, Pierre de (1891-1922), *Oeuvres*. Tannery, P. & Charles, H. (ed.), 4 vols. & spp. Gauthier-Villars, Paris, 65-71.

HÉRIGONE, Pierre (1634/1637/1642), *Cursus Mathematicus nova, brevi et clara methodo demonstratus, Per NOTAS reales & universales, citra usum cuiuscumque idiomatis, intellectu, faciles*. For the author and Henry Le Gras, Paris.

MASSA-ESTEVE, Mª Rosa (1997), "Mengoli on "Quasi Proportions"", *Historia Mathematica*, 24, 2, 257-280.

MASSA-ESTEVE, Mª Rosa (2006a), "Algebra and Geometry in Pietro Mengoli (1625-1686)", *Historia Mathematica*, 33, 82-112.

MASSA-ESTEVE, Mª Rosa (2006b), *L'Algebrització de les Matemàtiques. Pietro Mengoli (1625-1686)*. Institut d'Estudis Catalans, Barcelona.

MASSA-ESTEVE, Mª Rosa (2008), "Symbolic language in early modern mathematics: The Algebra of Pierre Hérigone (1580-1643)", *Historia Mathematica*, 35, 285-301.

MASSA-ESTEVE, Mª Rosa – DELSHAMS, Amadeu (2009a), "Euler's Beta integral in Pietro Mengoli's Works", *Archive for History of Exact Sciences*, 63, 325-356.

MASSA-ESTEVE, Mª Rosa (2010), "The symbolic treatment of Euclid's *Elements* in Hérigone's *Cursus mathematicus* (1634, 1637, 1642)", in Heeffer A. & Van Dyck, M. (eds) *Philosophical Aspects of Symbolical Reasoning in Early Modern Mathematics,* Vol. 26, College Publications, London.

MENGOLI, Pietro (1659), *Geometriae Speciosae Elementa,* Bologna.

PASCAL, Blaise (1954), *Oeuvres complètes*. Gallimard, Paris, 166-171.

SERFATI, Michel (2010), "Symbolic revolution, scientific revolution: mathematical and philosophical aspects", in Heeffer A. & Van Dyck, M. (eds) *Philosophical Aspects of Symbolical Reasoning in Early Modern Mathematics,* Vol. 26, College Publications, London.

VIÈTE, François (1970), *Opera Mathematica*, edited by F. A. Schooten. Georg Olms Verlag, Hildesheim- New York.

VIÈTE, François, (1983), *The Analytic Art*, T.R. Witmer (tr.). Kent State University Press, Kent, Ohio.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 415.

MATHEMATICS AND INSTRUMENTALIZATION AS "LINGUISTIC" TOOLS FOR THE WIDESPREAD CIRCULATION OF SCIENCE AND TECHNOLOGY

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Abstract

It is often said that mathematics is a language, a position reflected by sentences like "the sense of a physical expression in the common language", or when we hear that this or that concept can give rise to a mathematical "translation"

The concept of mathematics as a language of the sciences is also part of an epistemological debate concerning disciplinary claims for the definition of a specific but nonetheless scientific disciplinary culture. Since Newton's Mechanics initiated the mathematization of physics, later extended to other natural sciences, the common vision of science is more or less dominated by the idea that physics constitutes the paragon of science and that, as a result, a science can reach a threshold of scientificness only when a certain degree of mathematization is achieved. These claims very often depend on the benefits or losses induced by the process of mathematization. One argument against mathematization is that it utilizes a physicalist ontology and that this ontology leads one to ignore specificity (of biological, human, linguistic phenomena, etc.) thus offering no more than an abusive reductionism. To the latter claim may be opposed the idea that mathematics is a language and that the difference between qualitative and quantitative properties therefore lies not in the properties inherent in the objects but in the property of the language. I will address this question not at the ontological level but rather by showing how "mathematization" can be viewed by comparison to natural language as the elaboration of a standardized language (from a linguistic point of view) which has favored the circulation of science and technology on a large scale. This, in turn, will lead me to some observations about the meaning of mathematization and about what kind of elaboration of a common language may be needed in order to favor an equally widespread dissemination of social sciences.

Despite episodic disciplinary claim about the relevance of theoretical linguistics to science (see for example, Nowak and al.(2002)), theoretical linguistics remains a rather esoteric subject, which few people had even heard of and even fewer would recognize as a branch of science, I assume that it will not be for you a completely new subject, so I will limit myself to those aspects of theoretical linguistics which is of interest for the purpose of this communication. I would like first to stress that linguistic properties are not identical to logical properties, a confusion which is too often a cause for major bias, in most studies involving theoretical linguistics, especially since the raise of computer sciences.

So let's say that all languages involve a process of semiosis to relate signs to a particular meaning (Saussure (1912), Peirce (1931-58)) but that mathematics, as well as other formal languages, can be used without semantics (the relation between the signs and the things they refer to), so it can be used in a variety of fields with totally different meanings which is not the case with natural language, but retaining syntactics (the relation among the signs in the sentence).

In my opinion it is this particular aspect which establishes a complete separation between the hard sciences, that is to say the ones that use mathematics, and the soft ones, which do not, especially when the use of a formal language to describe a particular process can be in and of itself a process of discovery an aspect epitomized by the so called "linguistic" turn in natural sciences, that is, science applies now to facts that can no longer be separated from their narrative, i.e. they are only known through their mathematical description.

Turning back to the meaning of mathematization in science, if we assume that science is a collective process whose aim is to understand the functioning of particular objects on the basis of objectively verifiable observation to achieve specific goals, we can identify two fundamental aspects of the production of knowledge, first the process of acquiring reliable information, second the process of transmitting information, both having to deal with the intrinsic limitations (in that respect) of the human perception system.

This is a very rudimentary statement, which is largely in debt with the Shannon (1948) and Shannon and Weaver (1949) model of a point to point communication system, obviously doesn't cover all the aspects of scientific activity but is nevertheless an illustration of the kind of fundamental questions one may ask about science.

Let's consider the first point, acquiring reliable information. I shall now introduce a distinction between seeing and observing: Observing reduces vision to something that takes place at one stationary point when the entire process of vision involves two movable eyes and a brain (Andersen, 2006). I shall not discuss here in depth the problems posed by perception. I will simply summarize the problem by saying that the perception system allows individuals to assimilate information from the environment but we do not see the world directly, but rather by means of the treatment of stimuli coming from the retina, that one can roughly describe as a membrane sensitive to light. Besides the eyes, which one can define as a biological sensor, vision also implements a complex cognitive process whose nature remains at stake in current research, taken over by specialized zones of the brain allowing the rough perception of the stimuli to give rise to objects whose status is still disputed. Furthermore we can derive different information, every time in different way, from the same stimuli, which leads to different narrative, especially when human factors or social factors are involved in the process of acquiring information.

In that respect, one could see the history of science as a process to control the intrinsic holistic, egocentric and variable character of human perception. For example, one may say that viewed from the point of view of Astronomy, Greek optics, and probably all astronomical optics, may be considered as an attempt to assure that distant natural phenomena are the same, regardless the viewing angle. It's worth mentioning that classical Greek optics, like classical Greek geometry was chiefly concerned with angles; both disciplines are condensed in Euclid's notion of a visual cone. Semiotically speaking, this means that the distance is a sign of (and for) a certain kind of relationship. The instrumental verification will consist in assuring that the measured length has not been altered by the viewing angle, and/or the materiality of the instrument, and the observer himself. (Salvemini, 1990).

With this example, we can define a scientific information as a collective artifact (Latour, 1983), produced through the implementation of a formal language related to formal procedures, in order to identify and observe specific objects.

Now we have to answer the basic question of what kind of system of communication is needed in order to convey that particular type of information. Turning back to the theory of information, if we consider the following scenario, a scientist needs to communicate an information to another one, and he wants to convey as much information as possible with minimum errors, the first problem will be encoding the information, the second transmitting the information so that the information is received correctly, and the third assuring that the encoding system guarantees a minimum of error for the decoding. Information theory is only concerned by encoding and decoding process as means to ensure that a message generated by a source can be correctly delivered to a receiver depending of the materiality of communication system, without worrying about the information itself, and how this information will be received by the receiver. (Yeung, 2002: 2). It is nevertheless a good starting point to understand how the holistic character of human perception, tied to the ambiguity of natural language, led to the standardization of procedures of identification of objects, and of description of objects. With time, the consensus about the object (its identification and description) generated a certain, shall we say "routinisation" of the procedures and terminology, which became increasingly detailed. They reveal in the form of mini sequences of events, the principles (clear, concise, etc.) and rules (instructions on the order of the elements, terminology and syntax, which state unequivocally how to state this information) in order to be able to communicate the concepts clearly.

This standardization allowed increasingly larger procedures of abstraction, allowing the transfer and the acculturation of these procedures worldwide.

* * *

I shall now make an attempt to show that "mathematization" can be viewed by comparison to natural language as the elaboration of a standardized language (from a linguistic point of view) which has favored the circulation of science and technology on a large scale.

Let us define a relation between two groups, A and B, which together speak of the referent C.

The two groups speak about the same referent but don't share the same language, so they can't communicate. They even may not know that they speak about the same referent. If the language of group A is in fact an idiolect of its own categories, it cannot be understood by group B, which has its own idiolect. On the other hand, if group A and group B have in common something of truly abstract nature, such as quantities, for example, they can communicate. But doing so, they introduce a new term into the tripartite relation which is the product D of the communication between A and B on C.

I can't discuss here in length the relationship between this very rudimentary analysis of the use of mathematics and the major theories of the semiotic field to which I'm obviously in debt (Frege, 1892a, 1892b, Odgen and Richards, 1923; Peirce, 1931-58; Morris, 1938, 1941), I will rather go further taking a more concrete example:

A group A is able to represent two key premises of the concept of exact numbers: the relation of exact equality and the successor function. A group A can represent the cardinal of large sets in an approximate way, with analog magnitude representations and a group B is able to track individuals in small sets of objects (parallel individuation system), and the two groups are able to solve some numerical problems. (Izard, 2010)

However, neither of these systems are powerful enough to deal with large exact numerical quantities (i.e. integers), but if the two groups are allowed to communicate, they can from those two systems of equivalent properties acquired separately and inferred from different sources of evidence build a more general theory of number.

This is more likely to happen when a very sophisticated language such as mathematics is used for communication.

Because of the particular property of mathematics, (ignoring semantics, while retaining syntactics), makes that the product D of the communication between A and B on C is not a passive product of the relation A B C; but an active product, which modifies the relation while introducing a sort of fourth dimension of the relation. This fourth dimension is therefore a model of reality that may be tested.

The use of mathematics for communication between scientists leads to the unity of the so-called *hard* because of this specific property of mathematics as a language that is ignoring semantics (the relation between the signs and the things they refer to) while retaining syntactics (the relation among the signs in the sentence). This is due to the fact that when a problem is formulated in a way that allows associating a mathematical structure to the narrative specific of the field, the syntactics remains the same, allowing a huge circulation of concepts between very distant fields, while giving an illusion of naturality, and universality.

The role of mathematics as the language of science is now challenged by the raise of computer sciences. The algorithmization of other disciplines is also a factor of the diffusion of explanatory models of the computer sciences into those disciplines (Tedre, 2007; Easton, 2006).

The relationship between formal language and science, and mathematics and science, especially when applied to soft sciences, is also renovated by the extraordinary development of algorithmic techniques associated, which allow the transfer of human competences to machines, while requiring new formalism, borrowed in part from quantum physics.

The relationship between formal language and science, and mathematics and science as well as the more philosophical debate of "instrumental" realism, (Hacking, 1983) since the extraordinary development of algorithmic techniques has allowed the transfer of a growing number of human abilities to machines, while requiring new formalism, borrowed in part from quantum physics. This process stresses a new set of questions related to identification, formalization, but also to transfer, because the functioning of machines is not similar to the functioning of the human brain.

We observe the reduction of the cognitive functions of the human brain to algorithmic processes. However dubious this reduction can be, and the opacity beyond the computational processes (Humphreys, 2009) that produce the artifact of a moving machine in the real world, the transfer of cognitive functions previously not directly observable to machines acting in the real world is an intriguing case of instrumental realism.

References

ANDERSEN, Kirsti (2006). *The Geometry of an Art. The History of the Mathematical Theory of Perspective from Alberti to Monge*. New-York: Springer.

EASTON, Thomas A (2006). Beyond the Algorithmization of the Sciences. *Communications of the ACM* 49(5): pp. 31-33.
http://doi.acm.org/10.1145/1125944.1125967

HACKING, Ian (1983). *Representing and intervening*. Cambridge: Cambridge University Press.

HUMPHREYS, Paul (2009): "The Philosophical Novelty of Computer Simulation". *Synthèse* 169: 615-629.

IZARD Véronique (2010): "Two key premises of the concept exact numbers: Exact equality and successor function", paper presented at *The Number Concept: Axiomatization, Cognition and Genesis*, Wednesday-Friday 3-5 November 2010, Nancy, France.
http://poincare.univ-nancy2.fr/Activites/?contentId=7073

LATOUR Bruno (1986 [1979]). *Laboratory Life: The Social Construction of Scientific Facts*. Princeton: Princeton University Press.

NOWAK Martin A., KOMAROVA Natalia L., & PARTHA Niyogi (2002). Computational and evolutionary aspects of language. *Nature* (417): pp. 611-617.

MORRIS, Charles W. (1938). "Foundations of the Theory of Signs", *International Encyclopedia of Unified Science*, vol. I, n° 2, Chicago, 1938, pp. 77-138.

MORRIS, Charles W. (1946). *Signs, Language and Behavior*. New-York: Prentice Hall.

OGDEN Charles and Ivor RICHARDS. (1923). *The Meaning of Meaning*. Londres: Paul Kegan.

DE SAUSSURE Ferdinand (1972 [1916]) *Cours de linguistique générale*, 1916, Paris: Payot.

SALVEMINI, Francesca (1990). *La visione e il duo doppio. La prospettiva tra arte e scienza*. Bari: Laterza.

SHANNON, Claude E (1948). "A Mathematical Theory of Communication", Part I, *Bell Systems Technical Journal* 27, pp. 379-423.

SHANNON, Claude E. & Warren Weaver (1949). *A Mathematical Model of Communication*. Urbana, IL: University of Illinois Press.

SANDERS Peirce Charles (1931-58): *Collected Writings* (8 Vols.). (Ed. Charles Hartshorne, Paul Weiss & Arthur W Burks). Cambridge, MA: Harvard University Press.

TÈDRE, Matti (2007): *Lecture Notes in the Philosophy of Computer Science*.
http://cs.joensuu.fi/~mmeri/teaching/2007/philcs/

YEUNG, Raymond W: *A First Course in Information Theory*, Springer, New-York, 2002.

THE DEVELOPMENT OF NEW SCIENTIFIC IDEAS IN PORTUGAL AND OTHER PERIPHERAL COUNTRIES: SCIENTISTS, LABORATORIES, INSTRUMENTS AND TEXTS IN THE NINETEENTH AND TWENTIETH CENTURIES

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 420.

THE ROLE OF THEODOR GERDORF, FRIEDRICH KRANTZ AND ÉMILE DEYROLLE IN THE COLLECTIONS OF MINING, METALLURGY, MINERALOGY AND PALEONTOLOGY FROM THE SCHOOL OF ENGINEERING (ISEP) OF THE PORTO POLYTECHNIC, PORTUGAL

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Abstract

After a period of political turbulence that marked the first half of the nineteenth century in Portugal, began in 1851 a new stage of constitutional monarchy. With the first Regeneration government was created a new ministry for infrastructural and industrial development, the so-called Ministério das Obras Públicas Comércio e Indústria*, under leadership of António Maria de Fontes Pereira de Melo (1819-1887). The purposes of this effort were a reduction of the socio-economic delay of Portugal compared to other West European countries, by modernizing the administration to achieve long-term economic and social development. That resulted in a significant increase of railways and roads, together with the construction of the first telegraph lines, and the establishment of an industrial education system in 1852. Industrial education had a strong practical emphasis in its curriculum courses highlighted by numerous cabinets and laboratories, at the time known as* auxiliary offices of education*. During several years were purchased scientific instruments to supply these cabinets and experimental laboratories. The equipments exhibited in the Museum of ISEP (School of Engineering of the Porto Polytechnic) belonged to the ancient Cabinet of Mineralogy and Cabinet of Mine Art and Metallurgy, and had been made by the European manufacturers Friedrich Krantz, Theodor Gerdorf, and Émile Deyrolle. Currently, they still are at the Krantz House (probably one of the oldest and largest rock, mineral and fossil traders) and*

the Émile Deyrolle House (related specially to Paleontology). At that time these were considered the benchmarks with regard to the educational material, available on almost all prestigious scientific institutions in Europe. The international and universal exhibitions were also an excellent way of spreading the scientific and technological advances. In short, the acquisition of this type of collection indicates the degree of scientific knowledge at the time, which allowed a country like Portugal to develop the industrial education and experimental training for graduates.

Introduction and objectives

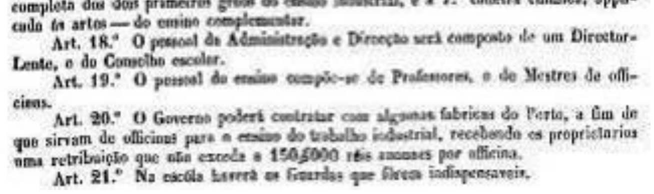
This work aims to understand foreign influences in industrial education and dissemination of science in the north of Portugal, more specifically the Industrial School of Porto in the second half of the 19th century.

In the mid-nineteenth century Portugal had only two conservatories in arts and crafts (Lisbon and Porto), without an official structure regulating industrial education. The main purpose of these conservatories was the practical instruction of all industrial processes in course on the emerging factories of the country. For this objective they had been, widely used as general storage of machinery, models, tools, drawings, descriptions and books on various arts and crafts.

After a long period of political turbulence during the first half of the 19th century in Portugal, a new stage of the constitutional monarchy began in 1851. With the first Regeneration government, a new ministry of infrastructural and industrial development was created, the so-called *Ministério das Obras Públicas, Comércio e Indústria*, under leadership of António Maria de Fontes Pereira de Melo (1819-1887). This ministry played an important role as facilitator of the national economy. This period became known as *Fontismo*; its action was marked by the intention of building bridges, roads and railways in an effort to modernize Portugal. The main idea was a significant reduction of the socio-economic delay of Portugal when compared with other European countries, by modernizing the administration to achieve long-term economic and social development. That resulted in a significant increase of railways and roads, together with the construction of the first telegraph lines and the establishment of an industrial education system in 1852.

It is in this context that arises the need to train people who could respond to the new challenges and needs of industry, where new inventions and means of production were being deployed with an increasing rhythm. As it was stated above, the industrial education was in the hands of private associations, as the case of Porto Industrial School[1] for example, which was the responsibility of the Industrial Association of Porto. In October 1852 the Industrial Association of Porto created its own industrial school, advancing to the government itself. In their statutes the need was clearly stated to instruct and educate the working classes. The major purpose would be to educate and train the working classes, teaching them to appreciate the work and providing knowledge to improve the industry.[2]

After an important decision of the government, two industrial schools were created on the main towns of Porto and Lisbon, with the aim of forming the new working class that was now emerging in our country (Decree Law December 30, 1852).


TITULO III.

Da Escola Industrial do Porto.

Art. 17.º É creado no Porto uma Escola Industrial, que comprehende a instrucção completa dos dois primeiros grãos do ensino industrial, e a 7.ª cadeira chimica, applicada ás artes — do ensino complementar.

Art. 18.º O pessoal da Administração e Direcção será composto de um Director-Lente, e do Conselho escolar.

Art. 19.º O pessoal do ensino compõe-se de Professores, e de Mestres de officinas.

Art. 20.º O Governo poderá contratar com algumas fabricas do Porto, a fim de que sirvam de officinas para o ensino do trabalho industrial, recebendo os proprietarios uma retribuição que não exceda a 150$000 réis annuaes por officina.

Art. 21.º Na escóla haverá os Guardas que forem indispensaveis.

Fig. 1: Extract of the Decree Law December 30, 1852, establishing an industrial education system in Portugal, with the creation of an Industrial School in Porto.

[1] This school had been planned for some time ago but it was only in 1852 that was achieved the law that regulated its creation and working. This law proposed to achieve a wide range of practical areas, revealing a set of activities necessary for our country.

[2] *Journal of Industrial Association of Porto*, Tome I, Porto, 1853, p. 3.

The seventeen article of the law can be translated as following:

> *"It is created in Porto an industrial school, which includes the complete instruction of the first two degrees of industrial education, and the 7th discipline –chemistry, applied to the arts– from supplementary education…"*

The government had the clear intention to adopt this effective means of teaching and, in this way, to contribute to national development. We emphasize the fact that industrial education is completely free, allowing access to education for all social classes.

Like other countries had already done, emphasis was placed on developing the national industry. To achieve this objective subsequent years were spent with significant efforts in order to introduce more new inventions, machinery and also to form their workers, teachers and leaders. Most of these people were illiterate, as most of the national population, although the official primary school existed since 1772. Thus, in 1850 it did not exist in Portugal a subsystem of industrial education. This kind of education had a strong practical emphasis in its curriculum courses, highlighted by numerous cabinets and laboratories, at the time known as auxiliary offices of education. The purchase of equipment for the classes began as early as 1853 with the mission of the government commissioner José Mauricio Vieira[3] to the city of Paris, in order to acquire certain machinery and equipment of physical chemistry. Every year were purchased diverse materials with the purpose of providing the laboratories and cabinets of the latest technology.

The industrial school and the practical education

Fig. 2: The building which housed both the Industrial School and the Polytechnic Academy, Porto.[4]

The Industrial School of Porto shared a building with the Polytechnic Academy of Porto, as well as some other spaces. We emphasize that the space that occupied the school was long considered inadequate given that the collection of

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[3] José Mauricio Vieira was the physical preparer at the Polytechnic School of Lisbon.

[4] *Annuario da Academia Pilytecnica do Porto, Anno lectivo de 1882-1883 (sexto anno)*, Porto, Tipographia central, 1883.

practical education increased considerably every year. The installation of cabinets, laboratories and the availability of study rooms had become indispensable to the success of this type of education. Next figures confirm this situation and also show the existence of a large amount of educational material in the Chemical Laboratory and in the Physics Cabinet of the Industrial Institute of Porto.

Figs. 3 and 4: The Physics Cabinet and the Chemistry Laboratory of the Industrial Institute of Porto, in the building of the Polytechnic Academy of Porto,19th century.

Among the several specialized laboratories and cabinets of that time, we highlight now the collections of Mineralogy, Geology and Palaeontology that belonged to the ancient Cabinet of Mineralogy, later called Cabinet of Mine Art and Metallurgy. These collections are presently housed and exhibited in the Museum of ISEP (School of Engineering of The Porto Polytechnic). The main purpose of this cabinet was to form a considerable collection of minerals, ores, rocks and fossils, available to students and teachers and adequate to illustrate the overall geology and mining of the country. Since 1883, however, attention was also given to the need to incorporate didactic models in the collection of mines and metallurgy (figures 5 and 6). Nevertheless, this area only integrated with the curriculum reform of industrial education in 1864, after the establishment of Geological Surveys in Portugal. In this date was established the 7th discipline –Art of mines. This cabinet was never officially established by any decree, although it was referenced in correspondence since 1867.

Most of the collection models and specimens were purchased from European manufactures and *comptoirs* of international reference. Among them we can already highlight the excellent examples of J. Schröder[5] from Darmstad, Germany and J. Digeon[6] from Paris, France (figures 7 and 8). However, the main provider of the models of the metallurgy collection was Theodor Gersdorf.

Theodore Gersdorf (Freiberg, Saxony) was a notable constructor of models of mines and metallurgy. He constructed several manufactured examples of high quality between the years of 1880 and 1890 for the Academy of Freiberg[7] –one of the most reputable and oldest universities in Germany. He provided to the school of mines, metallurgy and mineralogy educational models specially designed for the experimental teaching of engineering. Moreover, Theodore Gersdorf provided numerous models for other schools of geology, mining and metallurgy of several European countries and the United States of America.

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[5] *Budget of the material for the various establishments*, by the director Gustavo Adolfo Gonçalves e Sousa, 3 June 1887. (Document belonging to the Historical Archive of the School of Engineering of Porto|ISEP).

[6] André, L. (2006). Jules Digeon, l´âge d´or du modèle réduit. *La Revue*, 45. http://www.culture-technique.net/musee.php?P=157&id=10913&lang=fra&flash=f, accessed on September 15, 2009.

[7] The Academy of Mines of Freiberg was established in 1765, being one of the oldest universities in Europe and a source of knowledge and important development in the mining area.

Figs. 5 and 6: Some models belonging to the collection of mining and metallurgy of constructor Theodor Gersdorf, Germany.

Figs. 7 and 8: The manufacturers' label placed on the instruments.

In 1886, for installation of the Portuguese cabinet attached to the discipline of mining Art and metallurgy (16th discipline), a variety of new materials was commissioned. The theacher Manoel Rodrigues Miranda Junior [8], in the ordering letter of the models, wrote an interesting note which expressly refers the provider (figure 9).

This order demonstrates a perfect knowledge of what was done to the best standard of teaching material to enable the students of the school the contact with the latest technology.

At the same time and regarding the collection of Mineralogy, the largest supplier of the school cabinet was the *comptoir* "Les Fils d´Émile Deyrolle" (figure 10). It is also the first reference found in the documentation. There are numerous examples of this collection remaining in the Museum of ISEP.

Émile Deyrolle was a 19th century dealer from Paris that mainly worked with Natural History specimens and taxidermy (figure 11). For many years the *comptoir* Deyrolle specialized in Natural History publications, specimens (minerals, rocks, fossils, shells) and instruments. The fourth generation of the family also changed the company name to "Les Fils d´Émile Deyrolle" on the labels. They had taken over the business by 1896.

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[8] *Anuário da Academia Politécnica, ano lectivo 1889/1890*. Typografia Occidental, Porto, 1890, p. 9.

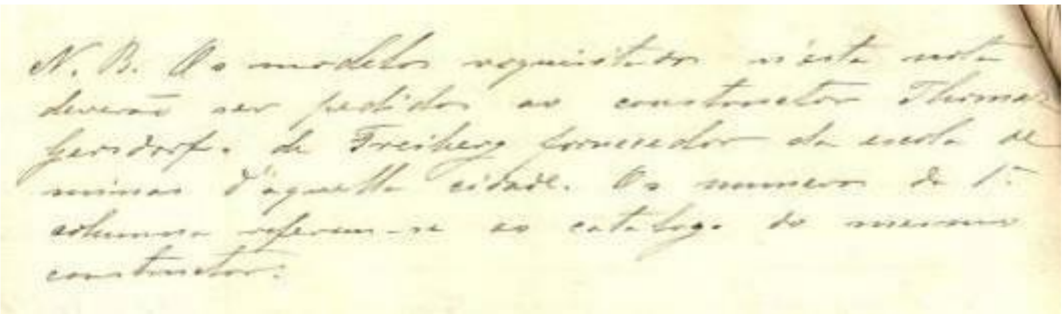

N. B. Os modelos requisitados n'esta nota deverão ser pedidos ao constructor Thomas Gersdorf. de Freiberg fornecedor da escola de minas d'aquella cidade. Os numeros da 1ª columna referem-se ao catalogo do mesmo constructor.

Fig. 9: Note of the teacher Manoel Rodrigues Miranda Junior on Theodore Gersdorf:
"The models required in this note should be ordered to the manufacturer
Theodor Gersdorf of Freiberg, provider of the School of Mines in that city…"

Fig. 10: Label of Theodor Gersdorf, Freiberg.

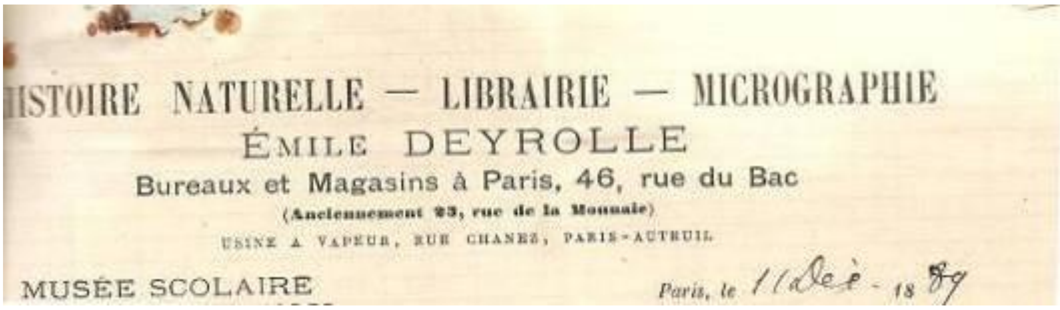
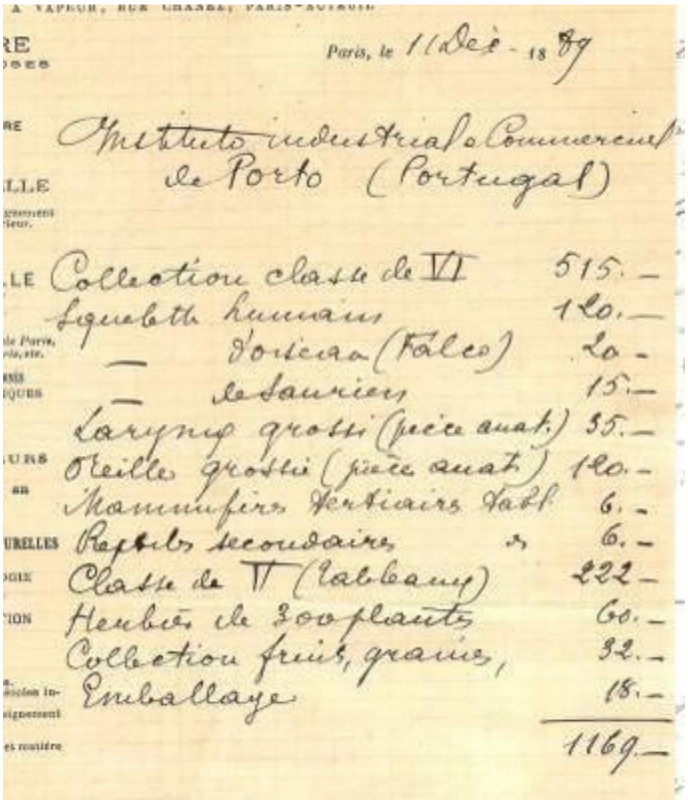

ISTOIRE NATURELLE — LIBRAIRIE — MICROGRAPHIE
ÉMILE DEYROLLE
Bureaux et Magasins à Paris, 46, rue du Bac
(Anciennement 23, rue de la Monnaie)
USINE A VAPEUR, RUE CHANEZ, PARIS-AUTEUIL
MUSÉE SCOLAIRE
Paris, le 11 Déc - 1889

Fig. 11: Head of the letter sent to the Director of the Industrial School from Émile Deyrolle, 1889.

The first reference to collections purchased from the *comptoir* of Émile Deyrolle has the date of 1886 (figure 12). As usual on the procedures of the school these specimens were destined to be used as a support for the practical teaching of Zoology and Botany. As shown in this document, a receipt sent by Deyrolle (figure 13), the list of collections acquired in 1889 was very extensive and suggests significant advancements on the practical teaching.

Fig. 12: Some specimens of the Émile Deyrolle collection.



Paris, le 11 Déc. 1887

Instituto industrial e Commercial de Porto (Portugal)

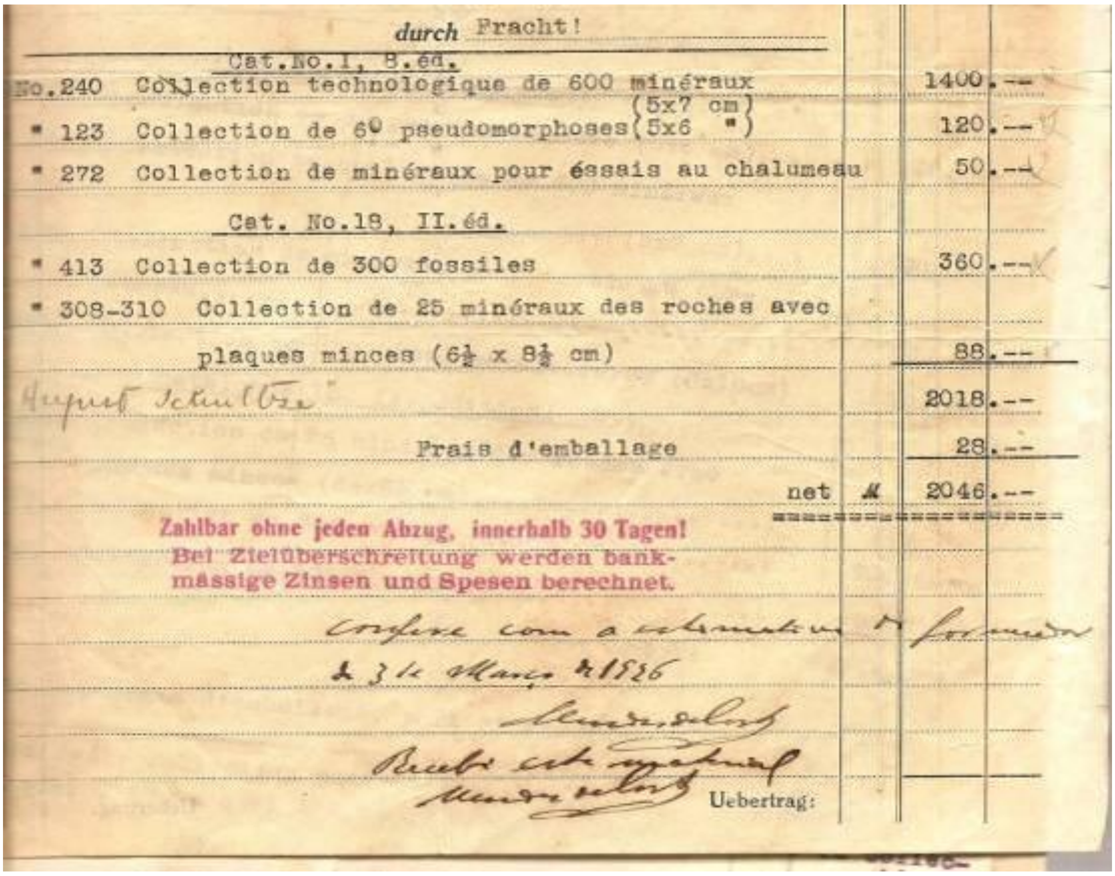
Collection classe de VI	515.–
Squelette humain	120.–
— d'oiseau (Falco)	20.–
— de Saurien	15.–
Larynx grossi (pièce anat.)	35.–
Oreille grossie (pièce anat.)	120.–
Mammifères tertiaires tabl.	6.–
Reptiles secondaires "	6.–
Classe de V (tableaux)	222.–
Herbier de 300 plantes	60.–
Collection fruits, graines,	32.–
Emballage	18.–
	1169.–

Fig. 13: List of Deyrolle collections of the specimens acquired for practical teaching.

Together with the previous *comptoir*, Friedrich Krantz was the supplier of much of the mineralogical collection of the Cabinet of Mineralogy, as if can be presently seen from the specimens housed in the Museum of ISEP. The company's founder was Dr. August Krantz (1809-1872). He developed extensive connections with main scientists and collectors, eventually becoming one of the most relevant mineral dealers in Europe. In 1850, he opened in Bonn one of the first worldwide *comptoirs* specialized in Geology and Paleontology.m Friedrich Ludwing Robert Krantz (1859-1926), August´s nephew, took over the company management in 1891; it was at this time that the *comptoir* changed its name. The label F. Krantz has been used since 1888 until today (figure 14).

The request to purchase mineral collections to the *comptoir* F. Krantz was made by a Mining professor, Artur Mendes da Costa, with the aim of equipping the cabinet, but also claiming that those samples were indispensable for teaching. According to that teacher, education had to be done in the presence of mining models and specimens that reproduced the main deposits. Most collections were purchased during the 20's of last century (figure 15).

Fig. 14: A specimen and labels from F. Krantz, Bonn.



durch Fracht!

	Cat. No. I, 8. éd.	
No. 240	Collection technologique de 600 minéraux (5x7 cm)	1400.--
" 123	Collection de 60 pseudomorphoses (5x6 ")	120.--
" 272	Collection de minéraux pour éssais au chalumeau	50.--
	Cat. No. 18, II. éd.	
" 413	Collection de 300 fossiles	360.--
" 308-310	Collection de 25 minéraux des roches avec plaques minces (6½ x 8½ cm)	88.--
		2018.--
	Frais d'emballage	28.--
	net M	2046.--

Zahlbar ohne jeden Abzug, innerhalb 30 Tagen!
Bei Zielüberschreitung werden bankmässige Zinsen und Spesen berechnet.

Uebertrag:

Fig. 15: Some examples of collections acquired in 1926 for the cabinet of practical teaching: collections of minerals, fossils and pseudo morphs.

At that time this kind of collections was considered the benchmark with regard to the educational material available on almost all prestigious scientific institutions in Europe. As we can see from contemporaneous documentation, teachers had better knowledge of what was produced in educational material, indicating in their orders the exact reference to the suppliers. The international and universal exhibitions were also an excellent way of spreading scientific and technological advances. Although performed with an economic purpose, they triggered a vigorous animation in various professions.

The emergence of universal exhibitions brought a major boost in the acquisition of new resources used in industrialized countries, in industry, commerce and even agriculture. It was a unique occasion for countries to show to the world the best they did in certain areas, which also enabled the countries for the latest technology, to equip themselves for technology that would allow the development of their economies.

The economic elite was aware of the reality of our country, expressing the wish of introducing new technologies in manufacturing processes and cultivation of land. The Industrial School of Porto was often invited to participate in some of these events, showing work performed by students. The exhibitions where this Institute participated, visited, or were invited to participate spam from 1855 to 1900 (table 1).

Table 1: Exhibitions where the Institute participated, visited, or were invited to participate.

Exhibition	Date
Paris	1855
London	1862
Vienna	1873
Philadelphia	1876
Madrid	1883
Paris	1900

During these exhibitions, teachers were charged to evaluate the best of what was being done, to later apply these lessons in their classrooms, giving students up-to-date knowledge.

Concluding remarks

This study highlights the importance of collections of mines and metallurgy and their suppliers existing at the Museum of the School of Engineering (ISEP) of The Porto Polytechnic. The acquisition of this type of collections is an indicator of the scientific knowledge achieved at that time, which allowed a country like Portugal to develop the industrial education and experimental training for graduates.

The Industrial School of Porto had a strong practical component, as it was demonstrated by the existence of laboratories, cabinets and museum. In this practical component, the acquisition of new equipment, technical and scientific instruments and materials was essential to transmit knowledge and would be of great scientific and pedagogical quality, resulting also in an important component in the training of their students.

Also the international events and the universal exhibitions allowed the release of the latest scientific and technological discoveries, being visited by many teachers of that time.

Another relevant aspect is the regular contacts that the professors had with the most renowned European constructors of instruments and teaching models, showing that they know the best that was being produced in Europe concerning the experimental education materials.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 429.

THE SCIENTIFIC LIFE OF MARIETA DA SILVEIRA

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Abstract

Marieta da Silveira (1917-2004) was born in the one of the Azores islands, and came to Lisbon where she graduated in physics and chemistry in the Faculty of Sciences.
She started research in the field of nuclear science in the Centre for Studies in Physics, where she studied the absorption of the Uranium X radiation. The hypothesis of the existence of natural radioactivity by spontaneous emission of neutrons was one of her results, published in Portugaliae Physica*. She obtained her PhD in 1945 and she continued working in the Centre until 1947, when senior researchers were expelled from the University for political reasons. The same political reasons led to a situation where she, although not expelled, was also segregated. Some years later she started working with geologists, studying radioactive minerals, doing research work where Portuguese uranium minerals, including those originated in the Portuguese colonies, are analysed. This work was more ambitious than the previous articles published in the first half of the century on the same theme –their authors were aware they were doing scientific research.*
Marieta da Siveira's research work was of great quality and some of her publications are mentioned in international documents of history of science (BAYER, R. T., Foundations of Nuclear Physics*, New York, Dover Publications Inc., 1949). Besides these research activities, she was also an outstanding teacher.*

Marieta da Silveira was born in the Azores islands in 1917. She did her secondary schooling in the Azores and, at the age of twenty, she went to Lisbon, where she started her higher education. She finished her degree in Physical and Chemical Sciences in 1941 at the Faculty of Sciences of Lisbon. At this time Marieta enrolled in a teacher training course which she abandoned because she was invited to teach at the Faculty of Sciences. Thus she started a new phase of her life in the Laboratory of Chemistry, later Department of Chemistry.

In those days faculty members were not requested to do research, and there were hardly any conditions to do so in the country. In the Faculty of Sciences there were two research centres: one in the Laboratory of Chemistry and the other in the Laboratory of Physics. Both of them dealt with radioactivity. Marieta, though she taught at the Laboratory of Chemistry, joined the Physics research centre, where she worked under the direction of Manuel Valadares and Marques da Silva, both with PhDs obtained at the Curie Laboratory, in Paris.

In 1938, Marguerite Perey had discovered the presence of the elements At and Fr in the radioactive families of Uranium and Actinium (see tables 1 and 2).

Table 1: The radioactive family of Uranium.

Nuclei	Disintegration mode	Semi disintegration period	Energy released (MeV)	Disintegration produce
U 238	α	$4.468 \cdot 10^{9}$ years	4.270	Th 234
Th 234	β-	24.10 days	0.273	Pa 234
Pa 234	β-	6.70 h	2.197	U 234
U 234	α	245500 years	4.859	Th 230
Th 230	α	75380 years	4.770	Ra 226
Ra 226	α	1602 years	4.871	Rn 222
Rn 222	α	3.8235 days	5.590	Po 218
Po 218	α 99.98 % β- 0.02 %	3.10 min	6.115 0.265	Pb 214 At 218
At 218	α 99.90 % β- 0.10 %	1.5 s	6.874 2.883	Bi 214 Rn 218
Rn 218	α	35 ms	7.263	Po 214
Pb 214	β-	26.8 min	1.024	Bi 214
Bi 214	β- 99.98 % α 0.02 %	19.9 min	3.272 5.617	Po 214 Tl 210
Po 214	α	0.1643 ms	7.883	Pb 210
Tl 210	β-	1.30 min	5.484	Pb 210
Pb 210	β-	22.3 years	0.064	Bi 210
Bi 210	β- 99.99987% α 0.00013%	5.013 days	1.426 5.982	Po 210 Tl 206
Po 210	α	138.376 days	5.407	Pb 206
Tl 206	β-	4.199 min	1.533	Pb 206
Pb 206	-	Stable	-	-

Table 2: The radioactive family of Actinium.

Nuclei	Disintegration mode	Semi disintegration period	Energy released (MeV)	Disintegration produce
Pu 241	*β-*	14.4 years	0.021	*Am* 241
Am 241	*α*	432.7 years	5.638	*Np* 237
Np 237	*α*	$2.14{\cdot}10^{6}$ years	4.959	*Pa* 233
Pa 233	*β-*	27.0 days	0.571	*U* 233
U 233	*α*	$1.592{\cdot}10^{5}$ years	4.909	*Th* 229
Th 229	*α*	$7.54{\cdot}10^{4}$ years	5.168	*Ra* 225
Ra 225	*β-*	14.9 days	0.36	*Ac* 225
Ac 225	*α*	10.0 days	5.935	*Fr* 221
Fr 221	*α*	4.8 m	6.3	*At* 217
At 217	*α*	32 ms	7.0	*Bi* 213
Bi 213	*α*	46.5 m	5.87	*Tl* 209
Tl 209	*β-*	2.2 min	3.99	*Pb* 209
Pb 209	*β-*	3.25 h	0.644	*Bi* 209
Bi 209	*α*	$1.9{\cdot}10^{19}$ years	3.14	*Tl* 205
Tl 205	.	Stable	.	.

Marieta was given the task of searching for these elements in the Thorium family. She worked in this project for one year but had to abandon because the Centre could not afford a strong source of thorium in order to acquire reliable results and be confident in the conclusions. Uranium was more available so she changed her subject and dedicated herself to the study of beta and gamma emissions of the first descendants of uranium.

Due to the financial shortage, Marieta had to build herself the Geiger counters she needed and she acquired an expertise in doing so. This is evident by the fact that in 1946, A. Vigon, a lecturer from a University of Madrid, came to Lisbon to learn with Marieta. She got some interesting results which led to several publications as well as her PhD thesis entitled “Contributions to the study of the radiations of the Uranium X complex”. These publications were cited by R. Bayer in *Foundations of Nuclear Physics*, 1947:

- SILVEIRA, M. (1944) Absorption of gamma rays emitted by U I and its immediate descendants, *Portugaliae Physica*, I, 151.
- SILVEIRA, M. (1945) Natural Radioactivity by neutron emission, *Portugaliae Physica*, I, 167.
- SILVEIRA, M. (1945) On the absorption of the gamma radiation emitted by the UX complex, *Portugaliae Physica*, I, 175.

She did her PhD in 1946. While studying the beta and gamma emissions of the uranium descendants she discovered that there was also emission of neutrons. She attributed this emission to the isotope uranium Z.

In 1947, due to political reasons, the government sacked both Manuel Valadares and Marques da Silva, together with several other college professors.

Marieta continued using the measurement facilities she had helped to build to a different end: she started a collaboration with the Geology professor Torre da Assunção studying radioactive minerals, both from Portugal and from the colonies. She studied their structure using X-ray diffraction and their radiological properties. Some of this work was also published:

- MENDES, F. J.; SILVEIRA, Marieta da & VIEIRA, Glaphyra (1956). Estrutura fina dos Halos Pleocróicos observados em Rochas do Ultramar Português, *Actas do XII Congresso da Associação Portuguesa Para o Progresso das Ciências*,Tomo V, pp. 131-136 Coimbra.
- MENDES, F. J.; SILVEIRA, Marieta da & TORRE DE ASSUNÇÃO, C. F. (1957). O Zircão de Alter Pedroso (Alter do Chão) e o dofilão da Boa Esperança (Ribaué, Moçambique). Estudo Radiográfico. *Bol. M. e L. Min. e Geol*., nº25, 7ª série, pp. 209-219
- TORRE DE ASSUNÇÃO, C.F.; MENDES, F.J. & SILVEIRA, M. (1957). Contribuições para o conhecimento dos minerais de urânio portugueses – I, a malha das pechblendas da Metrópole Portuguesa e as suas possíveis relações com a composição química, *Rev. F.C.* Lisboa, 2ª série, C, vol. V, fasc. II, pp. 261-268.
- TORRE DE ASSUNÇÃO, C.F.; MENDES, F.J. & SILVEIRA, M. (1957). Contribuições para o conhecimento dos minerais de urânio portugueses – II, Sobre a identificação do uranófano no minério negro da Metrópole Portuguesa, *Rev. F.C.* Lisboa, 2ª série, C, vol. V, fasc. II, pp. 269-275.
- TORRE DE ASSUNÇÃO, C.F.; MENDES, F.J. & SILVEIRA, M. (1958). Contribuições para o conhecimento dos minerais de urânio portugueses –III,sobre a identificação metatorbenite, , *Rev. F.C.* Lisboa, 2ª série, C, vol. VI, fasc. I, pp. 51-68.

Marieta also did some work with the biochemistry professor Kurt Jacobsohn:

- JACOBSOHN K.; Silveira M. (1950). Action des neutrons sur l'activité fermentaire, *Comptes rendues des séances de la Societé Portugaise de Biologie*, novembre.
- JACOBSOHN K.; SILVEIRA M. (1950). Étude sur le mécanisme de l'inactivation fermentaire sous l'action des neutrons, *Comptes rendues des séances de la Societé Portugaise de Biologie*, décembre.
- JACOBSOHN K.; SILVEIRA M. (1951). Sur la protection d'un enzyme contre l'action détruisante des neutrons, , *Comptes rendues des séances de la Societé Portugaise de Biologie*, mai.
- JACOBSOHN K.; SILVEIRA M. (1951). Sur le mécanisme le mécanisme de l'inactivation fermentaire sous l'action des neutrons, *Bulletin de la Societé de Chimie Biologique*
- JACOBSOHN K.; SILVEIRA M. (1950). Actividade enzimática e radiações ionizantes, *Revista da Faculdade de Ciências*

After this busy period, and for reasons unknown, she had to interrupt her research work. But she did not stop: she published several notes about everyday chemistry and she translated a Russian scientific dictionary. In the late fifties, the Laboratory purchased a Perkin Elmer infrared and a Beckman V and UV spectrometer. She immediately thought of using them for research and went to Orsay where she studied these techniques. Some of the results she obtained in this field were published, in 1970, by the *J. Inorg. Nucl. Chem*. She started a study on the complexes of niobium and other group V metals, though there are no publications.

She ended her career at the age of seventy. She was an extraordinarily kind and gentle woman, an outstanding teacher, gifted to reach out to the students. All those who were lucky to know her, remember her.

She died in 2004.

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JACOB BJERKNES AND THE WEATHER FORECAST IN PORTUGAL

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Abstract

In 1922, due to the initiative of António de Carvalho Brandão, the Meteorological Service of the Navy was founded in Portugal. This was the beginning of synoptic meteorology in the country. Brandão became not only the first director of that Meteorological Service but also one of the most known Portuguese meteorologists. Four years later, at an international meteorology meeting held in Zurich, Brandão announced the decision of the Portuguese government to install a wireless telegraphy station at Azores to provide meteorological services. In 1927, Colonel Emile Delcambre, head of the French meteorological services, and Jacob Bjerknes, the famous Norwegian meteorologist, came to Portugal to meet with Portuguese authorities and discuss details concerning the Azores international station. Bjerknes came earlier to study the local weather and to get acquainted with the Portuguese meteorology services. In the last day of his visit, 23rd May, he went to the University of Coimbra with Carvalho Brandão, where he delivered a conference, later published in O Instituto*, the journal of Coimbra's academic society. He then referred to the important role Portugal might play in European weather forecast and described a project of establishing several stations in Northern Atlantic to collect transmissions from all ocean liners and communicate this information to the International Meteorological Organization, founded in 1873. The international meteorological station of Azores started to operate in 1929. The inauguration was announced at the international meteorological congress held in Copenhagen in that same year. In 1934, the Meteorological Service of the Navy was receiving daily reports from meteorological observatories in Coimbra, Oporto, Azores and 30 other stations. Of these stations, 18 were international, i.e., their data were relayed abroad from Lisbon.*

We present an episode of the history of meteorology in Europe, between the two world wars, in which Portugal played a relevant role. But we start with a brief history of meteorology in Portugal to portray its state at the beginning of the twentieth century.

The Meteorology in Portugal in the Nineteenth Century

The first meteorological observations made with a scientific purpose were made in Portugal at the beginning of the nineteenth century. Although the major concern was to establish climatic patterns of some regions and to use this information to improve public health, the hope was that the gathered data could be used to deduce major laws of atmospheric phenomena. This was the case of the observations done by Constantino Botelho de Lacerda Lobo (1754–1820), Professor of Physics at the Natural Philosophy Faculty of the University of Coimbra (UC), done at the Cabinet of Experimental Physics from 1812 to 1817. Unfortunately, these observations were of little scientific value, since they were made indoors and not at fixed hours. Also Marino Miguel Franzini (1779–1861) registered daily meteorological data in Lisbon, from 1816 to 1826 and again from 1835 to 1855. He set up a small observatory at his home, where he collected weather data, and applied statistical analysis to it (Nunes, 1988).

In 1843, Guilherme Dias Pegado (1803−85) requested the government to establish a meteorological observatory in Lisbon, as part of the Polytechnic School. The Prince Luís Observatory, the first Portuguese meteorological observatory, started to operate in October 1854. The first daily meteorological bulletin, which was delivered by the Paris Observatory from 1858 on, included Lisbon records. Storm warnings were issued, and alert signs were hoisted at the coastal stations (Ferreira, 1940). From 1865 on, the Lisbon's Observatory published a daily bulletin that provided the weather in Lisbon for the next day. Along the second half of the nineteenth century, it was a monitoring centre, gathering, in 1892, the observations of 14 national meteorological stations, seven in Spain and one in Ireland (Aires, 1892).

In Coimbra, the meteorological observations at the Physics Cabinet were resumed in 1853, being the results published in *O Instituto*, the literary and scientific journal of the academic society with the same name (Institute of Coimbra – IC), which had been founded in Coimbra in 1852 (Leonardo *et al.,* 2009). In the pages of this periodic several articles relating to meteorology appeared in its first years. Some of them stressed the urgency to establish a meteorological observatory in Coimbra in order to promote meteorological studies and to attain the same progress that was being achieved in other institutions, in the country and abroad. In 1861, the Faculty Council asked Jacinto António de Sousa (1818−80) to gather the necessary means to establish a meteorological observatory. Meteorological observations were initiated in the new observatory on 1 February 1864, while its construction continued. A telegraphic communication with the Meteorological Observatory Prince Luís was established in 1867, transmitting the morning daily observations (Lopes, 1893).

By the end of the nineteenth century, the meteorological services were centred in Lisbon. Beside the observatories of Lisbon and Coimbra, the Meteorological Observatory Princess Amelia was established in Oporto in 1888.

In the early twentieth century a *"sharp decline"* in the work done at the observatories of Coimbra, Oporto and Lisbon was visible, mainly due to internal disputes, lack of funding and staff scarcity (Leonardo *et al.,* 2011). A Technical Committee of Meteorology was appointed in 1921 to organize the meteorological services. It included, among others, Anselmo Ferraz de Carvalho and António de Carvalho Brandão. In 1914, Anselmo Ferraz de Carvalho (1878−1955) was appointed Director of the Coimbra Meteorological and Magnetic Observatory (MMO). This committee stated that there was a *"lack of meteorological stations in many regions"* and *"oceanic weather, in which we should work with other maritime countries, has long been abandoned between us"*, so that *"the actual weather service […] is fighting with many deficiencies derived from the lack of personal and material resources"* (cited in Leonardo *et al.,* 2011). The creation of a Central Institute for Meteorology was recommended, including a weather forecast and climate services. In spite of that, the scenario remained bleak.

Fig. 1: António de Carvalho Brandão.

The new methods of Weather Forecasting after the First World War

To endeavour in weather prediction, the study of meteorological phenomena within large areas by simultaneous observation of the atmospheric elements was needed. It was necessary to lay down a network of stations, spread around the country, including the Atlantic islands of Madeira and Azores, relaying surface weather observations made at periodic times. This organization was only possible with the creation of the Meteorological Services of the Navy in 1922. They were an initiative of Vice-Admiral Eduardo Augusto Neuparth (1859−1925) and the navy officer and meteorologist António de Carvalho Brandão (1878−1937). The General Marine Stewardship requested the MMO and other observatories to implement synoptic observations. Synoptic meteorology entailed the elaboration of meteorological maps, covering Western Europe, and the application of new methods to weather forecasting. Brandão played a key role in Portuguese meteorology, particularly in weather prediction, attending many international conferences on behalf of the government (e.g., London, 1921, and Utrecht, 1923).

Weather forecasting was still controversial in the scientific community at the beginning of the twentieth century (the Portuguese proverb that stated that *"if you want to lie, start predicting the weather,"* was difficult to override). Local weather forecasts done by João Carlos de Brito Capelo (1831–1901), when he was director of the Observatory Prince Luís in Lisbon, only indicated that day's probable weather. They were based in his continued attention to the state of the sky, guided by his sailor's sixth sense (Leonardo *et al*., 2011).

The 3rd Congress of the Portuguese and Spanish Associations for the Advancement of Science was held in Coimbra in 1925. Brandão presented the memoir "*The modern methods of weather forecast in Portugal".* According to Brandão, meteorology had improved in the last decades, leading to the *"formation of new hypotheses and the discovery of new laws and new methods for predicting the weather […], characterized by an intense scientific activity, looking at last to mark the actual beginning of a Meteorological Science, and thus the reasonable expectation of a scientific forecast of weather, at least in the short term".*

Modern meteorology had developed from the discovery of the laws that established the relation between atmospheric pressure and winds. The use of wireless telegraphy could open new horizons to the application of these laws to weather forecast. But the depiction of isobar lines, in spite of their relation to the winds, could not by itself allow their prediction a few hours later due to the ignorance of the evolution of the barometric field. To solve this problem, two major methods had been advanced since the First World War: the French method, based on variation nuclei and cloud systems, and the Norwegian method, based on the polar front theory.

In spite of its less scientific character, the French method provided satisfactory results in weather prediction. By tracing lines of equal barometric variation in equal time intervals (3 h, 6 h, 12 h or 24 h), also called isallobar lines, it was possible to observe nuclei of maximum variation. The movement of these nuclei showed a perfect continuity, unlike the trajectory of the depressions, and this regularity could be used to draw new isobaric lines, for the next 12 h or 24 h, and to deduce the winds and the probable state of the weather. Besides the study of the maximum variation nuclei, the French method included the depiction of cloud systems in the synoptic charts. These large clusters of clouds formed according to certain principles and moved under certain rules. There were several kinds of cloud systems, each with special characteristics and some related to unstable weather and rain. Though its reliability, the cloud system process could not be implemented in Portugal or in other countries in the oriental margin of an ocean due to the extreme difficulty in determining the cloud systems coming from the west, even considering the observations aboard ships or other oceanic stations (Brandão, 1925).

The Norwegian method, the most scientifically advanced of the time, was based on surfaces of discontinuity between adjacent air masses with different temperatures: a warm mass of air coming from lower latitudes encountered a cold mass of air originated in the polar region. Due to the Earth's rotation, the movement of the warm mass had a component to the east while the cold air mass swayed to the west. The surface of boundary between both air masses was called the polar front.

This theory was proposed by the Bergen school, in Norway, founded and headed by the physicist Vilhelm Bjerknes (1862–1951), where a meteorology service operated and applied the new method to weather prediction (Friedman, 1989). The existence of these two adjacent air masses, within a fluid, with opposite directions of displacement tended to the formation of whirls by a process where a tongue of one of the masses was projected inside the other. This was the inception of a mid-latitude cyclonic event. The north extremity of the tongue of warm air was the centre of a barometric depression known as warm sector. In the northern hemisphere, the air moved around this depression, counter clockwise, and there was also an upward movement of the air mass within the warm sector that would give place to condensation and rainfall (Figure 2).

With the further movement to the east of the tongue of warm air, the warm sector would become bordered by two surfaces of discontinuity: a warm front ahead, moving slowly as the warm air mass is climbing over the colder air mass; and a cold front in the back, moving faster (Figure 3). Hence, the warm sector gradually decreases in width while the warm air rises in altitude, giving rise to an occlusion when this sector has vanished completely from the surface. The reported evolution of a cyclonic event was deeply related with the weather below in terms of temperature, pressure and rainfall (Brandão, 1925).

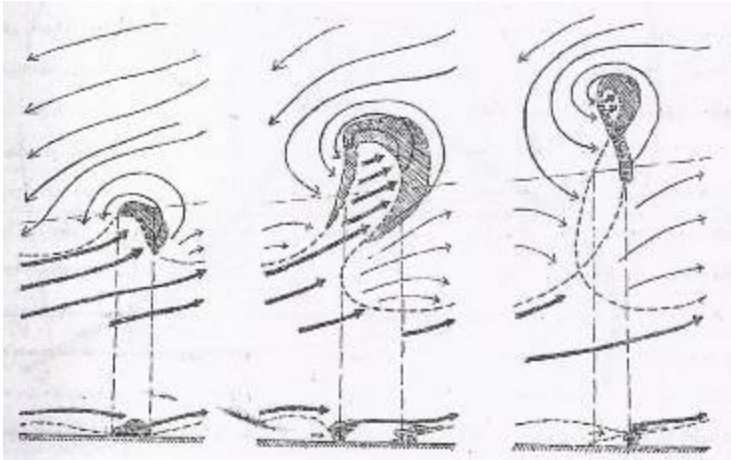

Fig. 2: Formation of a mid-latitude cyclone – front theory (Brandão, 1925).

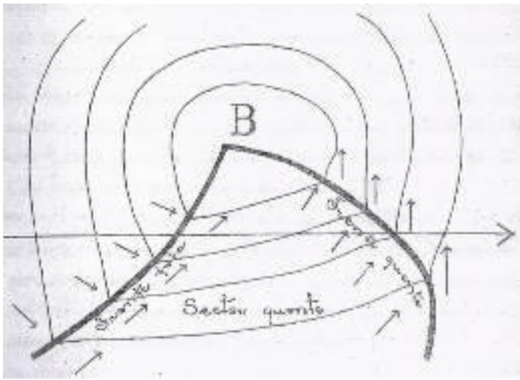

Fig. 3: Bjerknes' cyclone model (Brandão 1925).

Weather forecast by the Norwegian method relied in the regularity of the movement of the cyclonic event and its discontinuity surfaces. Therefore, the approximation of a front produced changes in pressure, temperature and wind direction. The passage of a warm front, where a retreating cold air mass was pursued by a warm current that was bound to rise (Figure 4), gave place to a temperature rise, the opposite occurring with a cold front, where a moving wedge of cold air caused the ascension of a mass of warm air (Figure 5). Both events generated the rising of hot air, which originated condensation and rain (*ibid*).

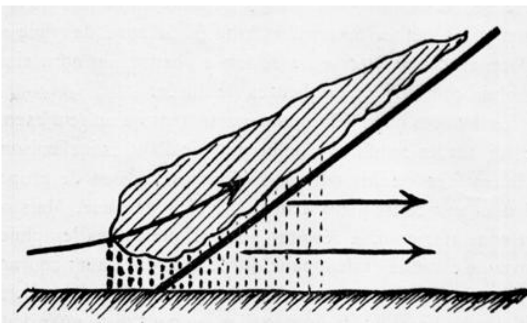

Fig. 4: Warm front (Bjerknes, 1928).

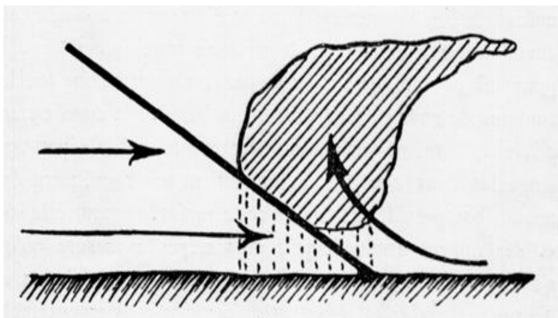

Fig. 5: Cold front (Bjerknes, 1928).

In Coimbra's congress Carvalho Brandão met António Gião (1906-69), then a young student of the University of Coimbra. In the same year, Gião departed to study at the Institute of Physics of the Globe, in Strasbourg, where he became a geophysical engineer. In 1927, Gião took an internship at Bergen School where he met Jacob Bjerknes (1897–1975), Vilhelm's son, who then headed the Norwegian National Weather Service (Gião, 1927).

Bjerknes and the International Meteorological Station of the Azores

The implementation of a worldwide weather forecasting service, based on the Norwegian method, depended on the existence of a large network of stations collecting meteorological information, especially over the North Atlantic. Also, the installation of radio transmitters aboard transatlantic liners allowing them to regularly relay meteorological data while crossing the ocean, would give additional coverage to the project. This was the issue in the meteorology international meeting which took place in Zurich in 1926. Carvalho Brandão, as chief of the Portuguese meteorological service, announced there the commitment of the Portuguese government to establish an international meteorological station at the Azores, which was prepared to receive and relay by radio meteorological data emitted by ships in the region. A committee, composed by Jacob Bjerknes and General Émile Delcambre (the director of the French meteorological services), was set up to follow this matter until its final resolution (Leonardo *et al.,* 2011).

Fig. 6: Jacob Aall Bonnevie Bjerknes (Bjerknes, 1928).

Jacob Bjerknes was a member of the group of meteorologists, led by his father, which developed the model of mid-latitude cyclones based in the front theory. In May 1927, he came to Portugal to study the local meteorology and to meet the Portuguese government to stress the urgency of the meteorological station of the Azores (Delcambre and Brandão were also present). This station would solve *"a problem that concerns the scientific bodies of Europe, in charge of the study and weather prediction"* due to the lack of observations in the Northern Atlantic.

Bjerknes was invited by Ferraz de Carvalho, on 23rd May 1927, to speak at the IC on *"Les bases scientifiques et techniques de la prévision du temps et le rôle du Portugal à ce rapport"*. He presented his polar front theory and the means to apply it to weather prediction. In his paper, published in *O Instituto*, he recalled the importance of weather forecasts for maritime countries. A system of stations relaying atmospheric data abroad by wireless telegraphy, scattered across the globe, was essential. In a map, Bjerknes showed the location of weather stations that, according to the international plan, would receive information from vessels with radio emitters that were regularly transmitting meteorological data within the areas covered by each station. These stations would relay the information, three or four times a day at established international hours, to the European Meteorological Services. The region covered by the Azores, Madeira and the Portuguese coast encompassed a large ocean surface. The others were the Bermudas and, further north, Newfoundland, Greenland and Iceland (Bjerknes, 1928).

Although a Meteorological Service had been established in Azores in 1901 (with four observatories) (Tavares, 2009), the International Meteorological Station of Azores, in Faial Island, only became operational in 1929. Delcambre, in the International Meteorological Congress of Copenhagen which took place in that same year, pointed out the highest service which was being provided to science by Portugal. He said it was one of the most important events contributing to the progress of meteorology in the century, adding that it was the beginning of a new era: indeed, the Azores station was the completion of a vast international network, a base to aero-transatlantic navigation. In Copenhagen the major collective emitters for the northern hemisphere were created. They should incorporate and link together emissions received from several international weather stations (Morna, 1935). The Azores' Station information was included in the collective emissions from Western Europe.

The Portuguese Meteorological Services before the Second World War

From its creation until 1946, the Meteorological Service of the Navy (MSN) was responsible for weather forecasting. In 1934, the MSN was receiving daily reports from meteorological observatories in Coimbra, Oporto, Azores and 30 other stations (26 in the continent, 2 in Madeira and Azores and 2 in Green Cape). Of these, 18 were international, i.e., their data were relayed abroad from Lisbon. More than 80% of the words used in the Navy radiotelegraphic services were taken by the meteorological services. The service produced daily meteorological maps of Europe and the Oriental Atlantic (Figure 7) along with weather forecasts (Morna, 1935).

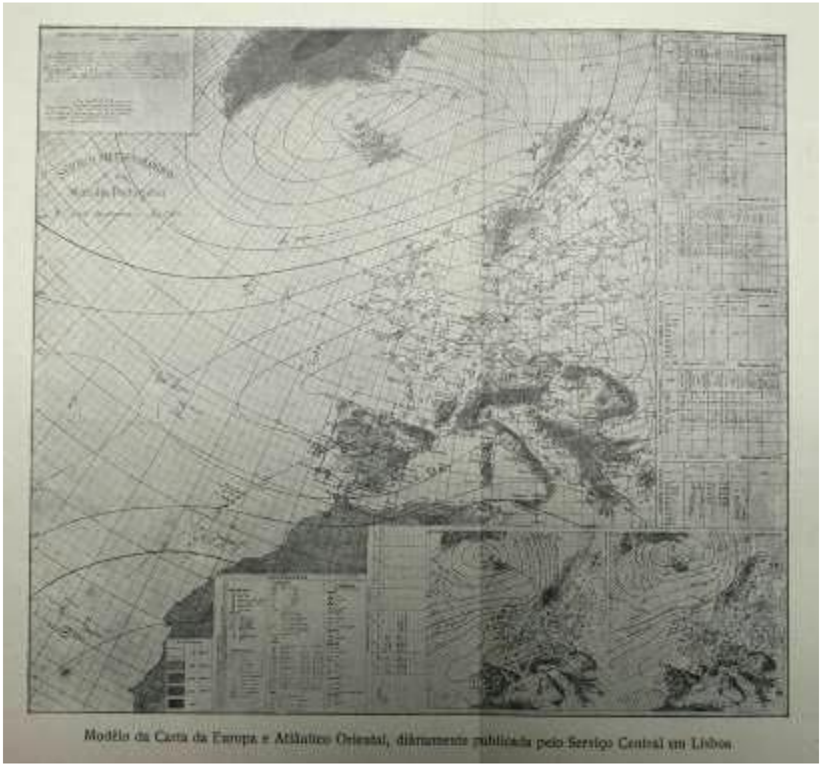

Fig. 7: Meteorological map of Europe and Eastern Atlantic (Morna, 1935).

The efficiency of the weather prediction service in the Atlantic was tested by the preparation of the flight across the Atlantic of the airplane ESA (*Espírito de Santo Agostinho*), from Lisbon to New York, in 1931 (Morna, 1934). The help of the MSN to decide the best route was requested by the flight crew, composed of Costa Veiga, Christhien Johnson and Willy Rody (Figure 8). This endeavour ended in a sea landing due to lack of fuel, the meteorological services being essential to rescue the pilots. According to the Spanish journal ABC, in 16 September 1931, *"the atmospheric conditions of the Atlantic are still being those predicted by the Meteorological Service of the Navy ministry. Everything seems to indicate that the aviators had displaced themselves from the route that those services delineated"*.

Despite the successes of the weather services, meteorological science was scarcely studied in Portugal. An exception was António Gião, one of the most renowned Portuguese meteorologists. Gião published *La Mechanique Différentielle des Fronts e du Champ isallobarique* (1930), a book containing forewords from Delcambre and Jacob Bjerknes. He had proposed to the National Board of Education in Portugal the creation of an Institute of Atmosphere Mechanics. The board counter offered him a scholarship, which he refused.

Brandão resumed the state of the art of meteorology science in an article entitled *Importance of General Movements in the study of the atmosphere*, published in 1931 in *A Terra,* a Portuguese journal on geophysics. He addressed the Norwegian method, stating that it *"did not lead in turn to a defined method of prediction, but merely to the acquisition of scientific knowledge on certain atmospheric phenomena in the dynamic point of view, hitherto almost unexplored […], the famous Norwegian method is but one method of analysis and diagnosis of meteorological situations […] [in] the regions where the discontinuities of meteorological elements occur"*. Regarding Gião's studies, *"however it should represent a major advance in meteorological science, still Gião's method cannot, naturally, give us a definitive solution to the problem of prediction"* (Brandão, 1931).

Fig. 8: The pilots of ESA after their rescue.

In 1933, the Society of Meteorology and Geophysics of Portugal (SMGP) was founded in Coimbra, being Anselmo de Carvalho its Honorary Chairman. Albeit its ambitious goals and the major figures involved, it did not receive the required government support to become permanent and was extinguished after a few years (Leonardo *et al.,* 2011).

By then, various meteorological services were scattered by several state and government departments, like the Civil Aviation Secretariat, the Observatory Prince Luís and the Meteorological Service of the Azores, under the ministry of Education; Meteorological Services of the Navy, supervised by the ministry of the Navy; the General Directorate of the Agricultural Services, under the ministry of Economy; and similar services in the ministries of the Army and of the Colonies.

* * *

Brandão, who always sought to implement scientific meteorology, established contacts with major European figures like Delcambre, Bjerknes and Shaw. His great ambition was the creation of a National Institute of Meteorology. According to Gião *"His project and initiatives collided with a wall of envy, with the rivalry of pulverized services in multiple ministries, and was with the bitterness of not being able to do anything that he passed away prematurely."* A major difficulty was the systematic lack of financial resources and qualified personnel. An example was the inability to meet Gião's challenge to create an institute devoted to the study of the atmosphere.

International pressure for advancing meteorology in Portugal has always been present, especially from the days when the gathered observations, particularly in the Atlantic islands, became essential to the development of the global effort of weather forecasting (Leonardo *et al.*, 2011).

Only in 29th August 1946, after the war's end, the creation of the National Meteorological Service was established, integrating all the meteorological services that were dispersed among different institutions and observatories.

References

AIRES, Bernardo. (1892). *A circulação atmosférica e a previsão do tempo*. Coimbra: Imprensa da Universidade.

BJERKNES, J. (1928). Les bases scientifiques et techniques de la prévision du temps et le rôle du Portugal à ce rapport. *O Instituto: journal scientifico e litterario* 75: 90–111.

BRANDÃO, A. Carvalho. (1925). *Os modernos métodos de previsão do tempo em Portugal*. Memória apresentada no Congresso de Coimbra de 1925. Lisboa.

BRANDÃO, A. Carvalho. (1931). Importância dos Movimentos Gerais no estudo da atmosfera. *A Terra: revista Portuguesa de geofísica* 2: 1–8.

FERREIRA, H. Amorim. (1940). *O observatório do Infante D. Luiz: memória apresentada pelo director do Observatório.* Congresso de História da Actividade Científica Portuguesa. Lisboa.

FRIEDMAN, Robert Marc. (1989). *Appropriating the Weather: Vilhelm Bjerknes and the Construction of a Modern Meteorology*. Ithaca: Cornwell University Press.

GIÃO, António. (1927–1928). La meteorologie à Bergen, aujourd'hui et demain. *Ciel e tierre: Bulletin de la Société belge d'astronomie* 43: 233–238, 44: 9–23, 45–53, 93–106, 147–153, 190–202, 253–255.

LEONARDO, A. J. F., MARTINS, D. R. and FIOLHAIS, C. (2009). O Instituto de Coimbra: breve história de uma academia científica, literária e artística. In: *Tesouros da biblioteca geral da Universidade de Coimbra (coordenação de A. E. Maia Amaral)*, 115–125. Coimbra: Imprensa da Universidade.

LEONARDO, A. J. F., MARTINS, D. R. and FIOLHAIS, C. (2011). The Meteorological Observations in Coimbra and the Portuguese participation in Weather Forecast in Europe. *Earth Sciences History* (preprint).

LOPES, Adriano de Jesus, (1893). Observatório meteorológico e magnético da Universidade de Coimbra. *O Instituto: journal scientifico e litterario* 40: 201–209.

MORNA, Álvaro de Freitas. (1934). A preparação meteorológica da travessia do Atlântico pelo avião "Esa". *A Terra: revista portuguesa de geofísica* 3: 1–11.

MORNA, Álvaro de Freitas. (1934–1935). Meteorologia. *A Terra: revista portuguesa de geofísica* 16: 27–32, 17: 29–30, 18: 19–31, 19: 16–27, 20: 20–25.

NUNES, Maria de Fátima. (1988), O universo estatístico de Marino Miguel Franzini (1800–1860). In: *O Liberalismo Português: Ideários e Ciências*. Lisboa: Instituto Nacional de Investigação Científica.

TAVARES, Conceição, (2009). *Albert I do Mónaco, Afonso Chaves e a Meteorologia nos Açores*. Ponta Delgada: Sociedade Afonso Chaves e Centro Interuniversitário de História das Ciências e da Tecnologia.

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MAKING SCIENCE COOLER: CARRÉ'S APPARATUS

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Abstract

The need of refrigeration in the laboratory is not a new idea, and this paper intends to make a tour through its history in the 19th century, where one of the first attempts to make things cooler was created by the Carré brothers. The aim is to explore the types of Carré Apparatus and the techniques used in it, and to study their evolution and improvement. This work will focus mostly in the instrument's collection of the Museum of Science of the University of Lisbon, which owns several exemplars of the Carré Apparatus as well as improved versions of it. Our approach tries to put these objects back in their real place in the 19th century lab work, corroborating equipment with archival sources by establishing links between equipment and photographs, reports, curricula, chemistry compendia, textbooks written by the professors, where certain experimental settings are described and depicted, invoices and other administration papers, e.g. the 1854 inventory, among other archival and bibliographical sources. The 19th century 'Laboratorio Chimico' *of the Polytechnic School (1837-1911), integrated in the Museum of Science of the University of Lisbon suffered a restoration and a musealization work (1998-2007) as well as a part of the museum's heritage, where a Ferdinand Carré's apparatus is included. This historical space and the pertaining collection allows us to go back to the 19th century scientific practice at the Lisbon Polytechnic School and breathe its atmosphere in a golden period.*

Artificial production of ice

In the 16th century, the discovery of chemical refrigeration was one of the first steps toward artificial means of refrigeration. The first attempt to artificial production of ice was implemented by Lahire in 1685: he produced ice by wrapping wet ammonia salt and a jar full of water, already cooled (Figuier; 1873). The first machines used in the mechanical production of cold were constructed on the principle of the vacuum machine, wherein the vacuum –which was obtained mechanically– permitted the refrigerant to boil at a sufficiently low temperature to secure the results desired. These machines included that of William Cullen (1710-1790), used in 1755 for producing ice using water under high vacuum; the optician and scientific instrument maker Edward Nairne (1726 – 1806) recognized that water vapour is rapidly absorbed if he put a container with water near a pot with sulphuric acid, both closed, inside a pneumatic machine bellflower. Later in 1811, the physicist John Leslie (1766 –1832) obtained water boiling at a low temperature (Leslie; 1813).

Ferdinand Carré's apparatus

In 1856, the French engineer, Ferdinand Philippe Edouard Carré (1824–1900) developed his first refrigeration apparatus using sulphuric ether, with the inconvenience that ether is highly inflammable. Later in 1859, Ferdinand Carré made the first ammonia / water refrigeration machine. The Carré absorption cycle used in this apparatus was an outgrowth of the observation by Michael Faraday (1791–1867) in 1823, which noticed the cooling properties of ammonia closed in a bent glass tube. Ferdinand replaced the ether by ammoniac because of its stability, low boiling point (44º C) and its affinity with water. This absorption machine (figure 1) was patented in France in 1859 and in the U.S.A. in 1860 (Figuier; 1873). The abstraction of heat is made by evaporation of a separate refrigerating agent of volatile nature under the direct action of heat, which agent again enters into solution with a liquid. This is termed the "absorption system" (Greene; 1916). When a body changes its state, a certain amount of energy must be absorbed by that body to bring about this changed state. The name "heat energy", or heat, is applied to this. Energy is required to change a body from the liquid state to the vapour state.

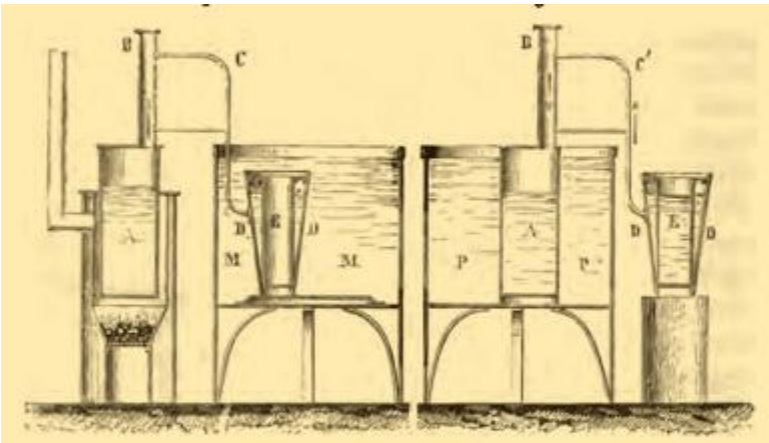


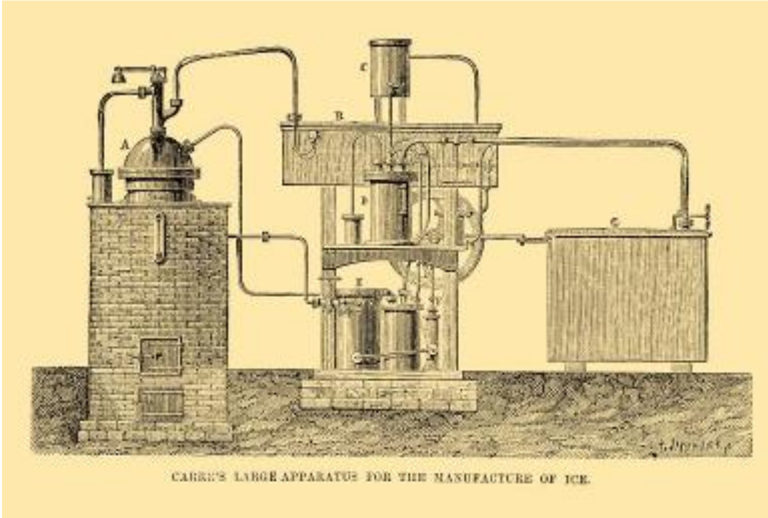
Fig. 1: Engraving representing a Ferdinand Carré's apparatus (Jamin; 1878).

This apparatus consists in two pots hermetically closed, in communication by means of tubes. The largest (A), a cylinder, contains a concentrated ammonia solution (ammoniac gas dissolved in water), which is heated with a furnace. The small one (E), a vessel in the shape of a truncated cone having a cavity in centre, was kept in a trough with water. This refrigerating machine is furnished with a thermometer which doesn't communicate with the interior of cylinder (A).

But how does this apparatus work? The cylinder (A) contains ammoniac gas, dissolved in water. When it is heated, the gas espies from the liquid and passes into a vessel (E), after having passed through all the connecting tubes. But on arriving there it finds no exit, accumulates, and under the influence of cooling and considerable pressure, becomes liquefied. This is the moment when the generating cylinder (A) is plunged into a cold water container and consequently cooled. The gas which is liquefied in vessel (E) returns to a gaseous state, a corresponding absorption of heat accompanies this change, and the water dissolves once more the ammoniac gas and the primitive ammoniac aqueous solution formed (in A). Soon it is possible to take from the cavity a block of ice (Tissandier; 1867).

Ferdinand apparatus had great advantages: ice could thus be produced using small portions of coal; its small dimensions prevented the production of large quantities of ice at once. But it could not be made to work continuously, and could never be of much industrial value. Another apparatus (figure 2), however, had been invented by him, constructed on a much larger scale, and had successfully solved the important problem of the artificial manufacture of ice or the production of cold.

A large boiler (A) contains the solution of ammonia. The gas escapes and becomes liquefied in a reservoir (B), cooled off as it is by the water which falls from a reservoir (C). The liquid ammonia penetrates into the hollow sides of the refrigerator (G), which contains cylinders filled with the water that is to be frozen. During this time an especial arrangement allows this waste water of the boilers to penetrate, after having cooled off, into a vessel (E) connected with the cylinder (D), in which the ammonia is distilled that has been volatilized in the refrigerator. The original liquid, thus regenerated, is conveyed into the boiler by means of a pump (F). This apparatus acts with great regularity, and it is astonishing to see large blocks of ice issuing from this refrigerator, which are formed as if by magic, without any visible agent to show the secret of their formation.



CARRÉ'S LARGE APPARATUS FOR THE MANUFACTURE OF ICE.

Fig. 2: Engraving representing a Ferdinand Carré industrial apparatus (Tissandier; 1867).

In 1862 Ferdinand Carré exhibited his ice-making machine at the Universal London Exhibition, producing an output of 200 kg/h (Figuier; 1873). Its design was based on the gas-vapour system. Ferdinand's industrial apparatus had the great advantage of allowing a continuous ice production.

Edmond Carré's apparatus

In 1811 John Leslie described a laboratory method of making ice by freezing water in a saucer placed in a receiver under vacuum –the water vapour, as it boiled off under low pressure maintained in the receiver, is absorbed by sulphuric acid in another saucer (Leslie; 1813). In the case of the vaporization of a liquid under reduced pressure, the aim of this reduction is to allow the evaporation at such a low temperature that heat may be removed from surrounding objects of low temperatures. When the pressure was reduced, the boiling temperature would decrease and with the evaporation of some liquid, ice could form. Leslie improved this machine later into a pneumatic machine, under the principle that the abstraction of heat is effected by the evaporation of a portion of the liquid to be cooled, the process being assisted by an air pump (Macintire, 1928). This is known as a vacuum system.

In 1866 Edmond Carré (1833-1894), Ferdinand's brother, repeated the Leslie experiment and found that it was necessary a larger quantity of sulphuric acid for a small quantity of water. At the same year, Edmond adapted the Leslie process to a pneumatic machine and added an agitator to renew the surface of sulphuric acid and increase the surface area in contact with water vapour and thus improve the capacity of sulphuric acid to absorb it. He obtained a bigger efficiency, allowing the production of greater quantities of ice (Jamin; 1878). Water is placed in a bottle that adapts to a tube, which will give a bath of sulphuric acid, continually being shaken with an agitator (figure 3).

This apparatus consists of a pneumatic pump (P) and a large tank (R). The sulphuric acid is introduced in the tank (R), which is in communication with the pneumatic pump (P) through the tube (b) and in communication either with the bottle of water through the tube (h) and the tap (r). The lever (M), which gives movement to the stem of the piston of the pneumatic pump, moves through the stalk (t) an agitator, which constantly renews the surface of the acid.

The Edmond's apparatus great advantage (Vidal; 1893) was that in three minutes the water temperature decreases to the freezing point and ice is produced.

Refrigeration apparatus in the Polytechnic School of Lisbon

The Polytechnic School of Lisbon was a landmark in Portugal because it was one of the few schools where chemistry was taught. It was founded in 1837 in order to prepare students for the Army and Navy schools, while offering access to higher science education for students of other professions, medicine and pharmacy. In accordance with the foundational document, the School was to be provided with a Library, an Astronomical Observatory, a Cabinet of Physics, a Chemical Laboratory, a Natural History Cabinet, a Botanical Garden and other common facilities. The disciplines were organized and taught in five courses. The *6ª Cadeira* –denominated 'General Chemistry and Principles of its Applications to the manufactures'– was usually lectured in the second year of most of the courses (Elvas; 2009). An additional discipline was created in 1859: Chemical Analysis and Organic Chemistry.

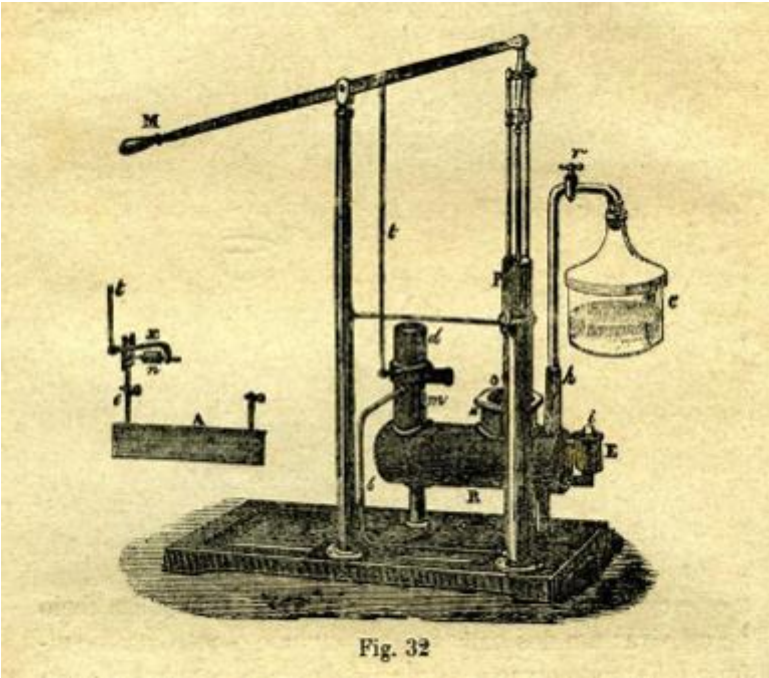


Fig. 3: Engraving representing an Edmond Carré's machine (Vidal; 1893).

Júlio Máximo de Oliveira Pimentel (1809-1884), the first Chemistry Professor of the Polytechnic School of Lisbon (between 1837 and 1864), mentioned on his chemical lesson's book chemical reactions where ice was used, for example, the hydrocyanic acid synthesis, the sulphurous acid synthesis and the hydrogen fluoride synthesis (figure 4). Adriano Augusto de Pina Vidal was a Physics Professor of the Polytechnic School of Lisbon (Vidal; 1893). He referred on the physical lesson's book the Ferdinand Carré and the Edmond Carré machines and there he described the mechanism of these machines.

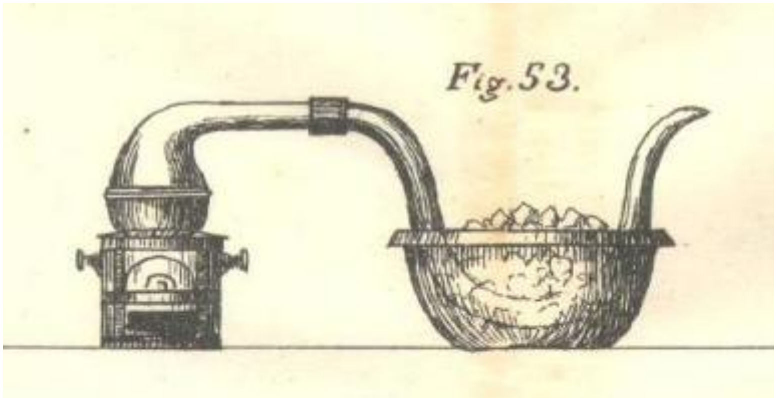


Fig. 4: Engraving representing the hydrogen fluoride synthesis (Pimentel; 1850).

Refrigeration apparatus in MCUL

As a case study we present the apparatus that belonged to the Polytechnic School of Lisbon. At the time there were a *Laboratorio Chimico* and a lecture theatre that is now part of the Museum of Science of the University of Lisbon's heritage. In parallel with these spaces, MCUL hosts a Historical Archive and a Cultural Heritage where Carré's apparatus are included.

The Museum of Science of the University of Lisbon's Historical Archive encloses documents concerning the icemaker apparatus acquisition, such as:[1]

- An account of expenditure for the *5ª Cadeira* of the Polytechnic School of Lisbon (1863) where we can read *'Ice maker apparatus' acquisition'*;
- An Invoice for the *6ª Cadeira* of the Polytechnic School of Lisbon (1869) which refers a *'Carré's apparatus acquisition'*;
- An account of expenditure for the *6ª Cadeira* of the Polytechnic School of Lisbon (1883) where we can see *'Remediation of a piece of ice apparatus'*

The Museum of Science of the University of Lisbon has three refrigeration domestic devices in its collection:

1. A refrigeration appliance which belonged to the Portuguese royalty –consists on a pneumatic machine for Leslie Experiments (MCUL153) (figure 5).
2. Ferdinand Carré's apparatus (MCUL 2035), actually displayed on the *Laboratorio Chimico*, the 19th historical space that belonged to the Polytechnic School of Lisbon (1837-1911) (figure 6).
3. An Edmond Carré's apparatus (MCUL 4294) (figure 7).

Fig. 5: Pneumatic machine for Leslie Experiments, MCUL153
(Photo: S. Carvalho, Courtesy Museum of Science, University of Lisbon)

[1] MCUL Historical Archive, University of Lisbon.

Fig. 6: *Laboratorio Chimico*
(Photo: M. C. Elvas, Courtesy Museum of Science, University of Lisbon)

Fig. 7: Edmond Carré's apparatus, MCUL 4294
(Photo: M. Peres, Courtesy Museum of Science, University of Lisbon)

Laboratorio Chimico and the lecture theatre of the Polytechnic School that are now part of the Museum of Science of the University of Lisbon's heritage suffered a restoration work between 1998 and 2007. The *Laboratorio Chimico* and its collection provide an excellent opportunity to look into the conditions of scientific practice of chemistry at the *Laboratorio Chimico* by the end of the 19th century. The restoration project of the *Laboratorio Chimico* included the restoration of a part of the Museum's chemistry collection. Ferdinand apparatus was one of those and it is displayed in the *Laboratorio Chimico* (figures 8 and 9).

Figs. 8 and 9: Ferdinand Carré's apparatus, before and after restoration, MCUL 2035
(Photos: M. C. Elvas, Courtesy Museum of Science, University of Lisbon)

The Ferdinand Carré's apparatus and its restoration was one of the several selected objects and results of a study which corroborates equipment with archival sources by establishing links between the collection and photographs, correspondence, reports, curricula, chemistry compendia, among other archival and bibliographical sources. These know-how then supplied the points of departure for further investigation and attempts for answering the following questions:

1. Which of the objects, present in the Museum collection today, were actually part of the *Laboratorio Chimico*? And for how long have they been used?
2. What was the context of their use during that particular period of teaching, research and development at the *Laboratorio Chimico*?
3. Which objects have supposedly been used, but are not to be found anymore in the collection?
4. When, from where and why exactly have they been purchased?

Ferdinand Carré's apparatus belongs to the objects set that could not be located on photographs, but could be identified in other documental sources of the archive, and still is present in the collection. This apparatus appeared on textbooks written by the professors, where certain experimental settings are described and depicted.

Carré´s apparatus: spread out

It was about the middle of the 19th century that the Carré brothers produced commercial machines for the freezing of water. Both machines operated by removing heat by vaporization of a volatile fluid. Edmond Carré's apparatus used the evaporation of water vapour at very low pressures and Ferdinand Carré's used the evaporation of liquid anhydrous ammonia.

The Carré's apparatus appeared in several catalogues of laboratory material of chemistry and physics; schoolbooks; medical, industrial and advertising publications; universal exhibitions and so on. Some examples are a 19th French advert (figure 10) in the *Revue des Instruments de Chirurgie* (1892).

Other Portuguese collections exhibit the Carré's apparatus, for example, the Science Museum of the University of Coimbra owns a Ferdinand Carré's apparatus (figure 11) and an Edmond Carré's apparatus.

From what was told above, the importance of this mechanical refrigeration can be seen, but with its development further applications have been made and used into many industries (Greene, 1916):

- In refining oils, refrigeration is used for the removal of certain paraffin products;
- In metallurgical operations, refrigeration allowed to get air of uniform quality;
- In textiles manufacture, in tobacco curing and cigar making, in perfumery making, in photographic films manufacture and other products.

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ADRESSE TÉLÉGRAPHIQUE : QUADRATUS, PARIS

Fig. 10: French announce (Courtesy of Paolo Brenni)

Fig. 11: Ferdinand Carré apparatus, QUI 0169
(Photo: Gilberto Pereira Courtesy of Science Museum of University of Coimbra)

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References

BUIGNET, Henri (1876). *Manipulations de Physique*, Bailliére et Fils, Paris.

CARRÉ, Edmond (1867). *Sur de nouveaux générateurs de froid*, Comptes Rendus des Séances de L'Académie des Sciences, Tome soixante-quatrième Paris, p. 897-898.

CARRÉ, Ferdinand (1860). *Note Sur de un appareil propre à produire du froid,* Comptes Rendus des Séances de L'Académie des Sciences, Tome cinquante et unième, Paris, pp. 1023-1027.

CARRÉ, Ferdinand (1862). *Rapport sur un appareil de M. Carré ayant pour objet la production du froid artificiel*, Comptes Rendus des Séances de L'Académie des Sciences, Tome cinquante-quatrième, Paris, pp. 827-840.

CARVALHO, S. (2003). *Experimental reconstitution work of Laboratory experiments of polytechnic school of Lisbon, at the end of XIX century*, Faculty of sciences of the University of Lisbon.

CAZIN, Achille (1868). *Bibliothèque des Merveilles, La Chaleur*, Librairie de L. Hachette et Cie, Paris.

ELVAS, M. C. PERES, I. M. & GESSNER, S. (2009). The Laboratorio Chimico of the Museum of Science, University of Lisbon: Reflections about documenting a collection. *In* M.C. LOURENÇO & A. CARNEIRO (ed), *The use of spaces, collections and archives in historical studies: Papers given at the occasion of the reopening of the Laboratorio Chimico, Museum of Science of the University of Lisbon*. MCUL, Lisbon.

FIGUIER, Louis (1873). *Les Merveilles de L'Industrie*, Furne, Jouvet et Cie, Paris.

GREENE, A. (1916). *The Elements of Refrigeration, A text Book for students, Engineers and Warehousemen*, JR. First Edition, John Wiley & Sons, New York.

HAMARD, Abbé (1890). *Glanures dans La Science*, René Hauton, Paris.

JAMIN, J. (1878). *Cours de Physique de l'École Polytechnique*, Gauthier-Villars, Paris.

LESLIE, J. (1813). *A short account of experiments and instruments depending on the relations of air to heat and moisture*, Edinburgh, William Blackwood.

MACINTIRE, H. (1928). *Handbook of Mechanical Refrigeration*, John Wiley & Sons, Inc. New York.

PIMENTEL; J. (1850.) *Lições de Chymica Geral e suas Principaes Applicações*, Casa de J. P. Lavado, Lisboa.

POUILLET, M. (1863). *Bulletin de La Société D'Encouragement pour L'Industrie Nationale*, Paris.

TELLIER, Ch. (1881). *Installation Frigorifique de La Morgue*, A. Chaix&Cie, Paris.

TISSANDIER, Gaston (1867). *Les Merveilles de L'Industrie, L'eau*, Furne, Jouvet et Cie, Paris.

VIDAL, P. & ALMEIDA C. (1893). *Curso de Physica da Escola Polytechnica, Calor*; Typographia da Academia Real das Sciencias, Lisboa.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 450.

REGULATIONS OF THE MINERAL CHEMISTRY LABORATORY OF THE POLYTECHNIC SCHOOL OF LISBON IN 1889

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Abstract

Built "with the thought of filling the lack, in Lisbon, of (...) an establishment where at least the rudiments of natural sciences could be taught", the Polytechnic School of Lisbon appears as a space where science teaching reveals specific characteristics, and laboratory practice, in spite of several difficulties, is progressively introduced as an important factor in the teaching and learning process.
The works carried out in the laboratory of 6th course –General Chemistry and Notions of its main Applications to Arts– that transformed the old laboratory in the magnificent Mineral Chemistry Laboratory, constituted a landmark in the evolution of the experimental teaching in our country.
The Director of the Laboratory, Professor José Júlio Bettencout Rodrigues wrote very detailed regulations for the work in this laboratory that we will analyze in this communication. In his "Projecto sumário de Regulamento dos Trabalhos e Serviços do Laboratório de Chimica Mineral da Escola Polytechnica de Lisboa" *in 1889/1890 he specifies the rules related to the management of the laboratory, like the opening hours timetable in different days of the year, as well as some elementary safety rules. He also details the process of assessment of students. These regulations for the practical work in the laboratory are very complete, as compared to those of other laboratories at that time. They reflect not only concerns for the good management of the laboratory, but also pedagogical concerns related to the teaching and learning of good practice of the students, and are an important conquest in science education in Portugal. The laboratorial practice is gradually an important agent in the process of teaching learning that deserves to be remembered.*

Built "with the thought of filling the lack, in Lisbon, of (...) an establishment where at least the rudiments of natural sciences could be taught"[1], the Polytechnic School of Lisbon appears as a space where science teaching reveals specific

[1] FACULDADE DE CIÊNCIAS DE LISBOA (1937). *Escola Politécnica de Lisboa. Primeiro Centenário da Escola Politécnica (1837-1937).* Lisboa. 14 vols, 2, vol.1, pp. 5-6.

characteristics, and laboratory practice, in spite of several difficulties, is progressively introduced as an important factor in the teaching and learning process.

Founded in 1837, the pathway from its beginnings until the publication of the Regulations[2] in 1889, when the practical work in the 6th Chair (Chemistry) was implemented in a systematic, although not compulsory way, was not straightforward. That is, during about half a century, there was a large gap between the will for the implementation of practical work and its effective concretization.

Portugal, like other European countries, recognized the potential of chemical knowledge for the development of the country, and strove for the implementation of a quality teaching of this discipline. Vincent and Stengers refer a "global process of professionalization of sciences that occurred during the first half of the 19th century, affecting all fields of knowledge" and "where chemistry is frequently in the frontline" because "this process is due to an *effective* and not only formal transformation of the chemical practices"[3]. They also refer that "In the formation of chemical, experimental work is recognized as a necessity. It is no longer making some spectacular demonstrations before an audience of curious, but to train students in laboratory work".

In this context of *effective* transformation, the implementation of chemistry practical work in the laboratory of the 6th Chair –"the most expensive chemistry discipline and the one with most students"[4]– is significant and deserves to be pointed out, referring the professor who undertook this transformation, José Júlio Bettencourt Rodrigues. In 1885, after several unfruitful attempts to establish practical classes, he wrote a striking document addressed to the Polytechnic School Council, where he brings to light the "critical state of chemistry teaching"[5]. Finally he succeeded in changing the situation, making clear the need for major works that were, by then, carried out in the laboratory of 6th course – *General Chemistry and Notions of its main Applications to Arts* – transforming the old laboratory in the magnificent *Mineral Chemistry Laboratory*, which constituted a landmark in the evolution of experimental teaching in our country.

The letter that A. W. Von Hoffmann wrote to José Júlio Rodrigues[6], which is transcribed below (with our underlining), in its original language, reveals the importance of the laboratory:

> « Lisbonne, août 17, 1890
> Monsieur et très cher collègue.
> Je ne puis pas quitter Lisbonne sans vous témoigner ma reconnaissance pour l'accueil aimable que je dois à vous et à M. Lourenço.
> Il m'est à la fois un plaisir et un devoir de vous dire l'impression qui a produit sur moi la visite à l'École Polytechnique de Portugal. J'ai été étonné de trouver **un établissement de premier ordre, dont tout pays aurait droit d'être fier.**
> J'admire surtout les laboratoires et l'amphithéâtre de chimie. Ayant construit les laboratoires des universités de Bohn et de Berlin, **je crois posséder quelques connaissances des institutions chimiques, et je n'hésite pas d'affirmer que je ne connais pas un laboratoire mieux installé pour l'enseignement et pour la recherche.**
> Les salles de travail et l'auditoire commandant une profusion d'espace, d'air et de lumière que je n'ai pas rencontré souvent ailleurs. Permettez en outre d'ajouter **que je ne me rappelle pas un laboratoire où on a réussi à combiner d'une manière semblable l'élégance et l'utilité.**
> Adieu, mon très cher collègue
> Monsieur le Professeur José Júlio Rodrigues
> Votre dévoué
> A. W. Von Hoffmann »

The first practical course was opened in the academic 1889-1890 and it was a remarkable achievement with an epistemological, didactical and pedagogical range. This step in chemistry teaching required new regulations for the laboratory practice.

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[2] RODRIGUES, José Júlio (1889). *Projecto summario de regulamento dos trabalhos e serviços do Laboratório de Chimica Mineral da Escola Polytechnica de Lisboa posto em execução como experiência e sob a responsabilidade do respectivo Director no anno lectivo de 1889 e 1890*. Lisboa, Imprensa nacional.

[3] BENSAUDE VINCENT, Bernadette et al. (1996). *História da química.* Instituto Piaget, pp140.

[4] RODRIGUES, José Júlio (1885). *Exposição ao Conselho da Escola Polytechnica sobre o ensino e mais serviços da 6ª cadeira, acompanhada de várias propostas tendentes a melhorarem e reformarem o ensino da Chimica Mineral*. Lisboa, Typographia Universal, pp 5.

[5] RODRIGUES, José Júlio (1885). *op. cit.* , pp. 4-8.

[6] HOFMANN, A. W. von (1891). "Carta a José Júlio Bettencourt Rodrigues" in *Estabelecimentos Scientíficos em Portugal.* O Occidente, Lisboa, vol.14 (434), 11 de Janeiro, pp 13-14.

The Director of the Laboratory, Professor José Júlio Bettencout Rodrigues wrote very detailed regulations for work in this laboratory. In his "*Projecto sumário de Regulamento dos Trabalhos e Serviços do Laboratório de Chimica Mineral da Escola Polytechnica de Lisboa*" in 1889-1890 he specifies the rules related to the management of the laboratory, like the opening hours in different days of the year, the timetable for students and workers, as well as some elementary safety rules. He also details the process of assessment of students.

These regulations for the practical work in the laboratory are very complete compared to those of other Portuguese laboratories at that time. They reflect not only concerns for the good management of the facility, but also pedagogical concerns related to the teaching and learning of good chemistry practice of students, being a landmark in science education in Portugal.

Comparing the *Regulamento* with the two previous Portuguese laboratory regulations before 1889, namely:

1. *Regulations of the Chemistry Laboratory of the Commercial and Industrial Institute of Lisbon,* written by professor António Augusto de Aguiar, dating from 1872, with 20 articles (it includes organization, laboratory work, the kind of teaching applied, the way it was performed, and to whom it was directed. It doesn't refer to security rules. It doesn't offer general information about the laboratory work. It mentions who subsidizes the laboratory and the necessary funding);
2. *Regulations of the Practical Chemistry Course of the Coimbra University,* written by Professor Corrêa Barata, dating from 1879, with 12 articles (it includes laboratory work, functions of the auxiliar personel who work in the laboratory, and also conditions for admission to the course. It refers five safety rules: the student is required to remain in the designated place for the implementation of his practical work, having no access to any other lab space without permission. The student should show, in his practice, order and cleanliness. The use of small quantities of chemicals and "fume cupboards" is recommended. The poor performance or the breach of the rules is punishable by expulsion of the course).

Indeed, the *Regulations of the Mineral Chemistry Laboratory of the Polytechnic School of Lisbon* in 1889, reflect a change from a *"teaching that previously was nothing but mere individual attempts, without a pedagogic range"*, into a *"regular course with a special application, independent teaching, budget, reliable personnel and adequate material"*[7]. This document, besides presenting a significant difference, the details of the articles submitted, informs a lot about the practical chemistry at the time.

It has 66 articles, divided into three parts:

- The *first part* consists of *19 articles* specifying the general instructions for the functioning of the laboratory of the 6th course (chemistry mineral).
- The *second part*, details the "demonstration work at the 6th course and scientific investigations that could be performed in the laboratory". It consists of *two sections*:
  - First section - Experiments of the "general course", with 1 article;
  - Second section – Scientific work not related to the regular course, with 3 articles.
- The *third part* contains instructions for the students' laboratory work in the 6th course, with a total of *43 articles* in one section.

The students, who until then only visualized experimentation during the theoretical course, started to perform laboratorial practice during the practical course. However, at the beginning, the practical course was not compulsory. The practical teaching, although having no obligatory character at first, was aimed at repeating "the experiments seen in the course"[8], that are exposed in "special tables", similar to the experimental manuals used nowadays.

The first course started in the middle of November, 1889, and finished in May, 1890, "the students had to be present in at least 20 lessons of 2 hours each, with schedule to choose between 10 and 12 o'clock or between 10 and 14 o'clock"[9].

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[7] JANEIRA, Ana Luísa (1987). *Sistemas Epistémicos e Ciências. Do Noviciado da Cotovia à Faculdade de Ciências de Lisboa*. Lisboa, Imprensa Nacional – Casa da Moeda.

[8] OSÓRIO, Balthasar (1894). "José Júlio Bettencout Rodrigues – Licção de Abertura do curso de Chimica Mineral na Escola Polytechnica de Lisboa (1893-1894)". *Revista de Educação e Ensino*, Lisboa (9), pp.1-13.

[9] RODRIGUES, José Júlio (1889). *Projecto summario de regulamento dos trabalhos e serviços do Laboratório de Chimica Mineral da Escola Polytechnica de Lisboa posto em execução como experiência e sob a responsabilidade do respectivo Director no anno lectivo de 1889 e 1890*. Lisboa, Imprensa nacional.

The maximum number of students in a class was 32 (24 in the gallery and 8 in the ground floor. The students usually stood up and worked "dressed with a special gown and bare-headed."[10]

The rules stating the management of the laboratory includes the working days in the week and along the year, the opening hours in different days of the year; the access to the laboratory; personnel in charge and their duties; service areas of the laboratory; experiments of the general course. It also details the process of assessment of students, and the regulations also define the characteristics of the general course (registration, duration and functioning of the course, recommendations on the handling of chemicals and safety rules, equipment distributed to the students for their practical work, responsibility for the equipment, assessment and classification of the students).

Students assessment depended on their "tranquillity and moderation", "cleanliness", "accuracy and quickness", "special attention" on their "way of taking notes" and how they "answered the questions made by the chief in charge or the assistant lecturer during the works and on their notebook". *The notebook* should have the following characteristics: "in quarter" format, hardcover with at least 100 pages, firmly bound. The student´s name, registration number in the general course and in the laboratorial works should be on the cover, as well as his practical work schedule, and should stay in the laboratory. The quality notebooks and products obtained in these practical works were kept and archived as a merit evidence. The regulation is detailed in the rules that students must meet during the course of practical work.

The students were requested to extinguish any gas fire "no longer needed" and to spend only the water indispensable to their experimental work, only to use the strictly necessary quantities of the reagents for the experiments.

The student did not have access to the deposits of material, chemicals, or utensils they needed, without "written or verbal request".

Some of the safety rules were related with the evaporation or manipulation of concentrated acids, corrosive or volatile liquids, chemicals with an unhealthy or unpleasant smell, namely sulphidric acid –outside the fume hoods, it was forbidden; spilling of water or chemicals on the tables or on the floor, throwing paper or other debris on the floor; pouring corrosive liquids in the bench sink; throwing thick liquids, debris, filters etc. In any other than the specific sink is forbidden; and on the tables there should be special containers for collecting residues.

The student had a list of the equipment and chemicals that he received and he had to account for at the end of each academic year. There was an audit of equipment and utensils. The first student in each group of students was the supervisor of equipment and utensils. He had the duty to inspect equipment and tools assigned to the group.

In 1901 the practical course became compulsory and its classification accounted for up to 6 points (in 20) of the final classification. The Mineral Chemistry Laboratory was restored, and opened its doors as a part of the Science Museum of the University of Lisbon in 2007.

Other references

ABREU, José Maria de (col), & VIÉGAS, António dos Santos (coord. ver e amp.) (1894). *Legislação Académica. Vol.1-1772-1850*. Coimbra, Imprensa da Universidade.
(It includes the Decree of 11 January 1837, creating the Polytechnic School of Lisboa, and may also contribute to the study of the legislative discourse of this period.)

JANEIRA, Ana Luísa (1944). *Os laboratórios de Química nas Politécnicas portuguesas. A filosofia inerente aos discursos regulamentares.* Comunicação apresentada no Simpósio Internacional Laboratórios de Química Ibéricos e Ibero - Americanos (séc. XVIII - XX): Espaços e Discursos, integrados no Colóquio Lavoisier entre Europa y América. As Ciências Químicas e Biológicas después (México D. F., Junho). Texto policopiado.

ESCOLA POLITÉCNICA DE LISBOA (1854). *"Ordens e Circulares da Escola Polytechnica. Janeiro de 1854".* Biblioteca Central da faculdade de Ciências de Lisboa, ms.

ESCOLA POLITÉCNICA DE LISBOA (1857). *"Programmas das Cadeiras da Escola Polytechnica. Anno Lectivo de 1856-57"*. Lisboa, Imprensa Nacional.

ESCOLA POLITÉCNICA DE LISBOA (1977-1878). *"Escola Polytechnica(1877-1878)".s.l., s. Ed., s.d.*

[10] RODRIGUES, José Júlio (1889). *Projecto summario de regulamento dos trabalhos e serviços do Laboratório de Chimica Mineral da Escola Polytechnica de Lisboa posto em execução como experiência e sob a responsabilidade do respectivo Director no anno lectivo de 1889 e 1890*. Lisboa, Imprensa nacional, p.10.

FORJAZ, António Pereira (194). "Achilles Machado(1862-1942)", *Revista de Química Pura e Aplicada,* Porto, sér.3, vol.17 (1-4), Janeiro-Dezembro, pp.81-91.

RODRIGUES, José Júlio (1983). *Simples apontamentos de alguns trabalhos e serviços de José Júlio Rodrigues durante 28 annos de vida publica em Portugal*. Lisboa, Typ. Academia.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 455.

"LABORATORY HANDS" ONCE MORE AND THE POLYTECHNIC SCHOOL OF LISBON (1837-1911)[1]

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Abstract

In two previous papers presented at the 6th International Conference on History of Chemistry, held in 2007 at Leuven, and 7th International Conference on History of Chemistry, held in 2009 at Sopron (Hungary) we brought up some facts regarding the identification of the chemistry preparers personality in some teaching institutions in Portugal, namely in the Polytechnic School of Lisbon. In those papers we also endeavoured to distinguish aspects of the professional evolution of the mentioned individuals.

In spite of being basically employed to assist the lecturer and relieve him of the so called minor preparatory tasks, necessary for the practical classes and investigation, the laboratory hand would, when needed, alternate between a mere laboratory assistant helper to practically an assistant lecturer. Yet, it is clear that there was a barrier that prevented the lecturer and the hand –each with a totally independent professional situation– from being mistaken one for the other.

New significant data provided by the intensive research done in documentation from the Polytechnic School archive, as well as in other exterior sources, related to the current CICTSUL project, "Scientists, laboratories and scientific instrumentation in the Polytechnic School of Lisbon" (later to become Faculty of Sciences), lead us to the necessity of delving deep into this issue. Therefore, the present work proposal approaches again the question of that specific professional area –the laboratory hand– and its role in the development of Chemistry teaching in Portugal, using the Polytechnic School of Lisbon as a case study. The objective is to outline more accurately the general points of evolution within the perspective of their work with the Chemistry lecturer, professor or investigator, and thus provide a better understanding of the historic process of Chemistry and its development in the educational institutions of the 19th century.

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Introduction

Despite the reputation of the scientific education institutions to which they were connected, Chemistry Preparers – also known as Laboratory Hands– were almost always absent from the records of its history. However, recent research about the development of chemistry teaching in Portugal has taught us more about them and how they were, or were not, a part of Chemistry tuition. This is what is intended to be addressed in this project, in the specific scenario of the Polytechnic School of Lisbon (1837-1911).

The Polytechnic School was established in January 11th, 1837 to provide scientific expertise in the training of Army and Navy soldiers and had, ever since its establishment, a subject of Chemistry named "6th Subject: General Chemistry and its main applications to the Arts". Later in 1859, another one was created: "Chemical Analysis and Organic Chemistry".

Between the moment of the creation of the School and the creation of the new Chemistry subject, a dramatic event radically changed the physical space allocated for this training project. On April 22nd, 1843, a devastating fire completely destroyed the interior of the old building of the Polytechnic School –the same building where the College of Nobles, abolished on January 4th 1837, once was. The rebuilding of the School would finally take place in 1846. Ten years later, there was a new Chemistry Laboratory at the Polytechnic School.

Before 1843, practical classes for the 6th Subject were performed in a chemistry lab: lacking a purpose built location, the kitchen of the boarding school which preceded the Polytechnic may have been –after some interventions– the interim solution in effect until 1843.

After the great fire, the Chemistry and the Physics class were transferred to the Royal Mint's Chemistry Laboratory. Some equipment that survived the fire was moved from the Polytechnic School to the Royal Mint, and located in the small houses by the Metallurgical Laboratory. In 1851, the Chemistry class was still held at the same Laboratory, headed by Júlio Máximo de Oliveira Pimentel, and with José Alexandre Rodrigues as lab preparer instead of Francisco Cardoso Leal Júnior, who had left around 1847.

With José Alexandre Rodrigues' resignation as lab preparer in 1854, it is certain that the activity of the first two chemistry lab preparers was undertaken in labs, either preceding or exterior to the historical building. It is, in a way, a pre-historical era to the Chemistry Laboratory of the Polytechnic School and its laboratory hands.

1837 to 1859: The Chemistry Laboratory within the one subject system

The development of chemistry and other issues related, such as the history of Chemistry preparers at the Polytechnic, has a primordial factor of analysis in its key subjects: the 6th Subject and Chemical Analysis and Organic Chemistry.

Until 1859, Chemistry at the Polytechnic was taught as one subject subdivided into two topics which themselves evolved with time. Initially and according to organic law, the first topic (or 1st part) constituted the elementary course and the second was reserved for the more developed areas of Chemistry. Students at the Polytechnic attended both parts of the 6th Subject or just the second one, according to the degrees they were reading. In the first section, lasting for 4 months, subjects such as cohesion, affinity, nomenclature, atomic theory and isomerism were taught; metals and metalloids and finally, organic chemistry. However, this system of separating Chemistry into an elementary course and a development course was not maintained over time, as can be seen in the programme for 1856/1857, where the 1st part is Inorganic Chemistry and the 2nd is Organic Chemistry.

But since 1840/1841 there was a programme for a practical course under the 6th Subject lecturer's remit: "practical lessons on manipulations". It is legitimate to ponder that this course was initially based on the Chemical Analysis element, present in his 6th Subject programme of 1838: "instruments and reactants –their use; analysis of gas, solid matter, alloys, acid mixtures and rock, organic and mineral water analysis".

It is still unknown how these practical lessons on manipulation worked in the chemistry lab. Later, according to the chemistry lab programme in the academic year of 1856/1857, the practical lab lessons –a given number of preparations and analysis– occurred between October and February as part of the 2nd part of the 6th Subject –Organic Chemistry.

The chemistry lab functioned under the direction of the 6th Subject lecturer. Given that as from July 1853 there was a new lecturer position specifically for Chemistry, since that date through to 1860 there were four people with direct intervention in the chemistry lab: the Director (and 6th Subject lecturer), his substitute, a preparer and an attendant.

The Chemistry Lab and the two subjects of Chemistry (1859 to 1889)

The need to coordinate education at the Polytechnic School with that of the war schools –namely that of the army– was not only a constant priority but also necessary given its position as introductory institution to the army and navy. Even if the creation of the subjects of Chemical Analysis and Organic Chemistry on the 7th of June 1859 can be regarded as a return of a civilian environment to the School, the fact is that the military impression lasted for a long time. Only in 1869 did the School Head become a civilian post undertaken by a permanent or retired lecturer, whilst previously it had been held by a General or Senior Officer of any of the Scientific Weapons.

According to the Lisbon Polytechnic School's programme, established on the 8th June 1860, the distribution of subjects across the various introductory courses, Army, Navy and for the main degree, Chemistry is only available in the latter. It is in the legal system of 1860 –which was followed for many years with only slight adjustments regarding the Army and Navy Schools– that Organic Chemistry becomes a full part of the "Chemical Analysis and Organic Chemistry" subject where it remains for over 30 years, mainly aimed at surgeons.

However, in 1896 another reorganisation of the Army School led to further changes in the Polytechnic's programme, when Organic Chemistry was established as a requirement for all candidates to Engineering, Weaponry and Civil Engineering, thus gaining a new found importance within the curriculum.

The first lecturer of this subject was Júlio Máximo de Oliveira Pimentel. The curriculum he organized for the subject of Chemical Analysis and Organic Chemistry for the academic year of 1860/1861, consisted of a first part (October to January) of quantitative and qualitative chemical analysis, and a second part (January to July) of organic chemistry. This is another fact that points to the theory that chemical manipulation topics from the 6th Subject through to the single subject system, was in fact a chemical analysis course, operating for a long period with the organic chemistry topics, perhaps because inorganic chemistry was always very extensive and didn't allow for it. Come the moment of the separation of Chemistry into two branches (organic and inorganic), the analysis topics remained attached to organic chemistry to the extent that the subject was called "Chemical Analysis and Organic Chemistry".

In April 1864, Agostinho Vicente Lourenço became the senior lecturer for this subject after Oliveira Pimentel's retirement. The programme for Organic Chemistry –apart from the generic topics– now presents a special segment dedicated to the development of its specific content. Chemical Analysis also benefits from an expansion here, with the inclusion of urine and blood analysis, toxicological experiments and spectroscopy.

During the 1860's the following group is perceived at the School: the director (a chemistry lecturer), his preparer, two lab preparers (for the 6th Subject and Organic Chemistry), and two attendants (one for Mineral Chemistry and one for Organic Chemistry).

According to Agostinho Vicente Lourenço's own statement, at the end of the 1870's the actual physical organization of the space already depicted a division between organic/inorganic subjects. However, this seems to be true only for the lecturer's work, not the students. In the early 80's the chemistry lab had two separate areas for organic and inorganic chemistry, and each respective lecturer was also director for their particular section. This situation –one lab with two sections– was maintained through the next decade despite some attempts at establishing new labs for chemical analysis and organic chemistry.

1889 to 1890: The Chemistry lab in practical context of the 6th Subject

The presentation of the educational programme of Chemistry classes at the Polytechnic would not be complete without addressing a significant moment of its existence: the creation of a practical subject for the 6th Subject, by José Julio Bettencourt Rodrigues back in 1885. At this point he presented to the School Board various proposals striving to improve the teaching of Mineral Chemistry.

In Bettencourt's understanding, the chemistry lab needed a complete overhaul. It also needed an increase in staff. He suggested a substitute Chemistry lecturer due to there being over 100 students attending the 6th Subject, and the resulting need for multiple forms. He also defended the need for a technician, besides the lecturer. This new technician was named "Assistant Chemist to the 6th Subject" –at times also referred to as the Head of Practical Work– and had as a primary objective the facilitation of the practical course (experimental chemistry) which José Julio Rodrigues wanted to establish at the School.

In the lecturer's proposal, the preparer chemist should be "an experienced practitioner, read in the best chemical practical processes, with vast lab experience and versed in chemical analysis and knowledgeable in Chemistry as a whole". It was through Roberto Duarte Silva's recommendation (at the time the professor of Analytical Chemistry at the Central School of Arts and Manufacturing in Paris) that Bettencourt Rodrigues found the right person: Charles Lepierre, recent graduate in chemical engineering from the Municipal School for Industrial Physics and Chemistry in Paris, where Roberto Duarte Silva also taught.

According to information sought from Charles Lepierre himself, the practical 6th Subject began in 1888-1889 in a lab which meanwhile was subject to major reformation. In the next academic year, 1889-1890, according to the Mineral Chemistry Lab programme which came into circulation, the practical subject was an elective subject, but with influence on the final grade, which basically worked in accordance with terms proposed back in 1885. Given Charles Lepierre's departure, the second year of the course was not achieved with the same collaborator.

During the period in which the 6th Subject practical course was active, the Chemistry lab at the Polytechnic belonged to its students, who occupied all the available space, including the galleries. It is thought that this may have been the cause for the lack of lab space for the Chemical Analysis practical course, hence motivating the search for a new lab location in the *campus*. It seems that (and delving slightly into the next topic) there was no practical course for the 6th Subject in the third year. José Julio Rodrigues was absent from the Polytechnic School, and Eduardo Burnay, recently transferred as substitute chemistry lecturer, assumed the role based on his wide experience as interim lecturer.

The last decade of the 19th century

As the last decade of the 19th century began, there was only one chemistry lab and only one director. The year 1883 opens a new chapter in the institution's history with the passing of the two Heads of both Chemistry subjects, and the consequent entrance of new lecturers in 1896: two army officials, Aquiles Machado for the 6th Subject and Tomas Cabreira for the Chemical Analysis and Organic Chemistry. Under the guidance of Aquiles Machado, the practical element gains new demands and contributes alongside assiduity to a heavier portion of the final grade. Meanwhile, a December 1893 decree appoints a new chemistry lab preparer who is believed to assume a supporting role in the tuition of the subject of Chemical Analysis and Organic Chemistry. En route to the 20th century, with no increased headcount in the Faculty and with an insignificant increase in lab staff, the School accumulates hardships in furthering practical teaching.

The search for solutions to this problem sets lab preparers –it is believed that for the first time– in a position to openly assume Faculty functions: "explaining subjects and topics exposed in the theoretical classes" within the remits of the practical class. This evolution is especially significant given the historical divide between lecturers and lab preparers: the latter were not allowed to teach, nor regarded as tutors, until then.

Chemistry Lab Preparers in the Polytechnic School (1837-1862)

The chemistry lab preparers were chemistry lab employees. In their job description, teaching was not to be found, hence they should not be regarded as candidates to, or successors of a professor.

The first lab preparers –those active between 1837 and 1861, and between Francisco Mendes Cardoso Leal Júnior, and Manuel Vicente de Jesus– may be incorporated into First Class Pharmacists, a hierarchy assumed between peers (there were two classes, although they were not contemplated by law), to distinguish between those who graduated from a Pharmacy degree and those who were eligible through work experience to sit the admission exam. Although legally both classes were eligible for admission, only First Class Pharmacists were allowed to access certain functions such as First Operator of the Lusitanian Pharmaceutical Society (certified to perform official analysis), and that of lab preparers in Higher Education.

They were, consequently, savvy chemical analysts especially with regards to water quality, chemical, fiscal and judicial matters, such as product adulteration and poisoning suspicion, as well as those with terms of Chemical relevance. These were the ones who accompanied the teaching of the 6th Subject of Chemistry at the Polytechnic, the paradigm of unique subject tuition.

But this continuity must have been disturbed with the new subject of Chemical Analysis and Organic Chemistry, since the discomfort of the Pharmacists regarding this new branch (Organic Chemistry) was evident, given that it was not a part of their own education. This was still the case in 1868, as Pharmacists claimed attendance to Chemical Analysis and Organic Chemistry from the Polytechnic or the University of Coimbra to be mandatory for admission of 2nd class candidates, and as a foundation course for eligibility for the Pharmacy degree, alongside the 6th Subject, which was already required.

Organic Chemistry and the Lab Preparers of the Polytechnic School

The moment of creation of a new Chemistry subject –"Chemical Analysis and Organic Chemistry"– and the appointment of a new senior lecturer in 1859 was critical for the institution. Agostinho Vicente Lourenço was a Chemist from Goa that had travelled to France, England, Germany and made contact with several renowned chemists abroad. Some years after the creation of this new subject, many foreign collaborators started working in the Chemistry lab. These were known as the "German lab preparers" although their nationality wasn't yet totally confirmed. It is believed that because pharmaceutical students stopped reading Chemical Analysis and Organic Chemistry, this might have influenced the recruitment in this sector and induced demand abroad due to the lack of domestic response.

From 1862/63 and for more than ten years, the presence of these collaborators is perceivable. They came to collaborate with Organic Chemistry and normally stayed in that position for a short period of time. Emílio Dias, an industrial chemist, was the only Portuguese lab preparer in this group.

Well into the second half of the 19th century however, and given the development of technical fields where Chemistry was an important subject, such as agronomy or industrial chemistry, new areas of growth were constantly revealing themselves across the nation, resulting in the increased probability of admission of a chemistry lab preparer at the Polytechnic. This was the case of César Justino de Lima Alves, named lab preparer in 1893, provisional chemistry demonstrator in 1898 and finally instituted as permanent demonstrator in 1900. As an agronomy graduate appointed according to article 99 of the 28th July 1886 decree, he was able to leave the country for two years to attend practical courses of Chemical Analysis in French and German labs at the expense of the Portuguese state. It was after his internship between 1890 and 1892 at the Agricultural Chemistry lab from the Agronomy Institute of Paris, within the Department of Agriculture and under the chemist Achilles Müntz as well as the Orth in Berlin, that Lima Alves arrived at the position of lab preparer at the Polytechnic. His exposure to chemistry practices applied to the context of agriculture must have given this agronomist special characteristics for the Chemical Analysis and Organic Chemistry course. César Justino da Lima Alves was nominated interim professor at the Superior Institute of Agronomy in 1911, becoming its Director between 1915 and 1916.

Having progressed from a lab preparer to a demonstrator, and then further to assistant lecturer, alongside with the progress and perspective the new Century had brought, this personality will bridge the gap through to the 20th century and undertake some of the most relevant developments in the role and expectations around lab preparers.

The transition into the 20th century: becoming the School of Science

Until the creation of the University of Lisbon (March 1911) and the consequent evolution into the School of Science, the subjects made available at the institution allowed for degrees of Chemistry, Inorganic Chemistry, Organic Chemistry and Chemical Analysis. This way a match was made between the institution's offer, and the demand for diversified education and related scientific grants (army and navy officials, surgeons, pharmacists, agronomists and vets) sought through the Polytechnic School over the years.

However, in 1911, with the University reform, important introductions and updates were made to the reigning programme of varied subjects of chemistry, namely the Biological and Physio-Chemistry programs which were so widely accepted. Lab preparers still remained even after the creation of the School of Science and those who qualified under the Polytechnic School were granted access to the Faculty as first assistants (César Justino da Lima Alves, nominated in 1893 as lab preparer) or second assistants (João Rocha, nominated in 1900 as lab preparer and assistant analyst of the Chemistry lab). António Dionísio Garras, Eurico Humberto Tavares Moreira, Daniel da Conceição Torres or Álvaro Pacheco de Teves, amongst others, are the new generation of "post Polytechnic School lab preparers" awaiting to be discovered and labelled.

References

ALVES, Maria Luísa (1996) – O curso prático no ano lectivo de 1889-1890. *In* Ana Luísa Janeira, Maria Elisa Maia, Pilar Pereira (ed.) – *O Laboratório de Química Mineral da Escola Politécnica de Lisboa (1884 – 1894)*. Lisboa, Livraria Escolar Editora.

COELHO, Latino (1860) – Júlio Máximo de Oliveira Pimentel. *Revista Contemporânea de Portugal e Brasil*, Tipografia da Sociedade Tipográfica Franco-Portuguesa

CRUZ, Isabel (2000) – *Química, Laboratórios de Química e valorização do ensino prático: a Escola Politécnica, a cadeira de Química e o seu laboratório (1837 – 1859)*. Lisboa, CICTSUL, private paper.

CRUZ, Isabel (2002) – Preparadores de Química da Escola Politécnica de Lisboa (1837 – 1856). *Atalaia/Intermundos*, N.º 10 – 11.

CRUZ, Isabel; LOPES, Sandra (2008) – Chemistry "Laboratory Hands" in Portugal in the Nineteenth Century: aspects of an evolution. *In* José Ramón Bertomeu-Sánchez, Duncan Thorburn Burns, Brigitte Van Tiggelen (ed.) - *Proceedings of the 6th International Conference on the History of Chemistry, Neighbours and territories; the evolving identity of Chemistry*. Louvain, Mémosciences asbl.

CUNHA, Pedro José da (1937) – *A Escola Politécnica de Lisboa; Breve notícia histórica*. Lisboa (Faculdade de Ciências de Lisboa: Primeiro Centenário da Fundação da Escola Politécnica de Lisboa, 1837 – 1937).

CUNHA, Pedro José da (1940) – D*o provimento dos lugares vagos de Lentes de Química da Escola Politécnica em 1896*. Lisboa, Academia das Ciências de Lisboa.

EÇA, Vicente M. M. C. Almeida d' (1892) – *Nota sobre os estabelecimentos de instrução naval em Portugal, principalmente sobre a Escola Naval*. Lisboa, Imprensa Nacional (Congresso Pedagógico -Português – Americano).

JANEIRA, Ana Luísa (1987) – *Sistemas Epistémicos e Ciências; do Noviciado da Cotovia à Faculdade de Ciências de Lisboa*. Lisboa, Composição e impressão da Imprensa Nacional-Casa da Moeda.

LOPES, Maria Beatriz (1948) – *O Laboratório Químico da Casa da Moeda (1801 – 1828)*. Lisboa, Escola Superior de Farmácia da Universidade de Lisboa (Comunicação apresentada ao I Congresso Luso-Espanhol de Farmácia, Madrid, 1948).

LOURENÇO, Agostinho Vicente (1877) – Laboratório e ensino químico. *In* Escola Politécnica, 1877 – 1878 (relatório apresentado na sessão de distribuição de prémios aos alunos em Dezembro de 1877).

NAVARRO, André (1956) – O Instituto Superior de Agronomia. *In* Moses Amzalak (org.) – *A Universidade Técnica de Lisboa e os seus mestres; Notas biobibliográficas*. Lisboa.

PIMENTEL, Júlio Máximo de Oliveira (1839) – *Curso de Química Elementar professado na Escola Politécnica*. Lisboa, Na Impressão de Galhardo Irmãos.

PIMENTEL, Júlio Máximo de Oliveira (1850 -1852) - *Lições de Química Geral e suas principais aplicações*, Tomo I a III. Lisboa, Em casa de J. P. Lavado.

PIMENTEL, Júlio Máximo de Oliveira (1857) – *Ano lectivo de 1856 -1857. Discurso na sessão solene da distribuição dos prémios e abertura oficial da Escola*. Lisboa, Imprensa Nacional.

Programas das Cadeiras da Escola Politécnica - Programa da 6.ª cadeira, para o ano lectivo de 1856 – 1857.

Programas das Cadeiras da Escola Politécnica - Programa da cadeira de Química Orgânica, para o ano lectivo de 1860 – 1861.

Programas das Cadeiras da Escola Politécnica - Programa da cadeira de Química Orgânica, para o ano lectivo de 1864 – 1865.

Regulamento da Escola Politécnica. Lisboa, Tip. de José Baptista Morando, 1854.

RODRIGUES, José Júlio (1885) – *Exposição ao Conselho da Escola Politécnica sobre o ensino e mais serviços da 6.ª cadeira*. Lisboa, Tipografia Universal.

RODRIGUES, José Júlio (1889) – Projecto sumário de regulamento dos trabalhos e serviços do Laboratório de Química Mineral da Escola Politécnica de Lisboa. *In* Ana Luísa Janeira, Maria Elisa Maia, Pilar Pereira (ed.) – *O Laboratório de Química Mineral da Escola Politécnica de Lisboa (1884 – 1894)*. Lisboa, Livraria Escolar Editora.

SILVA, Joaquim dos Santos e (1874) – *Elementos de Análise Química qualitativa*. Coimbra, Imprensa da Universidade.

SILVA, Joaquim dos Santos e (1883) – *Elementos de Análise Química qualitativa*. Coimbra, Imprensa da Universidade (2.ª edição).

SIMÕES, J. M. de Oliveira (1892) – *A Escola do Exército; breve notícia da sua história e da sua situação actual*. Lisboa, Imprensa Nacional (Congresso Pedagógico -Português – Americano).

Periodicals:

Anuário da Escola Politécnica; School Year 1909-1910. Lisboa, Imprensa Nacional, 1911.

Anuário da Faculdade de Ciências; School Year 1911-1912, Vol. IV. Lisboa, Imprensa Nacional, 1912.

Anuário da Faculdade de Ciências; School Year 1912-1913. Lisboa, Imprensa de Manuel Lucas Torres, 1913.

Anuário da Universidade de Lisboa; School Year 1914-1915. Lisboa, Imprensa.

Gazeta de Farmácia, 1886 and 1887.

Jornal de Farmácia e Ciências acessórias de Lisboa, 1.ª – 8.ª Séries, Tom. from 1848 to 1879.

Jornal da Sociedade de Ciências Médicas de Lisboa, from 1860 to 1868.

Jornal da Sociedade Farmacêutica de Lisboa, Tomo I, July 1835 to July 1837.

Jornal da Sociedade Farmacêutica Lusitana, Tom. from 1838 to 1863.

Faculty of Sciences Report; School Years of 1911–1912 and 1912–1913. Lisboa, Imprensa de Manuel Lucas Torres, 1913.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 462.

THE PHOTOGRAPHIC SELF-RECORDING OF NATURAL PHENOMENA IN THE NINETEENTH CENTURY

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Abstract

From the time of its discovery, photography participated in the production of evidence in many scientific fields. In the second half of the nineteenth century the quality of the photographic images as well as the discovery of easier and more reliable photographic techniques, transformed photography in a precious tool for scientists; they were now able to register in an indirect way atmospheric and magnetic phenomena.
Throughout Europe, Meteorological and Astronomical Observatories had started to be equipped with photographic self-recording instruments in order to be able to register in a continuous way temperature, pressure or atmospheric electricity variations. One of these Institutions was the Kew Observatory, considered one of the best in Europe.
By the end of the nineteenth century, the Infante D. Luiz Observatory of Lisbon, the Meteorological and Magnetic Observatory of the University of Coimbra, as well as the Meteorological and Magnetic Station of Oporto, owned photographic self-recording instruments for meteorological and magnetic purposes: barographs, psychrographs, electrographs and some magnetographs.
Portuguese scientists established privileged scientific contacts, namely with the Director of the Kew Observatory, Balfour Stewart (1828-1887) and with William Thomson (Lord Kelvin) (1824-1907), among others. In this paper we will present our research on the instruments, photographic processes and photographic data, as well as on the contributions of Portuguese scientists in this field focusing on the international cooperation between Portugal and other European countries.

Introduction

In 1839, the French physicist François Arago (1786-1853), presented to the Academy of Sciences in Paris the works of Louis Daguerre (1787-1851) and Joseph Niépce (1765-1833) relative to Photography. He considered with great forecast the contribution that photography would have for science and art. In his speech, Arago indicated the perspectives of use of this discovery as indispensable to the scientist in the areas of Astronomy, Archeology and Spectroscopy, among others[1].

Scientists quickly became aware of the capacity and importance of the photographic technique in the production of evidence in many scientific fields. In the second half of the nineteenth century the discovery of easier and more reliable photographic techniques, transformed photography in a precious tool for scientists; they were now able to register directly the image of nature captured with a photographic camera (figure 1), or in an indirect way, through the reading of data for a natural phenomenon in a photographic self-recording instrument.

Fig. 1: Stereoscopic map of Peak and great crater of Tenerife (C. Piazzi Smyth; 1858).[2]

Meteorological Observatories throughout Europe

The Royal Observatory of Greenwich in London (figure 2) played a major role in the history of astronomy and navigation, and it is best known as the location of the prime meridian. The observatory was commissioned in 1675 by King Charles II. At this time the King also created the position of Royal Astronomer. John Flamsteed (1646-1719) was the first to be appointed as director of the observatory. The earliest known measurement of magnetic declination was made by Flamsteed in 1680.

The difficult nature of magnetic and meteorological observations led to the development of automatic recording devices. Between 1846 and 1852, the English surgeon Charles Brooke (1804-1879) invented a series of self-recording instruments which used photography for the automatic registration of meteorological and magnetic data. A coal gas light-source, mirrors and optics to amplify readings and a clockwork drum covered in photographic paper to record the results were used (BROOKE; 1847; 1850; 1852). These instruments included a barometer, a thermometer, a psychrometer, and a magnetometer (figure 3)[3]. An example of a photographic record using these instruments is shown in figure 4. These type of self-recording instruments were adopted at the Royal Observatories of Kew and Greenwich, Paris, and other meteorological stations around the world.

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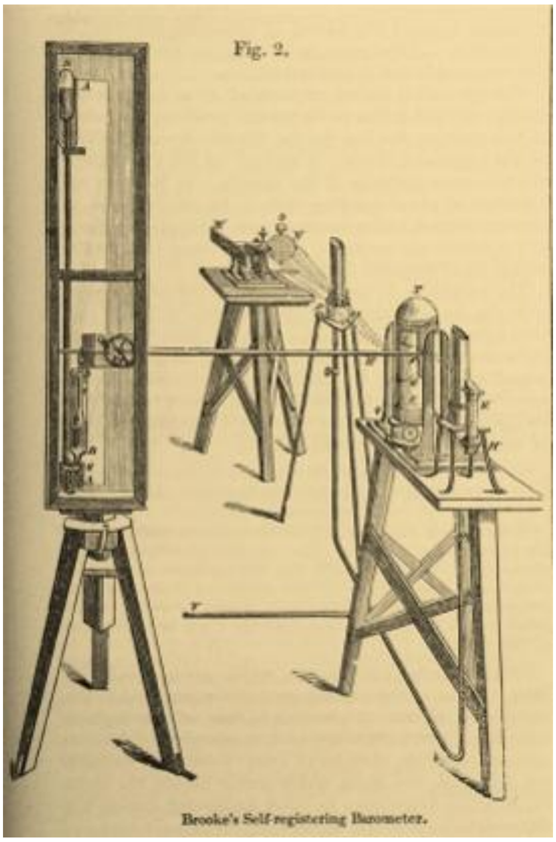
1 *"(…) À l'inspection de plusieurs des tableaux qui ont passé sous vos yeux, chacun songera à l'immense parti qu'on aurait tiré, pendant l'expédition d'Egypte. (…) les réactifs découverts par M. Daguerre hâteront les progrès d'une des sciences qui honorent le plus l'esprit humain. Avec leur secours, le physicien comparera les lumières par leurs effets (…), le même tableau donnera des empreintes du soleil, des rayons des étoiles."* (ARAGO; 1858).

2 In *Report on the Teneriffe astronomical experiment of 1856*, probably the first scientific book with stereoscopic photographs.

3 Charles Brooke's inventions were awarded by the British Government, and the jurors of the Great Exhibition in London (1851).

Fig. 2: Royal Observatory of Greenwich (MAUNDER; 1900).



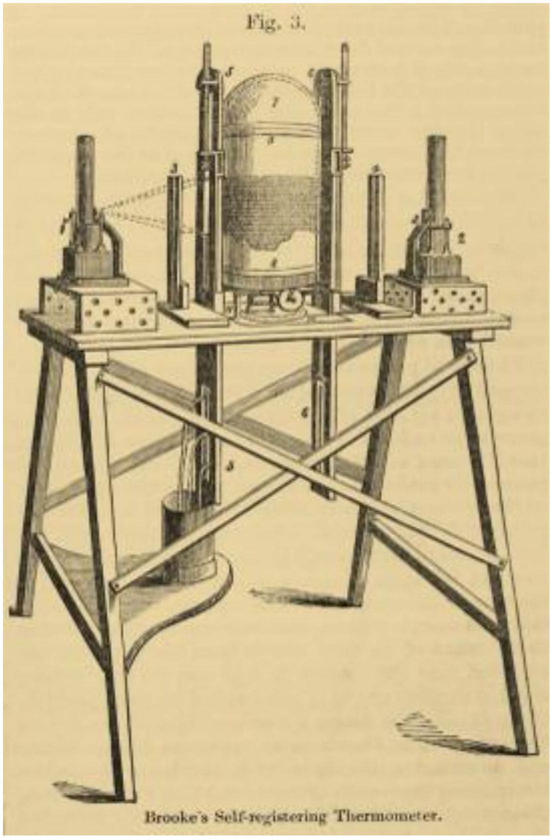


Fig. 3: Brooke's Self-recording barometer and thermometer
(GREAT EXHIBITION of the Works of Industry of All Nations London; 1851).

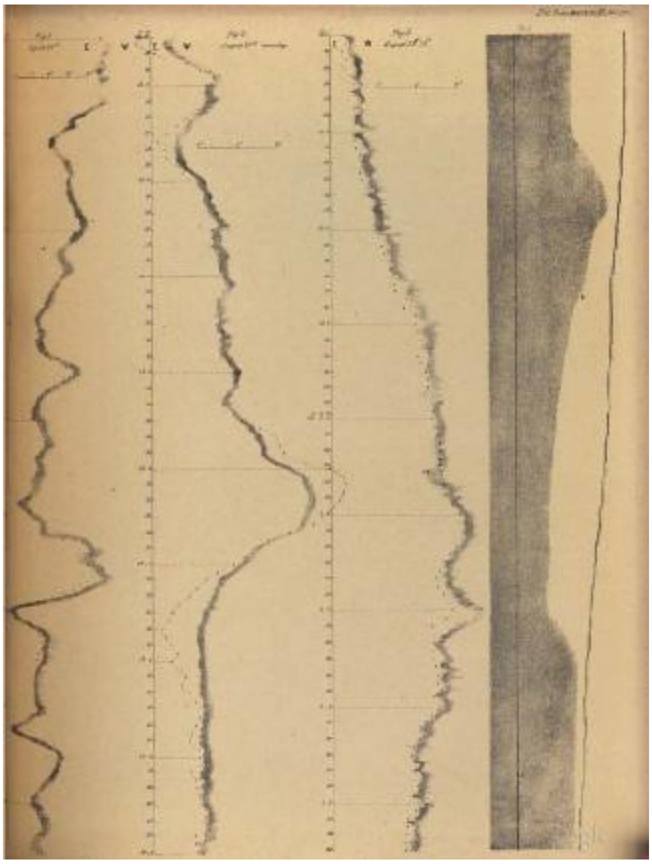


Fig. 4: Photographic records (BROOKE; 1847- plate VIII).

The Kew Observatory was an astronomical and terrestrial magnetic observatory founded by King George III in Richmond, Surrey. In 1842, the Council of the Royal Society proposed to establish, in connection with the British Association, a Physics Observatory in order to improve the knowledge of meteorology and magnetism. A Committee was created to construct a self-recording meteorological apparatus to be employed at the Kew Observatory (SCOTT; 1885).

A purchase order for a set of meteorological and magnetic instruments to be used for the self-registration of atmospheric data was granted to the meteorologist Francis Ronalds (1788-1873). In 1847 Ronalds presented to the Royal Society a description of the photographic self-recording instruments (RONALDS; 1847). These instruments (electrometer, thermometer, barometer, and declination magnetometer), were very similar to the instruments in use at the Royal Observatory of Greenwich.

In 1859 Balfour Stewart (1828-1887), a Scottish physicist, was appointed director of the Kew Observatory. He became interested in doing research on meteorology and terrestrial magnetism. These instruments were designed by J. Wesh and constructed by the maker Patrick Adie from London (STEWART; 1859). In the following year Stewart published "An Account of the Self-recording Magnetographs at present in operation at the Kew observatory" and presented a plate with its description (figure 5).

The first report (1867) of the Meteorological Committee of the Royal Society imparted a considerable impetus to the manufacture and use of these instruments (HICKS; 1886).

In 1885 several Observatories were supplied with Kew Pattern Magnetographs: Batavia, Coimbra, Lisbon, United States, St. Petersburg, Florence, Stonyhurst, Utrecht, Melbourne, Bombay, Mauritius, Vienna, Zi-Ka-Wei, San Fernando, Potsdam, Brussels, and Nice (SCOTT; 1885). The Kew Observatory was in the second half of the 19th century an important center of the internationalization of physics, and more specifically, of geomagnetism (MALAQUIAS; 2005).

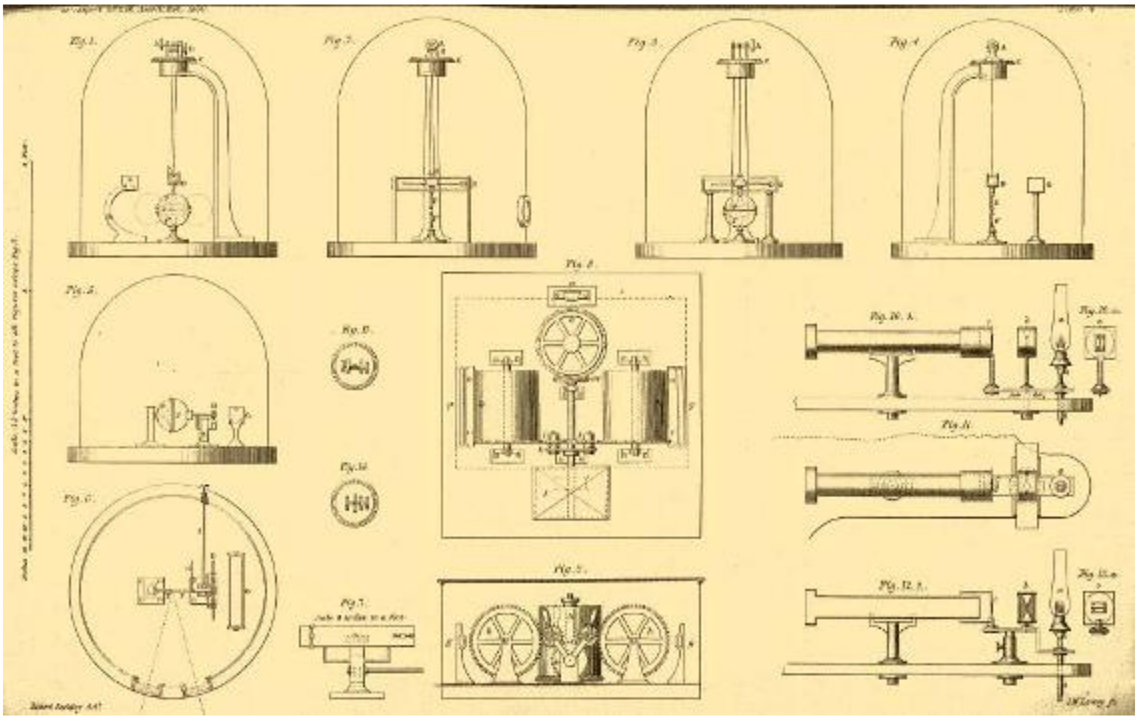


Fig. 5: Self-recording magnetographs (STEWART; 1860).[4]

Portuguese Meteorological Observatories

In the 1850s, the Lisbon Polytechnic School and the University of Coimbra submitted to the Portuguese government proposals to implement their own geomagnetic observatories and to collaborate with the already existing international network. Portugal joined the Magnetic Union in the summer of 1857.

Brito Capello (1841-1917), observer from the Observatory of Infante D. Luiz (figure 6), made a scientific trip to the Paris and London Observatories, to become acquainted with the instruments used in these institutions. Following Brito Capello's advice the Lisbon Observatory bought some instruments: magnetographs and the Thomson's electrograph from the maker Patrick Adie of London, and the baropsychrograph from the maker Jules Salleron of Paris (OBSERVATORIO Infante D. Luiz; 1862).

In 1860, with the same purpose, Jacinto António de Sousa (1818 -1880), a physicist professor from the University of Coimbra, visited Madrid, Paris, Brussels, London, Greenwich, and Kew. In a report presented to the respective University authorities he referred the reasons to adopt the Kew model for the magnetographs, a Thomson's electrograph and a baro-psychrograph from Patrick Adie. The Coimbra Observatory (figure 7) acquired these instruments in 1866 (SOUZA; 1862; 1875).

By the beginning of the 20th century (1903), the Directors of the Observatories of Lisbon, Coimbra, Oporto and Azores (respectively A. Pina Vidal, A. Santos Viega, F. Paula Azeredo and F. Afonso Chaves) decided to organize and distribute among their institutions the tasks of meteorological and magnetic determinations. Coimbra was responsible for the magnetic data and the Observatory of Oporto (figure 8) for the study of atmospheric electricity. Following this decision, a Kelvin-Mascart quadrant electrometer was bought in 1904 for this Observatory (MACHADO; 1949).

[4] Legend: Fig. 1 and 2 - the horizontal force magnetograph; fig. 3 and 4 - the vertical force magnetograph; fig. 5, 6 and 7 - the declination magnetograph; fig. 8 and 9 - the register cylinder and the clockwork and fig. 10, 11 and 12 - the gas lamp and the lens.

Fig. 6: Observatory of Infante D. Luiz (OBSERVATORIO Infante D. Luiz; 1862).

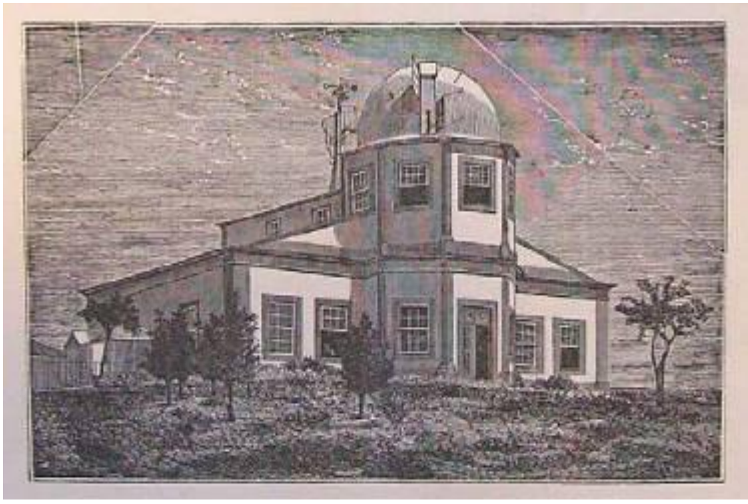


Fig. 7: Meteorological and Magnetic Observatory of Coimbra (SOUZA; 1875).

Fig. 8: Meteorological and Magnetic Observatory of Oporto (MACHADO; 1929).

Photographic self-recording instruments

William Thomson (Lord Kelvin) published for the first time a description of a divided-ring electrometer in the Memoirs of the Roman Academy of Sciences in 1857 (THOMSON; 1857). Later on, in 1865 the Portuguese periodical *Annaes do Observatório do Infante D. Luiz* reported a description of the Thomson's electrograph working method with the photographic register of atmospheric electricity variations in a continuously mode. The system for these measurements was composed by three units: one container full of water working as a collector (figure 9b) that was connected by a copper wire (A) to the electrometer (figure 9a); in its inside is suspended by a platinum wire an aluminum needle (a). This needle electrified by the Leyden bottle (b) becomes very sensitive and detects the variations of atmospheric electricity transmitted to the metallic semi circle (c); the air inside was dried with pieces of pumice stone imbibed with sulfuric acid in the lower part (h) of the electrometer unit . Above the needle is suspended a small round mirror (e) that reflects a beam of light to an adequate rotating cylinder unit, covered with a photographic paper able to register the needle deviations.

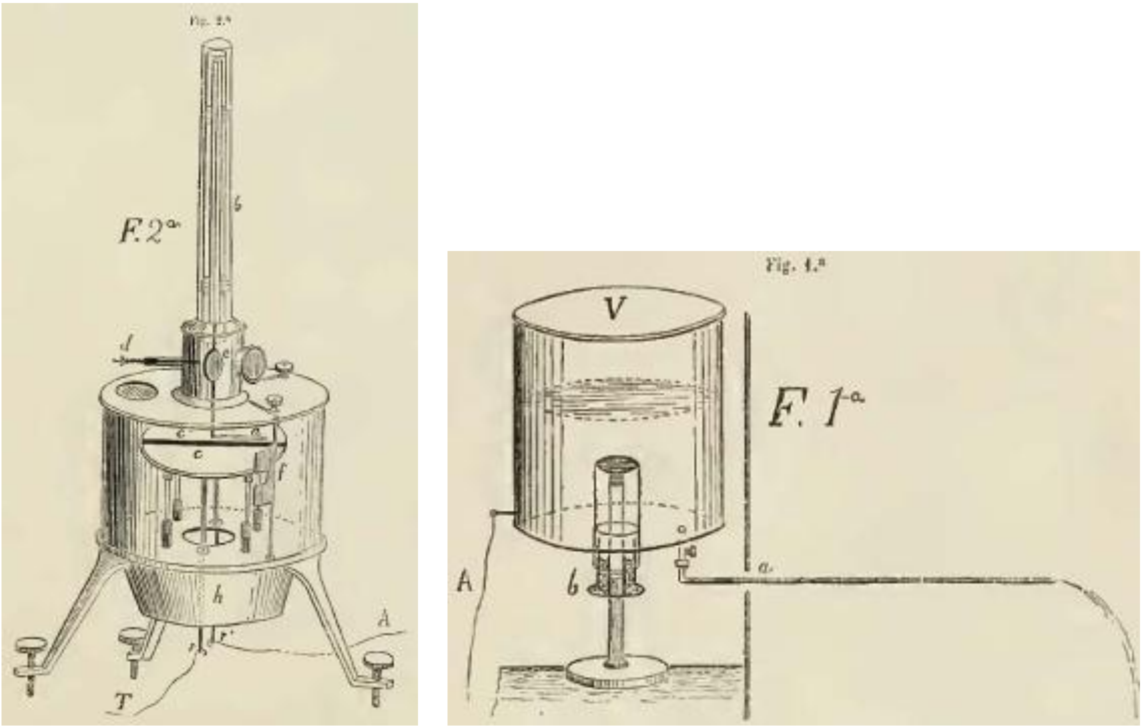


Fig. 9 a, b: Thomson's electrograph (SILVEIRA; 1865).

Brito Capello observed that sulphuric acid wasn't strong enough to dry the air and he wrote to William Thomson asking for advice on this problem. In his letter (figure 10), Lord Kelvin explains to Capello how he used the electrometer in order to get results with more accuracy for the studies of atmospheric electricity and the technique of photographic data recording (THOMSON; 1864).

Later, in 1867, Thomson presented, in a Report to the British Association of Science, an improvement to the previous design, including a light mirror which would indicate deflections of the moving body and substitution of the half rings by four quadrants[5] (figure 11).

Another instrument, Branly's electrometer, an improvement on the Thomson's electrometer was in use by 1877 in the Lisbon Observatory (OBSERVATORIO Infante D. Luiz; 1877)

[5] This instrument used the electrical force generated between charged electrodes. A butterfly-shaped electrode with two quadrants of a circular disk is supported by a torsion fiber inside a stationary circular box composed of four quadrants, with opposite pairs of which are electrically connected. The rotation of the suspended electrode depends on the potentials applied to the various electrodes.

Fig. 10: William Thomson's letter to Brito Capello: 22/12/1864 (IGIDL).

Fig. 11: Thomson's quadrant electrometer (c.a. 1867), MCUL00331 (M. Peres 2008).

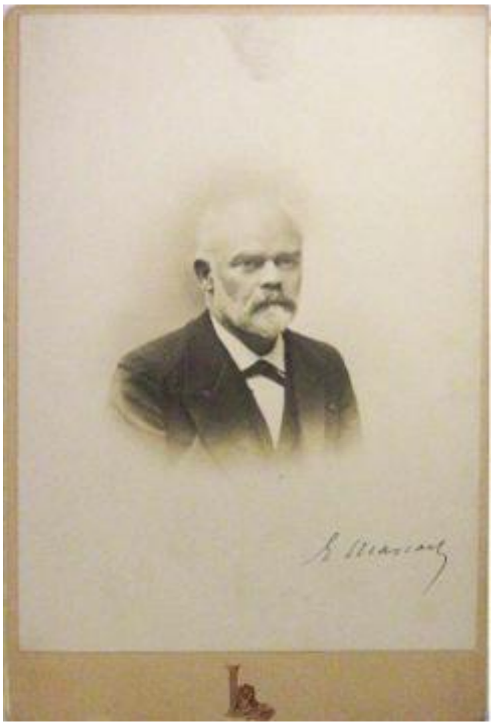


Fig. 12: Éleuthère Mascart's photo (Photographie des Grands Magasins du Louvre à Paris, c.a.1890) (IGIDL- Photographs folder).

Later on 1883 a further improvement of the Thomson's electrometer was described by the French physicist Éleuthère Élie Nicolas Mascart (1837 – 1908), in *La Nature* (MASCART; 1883). This new instrument was constructed by the French maker Carpentier.

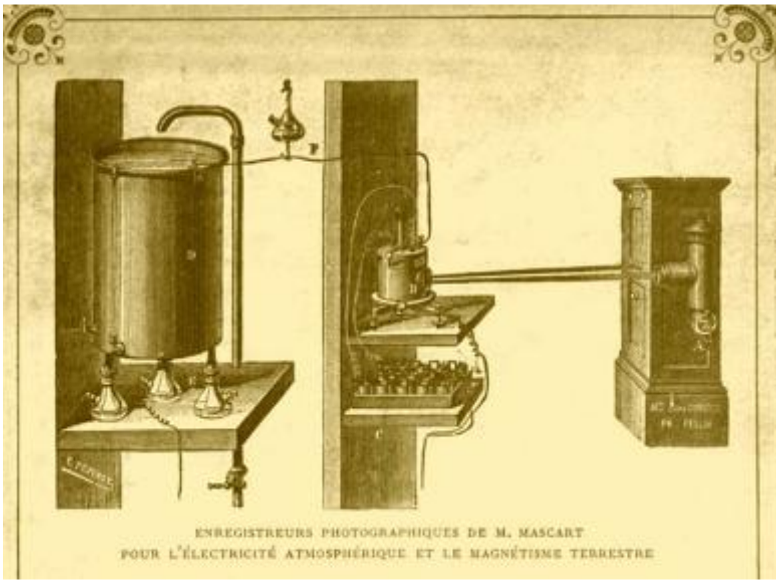


Fig. 13: Self-recording instruments for the electric potential,
adapted from Mascart (Ph. Péllin Catalogue; n.d.).

In figure 13 Mascart's self-recording electrometer is shown, comprising the collector (water drop system, isolated by Mascart's insulators), the electrometer with a Daniell's battery with ten elements, and on the far-right, the lantern with a gas lamp and the box with the photographic paper.

The Oporto Observatory in 1904 bought a Mascart's electrometer from the maker Carpentier with a Daniell's battery with a fifty elements. The photographic register and the lantern were bought from the maker Pellin (figure 14). However, the first experiments in atmospheric electricity started only in 1927.

Fig. 14: The photographic system from the Kelvin-Mascart Electrometer
(Instituto Geofísico da Universidade do Porto – photograph, courtesy of Marisa Monteiro, MCUP).

In 1864 Fradesso da Silveira, Director of the Infante D. Luiz Observatory, reported a new instrument: the baro-psychrograph (figure 15). This was a self-recording instrument that would simultaneously register pressure and temperature (readings of dry-bulb and wet-bulb thermometers) in two photographic papers, with the same lamp (SILVEIRA, 1864).

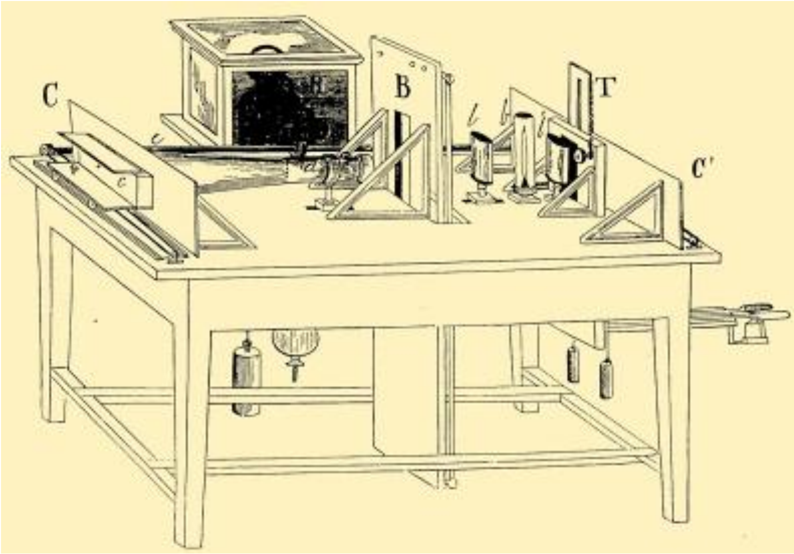


Fig. 15: Salleron's baro-psychrograph (SILVEIRA, 1864).

In Coimbra a baro-psychrograph made by Patrick Adie was also acquired in 1867. According to Jacinto de Souza, the Observatory's director, it was a unique instrument because it has resulted from scientific discussions between the maker and himself[6] (CORTE REAL; 1872).

In the Observatory Infante D. Luiz under the direction of Fradesso da Silveira the magnetographs (made by Adie) were in regular operation by the beginning of July, 1863 (figure 16).

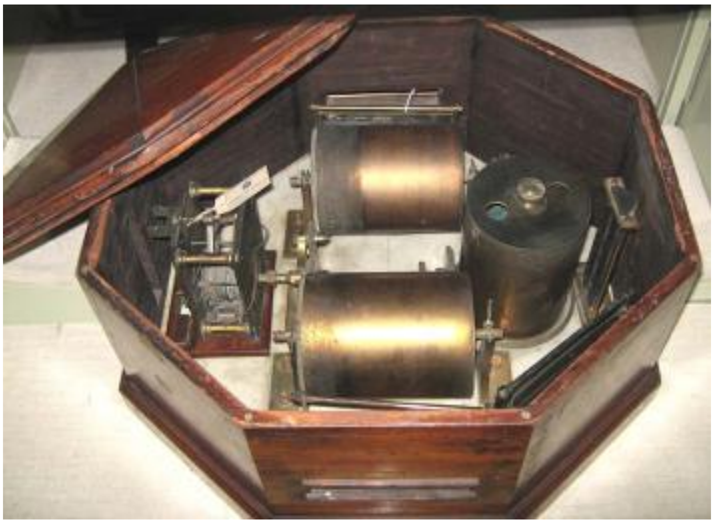


Fig. 16: Octagonal box with devices for registering the magnetographs (MCUL.00354) (Photograph by Marília Peres).

These magnetographs worked until 1902. According to Fradesso da Silveira: *"The three magnetographs were placed in the vault room on the first floor"* (figure 17) (SILVEIRA; 1864).

[6] The whereabouts of this instrument are unfortunately not known.

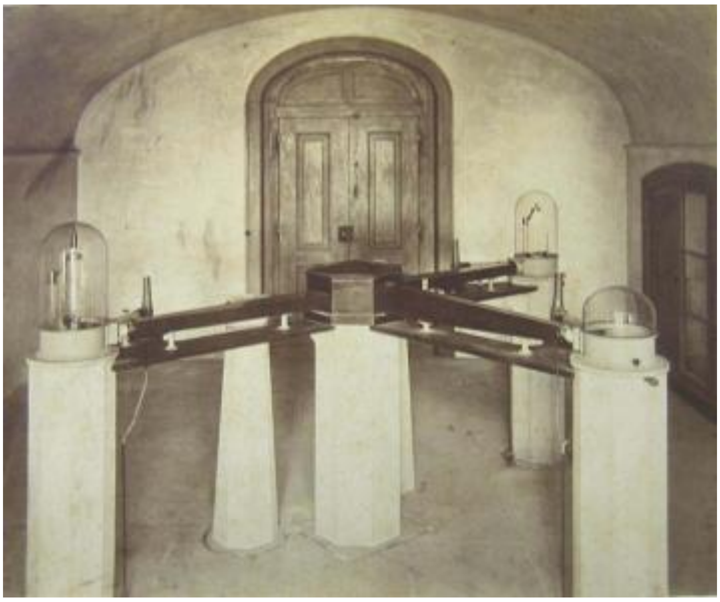


Fig. 17: The magnetographs' room (Archive of Ajuda Astronomical Observatory, Folder 723).[7]

In the context of the referred international collaboration, Brito Capello sent some copies of the Lisbon magnetic curves to Stewart, superintendent of the Kew observatory. The corresponding data (figure 18) of two observatories was discussed in a paper by Capello and Stewart published by the Royal Society (CAPELLO & STEWART; 1864).

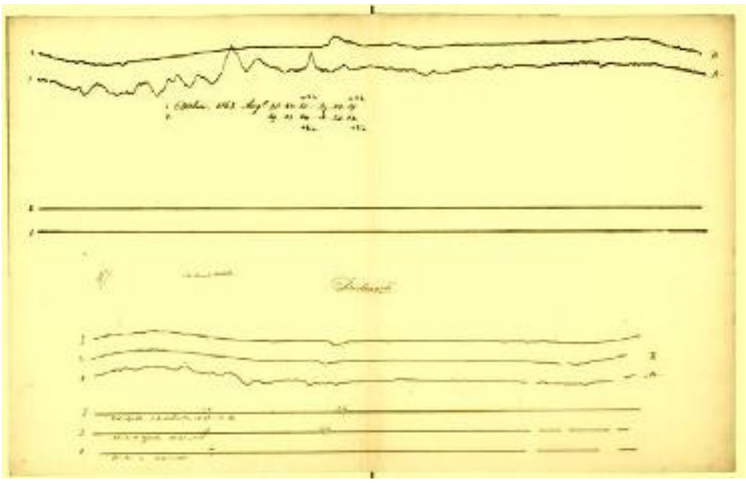


Fig. 18: Photo-lithographic Impressions of traces produced simultaneously by the magnetographs at Kew and Lisbon (Kew and Lisbon Observatories, 1864).

The first attempt to register magnetic data in the Meteorological Observatory of Coimbra was in 1867. However, some problems were reported by Jacinto de Souza: humidity, inconstancy of the gas-lamp light and the operators inexperience (Souza; 1875).

These magnetographs worked until 1930, when they were replaced by the Askania Model. The 19th-century instruments (figure 19) are now in the collection of the *Instituto Geofísico da Universidade de Coimbra*.

[7] This photograph was found in the archive of the Ajuda Observatory, in Lisbon. An identical one taken by Francisco Rocchini, entitled. "Astronomical Observatory at Ajuda", belongs to the collection of Arquivo Fotográfico da Câmara Municipal de Lisboa no. 8179" (PAVÃO; 1990).We think it represents the vault room referred by F. Silveira.

Fig. 19: The declination magnetograph; IGUC (photograph, Marília Peres).

The Geophysics Institute of the University of Coimbra holds an important collection of magnetograms from the long period of studies on geomagnetism at the Coimbra Observatory, since 1867 until today. These magnetograms are an important source for the study of solar activity. In figure 20 is shown a relevant example of one of this magnetograms; the great solar storm that took place on the 24th-25th October, 1870, is registered in this magnetogram. This photographic recording was studied in 2008 by Vaquero et al (VAQUERO; 2008).

Fig. 20: Photographic recording, IGUC (Photograph: Marília Peres).

The photographic process

The first known description for the photographic process, for self-recording instruments, was made in 1847 by Charles Brooke in the Philosophical Transactions (BROKE; 1847).

> *“ (…) to prepare by a ready process photographic paper sufficiently sensitive to receive the feeble impressions of artificial light, and at the same time sufficiently durable to retain those impressions during a period of at least twelve hours, as a more frequent attention to the apparatus would probably interfere with the ordinary arrangements of an observatory”.*

The paper was sensitized with silver bromide and a small quantity of silver iodide, and he used isinglass[8] as a catalyst in the development of the photographic image. The paper used in the photographic process was previously washed with a solution of silver nitrate. A saturated solution of Gallic acid with a very small quantity of acetic acid was used as a developing solution. The paper was then washed two or three times in water and the image was fixed with a solution of sodium thiosulphate.

For the chemist and physicist William Crookes (1832-1919) the advantage of the photographic process in self-recording instruments was in the sharpness and definition of the image which made it convenient for later consultation. However, there was a problem: the shrinkage of the paper when the chemical substances were applied. Crookes made an improvement. He used the wax-paper process invented by Gustave Le Gray (1820-1884) to prevent paper shrinkage and distortion of the image during its development in baths and washing of the chemical substances. He applied successfully the process to the barograph and the thermograph of the Radcliffe Observatory (CROOKES; 1857).

The crucial difference between the calotype and the wax paper process was in the preparation of the paper. In the calotype process the first step was the sensitization of a sheet of high quality paper with a combination of silver halides. After exposure in a camera the image was developed and fixed with sodium thiosulphate. The translucency of the paper negative could be increased by saturating with wax. This helped to increase the contrast and shorten printing times. In most respects, the preparation of wax paper negative parallels the preparation of the calotype, except for one important difference: in Le Gray's wax paper negative process, the paper was saturated with wax before the chemical sensitization. This simple reversal of one step deeply altered the quality of the photographic paper, improving its wet strength.

Finally, the most practical advantage offered by the new negative process was its impressive longevity. Because of the protective qualities of wax, a week's supply of fully prepared paper could be stored (CROOKES; 1857). This process was used by Balfour Stewart in Kew in 1859 and in the Coimbra Observatory.

To enhance stability, a gold toning bath using gold and sodium chloride was added to the positive proof, which also gave an artistic finishing to the photographic image (STEWART; 1860).

Final Notes

- The use of photographic devices for meteorological and magnetic studies has been crucial for its development.
- The photographic process had the advantage of improving the sharpness and sensitivity of the data record.
- The wax-process prevented paper shrinkage and distortion of the image during its development in baths and washing of chemical substances.
- A remarkable improvement of scientific instrumentation and data processing systems, in a short relatively period of time, was achieved by these institutions.
- European Observatories exchanged a great deal of information through an intensive use of scientific correspondence and circulation of publications, books and periodicals.

Acknowledgements:

We are most thankfully to the following institutions for their support during this research:

- Instituto Geofísico Infante D. Luís
- Instituto Geofísico da Universidade de Coimbra
- FCT – Fundação para a Ciência e Tecnologia
- Ministério da Educação de Portugal
- Museu de Ciência da Faculdade de Ciências da Universidade do Porto
- Museu de Ciência da Universidade de Lisboa
- Observatório Astronómico da Ajuda.

[8] A transparent, almost pure gelatin prepared from the air bladder of the sturgeon and certain other fishes and used as an adhesive and a clarifying agent.

References

ARAGO, F. (1858), *OEuvres Complètes*, tome IV – Notices, Paris, J. A. Barral.

BROOKE, C. (1847), On the Automatic Registration of Magnetometers, and other Meteorological Instruments by Photography, *Philosophical Transactions of the Royal Society of London*, Vol. 137, Part 1, pp. 69-77.

BROOKE, C. (1850), On the Automatic Registration of Magnetometers, and Meteorological Instruments, by Photography N. III, *Philosophical Transactions of the Royal Society of London*, Vol. 140, pp. 83-91.

CAPELLO, B. & STEWART, B. (1864), Results of a Comparison of Certain traces produces simultaneously by the Self-recording Magnetographs at Kew and at Lisbon; especially of those which record the Magnetic Disturbance of July 15, 1863*, Proceedings of the Royal society of London* 13, pp. 111-122.

CAPPELO, B. (1868), A Comparison of the Kew and Lisbon Magnetic Curves during the Magnetic Storm of February 20-25, 1866, *Proceedings of Royal Society*, Vol. 16, pp. 399-403.

CORTE REAL, J. et al (1872), *Viagem dos Imperadores do Brasil em Portugal*, Coimbra, Imprensa da Universidade.

CROOKES, W. (1857), *A Hand Book to the Waxed Paper Process in Photography*. London, Chapman and Hall Piccadilly.

FIGUIER, L. (n.d.), *Les Merveilles de la Science*, Paris, Jouvet et Cie Editeurs.

HICKS, J. (1885), *Hicks' Catalgue of Meteorololigical and Scientific Instruments*, London, James Hicks.

GREAT EXHIBITION of the Works of Industry of All Nations London, (1851), *Great Exhibition: Official, Descriptive and Illustrated Catalogue. Class X. Philosophical, Musical, Horological and Surgical Instruments*, London, Spicer brothers.

MACHADO, A. (1929), *Observatório da Serra do Pilar: Breves notas históricas. Estado actual. Desenvolvimento*, Porto, Publicações do Observatório da Serra do Pilar.

MALAQUIAS, I., et al (2005), The Genesis of Geomagnetic Observatories In Portugal, *Earth Sciences History*, 24(1), pp. 113-126.

MASCART, E. (1883), L' Électromètre enregistreur, *La Nature*, Vol. II, pp. 51–54.

MAUNDER, E. (1900), *The Royal Observatory Greenwich: a glance at its history and work*, London, The Religious Tract Society.

METEOROLOGICAL Committee of the Royal Society (1868), A Description of the Self-recording Instruments Recently Erected by the M.C.R.S., *Report of the Meteorological Committee of the R. S. for the year ending 31st December 1867*, pp. 27-54.

PAVÃO, L. (1990), The *photographers of Lisbon, Portugal from 1886 to 1914*, Rochester, George Eastman House.

RONALDS, F. (1847), On the Photographic Self-registering Meteorological and Magnetical Instruments, *Philosophical Transactions of the Royal Society of London*, Vol. 137, Part 1, pp. 111-117.

SCOTT, R. (1885), The History of the Kew Observatory, *Proceedings of the Royal Society of London*, Vol. XXXIX.

SILVEIRA, F. (1864), *Relatorio do Observatorio Infante D. Luiz no Anno Meteorologico 1863*–*1864*, Lisbon, Typographia Franco-Portuguesa.

SILVEIRA, F. (1864), O Baropsychrographo do Observatório do Infante D. Luiz, *Annaes do Observatório do Infante D. Luiz: 1864, decimo ano*, Lisboa, Imprensa Nacional.

SILVEIRA, F. (1865), O electrographo de Thomson, *Annaes do Observatório do Infante D. Luiz: 1865, decimo ano*, Lisboa, Imprensa Nacional.

SMYTH, C. (1858), *Report on the Teneriffe Astronomical Experiment of 1856*, London and Edinburgh, Richard Taylor and William Francis.

SOUZA, J. (1862), *Relatório da Visita aos Estabelecimentos Scientificos*, Coimbra, Imprensa da Universidade.

SOUZA, J. (1875), *Observações Meteorologicas e Magneticas feitas no Observatorio Meteorologico de Coimbra*, Coimbra, Imprensa Commercial e Industrial.

STEWART, B. (1860), An Account of the Construction of the Self-Recording Magnetographs at Present in Operation at the Kew Observatory of the British Association, *Report of the B. Ass. Adv. Sc. for 1859*, pp. 200-228.

THOMSON, W. (1857), *Math. and Phys. Papers,* vol. 2, p. 154

THOMSON, W. (1864), *William Thomson's letter to Brito Capello: 22/12/1864*, Folder correspondence - 1864, IGIDL.

TRUTAT, E. (1893), De l'Enregistrement Par la Photographie des Phénomènes Naturels, *Conférences Publiques sur la Photographie Organisées en 1891-1892, pour le directeur de l'Établissement*, Paris, Gauthier-Villars et Fils.

VAQUERO et al (2008), The 1870 space weather event: Geomagnetic and Auroral Records*, J. Geophys*. Res. 113 (A08230, doi: 10.1029/2007JA012943.), V. 16.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 477.

THE INFLUENCE OF FOREIGN SCIENTIFIC IDEAS ABOUT RACE AND MISCEGENATION ON BRAZILIAN SCIENCE AT THE END OF THE 19TH CENTURY

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Abstract

This paper attempts to provide a general overview about the way in which foreign scientists influenced Brazilian medical and anthropological discourses about race and miscegenation during the second half of the 19th century and the beginning of the 20th century, it is, coinciding with the introduction of evolutionism in Brazil.

The perils of miscegenation: the influence of foreign science on Brazilian racial thought

Throughout the nineteenth century, whenever Brazilian doctors or naturalists approached the study of “human races” and, therefore, the apparent problem of racial mixture, they did so looking for scientific models previously produced in Europe or the United States. The influence of those foreign analyses on Brazilian scientific discourse on races was imponderable. Emperor D. Pedro II would look continuously for European or American experts in order to lead and organize many of the first scientific institutions in the country: «A French team composed by Emmanuel Liais, Louis Cruls and Henrique Morizo was chosen to direct the *Imperial Observatory*, (...);Claude Henri Gorceix, a French as well, created the *School of Mines of Ouro Preto*, Hermann von Ihering, German, worked as a prominent naturalist in the *National Museum* and was the organizer of the *Museu Paulista*; Emil Goeldi, Swiss, founded the museum that has his name in Pará, Charles F. Hartt and Orville Derby, (...) Americans, took part in the formation of the *Geological Commission of the Empire* (later named *Mineralogical and Geological Survey*) and the *Geological Commission of São Paulo*, Franz W. Dafert, Austrian, started agricultural research in Brazil, with the *Agronomic Institute of Campinas*... »[1].

[1] SCWARTZMAN, S. (1990), Os estrangeiros na ciência brasileira, *Ciência Hoje*, vol. 11, 62, p. 12

The foreign presence in the Brazilian scientific milieu was so significant that it would even take a complete control over some of the best national scientific institutions. For example, out of the total number of scientific articles published by the *Revista del Museu Paulista* –the Journal of the São Paolo Museum, led by the German Hermann von Ihering– , only a tiny part was produced by Brazilian researchers. The rest was signed exclusively by European naturalists, a good number of whom where travellers, often hired temporarily to collect and analyze different samples of scientific interest. In a similar way, considering the *National Museum of Rio de Janeiro* –where, in 1877, Dr. Lacerda would inaugurate the first Anthropology course in the country–, out of the 44 members of the editorial board of its official journal, the *Archivos do Museu Nacional*, there were just three Brazilians[2].

This clear dominance of foreigners in the Brazilian scientific institutions was noticeable at all scientific levels. However, the influence of foreign scientific doctrines and theories was particularly sensitive when the discussion pointed to the natural status of the different human races present in the Brazilian society. In other words:

> «It is impossible to deny the enormous influence of European racial theories among the Brazilian intellectuals of late nineteenth century. The national intelligentsia expressed a systematic concern with the multi-ethnic origin of the Brazilian people, which was perceived as a source of social contradictions and as a potential barrier to the construction of an authentic national identity. 19th century Brazilian society –with deep historical roots based on slavery and a system of class domination overlapped with the racial hierarchies– constituted a perfect target for such theoretical influences»[3].

Brazilian Doctors, anthropologists, and natural scientists would readapt those scientific proposals coming from abroad, often giving them an original Brazilian character, as in the example of Raimundo Nina Rodrigues. As Nina Rodrigues pointed out in his pioneer work on the linkages between miscegenation, degeneration and crime in Brazil[4], some of the best naturalists and anthropologists of the most "progressive" and "civilized" nations of Europe and North-America had already studied the problem of racial fusion of the colonies before any Brazilian had tried to attempt that task (in fact, Nina himself was the first Brazilian to do so extensively). Moreover, as Nina explained, many of those foreign scientists had written about Brazil, as a giant natural laboratory for the experimental study of the social and biological effects of miscegenation. Therefore, it was worthy to learn carefully from those studies.

Neutral or favourable descriptions of Brazilian miscegenation were very rare among the international scientific community during the second half of the 19th century. In any case, even if they were taken into account –as in the case of Quatrefages, whose ideas on racial mixture were quoted by Nina Rodrigues–, the influence of these indecisive, if not contradictory, defenders of racial mixture was certainly incomparable with the influence of the defenders of the opposite point of view, for whom Brazilian miscegenation was estimated as a source of decadence and degeneration. Among this last group, a prominent place must be assigned to Count Arthur de Gobineau, who had lived in Rio de Janeiro as the French ambassador between 1869 and 1870, and eventually became a personal friend of Emperor Pedro II. Gobineau's influence in Brazilian scientific circles would be remarkable for decades, and even authors whose work was developed many years later, such as Nina Rodrigues himself, would keep using some of the ideas defended by the French Count as one respectable source for research on racial degeneration, recalling the Count's dramatic description of the «decline observed in the South-american mestiços»[5].

During his stay in Rio, the famous author of the Essay on the Inequality of the Human Races (1853-1855)[6], reported how, with moral and aesthetic horror, he had been a privileged witness of the racial peculiarities of Brazil: «no Brazilian has pure blood; all possible combinations of marriages between whites, blacks and Indians have proliferated to such an extent that the nuances in the colour of the skin are endless, and this has resulted –both in the lower as in the upper classes– in the saddest of degenerations»[7].

According to the French diplomat, the deleterious effects of Brazilian miscegenation were so patent that, following his calculations, decay caused by interracial crosses would «inevitably condemn the degenerate mestiços to disappearance (...)

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[2] Ibid., p. 71; ANDERMANN, J. (2008), The Museu Nacional and its European employees, www.bbk.ac.uk/ibamuseum/texts/Andermann01E.htm, (28-4-2008).

[3] PETRUCCELLI, J.L. (1996), Doutrinas francesas e o pensamento racial brasileiro, 1870-1930, *Estudos Sociedade e Agricultura*, 7, dezembro, pp. 134-149.

[4] NINA RODRIGUES, R. (1899), *Métissage, dégénérescence et crime,* Lyon, A. Stock & Cie, pp. 3-4.

[5] NINA RODRIGUES, R. (1899), p. 4.

[6] GOBINEAU, A. de (1967), *Essai sur l'Inégalité des Races Humaines* [1ª ed. 1853-1855], París, Pierre Belfond.

[7] GOBINEAU, A. de (1869), Letter to Caroline de Gobineau, April 19th, 1869. Quoted in RAEDERS, G. (1988), *O inimigo cordial do Brasil: o conde de Gobineau no Brasil,* Río de Janeiro, Paz e Terra, p. 90.

in about two hundred and seventy years»[8]. It was a very similar opinion to that maintained by another illustrious French visitor of Brasil, the eminent botanist and naturalist Auguste Saint-Hilaire, who, some years before Gobineau –during the course of his travels along the Brazilian coast and the diamond territories of the inner-lands– could observe «a horrible population, formed from the mixture of oppressed and oppressors»[9]. In his view, Brazilian Indigenous peoples constituted only «the sad remnants of a civilization that will quickly fade away, accompanied by the disappearance of the unhappy race to which it belongs»[10]. Tristes Tropiques …

No more flattering that the previous prognostications, the future of Brazilian people resulted equally dark for one of the greatest naturalists of the century, Louis Agassiz –who visited the country between 1865 and 1866. In his travel notes, he described the devastating effect that, on his mind, racial mixture had caused to the nation: «Those who doubt about the pernicious effects of racial mixing and, led by a false philanthropy, try to deny all the natural barriers existing between the human races, should come to Brazil. Here, better than anywhere else, it is not possible to deny the racial decline resulting from the crosses. They could not deny that miscegenation shuts down the best qualities of each races: in whites as in blacks or Indians, racial mixture produces an indescribable kind of hybrid, of poor physical and mental energy»[11].

Agassiz, as it is well known, used anti-evolutionary and creationist arguments to explain the natural differences between the human races –considered by him as different zoological species–, as well as the harmful effects of interracial crossing between them. But in Brazil, a country where –unlike in other Latin American states– the theories of biological evolution were accepted in the main national scientific institutions without much tension[12], these kinds of old fashioned anthropological arguments would rapidly be forgotten and replaced by more modern evolutionary arguments about the natural origin of the racial divergences. In the following section, we'll consider the impact left in the Brazilian scientific milieu by another group of scientists who, in the last part of the century, had already assumed the modern evolutionary theories, and were applying them to the study of the natural differences between human races.

Evolutionism, polygenism and the racial debate in Brazil

In the last decades of the 19th century, racial theories inspired in the Darwinian vision of human evolution –a biological struggle for survival between human groups, according to a scale of evolutionary perfection culminating in the "favoured races" and, especially, in the white man[13]– received an enthusiastic reception in Brazil. This was especially the case of different polygenist anthropological models of human evolution that defended –as previously had been done by Morton, Agassiz, and others in non evolutionary terms– that the major racial groups had evolved as completely different species, from a remote common ancestor. As it has been stated by Thomas Glick, a good part of the Brazilian elites –among which the Emperor himself, Don Pedro II, should be included in a prominent place[14]–, would receive this evolutionary racial theories with open arms, even before the advent of the Republic and the abolition of slavery. Paradoxical as it may seem – given the close connections that, traditionally, linked those Brazilian ruling classes with Catholicism–, many «members of the Catholic elite were prompt to accept polygenic evolutionary models of human evolution as a scientific justification for the maintenance of white supremacy in the social realm. Therefore, in spite of being Catholic, those elites could be reinforced by accepting the polygenic models of human evolution»[15].

Moreover, during the last decades of the 19th century, a good portion of the Brazilian scientific institutions that tried to make diagnostics and/or give potential solutions to the "problem" of miscegenation –such as Natural History Museums, Ethnographic Museums, Colleges...– were created and oriented under clear polygenist principles. As it is well known, polygenist models of human evolution –mostly developed in Europe and America during the late nineteenth and early twentieth centuries– defended that the human races were actually different species, evolved from a common ancestor. The

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[8] Ibid., p. 62.

[9]SAINT-HILAIRE, A. de (1941), *Viagens pelo distrito dos diamantes e litoral do Brasil,* São Paulo, Companhia editora nacional, p. 297

[10] Ibid., p. 296.

[11] AGASSIZ, L. and AGASSIZ, E.C. (1938), *Viagem ao Brasil* (1865-1866), São Paulo, Companhia editora nacional, p. 366, note 143.

[12] BERTOL, H., ROMERO, M. and GLICK, T. (2003), *A Recepção do Darwinismo no Brasil*, Rio de Janeiro, Ed. FIOCRUZ.

[13] DARWIN (1871), *The descent of man, and selection in relation to sex,* 2 vols., Londres, John Murray, 1. See especially, On the Extinction of the Races of Man, p. 236-239; HALLER, JOHN S, jr. (1995), *Outcasts from evolution. Scientific attitudes of racial inferiority 1859-1900,* Southern Illinois Univ. Press; STOCKING, GEORGE W. Jr. (1982), *Race, culture, and evolution. Essays in the history of anthropology,* Univ. Chicago Press; SÁNCHEZ ARTEAGA (2008), *La razón salvaje. Tecnociencia, racismo y racionalidad.* Madrid, Lengua de Trapo.

[14] The emperor Pedro II was a notorious admirer and financial promoter of different scientific projects, both in Brazil as in other countries. A member of the Paris Academy of Sciences (which only counted with 8 foreign Academics), he even got to give economic support to some of Louis Pasteur researches.

[15] BERTOL, H., ROMERO, M. and GLICK, T. (2003), *A Recepção do Darwinismo no Brasil*, Rio de Janeiro, Ed. FIOCRUZ, p. 23.

product of their mixture –the mulatto, or the *mestiço*–, although fertile, was defined for the poligenists as a real "hybrid". Some of the best physical anthropologist of the time, such as the French Paul Broca, defended that, notwithstanding their inter-fertility, those racial hybrids were often defective or inferior to the parental "pure" lines from a biological point of view[16]. In this sense, racial miscegenation could only constitute a process leading to both social and biological degeneration.

Assuming these same principles, some of the most prestigious Brazilian doctors and anthropologists of end of the century, often described the anatomy or physiology of these "hybrids" as "atavic". In this way, many scientists and academicians believed that the Brazilian population –which was essentially *mestiça*– was characterized by a whole range of abnormal features and stigmata, originated through undesirable racial crossings. A good part of the white learned elites considered that this supposed racial degeneration that affected the country had been caused by miscegenation. Nina Rodrigues, as many of his disciples, admitted that only the "upper grades of the miscegenation scale" –in which clearly dominated the Caucasian component–, could reach the same capacity for intellectual progress and "civilization" as pure whites. In all the other cases, the *Mestiço* constituted a kind of human sub-product, especially prone to biological and mental degradation. They could not be treated as the same kind of citizens –both at a legal and at an intellectual level–; on the contrary, they should be considered and judged under different standards since, considering numerous parameters, those hybrids were closer to a child than to an pure Caucasian adult. In this sense, one of the first books written by Nina was devoted to defend, from a medical point of view, the need for separate penal codes for whites, blacks and *mestiços*[17].

Since the last decade of the nineteenth century, the "problem of miscegenation" would always be considered from an evolutionary point of view by the Brazilian natural scientists. In order to understand the Brazilian reception of Darwinism and its applications to the racial question, it should be remembered that Darwin himself had visited different parts of Brazil, and commented the effects of the Brazilian racial mixture as he had observed during his voyage aboard the Beagle. In some of his later works, –and despite his definite belief in the unity of the human species[18], and his own personal belief in the existence of «many excellent and kind-hearted mulattost»[19]– Darwin admitted that «when two races low in the scale are crossed, the progeny seems to be eminently bad. Thus the noble-hearted Humboldt, who felt none of that prejudice against the inferior races now so current in England, speaks in strong terms of the bad and savage disposition of *Zambos*, or half-castes between Indians and Negroes; and this conclusion has been arrived at by various observers. From these facts we may perhaps infer that the degraded state of so many half-castes is in part due to reversion to a primitive and savage condition, induced by the act of crossing, as well as to the unfavourable moral conditions under which they generally exist»[20]. Darwin also recognized that, during his stay in South America, « many years ago, long before I had thought of the present subject, I was struck with the fact that, in South America, men of complicated descent between Negroes, Indians, and Spaniards, seldom had, whatever the cause might be, a good expression»[21].

But, in addition to Darwin, this same theoretical framework, in which miscegenation was contemplated as a potential source of evolutionary degeneration, was taken as an absolute a priori by some of the most pioneering evolutionary schools of anthropological thought of the end of the 19th century. Such was the case of the French Anthropological Society, led by Paul Broca, whose theories about human hybridization between different human species were published in 1860 –it is, prior to Darwin's writings on the subject– with the suggestive polygenist title of «On the phenomenom of Hybridity in the *Genus* Homo»[22]. Perhaps because of a simple question of linguistic closeness, the newest advances and theories of foreign science came to Brazil mainly through French publications (even though, in Brazilian scientific libraries, such as in the Medical School's Library in Salvador de Bahia, there could also be found scientific books in Italian, in English and, to a lesser extent, in German). For that reason, the French anthropological school (which included some of the most notorious polygenist thinkers of the time, such as Paul Topinard, Clémence Roger, or Gustave Le Bon), had a decisive influence among Brazilian physicians interested in the study of the "human races", as in the so-called "problem of degeneration" caused by racial mixture.

Nina Rodrigues' "medical school" in Bahia, in particular, resulted decisively influenced by the French school of physical anthropology. Some of Nina's works were responsible for the dissemination in Brazil of French poligenist discourse

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[16] BROCA, PAUL (1877), Recherches sur l'hibridité animale en géneral et sur l'hybridité humanine en particulier considerées dans leur rapports avec la question de la pluralité des espèces humaines, in *Memoires d'anthropologie,* París, Reiwald, t. III.

[17] NINA RODRIGUES, R. (1938), *As raças humanas e a Responsabilidade Penal no Brasil,* São Paulo, Editora Nacional, pp 161, 169, 215-217.

[18] DESMOND, ADRIAN AND MORE, JAMES (2009), *Darwin's sacred cause. Race, slavery, and the quest for human origins.* London, Allen Lane, Penguin Books.

[19] DARWIN, C. (1868), *The variation of animals and plants under domestication*, London, John Murray, vol. 2, p. 46

[20] DARWIN, C. (1868), *The variation of animals and plants under domestication*, London, John Murray, vol. 2, p. 46-47.

[21] Ibid.

[22] BROCA, P. (1864), *On the phenomenom of Hybridity in the Genus Homo*, London, Longman. English translation from the French original. BROCA, P. (1860), *Recherches sur l'hibridité animale en géneral et sur l'hybridité humanine en particulier considerées dans leur rapports avec la question de la pluralité des espèces humaines*, Paris, Reiwald, Claye.

about racial mixing. For example, the main theses of Nina's *Métissage, dégénérescence et crime*, that is, considering «miscegenation as a degenerative influence in the social aetiology of crime»[23], had been previously exposed –as Nina himself recognized–, in French by Darwin's translator Clémence Roger. Nina also used Gustave Le Bon famous studies on "the psychological laws of the evolution of peoples" as an authoritative anthropological source of information. There, he read and learned about «South American *mestiço* populations as being the incontestable proof of the disastrous social effect of racial mixture»[24].

But French racial thought also influenced some other leading Brazilian evolutionists working outside Bahia. The Pará Museum of Natural History was led by the Swiss naturalist Emil Goeldi, who had learnt about poligenism from Haeckel himself, since the German scientist –who defended the existence of 12 different human species– had been his scientific mentor in Germany. In São Paulo, as well, polygenist evolutionary discourse was introduced by some of the leaders of major scientific centres, such as the *Museu Paulista*, directed by the German Hermann von Ihering. His views about the nature of Brazilian populations can be easily understood through his own words: «degeneration, as frequently observed in the hybrids of the animal kingdom, is easily seen among the crossed products of the different human groups»[25]. Von Ihering himself somehow seemed to legitimize, in biological terms, the extermination of a group of Indians Kaingang who, reluctant to abandon their own lands, had entered in war by the white settlers. Von Ihering –defending the need for the construction of a railway that, starting from Sao Paulo, passed through the Kaingang territory– had invoked evolutionary principles to defend the expulsion of the natives on the bases of their evolutionary inferiority in the struggle for life. According to him, what actually condemned the «inferior indigenous groups» to extinction was nothing else than «nature herself», against whom it would be childish to invoke any kind of «humanist moral principles»[26].

Similarly, the Polytechnic School in Rio de Janeiro was directed by the French evolutionism defender Louis Couty, who, notwithstanding being contrary to slavery, expressed a strong belief in the intellectual and biological superiority of the Caucasian. Couty thought that blacks were actually inferior to Europeans by their biological and intellectual nature. But at the same time he also defended that their natural inferiority didn't imply the superimposition of slavery, as an unjust social penalty over those races who had been previously been "punished by nature". Couty believed in an innate natural "black laziness" that was, «in part, responsible for the backwardness of the country: even when they [black people] recovered freedom with emancipation, they did nothing to work or cultivate land or for social progress». According to him, despite slavery constituted an abhorrent institution in moral terms, it had functioned as a practical protection at the social level for the blacks, since, during that period «they were well fed, cared and protected against old age or Unemployment»[27]. Perhaps the greatest of all evolutionary naturalists living in Brazil at the period, the German Fritz Müller, showed total confidence that German immigration soon will help to make the Aryan race become the dominant racial element in the south of the country. German blood will soon overpass «the Latin decadent element», which, according to him, contributed to much of the backwardness of Brazilian populations.

Meanwhile, the two main scientific directors of the National Museum of Rio de Janeiro during the last decades of the nineteenth century, Ladislau Netto –who led the museum until the nineties– and João Batista de Lacerda –his immediate successor–, will support an extremely racist and polygenist biological anthropology. Deep admirers of the French School of physical anthropology developed by Broca and Topinard, since the 1870's they started a series of anthropometric research on the physical characteristics of the indigenous breeds of Brazil, publishing their results in the museum Journal, the *Arquivos do Museu National*. Also inspired by the work of the North-American polygenist Samuel Morton –who, decades earlier, had used his cranial metric data to defend that "human races" were actually completely independent species[28]– they began to form a collection of Brazilian skulls in the museum.

Both for Lacerda as for Netto, it was unquestionable that Aboriginal Brazilians came from a different evolutionary lineage than Europeans. In order to show this evolutionary diversity, they organized a great anthropological exhibition which counted, as the main attraction, with living Brazilian Indians, exposed as living representatives of the most brutalized kind of men: the First Brazilian Anthropological Exhibition, held in 1882 at the Rio de Janeiro National Museum.

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[23] NINA RODRIGUES, R. (1899), *Métissage, dégénérescence et crime,* Lyon, A. Stock & Cie, p. 4.

[24] LE BON, G. (1894), *Lois psychologiques de l'evolution dês peuples*, Paris, Felix Alcan.

[24] NINA RODRIGUES, R. (1899), *Métissage, dégénérescence et crime,* Lyon, A. Stock & Cie, p. 4.

[25] *Revista do museu Paulista,* 1895, pp. 19-24. Quoted in SCHWARCZ (1993), p. 80.

[26] *Revista do Museu Paulista,* 1897. Quoted in SCHWARCZ (1994), p. 140.

[27] PETRUCCELLI, J.L. (1996), Doutrinas francesas e o pensamento racial brasileiro, 1870-1930, *Estudos Sociedade e Agricultura*, 7, dezembro 1996: 134-149, p. 139.

[28] MORTON, S. (1839), *Crania Americana: or a comparative View of the Skulls of various Aboriginal nations of North and South America*, Philadelphia, Dobson.

SCANDINAVIAN SCIENCE DENATIONALIZED

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 483.

SCIENCE DE-INTERNATIONALIZED? THE CHALLENGES OF A LEARNED SOCIETY IN THE POST-NAPOLEONIC ERA

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Abstract

Learned societies of the 17th and 18th centuries formed an international network for exchange of scientific ideas and results through correspondence and circulation of publications. The era of the French revolution and the Napoleonic wars meant a blow to this network. In the post-Napoleonic era, learned societies had to find their place in a new order of scientific knowledge, where the universities were to play a more important part.
This paper will deal with how one such society, The Royal Norwegian Society of Sciences and Letters, met these challenges in the period ca. 1810-1870. The society, which had been founded in 1760 in the city of Trondheim, had participated in the international exchange of scientific knowledge through the network of learned societies, but by the turn of the century, it found itself in a peripheral position, both due to the lack of active members, but also hastened on by the troubled political situation. The Society met this challenge by reinventing itself as a locally based organization, aiming to play an important role on the national stage, and with pretences to uphold its international network.
The first two goals were successfully achieved, but the society struggled to place itself in a meaningful international context. Most successfully, this was done in the exchange of publications and the maintenance of the best scientific library in Norway, and partly it was done by trying to engage the personal networks of the leading members abroad. By the second half of the century, it was obvious that these strategies had to be based in a scientific environment of a more professional nature, such as a university or a research-based museum. The latter course was chosen by the society around 1870.

The topic of this paper is "The de-internationalization of science", which is a too complicated way of saying "the breakdown of scientific communication" when it comes to the phenomenon of learned societies in the 19th century. So the matter to be discussed is why the learned societies played a less important role in the 19th century scientific communities, and what strategies they had to try to keep up an international network. I will mainly be using The Royal Norwegian Society of Sciences and Letters, situated in the city Trondheim as an example, but also with some glances at the other Scandinavian societies.

But first we need to sort out a little. What were the learned societies and their networks, and how does the society in Trondheim fit in with these?

The learned societies of Europe had their heyday in the 18th century. The movement was initiated with the establishment of The Royal Society in London in 1660, and the Royal Academy in Paris in 1666. Both can be seen on a background of renaissance academies, but at the same time represented something new –scientific endeavour and communication outside the frameworks of the medieval universities, in the spirit of enlightenment. In the run of the next century, societies and academies, sometimes private, but mostly under royal protection, established themselves all over Europe. They formed a network of learned persons, exchanging knowledge, publications and artefacts in the spirit of enlightenment.

The Society in Trondheim was a part of this movement or network, with some peculiarities of its own. The society was founded in 1760 by a troika consisting of the bishop Johan Ernst Gunnerus, the headmaster at the cathedral school Gerhard Schøning, and well-to-do amateur Peter Fredrik Suhm. In a matter of a few years, they had organized a society who counted among their members both local clergy and public officials, and international intellectual superstars such as Carl von Linné. What they all had in common was an enthusiasm for collecting and sharing knowledge.

The sharing of knowledge in an international network between societies and their individual members took several paths. One was corresponding individually, in the knowledge that information in a private letter was likely to be shared with the equal-minded. But other ways were publishing, and trading.

Publishing transactions was an important side to the activities of the learned societies, stemming back to the 17th century Royal Society in London. The society in Trondheim lost no time in embarking on the same mission. From 1761, a more or less yearly volume of the writings of the members of the Society appeared. The volumes were published in Danish, but for the first decade, a parallel edition in German was also published, thus making the results of the struggles of the Norwegian scientist available to a larger public. These results would be of a varying nature. One of the special traits of the Trondheim society is that it was more than usually preoccupied with theology, and interpreting nature in its light. Another peculiarity, not connected to this, was that access to membership was fairly unrestricted –there was not an upper limit to the number of members of the society. But to return to publishing: The founding members were the most active ones in publishing in the society's series. But other members contributed as well. Once the volumes were published, they would enter the network of trading. The library of the Society in Trondheim was to no little degree augmented by trading volumes of transactions and the like from similar societies around Europe.

Another trade in which the societies engaged, was in artefacts. A learned society with self esteem had a collection, perhaps an exhibit, of objects of great rarity, beauty or significance. The Society in Trondheim could contribute to this network of trading objects with peculiarities of the north –stuffed birds, pressed plants etc from the arctic region. Not the least through bishop Gunnerus' network of priest in his northern diocese, the society had an abundance of these objects.

But these golden days did not last. The period of general warfare over pretty much the whole European continent from the early 1790s were a blow to the network of learned societies. The French convent, for instance, abolished all national academies and literary societies. Warfare also had its say in the disintegration of the networks. The academy of Berlin was plundered by French troops in 1806, while the academies in Brussels, Turin and Bologna were closed more or less permanently from the early 1790s, sometimes to the end of the Napoleonic wars. In Trondheim, we find an example of the difficulties of communicating with other societies, when an outstanding member of the Turin academy opened a correspondence in 1811 on the topic of classifying species of fish, and a long discussion was held at this meeting on how to organize correspondence that could break through the British blockade of the North Sea.

James McClellan, in his work on the learned societies of the early modern period, points to three paths of re-emergence for the learned societies once the turmoil of the Napoleonic wars was over. One path, particularly found in France, was that the societies became independent research institutions with a paid staff. Another option, more often found in the German area, was that the societies became more closely tied to the emerging universities; Berlin is a prime example here. The third possibility was that the societies fell into decline and became regional elite associations, more based on social value than scientific activities. The Trondheim society landed on a combination of the first and last of these three options: A locally based society with little scientific activity of its own and where the social value of membership was regarded as high, but on the other hand wealthy enough to support scientific work done by others in a relatively impressive manner, at least once in a while.

But keeping up the international networks was a harder challenge, not only for the Trondheim society, but for others as well. The publications went on, but not as before. From the 1790s the transactions of the society were published at a more seldom rate; from the beginning of the 19th century at intervals of ten years or more. This was not to change until the second half of the century. And already from the 1770s, the transactions were published in Danish, later in Norwegian only. The value of the publications for a foreign public must have been diminished by this. Nevertheless, trading publications with other societies remained an important way of keeping up the network throughout the 19th century.

But one of the main problems, seen from the side of the societies, was the emergence of the universities as a more important scene for scientific research. The societies had been the forums of the educated amateur, who didn't necessarily have scientific investigations as his sole occupation, but more as a way of life. The development of the scientific world in the direction of specialization and professionalization was not easily compatible with the learned gentlemen or local clergy who dabbled in disciplines of their liking. And even though the more central and established of the societies developed in the direction as mentioned above –as professional institutions of research or affiliated with universities, the more peripheral societies, such as the one in Trondheim, had no chance of accomplishing this. An alternative strategy for them was to try to upkeep their networks by electing foreign members who might contribute to the society in one way or another. This was a strategy followed to some degree in Trondheim. In the first years of the union with Sweden, a number of Swedish members were elected into the society. But a number of these were connected to the king's court, and were elected at the time of King Karl Johan's coronation in Trondheim. These members contributed to a very little degree to the activities of the Society, other than the letting nimbus of the royal court shine upon it. More serious was the policy of the president of the society in the 1830s and 40s, Frederik Moltke Bugge. Bugge was granted by the government the means to travel through Europe to study the organisation of higher education. This he thoroughly did over several years, and the contacts he made during these travels were often elected as members of the society at his request. However, these members hardly contributed to the society's activities beyond their mandatory contribution of books to the society's library at their election.

Another way of trying to recruit foreign members was to elect any passing stranger that seemed to fit the requirements of membership. Needless to say, this wasn't a very successful strategy either.

One noteworthy exception must be mentioned. Among the passing strangers in Trondheim was also the Recherche expedition in 1838. This French-led expedition to the arctic region was an early example of a cross-discipline research-project with members from several nations. The expedition consisted on scientists of both natural sciences and social sciences, in addition to a group of artists to illustrate their work and findings. The expedition was French, but relied heavily on cooperation with both Berlin University and Alexander Humboldt, and the Royal Swedish Academy of Sciences. The Trondheim Society's role in the expedition was modest indeed, as it amounted to hosting a special meeting of the society for the members of the expedition when the research vessel "La Recerche" anchored in Trondheim for a few days in June 1838. And of course, members of the expedition were elected as members of the Society.

Some attempts were also made to elect members that could contribute to the growing natural collections of the society. The election of the botanical gardens and zoological museum of Calcutta in the late 1840s might point in this direction. This coincides with a restoration and upgrading of the natural collections of the society. But it seems that not much came of it.

Perhaps one further explanation of why the society invested so little in keeping up their international contacts lies in the nature of what the society –and to a degree also the other Scandinavian societies– were preoccupied with. In the case of the Trondheim society, attention was to a fairly large degree turned to subjects of a strictly Norwegian nature –and published in Norwegian. Norwegian history, language, geology, botany and marine zoology were fields where much money and research was invested. That wouldn't necessarily collide with an international outlook, but it seems that the framework for these activities now to a larger degree were in a "nation-building" framework.

We might see something of the same in Denmark –the Royal Danish Academy of Sciences and Letters had been involved to a large degree in the literal mapping of Denmark through triangulations in the late 18th century, and this interest in the Danish realm and culture was only to expand in the following century. The Danish Society, however, was able to keep up its international relations through a more active policy of electing foreign members throughout the 19th century. The number of members of the Danish society was limited, and by 1830, foreign members would comprise more than half of the members. Their share would then decline for some decades. The Danish society on the other hand cut itself off from sharing their scientific activity with foreign societies and scientific institutions by publishing their transactions in the vernacular, as did the Norwegian society. Under the reign of the secretary Hans Christan Ørsted (who was secretary from 1815 to 1851), the policy of promoting the Danish language was an important issue for the society. Partially, this was done by investing a lot of time and money in publishing a dictionary of the Danish language.

On the other hand, the society was involved in an impressive expedition circumnavigating the globe in the years 1845-47, making many valuable observations during the journey. The expedition had both political and scientific goals. The political goal, to maintain Danish presence and rule over the Nicobar Islands in the Indian Ocean, was a failure. But the scientific results were more satisfying in the mass of observations of a physical, meteorological and astronomical nature done during the expedition.

As in Trondheim, the Danish society seems to have had an increasing interest in Danish history and language in the first half of the 19th century, embarking on publishing medieval sources, medieval history and medieval Danish language.

The Swedish academy seems to have taken another road, stressing their international contacts and partly also translating their writings to Latin, French or German, even though the majority of the articles published were originally and solely published in Swedish. The mighty secretary of the Academy, Jacob Berzelius (secretary 1818-1848), seems to have used his international reputation and network as a scientist to get the society as such involved in an international setting. Also, the annual report of the Society on the progress of sciences was followed with interest abroad, often being translated. One of the reasons for this was the reviews of recently published scientific works, which could be read for the information value, but also for entertainment –negative evaluation could be both quite merciless and eloquent.

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To sum up: Several factors contributed to the decline of the participation in international networks of science in the post-Napoleonic era. One was that the wars themselves had uprooted the networks and partially destroyed some of the societies. Another, and perhaps more important, was the degree of specialization and the development of scientific disciplines to a degree that was not compatible to the notion of the all-rounder or gentleman amateur scientist. And third: It seems that the currents of nationalism and national romanticism of the mid 19th century made the societies more insular, preoccupied with aspects of national culture and history to a larger degree than before, and of communicating these interests basically to a national audience.

References

BRENNA, Brita; ANDERSEN, Håkon With; NJÅSTAD; Magne and WALE, Astrid (2009). *Æmula Lauri. The Royal Norwegian Society of Arts and Letters,* Science History Publications, Sagamore Beach 2009

DRIVENES, Einar Arne (1992). La Recherche – forskningsekspedisjonen Frankrike glemte, *Historisk Tidsskrift* vol. 71, 11-35.

MCCLELLAN III, James (1985). *Science reorganized. Scientific Societies in the eighteenth century*, Columbia University Press, New York.

ODELBERG, Wilhelm (1989). Berzelius as Permanent Secretary" in FRÄNGSMYR, Tore (ed.), *Science in Sweden. The Royal Swedish Academy of Sciences 1739-1989,* Science History Publications, Canton MA, 124-147.

PEDERSEN, Olav (1992). *Lovers of learning. A history of the Royal Danish Academy of Sciences and Letters 1742-1992*, Munksgaard, Copenhagen.

‘MOVED’ NATURAL OBJECTS –SPACES IN BETWEEN

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INTRODUCTION TO THE SYMPOSIUM

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A culture of natural history in the 18th and 19th centuries cannot be imagined without referring to objects or natural objects and their circulation. Closely connected to these are knowledge spaces, such as the cabinets of natural history specimens, the botanical garden and so on, that is, places into which natural objects are incorporated. Currently there is a boom in research in this area. This is primarily due to the history of collecting them, but also to cultural factors, and the history of knowledge and science.

However, in contrast to this focus, there will also be an emphasis on other spaces, which have so far not been recognized as independent research topics. These contexts, which we will here refer to as “spaces in between”, concern neither the collected object’s place of origin nor its destination. Actually, they are located between these two poles, between the starting point and the place of destination, such as a collection.

The topic is the movement of objects outside these points of reference. This “movement” may be of a variety of “natures”. It may change the “nature” of the objects themselves, change it into “culture” and make them different. What cultural practices are involved in these movement activities, and what significance is generated by these activities in different “spaces in between”, beyond the material basis?

Quite different phenomena may work as “spaces in between”, it may be a tent during a journey, a container, a packet transporting objects, as well as a letter and various kinds of records accompanying the objects. The question of how these lead us towards epistemic references will be discussed with the aid of a number of case examples.

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GLOBAL TRANSPORTATION BY WAY OF SYSTEMIC TEMPORARY SPACES: SHIP, ISLAND, BOTANICAL GARDEN, PARADISE AND CONTAINER

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Abstract

In my contribution –looking at the world-wide transfer of plants that was established by botany in the 18th century– I am concerned with five 'spaces-in-between': ship, island, botanical garden, paradise and transport-container. For the 18th century I postulate a systemic relationship between these spaces, and I ask what effect this relationship had on the behaviour of the transport, on the one hand, and also on the dynamization of theories.

In the second half of the 18th century –because of the competition between European states– plant transfers mutated into a vital symbol for the world powers. This transfer was played out globally between 3 scientific spaces: between a ship, an island and a botanical garden. They are characterized not by their specific conditions, but they show themselves, rather, as instances of transfer.

The ruling elites of the early modern period loved exotic plants, admired them and brought them from distant places to Europe. At the same time the movement of plants from one part of the world to another, and over the oceans of the world, had moved to a new organized stage, particularly since it was taking place in the framework of a process of co-operation between science, the state and colonialism.[1] This co-operation also caused the movement of European institutions, such as botanical gardens, to tropical islands[2] (l'Île de France, then Ceylon and St. Vincent, to name only the first few.)

All three –ship, island and botanical garden– constituted spaces that were clearly distinguishable from one another and which formed a connective system for the 'travelling plants'. Island, ship and botanical garden certainly had something in

[1] David MACKAY: Agents of Empire: the Banksian Collectors and Evaluation of New Lands. In: David Philip MILLER, Peter Hanns REILL (Eds.): *Visions of Empire*. Cambridge 1996, S. 38-57; particularly on this: Londa SCHIEBINGER and Claudia SWAN (Eds.): *Colonial Botany. Science, Commerce, and Politics in the Early Modern World*. Philadelphia 2005.

[2] Lucile H. BROCKWAY: *Science and colonial expansion. The Role of the British Royal Botanic Gardens*. New York/ London / et al. 1979; Donald P. MCCRACKEN: *Gardens of Empire. Botanical Institutions of the Victorian British Empire*. London/ Washington 1997.

common with one another from a systemic point of view: they were culturally regulated and cognitively cut off from the immediate surrounding nature. In spatial terms they defined themselves by means of a constantly different dominating natural element. They rose markedly above their naturally determined environment, in that culturally they raised the internal in contrast to the external. In the same way as the instances of transfer (ship, island and garden),[3] the differently configured plant-transport containers –in spite of great variance– marked their isolation from their environment.

[3] On the term Transfer: Marianne KLEMUN: Globaler Pflanzentransfer und seine Transferinstanzen als Kultur-, Wissens- und Wissenschaftstransfer der frühen Neuzeit. In: *Berichte zur Wissenschaftsgeschichte. Organ der Deutschen Gesellschaft für Wissenschaftsgeschichte 29*, 2006, pp. 205-223.

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METAMORPHOSIS BETWEEN JUNGLE AND MUSEUM. COLLECTIONS IN THE MAKING

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Abstract

Artistically preserved crocodiles, apes, piranhas and birds, intestinal worms conserved in alcohol –millions of specimens assembled in museums for Natural History around the world represent the outcome of numerous scientific expeditions since the mid-18th century. The zoological collections of Johann Natterer (1787-1843), gathered during his travels throughout Brazil in the years 1817 to 1835, give an excellent example to point out how specimens were moved from "jungle" to "museum" and how they changed on their way between these two spaces of knowledge.

Johann Natterer was a member of an expedition to Brazil initiated by the Austrian emperor Francis I and carried out by the imperial cabinet for "naturalia", the "Hof-Naturalienkabinett". Natterer was one of the many naturalists who made specimens available for scientific research, for classification and for display at the museum. His collections of Brazilian fauna are to this day a valuable part of the stock of the Museum of Natural History in Vienna. In collecting, Natterer and his superiors at the imperial cabinet saw the appropriate solution for their major scientific task, which was to bring order into the "realms of nature". Classification was their predominant concern.

Before individual animals became part of an exhibition or collection as a classic representative of its species, the specimens underwent several transformations. The animals were hunted, killed and skilfully preserved with the help of instruments and chemical means, denoted, described and placed in a system of natural-scientific nomenclature. The specimens were transported from continent to continent, they were passed on in international networks and at last they were displayed in museums or stored in collections for further studies, allocated within specific cultures of knowledge and "nature". Written and visual documentation both tried to verify these procedures and to secure reliable data for each specimen collected in distant parts of the world. Natterer's reports of his travelling and collecting in Brazil described these metamorphoses in question and at the same time they played an import role in this process.

These metamorphoses, produced by cultural practices like the art of taxidermy, altered the animals' physical composition and their appearance could only be maintained with the help of artificial means (preservatives, colours, artificial parts). But only the improvement in preserving animals in the late 18th century by using new chemical devices of preservation made it possible, that specimens could withstand long overseas transport. With the rise of taxidermy, a flow of manuals and guides for naturalists and taxidermists was published which helped to spread new methods and to popularize collecting and preserving practices.

Due to the many alterations, one can hardly relate to the specimens shown at the museum as being identical with or even being authentical images of the living animals. They are "made" cultural objects. When considering all these changes it may be surprising that the specimens' scientific values were not in question at the time of their acquisition. Despite all transformations and their artificial construction as cultural objects, the specimens were regarded as authentic representations of a species and –together with other information (sketches, reports, notes) – accepted as valuable and reliable sources for certain scientific questions. But dealing with historic scientific travels and collections has to take in account that a whole set of cultural practices was involved in creating the specimens displayed as seemingly authentic representations of "nature" in a museum's exhibition.

References

BOURGET, Marie-Noëlle; LICOPPE, Christian; SIBUM, H. Otto (Ed.) (2002). *Science, Scientific Instruments and Travel. Itineraries of precision from the seventeenth to the twentieth century*, Routledge Studies in the History of Science, Technology and Medicine 16; London, New York.

FARBER, Paul Lawrence (1977). The Development of Taxidermy and the History of Ornithology. In: *Isis* 68, 550-566.

LARSEN, Anne: Equipment to the field. In: JARDINE, Nicholas; SECORD, James A.; SPARY, Emma (Ed.) (1996). *Cultures of natural history*, Cambridge, 358-377.

NAUMANN, Johann Friedrich (1848). *Taxidermie oder die Lehre Thiere aller Klassen ... für Naturaliensammlungen auszustopfen und aufzubewahren*. 2. verm. Aufl., Halle (first published in 1815).

SCHIEBINGER, Londa; SWAN, Claudia (Ed.) (2005). *Colonial Botany. Science, Commerce, and Politics in the Early Modern World*, Philadelphia.

SCHMUTZER, Kurt (2011). *Der Liebe zur Naturgeschichte halber. Johann Natterers Reisen in Brasilien 1817-1836*, Veröffentlichungen der Kommission für Geschichte der Naturwissenschaften, Mathematik und Medizin 64, Wien.

THON, Theodor (1827). *Handbuch für Naturaliensammler oder gründliche Anweisung, die Naturkörper aller drei Reiche zu sammeln, im Naturalienkabinett aufzustellen und aufzubewahren. Frei nach dem Französischen bearbeitet und vervollständigt*, Ilmenau.

TURGOT, Etienne-François (1758). *Mémoire instructif sur la manière de rassembler, de preparer, de conserver, et d´envoyer les diverses curiosités d'histoire naturelle*, Lyon.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 493.

FROM VESUVIUS TO THE WORLD. TEODORO MONTICELLI'S (1759-1845) COLLECTION AND HIS EUROPEAN CONTACTS. THE NEAPOLITAN CASE

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Abstract

Teodoro Monticelli (1759-1845), universally recognized by his contemporaries as the best of Vesuvius connoisseurs, was a man of science but also a man of power. He had been the rector of the University of Naples, and was at the centre of a wide and efficient network along which minerals, instruments and information had been circulating; but also scientific theories and new progressive ideas. This intense circulation is testified by Monticelli's correspondence.

Teodoro Monticelli (1759-1845), mineralogist, geologist, universally recognized by his contemporaries as the best of Vesuvius connoisseurs after the publication in 1825 of *Prodromo Vesuviano*, the first text of Vesuvian systematic mineralogy, in collaboration with his young pupil Nicola Covelli.[1] Man of science but also man of power, he had been the Rector of the University of Naples between 1826 and 1827, and had also been in touch with a large number of scholars inside and outside Europe; he was the centre of a wide and efficient network along which had been circulating minerals, scientific instruments and information: above all scientific theories, but also new progressive and democratic ideas. This intense circulation of things and ideas is testified by Monticelli's correspondence, a large number of manuscript documents very interesting for the history of science, the history of scientific institutions and collections, but also for social and political history.[2] From about two thousands letters received by Monticelli from his correspondents, emerge courses, directions and destinations of these movements. Generally, Monticelli established the relationships in the occasion of the visits to Vesuvius by these learned

[1] M. MONTICELLI, N. COVELLI, *Prodromo della mineralogia vesuviana*, Napoli Da' Torchi del Tramater 1825.

[2] There are lots of biography about Monticelli, many information in E. MONTICELLI, *L'abate Monticelli*, Napoli 1932.

strangers coming from all parts of Europe.[3] Owner of a large and famous Museum of Minerals, Monticelli exchanged specimens of minerals from Somma-Vesuvius volcanic complex with the ones found in other parts of the world; he had also been selling, to a number of museums and private collectors, sets of Vesuvian rocks of an established cost and composition, found and arranged by a team of expert mineralogists he had specifically this job.[4] Thanks to this activity, the Natural History Museum of London still holds a rich Vesuvian collection, called Monticelli collection because it was bought from Monticelli with the intercession of Humphrey Davy, one of the leading scientists of the 19th century in Europe, President of the Royal Society (1820-1827) and one of Monticelli's closest friends.[5]

Davy met Monticelli during his first journey in Italy, in 1813. Here he had the opportunity to observe the eruption of Vesuvius of the days 25 and 26 December, and to analyze Vesuvian productions, as it clearly emerges from a short text written by Monticelli about this eruption and dedicated to Davy, who also invites him to come again to Naples and spend time together making further studies on Vesuvian activity.[6]

And Davy accepted Monticelli's invitation; in fact he was back in Italy and in Naples again in 1819, in 1820 and in 1825. In his third visit to Vesuvius, Davy met also Christian Frederic, Prince of Denmark later King Christian VIII, an interesting figure of modern and open minded sovereign, but also a true intellectual whose scientific interests went far beyond the usual limits of the 18th and 19th century amateur. In fact he also wrote some texts about Vesuvian phenomena and about the composition of rocks in Italy, Denmark and abroad.[7] During the winter of 1820, Monticelli, Davy and Christian Frederic spend some months in Sant'Anastasia, a little village in the Neapolitan countryside, hosted by the chemist of the place Pasquale Miranda, to observe directly and collect specimens of rocks, minerals and any other sort of volcanic products from the Somma-Vesuvius volcanic complex.[8]

Speaking about observations made in 1820, in particular Monticelli himself refers to an excursion made not only in the company of Davy and Prince Christian, but also with the famous French mineralogists Alexandre and Adolphe Brogniart, Giambattista Brocchi, one of the more representative Venetian scientists, and the Spanish Gimbernat, all of them to become his correspondents later.[9] This episode evidently shows how Vesuvius and Naples had been a true open air laboratory where anyone interested in Natural History had to go to try to complete his scientific education. In other words Vesuvius became a proper cosmopolitan open air laboratory in which the best natural philosophers in the world had been meeting, where Monticelli played a leading role not only in coordinating action and visits and in establishing contacts between scientist of any part of the world, but also in surrounding information and suggestions and hypothesis which in some cases were the basis of new theories. So the Neapolitan volcano was not just an interesting site in which 'to catch the nature on the fact',[10] but also a cultural centre, where Monticelli, as a proprietor of what Davy defines as 'il Vostro gran laboratorio vesuviano' ('Your big Vesuvian laboratory') had been hosting generations of scientists from all over the world considering him not only the best means to get up to the mountain but also a kind of interpreter of the secret language of the volcano.[11]

After the rather long period spent studying together in 1819, friendship between Monticelli and Davy became far stronger, as it is testified by the intense and affective correspondence in which Davy tried to avoid the English language that his Italian friend did not understand, and prefered a strange and rather incorrect Italian, or –rarely– French. In the years between 1819 and 1825, Monticelli also tried to involve Davy in the development of papyri of Herculaneum thanks to a new

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[3] We are finalizing the publication of a vast selection of letters to and by Monticelli part of the *Carte Monticelli* found on the *Biblioteca Nazionale Vittorio Emanuele III di Napoli, Sezione Manoscritti* (BNN, Sez. Ms.). We thank you very much Doctor Mariolina Rascaglia and Doctor Francesca Stamuli for the information and collaboration.

[4] I. MENDITTI, C. PETTI, Un museo scomparso: il Museo Monticelli,in *«Bollettino Società Naturalisti»*, Napoli Nuova serie 1 (2001) pp. 143-155.

[5] P. TANDY, A. WOOLEY, The British museum collection of rocks and minerals form Vesuvius made by William Hamilton (1730-1803) and Teodoro Monticelli (1759-1846), and Hamilton's observations on Vesuvius between 1764 and 1800, in M. R. GHIARA, C. PETTI eds., *Atti del Bicentenario Real Museo Mineralogico. 1801/2001*, Napoli 2001, pp. 171-180.

[6] T. MONTICELLI, *Descrizione dell'eruzione del Vesuvio avvenuta ne giorni 25 e 26 dicembre dell'anno 1813*, Napoli Stamperia del Monitore delle Due Sicilie, 1815. Dedica.

[7] AA. VV., *Christian VIII & the National Museum. Antiquities, coins, medals*, Copenhagen 2000.

[8] T. MONTICELLI, Me*moria del Cavaliere Teodoro Monticelli sull'origine delle acque del Sebeto, di Napoli antica, di Pozzuoli, letta nella tornata del 19 giugno 1828 del Real Istituto d'Incoraggiamento alle Scienze Naturali ed inserita nel 5° volume degli Atti di detto istituto, Napoli, nello stabilimento dell'aquila*, 1840, pp. 5 and *passim*.

[9] T. MONTICELLI, Escursioni fatte sul Vesuvio in compagnia d'illustri soggetti, e celebri dotti stranieri dal 1817 al 1820, in Id, *Opere dell'abate Teodoro Monticelli segretario della Reale Accademia delle Scienze di Napoli*, Napoli Tipografia dell'Ariosto 1843, vol. I, p. 75.

[10] W. HAMILTON, *Observation on mount Vesuvius, Mount Etna, and other volcanoes in a series of letters addressed to the Royal Society from the honourable man Sir William Hamilton, KBFRS, his majesty's envoy extraordinary and plenipotentiary at the court of Naples to which are added explanatory notes by the author, hitherto unpublished*. The second edition. London Cadell in the Strand 1773. pp. 92-93.

[11] BNN, Sez. ms., Carte Monticelli, D079. Letter of H. Davy to T. Monticelli, Roma 19 ottobre 1819.

chemical method he had invented and supposedly to more efficient than the old one of Padre Piaggio. Thanks to Monticelli Davy had the permission by the Bourbon Court to test his method upon some exemplars and also obtained some funds from the British Government for this job. But it seems that Davy never completed the task, maybe because of his new important and quite unexpected charge of President of the Royal Society, which leaved him truly few time for any other things.[12]

Notwithstanding Davy tried to help his Neapolitan friend to sell his Vesuvian collection to the British Museum, of which he was one of the trustees as President of the Royal Society. From the correspondence between Davy and Monticelli in those years, it clearly emerges how Monticelli decided to sell his collection both for economical and logistic problems. It seems he feared the loss of Palazzo Penne, maybe because he bought the ancient palace in which he arranged his mineralogical museum for a very small price from the State during the Napoleonic era. Davy tried to have the best price for the collection for his friend Monticelli, who made at least two sendings. The Monticelli collection is still preserved in the collection of Natural History Museum with the original Monticelli manuscript labels.[13]

Heir of a long and strong tradition, Monticelli's relationships with the United Kingdom –apart from Davy– where particularly strong.[14] In fact, John Herschel (1792-1871) himself appreciated this Neapolitan scholar, as he clearly said in one of his letters. Herschel was also the one who introduced Charles Babbage to Monticelli. In his Italian voyage, in June 1826, Babbage not only visited Vesuvius with him, but was so brave as to dare climbing up to the crater and exploringe the bottom of it, making some experiences which were discussed in the Società Reale Borbonica, of which he was appointed fellow correspondent.[15] So Monticelli got in touch with Lyell's ideas, which he very much supported, as he had met him when he eventually came in Naples in 1828, and as it was impossible for him to descend the crater directly, Monticelli offered him the sketches made on the spot by the Neapolitan landscape painter Antonio Siesto,[16] as it is confirmed by Lyell himself.[17] The images of the Vesuvius in its various phases were an essential part of the researches of this cosmopolitan group of scientists that usually brought with them artists to take pictures and simple drawings, or sometimes executed them themselves.[18] These art pieces, despite their aesthetic value, were collected and exchanged just to testify the shape of the carter or the main character of a precise eruption. In fact, Christian Frederic donated to Monticelli the original drawings made by his official painter, because the Prince thought that they would be useful for the scientific theories of his Neapolitan friend.[19] Two other close British friends of Monticelli were the Wilbraham sisters. In particular Hanna Wilbraham, later Grosvenor, good drawer and a smart and not banal geologist, is the author of some of the plates in Monticelli's text on the Eruption of 1827. The young woman long corresponded with Monticelli, adjourning him about more recent British theories on the origin of Earth, and in particular about Lyell and Babbage's gradualism.[20]

If Vesuvian minerals had been for years leaving Naples toward North Europe, on the other hand lots of equipments, coming above all from the United Kingdom and Germany, had been reaching Naples because Monticelli needed scientific instruments to study Vesuvius and its eruptions with modern means, and to adequate the scientific institution of South Italy to European standards. Among scientists sending him equipments, there was Alexander von Humboldt, who also played a major role in the diffusion of the gradualism and the central fire theories among Monticelli and his pupils. Humboldt esteemed Monticelli very much and appreciated his competence about geologic and Vesuvian matters; in fact, in one of his letters he asked the Neapolitan scientist a series of questions on the activity of Vesuvius and the variations of its shape.[21] A interesting document, where the long friendship between the two scientists clearly appears, was later published by Monticelli in one of his short reports on the volcano.[22]

Monticelli's network made him and his nearest pupils aware of the most modern scientific theories and technical discoveries, but it didn't really create a school of adjourned scientists. Reasons are probably political ones; in 1830 the death of Francesco I di Borbone, a still young and very well oriented sovereign, and then succession by his son Ferdinando, determined a drastic cut to the funds destined to the university and other cultural and scientific institutions, and so the situation began to get slowly but inexorably worse. In fact, 1848 was the year of the international congress of Italian

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[12] BNN, Sez. ms., Carte Monticelli, D071-D108.

[13] P. TANDY, A. WOOLEY, *cit*. and *Ibidem*.

[14] About the instense and significant cultural relatioships between Kingdom of Naples and United Kingdom, see M. TOSCANO, *Gli archivi del mondo*, Firenze, Edifir, 2009.

[15] T. MONTICELLI, Altre escursioni fatte al Vesuvio nel marzo 1827, in *Idem, Opere…cit*., p. 120.

[16] Idem, Osservazioni dello stato del Vesuvio dal 1823 al 1829, in *Idem, Opere…cit*., pp. 106-112.

[17] C. LYELL, *Principles of geology*, London : John Murray, 1830, chap. 5.

[18] M. TOSCANO, *cit*.

[19] T. MONTICELLI, Escursioni…*cit*, p. 77bis.

[20] BNN, Sez. ms., Carte Monticelli, W012-021.

[21] BNN, Sez. ms., Carte Monticelli, H. 60. Letter of A. von Humboldt to T. Monticelli, Paris 22 Decembre 1825.

[22] The manuscript was published in French and with the original drawings of the Vesuvian crater made by HUMBOLDT, Lettera del Celeberrimo sig. Barone Alessandro De Humboldt al Cav. Monticelli, in T. MONTICELLI, *Opere…cit.*, p.126-161.

scientists in Naples, but also the one of Monticelli's death, who died just during the event, due to the revolutionary disorders which caused the death or the escape of many intellectuals.[23] Just Arcangelo Scacchi, Monticelli's best pupil, stands –as a skilled mineralogist, appreciated in Europe and author of the identification of some new Vesuvian minerals– to represent the high level reached from mineralogy in Naples in Monticelli times[24].

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[23] G. GALASSO, Modelli di interpretazione del 1848, in *La rivoluzione liberale e le nazioni divise, Atti del convegno internazionale di studio*, Venezia 2000, pp. 127-155.

[24] M. R. GHIARA, C. PETTI, Real Museo Mineralogico. Uno scrigno di meraviglie, in «*Rivista Mineralogica Italiana*», 1, 2008, pp. 24-45.

SCIENCE IN THE PUBLIC SPHERE: BARCELONA, 1868-1939

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THE EDISON TIN FOIL PHONOGRAPH IN BARCELONA: A DEMONSTRATION AT THE FREE ATHENAEUM OF CATALONIA (1878)[1]

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Abstract

Thomas A. Edison and his phonograph (1877) attracted great admiration. On 12th September 1878, following its presentation in other European cities, a phonographic soirée for members and guests was held at the Free Athenaeum of Catalonia in Barcelona, founded that year, mainly as a reaction to the banning of positivist or Darwinist lectures at the Barcelona Athenaeum. The innovative firm Francisco Dalmau and Son imported phonographs (the first one from London for the School of Industrial Engineers of Barcelona) and experimented with them. Before Tomàs J. Dalmau presented the device at the Royal Academy of Natural Sciences and Arts of Barcelona, he participated in the phonographic session at the Free Athenaeum. This demonstration was preceded by a talk by the writer and science popularizer Joaquim M. Bartrina. Although many other activities, sometimes repeating the Bartrina-Dalmau tandem, took place at this centre, the phonographic demonstration contributed especially to its establishment as a leading institution for the local popularization of science and to the propagation of its views. Other scientists and amateurs were also interested in the phonograph, which was used at several locations, even in conjuring performances. This paper will analyse a case of the circulation and social exposure of technology in a context of ideological controversies.

The wide recognition of Edison and his phonograph, despite its less echoed precedents and its deficiencies, is proven by a large number of writings, in which some clues can be found. The phonograph, as a fulfilment of an obvious idea,

[1] This paper, which has been further developed in an article, was funded by the research project HAR2009-12918-C03-02, directed by Agustí Nieto-Galan. Among others, I am also grateful to the staff of libraries and archives. Lindsey Myhren and the language service of the *Universitat Autònoma de Barcelona* helped me with the English correction. Studies on Edison, the phonograph and its diffusion, the circulation of technology, scientific performances, the Catalan context... are very abundant.

could be used to endorse the positivist model and to promote trust in the great possibilities of technology, as well as contribute to its humanization.

This amazing invention quickly also became well-known in Spain, the state to which the largest part of the Catalan Countries belongs. In Madrid, on 10th December, a commentary about the phonograph, quoting *Scientific American*, is published by an engineer, according to the press and several studies, native of Catalonia (Alcover 1877: 355-356; see also 1878: 119; Fernández Bremón 1877: 354), and soon afterwards a lot of information, a large part of which has been forgotten, circulated about it around the Spanish State. Also in its capital, in the academic year 1877-1878 a phonograph was imported by the Saint Isidro Institute (Jiménez & Lastra 2006).

Of course, Edison became very popular in the Catalan lands as well (Glick 1983), in the context of considerable admiration towards the US, with which several relationships were established. For instance, a famous Catalan tongue twister ('*Setze jutges d'un jutjat*...') was soon recorded by a visitor to Menlo Park (Croffut 1878; http://edison.rutgers.edu; Israel, Nier & Carlat 1998: 213-220). Who was he? In any case, the engraving that illustrates the article was signed by 'Ph[ilip]. G. Cusachs'. This Catalan artist collaborated in the newspaper as well as *La Llumanera de Nova York*, which obtained an autograph from Edison (figure 1).

LA LLUMANERA

VOLUM VI. NOVA YORK, OCTUBRE 1880 NUM. 66.

Salut als lectors de La Llumanera

Thomas A. Edison

Fig. 1: A[rtur]. C[uyàs], 'Thomas Alva Edison', *La Llumanera de Nova York*, 66, october 1880, 1.
Biblioteca de Catalunya. Barcelona.
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It is said that on 21st February 1878 Francisco Dalmau and Son had obtained a five-year privilege for introducing the phonograph to Spain (Maluquer 1992: 123, and further studies by him; I am grateful for his response concerning this question). However, it has not been localized in the National Patents and Trademarks Office (Madrid), in the Cuban equivalent, among governmental dispositions or in the general and specialized press.[2] On the other hand, Edison had obtained the patent for the US only two days before, even though he had requested it on 15th December 1877.

In any case, *El Porvenir de la Industria* (157, 22/3/1878, 103) announced that Dalmaus' firm had received phonographic recorded sheets and expected that a phonograph would be delivered soon. It added that phonographs, which would be supplied in a few days, had been tested by their representative in London, and the plates recorded were displayed

[2] Other files about Dalmaus' inventions and the phonograph are conserved.

in Dalmaus' shop windows, at 9 Rambla del Centre, in Barcelona (160, 12/4/1878, 128), as the phonograph imported for the physics laboratory of the School of Industrial Engineers of Barcelona would be exhibited. Apart from other reports, the *Boletín Mensual de la Asociación de Ingenieros Industriales* (3, May, 11) informed that on 13th April this example was tested for the first time.

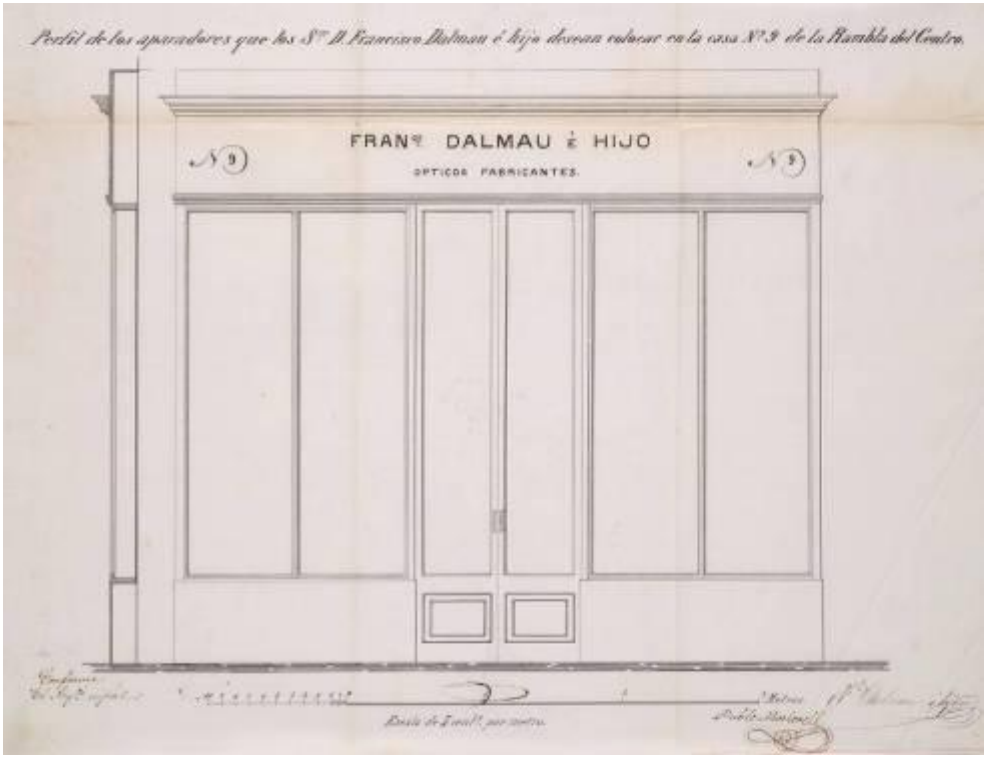

Fig. 2: Drawing for a renovation project of Dalmaus' shop windows,
at 9 Rambla del Centre, in Barcelona. 12th November 1873.
Arxiu Municipal Contemporani de Barcelona. Q127. Particular Works. *Foment*, file 2589-C.

Unfortunately, it is not conserved at the Higher Technical School of Industrial Engineering of Barcelona, where Lusa (2003) –whom, as well as Carles Puig-Pla and Antoni Roca Rosell, I thank for his help with research in this centre– unearthed a bill issued by Dalmaus' firm on 13th June, including a phonograph and phonographic plates. This would be the earliest phonograph in Spain, according to the press, as well as Suárez 1880: 461, an author studied by Sánchez Miñana, to whom I am grateful for his report on the status of his research on subjects related with this paper.

Josep Dalmau Montero (1956), the youngest son of Tomàs J., informed that he conserved part of the family heritage. Regrettably, it has not been preserved, according to some descendants. However, there is a large quantity of scattered information regarding the Dalmau family, which has been partially studied, as well as by economic historians (see also Cabana 1992), by historians of science.

Thanks to Sánchez Miñana and Lusa (2009, in addition to Sánchez 2006), we know that Francesc Dalmau was a musician from Manresa who became an optician in Barcelona. He had partners or collaborators, especially his son Tomàs J. and the engineer Narcís Xifra.[3] In 1881 Tomàs J. registered the Spanish Society of Electricity, becoming its managing director (see also Martín & Ollé 1961, García de la Infanta 1986, Altshuler & González 1998, Arroyo & Nahm 1994, Alayo 2007...).

The Dalmau family, despite their modest optician origins, imported, exported, manufactured, constructed, modified and invented many kinds of devices. I will only enumerate several activities: optics, photography, scientific instruments, lightning-conductors, electricity, illumination, telegraphy, telephony, industrial and agricultural machinery, armaments, electric bells...[4] A catalogue from 1877 was still well-known, but I discovered another from 1871 also at the Royal Academy of

[3] For more family details, see the files of his son Francesc de P. Xifra Montero and Manuel Dalmau Montero at the University of Barcelona.

[4] Several studies on some of these issues refer to the Dalmau family.

Sciences and Arts of Barcelona, significantly folded inside a secondary school textbook (Rico & Santiesteban 1864). There are also many advertisements in the press and in guides.[5]

Beyond marketing interest, they contributed to science popularization, through shows and writings. Moreover, they collaborated with societies, educational centres and professionals, for instance with doctors (Ausín 1999). Although Tomàs J. was a board member of the most important managers' association, *Foment* (minutes and the press), the Spanish Society of Electricity even earned the support of the anarcho-collectivist review *La Tramontana* (95, 13/4/1883, 3). The Dalmau family had a complex and important network of relationships and enjoyed great recognition in Spain, as well as obtaining certain acknowledgement abroad.

Fig. 3: Advertisement of a Dalmau and Son's delegation in Madrid, illustrated with a Gramme dynamo. *El Globo*, 26/10/1877, 4. ©Biblioteca Nacional de España. Madrid.

Fig. 4: Drawing for an illuminated advertisement announcing Francesc Dalmau's Cosmorama, at 8 Ciutat Street, in Barcelona. 14 April 1843. Arxiu Municipal Contemporani de Barcelona. Q127. Particular Works. *Foment*, file 65. See Sánchez Miñana & Lusa 2009: 92.

In 1874, finishing the Democratic Six-year Period, the Restoration began. However, the liberal substratum became apparent not only in political controversies, but also in scientific and philosophical ones, above all regarding positivism and Darwinism. One of the most important battlefields was the Barcelona Athenaeum. There, in 1877, the banning of lectures contributed to the constitution of the Free Athenaeum the following year.[6]

Having had its first site at 7 Rambla de Santa Mònica, it was relocated to 23 Avinyó Street (*Gaceta de Barcelona*, 109, 13/3/1878, 1581; *La Imprenta*, 72, 13/3/1878, 1764). There, on 12th April, an ode celebrating the 'inventor of the phonograph' was read (Roure 1878; *El Comercio de Barcelona*, 39, 13/4/1878, 513). What is more, on the evening of Thursday 12th September a phonographic session was held, following other European cities. This was considered to be the

[5] See also the documentation of the Municipal Contemporary Archive of Barcelona.

[6] See archives, minutes, the press and studies on Barcelona Athenaeum, some Free Athenaeum members and the reception of positivism and Darwinism.

first in Spain, even by Madrid press and Suárez 1880: 461.[7] Some details can be found in the Free Athenaeum printed memories, several evocations (such as Almirall 1880; Roure 1916, *s. a.*: 125-126; Dalmau, A. R., 1930) and multiple reports (one of which appeared in *La Academia*, 16, 30/10/1878, 254, illustrated with an engraving, 252 (figure 6), yet criticized by the *Gaceta de Cataluña*, 462, 5/11/1878, 2195).

Fig. 5: Caricature of the banning of lectures at the Barcelona Athenaeum.
Almanach de la Campana de Gracia. 1878.
Biblioteca de Catalunya. Barcelona.

Foreseeing a very large turnout, the soirée, which, unlike others, was only for associates and guests, took place in the largest room. In addition to the predominance of the society's ideology (with several tendencies), training levels were varied, from engineers (such as Manjarrés and Rojas) to people without scientific knowledge.

The act was opened by the writer Joaquim M. Bartrina (Reus 1850 – Barcelona, 1880), a science amateur and popularizer.[8] Having gained notoriety through the suspension of his lectures on pre-Columbus America (1881: 127-168) at the Barcelona Athenaeum, he was member of the board of the Fre Atheneum (sometimes as a secretary).[9]

We know that he was only enrolled in a few liberal arts courses at the University of Barcelona. However, he completed his training thanks to readings and contacts, evolving from romanticism to positivism, with a great dose of scepticism. He then worked at the *Gaceta de Cataluña* as an editor.

He wrote about multiple subjects, including scientific and technological topics, which are even present in his literary works. In addition to translating Darwin and applying his theories to several fields, his notes on physics are remarkable (Bartrina 1881: 270-288; 'Visión profética', *El Diluvio*, 145, 24/5/1916, 7; Ramon 1899; Roca [*s. a.*]: 19-21; Aguadé 1924...). Some of these would often be linked to the invention of the radio, yet this association had already been questioned. Without falling into exaggerations, Bartrina was far from being an ignorant visionary, since he had the support of the Dalmau family and information sources (from scientific studies to the general press). [10]

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[7] The Free Athenaeum's session was omitted (Alier 1985; Barreiro 1992), as well as attributed to Barcelona Athenaeum (Lladó 1991 – probably misunderstanding Riutort 1946: 29–, quoted by Jones & Baró 1995: 32, and Gómez Montejano: 32). On the use of the phonograph as a show, see Feaster 2007.

[8] There are quite a lot of studies on him, but some issues have yet to be researched thoroughly. For instance, publications (such as early collaborations in *El Tesoro*), manuscripts, news, commentaries, imitations, parodies... have been forgotten.

[9] The manuscript of Bartrina's lecture is partially conserved with papers of Joan Sardà i Lloret, collector of his works (Bartrina 1881; Solà: 325), which at present are owned by his great-grandson Joan Sardà i Ferrer, whom I thank for providing access. Among them, I discovered manuscript fragments of 'La meteorología popular' (Bartrina 1881: 193-218).

[10] Among the books bequeathed by Bartrina's family to the Library of Catalonia, some are regarding these kinds of subjects, one of which, dedicated by Niaudet, is reproduced in figure 7. For more information about this and further Dalmaus' contacts, see Lusa 2003; Nieto-Galan 2003.

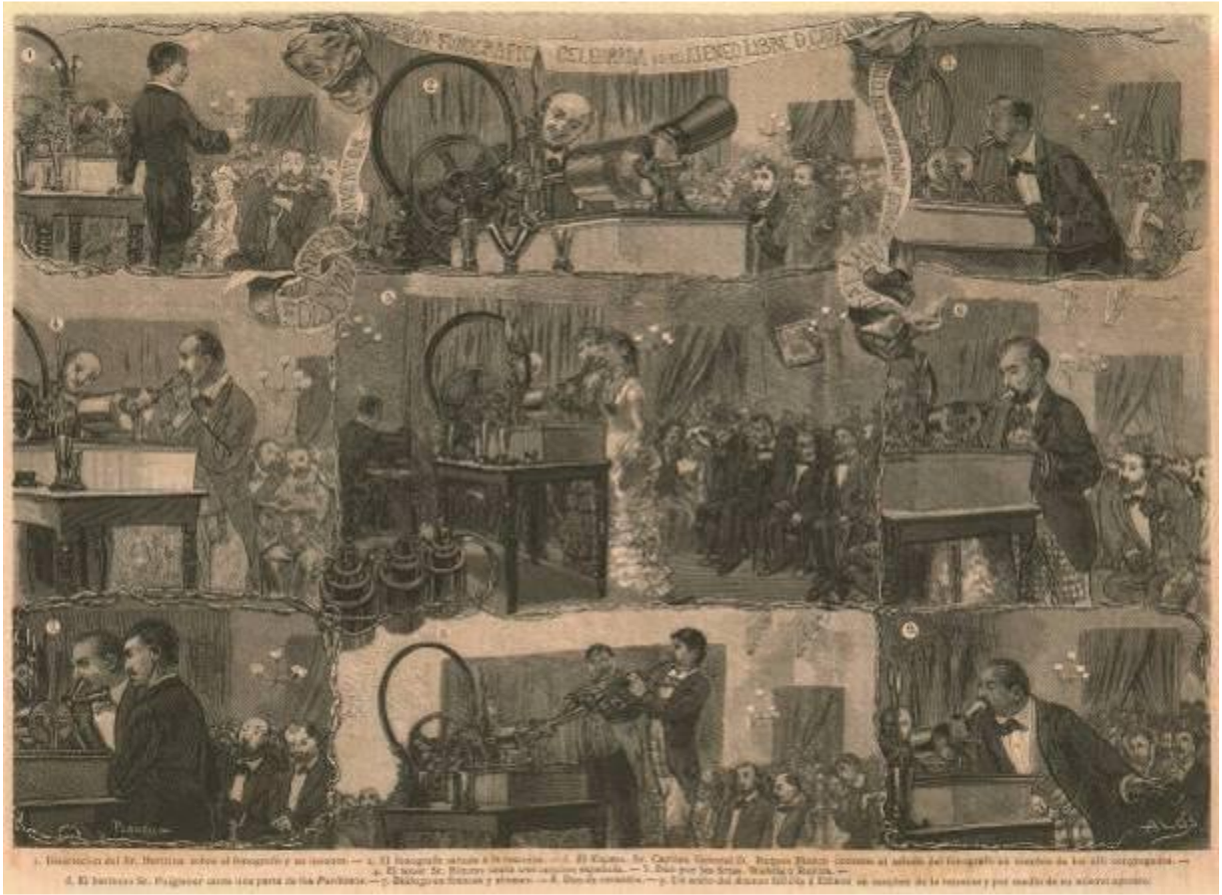

Fig. 6: Drawing by [Eugeni] Alós [i Marte] and [Josep] Planella [i Coromina].
Example of the author of the paper.

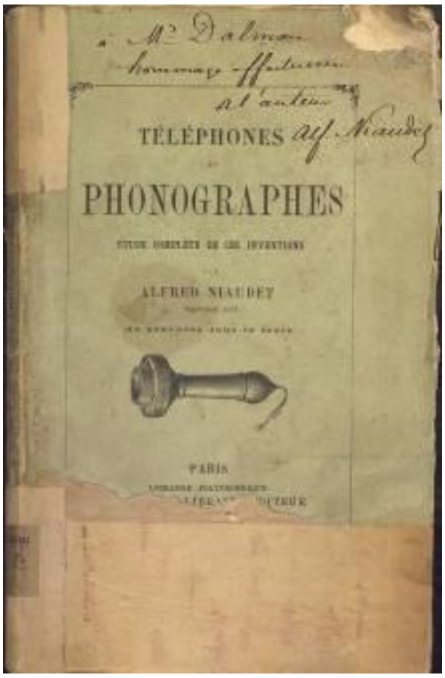

Fig. 7: Biblioteca de Catalunya. Barcelona. Catalogue number A 65-8-174. On pages 42-43, 'M. Dalmau from the academy of Barcelona', Tomàs J., is mentioned with regard to the telephone. The work was published for the first time in 1878.

Reports summarized that his presentation praised the American character, sketched Edison's biography in an anecdotal manner and described the device. Unfortunately, we do not have the text of his talk. Nevertheless, in addition to anonymous articles in the *Gaceta de Cataluña* about Edison and his phonograph, I found another similar to Bartrina's talk (A. T. O., 'L'inventor de fonògraf', *La Campana de Gracia*, 475, 15/9/1878, [1], figure 8). This pseudonym, the pronunciation of which sounds like the word 'Ateu' (in English, 'Atheist'), has already been attributed to him, as two articles from this review with the same signature were collected in his works (Bartrina 1881: 259-267).[11] The emphasis on Edison's self-education and the pun that the phonograph does not like 'capellans' (a word meaning not only 'a drop of saliva spat while talking', but also 'a priest') are the most remarkable issues.


BARCELONA

LA CAMPANA DE GRACIA.

CADA NÚMERO 2 CUARTOS PER TOT ESPANYA.

Fig. 8: Biblioteca de Catalunya. Barcelona.

Nevertheless, Bartrina considered that 'great discoveries often destroy statements considered *laws*' and, if Edison had respected them, some of his more notable inventions would not exist' (282). However, he thought that the phonograph and other devices were only 'primitive instruments' of 'the true physics', which 'in some centuries will turn into a science' (284).

Having yielded in part the role of speaker to Bartrina, Tomàs J. Dalmau began his experiments. In the same way as Edison and others conducted several testes with the phonograph and the phonographic sheets, at least, he used different materials for the membranes (steel, ivory and pine), positioned the device on top of a resonance box and substituted the clockwork mechanism by a Gramme dynamo. This association, although foreseeable, was attributed to Dalmau himself, who, as is well-known, had imported this machine. As Casas Barbosa ([1879]: 64) noticed, the dynamo improved the regularity of the mechanism, but it could not be a common solution.

After these experiments, the field marshal, Ramon Blanco, who had the support of progressive sectors and had already been present at the inauguration of the society, greeted the phonograph, as if talking to a person, in the name of the Free Athenaeum.

[11] Regarding his religious ideas, see, for instance, 'Rectificación y ratificación', *Gaceta de Cataluña*, 173, 12/11/1878, 2341-2342.

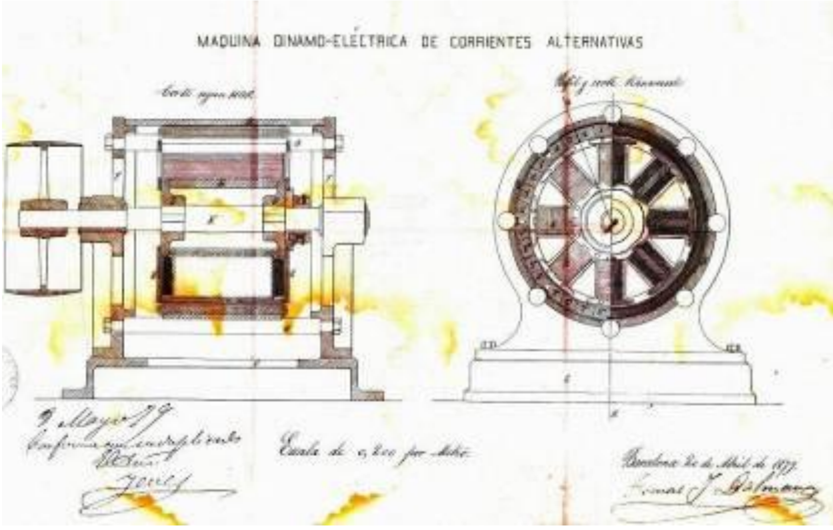

Fig. 9: Drawing of a Gramme dynamo design signed by Tomas J. Dalmau.
Ministerio de Industria, Turismo y Comercio. Oficina Nacional de Patentes y Marcas.
Achivo Histórico. Madrid. File 361.

Then, musical recordings were made. Music, in addition to attracting the interest of some of the Free Athenaeum members, had become the most popular use of the phonograph. Along with literary and other recordings, several languages were employed with the aim of suggesting cosmopolitism: Catalan (one poem by the popular 17th century writer Francesc Vicent Garcia and an idiom), Spanish, Galician, Italian, English, and a dialogue in French and German.

The act was brought to a close with a speech by Bartrina expressing the Athenaeum's gratitude. According to the engraving, a Free Athenaeum member recorded a greeting to Edison. However, the press reported that the actual recording was made three days later at Dalmaus' workshop. The plate was kept in a case with a dedication in Catalan, and was exhibited in the clockmaker Wehrle's shop window. Regrettably, these objects are not conserved at the Edison National Historic Site, according to Paul Israel's and Leonard DeGraaf's kind responses.

Apart from some technical shortcomings, the event received wide exposure, except for the majority of the conservative press. The Dalmau-Bartrina tandem was repeated in other sessions, such as the presentations of the singing condenser and electrical light. Unfortunately, the society, which organized various activities, would disappear in 1882, for multiple reasons.

Dalmau and Bartrina also coincided at the Barcelona Society for the Protection of Animals and Plants, founded the same year as the Free Athenaeum. Moreover, at the time of the phonographic session, both were presided by the Darwinist doctor Joan Giné i Partagàs (the press).

We have abundant references to the phonograph in many fields. Anecdotally, on 28th December 1878, Saint Innocents' Day (equivalent to April Fools' Day), *La Crónica de Cataluña* (601, 2) even published the hoax that a supposed phonograph supplied by Dalmaus' firm was used at the Council of Barcelona.

Focusing on phonographic demonstrations, on 19th December 1878 and 15th January 1879, Tomàs J. Dalmau presented the phonograph and his modifications to it at the Royal Academy of Natural Sciences and Arts of Barcelona (file; minutes; a phonographic recording of the academics' names is conserved).

In April 1879 the phonograph was shown at the Romea and the Principal theatres, as well as in other Catalan cities, by the French conjurer Bargeon, who was criticized for this use of the device (Alier 1985: 345-346; Lloret 1998: 131; the press; Julian Romea's Society published a bulletin, in which some of Bartrina's poems were reproduced).

In the same month, in lectures on experimental acoustics by Rojas, he and Tomàs J. Dalmau demonstrated the phonograph at the Barcelona Athenaeum (Alier 1985: 346-347; minutes; the press). Rojas (1887) would publish an article on this device in a review, of which Edison appears as a collaborator, sponsored by the Spanish Society of Electricity.

On 3rd September 1879, Rafael Roig i Torres presented the project of his inscriptor phonograph, which, like the inventions of others, would record phonetic registers, at the conference of the *Association Française pour l'Avancement des Sciences* in Montpellier (minutes, Roig 1880ab, figure 10), obtaining certain exposure. He would have liked to show the phonograph and other instruments at the Circ theatre during the Mercè festival, yet, due to the lack of support, this was not possible. Nevertheless, in the next year's festival a phonograph and other devices from Dalmaus' firm were shown in Catalunya Square and probably the Jovellanos Theatre.

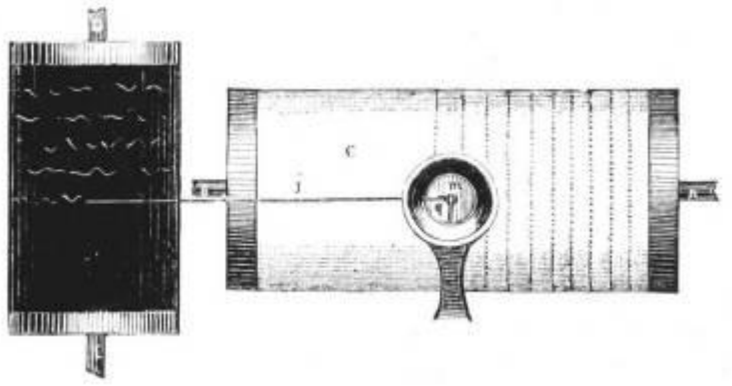

Fig. 10: Roig, 1880a, 228.
Biblioteca de Catalunya. Barcelona.

On 26th October 1880 Tomàs J. Dalmau still demonstrated the phonograph in a soirée organized by the Catalan Association of Excursions (minutes and the press) on the occasion of the Catalanist Congress.

During these years, the phonograph was also shown, often with the help of Dalmaus' firm, in other Catalan cities, such as, in 1879, Vic (Bernat 2010: 222), Mataró (at the Working Cooperative, by Xifra and Roig) and Pont de Vilomara (during its festivals, in a dancing marquee, also by Xifra); in 1880 and 1882, Reus (at the Reading Centre, respectively by Manuel Escudé and Àlvar Bielza), and, in 1881, Roda de Ter (during its festivals, at the Working Class Athenaeum). Of course the presence of the phonograph was increasing, rivalling other devices.

Although in 1880 an article possibly overestimated that the majority of laboratories had phonographs (Truillet 1880: 7) and the next year Dalmaus' firm did a stocktaking of five models (Historical Protocol Archive in Barcelona, notary Luis G. Soler y Pla, 27/4/1881, 1945), the majority of the public or private phonographic collections are from later or abroad.

It has been noted that the phonograph was also imported, constructed, innovated, explained, displayed and demonstrated in Barcelona and other Catalan cities and towns, emulating American and European modernity in a developing economy and contrary to political circumstances. Following an ancient link between science and performance, this device was presented by people from different backgrounds (in general, technicians, but also a writer and a conjurer) to various audiences in several sites (shops, schools, cultural centres, academies, clubs, cooperatives, theatres, fairs, dancing marquees...), on multiple events (specific displays, lectures, performances, festivals, political conferences...). 'Appropriation' (in the meaning established, with precedents, by Roger Chartier) could be spoken of, at least to the extent of using the phonograph to a certain juncture, with the aim of, over and above promoting technological progress, obtaining recognition and ideological acceptance. The analysis of this local case, supplying much previously unknown information from primary sources, illustrates, as well as some general possibilities regarding the circulation of technology, that, if environments are studied thoroughly, a richer scientific and social situation also emerges in the periphery.

References

AGUADÉ I PARÉS, Enric (1924), 'Biografia d'En Joaquim M.ª Bartrina i de Aixemús llegida [...] el 27 de novembre de 1915', *Revista del Centre de Lectura*, 111-112, 1-15/9, 197-204.

ALAYO MANUBENS, Joan Carles (2007), *L'electricitat a Catalunya: De 1875 a 1935*, Lleida: Pagès.

ALCOVER, J[osé]. (1877), 'El teléfono de Bell y el teléfono de repetición de M. Edison', *La Gaceta Industrial*, 23, 10/12, 354-356.

ALCOVER, J[osé]. (1878), 'El fonógrafo de Edison perfeccionado', *La Gaceta Industrial*, 8, 25/4, 119-123.

ALIER, Carlos (1985), 'Les primeres passes del fonògraf a Barcelona', *D'Art*, 11, 345-347.

ALMIRALL, Valentí (1880), '¡Bartrina!', *La Veu de Catalunya*, 7, 5/8, 47. Colleted in *id.*, *Cultura i societat*, Barcelona: Edicions 62, 1985, 75-82.

ALTSHULER, José & GONZÁLEZ, Miguel (1997), *Una Luz que llegó para quedarse. Comienzos del alumbrado eléctrico y su introducción en Cuba*, La Habana: Editorial Cientifico-Técnica.

ARROYO, Mercedes & NAHAM, Gerardo (1994), 'La sociedad española de Electricidad y los inicios de la industria eléctrica en Cataluña', in Horacio CAPEL, *Las tres chimeneas*, Barcelona: FECSA, I, 25-51.

AUSÍN HERVELLA, José Luis (1999), 'L'àmbit sanitari en els inicis del telèfon a Barcelona', *Gimbernat*, 31, 127-132.

BARREIRO, Javier (1992), *Raquel Meller y su tiempo*, [Zaragoza]: Departamento de Cultura y Educación, 125-133.

BARTRINA, Joaquín M. (1881), *Obras en prosa y verso*, Barcelona: Texidó y Parera.

BERNAT, Pasqual (2010), *Científics d'Osona*, Vic: Patronat d'Estudis Osonencs.

CABANA, Francesc (1992), 'Els Dalmau i la Societat Espanyola d'Electricitat', in *id.*, *Fàbriques i empresaris*, I, Barcelona: Enciclopèdia Catalana, 182-198.

CASAS Y BARBOSA, José ([1879]), *Maravillas de la telefonía*, Barcelona: Trilla y Serra.

C[ROFFUT]., W[illiam]. A[ugust]. (1878), 'The Papa of the Phonograph', *The Daily Graphic*, 1570, 2/4, [221] and 223, reproduced many times.

DALMAU, A. R. (1930), 'La primera audición fonográfica que se verificó en España', *Diario de Alicante*, 5927, 9/8, [1].

DALMAU MONTERO, J[osep]. (1956), '¿Quién fue el señor Dalmau?', *Destino*, 993, 18/8, 3.

FEASTER, Patrick (2007), *'The Following Record': Making Sense of Phonographic Performance, 1877-1908*, Ph. D. thesis, Indiana University.

FERNÁNDEZ BREMÓN, José (1877), 'Crónica general', *La Ilustración Española y Americana*, 45, 8/12, [353]-354.

GARCÍA DE LA INFANTA, José María (1986), *Los primeros pasos de la luz eléctrica en Madrid y otros acontecimientos*, Madrid: Fondo Natural.

GLICK, Thomas F. (1983), 'Els catalans i Edison: la llum elèctrica', *Ciència*, 32, November, 52-53.

GÓMEZ MONTEJANO, Mariano (2005), *El fonógrafo en España*, Madrid: the author.

ISRAEL, Paul B.; NIER, Keith A. & CARLAT, Luis (1998), *The Papers of Thomas A. Edison*, IV, Baltimore/London: The Johns Hopkins University Press.

JIMÉNEZ ALBARRÁN, Mª. Josefa & LASTRA GONZÁLEZ, Ignacio de la (2006), '¿Fonógrafos para la enseñanza?: Dos fonógrafos del Instituto San Isidro de Madrid en el Museo Nacional de Ciencia y Tecnología', *Actes de la VIII Trobada d'Història de la Ciència i de la Tècnica*, Barcelona: SCHCT, 291-298.

JONES, Daniel E. & BARÓ I QUERALT, Jaume (1995), *La indústria musical a Catalunya*, Barcelona: L'Índex.

LLADÓ, Josep Maria (1991), 'L'evolució del fonògraf', *Avui*, 6067, 8/12, Sunday supplement, 10.

LUSA MONFORTE, Guillermo (2003), 'La Escuela de Ingenieros Industriales de Barcelona y la introducción de la electricidad industrial en España (1872-1899)', *Actes de la VII Trobada d'Història de la Ciència i de la Tècnica*, Barcelona: SCHCT, 373-284.

MALUQUER DE MOTES, Jordi (1992), 'Los pioneros de la segunda revolución industrial en España: La Sociedad Española de Electricidad (1881-1894)', *Revista de Historia Industrial*, 2, 121-142.

MARTÍN RODRÍGUEZ, José Luís & OLLÉ ROMEU, José María (1961), *Orígenes de la industria eléctrica barcelonesa*, Barcelona: Instituto Municipal de Historia.

NIETO-GALAN, Agustí (2003), 'Under the Banner of Catalan Industry: Scientific Journeys and Technology Transfer in Nineteenth Century', in Ana Simões, Ana Carneiro & Maria Paula Diogo (eds.), *Travels of Learning*, Dordrecht/ Boston/London: Kluwer Academic Publisher, 189-212.

RAMON, Jaume (1899), 'Obras póstumas de Bartrina', *El Vendrellense*, 362, 3/9, [1]. There are further writings concerning Bartrina in this publication.

RICO AVELLO, Manuel & SANTIESTEBAN DE LA FUENTE, Mariano (1864, 4th edition), *Manual de física y química*, Madrid: Eusebio Aguado.

RIUTORT, J. Mª. (1946), *Anecdotario barcelonés ochocentista* [...], Barcelona: Millà.

ROCA Y ROCA, J[osep]. (s. a.), *Memoria biogràfica de Joaquim María Bartrina y d'Aixemús [...] llegida el 24 de maig del any 1916*, Barcelona: Ajuntament.

ROIG I TORRES, R[afael]. (1880a), 'Contribución al estudio de la fonografía', *Crónica Científica*, 58, 25/5, 227-230; also edited as a booklet by this review.

ROIG I TORRES, R[afael]. (1880b), 'Nota sobre el fónografo inscriptor', *Crónica Científica*, 68, 25/10, 480-481.

ROJAS, Francisco de P. (1887), 'Revelaciones de Edison: Su nuevo invento', *La Electricidad,* 22, 15/11, 257-258.

ROURE, Conrat (1878), 'Al nort-america Edisson, inventor del fonógrafo', *La Campana de Gracia*, 449, 28/4, 2-3.

ROURE, Conrat (1916), 'Recordant al amich Joaquim Bartrina', *L'Avençada*, 65, 27/5, 2.

ROURE, Conrat (*s.a.*), *Anys enllà*, Barcelona: La Il·lustració Catalana*.*

SÁNCHEZ MIÑANA, Jesús (2006), 'Las primeras aplicaciones de la electricidad en Barcelona en torno a 1850', *Quaderns d'Història de l'Enginyeria*, VII, 115-165.

SÁNCHEZ MIÑANA, Jesús & LUSA MONFORTE, Guillermo (2009), 'De músico a óptico: los orígenes de Francesc Dalmau i Faura, pionero de la luz eléctrica y el teléfono en España', *Actes d'història de la ciència i de la tècnica*, II (2), 87-98.

SOLÀ, Maria (2006), *Joan Sardà, crític literari de la Restauració*, Barcelona: Abadia de Montserrat.

SUÁREZ SAAVEDRA, Antonio (1880), *Tratado de telegrafía*, Barcelona: Jaime Jepús.

TRUILLET TEIXIDOR, José M. de (1880), 'El hombre y la naturaleza', *El Eco del Centro de Lectura*, 27, 4/7, 5-7.

THE DIALECTIC RELATION BETWEEN PHYSICS AND MATHEMATICS IN THE 20TH CENTURY

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 510.

THE CONSTRUCTION OF GROUP THEORY IN CRYSTALLOGRAPHY

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Abstract

The French molecular approach led to the distinction of fourteen types of lattice system, while "Naturphilosophie" can count 32 classes of symmetry. At the end of the century, the mathematical study led to 230 different groups belonging to 32 classes, 14 networks and 7 systems.
In this paper, I will show how, in the continuity of the founding works of Romé de l'Isle (1772) and René-Just Haüy (1801), introducing a triperiodic crystalline structure composed by "integrant molecules" filling the space (7 systems), Gabriel Delafosse (1843) introduces the concept of the crystal lattice repeating "polyhedral molecules" which do not fill the space.
This view helps explain the "notable exceptions" to the "law of symmetry" of Haüy (the "mériédries"). This approach is generalized in 1848 by Bravais in his study of "systems composed by systems of points" (enumeration of 14 modes of networks ").
Meanwhile, the German crystallographers, based on continuous view of matter characteristic of "Naturphilosophie", describe attractive and repulsive forces fighting in space.
From this idea, they induce the concept of the "axes of symmetry" (Weiss 1805). The combination of these elements of symmetry concurring, discussed by Weiss, continued by Mohs and Hessel, leads in 1830 to the counting of 32 "classes of symmetry".
The combination by Sohncke of networks and those of classes that he knew would lead to the enumeration of 61 various "groups of space" (1879). It is with an entirely mathematical approach, without figure, introducing elements of non-concurrent symmetry and introducing the problem of the regular partition of the sphere, that Fedorov and Schoenfliess manage to count, independently of one another, 230 groups of symmetry (1891).
The determination of groups of space is now complete. It can bring together the concepts of systems, classes and types of networks. The "groups", counted without reference to ideas of Galois and its successors, will play an essential role in physics after the work of Pierre Curie (1894).
It's a whole century of efforts from crystallographers working in various contexts and with different pre-supposed that I retrace.

The history of the construction of Group Theory in crystallography extends over two centuries. It crosses diverse branches of science (natural philosophy, physics, mineralogy, crystallography, chemistry and mathematics), and develops varying characteristics depending on the geographical or cultural backgrounds concerned. Different scientific traditions are set in motion giving rise to the use different methodologies. In the 17th century it is Kepler[1] and Hooke[2] who, amidst the fever of observations and interpretations typical of the birth of a "new science", study with both the naked eye and under microscope regular crystal forms and attempt to explain them through a subjacent structure which remains inaccessible to the best instruments at their disposal. Their efforts are taken up by Christian Huygens[3] who contributes to the creation of a profane, geometric and "hypothetical-deductive" science, based on mechanisms which apply in the world of Descartes: a world filled by an infinitely liquid ether, composed of elements comparable to hard spheres, in contact with one another. He observes calcite, its patterns, its cleavages and birefringence, and shows that all these properties possess a common symmetry. He represents them as a triperiodic assemblage underlying ether-immersed molecules, a structure which slows down the speed of passing light waves. He deduces various physical properties from this hypothesis, which are not yet perceived, but are to be so soon, and accurately predicts the numerical consequences which he verifies through experiment. The development of this rich approach is however brought to a halt by Isaac Newton, partisan of an empty world of discontinuous matter, of a corpuscular theory of light, a Newton who makes full use of his authority, and of false measurements, to discredit it[4].

It is in fact much later, and independently to the work of its precursors, that the history moves forward. It is linked to the Enlightenment, the century of such a prodigious effervescence, inspired by Locke and Malebranche. For them, the "great systems" of mechanics are incomplete: gravity can only be explained, according to Newton, through theology, and life cannot be accounted for merely by a machine. For Locke (1690), human understanding emanates from two sources: on the one hand external objects which provide qualities, and on the other hand perceptions and the mind which supply him with ideas. To increase knowledge, it is necessary to discover significant properties. If the definition of a world system remains the objective, it is an ideal that cannot be achieved without patient analysis. From that point, the focus must be on empiricism and finding in our non-mechanical reasoning new channels for science. The 'truth' is only the temporary matching of all our discoveries. Applying this limit to knowledge guarantees tolerance. Savants and philosophers, deists and atheists, study with passion all that had been neglected by the great systems: polyhedrons, electricity, magnetism, nature and life. The history of natural science becomes a moment in the history of all sciences, a history that is written in parlours, in the "cabinets de curiosité" where naturalists expose their findings or their purchases from their various excursions, and attempt to classify them, turning their attention to crystals. Romé de l'Isle wants to determine their significant properties: he turns his attention to the outer pattern of crystals. To classify them he measures, and notes:

> *"Although the faces of a crystal can vary in their shape and their relative dimensions, the respective inclination of these same faces is constant and invariable across all types."*[5]

It is *the first law of crystallography* (1772).

To explain this "constancy of angles" represented by idealised patterns of crystals, Romé de l'Isle considers them as assemblages of parallelepiped invisible to the microscope, possessing all the qualities of macroscopic crystals, filling space: "integrant molecules". He describes their possible forms and founds what he rightly considers to be a new science – crystallography[6].

Shortly afterwards, another French naturalist, René-Just Haüy carries out the same studies. He silences Romé de l'Isle's precedence and invents the tale of a chance discovery to explain that calcite always cleaves identically, and that it results from the triperiodic assemblage of «molecules intégrantes». In 1784, Haüy abandons the idea of *"establishing crystallography along fixed principles*": the physical and chemical properties of crystals are too diverse. *Forms can only be used in a subsidiary manner*[7]. In 1792, Haüy's studies are completed and diversified. His attention to patterns grows stronger: physicians and mathematicians (Laplace, Monge) show interest, noting to their great satisfaction that geometry can be applied even to natural science[8]. In his 1801 work on Mineralogy, *Traité de minéralogie*, Haüy sets out to describe all the

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[1] Johann KEPLER, *L'Etrenne ou la neige sexangulaire*, trans. Critique by Robert Halleux, Paris, CNRS/Vrin, 1975. The original text dates from 1609.

[2] Robert HOOKE, *Micrographia*, London, 1665, p. 85 – 86.

[3] Chistiaan HUYGENS, *Traité de la lumière*, Leyden, 1690, chap. 5.

[4] Isaac NEWTON, *Opticks*, 1704, Book 3, Question XXV.

[5] Jean-Baptiste ROMÉ DE L'ISLE, *Essai de cristallographie*, Paris, 1772.

[6] Jean-Baptiste ROMÉ DE L'ISLE, *Cristallographie, ou description des formes propres à tous les corps du Règne Minéral*, (4vols), Paris, 1783.

[7] René-Just HAÜY, *Essai d'une théorie des cristaux appliquée à plusieurs genres de substances cristallisées*, Paris, Goguée, 1784.

[8] René-Just HAÜY, *Exposition abrégée de la théorie sur la structure des cristaux*, Paris,1792.

properties of minerals and to classify them[9]. He writes: *"Physics, Chemistry and Geometry … must advance hand in hand along the same route…"* He classifies according to Chemistry, but attaches great importance to patterns: he organises them in six «primitive forms». This enables him to explain the inclination of sides by «truncation» of these patterns according to rational factors, and to explain the absence of regular pentagons in nature by the total filling of space.

In the «Comparative Table of… crystallography and chemical analysis…» (1809), Haüy changes his stance. He writes: *"Chemistry gives us applications; crystallography allows us to distinguish between types… It is only in geometry where all minerals are pure."*

He concludes his work in 1815[10], then in 1822, with his law of symmetry which generalises all his deductions: If the face of a primitive parallelepiped undergoes a certain reduction, or remains identical, the other analogical sides will undergo the same reduction or will remain identical… The same applies to angles. Haüy adds: *"I can prove that it is crystallography that rules the distinction of types and not chemistry*". But some crystals present tetrahedral shapes which do not obey his law. These are considered by Haüy as marvellous exceptions…

It is just these exceptions that German crystallographers set out to explain. Building on Haüy's work, and above all following the tradition of "Naturphilosophie", principally developed by Kant[11] and Schelling[12], they refuse at the beginning of the 19th century the molecular concept of matter. They prefer the theory of continuous matter, divisible to infinity, a space in which reside antagonistic forces, attractive and repulsive, occasionally reaching equilibrium. The place at which the points of equilibrium are reached is a crystal face. Homologous faces are produced by analogous forces, organised along convergent lines. Those with equivalent symmetry correspond to "axes of symmetry", a new concept introduced by Weiss[13]. He defines several convergent axis compatible to crystals, combines them and defines the symmetrical classes, grouping them in crystal systems. Haüy's exceptions are explained by these classes. In 1822, Friedrich Mohs[14] corrects some of Weiss' errors and distinguishes six (seven) crystal systems, whilst in 1830 J.F.C. Hessel completes his predecessor's approach[15]. He defines the direct and inversed axis compatible with crystals, combines them in all possible manners, shows that there can only be 32 classes of symmetry, spread over six (seven) systems. And yet this work is to be ignored for some time to come.

The results obtained in Germany are deliberately ignored by French mineralogists. They are unable to accept the introduction of axis of symmetry, which they could perhaps consider as lines of molecules, but which remain in their minds as vague and incomprehensible philosophical notions. Delafosse and Durozier write:

> *"[German] Mineralogists [are] hostile … to … the … molecular theory [the cause of which needs to be examined]… in the idealist theory: this type of metaphysical consideration which preoccupies all German intellectuals. A few ambiguities taken from the Greeks, a few fallacies based on Kant's contradictions, have led the German physicists to prefer in the study and interpretation of natural phenomena the sort of vague and obscure explanations that they call "dynamic" rather than the simple clear and positive notions that we draw from atomistic hypotheses."*[16]

In 1840 Delafosse takes up Haüy's research, regretting that he did not *"give to his work the character of a theory of physics"*, and explains the "marvellous exceptions" within a newtonian framework, by imagining that molecules of which crystals are constituted are of polyhedron form and repeat themselves in networks[17]. This lattice concept is assured a bright future: in 1848 Bravais lists 14 possible modes, classified into the seven systems[18]. He limits his speculation to "purely geometrical speculation", wishing to protect his work from relying on a newtonian vision of matter at a time when this concept is brought more and more into question. As soon as the following year, however, he applies his networks to crystallography

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[9] René-Just HAÜY, *Traité de Minéralogie*, Paris, 1801, Vol. 1.

[10] René-Just HAÜY, *Traité de Cristallographie*, Paris, 1815, Vol. 1.

[11] Immanuel KANT, *The Metaphysical Foundations of Natural Sciences*, trans. from German by E.B. Bax, London, 1883.

[12] Friedrich von SHELLING, *Von der Weltseele*, Hamburg, 1798, p. 189-219.

[13] Samuel Christian WEISS, *Uebersichlische Darstellung des verschiedenen natürlischen Abteilungen des Kristallisations-systeme*, ADB (1814-1815), pp. 289-344.

[14] Friedrich MOHS, *Grundriss der Mineralogie*, Dresden, 1822-1824.

[15] Johann Friedrich Christian HESSEL, *Kristallometrie oder Kristallonomie und Krystallographie*, Ostwald's *Klassiker der exacten Wissenschaften*, nos 88, 89, Leipzig 1897. The derivation of the thirty-two crystal classes appears in n° 89, pp 91-124.

[16] DELAFOSSE - DUROZIER, *René-Just Haüy*, Biographie universelle, Michaud, 2nd edition. (1843-1865), Vol 18 (1857), p 574-582.

[17] Gabriel DELAFOSSE, «Recherches relatives à la cristallisation considérée sous les rapports physiques et mathématiques», *Comptes-Rendus de l'Académie des Sciences,* , XI, 1840, p. 394-400.

[18] Auguste BRAVAIS, «Les systèmes formés par des points distribués régulièrement sur un plan ou dans l'espace»*, Journal de l'Ecole polytechnique,* XIX (1850) 1-128 (presented to the Academy of Sciences, Dec. 11, 1848).

and is the first in France to introduce axis of symmetry[19]. But it is a German, Sohncke, who extends the interest of the use of lattices through the combination of systems, classes and modes, and he inventories what we call the "*Sohncke groups*"[20]. The identification of groups in crystallography is not yet over. Fedorov introduces elements of symmetry with translation and sliding which leave lattice types invariable[21].

Setting out to achieve a formal mathematic study, he can show that (independently and simultaneously as Schönflies[22]) there are 232 sub-groups in the groups, the classes, the modes and the systems. It is to be noted that all the group plans are represented in the decorative patterns of the Alhambra in Granada (14th Century).

Reflexion on the approach of these crystallographers leads Pierre Curie[23] to take things one decisive step further: no longer limiting it to shapes and the study of forms, but considering symmetry as a reasoning tool, in which form becomes mentally integrated and generates ideas. This will allow the notions of invariants to evolve, which will be greatly used in classical physics, and later at the onset of relativity and quantum physics. The use of groups in Symmetry will also allow the birth and subsequent development of crystallography. In 1912, Von Laüe wants to check both that X-rays are electromagnetic waves and that crystals are networks. Placing a crystal in front of an X-ray source, he obtains diffraction effects. After the work of Debye (1924), it appears that these diffractions can be interpreted using the 230 groups. It is their first practical elucidation, 35 years after their enumeration.

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[19] Auguste BRAVAIS, «Etudes cristallographiques», *Journal de l'Ecole polytechnique*, XX, 1851 (presented to the Academy of Sciences, Feb. 26, Aug. 6, 1849).

[20] Leonard SOHNCKE, *Enwicklung einer Theorie der Kristallstruktur*, Leipzig, 1879.

[21] Evgraf Stepanovich FEDOROV, «Siimetriia Pravil'nykh Sistem Figur», *Zap. Min. Obshch.,* 1885, "The Symmetry of Real Systems of Configurations", *Transactions of the Mineralogical Society*, XXVIII, 1891, pp. 1-146.

[22] Artur SCHOENFLIES, *Kristallsysteme und Kristallstruktur*, Leipzig, 1891.

[23] Pierre CURIE, «Sur la symétrie dans les phénomènes physiques, symétrie d'un champ électrique et d'un champ magnétique», *Journal de physique*, 3ème série, Vol. III, p. 393.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 514.

ON THE BIRTH OF ELECTROMAGNETIC THEORY. FARADAY AND MAXWELL

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Abstract

In the 19th century, a complex system of approaches, models, and theories circulated. In order to interpret mathematically any kind of motion by means of central forces –differential equations–, a mechanical scientific program was presented by Laplace (1749-1827) in Traité de Mécanique céleste *(1805). Fourier (1768-1830) proposed a strong mathematical approach by differential equations (1807; 1822) without considering nature of heat and experiments. For new fields of electricity and magnetism phenomena, Ampère (1775-1836) also presented a mathematical approach (1820; 1828) based on previous Ørsted's (1777-1851) experiments (1820); latter showed that new and non-mathematical interaction, outside of mechanical foundations, could be observed. In this view, the lack of infinitesimal analysis in Sadi Carnot's (1796-1832) theory (1824), in Ørsted's works and in Faraday's (1791-1867)* Experimental Researches in Electricity *(1839-1855) are emblematical expectations. Particularly, Faraday –without any formula– introduced the basis for the concepts of field and vectors in electromagnetic induction theory. After the second half of the 19th century a mathematical approach still strongly emerged. Stressing the mathematics-physics relationship, many theories were included in mechanics becoming new «rational-analytical» theories, where principles «ne présuppose aucune loi physique» and experimental studies were not in attendance. E.g., in the wake of Fourier, propagation, velocity applied, etc… was proposed (1861) by Lamé (1795-1870). In late (electrothermal and) electromagnetic theory an advanced use of mathematics was presented (1864-1873) by Maxwell (1831-1879) to mechanically explain («vortex») new Faraday's phenomena by means of a unique mathematical scheme described by his four equations. Even if he frequently claimed that his work (*Treatise*) is a mathematical interpretation of Faraday's physics, the approach was strongly different: abstract concepts and new mathematical problems. The four equations and Lorentz's law essentially completed the classical electromagnetic theory.*

Some references

CAMPBEL L, GARNETT W (1969) *The life of James Clerk Maxwell*. Kargon RH. Johnson Reprint Corp., NY.

COULOMB CA (1785) Premier mémoire sur l'électricité et le magnétisme. Construction et usage d'une balance électrique.... In: H*istoire et mémoires de l'Académie [royale] des sciences avec les mémoires de mathématiques et de physique*, Partie «Mémoires», pp 569-577.

DARRIGOL O (2005) *Les équations de Maxwell. De McCullagh à Lorentz*. Belin, Paris.

FARADAY M (1839–1855) *Experimental Researches in Electricity*, 3 vols. Taylor, London.

FOX R (1974) The rise and fall of Laplacian physics. *Historical Studies in the Physical Sciences* 4:89-136.

FUFAY CF ([1733] 1735) Quatrième mémoire sur l'électricité. In: Hi*stoire et mémoires de l'Académie [royale] des sciences avec les mémoires de mathématiques et de physique*, Partie «Mémoires» pp 457-476.

GILLISPIE CC (ed) (1970-1980) *Dictionary of Scientific Biography*. Charles Scribner's Sons, New York.

HEILBRON JL (1979) *Electricity in the 17th and 18th Centuries*. The University of California Press, Berkeley.

KRAGH H (2002) The vortex atom: a Victorian theory of everything. *Centaurus* 44:32-114.

MAXWELL JC (1855-1856; 1861–1862) On physical lines of forces. *Philosophical Magazine* XXI:161-175, 281-291, 338-348; *Philosophical Magazine* XXII: 12-24, 85-95.

MAXWELL JC (1864) A dynamical theory of the Electromagnetic field. *Philosophical Transactions* CLV:459-512.

MAXWELL JC (1873) *A Treatise on Electricity and Magnetism*, The Clarendon Press, Oxford.

MAXWELL JC (1890) *The Scientific Papers*. The Cambridge University Press, Cambridge.

ØRSTED HC (1820) Expériences sur l'effet du conflit électrique sur l'aiguille aimantée. *Annales de chimie et physique* 14:417-425.

PEARCE WL (1965) *Michael Faraday: A Biography*. Basic Books, NY.

PISANO R (2003) Il ciclo di S Carnot e la pila di A Volta. *Proceedings XXIII SISFA Congress*, Progedit, Bari, pp 327-348.

POINCARÉ H (1890) *Électricité et optique. Les théories de Maxwell*. Carré. Paris.

POINCARÉ HJ (1897) Les rapports de l'analyse et de la physique mathématique. *Revue générale des sciences pures et appliquées* 8:857-861.

SIMPSON TK (1970) Some Observations on Maxwell's Treatise on Electricity and Magnetism On the Role of the «Dynamical Theory of the Electromagnetic Field» in Part IV of the Treatise. *Studies in History and Philosophy of Science* Part A 1/3:249-263.

TYNDALL J (1868) *Faraday as discover*. Longmans, London.

WHEWELL W (1840) *The Philosophy of the Inductive Sciences, founded upon their History*. 2 vols. Longmans–Green & Company, London.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 516.

MATHEMATICAL PHYSICS IN ITALY IN THE 19TH CENTURY: THE THEORY OF ELASTICITY

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Abstract

In the second half of 19th century there was in Italy an important group of mathematicians busied with mathematical physics. The most prominent of them are Enrico Betti (1823-1892), Eugenio Beltrami (1836-1900), Gregorio Ricci-Curbastro (1843-1925), Valentino Cerruti (1850-1909) and others, whose activity will persevere for many years in the 20th century: Vito Volterra (1860-1940), Carlo Somigliana (1860-1955) and Tullio Levi Civita (1873-1941).

In the paper I speak of the contribution of this group to the theory of elasticity.

Probably the best representative writing on continuum mechanics and elasticity as mathematical physics theories is the book Teoria della elasticità *by Enrico Betti (1872-1873). The book is interesting not only for the particular results found but also, may be mainly, for its framework which will become paradigmatic for the development of subsequent texts on elasticity, not only Italian.*

During the 19th century in Europe the gap between mathematics and physics was already becoming large. Mathematicians were concerned with the 'new' mathematics in the attempt to make it more rigorous and to solve some theoretical problems; physicists were instead mainly concerned with experimental activities. In this context a new branch of learning emerged, now called *mathematical physics*. It originated from some physical problems formulated by means of complex mathematical relations which needed to be solved with such a mathematical skill to be in possession only of mathematicians. Most of them were not interested in the physical aspects of the problems, but only in developing daring mathematical theories which had physical repercussions at most as a secondary scope. This trend became more pronounced towards the end of the century.

Italy followed Europe, even though with some delay. The general scientific level in Italy, at the end of the 18th century was quite low; this was true for mathematics too. During the Napoleonic period things changed. In particular, in the *Istituto nazionale* of Bologna, by means of Vincenzo Brunacci, the new mathematics reached very enthusiastic people that in the following were able to spread mathematics and mathematical physics into Italy.

Vincenzo Brunacci (1768-1818) was a follower of Lagrange. He became professor of mathematics in 1790 at the *Scuola marina* in Livorno and in 1798 published his first relevant work, *Calcolo integrale delle equazioni lineari* (Brunacci 1798). During the Napoleonic period he was member of the *Istituto nazionale* and rector of Pavia University. After a short period of banishment in Paris he came back to Pisa in 1800 and since1801 was professor of *Matematica sublime* in Pavia.

Brunacci transferred his ideas to his students, among which Antonio Bordoni, Fabrizio Ottaviano Mossotti and Gabrio Piola, the most brilliant mathematicians of the first half of the 19th century. Bordoni was an academic and succeeded Brunacci in the chair of mathematics in Pavia; Mossotti after some vicissitudes set up as professor of physics in Pisa; Piola followed a more private path.

Brunacci is then at the top of a genealogy collecting all the greatest Italian mathematicians, in a more or less direct way. In Milan: Piola, Brioschi, Tardy; in Pisa: Mossotti, Betti, Dini, Arzelà, Volterra, Ricci Curbastro, Enriques, etc.; in Pavia: Bordoni, Codazzi, Cremona, Beltrami, etc. (Pepe 2007).

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In the second half of the 19th century there was in Italy an important group of mathematicians busied with mathematical physics. The most prominent of them were Enrico Betti, Eugenio Beltrami, Gregorio Ricci-Curbastro and some others whose activity persevered for many years in the 20th century: Vito Volterra, Carlo Somigliana and Tullio Levi Civita. In the paper I speak of the contribution of this group to the theory of elasticity.

Probably the best representative writing on continuum mechanics and elasticity as mathematical physics theories was the book *Teoria della elasticità* by Enrico Betti. The book is interesting not only for the particular results found, but also, even perhaps mainly, for its structure which became paradigmatic for the development of subsequent texts on elasticity, not only Italian. Betti's interest is concentrated on the mathematical aspects of a physical theory. Physical principles are not discussed, but only exposed in the most formal way possible. The objective is to arrive, without discussing epistemological or empirical problems, to the formulation of differential equations which rule elasticity.

Beltrami has written no complete books on elasticity, but his contribution to this field was perhaps more original than Betti's. A similar discourse holds true for Volterra and Somigliana.

References

BOTTAZZINI U (1982) Enrico Betti e la formazione della Scuola Matematica Pisana. *Atti del Convegno 'La Storia delle Matematiche in Italia'*, Cagliari, pp. 229-27

BOTTAZZINI U (1989) *I matematici italiani e la 'moderna analisi' di Cauchy*. Archi

CAPECCHI D, RUta G, TAZZIOLI R (2006) *Enrico Betti. Teoria della elasticità*, Hevelius, Benevento

CAPECCHI D, RUTA G (2007) Piola's contribution to continuum mechanics. *Archive for history of exact sciences*, vol. 61, p. 303-341.

CAPECCHI D, RUTA G (2010) *La scienza delle costruzioni in Italia nell'Ottocento*. Springer, Milano.

PEPE L (2007) *Rinascita di una scienza. Matematica e matematici in Italia* (1715-1814). CLUEB, Bologna, 2007

REVES BJ (1989) Le tradizioni di ricerca fisica in Italia nel tardo diciannovesimo secolo. In ANCORANI V (editor) *La scienza meccanica nell'Italia post unitaria*. Franco Angeli, Milano.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 518.

THE MATHEMATICAL PHYSICS IN THE STYLE OF GABRIEL LAMÉ: THE ADVENT OF THE «PHYSIQUE MATHÉMATIQUE» IN EMILE MATHIEU'S TREATRISE

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Abstract

As a resumption of Poisson's project of a complete his Traité de Physique mathématique*, Lamé provided an introduction to* Physique mathématique *through a series of papers in the period 1830-1840, and later in four books edited between 1852 and 1861. In the introductions of these books he explained that the* Physique mathématique *has been a creation of the beginning of the 19th century, by men like Laplace, Fourier, Fresnel, Poisson, Cauchy. He strongly claimed for the unity of pure and applied mathematics. He emphasized on the technical unity of mathematics for physics, because there is always almost the same equation to solve: Poisson's or Laplace's equation. At the level of the explanation of phenomena, another unit has to be associated to this technical unity: for Lamé it is the existence of aether. His main technical inventions are the calculus of curvilinear coordinates and especially with ellipsoidal coordinates, and his famous 'Lamé's equation', deeply studied by Hermite later. Lamé's project was re-opened by Mathieu, in some sense a student of Lamé, which put explicitly his work (entitled Traité de Physique mathématique) under the patronage of Fourier, Poisson and Lamé.*

In his paper *Emile Mathieu, his life and works*, Pierre Duhem wrote: «The death of Lamé resulted in finally bringing mathematical physics into discredit in France». But nevertheless we can ask if really, with Lamé, or perhaps finally with its true successor in physics, Emile Mathieu, something is accomplished or finished, under the name of *Physique mathématique.*

In the beginning of the 19th century, Simeon-Denis Poisson had the project of a complete *Traité de Physique mathématique*, with his *Traité de mécanique*, 2 vol. (1811), as an introduction, and with chapters on capillarity (*Nouvelle théorie de l'action capillaire* (1831)), on heat (*Théorie mathématique de la chaleur* (1835)), on elastic bodies and fluids. But this vast project was not achieved. Let us insist on the opinion of Poisson saying that: «l'Analyse pure n'est point le but, mais l'instrument; les applications aux phénomènes sont l'objet essentiel» (Hermite, *Discours à la Sorbonne*, 1889).

Lamé himself provided a kind of introduction to the *Physique mathématique* through a series of papers in the period 1830-1840, and then with four books: *Leçons sur la théorie mathématique de l'élasticité des corps solides* (1852), *Leçons sur les fonctions inverses des transcendantes et les surfaces isothermes* (1857), *Leçons sur les coordonnées curvilignes et leurs diverses applications* (1859), *Leçons sur la théorie analytique de la chaleur* (1861). In the introductions of these books he expressed his opinion that the *Physique mathématique* was a creation of the beginning of the 19th century, by men like Laplace, Fourier, Fresnel, Cauchy.

He also strongly claimed for the unity of pure and applied mathematics, of both theory and practice. But mainly, he wrote on the unity of mathematics for physics, on the fact that for a large variety of problems we have always more or less to solve the same equation; that is Poisson's or Laplace's equation. To this unity at the level of analytical computations, something has to be associated at the level of the explanation of phenomena: for Lamé it is the fact that aether exists. His main technical contribution is the calculus of curvilinear coordinates and especially of ellipsoidal coordinates. In this setting, the examination of the question of the separation of variables led to the famous Lamé's equation, deeply studied by Hermite later.

Lamé's project was re-opened by Emile Mathieu, in some sense a student of Lamé, which put explicitly his work, entitled *Traité de Physique mathématique*, under the patronage of Fourier, Poisson and Lamé. Mathieu gave a *Dynamique analytique* (1878), with a purpose similar to the one in Lagrange's *Mécanique analytique* or Jacobi's *Vorlesungen ûber Dynamik*. In fact, it is explicitly proposed as a sequel to Lagrange. As a prolongation of Poisson's project, and with the help of the works of Lamé, Mathieu also developped a *Traité de Physique mathématique*, including: I -Cours de physique mathématique (1873); II -Théorie de la capillarité (1883); III and IV -Théorie du potentiel (1885), Applications à l'Electrostatique et au magnétisme (1886); V -Théorie de l'électrodynamique (1888); VI and VII -Théorie de l'élasticité des corps solides (1890). In the introduction of his *Cours de physique mathématique* (1873), he wrote that the "physique mathématique" is a branch of mathematics mainly created by the works of Laplace, Fourier, Poisson and Lamé.

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THE INTERACTION OF PHYSICS, MECHANICS AND MATHEMATICS IN JOSEPH LIOUVILLE'S RESEARCH

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Abstract

According to Joseph Liouville, mathematics owes its most important progress to physics, and in particular to mechanics. In fact, many of his own most important research was directly or indirectly inspired by physical problems. In the talk I shall exemplify the physical origin of some of Liouville's theories and results. Some of them are surprising. For example, it is rather surprising that his theory of differentiation of arbitrary order (fractional calculus) was a result of his attempt to find elementary forces in a Laplacian approach to physics, and that his celebrated theorem about the constancy of volume in phase space was inspired not by thermodynamics but by perturbation theory in celestial mechanics. I shall also show the close link existing between Liouville's research in mechanics and his research in differential geometry.
The talk will exemplify the close links between mathematics and physics in France at the beginning of the 19th century.

In a brief biography of Joseph Liouville (1809-1882) his former student Herman Laurent recalled:

> I have often heard Liouville say that it is to the study of natural phenomena and in particular to mechanics that mathematics owes its most important developments, and this truth certainly manifests itself in the memoires of this illustrious mathematician (Laurent 1894 p. 132).

Indeed, Liouville contributed to the following areas of physics:

1. Electrodynamics
2. Heat
3. Various subjects of mechanics:
    a. Rotating masses of fluid
    b. Hamilton-Jacobi mechanics
    c. Potential theory (gravitation and electrostatics)

Moreover his contributions to the following areas of pure mathematics

4. Differentiation of arbitrary order (fractional calculus)
5. Sturm-Liouville theory
6. Geometry

were directly inspired by physics. Let me exemplify some of these interactions:

3a. The shape of a fluid rotating planet had been a major question since the time of Newton. Maclaurin had shown that it could be an ellipsoid of revolution and in 1834 Jacobi announced that also three axial ellipsoids could be equilibrium figures. Liouville studied how these figures depend on the angular momentum of the planet, and discovered that for a momentum for which both types of ellipsoid exist, it is Jacobi's three-axial ellipsoid which is stable. He proved these results using new formulas for the so-called Lamé functions and later showed how these formulas can be used to solve Gauss' version of the Dirichlet problem for an ellipsoid. Moreover he observed that he could generalize this method to arbitrary closed surfaces, if he replaced Lamé functions by another system of "orthogonal" functions that are eigenvalues of a certain integral operator. He did not publish this theory, but it anticipated many results concerning integral operators by at least 40 years.

4. Liouville's theory of differentiation of arbitrary order (i.e. of operators of the form $(d/dx)^a$ where *a* can be an arbitrary complex number) grew out of his work with Ampère's electrodynamic theory and more generally from the fundamental question in Laplacean physics: From observed macroscopic forces to determine the microscopic fundamental force law that will give rise to the observations. Liouville pointed out that this question can be formulated as an integral equation, and he discovered that such equations can in many cases be solved using derivatives of non-integer order. This made him develop a theory of such generalized differential operators.

5. Sturm-Liouville theory grew out of physical problems concerning heat conduction and vibrations.

6. Finally, Liouville's famous theorem according to which any conformal mapping from space into space can be composed of similitudes and inversions in spheres was developed as a result of collaboration and correspondence with William Thomson (later Lord Kelvin) about Thomson's method of electrical images in electrostatics.

References

LAURENT, Herman (1895) "Liouville" *Livre du centenaire de l'École polytechnique I*, Gauthier-Villars, Paris.

LÜTZEN, Jesper (1990) *Joseph Liouville 1809-1882: Master of Pure and Applied Mathematics*, Springer, New York.

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THE EMERGENCE OF MATHEMATICAL PHYSICS AT THE UNIVERSITY OF LEIPZIG

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Abstract

Mathematical physics became a prominent discipline at the University of Leipzig due to C. Neumann and K. von der Mühll in the second half of the 19th century. However, this blossom of mathematical physics caused also a delay in the development of theoretical physics. A professorship for theoretical physics was not established until 1894. In the talk I discuss this development as well as Neumann's views to mathematical and theoretical physics. A comparison of this development with those at other universities reveals interesting differences in the process of establishing theoretical physics in the physics department. Neumann's opinion about mathematical physics played an important role in the discussion about the characterization of mathematical and theoretical physics.

Looking back upon the history of mathematics at the University of Leipzig, we can state a long lasting tradition of mathematical physics. Mathematicians like August Ferdinand Möbius (1790-1868), Carl Neumann (1832-1925), Leon Lichtenstein (1878–1933), Ernst Hölder (1901-1990), Herbert Beckert (1920-2004) or Paul Günther (1926-1996) were representatives of this tradition. The investigations below are concentrated on the upswing of mathematical physics at the end of the 1870s and Carl Neumann's role in this process. The upswing was mainly caused by the appointment of Neumann as full professor of mathematics in 1868 and the appointment of Karl von der Mühll (1841-1912) as lecturer for mathematical physics at the beginning of the same year. Both had attended the famous mathematical-physical school at the University of Koenigsberg and received their doctorate at that university in 1856 and 1866, respectively. Problems in mathematical physics, in particular in electrodynamics, and potential theory became the dominating topics in Carl Neumann's scientific work. Besides the treatment of concrete physical problems Neumann developed and shaped his methodological view on mathematical physics that he realized in several of his investigations. He took the opportunity of describing his views on mathematical physics in his inaugural lectures at the universities of Tubingen and Leipzig in 1865 and 1868. He formed a picture of mathematical physics in his work that was coined by mathematical aspects. However, he overestimated a high mathematical level as a criterion for the quality of a physical theory. This made it difficult for him to incorporate new physical ideas into the explanation of physical phenomena in his own research.

The blossom of mathematical physics at Leipzig University caused a delay in the foundation of theoretical physics at its physical institute. The first upswing of theoretical physics in Germany that took place in the 1870s and 1880s was missed by the Leipzig physicists. The first professor for theoretical physics was appointed at the University of Leipzig only in 1894. It was Hermann Ebert (1861-1913), who became an extraordinary professor but was followed by Paul Drude (1853-1906), an outstanding theoretical physicist, in the same year. Due to Drude's activities, the backlog in the development of theoretical physics was caught up within a few years.

For a closer description see the book BARBIN, Evelyne; PISANO, Raffaelle (eds.): *The Dialectic Relation between Physics and Mathematics in the XIXth century*. Springer Berlin et al. 2011.

CHEMICAL ORDER IN TRANSIT: COMPARATIVE STUDIES OF THE RESPONSE TO THE PERIODIC SYSTEM

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THE RECEPTION OF THE PERIODIC SYSTEM IN DENMARK

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Abstract

In this essay I examine how the periodic system or table was introduced in Denmark in the late nineteenth century, how it was used in chemical textbooks, and the way it was developed by a few of the country's scientists. I pay particular attention to the work of Julius Thomsen, which is an important example of neo-Proutean attempts to understand the periodic system in terms of internally structured atoms. Moreover, I direct attention to Mendeleev's connection to Danish science by way of his membership of the Royal Danish Academy of Sciences and Letters.

Early discussions of the periodic system

The first published recognition of the periodic system among Danish chemists dates from 1880, when the young chemist Odin Christensen reviewed the recent discoveries of chemical elements, including gallium and scandium. In this connection, he discussed the new elements' place in "the system", such as predicted by Mendeleev in the form of the hypothetical elements eka-boron and eka-aluminium. He concluded that gallium and scandium "provide strong support in favour of the view of Mendeleev, namely, that the properties of the elements and the constitution of their compounds are periodic functions of the atomic weights of the elements."[1] Christensen further noted that Emile Lecoq de Boisbaudran's discovery of gallium had taken place wholly independently of Mendeleev's prediction, which he found to be further confirmation of the essential truth of the periodic system.

Christensen was evidently impressed by the agreement between the predictions of Mendeleev and the metals discovered by Boisbaudran and Lars Frederik Nilson. Yet another triumph of Mendeleev's law was the outcome of the controversy concerning the correct classification of beryllium, as a homologue of either magnesium or aluminium. Based upon the law of Dulong and Petit, its atomic weight came out as approximately 14, a value which indicated that beryllium was a tervalent element, in disagreement with Mendeleev's conclusion. Only about 1880 was the element's atomic weight determined to 9.1, which largely settled the controversy. As Christensen saw it, the problem had been solved, with the new atomic weight being a "proof of the great significance that must be ascribed to Mendeleev's periodic law."[2]

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[1] Christensen 1880, p. 421. On the significance of Mendeleev's predictions for the general acceptance of his system, see Brush 1996.

[2] Christensen 1884, p. 311. On the beryllium anomaly, see Van Spronsen 1969, pp. 300-302.

Thomsen and Christensen were not the only Danish chemists who paid tribute to the periodic system in the 1880s. A 23-year old graduate student in chemistry, Rudolph Koefoed published in 1885 an extensive survey article on what he called the periodic law and in which he referred to Mendeleev's as well as Lothar Meyer's works of 1869-71. Like Christensen, he assigned much significance to the successful predictions of gallium and scandium. Ironically, adding to Koefoed's confidence in the periodic system was that it –apparently– resulted in atomic weights in agreement with recent measurements. For example, it was well known that tellurium's atomic weight of 128 conflicted with the periodic system, which required a value about 125. However, as Koefoed was happy to report, recent determinations made by Bohuslav Brauner gave just this value and "thus confirm Mendeleev's prediction."[3]

The system in textbooks and education

While the periodic system seems to have been well known among Danish chemists by the mid 1880s, naturally it took some time until it percolated to the level of education and became part of the teaching of chemistry students. Most university lectures in inorganic chemistry were given by S. M. Jørgensen, who however chose to ignore the periodic system. The system was absent from both the first and the second editions of his textbook on inorganic chemistry, published in 1888 and 1896, respectively.[4] Nor was the system to be found in his 1902 textbook on general chemistry, which was widely used and translated into several languages.[5] In this work Jørgensen listed the chemical elements and their atomic weights alphabetically, without any indication of relations between them. The discoveries of gallium and scandium were duly mentioned, but again without mentioning their relations to Mendeleev's system. This is all the more remarkable in regard of the fact that Jørgensen also disregarded the periodic system in the second edition of 1913. The absence of the periodic system was noted in an otherwise positive review in the German periodical *Naturwissenschaftlicher Rundschau*:

> Neither the periodic system of the elements nor the related question of a primary matter is mentioned in the book. The reviewer is unaware of the author's reason for this reservation, but it seems to him [the reviewer] that this question –which possibly goes deeper into the philosophical foundation of chemistry than any other subject– might well have fitted into the book.[6]

The first university-level textbook to incorporate the periodic system, written by Odin Christensen in connection with his lectures at the university, appeared in 1890 and ran through four editions. Without mentioning Mendeleev by name, he introduced his system in the form of an appendix, not as an organizing principle for treating the properties of the elements.[7] Using "periodic system" interchangeably with "periodic law," his main justification for the classification was its ability to predict new elements in accordance with later experiments.

Another advocate of the periodic system was Emil Petersen, who after studies in Paris and Leipzig had taken up the new physical chemistry of the Ostwald school. In 1889 he gave a lecture series at the University of Copenhagen on the rarer elements, with special emphasis on the problem of their places in the periodic system. By that time he was convinced of the basic truth of the system and also that it reflected an underlying unity of matter.[8] After having been appointed professor of chemistry in 1901, he wrote a textbook in inorganic chemistry in which he included a fairly detailed account of the system. According to Petersen, the periodic system was a useful classification, yet "it is far from a perfect expression of the facts [and] … many deficiencies are attached to it." Among these deficiencies he mentioned the Ar-K and Te-I atomic weight inversions. On the other hand, he was convinced of the importance of the periodic system, not least because "in several cases the existence of elements and their main properties were predicted in advance, many years before they were actually discovered." Rather than mentioning the classic cases of gallium, scandium and germanium, he called attention to the new element radium, "which is very similar to barium and, with an atomic weight of 225, fits nicely into the system."[9]

Among the elementary textbooks intended for the gymnasium schools that appeared in the early part of the twentieth century, some referred to or made use of the periodic system. This was the case with a book written by Julius Petersen, a polytechnically trained chemist who in 1908 was appointed professor of chemistry at the University of Copenhagen. Petersen followed the tradition by emphasizing the successful predictions of elements based on Mendeleev's system, and at the same time he, much like his colleague and namesake Emil Petersen, pointed to its incompleteness and problems such as the Ar-K

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[3] Koefoed 1885, p. 172 and similarly in Petersen 1890, p. 24. On the tellurium-iodine inversion problem, see Van Spronsen 1969, pp. 238-240 and Scerri 2007, pp. 130-131.

[4] Jørgensen 1888.

[5] Jørgensen 1902, with translations into German (1903), Greek (1904), Italian (1904) and English (1908).

[6] Review by "R.M." in *Naturwissenschaftlicher Rundschau* 19 (1904), 271.

[7] Christensen 1890.

[8] Petersen 1890.

[9] Petersen 1902, pp. 317-321.

and Te-I atomic weight anomalies. Another book for the gymnasium, written by the teacher Hans Rasmussen, is noteworthy because it presented the periodic system in the unconventional form suggested by Julius Thomsen, with vertical groups and horizontal periods.[10] The pedagogical value of the system was not always appreciated, and some teachers suggested that it, being too theoretical, should not be part of the curriculum. It took until 1958 before the periodic system became a formally required part of the Danish gymnasium education system.

Speculations on the complexity of atoms

A pioneer of thermochemistry, Julius Thomsen was first and foremost an experimentalist. Yet he also had an interest in chemical theories, and he was the only Danish scientist who, until Bohr in 1913, actively examined and contributed to the understanding of the periodic system. As mentioned, ever since the 1860s he entertained the heterodox view that the atoms of chemistry are complex particles and that this is revealed by regularities in their atomic weights. In a work of 1887 he connected for the first time this idea with the periodic system, undoubtedly inspired by an address that William Crookes had given the year before to the British Association for the Advancement of Science.[11]

Thomsen was particularly concerned with the question of why only some atomic weights are realized in nature, while other possible weights seem to be missing. An ardent advocate of so-called inorganic Darwinism, he thought that the answer was to be found in the slow evolution of elements from simple to more complex structures. “The right of the fittest has manifested itself and only allowed the formation of atoms with a structure firm enough for a continuous existence,” he wrote.[12] As to Mendeleev’s system, he praised it for its ability to identify missing elements and predict their properties, such as had been the case with gallium, scandium and germanium. Contrary to Mendeleev and most other chemists, he was convinced that the system was a key to understand the complexity of the elements and that it would eventually be possible to represent it as a mathematical function of the atomic weight. Like the Irish chemist Thomas Carnelley had done the year before, Thomsen suggested an analogy between the chemical elements and the hydrocarbon radicals.[13]

The questions addressed by Thomsen were taken up also by Emil Petersen, who in 1890 discussed the nature of the chemical elements and the idea of a basic unity of matter such as discussed by Crookes and others.[14] Evidently in sympathy with the idea, he suggested that it received support from the periodic system. “It is hardly to doubt,” he wrote, “that in this way we will eventually get insight into the unity that lies behind the varied diversity of the elements.” Petersen admitted that the dream of a primary matter was somewhat speculative, but he nonetheless found the dream worthy of pursuit. Whether in Mendeleev’s or Meyer’s version, he thought highly of the periodic law, which he summarized in the formula “The properties of the elements stand in a periodic relationship to the atomic weight.” He explained that there were two major reasons for accepting the truth of the law, the one relating to its unifying power and the other to its predictive power:

> It is the merit of the periodic law that it has arranged all known elements –and in some cases also unknown elements– in one coherent system and demonstrated the intimate mutual relationship between their properties. … These and other applications of the system are of considerable scientific importance. Another application of the system is less important, but on the other hand more striking and amazing, namely, its ability to predict as yet undiscovered elements –to predict their existence and most important properties years before there were actually discovered and manufactured.[15]

That is, according to Petersen the scientific value of the periodic law was primarily its ability to arrange the elements into a coherent system, whereas he gave lower priority to its predictive power. No other Danish chemist expressed a similar view.

To return to Thomsen, in a memoir of 1894 published by the Royal Danish Academy of Sciences, he offered a detailed examination of the atomic weights and their significance. His purpose was to establish that they, if only properly interpreted, revealed that “the so-called atoms of our elements have evolved out of combination of particles of a common basic substance.”[16] He did not on this occasion discuss the relation to the periodic system, but this is what he did the following year, in a paper in which he proposed a new classification of the elements.[17] From a formal point of view,

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[10] Petersen 1907. Rasmussen 1912.

[11] Thomsen 1887. Crookes 1886.

[12] Thomsen 1887, p. 37. On Thomsen’s neo-Prouteanism and inorganic Darwinism, see Kragh 1982.

[13] Thomsen 1887, p. 22. Carnelley 1886.

[14] Petersen 1890.

[15] Petersen 1890, pp. 22-23.

[16] Thomsen 1894, p. 324.

[17] Thomsen 1895a.

Thomsen's innovation was merely to reverse periods and groups, which was not entirely original since versions of this kind had been proposed earlier, by Thomas Bayley in 1882 and Carnelley in 1886. However, in 1894 Thomsen was unaware of these two systems, such as he stated in a letter to the American chemist Francis Venable, who in a book of 1896 described Thomsen's system in some detail.[18]

Thomsen designed his version of the periodic system in such a way that it immediately suggested a common origin of the elements, that is, an evolutionary interpretation. Irrespective of such an interpretation, it included several novel features and indicated the existence of possible new elements. For example, it was the first version of the periodic system that included the correct number of rare earth metals, namely 14, and placed this group between cerium and an unknown element of atomic weight 180 with chemical properties analogous to those of zirconium. This hypothetical element –later identified as hafnium– also implicitly appeared in Mendeleev's original periodic system of 1869, but it was only with Thomsen that it was given explicit attention and placed outside the rare earth group.

Another feature of Thomsen's brief paper deserves mention, namely the "curious fact" that the number of elements in the periods are 1, 7, 17 and 31. These numbers, Thomsen pointed out, can be written as 1, 1 + 2×3, 1 + 2×3 + 2×5, and 1 + 2×3 + 2×5 + 2×7. Expressed slightly differently, the number of elements follows the expression $N = 2n^2 - 1$, or, if the inert gases are included, $N = 2n^2$. "Is this relation more than a coincidence," Thomsen asked, cautiously answering that, "Only the future will show, but I have nevertheless wished to expose the possibility of a more profound cause."[19] He probably referred to a systematic arrangement of the proto-atoms of which he assumed the elements to be built up, such as he had indicated in his essay of 1887. The numerical law suggested by Thomsen came to be known as Rydberg's rule, after the Swedish spectroscopist Janne Rydberg who proposed it in different forms in works of 1906 and 1913. Apparently Rydberg was unaware of Thomsen's earlier speculation.

The position of the inert gases

It is well known that the discovery of argon in late 1894, and also of helium half a year later, caused a major problem for the periodic system. The problem was not only that there was no natural place for argon, but also that the new gas appeared to be mono-atomic and with an atomic weight of 39.9, greater than the one of potassium. However, the crisis disappeared and was turned into a triumph when it was realized that the new inert gases could be added as a separate group of zero-valence elements. This was an important test for the still young periodic system, and it has been suggested that the successful incorporation of the inert gases was of no less importance for the authoritative status of the system than the earlier predictions of metallic elements.[20]

The problems that emerged with the discovery of argon were known among Danish chemists and reflected in their works. It were these problems that induced Thomsen to "publish some ideas, with which I have been occupied for years, but which I have wished not to publish until now, because I would not encumber science with unverifiable hypotheses."[21] The ideas he referred to were probably mathematical relations between the electrochemical character of the elements and their atomic weights. From such considerations Thomsen argued that there supposedly existed a new group of chemical elements that were electrochemically indifferent and possessed zero valence. Moreover, based on his new and still unpublished periodic system he suggested that the atomic weights of the elements –of which only argon was known at the time– were 4, 20, 36, 84, 132, and 212. For the seventh period he proposed that it would end with a noble-gas element of atomic weight 292. Like several other scientists at the time, Thomsen searched for a mathematical representation of the periodic system, and he thought that his new extension of the system was a step in the right direction.

In his essay of 1887, Thomsen speculated that the hypothetical solar element, helium, might be a subhydrogenic primary element. When he read his paper on the inert gases to the Royal Danish Academy on 19 April 1895, William Ramsay had not yet announced his discovery of helium in terrestrial sources. Helium initially raised questions with regard to its place in the periodic system, but after a couple of years it was realized that it and argon belonged to a new group, in agreement with Thomsen's proposal. Thomsen kept an interest in the inert gases. In 1898 he gave an address to the 15th Scandinavian Meeting of Natural Scientists, held in Stockholm, in which he emphasized the scientific importance of what appeared to be a new group of gases belonging to the periodic system.[22]

Thomsen was not the only Danish chemist with an interest in the new gases. In a survey article addressed to Scandinavian pharmacists of June 1895, Emil Petersen discussed the sensational discovery made by Ramsay and Lord

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[18] Venable 1896, pp. 209, 271-276.

[19] Thomsen 1895a, p. 136.

[20] Scerri 2007, p. 156.

[21] Thomsen 1895b, p. 283.

[22] Thomsen 1898.

Rayleigh.[23] In agreement with the "two distinguished British chemists," he concluded that the evidence spoke in favour of argon being mono-atomic and with an atomic weight close to 40. He was confident that there was no fundamental disagreement between argon and Mendeleev's system. After having discussed various solutions to the problem of argon's place in the system, he ended up with suggesting that the standard version of Mendeleev's system was probably incomplete.

Conclusions

Although dating from 1869, Mendeleev's periodic system was only explicitly noticed by Danish chemists about a decade later. By the mid-1880s it seems to have been broadly known and also accepted as a useful classification by many chemists. The delay in the reception was not unusual, if compared with other small countries, and it does not indicate any particular backwardness of chemistry in Denmark. The system or law was considered of interest only by relatively few chemists, whereas it tended to be ignored by the majority who worked within the more practical fields of chemistry, such as related to engineering, pharmacy and dairy products. Generally speaking, one should be careful not to confuse lack of references in the literature with ignorance: Although the periodic system was not mentioned very frequently by Danish chemists, this does not mean that it was unknown or considered unimportant.

One indication of the status that Mendeleev, and by implication the periodic system, enjoyed in Denmark is that on 5 April 1889 the Russian chemist was elected a foreign member of the Royal Danish Academy of Sciences and Letters. Julius Thomsen served at the time as president of the Academy, and it appears to have been on his initiative that Mendeleev was invited to become a member of the prestigious society. The letter of motivation was written by Thomsen and signed jointly by him and S.M. Jørgensen. Denmark's two leading chemists, both of them of international repute, motivated their proposal as follows:[24]

> Mendeleev's name has become even more generally known by his brilliant work on the theory of how the chemical and physical properties of the elements depend on their atomic weights –the so-called periodic system. In this way he has opened a wide field for a philosophical discussion of the most important chemical phenomena; his theories have several times been remarkably confirmed by the discovery of elements whose existence and most important properties he had predicted as a consequence of the system. Objections can indeed be raised against the full justification of the system, such as can be done against many other theories; but the system has, to a very high degree, advanced chemistry as a science, and for this reason Mendeleev's name will for ever be inscribed among the first in the history of chemistry.

As seen from the perspective of Danish chemists, the periodic system was of importance primarily because of its successful predictions of new elements. It was this feature which provided the system with a measure of credibility and authority. Because the predictions were associated with Mendeleev and his version, rather than the versions of Meyer and others, the periodic system was invariably associated with the name of the Russian chemist. Whereas the periodic system did not appear prominently in Danish academic textbooks in chemistry between 1880 and 1900, it was introduced in elementary textbooks at a relatively early date. By 1910, most Danish students in the gymnasium schools would have encountered the system, if only in its most rudimentary form. On the other hand, both in university and gymnasium level textbooks it typically appeared isolated from the systematic description of the elements and their properties.

Given the vast difference in the amount of consulted sources, whether textbooks or articles, it is problematic to compare the case of Denmark with Stephen Brush's much more detailed study of the reception in the United States and Britain. Nonetheless, I think two comments may be appropriate. First, among Danish chemists the prediction of new elements was generally given more attention than the correlation between the physico-chemical properties and atomic weights. This is contrary to what Brush found in his survey. Second, Brush observes that in chemistry textbooks the periodic system was "not as a rule introduced at the beginning or used as an organizing principle for those books."[25] This conclusion fully agrees with my more limited study of the Danish case.

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23 Petersen 1895, p. 238.

24 Letter in Thomsen's handwriting to the Academy's secretary of 25 February 1889. In Danish, author's translation. Archive of the Royal Danish Academy of Science.

25 Brush 1996, p. 612.

References

BRUSH, Stephen G. (1996). ”The reception of Mendeleev's periodic law in America and Britain,” *Isis* 87, 595-628.

CARNELLEY, Thomas (1886). “Suggestions as to the cause of the periodic law and the nature of the chemical elements*,” Chemical News* 53, 169-172.

CHRISTENSEN, Odin T. (1880). “Nogle i de senere Aar opdagede Grundstoffer,” *Tidsskrift for Populære Fremstillinger af Naturvidenskaben* 27, 417-429.

CHRISTENSEN, Odin T. (1884). “Berylliums Atomvægt,” *Tidsskrift for Physik og Chemi* 23, 310-311.

CHRISTENSEN, Odin T. (1890). *Grundtræk af den Uorganiske Kemi* (Copenhagen: Wilhelm Priors Forlag).

CROOKES, William (1886). ”Address, president of section on chemical science,” *Report, British Association for the Advancement of Science*, 558-577.

JØRGENSEN, Sophus M. (1888*). Mindre Lærebog i Uorganisk Chemi* (Copenhagen: Gad).

JØRGENSEN, Sophus M. (1902). *Kemiens Grundbegreber Oplyste ved Exempler og Simple Forsøg* (Copenhagen: Gad).

KOEFOED, Rudolph G. (1885). “Den periodiske Lov,” *Tidsskrift for Physik og Chemi* 24, 161-174.

KRAGH, Helge (1982). “Julius Thomsen and 19th-century speculations on the complexity of atoms,” *Annals of Science* 39, 37-60.

PETERSEN, Emil (1890). “Grundstoffernes Natur,” *Naturen og Mennesket* 4, 13-32.

PETERSEN, Emil (1895). “Om Argon,” *Nordisk Farmaceutisk Tidskrift* 2, 233-240.

PETERSEN, Emil (1902). *Lærebog i Uorganisk Kemi for Begyndere* (Copenhagen: Schubotheske Forlag).

PETERSEN, Julius C. (1907). *Kemi for Gymnasiet* (Copenhagen: Gjellerup).

RASMUSSEN, Hans (1912). *Kemi for Gymnasiet* (Copenhagen: Gyldendal).

SCERRI, Eric (2007). *The Periodic Table: Its Story and Its Significance* (Oxford: Oxford University Press).

THOMSEN, Julius (1887). *Om Materiens Enhed* (Copenhagen: J. H. Schultz).

THOMSEN, Julius (1894). ”Relation remarquable entre les poids atomiques des éléments chimiques,” *Royal Danish Academy of Sciences and Letters, Oversigt*, 325-343.

THOMSEN, Julius (1895a). “Classifications des corps simples,” *Royal Danish Academy of Sciences and Letters, Oversigt*, 132-136.

THOMSEN, Julius (1895b). “Über die mutmassliche Gruppe inaktiver Elemente,” *Zeitschrift für anorganische Chemie* 8, 283-288.

THOMSEN, Julius (1898). “Nogle Resultater af de seneste Aars Naturforskning,” *Förhandlingar vid det 15de Skandinaviska Naturforskarmötet*, 66-67.

VAN SPRONSEN, Jan W. (1969b). *The Periodic System of Chemical Elements: A History of the First Hundred Years* (Amsterdam: Elsevier).

VENABLE, Francis P. (1896). *The Development of the Periodic Law*. Easton, Pa.: Chemical Publishing Company.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 531.

PICCINI, CIAMICIAN AND THE PERIODIC LAW IN ITALY

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Abstract

The paper discusses the reception of the periodic law in late nineteenth-century Italy. The research main objective is to fill a serious gap in the ambit of studies in the history of chemistry in Italy in the period that runs from the mid-nineteenth century to the early twentieth century. Despite the unquestionable level of professionalization and specialisation that the historiography of science and technology has attained also in Italy, the temporal arch that the study intends to consider, still today represents a largely unexplored territory, firstly for the lack of a systematic exploration of primary sources, such as collections of letters, manuscripts and archive documents.

In the period considered, despite a series of delays and shortcomings mainly on the structural and organisational levels, Italy played a leading role thanks to the work of several extraordinary protagonists like Stanislao Cannizzaro, Giacomo Ciamician, and Augusto Piccini. If the relationship between Cannizzaro and Mendeleev are well known enough, we have more to learn about Ciamician and Piccini. For example, a fundamental spectroscopic study of the young Ciamician about spectroscopic analogies between elements in the same group was remembered by Mendeleev in his 1889 Faraday Lecture; at the same time, Piccini studied with originality many aspects of the periodic law, and he was in correspondence with Mendeleev.

The study will especially concentrate on the development of Mendeleev's ideas in Italy under the profile of theoretical development, as well as in relation to the growth and definition of its pedagogical uses by means of a survey of Ciamician and Piccini notes and textbooks.

The first encounter between Mendeleev and Italy occurred in October 1860, after the Karlsruhe Congress. Together with his friend Borodin, who also was a member of the Russian delegation at the Congress, Mendeleev crossed over the St. Gotthard Pass and travelled to Genoa, where he spent the night of 15th October. The following day, they both left for Civitavecchia. Mendeleev liked Italy very much; he wrote: "We enjoyed Italy so much after the suffocating and reserved life spent in Heidelberg". On arriving in Rome, Mendeleev and Borodin visited the Basilica of St. Peter and the Sistine Chapel, where they admired not only the frescoes of Michelangelo, but also attended a solemn mass celebrated by the Pope.

The years passed and Mendeleev became known as a scientist and one of the most important Italian scientific institutions, the Reale Accademia dei Lincei,with its seat in the capital, nominated him a Corresponding Member (1893). To this acknowledgement was added that of the Reale Accademia delle Scienze of Turin (1893) and, some while later, that of Bologna (1901).

After his death in St. Petersburg on the 2nd February 1907, Mendeleev was commemorated at the Lincei by Raffaello Nasini, a physical chemist of considerable prestige. According to Nasini [1], the ideas of Mendeleev started to be taken seriously in Italy around 1878, especially thanks to Cannizzaro, who was in charge of the Istituto Chimico of Rome. As he declared, Mendeleev owed a lot to the so-called pamphlet of Cannizzaro ("Sunto") distributed at the Karlsruhe Congress of 1860. In 1880, the Tuscan Augusto Piccini joined the Cannizzaro group as an assistant. It was probably Cannizzaro that entrusted the study of the periodic system to Piccini and Francesco Mauro. The two chemists undertook experimental researches on certain rare elements but, as we will see, it was Piccini above all who stood out, introducing an integration to the periodic system necessary to support the classification and diffusing in Italy the ideas of Mendeleev in his writings. At the same time as Piccini, Giacomo Ciamician joined the Cannizzaro group and the two became friends so much that on the death of Piccini, it was Ciamician who pronounced the funeral oration.

In this communication, a short biographical profile of both men will be presented and it will be seen why their names have remained connected to the success of the Mendeleev system in Italy.

Nevertheless, precedence must be given to an unfortunate previous attempt of Piccini's work. It was the initiative of Paolo Tassinari (1829-1909), a chemistry professor in Pisa and a brilliant teacher and author of one of the first Italian didactic tests containing the atomic theory. Before the French translation of Mendeleev's work was published, Professor Tassinari, as soon as he heard the news of the discovery of gallium (1875), urged a young Russian born student in his laboratory called Alessio Alessi (Moscow 1857 - Reggio Emilia 1934) to translate it into Italian. Alessi asked Mendeleev's permission to publish it; he agreed, but it was not easy to find the means. He entrusted the publication to a small journal called the *Monitore dei Farmacisti* and after numerous hesitations it was decided to publish a page every now and again, unfortunately with various typographical errors. Discouraged, and once the French translation came out, Alessi interrupted his publication.

Augusto Piccini

He was born in 1854 in San Miniato in the province of Pisa and graduated in chemistry at the University of Padova in 1876 [2]. After several years spent at the School of Cannizzaro, in 1885 he was called to teach general chemistry at the University of Catania. Two years later, he returned to Rome to teach docimastic chemistry at the School of Application for Engineers in Rome. Finally, in 1893 he transferred to Florence where he was called to teach Pharmaceutical and Toxicological Chemistry at the Istituto di Studi Superiori.

Piccini applied himself to organic chemistry and then to chemical analysis, and successively started to cultivate a special commitment to inorganic chemistry. He soon demonstrated a complete familiarity with the system by a dissertation on the oxidisation of titanic acid published in 1882 in the Records of the Reale Accademia dei Lincei [3], that was followed by a similar article in the *Gazzetta Chimica Italiana* in 1883. To understand the work of Mendeleev, Piccini started to study Russian in 1883. From that moment, as Giulio Provenzal described, who was a student of Piccini from 1892 to 1895, Piccini became "the best exhibitor and at the same time the best critic of the periodic system". Above all, he succeeded in integrating his translation of the "Treaty of Inorganic Chemistry" by Victor von Richter with a long appendix (55 pages; 30 of which were dedicated to the system) which was published in 1885 [4]. Piccini particularly dwelled upon the controversies of limit forms and *extra limits* of the elements and particularly asking if peroxides (BaO_2 and others) should also be taken into consideration, because in that case it would be necessary to shift the maximum limit. After a general review of the groups, he concluded by suggesting a closer study of the dependence of properties to combination form; a dependence that creates "in one same element a quantity of partial relations with many others that are different". His objective was to construct a "large picture in which the truths discovered by Mendeleev would be harmonised with those that in his time would be discovered". Giulio Provenzal lived the emotion of that moment with the master of the discovery of the first noble gases: "a new and huge problem with regards to the periodic system". The discovery did not affect Piccini's faith in the Mendeleev system. In 1903, Piccini published in the first volume of the "New Chemical Encyclopaedia" [5], run by the chemist Icilio Guareschi, one of the most important historical articles of Italian chemistry; a long article that would have represented not only his scientific testament, but also an extraordinary work of popular science of Mendeleev's work. The Tuscan chemist explained: "For more than thirty years the periodic law has received secure confirmations and questionable confutations. No fact, definitively verified, is discovered to invalidate the spirit and the letter". Naturally, everything was meant as "healthy criticism", without demanding more from the periodic law than it could promise. The same Mendeleev, wrote that Piccini, "also recognising that the analogy concept of the elements, summarised in the identity of the combination limit, is that which is destined to remain in science", was aware that "the theoretical foundation of the periodic law" was missing. In short, the periodic law was like the "laws of Kepler waiting for Newton to explain it".

The presentation of Mendeleev's work was carried out by Piccini, above all in the fifth chapter of *Numerical relationship between atomic weights and the elements.* In this chapter, the Tuscan chemist retraced the history that brought to the "idea of establishing the true function that connects all atomic weights with the physical and chemical behaviour of all elements". Piccini supported the view that precursors to Mendeleev's work did not exist. The work of the Russian chemist was an absolutely original creation. After dwelling on the theory of William Prout, Piccini dedicated ample space to the exposure of Mendeleev's dissertations of 1869 and 1871 that were compared to the dissertations of Lothar Meyer. At the end of the examination, Piccini's judgement was unequivocal: "Meyer did not discover the periodic law, either independently or dependently of Mendeleev". According to Piccini, Mendeleev's work was comparable to that of Lavoisier, also an author of a revolution, because he knew how to "interrogate facts known for a long time and facts just discovered, forcing them to reveal their jealously guarded secrets".

The exposure of the real periodic system followed the historical part, with the presentation of the most recent Mendeleev's table. In the final part of the article, Piccini also dealt with the difficult question of "objections to the periodic law". In particular, he again dwelled upon the problem of the choice of "combination limit", summarising his contribution to the discussion and investigations on peroxides, claiming them to be a priority.

Mendeleev gave credit to Piccini for the diffusion of the periodic system. In a letter sent to Piccini on the 29th January 1903, Mendeleev wrote: "I am really pleased to see a scientist in a far-away country that is so profoundly well-versed in the principles of the Periodic System".

Giacomo Ciamician

Giacomo Ciamician (Trieste 1857 - Bologna 1922) was the first Italian chemist presented as a candidate for the Nobel Prize. On the occasion of 150th anniversary of his birth, Bologna hosted a scientific meeting and one of us (M.T.) reported new biographical studies useful for understanding his scientific career [6]. Ciamician's name could be placed in relation to Mendeleev's periodic system for:

1. The early researches on spectroscopy conducted in Vienna.
2. His activity as Chemistry Professor at Bologna.

Ciamician studied in Trieste and Vienna and graduated in Giessen in 1880. During the Viennese period, Ciamician the student committed himself to cultivating various scientific interests, frequenting certain laboratories of the Polytechnic. The precociousness of the genius is deducted by approximately fifteen communications, partly published between 1875 and 1879. Of particular interest were the researches on spectroscopy that ended in the physics laboratory run by Professor Pierre, when Ciamician was not even twenty years old. The results were published in the *Wiener Sitzungsberichte* signed by "G. Ciamician, *Chemical Student*". The work that was of major interest was "Spectra of chemical elements and their compounds" that was followed by the effect of pressure and temperature on emission spectra. With the first work, Ciamician highlighted the spectral analogies between elements of the same group of the periodic system, achieving the analogy that was defined as the *law of homology* and overtaking the previous researches of Huggins and Thalén relative to emission spectra of metals, as well as that of Salet on non-metals. In 1880, he extended his research of analogies to 20 elements and to a certain number of compounds. He published the new works in the *Monatshefte für chemie* and also illustrated the equipment utilised. Ciamician found that the spectral lines of chemically similar elements (e.g., O, S, Se, Te) were comparable singly or in groups. He concluded that a natural group of elements had its characteristic spectrum, that differed from the single members of the same group, except for the fact that the homologous lines (*lines that fluctuate in the same measure from discharge to discharge*) shifted towards one side or the other side of the spectrum. This meant that the wavelengths of the transitions increased or decreased and that, amongst other things, certain lines or groups of lines disappeared. In the second work: "The influence of density and temperature on vapour and gas spectra", he analysed the spectral behaviour of chlorine, bromine and iodine, as well as the vapours of mercury and sodium. He noticed that the spectra of elements appertaining to groups of halogens changed notably, depending on the temperature and pressure conditions, with regards to the number of spectral lines and the intensity of the same. For this reason, different spectra (partial spectra) can be distinguished for each of the elements, corresponding to different pressure intervals from which a complete spectrum of the element can be reconstructed. Taking into examination the variable lines caused by pressure, he formulated the relation between the various partial spectra of different elements. For example, he saw that the spectrum of diluted bromine vapours came closer to the spectrum of chlorine the more he diluted the vapours, whilst the spectrum of concentrated bromine vapours seemed more comparable to the spectrum of iodine. If all the lines that appear distributed in the various partial spectra for each element of the halogen group are grouped together and make a complete spectrum, he noted that the three complete spectra were perfectly similar, with a decrease in the wavelength of the homologous line passing from chlorine to iodine. Mendeleev acknowledged the contribution of Ciamician on the confirmation of the periodic system in the "Principles of Chemistry". In the 1891 German version, two citations of Ciamician's work are found.

Also in the article "The Periodic Law of the Chemical Elements" by Dmitrii Mendeleev, published in 1889 by the *Journal of the Chemical Society* and taken from the Faraday Lecture, Mendeleev recognised the contribution of Ciamician. N.B. "A distinct periodicity can also be discovered in the spectra of the elements. Thus the researches of Hartley, Ciamician, and others have disclosed, first, the homology of the spectra of analogous elements; secondly, that the alkaline metals have simpler spectra than the metals of the following groups; and thirdly, that there is a certain likeness between the complicated spectra of manganese and iron on the one hand, and the no less complicated spectra of chlorine and bromine on the other hand, and their likeness corresponds to the degree of analogy between those elements which are indicated by the periodic law" [7].

After the degree at Giessen, Ciamician joined the group of Stanislao Cannizzaro in Rome, where he continued with brilliant researches on pyrrole started in Vienna (Ciamician-Dennstedt reaction, 1881), and started to study photochemistry. He became extraordinary professor in Padua in 1887 and after only two years, was called to hold the chair of General Chemistry at Bologna. At Bologna, Ciamician carried out important researches in the field of photochemistry and the chemistry of natural substances and plants. Ciamician took great care in teaching and his lessons earned the applause not only of the students but also of citizens who came to listen as unregistered students. Ciamician never collected his lessons into books or in lecture notes. The lessons of his last academic year in which he taught (1920-1921) were reconstructed by Bruno Ghetti. Previously, notes of his lessons were collected by students Bruno Maggesi and Andrea Stagni and were repeatedly checked until approximately 1915. In these notes, the periodic system occupied less than eleven pages. It was preceded by three pages dedicated to the "Theory of Valency" that were followed by nine pages of "Systematics". Ciamician started his presentation defining the periodic system of elements: "in the field of chemistry, one of the most successful attempts of expressing the qualifying differences via numerical data between the various objects of the study". After dabbling with the theory of triads, Newland's law of octaves and the studies of Lothar Meyer, Ciamician passed immediately to Mendeleev and he remembered the guiding concept: "a connection exists that algebraically we still ignore, between the atomic weight of an element and its property". He recounted that Mendeleev, distributing the elements according to the order of increasing atomic weights (H being a unit of measure), had observed an evident regularity corresponding to the distribution in periods, and a repetition with the same characters of the physical properties and the chemical behaviour. This regularity appeared to stop after potassium because seventeen termini were required, and not seven, to arrive at the halogen bromine. In this series, two sub-periods of seven termini each could be seen, joined together by three new "transitory" termini between Mn (negative) and Cr (positive). The same situation repeated itself approximately between bromine and iodine. The periods of seven termini were defined as "small periods" and the others as "large periods", with each period subdivided into sub-periods of seven termini. Taking the elements on the same horizontal line, groups of "bodies" having similar physical and chemical properties were constituted. Ciamician noted that certain periods were incomplete and he dwelled upon the unknown element with an approximate weight of 100. This element had disappeared or had not yet been discovered. We now know that this element is technetium, the discovery of which occurred in December 1936 by Perrier and Segrè. Mendeleev left numerous other places vacant that were occupied much later. After certain considerations on the periodic function, that was not continuous but "in leaps" and therefore not representative of a geometric function but a numerical function, Ciamician affirmed that the periodicity was a complex function that was not completely clear. Certain considerations followed on the general properties of the groups and the observation was made that from the position of one its valency could be deducted immediately and therefore the form of combination limit. A table was presented in which elements were put in order depending on the limit valency and their limit oxide which highlighted the more or less accentuated acid or basic character. Passing to dealing with compounds different from oxides (e.g., hydrogenates and chlorinates) after having lingered over "special forms", Ciamician acknowledged the difficulty in correlating the periodic system with Dalton's law. The position of an element in the periodic system does not therefore depend only on the atomic weight but also on the physical and chemical properties. Mendeleev thought about the existence of other elements, the properties of which he was able to foresee. After the discovery of gallium (1875), scandium (1879) and germanium (1886), the system of the Russian chemist was "universally accepted". Ciamician affirmed that the systematic order of Mendeleev was "up until now the best attempt" and asked if it was possible to discover new elements. He was certain of it and returned to the atomic weight 100 and advanced the hypothesis that periods could be completed. Maybe one day, "periodicity could become greater and the elements of the system will be subject to a different arrangement". After a retest regarding the recent discovery of new elements (noble gases), of which the compounds were not known, their properties were thought to be monatomic. They did not come within the periodic system because this was based upon the combination form. The new group should be called group 0. Ciamician also spoke about recently discovered rare earths, to be positioned between lanthanum and ytterbium. He then lingered over the particular behaviour of oxygen, which was anomalous in terms of valency. The main inconsistencies of the system left were two:

1. Nickel, that followed cobalt because of its chemical properties, had a lesser atomic weight (Co=59, Ni=58.7)
2. Iodine, that followed tellurium, was always thought to have a lesser atomic weight (126.97 against 127.9). Ciamician said that maybe it was an experimental error.

A law so general created many problems that, according to Ciamician, from the field of experience were transported into a "fantasy" world. For him, phenomena of radioactive bodies reinforced the hypothesis that a single fundamental material existed from which the various elements were derived from, whilst "from experiences and reasoning of electrochemical nature, he was mislead into believing that electrical material was constituted of electrons". Ciamician gave space to the interpretation of those who thought that the order of the elements in the Mendeleev classification did not depend on the weight but the electrical charge of the atoms. Referring to the hypothesis of Rutherford, Van den Broek and Bohr on atomic structure, he affirmed that they found confirmation in the spectra of emissions of elements in the x-ray zone obtained by Moseley.

Presenting the final table, organised according to the atomic numbers, or rather "the algebraic sum of the quantities of positive and negative electricity existing in the nucleus of an atom", Ciamician explained that for each group of isotopes, only one was indicated and that only six elements before uranium were left to be discovered. Considering that the experiment of Geiger, Marsden and Rutherford was carried out in 1911, as well as Van den Broek's article in *Nature*, whilst the Bohr model, like the experiments of Moseley were carried out in 1913, it can be said that Ciamician took care of the updating of the lessons almost in real time.

On 7th November 1903, Giacomo Ciamician held the solemn inauguration of the 1903-1904 academic year of the University of Bologna. His dissertation was entitled "Chemical problems of the new century" [8]. Quoting Mendeleev's system, he said that "As our Piccini demonstrated by words and facts, it was the only guide of the intricate labyrinth of inorganic chemistry for thirty years and numerous researches based upon it inspired a great quantity of results». Certainly, the system was passing through some difficulty. Notwithstanding that, according to Ciamician it should be "transformed", "but not swept away on a wave of scientific progress". He was right.

References

[1] NASINI R. (1907), *Rend. Acc. Lincei*, 16, 8.

[2] BALBIANO L. (1905), *Annuario Società Chimica di Milano*, 91.

[3] PICCINI A. (1881-1882), *Atti R. Acc. Lincei*, Serie terza, 13, 3 (Salvucci, Roma, 1882).

[4] PICCINI A. (1885), Appendice del traduttore*, in* Von Richter V., Trattato di chimica inorganica, Loescher, Torino, p. 404.

[5] PICCINI A. (1924), Correlazioni numeriche fra i pesi atomici e Classificazione degli elementi, *in* Guareschi I., Garelli F. (eds.) Nuova enciclopedia di chimica scientifica, tecnologica…, UTET, Torino, Vol. I, Cap. 5, p. 322 .

[6] TADDIA M. (2009), Ciamician, un chimico di vario sapere*, in* VENTURI M. (ed.) Ciamician, Profeta dell'energia solare, Fondazione Eni Enrico Mattei, Milano, p. 7.

[7] MENDELEEV D. (1889), *J. Chem. Soc*., 55, 634.

[8] CIARDI M., Linguerri S. (eds.) (2007), Giacomo Ciamician, *Chimica, filosofia, energia. Conferenze e discorsi,* Bononia University Press, Bologna, p. 75.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 536.

VON ANTROPOFF'S PERIODIC TABLE: HISTORY, SIGNIFICANCE, AND PROPAGATION FROM GERMANY TO SPAIN

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Abstract

A. v. Antropoff combined the ideas on the periodic system of chemical elements of previous authors into an impressive and colorful-mnemonic graphical design, to act as an ordering principle in both research and education. He stressed the development of elements along the linear array of nuclear charges from Z = 0 to 92, the pseudo-periodicity of 2, 8, 18 and 32, and the kinships of main groups and subgroups. His design was largely lost in Germany after WW II, partly due to his Nazi affiliation. However, his scientific legacy was transferred to Spain. In 1928 Antonio García-Banús, professor of Organic Chemistry at the University of Barcelona, made a study trip to Europe including Germany. At his return he proposed the creation of a lecture hall with references to chemistry, with a wall painting of the periodic table of v. Antropoff. This took place in 1934. After the Spanish Civil War, the painting remained in the classroom, and was restored in 2008. We have yet not heard of any other still existing wall copies of this design.

Introduction

We review the history of the ideas behind the periodic system of chemical elements and its various graphical representations (sect. *Discovery of the Principles of the Periodic System of Chemical Elements*, see van Spronsen 1969; Mazurs 1974; Scerri 2007; Leach 2010). In 1926 von Antropoff, professor of Physical Chemistry of the University of Bonn (Germany), combined some of those ideas with colors in a very satisfying and impressive balance (sect. *Von Antropoff's Eclectic Periodic Table of 1926*). A short biography of von Antropoff is given inbetween (sect. *Andreas von Antropoff*) Von Antropoff's periodic table became quite popular in German schools, but quickly disappeared after World War II. However, von Antropoff's visiting professor García Banús reproduced it at the wall of the lecture hall at the main building of the University of Barcelona (Spain). It is still there and it has been restored in 2008 (sect. *Propagation of von Antropoff's Design from Germany to Spain*).

History and Significance of von Antropoff's Graphical Representation

1. Discovery of the Principles of the Periodic System of Chemical Elements

After Boyle (1661) had suggested an experimentally based modern-science definition of chemical elements, Lavoisier and his companions (Guyton de Morveau et al. 1787; Lavoisier 1789; Fourcroy 1800) eventually identified the first few dozens of them. Soon, endeavors were initiated to arrange these elements in a systematic order, thereby displaying the similarities and the various trends of chemical and physical properties of their compounds. Gmelin (1817, 1843, 1852) for instance created two-dimensional arrangements of most similar elements (halogens, chalcogens, pnicogens, etc.; alkali metals, alkaline earth metals, earth metals, etc.) along one direction, and a systematic variation of properties of these element groups (such as valency, electrochemical behavior etc.) in the other direction.

Relations between the atomic or the equivalence weights of the elements were already discovered by Döbereiner (1817) and then more relations were found by several scientists in subsequent decades, nicely described by van Spronsen (1969). But the great breakthrough came only after Cannizzaro's (1858) clarification of the two different weight concepts and of the valence numbers at the first international congress of chemistry in Karlsruhe (Anonym 1860). Now the chemical elements could be arranged on a (nearly) unique *linear array*, as done for the first time by Béguyer de Chancourtois (1862). In order to display the various relations and so-called periodicities of the properties, numerous layouts were suggested for the graphical representations of the system of elements (Mazurs 1974; Leach 2010). The periodicity was pin-pointed by the repeated occurrence of two very different elements directly adjacent on the array, namely the halogens and the alkali metals, with the chemically unique noble gases, discovered at the end of the 19th century, now in between (Schwarz and Rich 2010).

Winding the array of elements up on a cylinder, Béguyer (1862) developed a three-dimensional helical design that emphasizes the *continuity of the elemental array* and the relations between *the main groups and the transition groups* (and the inner transition or f elements, found later). Baumhauer (1870) quasi point-projected the helix onto a two-dimensional plane, obtaining a flat *spiral*. Cutting the cylinder before the alkali and coinage metals (i.e. along the yet unknown noble gases) leads to the early short form of rectangular periodic *tables*, where the explicit reference to the continuing array is no longer emphasized (Meyer 1864, 1872, 1870; Newlands 1865; Mendeleev 1869, 1870; and others, see Mazurs 1974).

The increasing lengths of the periods (2, 8, 8, 18, 18, 32, …) and the differences between the main and transition (and inner-transition) groups are better represented, if the array of elements is wound up as a *helix with increasing radius*, or on a cone, or on a cylinder with bulges (Emerson 1911; Stintzing 1916; Courtines 1925). Projection onto a plane yields a spiral with increasing radii (Emerson 1911; Hackh 1914; Clark 1921) or spirals with nooses (Janet 1928). Cutting the helix or cone along the noble gases leads to the early medium or long forms of rectangular tables (Mendeleev 1869; Meyer 1872; Bassett 1892). In order not to fully loose the relation between the main and the subgroups of same index (groups *g* and 1*g*, in modern nomenclature), additional *connecting lines* were inserted (Bayley 1882; Thomsen 1895; Carnelley 1886; Bohr 1922).

Most early chemists accounted for the growing length of the periods in the following systematic manner: the periods of 8 with the p-blocks *after group 1* (i.e. below H and He), the periods of 18 with the d-blocks *after group 2* (i.e.below Be,Mg and B,Al), and the periods of 32 with the f-blocks *after group 3* (i.e. below Sc,Y and Ti,Zr). Akhumov (1946) has nicely displayed it and indicated the hypothetical periods of 50 with g-blocks *after group 4* (i.e.below Ce,Th and Pr,Pa). This corresponds to the observed chemical trends as well as to a realistic order of orbital energies of atoms in molecules (Wang and Schwarz 2009, Schwarz and Wang 2010). Remarkably, the writings of physicists like Bohr (1922) and Madelung (1936) influenced the community of chemists to begin with a new angular momentum shell always after the 2nd column. This nonrealistic "*n+l,n* rule" of orbital occupation, namely $ns < (n\text{-}3)g < (n\text{-}2)f < (n\text{-}1)d < np$ became a 'scientific fact in the chemical community' in the sense of Ludwik Fleck (1935), as discussed by Wang and Schwarz (2009) or Schwarz and Rich (2010).

2. Andreas von Antropoff

Andreas von Antropoff was born on August 16, 1878, in a distinguished German-Baltic noble family in the Estonian capital Reval (now Tallinn) which then belonged to the Russian empire. Since 1897 he studied at the German-Lithuanian Polytechnic of Riga which had become russified. In 1904 he moved to Heidelberg in southwestern Germany, where he worked under the guidance of Professor G. Bredig (one of the founders of the science of catalysis) for his doctoral thesis on the pulsating mercury-H_2O_2 catalysis and on the formation of HgO_2 (Bredig and v.Antropoff 1906; v.Antropoff 1908). During his postdoctoral fellowship in London with Sir William Ramsey, he investigated the solubility of noble gases in water (v.Antropoff 1910). He then obtained the position of an Assistant and later of a Docent at the Polytechnic of Riga, where he continued those researches (v.Antropoff 1919).

During World War I, he acted as a departmental chief at the Central Bureau of Standards in the Russian capital, then called Petrograd. Since he got involved with the liberation of the Baltic region and against Bolshevism (v.Antropoff 1935) and favored the German side, he run into troubles. Eventually, he emigrated to southwestern Germany, obtained the position of an Assistant at the Karlsruhe Institute of Technology, and the German citizenship in 1921. He achieved his habilitation, and in 1925 was appointed as Professor of the new division of Physical Chemistry at the University of Bonn. There he cooperated with the chemical industry, supported Germany's economic autarky (v.Antropoff 1941) and also entered a national-conservative political party.

He published various editions of his introductory study-book of experimental chemistry in German and in Spanish (v.Antropoff 1919, 1955; v.Antropoff 1945). He joined the group of authors preparing the 8th edition of Gmelin's handbook of inorganic chemistry and contributed to the Strontium and Barium volumes (Gmelin 1931, 1932). In 1929 he published, in cooperation with his Assistant von Stackelberg the Atlas of Physical and Inorganic Chemistry (v.Antropoff, v.Stackelberg 1929, 1932), a rich volume of numerical data tables in the format of his Periodic Table (v.Antropoff 1926b, Fig.1). Besides these compilation and review works, he conducted various thermodynamic and kinetic research works, for instance on alkali and alkaline earth carbides, nitrides, halides etc. (e.g. v.Antropoff 1926a, 1932, 1933b) or on gas adsorption at wide ranges of pressure (a long series of papers by v.Antropoff from 1936 till 1955). It is a pity that his early speculation and proof of the existence of noble gas halides (e.g. v.Antropoff 1924a, 1933a) seem to have been nearly forgotten.

After the takeover of power in Germany by the National Socialists, von Antropoff became a member of Hitler's NSDAP party and of the paramilitary SS organization. He organized the raising of the NAZI swastika flag by radical students over Bonn University in 1933 (see Fig. 3 below). Despite his explicit anti-Jewish attitude, he nevertheless supported Einstein's scientific theory of relativity, which was uncommon among the NAZIs. Because of his disputable NAZI activities, combined with medium scientific activities, he lost his professorship after World War II around the date of retirement, and then founded a small pharmaceutical and cosmetic enterprise. He died on June 02, 1956, in Bonn, at the age of 77. (Bonn 2010; Wamhoff and Peter 2010; Leach 2010; Garleff 2008; Harten et al. 2006)

3. Von Antropoff's Eclectic Periodic Table of 1926

Von Antropoff obviously had broad chemical background knowledge. So it is not astonishing that he designed and promoted a periodic table that combined several important aspects of the previous designs in the literature. Von Antropoff recognized periodic tables as most fruitful, organizing principles for research and education in the proliferating field of chemistry. While nature gives us one system of chemical elements, the numerous relations between the elements can be represented more or less efficiently by various graphical designs. The main points he wanted to embody were the *continuity of the elemental array*, the *growing lengths of the periods*, and the more or less pronounced periodicities of the *main, transition and inner transition groups*. In order to obtain some optimal compromise between the short and medium rectangular and pyramidal forms and to account for the kinships between the main and transition groups (Bayley 1882; Thomsen 1895), he suggested the eclectic black-and-white design of Fig. 1 (v. Antropoff 1926b). In that set of two papers, he also suggested a mnemonic coloring of the main groups and their related subgroups (Fig. 2). His tables are distinguished by two characteristics. i) The noble gas at the right end of each row is repeated at the left onset of the next row, thereby highlighting the continuity of the array of elements. ii) Each of the 8 main groups of the upper periods bifurcates into two groups of the lower periods. This was no new idea, but he also indicated the extent of the kinships, for instance between O,S and Se,Te,Po, while Cr.Mo,W,U are less akin (see the yellow region in Fig. 2). On the other hand, Be,Mg are nearly as similar to Ca,Sr,Ba,Ra as to Zn,Cd,Hg and are both uninterruptedly connected by the violet region.

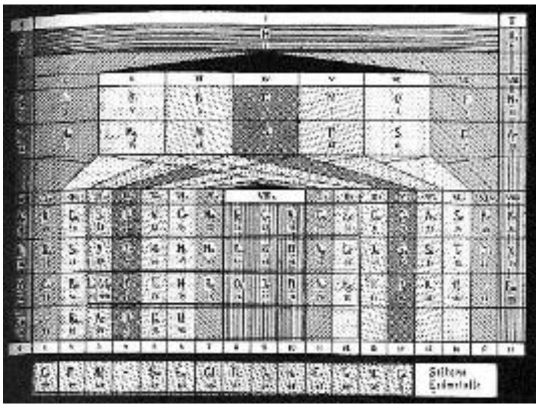

Fig. 1: Von Antropoff's black-and-white Periodic Table (1926b).

Fig. 2: Von Antropoff's colored Periodic Table, as restored in Barcelona in 2008.

The impressive chemical laboratory building of the University of Bonn had been designed by A. W. von Hoffmann in the 1860s when he was to move from London to Bonn, but eventually moved to Berlin. So, the laboratory was finished by, and became the domain of Kekulé von Stradonitz. Von Antropoff managed to get his colorful design of the periodic table onto the wall of the large chemistry lecture hall, often called the Kekulé auditorium. His table became quite common in German chemistry textbooks, but disappeared after the end of the NAZI era. Also von Antropoff's wall chart disappeared during the renovation of the university lecture hall years after the war. On the other hand, Pauling (1949) 'borrowed' von Antropoff's design for his famous book.

Von Antropoff's tables contained seven element symbols of the time that are no longer in use nowadays. For argon, both Ar and A were used, and for xenon X instead of Xe. Thulium was abbreviated by Tu instead of Tm, and iodine by the German J (Jod) instead of I. The old name emanation, Em, was still used for radon. In Germany, technetium and lutetium were at that time called masurium, Ma, and cassiopeium, Cp. The elements francium, astatine and promethium were only found in 1939, 1940 and 1945, respectively, and were still indicated by hyphens. The same was done for the entry Z=0, the neutron. Von Antropoff (1924b) had suggested the element-name neutronium for it. Also, a few other authors included Z=0 or 'n' on their tables, mostly for esthetic reasons, before and after von Antropoff, for instance Emerson (1911), Akhumov (1946) or Longman (1951).

A particular aspect of von Antropoff's table is the appearance of the noble gases at the end of one period and again at the beginning of the next one. He (1926b) stressed the periodic character of the system as a 'cylinder cut along the column of noble gases', so that the latter become the left and right 'corner-pillars' of his PT (see also Wang and Schwarz 2009). He suggested to display (as bars, circles or values) the properties of the elements, such as atomic radii, various thermodynamic, mechanic and electric parameters of the element's compounds on this tabular construction, finally published in his Atlas of 1929. This has nowadays become standard, using the medium long rectangular form advocated for by the IUPAC (2010), see e.g. Lof (1987).

Propagation of von Antropoff's Design from Germany to Spain

1. Antonio García-Banús

Until 1969, the Section of Chemistry of the Faculty of Sciences of the University of Barcelona (UB) was located in the historic building at the University Square of Barcelona, where all theoretical and practical chemistry classes were taught. Antonio García-Banús (Fig. 4), born in 1888, was a renowned professor and researcher in Organic Chemistry since 1915 (Nieto-Galan 2004). In 1928 the university granted him a stipend of 6,000 pesetas to go on a study trip and scientific stay in Germany, France and Switzerland, probably during the period from July to December 1928. During this stay he must have seen various mural periodic tables at the visited schools. The formats of periodic tables so far released in Spain were the Mendeleev models in its various forms, as in the textbooks of Luanco (1898) and Mascareñas (1921).

In 1933 García-Banús was appointed to the Board of the UB, which had received the name *Autonomous University of Barcelona* in accordance with the Statute of Autonomy passed in June 1933. From his position, and among many other tasks, García-Banús promoted several works of conditioning and improvement of the faculties. On 24 September 1933, the Board approved the "Construction of an Auditorium for General Chemistry" (Actas 1933) on the ground floor of the cloister of Humanities. The furniture was changed at the chosen site, and a periodic table was painted on the wall. García-Banús was very proud of that performance, as evidenced by his speech at a meeting of the Faculty Board in December 1934, with the statute of autonomy of the university suspended and some dismissed members of the board in prison (Actas 1933-1946). Because of alleged Republican militancy, García-Banús had to go into exile in 1938. He got a position at the National University of Colombia and then moved to Caracas (Venezuela), where he created a school of organic chemistry. There he died in 1955.

Fig. 3: Andreas von Antropoff in front of the University of Bonn 1933.

Fig. 4: Antonio García-Banús.

2. The Mural Periodic Table of the University of Barcelona

The Barcelona classroom became known popularly as the 'García-Banús room' already before the Spanish Civil War, but obviously this name never became official. All Barcelona students of chemistry between 1934 and 1969 have received some lessons in this room at some point of their studies. Unfortunately it was not possible to find information about the designer or the professional painter of the mural Periodic Table. It is known that on 31 May 1934, the Faculty paid a bill for 3,248 pesetas to Fernando Blasi, and therein was only a note for "Auditorium General Chemistry" without any further specification (Libro Mayor 1934).

The mural is an oil painting, 2.720 × 2.175 m, painted directly on the wall ready and unframed. In 2008 its state was readable but very rough and eroded in many places. The restoration was led by Maria Antonia Heredero, professor at the Restoration Unit of the Department of Painting at the Faculty of Fine Arts of the UB. The work team had been in charge of Sara López Busquets, Marta Sánchez Natera and Mar Rodríguez García. They have followed the current criteria for restoration, using little invasive and reversible procedures when possible. The task of restoration began with a careful examination of the mural, painting solubility tests, sampling for pigment analysis and process photography. The paint layer has been set with Japan paper and Tylose (3% polycarboxymethylcellulose sodium in distilled water). A careful process of cleaning with a concentrated solution of anionic detergent has been carried out in advance. Subsequently the wall has been retouched with Maimieri watercolor paints. Finally the wall has been covered with a protective layer of Paraloid B-72 (trade name of an acrylic resin, a copolymer of methyl acrylate and ethyl methacrylate). The work had been carried out between September and November 2008. We detected the interesting detail that element-entry hydrogen was originally painted in a different color and was later covered by the current navy blue. Also, there are several places with small incisions from 1934 on the plaster wall specifying the names of the colors that had to be painted at each division. These colors resemble the ones in von Antropoff and von Stackelberg's Atlas of 1929, which is still available at several places.

The periodic table in the classroom "Aula García-Banús" is probably the last visible vestige of the residence of the Section of Chemistry of the Faculty of Sciences at the historical University building, that was left in 1969. It is the only existing mural of this design we have heard of. A commercial large periodic table made of fabric and stamped rubber, in not very good state, is kept at the middle school IES Maragall in Barcelona. It has exactly the same design as the painted mural. It has a copyright of 1925 from Verlag Koehler & Volckman AG&Co, Leipzig. At the library of the school they have a copy of the Atlas already mentioned. No information is available on how or when this material was acquired, but it suggests that the materials designed by von Antropoff were quite widespread among teachers at such levels in those times.

Acknowledgments:

The company BASF Española S.L. has sponsored the restoration of the wall table. Vice President and Chief Executive Officer Karl-Peter Bercio initially, and after his retirement Gerhard W. Schwarz, have supported the project, which has been managed by the company's Public Relations and Communications department, headed by Josep Bach and later by Ulia de Domenech. The authors appreciate the support of Ainhoa Carcadilla, Laia Encinas, Marta Fernandez, Neus Jaumot and Isabel Jimenez, of the Historical Archive of the University of Barcelona (AHUB). The comments of Maria Pilar Ricol and Esperanza Colombo have been useful to guide the searches.

References

ACTAS Pleno I del Patronato de la Universidad Autónoma (1933), AHUB (Historical Archive of the University of Barcelona) p.55.

ACTAS Facultad de Ciencias U. (1933-1946), AHUB 41/6/3/33, p. 64.

ANONYM, 1860, *Chem. News*, 2:226.

AKHUMOV, E.I. (1946), *Zhur. Obshchei Khim*. 16:961.

Von ANTROPOFF, A. (1908), *Z. Phys. Chem*. 62:513.

Von ANTROPOFF, A. (1910), *Proc. Roy. Soc. London* A 83:474.

Von ANTROPOFF, A. (1919), *Z. Elektrochem*. 25:269.

Von ANTROPOFF, A. (1919 *and* 1955), 1st *and* 4th ed., *Experimentelle Einführung in die Chemie*, Braun, Karlsruhe, *and* Verlag für Angewandte Wissenschaften, Baden-Baden.

Von ANTROPOFF, A. (1924a, *Z. Angew. Chem*. 37:217,695.

Von ANTROPOFF, A. (1924b, *Z. Angew. Chem*. 37:827.

Von ANTROPOFF, A. (1926a, *Z. Phys. Chem*. 123:161.

Von ANTROPOFF, A. (1926b, *Z. Angew. Chem*. 39:722,725.

Von ANTROPOFF, A.; Von STACKELBERG, M. (1929 *and* 1932), *Atlas der physikalischen und anorganischen Chemie* with *Nachträge für die Jahre 1929-1931*, Verlag Chemie, Berlin.

Von ANTROPOFF, A. (1932), *Z. Angew. Chem*. 45:49.

Von ANTROPOFF, A. (1933a), *Z. Angew. Chem*. 46:23.

Von ANTROPOFF, A. (1933b), *Z. Phys. Chem*. A 167:49.

Von ANTROPOFF, A. (1935), *Die Befreiung des Baltenlandes von der Bolschewiken-Herrschaft im Jahre 1919*, Bonn.

Von ANTROPOFF, A. (1936), *Z. Elektrochem*. 42:544.

Von ANTROPOFF, A. (1941), *Die Versorgung Deutschlands mit Metallen*, Scheur, Bonn.

Von ANTROPOFF, A. (1945), *Introducción Experimental a la Química*, Ediciones Nueva Epoca, Madrid.

Von ANTROPOFF, A. (1955), *Kolloid Z*. 143:98.

BAUMHAUER, H. (1870), *Die Beziehungen zwischen den Atomgewichtebn und der Natur der chemischen Elemente*, Vieweg, Braunschweig.

BAYLEY, T. (1882), *Phil. Mag*.13:26.

BÉGUYER de CHANCOURTOIS, A.E. (1862), C. R. Hebd. Seances Acad. Sci., 54:757, 840, 967.

BOHR, N. (1922), *Z. Phys*. 9:1; compare van Spronsen 1969, p.156.

BONN, Universitätsarchiv (2010), http://www3.uni-bonn.de/einrichtungen/universitaetsverwaltung/organisationsplan/archiv/universitaetsgeschichte/ausstellung-der-neubeginn/der-umgang-mit-der-vergangenheit/

BOYLE, R. (1661), *The Sceptical Chymist*, Cadwell, London.

BREDIG, G.; Von ANTROPOFF, A. (1906), *Z. Elektrochem*. 12:585.

CANNIZZARO, S. (1858), *Nuovo Cimento*, 7:321.

CLARK, J.D. (1921), *J. Chem. Educ*. 10:675.

COURTINES, M.(1925), *J. Chem. Educ*. 2:107.

DÖBEREINER, J.W. (1817), *Ann. Phys*. 56:331.

EMERSON, B.K. (1911), *Am. Chem. J*. 45:160.

FLECK, L.. (1935), *Entstehung und Entwicklung einer wissenschaftlichen Tatsache*, Schwabe, Basel. English translation, 1979, *The Genesis and Development of a Scientific Fact*, University of Chicago Press, Chicago IL.

FOURCROY,.A.F. (1800), *Tableaux Synoptiques de Chimie*, Baudouin, Paris.

GARLEFF, M. (Ed), (2008), *Deutschbalten, Weimarer Republik und Drittes Reich*, Bölau, Köln.

GMELIN, L. (1817), *Handbuch der Theoretischen Chemie*, 1st ed., Varrentrapp, Frankfurt-Main.

GMELIN, L. (1843, 1852), *Handbuch der Anorganischen Chemie*, 4th and 5th ed., Karl Winter, Heidelberg.

GMELIN, L. (1931), *Handbuch der Anorganischen Chemie*, 8th ed. by Deutsche Chemische Gesellschaft, No.29 Strontium, Verlag Chemie, Weinheim.

GMELIN, L. (1932), *Handbuch der Anorganischen Chemie*, 8th ed. by Deutsche Chemische Gesellschaft, No.30 Barium, Verlag Chemie, Weinheim.

GUYTON de MORVEAU, L.B.; HASSENFRATZ, J.H.; FOURCROY, A.F.; LAVOISIER, A.L.; ADET, P.A.; BERTHOLET, C.L. (1787), *Méthode de nomenclature chimique*, Cuchet, Paris.

HARTEN, H.C.; NEIRICH, U.; SCHWERENDT, M. (2006), *Rassenhygiene als Erziehungsideologie des Dritten Reiches*, Akademie Verlag, Berlin.

IUPAC, 2010, http://old.iupac.org/reports/periodic_table/

LAVOISIER, A.L. (1789), *Traité Élémentaire de Chimie*, Cuchet, Paris.

LEACH, M.R. (2010), http://www.meta-synthesis.com/webbook/35_pt/pt_database.php?PT_id =26

LIBRO MAYOR de la Universidad de Barcelona (1934), AHUB 41/4/5/10, p. 183 *verso*.

LOF, P. (1987), *Elsevier's Periodic Table of the Elements*, Elsevier, Amsterdam.

LONGMAN, E. (1951), see Leach (2010).

De LUANCO, J.R. (1893), *Química General*, 3rd ed., Redondo y Xumetra, Barcelona.

MADELUNG, E. (1936), *Die Mathematischen Hilfsmittel des Physikers*, 3rd ed., p. 359, Springer, Berlin.

MANS, C. (2009), *Restauració de la Taula Periòdica de l'Aula García-Banús*, University of Barcelona, Barcelona.

MASCAREÑAS, E. (1921), *Elementos de Química General y Descriptiva*, Imprenta Clarasó, Barcelona.

MAZURS, E.G. (1974), *Graphic Representations of the Periodic System During One Hundred Years*, 2nd ed., University of Ala-bama Press, Tuscaloosa AL.

MENDELEJEFF, D. (1869a), *Z. Chem*. 12:405.

MENDELEEV, D. (1869b), *Zhur. Russ. Khim. Obshch*. 1:60.

MENDELEEV, D. (1870), *Zhur. Russ. Khim. Obshch*. 2:14.

MEYER, L. (1864, 1872), *Die modernen Theorien der Chemie, Maruschke und Berendt*, 1st and 2nd ed., Breslau.

MEYER, L. (1870), *Ann. Chem. Pharm*. S7:354.

NEWLANDS, J.A.R. (1865), *Chem. News* 12:83.

NIETO-GALÁN, A. (2004), *Brit. J. Hist. Sci*. 37(2):167-191.

PAULING, L. (1949), *General chemistry*, Freeman, San Francisco.

SCHWARZ, W.H.E.; RICH, R.L. (2010), *J. Chem. Educ*. 87:435.

SCHWARZ, W.H.E.; WANG, S.G. (2010), *Int. J. Quantum Chem*. 110:1455.

Van SPRONSEN, J.W. (1969), *The Periodic System of Chemical Elements*, Elsevier, Amsterdam.

STINTZIG, H. (1916), *Z. Phys. Chem*. 91:500.

THOMSEN, J. (1895), *Z. Anorg. Chem*. 9:190.

WAMHOFF, H.; PETER, M.G. (2010), http://www.chemie.uni-bonn.de/oc/geschichte

WANG, S.G.; SCHWARZ, W.H.E. (2009), *Angew. Chem. Int. Ed*. 48:3404.

CIRCULATION OF MATHEMATICAL KNOWLEDGE IN 18TH-CENTURY BRITAIN: NEW PERSPECTIVES

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 544.

FRANCIS BLAKE AND THE METHOD OF FLUXIONS

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Abstract

In 1750 Thomas Simpson published the Doctrine and Application of Fluxions*, which the author considered to be a new book rather than a second edition of his* New Treatise of Fluxions *(1737). It is true that both works contained the direct and the inverse method of fluxions. However, in the preface of the* Doctrine and Application of Fluxions *Simpson stated that this second work was more comprehensive and the principal matters were handled in a different way. In this regard, the relationship between Thomas Simpson and Francis Blake seems to have been crucial in the different approach that Simpson adopted in his work of 1750. Although his usual place of residence was Herrington (near Durham), Francis Blake spent some time in London. There he became a friend and a pupil of Simpson, who had set himself up as a teacher of mathematics in Spitalfields around 1736. Between 1738 and 1740 Simpson and Blake exchanged letters on the subject of the method of fluxions. An immediate outcome of their correspondence was an anonymous tract,* An Explanation of Fluxions in a short Essay on the Theory*, published in 1741 and written by Blake himself. Actually, Simpson mentioned Blake's short treatise in his preface of 1750 and acknowledged his indebtedness to it. Focusing on the direct method of fluxions, the goal of this contribution is to analyse the impact of the communication between Blake and Simpson on the writing of the* Doctrine and Application of Fluxions*. Beyond this particular episode, this analysis provides new insights into the communication of mathematical ideas as developed in Spitalfields at the time.*

In 1750 Thomas Simpson (1710-1781) published the *Doctrine and Application of Fluxions*, which the author considered to be a new book rather than a second edition of his *New Treatise of Fluxions*(1737). It is true that both works contained the direct and the inverse method of fluxions. However, in the preface of the *Doctrine and Application of Fluxions* Simpson stated that this second work was more comprehensive and the principal topics were dealt with in a different way. In his preface of 1750 Simpson acknowledged his indebtedness to a certain short treatise:

> I cannot put an end to this preface without acknowledging my obligations to a small tract, intitled, *An Explanation of Fluxions in a short essay on the theory*; printed for W. Innys: wrote by a worthy friend of mine (who was too modest to put his name to that, his first attempt) whose manner of

> determining the fluxion of a rectangle, and illustrating the higher orders of fluxions I have, in particular, follow'd, with little or no variation.[1]

The anonymous treatise mentioned above had been written by Francis Blake (1707/8-80), a personal acquaintance of Thomas Simpson.What little facts are known of Blake's life are mainly based on his correspondence with Simpson.[2] Educated at Lincoln College Oxford (1725), he became a baronet in 1774. His interests covered experimental philosophy and mechanics, in particular compressibility of water and design of steam engines. He actually contributed with several papers on these topics to the *Philosophical Transactions*, being elected Fellow of the Royal Society in 1746.[3]

Although his usual residence was at Herrington (near Durham), Francis Blake spent some time in London. There he became a friend and a pupil of Simpson, who had set himself up as a teacher of mathematics in Spitalfields around 1736. Between 1738 and 1740 Simpson and Blake exchanged letters on the subject of the method of fluxions. From their correspondence it is clear that Blake attended Simpson's classes in Lincoln's Inn Square (London).

An immediate outcome of their correspondence was an anonymous tract, *An Explanation of Fluxions in a short Essay on the Theory*, published in 1741 and written by Blake himself. Though not significantly popular, Blake's tract was highly regarded for its rigour in rendering the principles of the fluxional method. Besides its reprints of 1763 and 1809, Blake's tract was reprinted as late as 1809 with the fourth edition of John Rowe's *An Introduction to the Doctrine of Fluxions* (1751).[4] It was William Davis[5] who revised and corrected this fourth edition and who added Blake's essay, as the Advertisement reads:

> … he [Davis] has to this fourth edition added that short but valuable little essay on the Explanation of the Theory of Fluxions, printed for Wm. Innys, and taken notice of by the late celebrated mathematician Thomas Simpson, in the preface to his excellent Treatise of Fluxions.[6]

The relationship between Simpson and Blake turned out to be crucial in the approach adopted by Simpson in 1750. It is worth stressing that the definitions, propositions, illustrations and rules included in Blake's essay were literally derived from the correspondence between Blake and Simpson, and subsequently appeared in Simpson's work of 1750. What is more, Blake sent Simpson the introduction and conclusions he intended to include in his essay to have them corrected and discussed. Changes detected in Simpson's work of 1750 regarding the general definition of fluxion, higher-order fluxions and the rule of the product appeared in Blake's essay, as Simpson himself acknowledged in his preface:

> … whose manner [Blake's] of determining the fluxion of a rectangle, and illustrating the higher orders of fluxions I have, in particular, follow'd, with little or no variation.[7]

Compare, for instance, Figure 1, a), b) and c), regarding the rule of the product.

* * *

This contribution is essentially a part of a work-in-progress paper. Focusing on the direct method of fluxions, one of the goals of this uncompleted paper is to analyse in depth the impact of the communication of mathematical ideas between Blake and Simpson on the writing of Simpson's work on fluxions. This, on the one hand, will provide new insights into the role of correspondence in the progress of private knowledge towards public knowledge. On the other hand, how Simpson's work on fluxions took shape between 1737 and 1750 entails the study of circulation, interpretation and actual use of

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1 Simpson (1750), Preface.

2 There is a thorough account of Blake's connection with Simpson in Clarke (1929).The papers of Thomas Simpson constitute the Smith Historical Collection (Smith (1931)).All in all, this collection consists of 178 letters, most of them belonging to the correspondence between Blake and Simpson.

3 Wallis & Wallis (1986), entry 738 BLA, p. 246.

4 John Rowe (of Exeter) was a scientific writer, according to Baillie and Sieveking, eds.(1984), VII, p. 3475.

5 William Davis (1771-1807) was a mathematician and an editor of mathematical books. A member of the Mathematical and Philosophical Society in London, he was also the editor of the periodical *Gentleman's Mathematical Companion*, the first part of the republication of the *Gentleman's Diary or the Mathematical repository*, founded in 1741 following the success of *The Ladies' Diary*. The *Gentleman's Mathematical Companion* was allegedly linked with the Spitalfields Mathematical Society. Davis was also in charge of the 1805 edition of *The Doctrine and Application of Fluxions*. See Cassels (1979); Baillie and Sieveking, eds. (1984-), III, p. 1144;Albree and Brown (2009).

6 Rowe (1751), Advertisement.

7 Simpson (1750), Preface.

mathematical knowledge, taking on board Lissa Robert's call for local appropriation.[8] In addition, my analysis of the communication practices involved in the writing of *Doctrine and Application of Fluxions* (1750) can shed some light on the relevance of characters traditionally regarded as "peripheral", such as Francis Blake, in the making of the fluxional calculus.

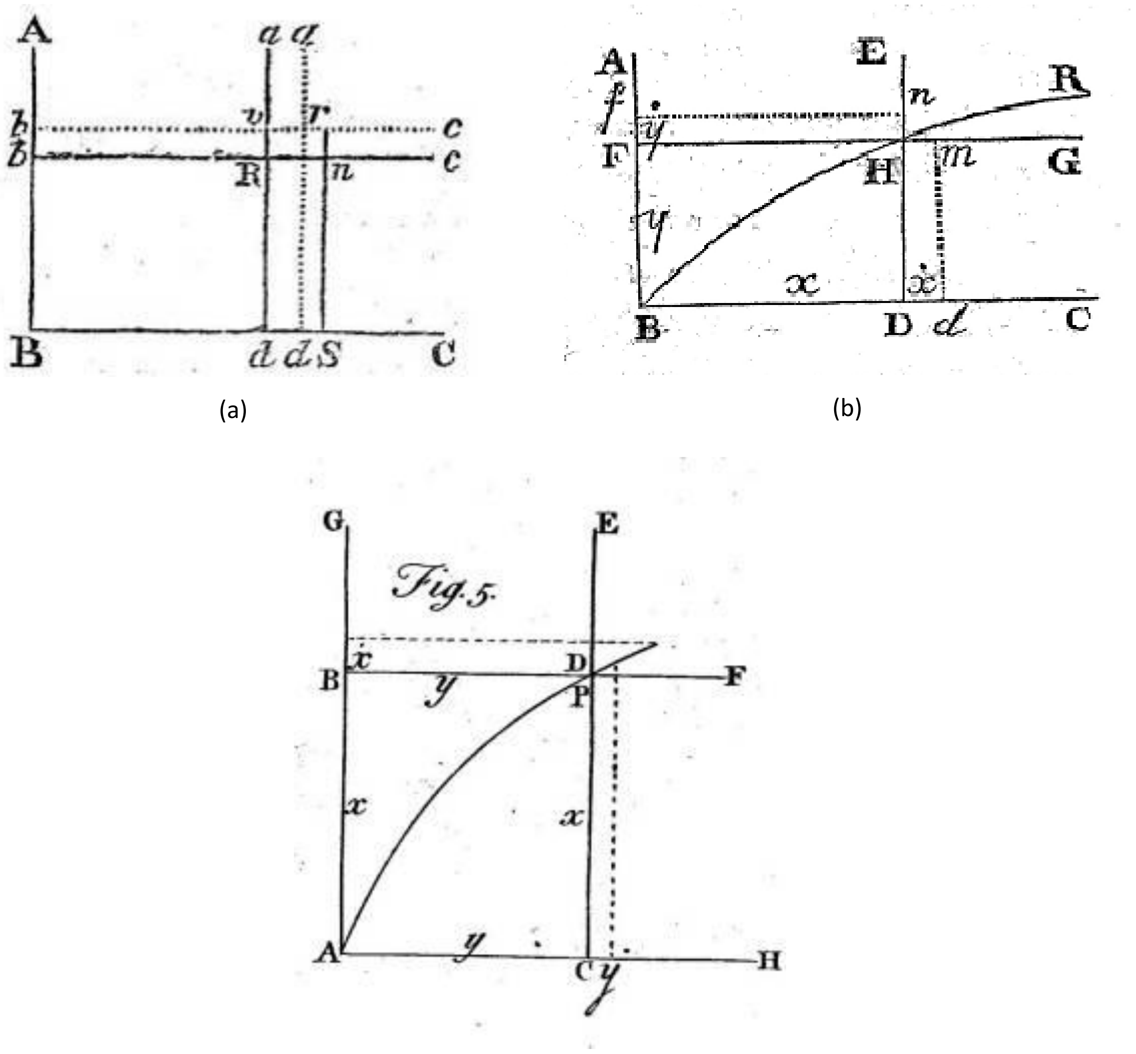

Fig. 1: Rule of the product in a) Simpson (1737); b) Simpson (1750); c) Blake (1741).

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[8] See Roberts (2005). On appropriation and reading practices see also Topham, J. R. (2000). "Scientific Publishing and the Reading of Science in Nineteenth-Century Britain: A Historiographical Survey and Guide to Sources". *Studies in History and Philosophy of Science* 31 (4), pp. 559-612.

References

ALBREE, J., BROWN, S. H.(2009). A valuable monument of mathematical genius: The Ladies' Diary (1704-1840). *HistoriaMathematica,* 36, 10-47.

BAILLIE, L., SIEVEKING, P., eds. (1984). *British Biographical Archive*. K. G. Saur, London and New York [microform].

BLAKE, F.(1741). *An Explanation of Fluxions in a short Essay on the Theory*. For W. Innys, London.

CASSELS, J. W. S. (1979). The Spitalfields Mathematical Society. *Bulletin of the London Mathematical Society* **11**, 241-258.

CLARKE, F. M. (1929).*Thomas Simpson and his times*. Waverley Press (Baltimore), New York.

ROBERTS, L. (2005). "Circulation Promises and Challenges". *The Circulation of Knowledge and Practices: The Low Countries as an historical laboratory*. Workshop, Woudschoten, 27-28 May, p. 9. *http://www.gewina.nl/werkgroep/*

ROWE, J. (1751). *An Introduction to the Doctrine of Fluxions*.4th ed. (1809).

SIMPSON, T.(1737). *A new Treatise of Fluxions*. By T. Gardner, for the Author, G. Powell, R. Shirtcliffe, D. Eagland, London.

SIMPSON, T. (1750).*The Doctrine and Application of Fluxions*. For J. Nourse, London (2 vols.) 2nd ed. enlarged 1776; 3rd. ed. 1805; 4th ed. revised and adapted 1823.

SMITH, D. E., collector (1931). Spec MsColl Smith, D. E. Historical, Archival Collections, Columbia University.

WALLIS, R. V., Wallis, P. J. (1986). *Biobibliography of British Mathematics and its Applications*. Part II. 1701-1760. Epsilon Press, Newcastle upon Tyne.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 548.

MATHEMATICAL KNOWLEDGE IN NAVIGATION; EXPLORING THE TRANSFER OF SKILLS IN THE YEARS UP TO THE ALMANACK

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Abstract

It is well-known that one of the major drivers for mathematics in the 18th century was the demand for improved navigation. While the history of the chronometer method has received the most attention, the astronomical methods have also been lucidly documented particularly by Derek Howse and Allan Chapman. Mary Croarken has provided a lively account of the compiling of the Nautical Almanack during the last third of the century. There have also been studies undertaken on mathematical education aimed at aspiring navigators, and contemporary texts are available. In addition to written material, 18th century navigational aids, including mathematical rules, abound in all major collections of scientific instruments.

This paper aims to bring these components together to explore how the mathematical skils necessary for improved navigation were transferred and disseminated, taking as a starting point Samuel Newton's An Idea of Geography and Navigation... *of 1695, and an end point the stable running of the Almanack computations achieved around 1789. It will explore where the expertise resided at different times, question how this expertise was used and communicated, and note any shifts in the skilling or de-skilling of navigators. It will also aim to incorporate the role of the leading mathematicians, and those with commercial interests represented by the East India Company and the Merchant Adventurers.*

First a quote from Isaac Barrow from *The Usefulness of Mathematical Learning Explained and Demonstrated…* 1734:

> 'We may be said to receive from the mathematics increase of fortune, convenience of labour, we have a safe traffic through the deceitful billows, pass in a direct road through the tractless ways of the sea, and come to the designed Ports by the uncertain impulse of the winds'.

But on the other hand, from Richard Norwood in *The Seaman's Practice* of 1712:

> 'Mathematics affords large fields of delightful speculations wherein a man might walk far with much pleasure, but if from so many fair flowers he brings home no honey, if he brings not from these

> speculations some useful practices, neither himself or others are likely to receive much Fruit from them.'

The 'unreasonable effectiveness of mathematics'[1] here given an early airing by Barrow, has become an established dictum in the late 20th and early 21st centuries. However, this paper argues that mathematics had to struggle to become central to the crucial practice of navigation in an era still very much early modern, where mathematics was a fledgling component of practice, showing potential but gaining ground only gradually. While some historians, particularly those closest to the practice, consider navigation to be an art[2], most tell of the 'science' of navigation and most put the start of scientific navigation with the advent of the Almanack.[3] Obviously these developments, from intuition to a reliance on a mathematically-based procedure, were not instantaneous or without problems.

This paper will endeavour to link the instruments in our collections in the Science Museum, London with four other aspects of navigation: commercial interests, contemporary elite mathematicians, the mathematics taught to navigators as evidenced by the texts, and actual practice.

Commercial and National interests

We can connect the mathematisation of navigation in the desire to find Longitude, with the improvements necessary for the development of Britain as a nation state. John Gascoigne has documented the increasing role of the Royal Society in government in *Science in the Service of Empire*[4] The influence of commercial interests specifically in navigation in England during the century from 1670 is apparent in the setting up of Greenwich Observatory, the Christ's Hospital Mathematical School, the Longitude prize, the Smith Prize, and the well-known multiple purposes of Cook's voyages. This was a period when according to Andrew Lambert[5], Britain was starting to benefit from a Tudor decision to become a maritime nation. Besides the national interest, the vast British East India Company with tax-collecting rights and a large army, was commissioning ships at a rate of 30 a year in the mid 18th century and did not want to lose them or their precious cargos. The rate of construction and size of the vessels was such that it threatened the Navy's ability to obtain good oak wood for their own ships.[6]

The 'Mathematicians of the First character'[7]

The above is a phrase from the preface of Isaac Newton's *A Treatise on the Method of Fluxions and Infinite Series* (1736). The major players were Isaac Newton, Edmund Halley, John Flamsteed and Leonard Euler, but also James Bradley and of course Tobias Mayer. There were also those endeavouring to decipher their work for able and interested audiences in England for example: William Whiston, James Gregory, James Hodgson, and Charles Leadbetter, and those who later contributed to reforming the various tables: Richard Dunthorne and Israel Lyons. The figure who eventually brought this to a conclusion was of course Nevil Maskelyne. The central concern for navigation had been long recognised as finding the longitude, and the Theory of the Moon's motion became the central issue. However, the aberration and refraction of light, and the shape of the Earth were other salient aspects of what was implicitly and sometimes explicitly seen as the problem with potential.

The start of this episode in the 'harrassment of the Moon' as Whiston charmingly put it in 1728[8] was a visit by Isaac Newton to Greenwich in September 1694 when he was shown Flamsteed's lunar data. At that time Newton felt there was just a possibility that the Moon's motion could be understood in a way which could solve the longitude problem. It is not known exactly what he was sent, but Newton was worried that Flamsteed's disclosure of his work on the Moon would show

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[1] Eugene P. Wigner 'The Unreasonable Effectiveness of Mathematics in the Natural Sciences' *Communications on Pure and Applied Mathematics* vol 13 (1): 1–14…

[2] W.E. May *A History of Marine Navigation* Salisbury 1973 p.xiii.

[3] J.D. Williams *From Sails to Satellites* Oxford 1992 has a chapter 'Numerate navigators without science' and 'Man is Not Lost' National Maritime Museum, London 1968.

[4] John Gascoigne *Science in the Service of Empire: Joseph Banks, The British State and Science in the Age of Revolution*. Cambridge 1998.p.24.

[5] Andrew Lambert: 'Science for a Maritime Nation: the Royal Society and the Royal Navy' talk at the National Maritime Museum, Greenwich, 1st October 2010.

[6] Sir Evan Cotton *East Indiamen: The East India Company's Maritime Service* London 1949 p.41.

[7] I Newton *A Treatise on the Method of Fluxions and Infinite Series* London 1736 preface.

[8] Nicolas Kollerstom *Newton's Forgotten Lunar Theory* Santa Fe N.M 2000 p.18.

he was 'trifling with mathematics.'[9] The implication from earlier historians that Flamsteed withheld data appears untrue, a ruse by Newton to disguise the fact he had made little progress. His struggle with this problem has been recounted by Whiteside, Westfall, and more recently, Iliffe. Newton is reported to have said to De Moivre that he would like to have had another go at it if he was a younger man.[10]

Newton's 'Theory of the Moon's Motion' eventually appeared in a pamphlet in English in 1702 just after it had appeared in David Gregory's *Astronomiae, Physicae et Geometricae Elementa* in Latin. It consists of a mathematical recipe without the expected philosophical causes or geometrical proofs of the Principia. It gave seven-step instructions in tortuous and opaque text for applying equations to solve the lunar longitude. It was included in a slightly different form in the second edition of the Principia in 1713, and again in the 1726 English edition, but without a specific reference to its use. The statement 'The mean motion of the moon and its apogee are not yet known with sufficient accuracy' made in 1726 begs the question, 'for what?'

Edmund Halley, another important player, investigated the Moon's motion during a complete Saros cycle of 18 years while Astronomer Royal between 1721 and 1739, with the results published after his death in 1749. He backed Newton's theory of the Moon's Motion, although he modified it, and was the first to use it systematically.[11]

Aberration of light model

James Bradley, the following Astronomer Royal, detected and explained the aberration of light, using a model in the Science Museum's collections to demonstrate the phenomenon using two simultaneous motions. He had been working on the problem of lunar parallax in 1727, and reported his findings in the Philosophical Transactions the following year.[12] 'The sine between the real and visible place of an object will be to the sine of the visible inclination of the object to the line in which the eye is moving, as the velocity of the eye to the velocity of light'. It is said he was boating at the time when he realised what was happening.[13]

However, it is well-known that for the eventual compilation of the Almanack, Maskelyne used data provided by Tobias Mayer of Gottingen, based on the methods of Leonard Euler, although these in turn owed much to Newton's work. Mayer in his letter to Euler of 4th July 1751 said 'Your tracts on analysis and mechanics, honerable sir, have enabled me to study the Moon's motion according to Newton's theory.'

Mayer was first and foremost a surveyor who wanted to establish better co-ordinates for terrestrial locations.

Tables and texts

About 30 texts, published between 1680 and 1780 for the instruction of seamen, have been studied, and many more exist.[14] Most contain some sort of tables even if only for meridional parts, (that is, the adjustment to be made for the spherical nature of the Earth) Mostly they are fairly basic and changed little during the period. The vast majority follow a similar path, through plane trigonometry to plain sailing, where no account is taken of the spherical, never mind spheroidal, nature of the Earth. After that comes parallel sailing, mid-latitude sailing, the use of the Mercator chart, and the more complex traverse tables.

The attitude towards sailing by any other means than dead reckoning was often disparaged in the earlier part of this century, but even when not actually dismissed, longitude by celestial observation was not generally tackled, other than by giving a list of the possibilities.[15] On the other hand latitude was usually listed as one of the three 'knowns', which were direction from starting point (course), distance from starting point (departure), and latitude. Latitude was the piece of information requiring astronomical observation. The calculations involved in plotting a traverse table, i.e. a summary of the various courses and distances the ship had travelled over a period of time, were tedious in the extreme.

Samuel Newton has been chosen here as the author to represent the many text writers producing manuals for the seaman in the period under consideration, hence a 'Tale of Two Newtons'.

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[9] John Birks *John Flamsteed: The First Astronomer Royal* London 1999.

[10] Richard Westfall *Never at Rest* Cambridge 1980 p. 548.

[11] *Ibid* Kollerstrom p.xix.

[12] James Bradley 'an Account of the New Discovered Motion of the Fixed Stars' *P.t.* 1728 pp637-660.

[13] Harold Spencer Jones *The Royal Greenwich Observatory* London 1946 p.10.

[14] Thomas Adams and David Waters *English Maritime Books Printed Before 1801*. Greenwich 1995

[15] Samuel Newton *An Idea of Geography and Navigation* London 1695 p. 79 and James Hodgeson *A System of the Mathematics* 1723 .p.375 'when a good theory of the Moon can be obtained'.

While Isaac was concerned with the spheroidal nature of the Earth, Samuel tells his readers that the Earth is 'not flat but spherical'. Like most others of this period he states that the difference of Longitude, which was what was normally measured rather than an absolute longitude, could not be measured by celestial observation. He lists eclipses, satellites of Jupiter, a clock,or the Moon as possibilities but then says, reasonably, 'I wish any of them could be put into practice at sea.'[16]

Samuel was Master of the Mathematical School at Christ's Hospital as were James Hodgson and John Robertson more successfully later in the period. This was a crucial institution without parallel in its role in the training of young navigators. However, the low level of performance has been studied by Robert Iliffe.[17] Samuel Newton was thought to have a good character and was supported by Isaac for the post, despite being considered inferior in some respects.[18] The school was found wanting by critics, who believed more should be required of the boys.[19]

As well as Christ's Hospital and the military schools at Woolwich and Portsmouth, navigation was starting to be taught at the coffee houses in London, courses in geometry and spherical trigonometry were taught by John Harris from 1707.[20]

Instruments and instrument makers

These played a vital part, as measuring and calculating tools but also as objects of status. Edmund Stone in the translators preface to *The Construction and Principal Uses of Mathematical Instruments* of 1758 says of instruments: 'They connect, as it were, the Theory to the Practice, and turn what was bare contemplation to the most substantial uses' The most important calculating instrument for navigation was the 'Gunter'. Observational instruments employed on land by elite astronomers were essential to table construction but are outside the scope of the paper.

Instruments for the seamen employing dead reckoning consisted of the log line for distance, which were becoming more 'scientific' during the period.[21] There was a compass for direction and hour glasses for time. In the earlier part of the period a back-staff was used for altitude of the stars, or more often Sun. After 1732 invention of the reflecting quadrant allowed easy use at sea by bringing the Sun or observed celestial object in contact with the horizon by means of double reflection. The movement of the ship was thus avoided. Writers after this date tend to recommend it above others.[22] When the horizon was not visible the octant could be used with an artificial horizon which acted to reflect the Sun from a horizontal surface, so the final reading would be halved.

Mayer himself proposed a circle with a repeating principle, but this was found not to be as useful as the octant at sea on a crucial test by John Campbell in 1756, when Bradley asked the Board of Longitude if Campbell could test Mayer's tables and circle. Campbell suggested the sextant, made of brass with a scale of 60 degrees, it combined the best of both instruments and became the fundamental observing tool on board ship for two hundred years. Other ways of determining longitude were also proposed, especially the magnetic variation by Halley, which gave rise to beautiful instruments such as the dip circle and meridional compass, and the well-known variation charts

Much more important mathematically was the 'Gunter', which was a considerable modification of the plain scale by Edmund Gunter, who put John Napier's logarithms onto a line soon after their discovery in about 1620. There was also a sliding Gunter, particularly advocated by William Mountaine[23] which does not appear to have been so successful in this period.

The Gunter contains a number of standard lines which are on the 'plain' side: rhumbs, which are the eight compass points in a right-angle, chords, tangents, sines, a line to give the length of a degree of longitude for various degrees of latitude. On the Gunter or logarithmic side: sine of the rhumbs, a logarithmic line, sines, cosines, and secants on the same scale, versed sine, which is one minus the cosine, tangents, the meridian line which is used with the Mercator chart, a

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[16] *Ibid* Samuel Newton p.79

[17] Robert Iliffe 'Mathematical Characters' in *Flamsteed's Stars* ed Frances Wilmoth. Woodbridge 1997.

[18] Isaac Newton wrote to Nathaniel Hawes on 14th June 1695 praising him but saying 'As for Mr. Newton I never took him for a deep mathematician but recommended him as one who had mathematics enough for your business'. *The Correspondence of Isaac Newton* ed H.W. Turnbull, Cambridge 1961 vol 4 p.132.

[19] Lewis Maidwell *An Essay upon the Necessity and Excellency of Education…* London 1705. preface. Maidwell thought they should receive 'an exact knowledge of the motions of the Sun and Moon'.

[20] David Bryden, 'Evidence from Advertising for Mathematical Instrument Making in London, 1556-1714.' *Annals of Science* vol 49(1992) p. 332.

[21] For example there is one by Foxon patended in 1772 which uses a spiral mechanism, the turns of which can be counted to give a more sophisticated reading., Science Museum, London inv no 1857-51.

[22] Mungo Murray A Treatise on Ship Building and Navigation London 1765 p.255.

[23] W. Mountain *A Description of the Lines Drawn on a Gunter Scale* London 1778.

logarithmic line of square roots and a logarithmic line of numbers cubed. Distances were taken off using dividers, and all problems were solved on the basis of the proportional 'rule of three.'

The problems were all to do with dead reckoning; that is with plane trigonometry. The 'cannons', or proportional rules, were sometimes set out on the instrument. The first is: 'As the Radius is to Distance so is the sine of the course of departure, and so is the sine complement of the course to the difference of latitude.' This is followed by three similar cannons to do with finding latitude, mid latitude sailing and using the Mercator chart.

Practice

It is difficult to establish how the various types of ship were actually navigated during this period. Historians complain of a backward culture[24] and many contemporary writers and mathematicians complain of the difficulty in teaching new methods to seamen. Halley wrote to Pepys complaining that 'obstinately they resolve to make use of no other than the plain chart.'[25] As late as 1773 we find William Puddicombe saying 'the manner of keeping a reckoning by our English navigators is by the log line and half minute glass'.[26] After 1767 when with the appearance of the Almanack we might expect a change in the texts, no such paradigm shift appears and they continue in similar vein[27].

P.C.H. Clissold[28] describes a ship's log relating a journal for a voyage in 1704 in the George from The Lizard on the south west tip of England, which was a popular reference point as it was taken as being 50 degrees north, to Madeira. We learn that East Indiamen and King's ships i.e. the Navy, took logs every hour, but Merchant ships every two. There is a traverse table constructed from the compass and log lines and variation is recorded. The log books start with plane trig problems and the tangent formula written out in words. All information points towards the maths being used being what was necessary for dead reckoning- long and laborious trigonometry.

Links between commercial interests and texts

The commercial and national interests are often specifically referred to in the texts. An early example is Matthew Norwood's *System of Navigation* of 1685: 'I have endeavoured to promote and advance a Science wherein the Safety, wealth and Glory of the English Nation is so much concerned'. Francis Cawood in *Navigation Completed…* of 1710 writes 'Among the useful arts none is so beneficial to your Majesties Dominions as the improvement of navigation' and in a poem 'Longitude Discerned' writes:

> 'To make her ships on all known shores soonest seen
> And unknown Worlds obey Great Britain's Queen'

An example of direct commercial interests in producing the texts can be found in the East India Company's endorsement of Maskelyne's recommendation for the Nautical Almanack in 1765.[29] Maskelyne also had considerable help from the East India Company in setting up the observatory on St Helena in 1761, and The Royal Society thanked them formally.

Links between commercial interests and instruments

These links can be seen at many levels. By the early 18th century there was a thriving trade in London of makers producing 'mathematical instruments', anything used for measurement for those in trade. On a national scale, any instrument which could deliver improved navigation was greatly valued, the development of the chronometer and sextant being the prime examples. At the end of the period under discussion sextants were starting to be produced by machine by the dividing engines of Jesse Ramsden and Edward Troughton, so instruments became 'off the peg' commodities. Simon Schaffer has recently made a direct connection between Empire building and the use of instruments across the globe, where they played a role in establishing authority and superiority.[30]

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[24] Derek Howse *Nevil Maskelyne: The Seaman's Astronomer* Cambridge 1989 p.94.

[25] J.B.Hewson *A History of the Practice of Navigation* Glasgow 1951 p. 101.

[26] William Puddicombe *The Mariner's Instructor* Exeter 1773. p. 132.

[27] Alexander Ewing *A Synopsis of Practical Mathematics* Edinburgh 1779 and Thomas Haselden *The Seaman's Daily Assistant* Dublin 1774.

[28] P.C.H. Clissold 'An 18th Century Voyage' *Journal Institute Navigation* vol 9 pp191-197.

[29] Derek Howse *Greenwich Time and the Longitude* London 1997 p.70.

[30] Simon Schaffer 'Expeditions and Empire' talk at the National Maritime Museum, Greenwich, October 28th 2010.

Links between texts and the instruments

Some texts such as that by Benjamin Donn, explicitly promoted a particular instrument.[31] More usually the texts include descriptions of the Gunter in general increasingly from the mid 18th century, for example John Robertson in 1754 and William Mountaine in 1778. It appears the Gunter did not fall out of favour until the 1840s.[32] These books solve problems with the Plain sailing using geometry, and then by the Gunter.

Links between the texts and practice

W.E. May, a practical seaman and historian complains of 'works on navigation clogged with much mathematical material, of no real value to the seaman.'[33] In the early part of the period E.G.R. Taylor relates that Samuel Pepys had said that the ship's captains were dismissive of the mathematical boys, and for their own safety they had to be shipped in pairs.[34] In the later part, a report by Alexander Dalrymple, then a passenger in the Grenville crossing the Atlantic in 1775 says that lunars were taken, but relying on this method resulted in being half a degree (more than 30 miles) too far East, as there was no visibility for several days.[35] A movement towards mathematisation had taken place, but the outcomes were still far from guaranteed.

The text to break the mould was Maskelyne's *The British Mariner's Guide* of 1763, where for the first time, the use of lunar distances to find longitude was advocated for the ordinary seaman. 'They will be found much more easy and concise than any yet proposed' claimed Maskelyne for his instructions on how to proceed. But they were very far from easy or concise. The fact that the first edition of the Almanack had sold 10,000 copies by 1780 is misleading regarding its use because they were supplied to all the Navy's ships in 1767.[36]

Links between instruments and practice

We know a lot about the experimental use of the pioneers –John Campbell with the reflecting circle and quadrant, and Maskelyne with the sextant, but less about ordinary use. In our collection at the Science Museum there are a number of wooden octants dating from well after the invention of the sextant, showing that new instruments had a long gestation period before being accepted. While the Gunter was invented in 1624, it was still growing in use in the mid 18th century. Puddicombe writing in 1773 says 'Perhaps it may be thought I too much prefer the Gunter to the more correct method by help of logs (tables?), but as these are seldom used at sea…'

Links between elite mathematicians and the texts:

Flamsteed's *Doctrine of the Sphere* was incorporated into Jonas Moore's *New System of the Mathematicks* in 1681, and Newtonian-based discussions of the mean motion of the Moon featured in the works of Whiston, Dunthorne, Leadbetter and Halley, but these were not texts for navigators. The vast majority of texts don't mention the elite mathematicians at all. They are using methods which do not require the new mathematics of the late 17th and early 18th centuries. It was not necessary to have understood fluxions, series, or be able to manipulate algebraic equations. It was necessary to have a good grasp of plane and spherical trigonometry, and an ability to use logarithms either in table form or by the Gunter. Only a small minority of the text writers include concepts and mathematics on a higher plane than that necessary for dead reckoning.[37]

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[31] William Mountaine *A Description of the Lines Drawn on a Gunter Scale* London 1778.

[32] E.G.R Taylor and M.W. Richey *The Geometrical Seaman*.London 1962 p.75.

[33] W.E. May *A History of Marine Navigation* Henley-on Thames 1973 p.17.

[34] E.G.R Taylor 'The Dawn of Modern Navigation' in *The Journal of Navigation* vol 1 (1948) p.288.

[35] Andrew S Cook 'surveying the Seas' in *Cartographies of Travel and Navigation* p.74.

[36] *Ibid* Howse note 24 p.94.

[37] For example Robert Heath, John Robertson and Patrick Murdoch.

Links between commercial interests and elite mathematicians

It is entirely to be expected that commercial interests would chime with the desire to save souls at sea, and would support and stimulate the elite mathematicians. However, desirous outcomes were partly thwarted by two opposing factors: underfunding of practice and over-concentration on a big prize. It is well-known that Flamsteed had to buy his own instruments for Greenwich and his widow took them away when he died. It is also well-known that a similar situation occurred with Bradley's results which Maskelyne endeavoured to recover for Greenwich. Maskelyne lamented that by the loss of these observations 'the improvement of astronomy has been kept 50 years behind.'[38] While Tobias Mayer had great support from Euler, it has been suggested that Halley kept his methods secret, presumably because he had an eye on the prize money, and he has also been accused of delaying publication in order that others should not use his data.[39]

It is interesting to note Morris Kline's view.[40] 'He believes 'mathematics is first and foremost a subject to solve problems in the physical world.' Were Newton and his contemporaries at the end of the 17th century working towards solving the Longitude, which at that time seemed so remote? Kline says 'Because they were great they could discern directions of investigation which must ultimately prove significant.'

Conclusions

There are some long-held views of this episode which have been promulgated by a series of historians which now need to be challenged.

The first is that the Almanack changed everything. Derek Howse claims that 'a very high proportion of the world's deep sea navigators began to … (use the Almanack) … from 1767.'[41] However, while 10,000 copies were dispersed by 1781, it is extremely unlikely they were used. The Almanack is not mentioned in the navigator's textbooks of the time.

The second is that the Almanack was easy to use, an error made by taking the word of the person who was promoting it. Maskelyne's assertion that 'it demands only care in the computer and no knowledge of spherical trigonometery is economical with the truth.

The third is that the tables did away with the most time consuming and difficult parts reducing the time from 4 hours to 30 minutes.[42] Yes if you were Maskelyne, Captain Cook or John Campbell, but otherwise you were not using the lunar distance method. The impression that this was somehow standard practice is very misleading.

The fourth is that seamen were backward. It may be that practitioners take up new methods slowly, but in fact, as Thomas Haselden put it in 1722 'Navigation is no trifling knowledge', never mind the lunar distances. Using plain trigonometry, sometimes with the Mercator chart, following through the individual problems with the Gunter to form a traverse table to produce the days work is not elementary.

So, the conclusion is that there was considerable skill in navigation which became increasingly mathematical in this period, not through the use of lunar distances but through gradually improved teaching by means of schools, lectures and texts in the more routine but considerably demanding methods. The use of instruments such as the Gunter necessitated conceptual understanding and could not be done just by rote. The increased use of the Gunter, if we go by the texts, indicates increased practice of calculation where there was very little before, not an increasing reliance on rules. The dream of lunar distances, more than any other mathematical/astronomical method, was to stimulate new mathematics in the shape of what we would now call perturbation theory, which eventually fed into the tables and would eventually change practice in the future.

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[38] *Ibid* Howse note 24 p. 43.

[39] *Ibid* Harold Spencer Jones note 13 p.9 and *Ibid* Kollerstrom p.xx.

[40] M.Kline *Mathematics and the Physical World* New York 1959.

[41] *Ibid* Howse note 29 p.71.

[42] *Ibid* Howse note 24 p.120.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 555.

BRITISH INFLUENCES IN THE INTRODUCTION OF CALCULUS IN SPAIN (1717-1767)

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Abstract

It is well known that military men and Jesuits are the main characters of the process of introduction of calculus in Spain. The first documentary evidence of a Spaniard on Calculus appears in 1711 with the name of Francisco Argáiz de la Torre, who submitted his thesis at the University of Toulouse, under the direction of the Jesuit Jean Durranc. The reorganization of the colleges after the expulsion of the Jesuits (1767) marks the end of a first stage in this process from an educational point of view, and it focused on teaching institutions and textbooks.
During this period, calculus was taught in Madrid at the Real Seminario de Nobles *and at the* Colegio Imperial*, at the* Colegio de Nobles de Cordelles *in Barcelona (all Jesuit), as well as in the Navy, but it was not taught neither in universities nor in the Army.*
Within the framework of Newton's De methodus fluxionum *[1740], the two main influential British textbooks were MacLaurin's* Treatise of fluxions *[1742] and Simpson's* Doctrine and Applications of Fluxions *[1750]. The former was used as a basis by Pedro Padilla. a military engineer, to write* De los cálculos diferencial e integral o méthodo de las fluxiones *(1756), the only textbook on calculus actually printed in this period – that was used at the short lived* Academia de Guardias de Corps *(Corps Guards Academy). Simpson's work was re-elaborated by the Jesuit Tomás Cerdà in differential notation, and was probably used at the Colegio de Nobles de Cordelles and at the Colegio Imperial. It is also worth mentioning that Cerdá was a disciple of the Jesuit Esprit Pezenas, MacLaurin's translator into French (1749), and the most probable source of Padilla who, by the way, also referenced Simpson in his book.*

The introduction of infinitesimal calculus in Spanish teaching institutions (1717-1767)[1]

Jesuits

Although Francisco Argáiz de la Torre, the first Spaniard active in calculus, was a pensioner at the Jesuit College in Toulouse, and his thesis on infinitesimal calculus was presented at the University of Toulouse in 1717, under the direction of the Jesuit Professor Jean Durranc, there is no further evidence of Jesuits on calculus in Spanish institutions until 1751, when one single student of the Real Seminario de Nobles took part in the first public *Mathematical Conclusions* (Jesuit contests) that included differential and integral calculus, under the direction of Father Esteban Terreros, professor between 1746 and

[1] An extensive and in-depth discussion on this subject, as well as further detailed references, can be found in [AUSEJO & MEDRANO, 2010].

1754 [UDÍAS, 2011; 2010, p. 26]. Again in 1760, two students took part in public Mathematical Conclusions which included differential calculus, this time under the direction of Father Esteban Bramieri, professor between 1757 and 1767 [UDÍAS, 2010, p. 26]. Finally, in 1764 two students took part in examinations also including integral calculus. Although no textbook was published either by Terreros or Bramieri, and no manuscript has survived, the presentations show that teaching followed the infinitesimal trend.

The situation was different at the Colegio Imperial, where Terreros was transferred from 1755 until 1767. There he taught between 1755 and 1760 [UDÍAS, 2005] with Father Johann Wendlingen (1750-1767). From 1761 onwards, Christian Rieger (1761-1765), Miguel Benavente (1761-1767), and Tomás Cerdá (1765-17167) were active professors alongside Wendlingen [UDÍAS, 2010, p. 26]. It turned out that around 1761 hard criticism emerged against the level and quality of mathematics at this college –especially against Wendlingen–, and the lack of public conclusions and published mathematical courses were specifically censured. As a matter of fact, the only public conclusions of this period –which included no calculus–, seem to be those of 1762, under the direction of Benavente.

As regards editions of courses on mathematics, the balance sheet is not better. Among the professors of the Colegio Imperial, only Wedlingen and Cerdá published on mathematics, both of them on elementary mathematics. Nevertheless, the latter left unpublished manuscripts which prove his mathematical skills, while the former report on Lucuze's memoir on the inclined cone surface shows his wide gaps on infinitesimal calculus.

Concerning manuscripts, the situation is more interesting, since the Spanish Royal Academy of History keeps an anonymous forty-two page (in quarto) Latin manuscript on fluxional calculus; a *Complete Course on Mathematics,* probably authored by Rieger and Benavente, that includes a seven chapter (twenty-eight pages in folio) *Easy Introduction to the Algorithm of Fluxions* ; and a treatise on calculus, possibly the one which Cerdá announced in his *Liciones de mathematica* (1758), which is a re-elaboration in differential notation of Simpson [1750], except for Sections I, XI and XII of Simpson's First Part and Section IX of Simpson's Second Part. There is no evidence of Terreros and Wendlingen teaching calculus between 1755 and 1760, therefore calculus perhaps was taught at the Colegio Imperial from 1761 onwards by Rieger, Benavente, and Cerdá. The latter maybe also taught calculus at the Seminario de Nobles de Cordelles in Barcelona, from 1758 or 1760 until 1764.

Military men

Two military teaching institutions, the *Real Academia de Guardiamarinas* in Cádiz (Midshipmen Academy) and the Corps Guards Academy, also introduced calculus by the mid-eighteenth century.

Jorge Juan was the director of the Midshipmen Academy as of 1751. He was a great supporter of infinitesimal calculus and, as it will be shown in this paper, a key figure in the process of the introduction of calculus in Spain. His *Observaciones Astronómicas y Físicas* (Physical and Astronomical Observations) used infinitesimal calculus already in 1748. After the French astronomer Louis Godin was appointed Director by Jorge Juan in 1753, the exams of 1754 prove the actual teaching of calculus.

The short-lived Corps Guards Academy (1750-1761) closed due to lack of students (only three studying sciences in 1758), but in the meanwhile its Director, military engineer Pedro Padilla, must have taught calculus, since Silvio Panego, one of the eight students taking the exams of 1752, disserted on this matter. Moreover, Padilla published between 1753 and 1756 four volumes of a Military Course on Mathematics to be used at the Corps Guards Academy [PADILLA, 1753-1756], whose fourth and last volume –namely, *De los cálculos diferencial e integral o méthodo de las fluxiones* (1756)– was devoted to fluxional calculus. Based on MacLaurin's *Treatise of fluxions* [1742], this textbook also includes references to Wolf and to Newton's book on fluxions –that is, the French translation of *De methodus fluxionum* by Buffon [NEWTON, 1740].

On 18 May 1756, Padilla's treatise on fluxions obtained Jorge Juan's approval. As a matter of fact, there existed a governmental concern about the need to produce good modern treatises and textbooks on mathematics in Spanish. For this purpose the Count of Aranda, General Director of Artillery and Military Engineers, promoted in 1755 the foundation of the *Real Sociedad Matemática Militar* (Military Mathematical Society). Although no volume of a treatise on mathematics had been published when the Society was wound up in 1760, it is worth mentioning that military engineer Antonio Córdova included series and calculus in the table of contents of the volume devoted to algebra. Nevertheless, military engineer Carlos Lemaur, who was in charge of the volume on Mechanics, published in 1778 a book titled *Elements of Pure Mathematics* in two volumes –to be used at the Military Engineers Academy– which included no trace of calculus. Actually, it seems that the ineffectiveness of this institution is somehow related with differences between Lemaur and the Director of the Military Mathematical Society, military engineer Pedro Lucuze.

Lucuze also directed the Military Engineers Academy from 1738 to 1779. No calculus appears in the manuscript lecture notes either by the Academy students or by Lucuze himself, although he kept notebooks on calculus among his private papers, and even produced a memoir on the inclined cone surface. The same policy was followed by Juan Caballero and Miguel Sánchez Taramas, the Directors who followed him. Despite severe criticism from military engineer Tadeo Lope y Aguilar in the eighties, they only considered the possibility of introducing a summary of calculus, convinced as they were that extensive and deep study was unnecessary for military engineers.

Similarly, when in 1767 four students of the Artillery Academy asked to be taught fluxional calculus, their Professor of Mathematics, Lorenzo Lasso, expressed his opposition to higher mathematical instruction for artillerymen. Actually, the Mathematical Course for Military Instruction (*Curso Mathematico para la instrucción de los Militares)* by the former and first Professor of Mathematics of the Artillery Academy, Jesuit Antonio Eximeno [UDÍAS, 2010, p. 19], did not include calculus.

As a matter of fact, calculus was not included in the syllabus for artillerymen until 1795, and in the syllabus for military engineers until 1803.

It is worth mentioning that the libraries of the Corps Guards Academy, the Military Mathematical Society, and the Military Engineers Academy kept, together with continental bibliography on infinitesimal calculus, British references. Besides Newton [1740] and MacLaurin [1742], Saunderson's *Elements of Algebra* (1740) could be found at the Military Mathematical Society and the Military Engineers Academy. The latter also owned Edmund Stone's *The Method of Fluxion* (1730) and Benjamin Martin's *Philosophia Britannica, or A new & comprehensive system of the Newtonian Philosophy, Astronomy and Geography* (1747), while the Corps Guards Academy kept the *Philosophical Transactions* of the Royal Society and Wallis *Opera mathematica* (1693-99). Curiously enough, none of these libraries owned Simpson [1750].

British influences in the introduction of infinitesimal calculus in Spain (1717-1767)

The failure by Spanish universities to assimilate new scientific ideas, their commitment to scholastic ideas, and the lack of financial support for chairs of mathematics and physics, eventually left them on the wayside of scientific teaching.

All attempts to introduce infinitesimal calculus in university curricula, as well as the endeavours to reform a teaching system that was not useful for the Crown's plans, failed throughout the 18th century. This led the government to turn to those parts of society that were not hampered by the *historical* drawbacks typical of University, and who were more inclined towards modern science. Only the Jesuits and the Army were capable of channelling the new science and ready to solve very pressing problems, such as the training of technicians and the education of the leaders.

As far as infinitesimal calculus and, thus, mathematical modernity are concerned, we can state that the expulsion of the Jesuits unquestionably cut short any modernization plans that might have been undergoing implementation. Nonetheless, the four decades that elapsed between these frustrated plans and Argaiz's thesis do not place the Jesuits at the forefront of mathematical knowledge in Spain. Moreover, there is no official policy aiming at the promotion of scientific knowledge. Their influence in the introduction of modern mathematics in Spain is really modest: calculus is adopted only during the final decade of its presence in Spain, with a very limited audience –we only know of five students at the high mathematical level–, and no published textbooks on calculus, which survive only as manuscripts.

As for the education given by the Army, the assessment one can make of the military institutions over this period is definitely poor. We have proof that it was taught at the short-lived Corps Guards Academy and at the Midshipmen Academy, but we also know for a fact that it was missing at the Artillery Academy, and we are already in a position to establish almost definitely that it could not be set up at the Military Engineers Academy who, unlike the Navy, faced greater difficulties to appreciate the significance of the practical side of the new calculus. In fact, from that moment on we can identify positions that were totally contrary to its introduction (Lorenzo Lasso, Juan Caballero, Sánchez Taramas, and Carlos Lemaur) in the context of an internationally persistent debate throughout the history of engineers' training: their mathematical education. Thus, due to the failure of the fleeting Military Mathematical Society to develop a thorough mathematical monograph, including infinitesimal calculus, the first and only textbook on calculus for that period is Pedro Padilla's.

However, it is worth mentioning some significant developments in the process of the introduction of calculus in Spain: 1) institutional support, embodied in the Count of Aranda –Jorge Juan tandem that, nevertheless, only produced results in the Navy, Jorge Juan's direct influential realm; 2) updated bibliography accessible through the institutions –at the Corps Guards Academy, at the Military Mathematical Society, and at the Military Engineers Academy; 3) educated promoters – even if they failed in their publication attempts, like Tadeo Lope and Antonio Córdova.

Jorge Juan is especially noteworthy, as he was the designer of the renewal and improvement of the syllabus of the Midshipmen Academy, the author of textbooks to be used therein –such as the *Compendio de Navegación* (1757)–, the founder of the Astronomical Observatory of Cadiz, the importer of foreign scientists (Godin), the promoter of new textbooks (Padilla), and possibly responsible for collecting the Military Mathematical Society library.

Significant is also the institutional concern with teaching, and particularly the production of textbooks, which incidentally was also an unresolved problem around the world. In the 19th century it was not difficult to find inspiration from abroad, especially France, for textbooks aimed at the mathematical training of different groups, but in the 18th century, the very same concept of textbook was under construction. This accounts for the initial preference for the fluxional approach, which evolved towards infinitesimal trends through the century.

The systematic collection and analysis of teaching materials from Spanish teaching institutions –manuscript courses, examinations and textbooks– enables us to conclude that the main trend in the introduction of calculus was fluxional (Padilla, Rieger-Benavente, Cerdá), although references to the differential approach exist (Argaiz, Jorge Juan, Bramieri, Cerdá's notation), and it might well have been the need to produce textbooks in Spanish which led to the fluxional approach through the appropriation of English works.

As far as cultural transfers are concerned, it is important to stress the appropriation of English texts (McLaurin, Simpson) within a socio-political and cultural context that has so far been traditionally considered under French influence. This is especially striking when reviewing the questionable decline of 18th century British mathematics which, at least in Spain, seems to have gone unnoticed.

For instance, Jesuit Cerdá and military engineer Padilla appear strangely connected through Father Pezenas: Cerdá was Pezenas' disciple, and Padilla probably used Pezenas' translation into French (1749) as the basis for his volume on calculus; in this work, Padilla referred to Simpson [1750], the treatise on calculus Cerdá chose to re-elaborate in differential notation. Both authors use English fluxional works, but Cerdá shows he does not find any contradiction in using fluxional concepts together with differential notation.

Actually, differences between fluxional or infinitesimal approaches do not seem to be so important for Spanish authors in this period. By the end of the 18th century, Leibniz's notation was absolutely dominant, but fluxional concepts were still considered more rigorous –a position that appears in authors such as Bails, Juan Justo García, Ciscar, Chaix, and even Vallejo.

In view of the above, whilst in Spain, during the half century elapsed between 1717 and 1767, calculus was taught at the Real Seminario de Nobles –and perhaps also at the Colegio Imperial and the Seminario de Nobles de Cordelles– and in the Navy, it was ignored by both the specialized bodies of the Army (military engineers and artillerymen), and by the universities, and only one textbook on calculus was published. In such circumstances, and given the uphill battle, we should admire what was achieved after all, though we cannot really affirm that calculus was fully introduced during this period –not even in the Jesuit and military environments–, nor consider that the country acceded to mathematical modernity, and certainly not identify the Jesuits and the military as the authors of this accession.

These facts confirm that also in Mathematics Spain's accession to the enlightened process started during the second half of the 18th century, and that the first tangible results were obtained only during Charles III's reign. The best was still to come. Starting over, though not from scratch, by setting up the colleges and seminars abandoned after the Jesuits' expulsion eventually facilitated the emergence of a series of teachers and institutions that gave a new impetus to science teaching in Spain and allowed a certain blossoming of mathematics during the following period, up to the Independence War (1808-1814).

Conclusion: the civil way

Since 1757 the Count of Aranda also headed, as *consiliario*, the *Real Academia de Bellas Artes de San Fernando* (Royal Academy of Fine Arts of San Fernando). In 1759, he proposed a reform of the Architecture syllabus, insisting on the need to have good textbooks in Spanish and to include mathematics.

Not much had been done in that area when Jorge Juan was appointed honorary fellow in 1767. He started by rejecting in 1768 the Treatise on Geometry and Arithmetic by José de Castañeda, and that very year Francisco Subirás and Benito Bails were appointed Professors of the two new chairs of mathematics. The latter accepted nomination but Subirás, who had studied at the Seminario de Nobles de Cordelles with Cerdá and was teaching architecture in Madrid since 1764, rejected the appointment in order to go back to Barcelona as Director of the Seminario de Nobles de Cordelles. It should be noted that both chairs were independent from the three Sections of the Academy –painting, sculpture and architecture.

Besides teaching, Bails was commissioned to write a textbook on mathematics under the direction of Jorge Juan. As demanded in 1759, he was asked to translate, extract and summarize the best available European works in two different monographs: *Elements of Mathematics,* a complete mathematical treatise in ten volumes [BAILS, 1772-1783]; and *Principles of Mathematics,* a more elementary and concise treatise in three volumes [BAILS, 1776].

Bails's work plan of December 26, 1769 for the *Elements* included differential calculus – "*taken from Euler and L´Hôpital*"– and integral calculus –he pointed at MacLaurin, Bernoulli, Euler and Jaquier as possible sources. As regards the *Principles,* infinitesimal calculus was included in the second volume from the second edition (1789) onwards. Both works were the first widely circulated textbooks in Spanish extensively dealing with infinitesimal calculus, which was thereafter progressively considered in higher education.

As for British influences, the powerful influence of Jorge Juan definitely tipped the scales in favour of continental calculus. For instance, Subirás explains he uses infinitesimal calculus, despite his preference for the principles of fluxional calculus, in order to follow Jorge Juan's method [SUBIRÁS, 1776, p. VII]. The same position is to be found in Gabriel Ciscar's enlarged re-edition of Jorge Juan's *Examen Marítimo* [Ciscar, 1793, p. VII][2].

References

AUSEJO, E. & MEDRANO, F.J. (2010) "Construyendo la modernidad: nuevos datos y enfoques sobre la introducción del Cálculo Infinitesimal en España (1717-1787)". *LLULL, Revista de la Sociedad Española de Historia de las Ciencias y de las Técnicas, 33(71),* 25-56.

AUSEJO, E. & MEDRANO, F.J. (2011) "Gabriel Ciscar, matemático". In: *Jornadas de Historia sobre el Almirante D. Gabriel Ciscar y Ciscar.* Universidad de Valencia, forthcoming.

BAILS, B. (1772-1783) *Elementos de Matemática.* Madrid, Imprenta de Joachim Ibarra, 10 vols.

BAILS, B. (1776) *Principios de Matemática*. Madrid, Vda. de Ibarra, 3 vols.

CISCAR, G. (1793) «Principios generales que facilitan la inteligencia de la obra». In: J. JUAN, *Exâmen Marítimo teórico práctico, ó Tratado de Mecánica aplicado á la construcción, conocimiento y manejo de los navíos y demás embarcaciones*. *Edicion segunda. Aumentada con una exposicion de los principios del cálculo, notas al texto y adiciones*, Madrid, Imprenta Real.

MACLAURIN, C. (1742) *A Treatise of fluxions in two books*. Edinburgh, T.W. and T. Ruddimans.

NEWTON, I. (1740) *La méthode des fluxions et des suites infinies*. Paris, chez de Bure. Translated into French by Buffon.

PADILLA ARCOS, P. (1753-1756) *Curso Militar de Mathemáticas sobre las partes de estas Ciencias, pertenecientes al Arte de la Guerra, para el uso de la Real Academia, establecida en el Quartel de Guardias de Corps.* Madrid, Imprenta de Antonio Marín, 4 vols.

SIMPSON, T. (1750) *The Doctrine and Application of Fluxions. Containing (Besides what is common on the Subject) a Number of New Improvements in the Theory. And The Solution of a Variety of New, and very Interesting, Problems in different Branches of the Mathematics*. 2º ed. revisada y corregida, Londres, John Nourse.

SUBIRÁS Y BARRA, F. (1776) *Certamen Público de los Tratados de Matemáticas, Geometría sublime, y Mecánica que en el Real Seminario de Nobles tendrá el caballero seminarista D. Felipe Ward, Subteniente del Regimiento de Infantería de Irlanda, baxo la dirección del primer profesor D. Francisco Subirás y Barra, el día 4 de Enero de 1776 á las 3 1/2 de la tarde*. Madrid, Joachin Ibarra.

UDÍAS, A. (2005) "Los libros y manuscritos de los profesores de matemáticas del Colegio Imperial". *Archivum Historicum Societatis Iesu, 74,* 369-448.

UDÍAS, A. (2010) "Profesores de matemáticas en los colegios de la Compañía en España, 1620-1767". *Archivum Historicum Societatis Iesu u, 79,* 3-27.

UDÍAS, A. (2011) "*Conclusiones Mathematicas* en los colegios jesuitas de Madrid, 1704-1762". *Archivum Historicum Societatis Iesu,* forthcoming [by courtesy of the author].

[2] On Ciscar evolution from fluxional to infinitesimal calculus see [AUSEJO & MEDRANO, 2011].

CIRCULATIONS OF MATHEMATICAL TEXTS, IDEAS AND PRACTICES (1870-1945)

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 561.

INTRODUCTION OF THE ELEMENTARY DIVISOR THEORY IN SPAIN

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Abstract

This article presents an historical approach to the introduction of the elementary divisor theory in Spain. It aims at answering the question: When did this theory first appear in Spain? We will try to answer this in two different settings, with attention both to research, and teaching. We will refer mainly to teaching because it is there that the main activity was produced.
Although the origins of the elementary divisor theory are seen in the work of other authors, it is well-known [21] that this theory was consolidated by Weierstrass in his work (1868) about bilinear and quadratic forms [32].

In the early 20th Century

The 20th Century in Spain began with significant efforts in education and scientific policy to achieve European standards. In 1900 there was a great educational reform promoted by the Minister of Public Instruction and Arts, Antonio García Alix (1852-1911). In terms of research, we can mention the creation of public and private institutions such as the Junta para Ampliación de Estudios e Investigaciones Científicas (JAE, 1907) and the Asociación Española para el Progreso de las Ciencias (AEPPC, 1908), [4].

JAE awarded grants to study abroad. Its intention was to send young graduates and professors abroad to complement their academic training within their respective disciplines and to promote their research skills. This institution founded several centres in which the grantees could continue working and create a school, for instance a biological research laboratory and an automatics laboratory. Julio Rey Pastor[1] (1888-1962) studied in Berlin (1911-12) and Göttingen (1913-14) with a grant from JAE, became the director of research in the then called Laboratorio Seminario Matemático (LSM, 1915).

On the other hand, the AEPPC had the intention of spreading the knowledge by means of scientific conferences and supported a new institution, the Sociedad Matemática Española founded in 1911.

[1] In 1913 he became a professor of Mathematical Analysis at the Central University of Madrid, [30, 15].

With respect to teaching, the following chart shows the official syllabus for mathematics in 1915, wherein the subjects Elements of Infinitesimal Calculus, and Celestial Mechanics were introduced in 1909 and 1915, respectively.

Table 1: García Alix's plan for Exact Science Degrees. Syllabus (1915).

1	2	3	4	**Doctorate**
Metric Geometry	Analytical Geometry	Geometry of position	Decriptive Geometry	Higher Studies of Geometry
Mathematical Analysis, 1st	Mathematical Analysis, 2nd	Elements of Infinitesimal Calculus	Complements of Infinitesimal Calculus	Higher Analysis Course
			Rational Mechanics	
General Chemistry	General Physics	Cosmography and Physics of the Globe	Spherical Astronomy and Geodesy	Celestial Mechanics
				Solar System Astronomy

At that time there were three centres for studying Exact Science in Spain: Barcelona, Madrid and Zaragoza. A Ph.D. in Exact Science was offered only in Madrid. Professors in the other cities were usually interested in moving there.

The plan for mathematics was designed under the direction of Eduardo Torroja[2] (1847-1918) and was assisted by Miguel Vegas Puebla-Collado[3] (1856-1943). This syllabus was called the "plan of the geometries" by opponents, due to its excessive content of synthetic geometry. All geometric subjects for that degree were synthetic, except for analytical geometry. The synthetic geometry was also predominant in Ph.D. studies and analytical geometry was merely used to give an algebraic version to the synthetic theory. Geometric doctoral theses presented during these early years of the 20th century had a particular characteristic: if theses dealt with synthetic geometry, they received a higher mark than if they dealt with analytical geometry, [14].

Among the opponents to this syllabus we primarily find Zoel García de Galdeano y Yanguas[4] (1846-1924) who was described by Rey Pastor as "hard-working paladin of modern mathematics in Spain"[5]. Rey Pastor was right: Galdeano was a great proponent of European mathematics during his time in Spain. In 1907, he published a book as a concise setting out of mathematical topics that were omitted in the Spanish syllabus, in his words, [20, p. 6], "what we must do, is what is left out of our syllabus".

In that book, elementary divisors were quickly mentioned, [20, p. 195], as follows:

> Also the elementary divisors, which are irrational invariants of pencil forms, are valid as a criterion for their transformation. Thus, in order for a couple of pencils to transform from one into another, it is necessary that the corresponding determinants have the same elementary divisors. Weierstrass and Kronecker were the first ones devoted to these investigations.

Analytical geometry was subordinated to synthetic geometry, and mathematical analysis had little progress.[6] For these reasons the elementary divisors appeared later in Spanish mathematics. S. Cámara, J. Rey Pastor and O. Fernández-Baños were the first Spanish mathematicians who dealt with the topic of elementary divisors (see next section). However, their theses were related to synthetic geometry. Namely:

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[2] He was a professor of Descriptive Geometry at the Central University of Madrid. He was a geometer who formed a school dedicated to the study of synthetic projective geometry, where he followed the work of von Staudt, [25].

[3] Vegas was a professor of Analytical Geometry in Madrid during the period 1891-1935. He wrote textbooks without considering elementary divisors.

[4] He taught Elements of Infinitesimal Calculus at the Faculty of Sciences of the University of Zaragoza (1896-1918) where Rey Pastor studied (1904-1908), [23, 22].

[5] [28, Inscription]. This and other quotes have been translated by the authors.

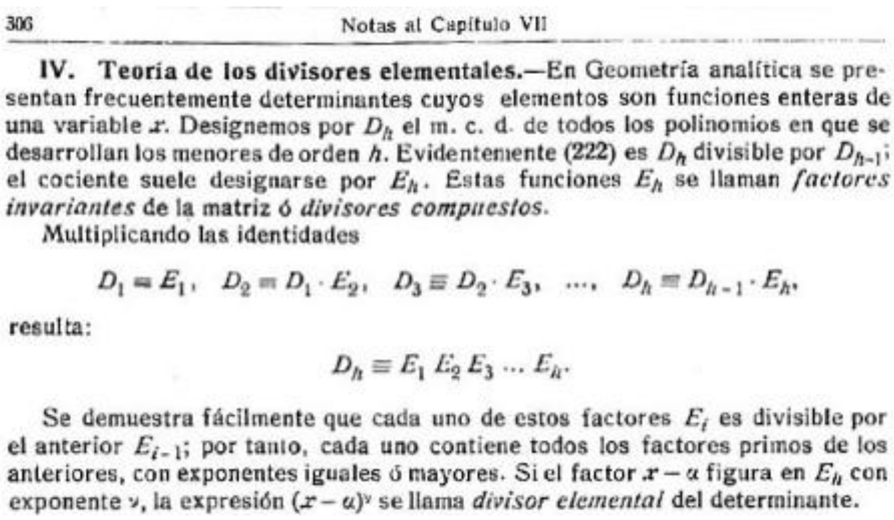
[6] A subject named Ordinary Differential Equations appeared in the 1931 Syllabus as Mathematical Analysis 3rd. Later, in the 1943 Syllabus, Linear Algebra appeared as part of Mathematical Analysis 1st.

- S. Cámara on the geometric theory of cyclical lines of the fourth-order and first type [9], as spherical curves analytically studied by Darboux.
- J. Rey Pastor on "algebraic" curves defined by correspondences [27] following Cremona's synthetic methods.
- O. Fernández-Baños on complex n-dimensional synthetic geometry [18] following the axiomatic approach of projective geometry that Julio Rey Pastor developed during his stay in Göttingen.

Julio Rey Pastor began his academical work as a geometer, but he became a professor of Mathematical Analysis at the University of Oviedo and a later at the University of Madrid. He published his lecture notes titled *Resumen de las lecciones de análisis matemático (primer curso)*, 1914-15, and *Resumen de las lecciones de análisis matemático (segundo curso)*, 1915-16. From these early drafts, he published three well-known textbooks, [17]. The first one (1917) was *Elementos de análisis algebraico*, [29], wherein the author introduces a new orientation[7] for 1st and 2nd Mathematical Analysis subjects in Spain.

The book is comprised of 4 parts, each dealing with the operations and properties of a number system, and containing the mathematical theory belonging to it. The four parts are devoted to the natural, rational, real and complex numbers, respectively. The "algebra" and its finite algorithms are considered in the first and second parts, and the "analysis" and its unlimited algorithms based on the notion of limit, in the third and fourth. The "specific theory" of the rational numbers talks about the algorithms dealing with progressions, continued fractions (finite and periodicals) or determinants, and the "algebraic algorithm" (study of polynomials of several variables and its divisibility) to which Chapter VII is devoted. Merely as an endnote to this chapter, Rey Pastor includes a brief reference to elementary divisors in their algorithmic formulation, in terms of data related to certain determinants. In the following paragraph the whole text of the note is reproduced.

> **IV. Elementary divisors theory.—** In analytical geometry there are often determinants whose elements are entire functions of one variable x. We denote by D_h the g.c.d of all polynomials where the minors of rank h develop. Obviously (222) D_h is divisible by D_{h-1}; the quotient is usually denoted by E_h. These functions E_h are named invariant factors of the matrix or composite divisors. Multiplying the identities
>
> $$D_1 \equiv E_1,\ \ D_2 \equiv D_1 \cdot E_2,\ \ D_3 \equiv D_2 \cdot E_3,\ \cdots,\ \ D_h \equiv D_{h-1} \cdot E_h,$$
>
> it follows
>
> $$D_h \equiv E_1 E_2 E_3 \cdots E_h.$$
>
> We can easily show that each of these factors E_i is divisible by the previous one E_{i-1}; therefore, each one contains all the prime factors of the previous ones, with equal or greater exponents. If the factor $x-\alpha$ appears in E_h with exponent v, the expression $(x-\alpha)^v$ is named elementary divisor of the determinant.


306 Notas al Capítulo VII

IV. Teoría de los divisores elementales.—En Geometría analítica se presentan frecuentemente determinantes cuyos elementos son funciones enteras de una variable x. Designemos por D_h el m. c. d. de todos los polinomios en que se desarrollan los menores de orden h. Evidentemente (222) es D_h divisible por D_{h-1}; el cociente suele designarse por E_h. Estas funciones E_h se llaman *factores invariantes* de la matriz ó *divisores compuestos*.

Multiplicando las identidades

$$D_1 \equiv E_1,\quad D_2 \equiv D_1 \cdot E_2,\quad D_3 \equiv D_2 \cdot E_3,\quad \ldots,\quad D_h \equiv D_{h-1} \cdot E_h,$$

resulta:

$$D_h \equiv E_1 E_2 E_3 \ldots E_h.$$

Se demuestra fácilmente que cada uno de estos factores E_i es divisible por el anterior E_{i-1}; por tanto, cada uno contiene todos los factores primos de los anteriores, con exponentes iguales ó mayores. Si el factor $x-\alpha$ figura en E_h con exponente ν, la expresión $(x-\alpha)^\nu$ se llama *divisor elemental* del determinante.

It is remarkable that Rey Pastor gives the above definition accompanied with the following comment, [29, p. 306]:

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[7] This book was inspired by the book of algebraic analysis [1] by Alfredo Capelli (1855-1910).

> In parallel to the theory of these algebraic elementary divisors, the theory of numerical elementary divisors can be developed in the same way, based on a determinant whose elements are integers, and applying the numerical divisibility rather than the algebraic. The nature of this work does not allow covering either of these two interesting theories.

Among the books that Rey Pastor recommends for this chapter we find Böcher's widely-read 1907 algebra text [5] which deals with this topic.

The period 1918-1936

A first sketch of elementary divisors theory is due to a pair of professors who developed a mathematical project with their young colleague Rey Pastor: Sixto Cámara Tecedor (1878-1964), [13], and Olegario Fernández-Baños (1886-1946), [24].

1. Fernández-Baños' research

When Fernández-Baños finished his doctorate he obtained a grant from JAE to study abroad. He worked with the geometer F. Enriques (1871-1946) at Bologna (Italy). When he finished the stay he presented a report to the JAE on the work, which contained two papers, the second of which was *Contribution to the study of networks of homographies that contain the identity of E_n. Generalization of Weierstrass' theorem*, referred to the elementary divisors, [19]. The article was dated Bologna, 15th December, 1917, and published in Madrid a year later.

Weierstrass considered the general problem of determining the necessary and sufficient condition under which one pair of bilinear forms P and Q can be transformed into another pair P' and Q' by means of non-singular linear transformations. The condition is that both determinants $| pP + qQ |$ and $| pP' + qQ' |$ have the same elementary divisors [32], being Weierstrass' completely algebraic treatment.

The aim of Fernández's paper was the generalization of Weierstrass' theorem of triples (I, P, Q) instead of pairs, where I is the identity. Once the problem was established, Fernández-Baños, according to the geometric training received in Spain, left the algebraic route to follow the "geometric translation" of Weierstrass' theory given by Segre, according to which, "a general homograph is perfect and uniquely determined by its *fundamentals spaces* and *absolute invariants*" [19, p. 233]. This path had already been established by Bonola[8] in spaces E_2 and E_3, characterizing in E_2 a network (I, P, Q) of homographies by means of a cubic curve C and an involution g in it, with a similar version for E_3. The work of Fernández-Baños, newly making good use of his doctoral thesis, consisted of extending Bonola's results to space E_n of arbitrary finite dimension, so that he was obliged to modify the procedure of his Italian predecessor. The general result of Fernández-Baños is that "the necessary and sufficient condition such that a couple of networks of general homographies in E_n, that contain the identity, to be projectively identical is that they have the same line of double elements, and the same involution in it, that is, the same pair (C, g)" [19, p. 245]. The author finished his paper uniting the modernizing spirit that impelled the JAE, affirming the end of his paper, "hoping that the processes that we have employed acquire some development in our homeland, at least to contribute some progress to modern algebraic geometry", [19, p. 249].

Nevertheless, Fernández was dedicated himself to other things later. Therefore, there was no need to continue researching in that direction and he became a professor of Analytical Geometry at the University of Santiago de Compostela in 1921, an isolated university on the periphery of research. In 1923 he obtained a grant to study Mathematical Economics and Financial Economics in France and Italy. His research focused on Economic Statistics thereafter. In 1928, he obtained another grant from University of Santiago de Compostela[9] to study Applications of Mathematics towards Statistics and Economics in Switzerland and Italy. In 1930 he was designated deputy head of Economical and Financial Studies at the Banco de España. Finally, in 1933 he became a professor of Mathematical Statistics at the Central University of Madrid.

2. Cámara's teaching

S. Cámara became a professor of Analytical Geometry in 1917 at the University of Valencia, where in 1920 he published the first part of his textbook [10], devoted to plane geometry. Influenced by Rey Pastor, Cámara follows Felix Klein's Erlangen Program, where geometry is characterized by transformations. In his words [10, Preface]:

> The professor Julio Rey Pastor has indicated the milestones that should be a guide for the mathematical progress of our homeland; in these milestones we have been inspired to write this book; fruit of the ambience of the mathematical seminar adviser of our studies towards the high questions of current mathematics. […] located from the point of view of Klein and Rey Pastor we have inspired a new work in the great syntheses of the groups of geometric transformations, whose

[8] R. Bonola (1874-1911). See [6, Nota 2ª].

[9] In this University he obtained a degree in Law.

> invariant properties characterize the geometry of the group. It is clear that by the elemental nature and pursued object in the book we have merely referred ourselves to linear transformations and quadratic curves.

This textbook is remarkable for the use of matrices in the study of geometry. Thus, Cámara included in this text several indications about linear transformations and group theory; Weierstrass' elementary divisor theory was also referenced in order to classify collineations and quadratic curves.

Cámara studies collineations in the 3rd chapter, titled "Line and point" dealing with the two dual notions. There he considers the characteristic matrix, and the roots of the characteristic equation which determine fixed points and lines; according to the number and the incidence relations between these invariants, different types of collineations are obtained (homologies, etc.).

$$\begin{bmatrix} a_{11}-\rho & a_{12} & a_{13} \\ a_{21} & a_{22}-\rho & a_{23} \\ a_{31} & a_{32} & a_{33}-\rho \end{bmatrix}$$

At the end of the chapter, there are six pages of "Notes" in small characters, where Cámara explains that the "Weierstrass' elementary divisors" of the characteristic matrix are enough to classify collineations, because they determine "double elements; their relative position; the absolute invariants of the collineation, and its canonical equations", (see table 2, [10, p.158]). For information about elementary divisors Cámara refers to the short note in [29] (Cámara wrote: "See Rey Pastor, *E. de A. A.* , page 306"), and his explanation is also very brief, only a sketch to encourage further study.

Table 2.

Exponentes de los divisores elementales de Weierstras	Orden de multiplicidad de las raices: 1 1 1	2 1	3
1 1 1	$(\rho-\rho_1)(\rho-\rho_2)(\rho-\rho_3)=0$ Un triángulo de elementos dobles; dos vértices, y los lados o, uestos, pueden ser imaginarios conjs.	$(\rho-\rho_1)(\rho-\rho_1)(\rho-\rho_2)=0$ Homología con centro exterior al eje.	$(\rho-\rho_1)(\rho-\rho_1)(\rho-\rho_1)=0$ Coincidencia de los dos planos; todos sus elementos son dobles
2 1		$(\rho-\rho_1)^2\ (\rho-\rho_2)=0$ Un punto y una recta dobles, incidentes; y otro punto y otra recta dobles, no incidentes.	$(\rho-\rho_1)^2\ (\rho-\rho_1)=0$ Homologia con el centro en el eje.
3			$(\rho-\rho_1)^3=0$ Un sólo punto y una sola recta dobles, incidentes.

In the 5th chapter, "Elements of the general theory of conics", the author also shows that the elementary divisors of the matrix

$$\begin{bmatrix} A+\lambda A' & H+\lambda H' & G+\lambda G' \\ H+\lambda H' & B+\lambda B' & F+\lambda F' \\ G+\lambda G' & F+\lambda F' & C+\lambda C' \end{bmatrix}$$

are enough to determine the relative position of two conics, and he gives a similar table [10, p.380].

At that time, Cámara taught in the Science Faculty of the University of Valencia, where the Exact Science degree course was not offered, Cámara's students were chemists, to which an in-depth treatment of these topics were not adequate. During his stay in Valencia, Cámara studied and published on probability and statistics, with an interest in its applications to mechanics, and in 1933 he competed with Fernández-Baños for the Chair of Mathematical Statistics at the Central University. Two years later Cámara moved to the Central University of Madrid, where he got the chair that had long belonged to Vegas. However, during 1936-1939 the Spanish Civil War took place; due to this social upheaval, research and teaching were seriously interrupted.

After the Civil War

After the war, Cámara restarted his activity as professor of Analytical Geometry at the Central University of Madrid. The final period of his academic activity lasted until his retirement in 1948. The second edition of Cámara's book [11] appeared in 1941, and the third [12] in 1945, including modifications due to the suggestions of Álvarez Ude.[10] The book was intended for students of Exact Science, and the first edition was clearly improved in content, theoretical explanations and applications. In the third edition, Cámara added two chapters devoted to Euclidean transformations and a smattering of other algebraic concepts: fields, rings, isomorphisms, notions of tensor algebra and matrix theory. However, Cámara did not undertake the unified treatment of the geometry in arbitrary finite dimension.

At that time he lectured on n-dimensional geometry, but the book continued to be divided in plane and space geometry, like twenty years before. This produced some repetitions that he was trying to avoid. For instance, the elementary divisors that had appeared in the first edition in plane geometry, are not considered in dimension two, but in the geometry of the tridimensional space. There, in the section of complementary notes, he incorporated an explanation of the n-dimensional elementary divisor theory, this was much more thorough than that of 1921, but yet without proofs. Cámara referred to [5] and [31] to study the complete theory.

In order to explain completely the elementary divisor theory, more algebraic subjects were needed, but the official syllabus remained unchanged until the reform of 1944. Then the old *Exact Science* was renamed as *Mathematics* and its syllabus enlarged to five years. The projective geometry course evolved from synthetic to algebraic geometry in the new subject "Geometry 3rd", where the abstract linear algebra and the elementary divisor theory came to find its location. The new image of algebra and modern geometry arrived to Spain, but this task was not done by Cámara, but rather by the younger mathematicians. Cámara re-edited his book in 1963, with the incorporation of suggestions due to "some professors of Technical Higher Schools"; this fact indicated that the use of his book had been shifted to a more practical teaching, because of its numerous exercises and examples of applications to physics.

Since before the Civil War, Tomás Rodríguez Bachiller (1899-1980) furthered the development of new abstract mathematics, algebra and topology.[11] The reception process of abstract linear algebra and its applications to geometry was completed by the first postwar doctoral students. Among them,[12] the final step was given by Abellanas,[13] who in 1942 studied with the algebraist B. L. Van der Waerden in Leipzig, thanks to a grant from the Fundación Conde de Cartagena. In the same year, Abellanas became a professor of Analytical Geometry and Topology at the University of Zaragoza. Then, he was a professor of Projective Geometry at the University of Madrid from 1949 onwards.

In 1961 Abellanas published the textbook *Geometría Básica* [3], with four chapters devoted to abstract (over an arbitrary field) n-dimensional linear algebra and geometry (pp. 1-388), and four more chapters on elementary differential geometry of curves and surfaces (pp. 389–562). Now we are interested only in the first part of the book: Chapter 1, The vector space; Chapter 2, The affine space and the Euclidean space; Chapter 3, The projective space; Chapter 4, Affine and Euclidean spaces as subspaces of the projective space. In each case, mappings among the corresponding spaces are studied. The first chapter include §8. *Structure of the endomorphisms of the vector space,*[14] which deals with elementary divisors for matrices of polynomials.[15] The third chapter concerns the applications of elementary divisors to classify

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[10] J.G. Álvarez Ude (1876-1958) was Cámara's professor of Descriptive Geometry at the University of Zaragoza. He moved then to Madrid when Torroja retired in 1916.

[11] See [16] and L. Espanol, Mª A. Martínez. *The biography of the Spanish mathematician Tomás Rodríguez Bachiller (1899-1980)* in these Proceedings of the 4th ICESHS.

[12] In the early forties, Francisco Botella (1915-1987) published two elementary papers following Cámara's lectures [7, 8].

[13] Pedro Abellanas (1914-1999) prepared his doctoral thesis on differential geometry under the adviser Tomás Rodríguez Bachiller in 1941 [26].

[14] The book presents a typo, it says automorphisms where it should say endomorphisms, as we have written.

[15] In 1958, Abellanas had published the first edition of a textbook, *Elementos de Matemáticas* [2], on linear algebra and calculus, addressed to first year students. In the fourth edition (1965) the classification of finitely generated abelian groups was included. But the

collineations and pencils of quadratic forms. Abellanas' books [2, 3] make no reference to Spanish or foreign authors, they were as self-sufficient as Franco's Spain, of which the author was a supporter, and the study of these books was required in Madrid and other Spanish universities for more than a decade.

References

[1] CAPELLI, A. (1909). *Istituzioni di analisi algebrica*. 4ª Ed. Pellerano, Napoli.

[2] ABELLANAS, P. (1958). *Elementos de matemáticas*. Madrid.

[3] ABELLANAS, P. (1961). *Geometría básica*. Romo, Madrid.

[4] AUSEJO, E. (2002). Spanish Initiatives to bring Mathematics in Spain into the international maistream. In: PARSHALL, K. H. & RICE, A. C. (Eds.) *Mathematics Unbound: The Evolution of an International Mathematical Research Community 1800-1945*. AMS/LMS, 45-60.

[5] BÖCHER, M. (1907). *Introduction to higher algebra*. Macmillan, New York.

[6] BONOLA, R. (1908). Ricerche sui sistemi lineari di omografie nello spazio. Nota 1ª: Fasci di omografie speziali. Nota 2ª: Reti continenti l'identit`a. Nota 3ª: Reti generali d'omografie e sistemi lineari $\infty^3$ continenti l'identita.`*Lomb. Ist. Rend.* 41.2, 175-190, 559-568, 648-662.

[7] BOTELLA, F. (1942). Nota sobre el empleo de las transformaciones lineales en cuestiones elementales de geometría analítica. *Matemática Elemental*, 2.1, 6-10.

[8] BOTELLA, F. (1942). La signatura y la clasificación de las formas cuadráticas. *Matemática Elemental*, 2.3, 95-103.

[9] CÁMARA, S. (1909). *Apuntes para la teoría geométrica de las líneas cíclicas de cuarto orden y primera especie*. Zaragoza.

[10] CÁMARA, S. (1920). *Elementos de geometría analítica plana*. Valencia.

[11] CAMARA, S. (1941). *Elementos de geometría analítica*. 2ª Ed. Madrid.

[12] CÁMARA, S. (1945). *Elementos de geometría analítica*. 3ª Ed. Madrid.

[13] ESCRIBANO, J.J. (2004). *Sixto Cámara: biografía de un matemático*. Instituto de Estudios Riojanos, Logroño.

[14] ESCRIBANO, J.J.; ESPANOL, L.; MARTÍNEZ, Mª. A. (2006). Tesis doctorales de geometría en España entre 1920 y 1921. In: *Actas del IX Congreso de la SEHCYT* T. I, Cádiz, 233-246.

[15] ESPAÑOL, L. (2006). Julio Rey Pastor. Primeros años españoles: hasta 1920. *La Gaceta de la RSME*, 9.2, 546-585.

[16] ESPAÑOL, L.; MARTÍNEZ, Mª. A. (2010). Hacia la matemática abstracta: Tomás Rodríguez Bachiller (1899-1980). *La Gaceta de la RSME*, 13.4, 769-795.

[17] ESPAÑOL, L.; MARTÍNEZ, Mª. A.; ÁLVAREZ, Y.; VELA, C. (2010). Julio Rey Pastor y el análisis algebraico. De los apuntes de 1914-16 a tres libros de texto (1917-1925). *Zubía* 28, 139-166.

[18] FERNÁNDEZ, O. (1917). *Estudio sintético de los espacios complejos de n dimensiones*. JAE, T. II. Madrid.

[19] FERNÁNDEZ, O. (1918). Contribución al estudio de las redes de homografías que contienen la identidad en E_n. Generalización de un teorema de Weierstrass. *Anales JAE*, 17.5, 233-249.

[20] GARCÍA DE GALDEANO, Z. (1907). *Exposición sumaria de las teorías matemáticas*. Zaragoza.

[21] HAWKINS, T. (1977). Weierstrass and the theory of matrices. *Arch. Hist. Exact Sci.*, 119-163.

[22] HORMIGÓN, M. La formación de Rey Pastor como estudiante en la Universidad de Zaragoza. In: Español, L. (Ed.) *Actas del I Simposio sobre Julio Rey Pastor*, IER, Logroño (1985), 193-204.

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unified treatment of both cases in the classification of finitely generated modules over principal domain was not considered in Abellanas' books.

[23] HORMIGÓN, M. (2004). Una aproximación a la biografía científica de Zoel García de Galdeano. *La Gaceta de la RSME*, 7.1, 281-294.

[24] MARTÍNEZ, Mª.V. (1995). *Olegario Fernández-Baños (Apuntes para una biografía)*. Logroño.

[25] MILLÁN, A. (1991). Los estudios de geometría superior en España en el siglo XIX. *Llull*, 14, 117-186.

[26] RECIO, T. (2001). Pedro Abellanas Cebollero: cuarenta años de matemática española. *La Gaceta de la RSME*, 4.1, 119-133.

[27] REY PASTOR, J. (1910). *Correspondencia de figuras elementales*. Madrid.

[28] REY PASTOR, J. (1916). *Introducción a la matemática superior. Estado actual, métodos y problemas*. Biblioteca Corona, Madrid.

[29] REY PASTOR, J. (1917). *Elementos de análisis algebraico*. Madrid.

[30] RÍOS, S.; SANTALO, L.A.; BALANZAT, M. (1979). *Julio Rey Pastor: matemático*. Instituto de España, Madrid.

[31] SCHREIER, O; SPERNER E. (1951). *Introduction to modern algebra and matrix theory*. Chelsea, New York.

[32] WEIERSTRASS, K. (1868). Zur Theorie der bilinearen und quadratischen Formen. *Monatsber. Akad. Wiss.*, 310-338.

2010, THE BICENTENARY OF THE *ANNALS OF MATHEMATICS* OF GERGONNE: THE EMERGENCE OF THE JOURNALS OF MATHEMATICS IN THE 19TH CENTURY AND THEIR ROLE IN THE DIFFUSION AND THE PROGRESS OF THIS SCIENCE

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 570.

THE CIRCULATION OF MATHEMATICS IN THE EUROPE OF THE BEGINNING OF THE 19TH CENTURY: THE FUNDAMENTAL CONTRIBUTION OF THE FIRST JOURNAL DEDICATED TO THIS SCIENCE

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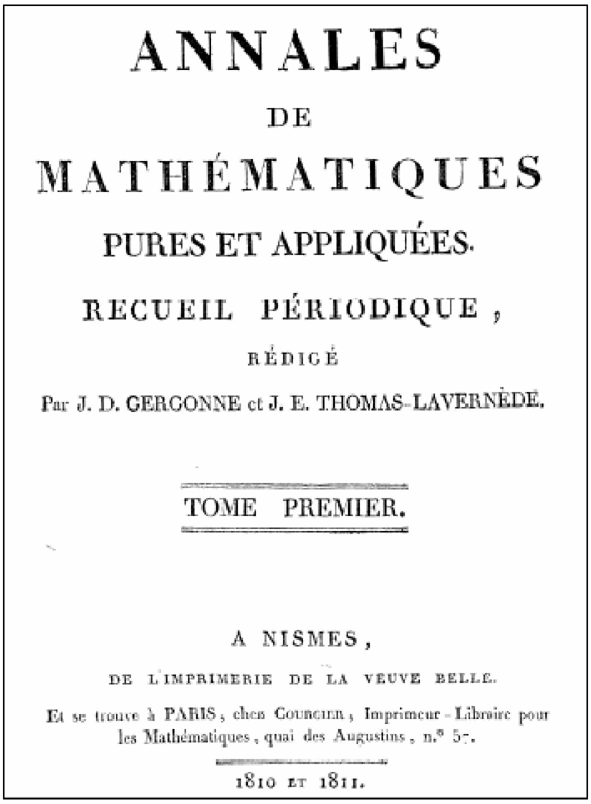
Abstract

We commemorate in 2010 the bicentenary of the edition of the journal of mathematics, the Annales de Mathématiques Pures et Appliquées *de Joseph-Diez Gergonne (22 vol., 1810-1832). This journal profoundly modified the ways of circulation and of diffusion of this science. The initiative of Gergonne quickly involved many mathematicians all over Europe in this new form of communication. But this French initiative also incited Germany and neighbouring countries of France to publish journals specifically devoted to mathematics:* Journal de Crelle *(since 1826),* Annali di Matematica *(since 1858), etc. We can also thus measure in which way a kind of modernity and of specialization had emerged in the mathematics thanks to those journals. And we will try to demonstrate in our introduction that the Annals of Gergonne really were the first modern journal in history devoted to mathematics.*

Warning: this paper is the result of a quick talk that we gave in introduction of our symposium in Barcelona (4th ICESHS, November, 2010) devoted to: "2010, the bicentenary of the Annals of Mathematics of Gergonne: the emergence of the European Journals of Mathematics in the 19th century and their role in the dissemination and the progress of this science". So we apologize if our readers find it too short and would have expected more details.

During the 19th century, Mathematics benefited in Europe from a new form of communication that radically changed exchanges amongst mathematicians of all grades and of many countries: the regular specialized periodicals specifically dedicated to this science. These journals have helped the specialization of this science in an age of rapid technological and scientific developments. In 1810, the French mathematician Jospeh-Diez Gergonne published the *Annales de mathématiques pures et appliquées* (currently called the *Annales de Gergonne*), the first notable monthly journal of mathematics published on the continent. So we celebrate in year 2010 the bicentenary of this important journal of the history of European science.


ANNALES
DE
MATHÉMATIQUES
PURES ET APPLIQUÉES.
RECUEIL PÉRIODIQUE,
RÉDIGÉ
Par J. D. GERGONNE et J. E. THOMAS-LAVERNÈDE.

TOME PREMIER.

A NISMES,
DE L'IMPRIMERIE DE LA VEUVE BELLE.
Et se trouve à PARIS, chez COURCIER, Imprimeur-Libraire pour les Mathématiques, quai des Augustins, n.° 57.
1810 ET 1811.

This paper is a short presentation of what existed in that matter before the *Annales de Gergonne*. We will not develop the topics of the *Annales* themselves but consider the attempts of periodic publications that preceded the *Annales*, in order to prove that they were the first international and regular journal introducing a kind of modernity in the communication between mathematicians. We will give in our conclusion an overview of the initiatives the *Annales* provoked in France and in Europe, from Crelle's *Journal für die reine und angewandte Mathematik* (1826) to Liouville's *Journal de Mathématiques Pures et Appliquées* ("*Journal de Liouville*", 1836) and the *Nouvelles annales* of Terquem and Gerono (1842).

The *Annales de Gergonne* and the mathematics of their time

The *Annales de Gergonne* were published monthly from 1810 to 1832. At the request of Napoleon, Jean Baptiste Delambre wrote in 1808 a report on the state of the mathematics of his time and this document, compared to the contents of the *Annales*, allows us to affirm that those contents effectively dealt with all that was then called "Mathematics" or "Applied Mathematics".

One can see, thanks to the work done by Jean Dhombres in 1989, what made up the mathematics at that time, first in Delambre's classification, secondly in the memories of the first class of the Institute (Academy of Sciences) before 1808[1]:

Geometry / Geodesy and tables / Algebra / Calculus / Analytical mechanics / Astronomy / Geography and travels / Mathematical physics / Mechanics / Manufacture and arts.

If we consider the editorial project of Gergonne (that he gives in the "Propectus", in the first pages of the first issue of his journal), as well as the contents of the *Annales*, we find nearly the same thing:

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1 Jean DHOMBRES (1989), *Rapport à l'Empereur sur le progrès des sciences, des lettres et des arts depuis 1789 et leur état actuel*, Vol. 1: *Sciences mathématiques*, édition critique du texte de 1810 de Jean-Baptiste Delambre, préface de Denis Woronoff, présentation et notes de Jean Dhombres, Paris: Belin, 1989.

> «Ces Annales seront principalement consacrées aux Mathématiques pures, et surtout aux recherches qui auront pour objet d'en perfectionner et d'en simplifier l'enseignement. Le titre de l'ouvrage annonce assez d'ailleurs que, si l'on n'y doit rien rencontrer d'absolument étranger au Calcul, à la Géométrie et à la Mécanique rationnelle, les rédacteurs sont néanmoins dans l'intention de n'en rien exclure de ce qui pourra donner lieu à des applications de ces diverses branches des sciences exactes.»
>
> Transl: *"The Annals will be mainly devoted to pure mathematics, especially in researches that will aim to refine and simplify their teaching. The book title announces, moreover, that if the journal effectively deals with Calculus, Geometry and rational Mechanics, the editors do not want to preclude any contents that may give rise to Applications of these various branches of sciences".*

For Gergonne, pure mathematics is: arithmetic, algebra, calculus, geometry, and mechanics. And the "applied mathematics" deal with the whole applications of pure mathematics to other sciences (and techniques or arts). And here we find: astronomy, geodesy, optics, gnomonic, catoptrics, etc. He added to his journal all along the 22 years of its edition a discipline called «Philosophy of mathematics»[2].

This allows a distinction a *minimum* among the publications of the second half of the eighteenth century and the first half of the nineteenth century, between those truly interested in mathematics –as they were defined at this period– and those that gave to that mathematics only a secondary or negligible place. We therefore generally examined in our study the periodical publications falling in the first category. The word "periodic" is also too unreliable: there were journals published semi-annually, or annually, but that we can still call "periodic" because they kept the same title and were indexed by successive numbers.

Until 1794: mostly non-French journals

So we can consider among the journals edited in Europe before the *Annales de Gergonne*:

- The *Beyträge zur Aufnahme der theoretischen Mathematik* published in Germany from 1758 to 1761 by W. J. G. Karsten.
- The *Leipziger Magazin für reine und angewandte Mathematik* published quarterly from 1786 to 1788 by Jean Bernoulli and Carl Friedrich Hindenburg.
- The eleven journals of the *Archiv der reinen und angewandten Mathematik* semiannually published by C. F. Hindenburg alone from 1795to 1800.

One can see in it some premises of the ideas that Gergonne succeeded in imposing a decade later with his *Annals*, so that Marteen Bullynck wrote:

> «Les journaux de Hindenburg, enfin, sont les premiers exemples d'un journal spécialisé, forme de communication qui ne s'établirait définitivement qu'au 19ème siècle.»[3]
>
> Transl. *"The Hindenburg's journals, finally, are the first examples of a specialisation in the periodical edition in mathematics that will be definitely established in the nineteenth century".*

We do find in it a real periodicity: Hindenburg first wanted a journal published four times per year and succeeded in maintaining a semi-annually publication. The population of authors is larger, even if they are geographically based in northern Germany. And here we found a new generation of mathematicians and not only those who, until then, shared with Hindenburg the passion of combinatorial analysis and had written a majority of the articles of *Leipziger Magazine*:

> The Archiv did, however, not become a mere vehicle of combinatorial analysis. It included a large variety of contributions and contributors. Not only did the old generation that already wrote for the Magazin (Kästner, Klügel, Hennert) continue to contribute, but also professors, teachers and enthusiasts from the new generation filled the Archiv's pages.[4]

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[2] See for example for more details our book: Christian GERINI (2003), *Les Annales de Gergonne. Apport scientifique et épistémologique dans l'histoire des mathématiques*, Villeneuve d'Ascq: éditions du Septentrion.

[3] Marteen BULLYNCK (2011), Stages Towards a German Mathematical Journal (1750-1800), in: Jeanne PEIFFER et Jean-Pierre VITTU (eds.), *Les journaux savants dans l'Europe des XVIIe et XVIIIe siècles. Formes de la communication et agents de la construction des savoirs*, Amsterdam: Brepols, to be published.

[4] *Ibid*.

In England, at the end of the 18th century, the *Leybourn's Mathematical Repository*, published by Thomas Leybourn, Pr. at the Royal Military College of Great Marlow, has also been one of the first journals of mathematics with a durable periodicity. But the authors were specifically English, and not so many, at least during the period which preceded the publication of the *Annales de Gergonne*, as it can be seen from the list of authors of original articles published in Volume III in 1814: Gough, Knight, Cunliffe, Barlow, J. F. W. Herschel, Bransby, White and the baron Maseres. So it was not a journal of large circulation and it was not open to the European mathematical community. In addition, only 14 issues appeared in the first series (1795-1804) and 24 in the second one (1804-1835). It represents on average about one issue per year: it is far from a monthly frequency like that of *Annals de Gergonne*.

We now quickly mention here the journals which, while publishing in their pages some mathematical texts, or rather for most of them texts about mathematics, were overwhelmingly devoted to other information. So did for example the *Journal des savants*, published in France since 1665[5], the *Ladies diary* published in England from 1704 to 1841[6], or the general or popular German periodicals which only published very little information on Mathematics (generally some reading notes) and which were distributed in some small communities: for example the *Göttingische Anzeigen von gelehrten Sachen* (Göttingen, 1753-1801), the *Allgemeine deutsche Bibliothek*, published in Berlin by Friedrich Nicolaï from 1765 to 1796, the *Neue Bibliothek der Schönen Wissenschaften*, (Leipzig, 1765-1806) or the *Allgemeine Literatur-Zeitung* of Bertich published in Yéna from 1785 to 1803[7].

The journals of European academies and universities had a variable periodicity (but were not often published more than once per year). They were focusing on every science and were not widely disseminated: it is the case, for example, of the *Eruditorum Acta* of Leipzig, and of the *Memoirs* of the Berlin Academy.

From 1794 to 1810: the *JEP* and the *Correspondance* of Hachette

In his *Prospectus* (N°1, June 1810), Gergonne stated his intentions and the reasons for the publication of his journal.

Since 1794 and the creation of the Ecole Polytechnique, two journals dealing with sciences –and more specifically with mathematics– had appeared in France: the *Journal de l'Ecole Polytechnique* (that we will note *JEP*), and the *Correspondance sur l'École Polytechnique* of Jean Nicolas Pierre Hachette.

Gergonne tells us:

> «Les Sciences exactes, cultivées aujourd'hui si universellement et avec tant de succès, ne comptent pas encore un seul recueil périodique qui leur soit spécialement consacré».
>
> Transl: *"The exact sciences [mathematics], now so universally and so successfully grown, do not yet have a single periodical journal devoted specifically to them".*

He adds:

> «On ne saurait, en effet, considérer comme tels, le Journal de l'école Polytechnique, non plus que la Correspondance que rédige M. Hachette: recueils très précieux sans doute, mais qui, outre qu'ils ne paraissaient qu'à des époques peu rapprochées, sont consacrés presque uniquement aux travaux d'un seul établissement.»
>
> Transl: *"We can not, in fact, consider as such the Journal of the Ecole Polytechnique, nor the correspondence that Mr. Hachette proposes: valuable journals without doubt, but which, besides the fact that their issues are not published so close from each other, are devoted almost entirely to the work of a single institution."*

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[5] Successively written «sçavans», «savans» et «savants». Cf. Jeanne PEIFFER & Jean-Pierre VITTU (eds.) (2010), *Les journaux savants dans l'Europe des XVIIe et XVIIIe siècles. Formes de la communication et agents de la construction des savoirs*, Amsterdam: Brepols, to be published.

[6] See for example: Shelley COSTA (2002), "The Ladies Diary: Gender, mathematics, and civil society in earlyeighteenth-century England", in: *The History of Science Society*, 17, pp. 49-73.

[7] Maarten Bullynck showed that one can find in it only about 2% of the articles which are devoted to reports on mathematical writings: Maarten BULLYNCK, «*Les interactions entre Rezensionszeitschriften, périodiques scientifiques et périodiques spécialisés. Le cas des mathématiques dans l'Allemagne des Lumières (1760-1800)*»: http://www.histnet.cnrs.fr/research/periodiques-savants/article.php3?id_article=75, web site of the research program: «Les périodiques savants dans l'Europe des XVIIe et XVIIIe siècles», France, CNRS, with the support of the European Science Foundation.

The studies we made of these two journals (and which will be edited in the next Yearbook for European Culture of Sciences), and other works done on that topic[8], allow us to affirm that:

- First, Gergonne is right about the two assertions he gives in his *Prospectus* about them.
- Secondly, the periodicity of those journals was really irregular and with a poor frequency: only 20 issues of the *JEP* have been published from 1794 to 1831 (so with an average of one every two year), and only 10 issues for the *Correspondance* from 1804 to 1810.
- Third, the *JEP* was in fact reserved to an elite (the professors at the Ecole: Monge, Laplace, Poisson, etc.), and the *Correspondance* didn't deal frequently with mathematics.

Conclusion:

We can therefore hypothesize that, historically, the *Annals de Gergonne* were the first journal solely devoted to mathematics and who possessed the qualities of stability, of a real periodicity with short intervals between two numbers (no interruption in the publication, monthly), of a long duration (twenty-one years of existence) and of an international impact: these *Annales* ushered in the principle of a real international journal in this mathematical field. They participated in an essential way in the entry of mathematics in the specialization and the modernity.

As a proof of our conclusion, one can consider the initiatives the *Annales* provoked in France and in Europe, from Crelle's *Journal für die reine und angewandte Mathematik* (Germany, 1826) and the "*Correspondance*" of Garnier and Quetelet (Belgium, 1826) to the *Journal de Liouville*[9] (France, 1836) and the *Nouvelles annales* of Terquem and Gerono (France, 1842). We find again in them the qualities we noticed about the *Annales*: presence of all the mathematical fields, short intervals between the issues (one month), longevity, texts by mathematicians of all communities – domestic or foreign, pupils and professors of college or universities, academicians–, etc.

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[8] For example
- Laura GUGGENBUHL (1959), «Gergonne, founder of the Annales de Mathématiques», in: *The Mathematics Teacher*, 52, pp. 621-29.
- Loïc LAMY (1995), «Le Journal de l'École Polytechnique de 1795 à 1831, journal savant, journal institutionnel», in: Centre François Viète (Ed.), *Sciences et Techniques en Perspective*, 1995.
- Mario OTERO (1997), «Joseph-Diez Geronne (1771-1859). Histoire et philosophie des sciences», in: Centre François Viète (Ed.), *Sciences et Techniques en Perspective*, 1997.

[9] One can read the work done by Norbert Verdier about this journal and other ones: Norbert VERDIER (2009), *Le Journal de Liouville et la presse de son temps: une entreprise d'édition et de circulation des mathématiques au XIX siècle (1824 – 1885)*, Thèse de doctorat de l'université Paris-Sud 11, Paris: Université Paris 11.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 575.

MATHEMATICAL JOURNALS IN SPAIN DURING THE NINETEENTH CENTURY AND THE BEGINNING OF THE TWENTIETH CENTURY

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Abstract:

In Spain, the Nineteenth Century, was a very difficult period in general and, in issues related to Mathematics Science, in particular. Generalized opinion in relation with the Spanish backwardness of that period in Mathematics was very important. In the Nineteenth Century Scientific Journalism was very affected by the continuous political changes. It will only appear in Spain through certain figures of scientific importance, whose wishes and efforts will allow that those initiatives are an improvement of the Spanish sensibility towards the scientific means of communication that will be developed in Nineteenth Century Spain.
Prominent Spanish Mathematicians were interested in promoting Mathematics in Spain, and from them emerges the idea of editing Mathematics Journals where it would be possible to publish articles and information in relation with Mathematics, but developed within and outside the country. In this paper we look at the first Mathematics Journals that were published in Spain from the Nineteenth Century, which started, most of the times, by private initiative.
In Spain, the spread of Mathematic Science began its evolution thanks to the appearance of those journals with the collaboration of non-Spanish mathematicians.

Introduction

The scientific Spanish community lives a quite erratic Nineteenth Century, since among other things the political situation in Spain has enough political sways during all the century and the first half of the following, which have necessarily repercussions in the scientific and educational environment. The beginning of the new century is not calm either, as in 1923 General Primo de Rivera gives a coup which turns Spain into a Dictatorship until 1931. In that year, the Second Republic is proclaimed and lasts until the Spanish civil war (1936-1939), after which again a dictatorship is established by General Franco. This continuous change of moderate and progressive political currents in the government, that in some cases happened with only some months of difference, supposes a long list of projects and ideas that do not come to light, as every new government implies a change of the previous governments policies.

Quite the same goes for scientific journalism, which in the 19th century will only appear in Spain through certain scientific characters, whose will and effort allow these initiatives to be an advance of Spanish sensitivity toward the scientific means of communication that will be developed in the 20th-century Spain. In particular, concerning Mathematics, some newspapers or periodicals began to be published, in most cases owing to the impulse of Spanish scientists of international recognition.

The Periodical of the Progresses of Exact, Physical and Natural Sciences could be considered as the first Spanish scientific periodical. This periodical is born as a public expression of the Academy of Sciences in 1850[1]. It was published between 1850 and 1905, with a total of 22 volumes. In 1905 the periodical comes to an end, passing to be called *Periodical of the Royal Academy of Exact, Physical and Natural Sciences*. Each volume consists of three sections. At the beginning, the first two years, it was published as a part of the Bulletin of the Ministry of Trade, Instruction and Works, in charge of which their edition ran. The third year, 1852, it was not published due to financing problems, and from 1853 it is published independently of the mentioned bulletin. Between 1853 and 1868, it is published regularly[2], a number for every month of academic activity (9 numbers per year), and starting from this moment, because of several events, the next volume is published in 1871, and the following volume in 1879, another in 1886 and the last published volume in 1905. In the section of Exact Sciences, most of the articles are by foreign authors.


REVISTA

PROGRESOS DE LAS CIENCIAS

MADRID.

Febrero de 1861.

But looking at our mathematical environment, the first periodical with mathematical character that even takes this word in its title is the *Monthly Newspaper of Mathematical and Physical Sciences*, born in 1848 by wish of the military Sanchez Cerquero, in the Spanish province of Cadis.

The *Monthly Newspaper of Mathematical and Physical Sciences* of Cadis

In the military environment of the Astronomical Observatory of San Fernando in Cadis, in 1848 appears the first specialized periodical in Mathematics and Physics, published by the ex director of the Observatory, the retired Brigadier of the Armada, D. José Sánchez Cerquero.

[1] See Pérez García, M.C. and Muñoz Box, F. (1988): *Estudios sobre historia de la ciencia y de la Técnica: IV Congreso de la Sociedad Española de Historia de las Ciencias y de las Técnicas: Valladolid, 22-27 de Septiembre de 1986*. Págs. 543-552.

[2] Their regular publication does not mean a regular number of pages; these oscillate between 384 pages of the III volume to 576 pages of the XVI volume.

In the first number of this periodical, July 1848, one could read about the intention that each new number appeared on the first day in every month, with a total of 32 pages. In the last number published of December of 1848, a note is inserted where it is said that the editor suspends publication due to the sacrifices that he has had to endure during the six months of existence of the periodical, since there were only twenty-eight subscribers. The periodical was distributed in twenty-two cities of the Peninsula besides La Havana, Mexico and Santa Cruz de Tenerife. It is obvious that there was a total lack of institutional support.

During the life of the periodical, six numbers, a total of eleven works, are published, nine of which are of Mathematics (starting from the third number it is only a newspaper of Mathematics) and two of Physics; six of these eleven works were written by the editor. From the works done by the editor, Sánchez Cerquero, we would like to mention the one entitled[3] "Interesting addition about the value of the semi circumference (radio = 1) up to 208 decimals"[4], where the author makes reference to an article of Rutherford in the *Philosophical Transactions* published in 1841, and to another of Schumacher that appeared in 1847 in the *Astronomische Nachrichten*. This reference is used to highlight the existence in Spain of centres where there were investigators with a good formation and mathematical information, and laudable international relationships.

The Mathematical Progress of Saragossa


El Progreso Matemático

PERIÓDICO DE MATEMÁTICAS PURAS Y APLICADAS

DIRECTOR

Don Zoel G. de Galdeano

CATEDRÁTICO

DE

GEOMETRÍA ANALÍTICA

en la

UNIVERSIDAD DE ZARAGOZA

TOMO II -- AÑO 1892

ZARAGOZA

Imprenta de C. ARINO, Coso, núm. 100, bajos

1892

This is the first Spanish periodical dedicated exclusively to Mathematics. It was published by García de Galdeano between 1891 and 1896 in its first period, and between 1899 and 1900 in a second stage; in this occasion it reappears with the idea of sensitizing public opinion and public powers in the face of the imminent educational reformations. This periodical had the five following sections: Articles and memoirs on mathematical topics (Doctrinal Section), Bibliographical Section, Articles on philosophy on mathematical topics, Historical topics and matters of varied information. In total, two series with seven volumes[5] were published.

"*The Mathematical Progress* was the first channel of accommodation of the mathematical Spanish community to the moulds of modern Mathematics in the international context, so much to internal and doctrinal aspects, for the work of conceptual diffusion that their pages show, like regarding the opening of relationship channels and internal and external

[3] In this work on the calculation of the π number with a total of 200 exact decimals, the author only corrects Rutherford's, but he insists in the uselessness of new works on this topic.

[4] Published in number 5 of the periodical. Pp. 143-146.

[5] For instance, the second volume corresponding to year 1892 consists of twelve numbers, with a total of 368 pages.

exchange between mathematical communities”[6].


El Progreso matemático
PERIÓDICO DE MATEMÁTICAS PURAS Y APLICADAS
Director: D. Zoel G. de Galdeano
Año I. | Zaragoza 20 de Enero de 1891. | Núm. 1.º

Zoel García de Galdeano y Yanguas (1846-1924) was the creator, responsible founder, editor, collaborator and director of the periodical. He was one of the first Spanish mathematicians that participated assiduously, presenting communications and participating actively, in the International Congresses of Mathematicians. It is said that he was one of the first Spanish mathematicians in maintaining scientific correspondence[7] with other European mathematicians.

The *Archive of Pure and Applied Mathematics* of Valencia

This is the name of the second periodical published in Spain, dedicated to Physics-Mathematical Sciences, and the only one that is published in Valencia during the 19th century. Its founder was D. Luis Gonzaga Gascó y Albert (1846-1899). The publication of this periodical began in 1896 as a “monthly newspaper” published by Luis Gonzaga Gascó with the collaboration of Eduardo León, D. Mariano Belmás, and several Spanish and foreigners professors.[8]

In the first number of the periodical, their objectives were exposed, and they were: spreading the works of Valencia’s professors in particular and Spanish professors in general, establishing personal relationships with the mathematicians of the scientific international community, translating works published in foreign periodicals, and publishing scientific classical memoirs continuing the example of some scientific societies.

The periodical published a total of twenty numbers until the death of Gascó, who was the real heart and motor of the periodical, since in fact his personal effort made it possible that the periodical came out.

The Applicant of Toledo

Published by Ventura Reyes Prósper, at end of the 19th century in the city of Toledo. This periodical is mentioned by the Toledan professor Ricardo San Juan (1908-1969), professor of the Central University of Madrid, that had been student of Ventura Reyes Prósper in the Institute of Toledo; and however, so far it has not been physically found in any newspaper library.

Reyes Prósper is also well-known for being the first Spanish to publish in a foreign periodical of Mathematics of fame like *Mathematische Annales*, or in the Bulletin *of the Societé physico-mathematique de Kazán* (Russia), in the *Bulletin of the Société Géologique* of France, and in *The Educational Time of London*. Besides never abandoning his first discipline, the Natural Sciences, in Mathematics he devoted himself mainly to mathematical logic and to non-Euclidean geometries.

Quarterly Periodical of Mathematics of Saragossa

This periodical was published by professor D. José Ríus y Casas, in Saragossa. It had a relatively short life because it began in March of 1901 and ended in September of 1906. A total of twenty one numbers were made, which were distributed in five volumes plus one loose number. 72 articles and notes could be counted in total.

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[6] Elena Ausejo, “Zoel García de Galdeano y Yanguas” in www.divulgamat.net. Section: History of Mathematics and Biographies of Spanish Mathematicians.

[7] He and his friend Ventura Reyes Prósper maintained scientific correspondence.

[8] It is according to their complete title. See page 163 of “Las Ciencias”, by Victor Navarro Brotons and Jesús Catalá Gorgues, in Peset Reig, Mariano (dir.), *Historia de la Universidad de Valencia*. Vol. III.

Professor Ríus attended the International Congress of Mathematics,held in Paris in 1900, together with García de Galdeano, and after his return, he began the publication of the periodical, as a continuation or substitution of the periodical *The Mathematical Progress* of Saragossa[9] that had just concluded its publication. In the first number of the publication, Professor Ríus y Casas exposed the reasons for the creation of this periodical, alluding to the completion of the publications of *The Mathematical Progress* of Saragossa and of the *Archive of Pure and Applied Mathematics* of Valencia, and to the need for this type of periodicals, that at that time did not exist in Spain and, however, were common in other countries.

The reason for the disappearance of the periodical was, as usual, the material impossibility of sustaining it by its editor, professor Ríus y Casas, although his international fame is demonstrated with the exchange that it maintained with 25 foreign periodicals.

Gazette of Elementary Mathematics – Gazette of Mathematics

It began its publication in Vitoria in 1903, by Angel Bozal y Obejero, who in the presentation of this new periodical alludes to the only existent periodical of Mathematics, *Quarterly Periodical of Mathematics*, directed by the intelligent and laborious professor D. José Ríus y Casas.

The first number of this new periodical, *Gazette of Elementary Mathematics*, sees the light in Vitoria in January 31st 1903; it seems to be the first mathematical periodical that is published in the Basque Country. In this first number a presentation is made indicating that the objective of the periodical will be writing about "all matter concerning Mathematics, exposed in an elementary way, without forgetting any of the multiple branches that form part of such universal science". In the number of December 1904, a change in the orientation of the publication is announced, suggested by a "considerable number of distinguishing becoming"; and after seeing the convenience, the director, "is delighted in announcing that, starting from the next number, the first that corresponds to year 1905, a suitable reformation will be introduced, suppressing for this purpose, in the title of the current periodical, the word "Elementary", with which it will remain reduced, gaining in generality, to *Gazette of Mathematics*, being understood that, in spite of this suppression, it will predominate in the bottom of their mathematical works, …., the genuinely elementary character. We will include, then, any article, note, problem, etc. of a little more superior character to the one used until now …"

In spite of its short life, thanks to the thousand eighty six pages published by several authors during the life of this periodical, some news about the Spanish scientists are known between the end of the 19th century and the beginning of the 20th, and they helped with their effort to the development of Mathematics in Spain, as well as to the interests of the mathematical community at that time.

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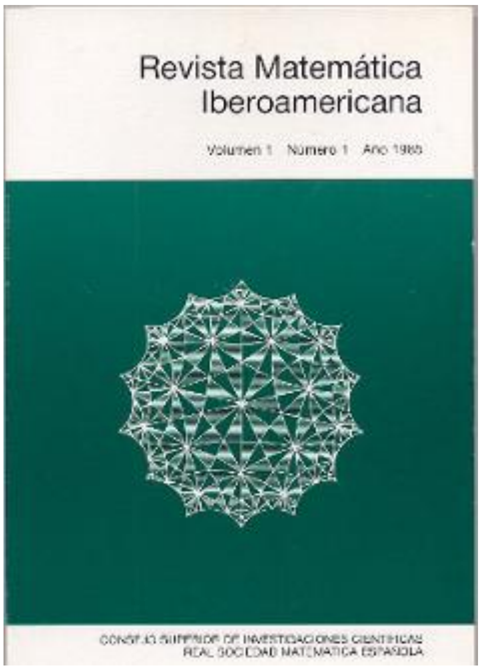
[9] In opinion of Javier Peralta in his book *La matemática española y la crisis de finales del siglo XIX*, see page 105, "The Mathematical Progress is without a doubt of more important that the Quarterly Periodical of Mathematics".

Journals of the SME

The Mathematical Spanish Society (SME) is finally created in 1911 with great hopes put on it, and a total of three hundred fifty nine partners, that in February of the following year have increased up to four hundred twenty three. It began to publish journals, starting from that same year, in May 1911, with a total of ten numbers in each academic course, organized in a volume. The title of the Journal, as displayed in its cover, is *Monthly Journal of Pure and Applied Mathematics, Historical and Pedagogic Questions*. The person responsible for the Journal is Julio Rey Pastor, who holds the position of Secretary of the SME.

The first volume of the Journal corresponds to the months from May 1911 to July 1912 (with the exception of the months of July, August and October 1911 and January and June 1912) and therefore it consists of ten numbers. The four following volumes go from October to July, like the academic cycle, although in some occasions two numbers come out together[10]. For the 1916-1917 course, that would correspond to the sixth volume, only one number in January 1917 and another in April of the same year are published. The reason for this interruption is the problems and discussions there have been on the Journal. When the Journal begins its fifth year of life, a publishing house appears[11] in which the structural problems of the Spanish Mathematics of that time are exposed to the readers.

In 1919, The Mathematical Spanish Society creates its second official journal, the *Hispano-American Mathematical Journal*, directed by Rey Pastor, which begins its activity with the publication of two volumes, opening the panorama to the twenty American nations that had been born to scientific life.



This Journal is still published, and it will change its name in 1985 to *Ibero-American Mathematical Journal*, which continues to be published at the present time, by the Department of Mathematics of the Autonomous University of Madrid, being from 2006 a scientific publication of the Society.

The *Mathematical Elementary Journal* was created by Julio Rey Pastor, like an organ of the student's mathematical circles between Madrid and Buenos Aires, in 1931. It was published under the auspices of the Spanish Mathematical Society

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[10] For instance, were issued together: February and March 1914, June and July 1915, December 1915 and January 1916, and those of April and May 1916.

[11] *The Mathematical Spanish Society Periodical*, volume V, number 41(1915-1916), pages 1-10.

and the Argentinean Mathematical Society[12]. In 1932, José María Plans y Freyre in collaboration with José Barinaga Mata takes care of the composition of this *Mathematical Elementary Journal*. This Journal is born as the breakdown of the elementary part of the *Hispano-American Mathematical Journal*.

The *Mathematical Gazette* appeared in 1949 in replacement of the *Mathematical Elementary Journal*. The *Gazette* of the Royal Spanish Mathematical Society is the current expression organ of the Society. At present it exists a digital version in electronic form of the *Gazette*. It also exists, at present time, another periodical of the Society, entitled *Bulletin of the RSME*, is an electronic publication with weekly regular recurrence, where the last news on the activities of the Society and the mathematical community are included.

Journal of the Centre of Scientific Studies

In the Basque Country, the Republic started in 1931 and some intellectuals like Carlos Santamaría and José Oñate began the organization of what would be the origin of a Future university in this Spanish region that until this moment, for political reasons, had not been possible. So, with the idea of the creation of a future Faculty of Science, as a heart of this university, the Centre of Scientific Studies was created in San Sebastián on 28th May 1932 with the purpose of giving an incentive to the studies of pure and applied sciences as a help to industry and research.

The *Journal of Centre of Scientific Studies Section of Mathematics* is born in December 1932 with a format inspired in the French mathematical periodicals of that time and with a monthly character. There are published a total of 19 numbers in two-column pages, with a total of 188 pages until December 1934. The Director of the Journal is Carlos Santamaría. The Journal is distributed mainly for subscription in Spain and Latin America. There is a record of seven hundred subscribers with a total edition of one thousand copies. Its readers are mainly students that prepare their entrance at the Technical Schools and Secondary Education teachers.

The section of Physics of the *Journal of the Centre of Scientific Studies* was first published in March 1934, every month, until December of the same year. However, from January 1935, the two periodicals, section of Mathematics and section of Physics, were joined in a single publication. A total of thirteen numbers were made until March 1936.

Conclusions

The scientific Spanish periodicals of the end of the 19th century and the beginning of the 20th century have been appearing and disappearing due to various reasons. The most important mathematical periodicals of this period are shown in the following table.

The main inconveniences for the publication of scientific periodicals in the 19th century are economical difficulties. These reasons led to the disappearance of the majority of scientific periodicals, and in particular mathematical periodicals that were created in this 19th century and the first third of the 20th century.

[12] The Argentinean Mathematical Society is created under the impulse of Julio Rey Pastor in 1924, and it arrives to its term in 1927.

Title of the periodicals	Year of creation	Year of the end of their publication	City of publication
Periodical of the Progresses of the Exact, Physical and Natural Sciences	1850	1905	Madrid
The Monthly Newspaper of Mathematics and Physical Sciences	1848	1848	Cadis
The Mathematical Progress	1891	1900	Saragossa
The Applicant	¿1896-7?	¿?	Toledo
Archive of Pure and Applied Mathematics	1896	1899	Valencia
Quarterly Periodical of Mathematics	1901	1906	Saragossa
Gazette of Mathematics (Elementals)	1903	1906	Vitoria
Journal of the SME	1911	1917	Madrid
Hispano American Mathematical Journal	1919	…..	Madrid
Elemental Mathematics	1931	1936	Madrid-Buenos Aires
The Mathematical Gazette	1949	…..	Madrid
Journal of the Center of Scientific Studies	1934	1936	San Sebastian

References

AUSEJO, E. (1993): “El Periódico Mensual de Ciencias Matemáticas y Físicas”, *Revista de Storia delle Science*, vol. 3 nº 1. Págs. 55-66.

AUSEJO, E.; HORMIGÓN, M. (1993): *Messengers of Mathematics: European Mathematical Journals (1800-1946)*. Siglo XXI de España Editores. Madrid.

CERUTTI, R.A. (2004): “Zoel García de Galdeano en la revista de Matemáticas Elementales”, Ll*ull revista de la Sociedad Española de Historia de las Ciencias y las Técnicas*, vol. 27, n º 60, Págs. 813-816.

COBO ÁVILA, J. (1991): *Ventura Reyes Prósper*. Publicaciones de la Diputación de Badajoz.

COBO BUENO, J. (1994): “Un geómetra extremeño del siglo XIX: Ventura Reyes Prósper”, en Memorias *de la Real Academia de Extremadura de las Letras y las Artes*, vol. II Págs. 91-137.

ESCRIBANO RÓDENAS, M.C. (coord.)(2000): *Matemáticos Madrileños*. Anaya educación. Madrid.

ESPAÑOL GONZÁLEZ, L. (1996): “Julio Rey Pastor en la Revista de la Sociedad Matemáticas Española (1911-1917)”, *Llull, revista de la Sociedad Española de Historia de las Ciencias y las Técnicas*, vol. 19, Págs. 381-424.

GONZÁLEZ REDONDO, F.A.; León, M. de (2000): “Aproximación a la Historia de la Matemática en España. La Real Sociedad Matemática Española”. Sección Mirando hacia atrás. En *La Gaceta de la RSME*. Vol. 3, nº 2. Madrid Págs. 363-370

GONZÁLEZ REDONDO, F.A.; León, M. de (2001): “El primer congreso matemático en España (Zaragoza, 1908) y los orígenes de la RSME”. En *La Gaceta de la RSME*. Vol. 4, nº 1. Madrid Págs. 280-291

HORMIGÓN, M. (1977): “El progreso matemático” como protagonista de la primera transformación matemática contemporánea en España. En *Actas de las IV Jornadas Matemáticas Luso-Españolas, Jaca, mayo 1977*. Tomo III. Sección de Matemáticas, 885-900.

LLOMBART, J.(1993) J, *“Mathematical journals in the Basque country in the first third of the 20th century”*. En AUSEJO, E.; HORMIGÓN, M. *Messengers of Mathematics: European Mathematical Journals (1800-1946).* Siglo XXI de España Editores. Madrid.

PERALTA, J. (2000): *La matemática española y la crisis de finales del siglo XIX*. Colección Ciencia Abierta nº 1. Ed. Nivola, Madrid.

PESET, M. (coord.) (1999): *Historia de la Universidad de Valencia*, 3 vols. Servicio de Publicaciones de la Universidad de Valencia. Colección Cinc Segles. Valencia.

RÍOS, S.; SANTALÓ, L. A.; BALANZAT, M. (1979): *Julio Rey Pastor, matemático*. Colección Cultura y Ciencia. Instituto de España. Madrid.

ROCA ROSELL, A. y SÁNCHEZ, J. M. (1990): *Esteban Terradas. Ciencia y Técnica en la España contemporánea*, INTA, Ed. Del Serbal. Madrid.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 584.

INVESTIGATING 19TH-CENTURY MATHEMATICAL JOURNALS: IMPORTANCE AND USE OF OTHER PERIODICALS IN *NOUVELLES ANNALES DE MATHÉMATIQUES* FROM 1842 TO 1870

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Abstract

One way to investigate 19th-century mathematical journals is to see their importance and their use in other reviews. The purpose of my talk is to deal with this subject from an example, Nouvelles annales de mathématiques*, a French journal created by Orly Terquem in 1842, whose announced purpose was to prepare young men for the entrance examination of the Ecole Polytechnique. My study will cover the years 1842 to 1870, trying to answer the following questions:*
How many journals are there? Which journals are mentioned? On which occasions and on what themes? For what scientific, educational or political aims are they employed? Is there any evolution due to different editors, public expectation and political events?
In discussing those points I will focus my analysis, first, on a quantitative and qualitative study to measure the importance of the other periodicals on different subjects and at various times; and second, on a reflection upon the use of these journals in order to serve the editors' goals.

The topic of this communication is to investigate the importance and the use of others journals, by the different editors of *Nouvelles Annales*, from its creation in 1842 to 1870.

I wish to make it clear that what I mean by "other journals" is any regular scientific publication with mathematical articles, such as mathematical journals (*Crelle*, *Liouville*, etc..) but also academic publications (memoirs compilation or proceedings of an academy).

To understand the use of other journals in *Nouvelles Annales*, it is important to know that *Nouvelles Annales* is a journal for youngsters aiming at the Polytechnique and Normale schools.

This is the first page in 1842, which points out the complete title and aim of the journal as well as the editors':

NOUVELLES ANNALES
DE
MATHÉMATIQUES
JOURNAL DES CANDIDATS
AUX ÉCOLES POLYTECHNIQUE ET NORMALE
Rédigé par MM.
Terquem
Officier de l'Université, Docteur es sciences, Professeur aux Écoles Royales d'Artillerie
et
Gérono
Professeur de Mathématiques

It's not a journal for mathematical researchers, it's a journal for students and professors who are concerned by these schools and, more generally, for any people who have an interest for this level of mathematics.

Terquem and Gerono created it in 1842, but, as we see on the first page of the journal, Terquem seems to be the most important editor. *Nouvelles Annales* is his own journal and, as we will see, he uses it to serve his own goals and his own vision of what the teaching should be in high-level schools.

From 1842 to 1870, the editorial staffs were:

- 1842 creation, editors TERQUEM and Gérono

1862: the death of Terquem

- 1863: editors Gérono and Prouhet

1867: the death of Prouhet

- 1868: editors Gérono and Bourget

I would now like to explain what my method was.

At the beginning, I looked at the volumes from 1842 to 1870, page by page, noting all occurrences of other journals and trying to determine the most important years for my study.

After this first glance, it appeared to me that I had to focus on:

1842, 43, 44 because these are the first years of the journal.

1848 because it is the year of a revolution (against the king), which had an influence on École Polytechnique because École Polytechnique students were essentially republicans and because it was in that year, that studies became free at this school.

1850, 51, 52, 53, 54 because there was a very important program change in November 1850, and Terquem vigorously fought against this change, as we will see, using foreign journals until 1854.

1855 Terquem acknowledged himself beaten, and fought in another way.

1863, 64, 65, it is Prouhet's beginning as editor and a great change for *Nouvelles Annales* and the use of other journals.

1868, 69, 70, it is the same thing for Bourget.

For all of these years, I noted for all occurrences, the journal, the author, the subject and the nature of occurrence:

- either a full article of another journal
- or an extract or a revised version of the article
- or a *Nouvelles Annales* article based on another journal's article (contestation, prolongation…)

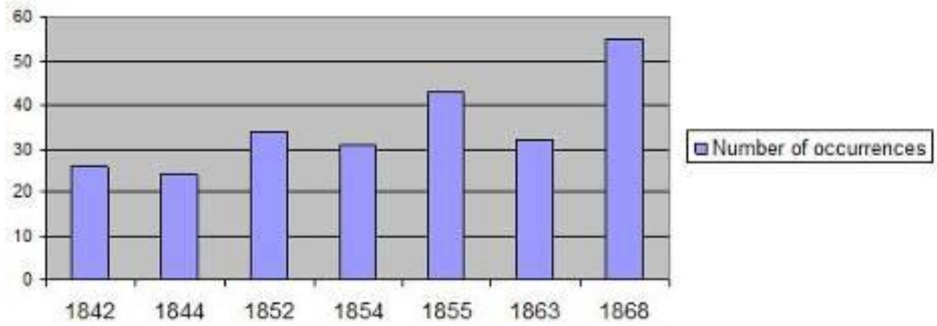
- or a summary
- or claims of priority order
- or questions asked / answers given
- or reference points cited and works used
- or miscellaneous.

After this work, what were the quantitative and qualitative results?

Quantitative results

The first quantitative results are the number of occurrences for every year pointed out.

Years	1842	1844	1852	1854	1855	1863	1868
Number of occurrences	26	24	34	31	43	32	55



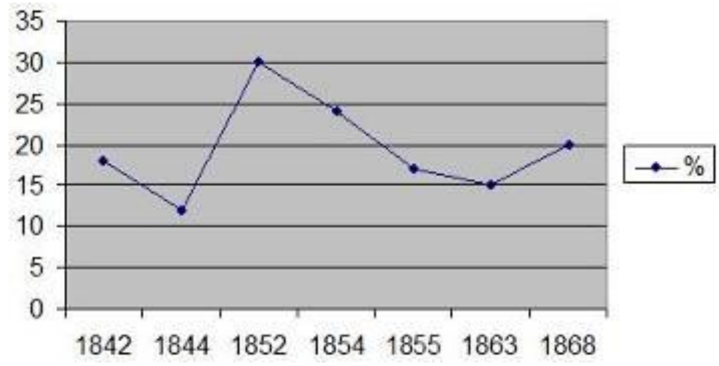
I only put on the graph significant years. We see that occurrences rise from 26 to 55 and that there is a regular progression with Terquem.

There is a peak in 1855 which has a lot of reasons; among these, the creation of a new part of *Nouvelles Annales*: The *Bulletin de Bibliographie, d'Histoire et de Biographie Mathématiques*.

Despite the peak in 1868 with Bourget's beginning as editor, it's not true that other journals had a more important role in *Nouvelles Annales*, in 1868 than in 1855. We see that point now with another graph, knowing that an article from another journal (***f another J***) is a *Nouvelles Annales* (***NA***) article made of

- either a full article
- or selected passages
- or summary
- or analysis
- or a reference to an article published in another journal or in academic publications.

years	1842	1844	1852	1854	1855	1863	1868
f another J	20	15	31	24	21	23	16
Total NA articles	113	129	103	150	121	149	79
%	18	12	30	24	17	15	20

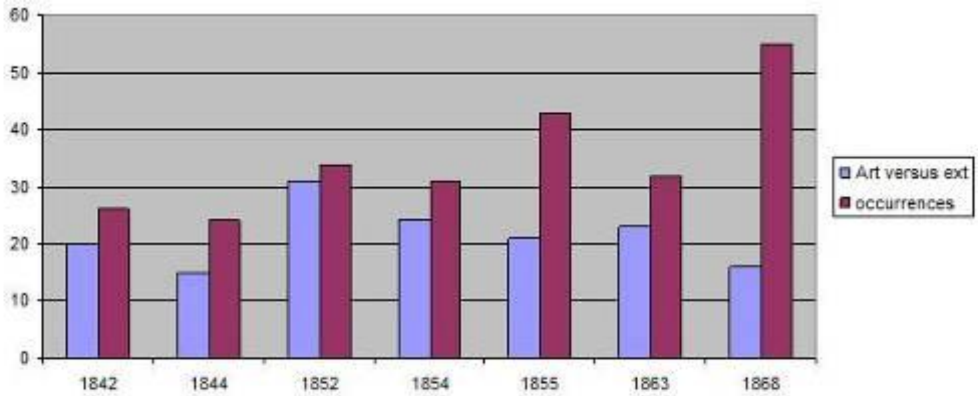


Here we have the curve of the percentage of foreign articles in the *Nouvelles Annales*. We can see that it is in 1852 with Terquem, that there are the most foreign articles in *Nouvelles Annales* whereas Bourget (1868) who had the most occurrences appearing, had far less foreign articles.

There is a trough in 1863 when Prouhet arrives and a peak in 1852 with Terquem's fight against the programs.

Another graph shows that with Terquem, occurrences are most of all, articles from other journals, except from 1855, because Terquem changes his mind and acknowledges himself beaten.

years	1842	1844	1852	1854	1855	1863	1868
f another J	20	15	31	24	21	23	16
occurrences	26	24	34	31	43	32	55



The most remarkable thing is the last diagram: With Bourget, in 1868, there is a peak of occurrences but a trough of articles from other journals because most of occurrences are footnotes reference points, questions, etc.

Qualitative results

For a qualitative study, I took, for every year, the five journals that have the greatest number of quotations.

With Terquem it's *Crelle Journal*, a German Journal very important for researchers that is number one. This is a proof of the use by Terquem of other journals in *Nouvelles Annales*: he tries, with researcher's journals, to raise the level of teaching mathematics in the higher French schools.

With Prouhet in 1863, Italian journals arrive first: the journal of Institute of Bologna and the *Annali delle scienze mathematiche*.

And with Bourget, in 1868, *Nouvelles Annales* sticks more closely to the Polytechnique and Normale schools' program and there are more old journals and French journals: the *Mémoires de l'Académie des sciences de Paris* and the *Annales de Gergonne*, a French journal which is over since 1854.

1842	***Acad Berlin***	***Acad Paris***	***Crelle***	***Gergonne***	***Journal X***
number	**4**	3	3	**7**	**4**

1854	***Arch Math Grunert***	***Camb& Dublin***	***Crelle***	***Gergonne***	***Liouville***
number	**4**	3	**7**	2	**4**

1855	***Acad Paris***	***Ann sc math***	***Astro nachrich***	***Crelle***	***Liouville***
number	**9**	2	2	**10**	4

1863	***Acad Paris***	***Ann sc math***	***Crelle***	***Instit Bologne***	***Liouville***
number	2	**4**	2	**5**	2

1868	***Acad Berlin***	***Acad Paris***	***Bull stor M&P***	***Gergonne***	***Liouville***
number	4	**8**	4	**6**	5

The use of other journals by the different editors of *Nouvelles Annales*

Now I will try to show the use of other journals by the different editors and I begin with Terquem.

1. Terquem (1842-1862)

As we can see in the first example below, articles are clearly noted as being from another journal (here *Crelle's journal*, 1835), in the beginning of the text, the author (here Jacobi) is named at the same time.

DE L'ELIMINATION DE LA VARIABLE

entre deux équations algébriques.

Par C. G. J. JACOBI, professeur à Kœnigsberg. (Crelle, XV, 101, 1835, latin.)

—

1.

Entre les diverses méthodes d'élimination qui ont été proposées, il en est une que je me rappelle avoir lue jadis dans les ouvrages élémentaires composés par le célèbre Bezout, et qui se distingue avantageusement des autres à divers titres. Mon but est d'exposer brièvement cette méthode et d'y ajouter diverses observations (*).

Nous pouvons supposer que les deux équations sont de même ordre; car si l'une des équations est d'un ordre inférieur à l'autre, il suffira d'égaler à 0 les coefficients des puissances manquantes. Soient donc ces équations :

$$f(x)=a_nx^n+a_{n-1}x^{n-1}+a_{n-2}x^{n-2}\ldots+a_0=0,$$
$$\varphi(x)=b_nx^n+b_{n-1}x^{n-1}+b_{n-2}x^{n-2}\ldots+b_0=0.$$

(*) Les auteurs du programme d'admission à l'École polytechnique n'ont pas eu la main heureuse en choisissant parmi les méthodes d'élimination celle du p. g. c. d ; la plus mauvaise, et tellement longue, qu'on a été obligé de *proscrire* la partie essentielle, la discussion des facteurs étrangers. Ce programme aurait besoin d'être totalement remanié, car il domine et entrave l'enseignement.

It's an opportunity for Terquem to give his advice on Polytechnique program and to criticize it in a footnote.

Often Terquem also gives his advice in long commentaries after an article, as we can see in this second example. After a Salmon's article in the *Cambridge & Dublin Mathematical Journal* (1848), in which Salmon says that English students learn cono-spheric curves, Terquem hardly criticizes the higher schools' programs in France:

> «Teaching in France, has been made throughout the year on the basis of conics which nevertheless remain cono-spheric cases, because the plane constitutes a particular case of a sphere. The fact that these curves are not mentioned in colleges is a pity; ***but that they are not even mentioned in the higher schools such as the École Normale and the École Polytechnique, is totally unacceptable.***
> ***To the credit of German and English universities, these curves are commonly taught which is perfectly right***.»

His criticism on the Polytechnique program, about conics, is very strong and he doesn't mince his words.

Terquem uses also articles of foreign journals as an educative tribune and to criticize French teaching.

Giving a summary of the Dublin calendar for 1850, he writes:

> «I read with great interest a report on Oxford University […] It finished with remarkable conclusions that would be of a great interest in France, when the French government will be able to do other things than an arms' factory, sometimes in favour of, sometimes against University and when the principal goal will be education and not hierarchical trivial rewards. ***These questions show what is the state of teaching and enable us to make comparisons that, for mathematics, as I can see, aren't to our advantage. Overseas, there is a spirit of progress: teaching follows science. In France, there is a stationary spirit, often, retrograde spirit***.»

He uses caustic humour to criticize teaching choices for the Polytechnique program, always starting from other journals:

In 1850,

> «In *M. Crelle's Journal* (t. XL, 2) there are three articles on number theory, by M. E.-E. Kummer, a famous professor at Breslau University. M. Crelle has acquired eternal rights to the mathematicians' gratefulness for having published tremendous works on this most noble, generous and sublime part of mathematical science. (<u>See the note at the end</u>)
>
> ***<u>Note</u>. The number theory is very little known and even disdained in France and for good reasons. This theory doesn't make wheels turn round, water gates open, gas condense and, worse than all that, isn't used in examinations; because of these reasons, calculating persons, in a majority position, ask why there is any need to study a theory which doesn't bring anything. My stubborn intelligence doesn't give me any answers to such questions. Rumour has it, by the same calculating mind that the next program for the École Polytechnique will throw away rational mechanics by Lagrange, Laplace, Poisson, and will put in place industrial mechanics***. As I believe in decency, I don't believe in this news: there is some misunderstanding.»

And in 1851, concerning Hess's article in Crelle's Journal:

> «Determinants (Cramerian functions) today rule over all mathematics. So, I suppose, it's a right thing that they have been taken away from the new program. They have been replaced by the auxiliary plane, logarithm, elementary work which have taken fruitfully their place; lovely triad, without forgetting Saint Gunther's ruler that every mathematician needs to have all the time in his pocket or in his hands.»

With the same humour, he also strongly criticizes Le Verrier, the most important responsible of the Polytechnique program, for his choices, in contradiction with his own work, starting from the *Connaissance des temps*, which is a publication of French Institute:

> «In the Connaissance des Temps for 1849, there is, in a Le Verrier's report on Uranus planet, lots of linear equation systems with four unknowns; exercises of the day, taken from good authority. ***At page 169, he uses determinants. That's what we shouldn't copy. Because we have removed these formulae from the Education program; but how would this illustrious mathematician have used them if they hadn't been taught to him?***
>
> (*) The famous mathematician astronomer used the Sturm theorem judiciously; An academician of the same name has taken away from the education program this theorem as belonging to high-level theory, useless thing; A representative of the same name has declared a high theory to be an indispensable thing. Do these three names designate the same person? O. TERQUEM.»

After an article from M. Jacobi, (*Crelle's Journal*, t. XXX, p. 51-94; 1846) concerning a particular system of first degree equations Terquem wrote (1851):

> «Astronomic application: The aim of the present report is not purely analytical. The illustrious author offered to put forward a simple process to solve numerically the equations present in the theory of the secular perturbations of the planets. ***The numeric data are taken from the remarkable work of Le Verrier on the same subject. The compared results show that the Jacobi process is far more exact than the one Le Verrier used***. ***Due to the dire lack of a journal of Astronomy, M. Liouville would fill the gap, as far as he could, to such a shameful lack for the country by inserting in extenso the report of the illustrious Prussian*** and other analogous works in his precious journal also destined to applied mathematics.»

After this vigorous fight, Terquem in 1855 acknowledges himself beaten. He doesn't think that the *Nouvelles Annales* hadn't followed sufficiently the Polytechnique program, but that it was the official program, which hadn't given sufficient attention to the *Nouvelles Annales* and their ideas. That's what he writes in the editorial of 1855:

> «The *Nouvelles Annales de Mathématiques* are entering their fourteenth year and despite the sacrifices we have often imposed ourselves, ***they haven't yet, we have to admit, been able to give us a just remuneration***. Would they have missed the goal pursued, to offer youngsters aiming to the Ecoles Polytechnique and Normale the solutions to problems that can interest them most? We think not. In fact, the *Annales* have dealt with the beautiful and difficult questions of passed exams, and also ***the questions of the new exams, when they presented some interest***. There was given numerical calculus exercises, logarithms with a scope that are to be found nowhere else. ***Despite the narrow position given to the Nouvelles Annales by the official programs,*** we shall as from January 1855 make new efforts in the interest of science.»

And he announces the creation of the *Bulletin de Bibliographie, d'Histoire et de Biographie mathématiques*, where he will put from now on, the most parts of articles of others journals:

> «***Each month there will include an added brochure with at least a pagination of its own under the title: Bulletin de Bibliographie, d'Histoire et de Biographie mathématiques, par M. O. Terquem***. The title of the brochure makes the object clear. The history of Science is that of the human mind […] All the dignity of Man is in the thought process. Mathematics are a perpetual thought process. May we be worthy of our new mission!»

2. Prouhet (1863-1867)

It is Prouhet's arrival, which gives a greater respect of programs.

He decides the end of the *Bulletin*, initiated by Terquem as we have seen before, and gives fewer articles of other journals but more quick summaries in biographical articles.

In fact, the *Nouvelles Annales* loose their great originality, due to Terquem, to become only a way to prepare Polytechnique and Normale examinations seriously.

The part "questions asked / answers given" takes more and more place in the journal, and when articles are chosen, they are articles the subject of which is part of the program examination.

For the articles of other journals, he takes only fragments and he rewrites them. He only puts as a footnote the sources of the articles, such as the author and the name of the journal, whereas Terquem is more explicit on his sources and makes it perfectly clear where his articles are taken from.

We can see the difference between the two editors, looking at the first example of Terquem (article by Jacobi taken from the *Crelle's journal*) and at the example of Prouhet below:

It is only in a footnote that we learn in which journal and from what author Prouhet took the article.

Prouhet definitively stops using other journals with the aim of giving advice and criticism on the programs.

SUR LE NOMBRE DES DIAGONALES D'UN POLYÈDRE (*).

Je désignerai par f le nombre des faces, par s le nombre des sommets et par a le nombre des arêtes du polyèdre. On sait qu'il existe entre ces trois quantités la relation

$$f + s = a + 2 \tag{1}$$

découverte par Euler.

(*) Ce problème a été traité par M. Henri Binder dans les *Archives* de Grunert, t. VIII, p. 221.

3. Bourget (1868-1870 –the end of our study)

This trend increases with Bourget as editor. He chooses to put in the *Nouvelles Annales* still fewer articles from other journals and more historical articles than mathematical articles. The fact that an article is taken from another journal is much worse indicated as it was by Prouhet.

As an example which we can see below, we will know that «Etude sur des surfaces algébriques», is taken from the *Journal des savants*, and its author only at the end of the article, and written in very small letters:

c'est-à-dire que par chaque point du plan il passe deux ovales se coupant à angle droit, dont l'étude a conduit récemment M. Darboux à une démonstration nouvelle et fort élégante du célèbre théorème d'Euler, sur l'addition des fonctions elliptiques. J. Bertrand.

(Extrait du *Journal des Savants*.)

Bourget uses of other journals most of all in footnotes and used as reference points. He respects official programs and so the articles of *Nouvelles Annales* are, for the most, of the Polytechnique and Normale schools' level and made especially by authors who write for the journal. *Nouvelles Annales* becomes a journal which lives essentially in autarky.

Conclusion of this study from 1842 to 1870

For these years, the most important editor was Terquem, who founded *Nouvelles Annales* in 1842. He wanted to use the journal for the aim of influencing on the program examination of Polytechnique and Normale schools and, more generally, French teaching, in the sense of higher level.

He took articles in researchers journals (*Crelle*, *Liouville*, etc.) that he put in *Nouvelles Annales* as full articles or long selected passages, by one hand to improve the level of students and teachers who bought his journal, and by another hand to write what the government and the men responsible for mathematical French teaching had to do.

He also took articles on the history of mathematics to give examples to follow and a more general mathematic culture to youngsters.

For all articles, the author and the original journal are mentioned at the beginning and very clearly, and Terquem used of their authority to serve his purpose and to give lessons to the ministry of French education and his staff.

It is only in 1855 that he changed his mind, seeing that his journal was not very much bought, because of the non-respect for the real program examination. So he took minus mathematical articles from researcher journals and more from books or revues of mathematical history, creating the *Bulletin de Bibliographie, d'Histoire et de Biographie mathématiques* in an appendix of *Nouvelles Annales*.

His successors, Prouhet and Bourget, were more classical. They did not use many articles from researcher's journals, and the few they used were in footnotes or in cited referenced points. Criticism of French teaching and of program examination, which was the principal aim of articles of other journals with Terquem, disappeared completely. And if the *Nouvelles Annales* was more sold, it lost (from 1863 to 1870) his high mathematical and culture level, his function of mathematics teaching consciousness and, as Terquem said, its mission: «All the dignity of Man is in the thought process. Mathematics are a perpetual thought process. May we be worthy of our new mission!»

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 593.

THE *RIVISTA DI GIORNALI* (1859-1879) AND THE CIRCULATION OF THE EUROPEAN MATHEMATICAL CULTURE IN 19TH CENTURY ITALY: A CASE STUDY

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Abstract

This presentation examines the emergence of the Journals of Mathematics in the 19th century and their role in the diffusion and the progress of this science as practiced in 19th century Italy by Giusto Bellavitis (1803-1880) in the several issues of his Rivista di Giornali*, published from 1859 to 1879.*

The present report stays within the field of the researches concerning the PRIN project on "The birth of the Italian School of Mathematics: Publications of Electronic Archives and Correspondences" coordinated by Prof. Aldo Brigaglia from Palermo University. In the project, for what Giusto Bellavitis is concerned, his correspondence with Placido Tardy, an Italian mathematician and also Rector of Genoa University, has been recently studied. In his letters Bellavitis announced the start of the publication of his *Rivista di Giornali* in 1859, in the *Atti dell'Imperial Regio Istituto Veneto di Scienze Lettere ed Arti* in Venice, and he informed about the following issues up to 1879. Our aim is to consider the topics the *Rivista di Giornali* deals with and its circulation in the main universities and research centers. In particular our interest is focused on reviews related to the articles published in the *Comptes Rendu*, in the *Nouvelles Annales de Mathématiques, in the Journal de Crelle, and in the Annali di Matematica pura e applicata*.

As for Giusto Bellavitis's biography we can give a brief outline.

He was born in Bassano del Grappa (Vicenza) on the 22nd November 1803 and he died in Tezze (Vicenza) on the 6th November 1880. He began his studies under the guidance of his father Ernesto, but he was essentially a self-taught person and this experience made him especially sensitive to teaching.

After some time spent as a municipal employee in 1843 he was appointed Mathematics teacher at the Liceo of Vicenza.

In 1845 he was awarded an honorary degree in Philosophy and Mathematics (*Laurea honoris causa*) at Padua University where he taught descriptive geometry, then advanced geometry, theory of probability, physics, complementary algebra and finally co-ordinate geometry. In 1840 he became a member of the Istituto Veneto. Elected member of the Society of XL in 1850, in 1855-57 served as Inspector of Scuola Reale Superiore di Venezia, then in 1866 he became Senator of the Regno d'Italia, and in 1866-67 Rector of Padua University.

In 1880 he died accidentally tumbling down the stairs at home.

He published 223 works and the *Rivista di Giornali*, and he gave relevant contributions to analysis, resolution of equations, curves classification, complex numbers algebra; he also developed some studies in physics, in particular optics and electrophysics and in chemistry.

He studied algebra of imaginary numbers on geometric base and he achieved his first work on *equipollenze* in 1832. In 1835 [1] he published a fundamental paper on this topic influenced by some articles and books by Buée [5], Poncelet [16] and Steiner [17].

Previous works on the same topics, but unknown to Bellavitis in 1832, were developed by Carnot [8] and Möbius [15]. Similar methods were later developed by Grassman [12] and Hamilton [13][1].

The scientific work of Bellavitis has been extensively analyzed in recent publications [4], [6], [7], [9], [10], [11].

Bellavitis together with Genocchi, Tardy and Chelini belongs to a generation which played a relevant intermediary role between the group of the old masters in the pre-unitarian Italy (Bordoni, Piola, Chiò, Mossotti) and the generation of young mathematicians (Beltrami, Betti, Brioschi, Casorati, Cremona) who developed their research after the Italian unification. Bellavitis and his colleagues gave relevant contributions to the birth of the Italian school of Mathematics thanks to their international relationships[2] and in particular Bellavitis can be considered not only one of the vector calculus founders, thanks to his method of *equipollenze*, but also the main introducer in Italy of the new European developments in this field.

The *Rivista di Giornali* was published from 1859 to 1879 in *Atti dell'Imperial Regio Istituto Veneto di Scienze Lettere ed Arti* in Venice.

It is worth noticing that Bellavitis started publishing his *Rivista* in the year following Betti's, Brioschi's and Casorati's European journey in September 1858 as if he wanted to open Italy to wide perspectives or to strengthen the attempt to "make Italy worldly".

The First Issue of the Rivista was published on the 22nd August 1859, with the title:

"Rivista di alcuni articoli dei Comptes Rendu dell'Accademia delle Scienze di Francia del prof. Giusto Bellavitis, Membro effettivo dell' I.R. Istituto Veneto di Scienze, Lettere ed Arti".

From Comptes Rendu we give a list of the arguments considered by Bellavitis (on the left of the following table) and the authors of the articles on these arguments (on the right of the table):

Interpolazione approssimata	Hermite[3]
Generazione spontanea	Quatrefages
Corpi semplici	Dumas and Despretz
Spiriti picchiatori	(Conversazione)

[1] In a previous session of this Conference a very interesting lecture on these authors has been delivered by Josep Manel Parra Serra (Barcelona University) "Geometric Algebra versus Vector Algebra as Physical Mathematics: Bridging Past and Future".

[2] The journey made in 1858 by Betti, Brioschi and Casorati in the most relevant European centers of research –organized by Genocchi and Tardy– has played a central role in the innovation of Italian mathematical research and has been extensively quoted by Volterra at the International Congress held in Paris in 1900 [18].

[3] The authors and the journals are quoted from the original issues of the *Rivista* frequently without the initials of the first name and according to the abbreviations used by Bellavitis.

The Second Issue was published, with the same title of the First, on the 18th June 1860. In the following table the subjects treated and the corresponding authors are listed.

Risoluzione delle equazioni e decomposizione delle frazioni	Valz, Ferola, Montucchi, Vieille
Ricerche sui numeri primi	Polignac
Formule per le congruenze	Sylvester
Condizione di decomposizione delle cubiche	Painvin
Cangiamento di variabile indipendente	Spitzer
Calcolo delle variazioni	Lindeloff
Porismi di Euclid	Chasles
Proprietà generali dei poliedri	Prouhet, Bertrand
Proprietà dell'ellisse	Lino de Pombo
Curve e superficie derivate	W.Roberts
Coni omofocali	Chasles
Generazione delle curve gobbe	Jonquieres
Carte geografiche	Tissot
Linee di curvatura	Aoust
Coordinate curvilinee	Lamé and Valson
Curvatura delle superficie	Babinet
Equivalenza delle copie	Tessan
Urto dei corpi	Poisot
Moto della terra	Perrot
Macchina pneumatica	Gairaud
Alimentatore delle caldaie a vapore	Gairaud
Ascensione capillare	Jamin
Cangiamento di volumi di alcuni sali	Tissier
Calore sviluppato nelle combinazioni chimiche	H. Sainte-Claire Deville
Ombre colorate	Babinet and Fournel
Striscie oscure negli spettri colorati	Robiquet
Aberrazione della luce	Fizeau, Faye and Tessan
Comete	Faye
Calcoli Astronomici	Puiseux
Fulmini Globulari	Tessan
Nuove pile voltiane	Davy
Produzione dell'ozono	Le Roux
Equivalenti chimici	Baudrimont
Agricoltura	S. Cloez and E. Dufour
Ipnotismo	Tigr

As we can see, Bellavitis's interest as a reviser is mostly oriented to the French culture and research and to one of its most important journal: *Comptes Rendu*.

Being particularly sensitive to French culture in the title of the Third Issue Bellavitis referred to another prestigious journal: the *Nouvelles Annales de Mathématiques*. In fact in March 1861 the title of the *Rivista* changed and became "*Rivista di alcuni articoli dei Comptes Rendu dell'Accademia delle Scienze di Francia e di alcune questioni des Nouvelles Annales de Mathématiques del prof. Giusto Bellavitis, Membro effettivo dell' I.R. Istituto Veneto di Scienze, Lettere ed Arti*".

The arguments treated and the corresponding authors from the Comptes Rendu are the following:

Macchina da calcolare	Dubois
Criterio per la separazione delle radici	Turquan
Numeri bernoulliani	Thoman
Somma di degli angoli di un poliedro anche stellato	Valat
Classificazione dei poliedri	Bretond
Cellette degli alveari	Villich
Relazioni di posizione tra due figure uguali	Chasles
Linee di curvatura dei ditomoidi	Aoust
Carte geografiche	Tissot
Ombra dell'elica	Dunesme
Pendolo di Foucault	Dehaut and Laroque
Derivazione in Meccanica applicata alla doppia rifrazione	D'Estocquois
Stato globulare dell'acqua	De Luca and Sudre
Freddo artificiale	Carre
Pluvioscopio	Herne and Mancon
Influenze della luna	Faye
Tuono musicale	Govi
Della visione	Giraud and Teulon
Generazione spontanea	Pouchet, Pasteur and Mantegazza
Concepimento	Demaux
Antidoto per l'eterizzazione	Ozanam and Zinno
Agricoltura	Guerin- Meneville and Paravet

The following "Quéstions", taken from the *Nouvelles Annales de Mathématiques*, are dealt with and Bellavitis gives his own solutions:

Teorema del Côtes
Sul perimetro dell'ellisse
Classificazione di una tritoma
Generazione di una triattomena
Tetraedro dato dalle posizioni di tre altezze
Problema di situazione
Disposizione sullo scacchiere di otto regine

Therefore the first three years of the *Rivista* were meant to introduce in Italy ideas and results from the European scientific culture, the French one above all.

From the Fourth Issue on, dated 1862, the title simply became: *Rivista di Giornali*, still published in Venice at "Istituto Veneto". Here Bellavitis greatly changed the organization of his critical revisions.

Author's specifications about his aim are very relevant:

> *"The increasing number of valuable mathematical works published in Europe and America […requires] a well structured collection of all the wide doctrine […] and longs for a repertoire of everything that is going to be published in mathematical science […]. It takes a long time to understand the meaning of denominations and operational symbols, and to get a right idea of the aim and the main results […]. Furthermore it is difficult to collect all the reviews that are periodically published and to understand the different languages they are written in. […]. Now, the mathematicians would avoid this reciprocal waste of time by publishing the articles of a Repertoire; and everyone could study the article he liked most […].*
> *Allow me to outline the arrangement of a repertoire suitable for my own use. In one book only, the different categories of human knowledge are included in separate pages; for each category I write down all the articles and abstracts following a progressive number, by registering the author's name and the title of the review or the work in general, and the quotations of the previous or following articles, related to it in proper columns […]"*

We can notice that Bellavitis refers to the style of his "Otto Repertori", a fundamental subject in his personal studying method and scientific data collection. In a "Repertorio" he registered theorems, problems, theories, debates, proposed or solved by the most important mathematicians of the time, following a chronological and thematic order, containing precise information about the author, the article and the journal. That thanks to the "Repertori" it is evident one could verify immediately if a given subject was been discussed, by whom, in what period and in what issues. Bellavitis also points out some subjects he deals with in the "Repertori":

> *"aritmetica, algebra, geometria (elementare, descrittiva, piana, sferica, etc.), geodesia, analisi della probabilità, meccanica, idraulica, meccanica dell'universo, merilogia ossia scienza delle azioni molecolari e dei relativi fenomeni, teoria delle vibrazioni ed acustica, aerologia, scienza del calorico, ottica, azioni chimiche della luce, magnetismo, elettricismo, chimica, meteorologia, astronomia, mineralogia, fitologia, zoologia, microbiologia, geografia fisica e geologia, arte dell'ingegnere, agricoltura ed economia domestica, medicina, arti, belle arti ed estetica, letteratura, geografia e statistica, storia civile, storia letteraria, filosofia, scienze sociali, scienze naturali, biografie, miscellanea."*[4]

In the following list we give the arguments and the authors of the articles from the Comptes Rendu analized in the Fourth Issue of the *Rivista* (1862):

Algebra	Heegmann; Sylvester
Aritmetica	C. Polignac
Calcolo sublime	Rouché
Geometria dello Spazio	C. Polignac; Chasles; Transon, Chasles and Bonnet; Bour and Codazzi; Sylvester and Chasles; Sylvester; Cremona
Geodesia	Faye
Meccanica dell'Universo	Abbadie
Scienza del Calorico	Carvallo, Clausius; A.Loir and Ch. Drion; L.Dufour
Ottica	Giraud-Teulon; Faye; Briot
Elettricismo	Marié and Davy; Moncel
Chimica	Fremy

[4] A first analysis can be found in N. Legnazzi [14] pp. 25-27 and footnotes from 24 to 29 (pp. 88-105).

Metereologia	Renou; Houzeau
Astronomia	Faye
Zoologia	Em. Rousseau
Microbiologia	Pouchet; Pasteur; A. Terreil
Geografia fisica	Griniaud de Caux
Agricolt.e Ec. Dom.	G.Saint-Hilaire; Guerin-Meneville

Here the data from *Nouvelles Annales de Mathématiques*:

Algebra	Painvin
Geometria piana	Paul Serret; Mannheim; Jonquières
Geometria dello spazio	Faure

In his introduction of Fifth Issue of the *Rivista* (1862) Bellavitis explains:

> *"Although what I define as a review has the structure of a Repertoire, it is evident that more often it is only the expression of my personal point of view; and whoever wants to get the right idea of the whole articles will have to read them in the original form, for I wish I could be an inciter. Even the quotations I made are not meant to introduce the history of every single subjects, I won't do anything but giving the example of what could be useful to let the young scholar be guided in the right way to get the knowledge of the wide field every mathematical question is a part of."*

and in the third part of the same issue he specifies:

> "I wish these debates could entreat the young people living in our provinces to study the fruitful geometrical theories, and for their benefit I add the missing quotations, as far as I know […]
> [...] It seems regrettable to me that in some articles even one only word, whose meaning is not generally adopted ,is used without being explained, as feeling like the author disdains from reading his work all those who do not consider foreign works, where that word was used, in their thinking."

In the fourth part of the Fifth Issue Bellavitis pays great attention to some articles by the Italian mathematicians. In the following we give the lists of the periodicals examined and the topics considered:

> Topics:
> *Arti scientifiche, Algebra, Calcolo sublime, Geometria piana, Geometria elementare, Geometria dello spazio, Ottica , Meccanica, Meteorologia, Mineralogia*
>
> Periodicals:
> *Annali di Tortolini, Memorie Istituto di Bologna, Nouv. Ann. Math., Comptes Rendu, The Quart. J. of Math., Rend. e Mem. Accad. Napoli*

It is interesting to analyze the aims of the reviewer. The first three issues deal with French and German periodicals in order to spread into the Italian community new ideas and recent results of the European research centres, while in the following issues Bellavitis' aim consists in involving new generations into new research subjects and into close confrontation with European scientists.

Also the Sixth Issue of *Rivista di Giornali* (1863) contains very useful observations to understand Bellavitis's thought:

> "The topics dealt with by most journals stimulates experimentation and confront between the different methods followed by modern science: among the problems raised in the *Nouvelles Annales par Terquem et Gérono* I solved some of them, the ones under the numbers [...]"
> "I do not think useless a new publication of the questions posed and not yet solved in order to stimulate Italian Geometers [...]
> In the *Giornale di Napoli*, which tries to follow the example of Terquem, some interesting questions are posed [...] The new *Giornale di Napoli*, interested in the diffusion of the mathematical knowledge and in stimulation of young minds, accepts the articles by these young researchers, who in the future will join the crown of mathematicians with which the city shines"

The following lists indicate topics and periodicals considered from the Sixth Issue to the Fifteenth one.

Sixth Issue *Rivista di Giornali* – 1863

Topics:
Algebra, Geometria piana, Geometria dello spazio, Idraulica, Calcolo sublime, Geometria elementare e descrittiva, Meccanica

Periodicals:
Atti Istituto Lombardo, Annali del Tortolini, Rendiconti e Memorie Accademia Napoli, Journal Crelle, Arch. Von Grunert, Memorie Istituto di Bologna, Memorie Accademia di Torino, Giornale Matematico di Napoli, Annali delle Università Toscane

Seventh Issue *Rivista di Giornali* – 1864 and 1865

Topics:
Arti Scientifiche, Scienze sociali, Algebra, Fitologia, Geometria piana, Geometria dello spazio, Geometria sferica, Calcolo sublime, Geometria elementare e descrittiva, Meccanica

Periodicals:
Atti Istit. Lomb., Rend. e Mem. Accad. Napoli, Journal Crelle, Mem. Istit. Bologna, Mem. Accad. Bologna, Giorn. Mat. di Napoli, Nouv. Ann. Math. par Terquem et Gérono, Comptes Rendu, The Quart. J. of Math., Memor. Soc. Ital, Proceeding of the R. Society, The Educat. Times

Eighth Issue *Rivista di Giornali* – 1866 and 1867

Topics:
Algebra, Elettricismo, Geografia, Geometria piana, Geometria dello spazio, Letteratura, Calcolo sublime, Geometria elementare e descrittiva, Meccanica

Periodicals:
Atti e Rend. Istit. Lomb., Bull. Acad. Pétersburg, Rend. e Mem. Accad. Napoli, Journal Crelle, Mem. Istit. Bologna, Ann. del Tortolini, Giorn. Mat. di Napoli, Nouv. Ann. Math. par Terquem et Gérono, Memor. Soc. Ital., Philos. Magazine, Proceeding of the R. Society, Messeng. of Math., Ann. Der Phys

Ninth Issue *Rivista di Giornali* – 1868 and 1869

Topics:
Algebra, Geometria piana, Geometria dello spazio, Calcolo sublime, Geometria elementare e descrittiva

Periodicals:
Atti Lincei, Atti dell'Ist.d'Incor. in Napoli, Comptes Rendu, Journal Crelle, The Quart. J. of Math., Ann. Del Tortolini, Giorn. Mat. di Napoli, Nouv. Ann. Math. par Terquem et Gérono, Journal Liouville

Tenth Issue *Rivista di Giornali* – 1870 and 1871

Topics:
Algebra, Analisi delle probabilità, Fisica, Geometria piana, Geometria dello spazio, Geometria sferica, Geometria elementare e descrittiva, Meccanica, Meteorologia

Periodicals:
Annali di matem. di Milano, Ann. del R. Museo industriale italiano di Torino, Ann. R. Ist. Tecnico Udine, Atti Accad. Gioenia, Atti Istit. Veneto, Comptes Rendu, Giornale dell'Ital. Letter., Journal Crelle, The Quart. J. of Math. London, Mem. Istit. Bologna, Giorn. Mat. di Napoli, Nouv. Ann. Math. par Terquem et Gérono, Memor. Soc. Ital, Journal Liouville

Eleventh Issue *Rivista di Giornali* – 1871, 1872 and 1873

Topics:
Algebra, Geometria piana, Geometria dello spazio, Geometria elementare e descrittiva, Calcolo sublime, Idraulica, Meccanica

Periodicals:
Annali di matem. di Milano, Bullet. Sc. Math. Paris, Comptes Rendu, The Quart. J. of Math. London, Giorn. Mat. di Napoli, Nouv. Ann. Math. par Terquem et Gérono, Il Filocritico, Annali Sc. Norm. di Pisa, Trans. Cambr. Phil. Soc., Archiv der Math.Von Grunert, Politecnico di Milano

Twelfth Issue *Rivista di Giornali* – 1873 and 1874

Topics:
Arti Scientifiche, Aritmetica, Algebra, Analisi delle probabilità, Astronomia, Calorico, Chimica, Elettricismo, Filosofia, Fisica, Fitologia, Geometria piana, Geometria dello spazio, Geometria sferica Geometria elementare e descrittiva, Calcolo sublime, Geodesia, Meccanica, Meteorologia, Microbiologia, Mineralogia, Ottica, Scienze sociali, Zoologia

Periodicals:
Abhandl. Böhm. Gesellsch., Analectes. Athènes , Atti Lincei, Atti Accad. Torino , The Quart. J. of Math. London, J. École Polytéchnique , Giorn. Mat. di Napoli, Nouv. Ann. Math. par Terquem et Gérono, Archiv der Math.Von Grunert

Thirteenth Issue *Rivista di Giornali* – 1876 and 1877

Topics:
Acustica, Arti Scientifiche, Algebra, Analisi delle probabilità, Calorico, Geometria piana, Geometria dello spazio, Calcolo sublime, Geometria elementare e descrittiva

Periodicals:
Assoc. Franç. Pour l'avancement. des sciences, Atti R. Accad. Torino, Atti Istit. Veneto, Atti e Rend. Istit. Lomb, Rendic. e Mem. Accad. Napoli, Nouvelles Annales Mathématiques par Terquem e Gérono

Fourteenth Issue *Rivista di Giornali* – 1877

Topics:
Algebra, Chimica, Geometria piana, Geometria dello spazio, Calcolo sublime, Geometria elementare e descrittiva, Meccanica,

Periodicals:
Archives Néerlandaises, Atti R. Accad. Torino, Comptes Rendu, Rendic. e Mem. Accad. Napoli, Giorn. Matem. Di Napoli, Nouvelles Annales Mathématiques, par Terquem e Gérono, Mem. Spettroscopisti Italiani, Mém. Soc. de Bordeaux, Mem. Accad. di Bologna, Monthly Notice of the R. Astron. Soc.

Fifteenth Issue *Rivista di Giornali* – 1879

Topics:
Algebra, Geometria piana, Geometria dello spazio, Geometria sferica

Periodicals:
Atti Istit. Veneto, Nouvelles Annales Mathématiques par Terquem e Gérono

An article published in 1869 by M.J.Houel on the *Nouvelles Annales de Mathématiques* (2° serie, tome VIII) "Sur le calcul des équipollences, methode d'analyse géometrique de M. Bellavitis" witnesses of the attention shown by the European scientific community to the *Rivista di Giornali*. The author describes the methods of the *equipollenze*, and quotes from *Rivista di Giornali* intending to report on some research by Bellavitis in the following issues of the *Nouvelles Annales*.

A more extensively research has been developed where we try to reconstruct the map of the diffusion of the *Rivista* in the most important European Universities and research centres. As for a first overview we can list the cities and the libraries where some or all the issues of the *Rivista* have been registered:

Innsbruck (Universitaätsbibliothek), Wien (Östtereichische Akademie der Wissenschaften, Bibliothek), Paris (Bibliothèque Nationale de France), Paris (Bibliothèque de la Sorbonne), Edinburgh (National Library of Scotland), London (The British Library), Berlin (Staatbibliothek zu Berlin), Braunschweig (Universitaätsbibliothek), Dresden (Sachsische Landesbibliothek-Staats-und Universitaätsbibliothek), Düsseldorf (Universitaäts-und Landesbibliothek), Göttingen (Niedersächsische Staat-und Universitaätsbibliothek), Halle/Saale (Universitäts-und Landesbibliothek Sachsen-Anhalt/Zentrale), Heidelberg (Universitaätsbibliothek), Leipzig (Universitaätsbibliothek "Biblioteca Albertina"), München (Universitaätsbibliothek), Resengurg (Universitaätsbibliothek), Passau (Staatliche Bibliothek).

A more detailed account of the diffusion of this periodical will be available in further articles.

Essential references

[1] BELLAVITIS, G. (1835) "Saggio di applicazioni di un nuovo metodo di Geometria analitica (Calcolo delle equipollenze)", *Annali delle Scienze del Regno Lombardo-Veneto*, T.V, pp. 2-18.

[2] BELLAVITIS, G. *Rivista di Giornali*, in Atti *Istituto Veneto di Scienze, Lettere e Arti*:
(1859) *Rivista di alcuni articoli dei Comptes Rendu dell'Accademia delle Scienze di Francia*. T. IV, p. 1109
(1860) *Seconda Rivista di alcuni articoli dei Comptes Rendu*. T. V, p.821
(1861) *Terza Rivista di alcuni articoli dei Comptes Rendu della Accademia delle Scienze di Francia e di alcune questioni di Nouvelles Annales de Mathématiques*. T. VI, p. 376, 625
(1862) *Quarta Rivista di giornali*. T. VII, p. 5, 123
(1862) *Quinta Rivista di giornali*. T. VII, p. 244, 249, 619, 889
(1863) *Sesta Rivista di giornali*. T. VIII, p. 171, 533, 921, 1266
(1864 – 1865) *Settima Rivista di giornali*. T. IX, p. 304, 405 – T. X, p.17, 124, 139, 307, 1019, 1335
(1866 – 1867) *Ottava Rivista di giornali*. T. XI, p. 275, 880 – T. XIII, p. 53
(1868 – 1869) *Nona Rivista di giornali*. T. XIII, p. 1461 – T. XIV, p. 455, 1249, 1993
(1870 – 1871) *Decima Rivista di giornali*. T. XV, p. 840, 1659 – T. XVI, p. 729, 1651
(1871 – 1872 – 1873) *Undecima Rivista di giornali*. T. XVI, p. 2297 – IV. Serie, T. I, p. 393 – T. II, p. 382
(1873 – 1874) *Duodecima Rivista di giornali*. T. II, p. 1197 – T. III, p. 203, 311, 1035, 1179, 1223
(1876 – 1877) *Tredicesima Rivista di giornali*. T. II, p. 121, 163, 317 – T. III, p. 173
(1877) *Quattordicesima Rivista di giornali*. T. III, p. 247, 357, 1069, 1099, 1147
(1879) *Quindicesima Rivista di giornali*. T. V, p. 299

[3] BELCASTRO, A.; CANEPA, G.; FENAROLI, G.; MODONESI, M. (2003) "Alcuni manoscritti relativi all'insegnamento del calcolo delle probabilità presenti nelle carte di Giusto Bellavitis (1803-1880)", *Atti Istituto Veneto di Scienze, Lettere ed Arti*, vol. 161, II, pp. 331-370.

[4] BORGA, M.; FREGUGLIA, P.; PALLADINO, D. (1985) *I contributi fondazionali della scuola di Peano*, Franco Angeli, Milano.

[5] BUÉE, M. (1806) "Mémoire sur les quantités imaginaries", comunicated by William Morgan, Read June 20, 1805, *Philosophical Transactions*, 1806, pp. 23-88.

[6] CANEPA, G. (1994) "Le carte Bellavitis", in *Le Scienze Matematiche nel Veneto dell'Ottocento*, Venezia, Istituto Veneto di Scienze, Lettere ed Arti, pp. 49-59.

[7] CAPARRINI, S. (2003) "Early theories of vectors", in M. CORRADI, A . BECCHI, F. FOCE, O. PEDEMONTE (Eds.), *Essays on the History of Mechanics: in Memory of Clifford Ambrose Truesdell and Edoardo Benvenuto*, Birkhäuser, Basel-Boston-Berlin,, pp.179-198.

[8] CARNOT, L. (1803) *Géométrie de Position*, Paris.

[9] CORRADI, M.; BECCHI, A.; FOCE, F.; PEDEMONTE O. (Eds.) (2003) *Essays on the History of Mechanics: in Memory of Clifford Ambrose Truesdell and Edoardo Benvenuto*, Birkhäuser, Basel-Boston-Berlin.

[10] FREGUGLIA, P. (1992) *Dalle equipollenze ai sistemi lineari*, QuattroVenti, Urbino.

[11] FREGUGLIA, P. (1994) "Il calcolo delle equipollenze di Giusto Bellavitis", in *Le Scienze Matematiche nel Veneto dell'Ottocento*, Istituto Veneto di Scienze, Lettere e Arti, Venezia, pp. 11-48.

[12] GRASSMAN, H. G. (1844) *Die Lineale Ausdehnungslehre ein neuer Zweig der Mathematik*, Leipzig.

[13] HAMILTON, W. R. (1853) *Lectures on Quaternions*, Dublin.

[14] LEGNAZZI, N. (1881) *Commemorazione del Conte Giusto Bellavitis letta il 6 Dicembre 1880 trigesimo della sua morte nell'Aula Magna della Rª. Università di Padova*, Padova.

[15] MÖBIUS, A. (1827) *Der barycentrische Calcul*, Leipzig.

[16] PONCELET, J. V. (1822) *Traité des propriétés projectives des figures*, Paris.

[17] STEINER, J. (1832) *Systematische Entwickelung der Anbhangigkeit geometrischer Gestalten von einander*, Berlin.

[18] VOLTERRA, V. (1902) "Betti, Brioschi, Casorati, trois analystes italiens …", *Compte rendu du deuxième Congrès international des mathématiciens tenu à Paris du 6 au 12 août 1900*, Gauthier-Villars, Paris, pp. 43-57.

APPLIED BIOLOGY: PRACTICAL TASKS AND FUNDAMENTAL RESEARCH

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 603.

THE INSTITUTIONALIZATION OF APPLIED BIOLOGICAL RESEARCH IN RUSSIA: BIOLOGY AT ST. PETERSBURG

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Abstract

Conventionally, the establishment of the V.I. Lenin All-Union Academy of Agricultural Sciences (the VASKhNIL) in 1929 has been considered as an outcome of rapid institutionalization of biological research in line with the Bolsheviks' agenda. In reality, scholars themselves initiated and supported the process using Bolshevik authorities in order to implement their own vision of the advancement of science in Russia. The foundation of VASKhNIL was not the beginning but the result of almost a century-long process of institutionalization of applied research in biology that began in 1837 inside the Ministry of State Domains. The Ministry was active in promoting new methods of land cultivation and rational use of natural resources. Under the aegis of the Ministry various academic committees and commissions were established that included agronomists, cattle breeders, ichthyologists, soil scientists, entomologists on their boards. When in 1894 the Ministry of State Domains was reorganized into the Ministry of Agriculture and State Domains, its Academic Committee defined the research agenda, planned and supervised research in the fields of agriculture and management of biological and water resources. The Academic Committee set up agricultural laboratories and experimental stations, and outlined their research programs and instructions. There were even plans for establishing the Central Institute of Agronomy, however in practice its academic and administrative tasks were divided among various bureaus affiliated with the Academic Committee (for entomology, applied biology, zootechnology, soil science, zoology of fur-bearing animals and marketable fish, etc) and created in 1894-1899. In 1901 an Academic Commission was established at the Academic Committee, which was concerned with organizing agricultural research institutions, experimental studies in agriculture and seed-farming. It was these bodies (bureaus and the Academic Commission) that eventually became the foundation for the future institutes of the VASKhNIL with Nikolai I. Vavilov serving as its first president.

Introduction

It was previously assumed that the fields of applied and fundamental research in pre-Revolutionary Russia were separated by institutional barriers; different scholars were involved in carrying out projects in these areas. Fundamental research was advanced first and foremost at the Imperial Academy of Sciences and the St. Petersburg University, whereas applied research was pursued by various governmental agencies and ministries. Therefore historians have usually claimed that intensive institutionalization of applied biological research in St. Petersburg (Leningrad) began after the 1917 Revolution and was caused by the Soviet government urging scientists to address practical problems. Subsequently a rapid growth of research institutions in agricultural sciences became one of the reasons for the rise of Lysenkoism in the 1930s. However recent studies have changed our understanding of these processes. In this year a research project ‘Biology at St. Petersburg: an encyclopedic dictionary’ has been completed.[1] It contains around 1 700 entries on various institutions (academies, research institutes, universities, experimental stations, laboratories, museums, learned societies, periodicals etc.), which have been concerned with biological and biomedical research, i.e. with life sciences. Also it contains entries on prominent scientists who made substantial contributions to the advancement of biology, to the exploration, conservation and protection of biological resources in Russia, and to the development of agriculture. Some of these entries were already published earlier in “*Biology at St. Petersburg, 1703-2010*” (2007) and “*Medicine at St. Petersburg, 1703-2008*” (2008).[2] The interaction of applied and fundamental research in the fields of agriculture and medicine in Russia, and in St. Petersburg in particular, has been recently examined in a number of publications.[3] These studies have contributed to a better understanding of the complexity and contradictions in the establishment and development of biological, medical and agricultural sciences in St. Petersburg; they have enabled us to correct many misconceived ideas about the history of institutionalization of these disciplines in Russia.

In this paper I will try to argue: 1) From the early days of biological research in Russia applied and fundamental problems were closely interrelated; 2) The institutionalization of biomedical and agricultural sciences in Russia began in the middle of the 18th century and was particularly intensive in the late 19th - early 20th centuries; 3) The establishment of the All-Union Academy for Agricultural Sciences (VASKhNIL) was not the beginning but the final phase in a long process of institutionalization of applied biological research; 4) Mass repressions of 1930s-1940s and the endorsement given to Lysenkoism by the Soviet government in the 1950s-1960s exercised a profound impact on biological, agricultural and medical research in Russia.

Original unity of fundamental and applied knowledge

The establishment of the Academy of Sciences at St. Petersburg in 1724 became an important element in the radical transformation of the country carried out by Peter the Great in the early 18th century. The reforms were necessitated by economic demands, the need to promote industrial development and popular education, modernize agriculture, and strengthen the Russian state, its international position and its potential for defence. The importation of science to Russia became an important part of the country’s modernization, which was initiated from above.[4] It is not by chance that it was arts and not sciences, which were mentioned first in the Peter I’s Decree of February 28, 1724, on the establishment of the Academy of Arts and Sciences (*Akademiia, ili sotsietet khudozhestv i nauk*).[5] From its early days science in Russia was seen as the ruler’s business, therefore it became a national tradition to give priority to applied knowledge. A disposition for collective projects was also a part of the same tradition; this tendency particularly manifested itself in expeditions organized and carried out by the Academy of Sciences in the 18th century.

Vast territories of the Russian empire could be explored only by expeditions; the scale of these voyages was unprecedented. For about two centuries scholars employed by the Academy of Sciences were engaged in the exploration of Russia’s natural resources and biodiversity. These expeditions encompassed vast territories of the Russian empire, from the Baltic Sea to the Pacific, from the Polar areas to the southern frontiers.[6] They helped to establish a strong connection between fundamental science and the needs of the Russian state. The early works produced by Russian naturalists Johann Gmelin, Georg Steller, Stepan Krasheninnikov, Peter Pallas, Ivan Lepekhin already provided information, which was extremely valuable not only for the advancement of science but also for pragmatic reasons: they described various species

[1] Kolchinsky, Eduard I., Fedotova Anastasiia A., eds. *Biologiia v Sankt-Peterburge, 1703-2008: Entsiklopedicheskii slovar’*. St. Petersburg: Nestor-Istoriia, 2011, 568 p.

[2] Kolchinsky, Eduard I., ed., *Nauchnyi Sankt-Peterburg. Predvaritel’nye materialy k vypusku II. Biologiia v Sankt-Peterburge, 1703-2007. St. Petersburg: Nestor-Istoriia, 2007*, 198 p.; Golikov, Yurii P., ed. *Nauchnyi Sankt-Peterburg. Predvaritel’nye materialy k vypusku III. Meditsina v Sankt-Peterburge, 1703-2007*. St. Petersburg: Nestor-Istoriia, 2008, 288 p.

[3] Grekova, Tat’iana I., Golikov, Yurii P. *Meditsinskii Peterburg*. St. Petersburg: Folio-press, 2001. 415 p; Elina, Ol’ga Yu. *Ot tsarskikh sadov do sovetskich polei. Istoriia sel’skokhoziastvennykh opytnykh uchrezhdenii XVIII -20-e gody XX veka*. Moscow: IIET RAN, 2008, vol. 1. 479 p; vol. 2, 487 p.

[4] Kopelevich, Yudif’. *Osnovanie Peterburgskoi Akademii nauk*. Leningrad: Nauka, 1977, pp. 32-38.

[5] Osipov, Yurii S., ed. *Ustavy Rossiiskoi Akademii nauk. 1724-2009*. Moscow: Nauka, p.47.

[6] Kolchinsky, Eduard I. “The Role of Eighteenth Century Russian Expeditions in the Development of Natural History”. In *Proceedings of the California Academy of Sciences*. 2004, vol. 55, suppl. II, pp. 106-116.

of animals and plants as biological resources, and considered their potential for economic exploitation, such as fishing and hunting.[7]

Scientists, who worked for the Academy of Sciences at the Kunstkammer and the botanical gardens on Aptekarskii and Vasil'evskii Islands in St. Petersburg, carried out research on the introduction of new species of plants, which were transported from afar by exploratory missions and could be used in medicine or in various industries. They experimented with species hybridization, studied anatomy and embryology trying to find out the causes of deformity and abnormal development.[8] Later, as a rule, the members of the Academy were called to serve on various advisory boards of the state administration; they consulted the government on issues concerning economic affairs, carried out governmental assignments, took part in the training of experts on the country's biological resources, flora and fauna, agriculture and forestry, soil science, hunting and fishery, medicine, etc.[9]

Side by side with the Academy of Science emerged other centres of biological research. "Hospital schools" trained not only physicians but prominent anatomists, physiologists and microbiologists (Daniil S. Samoilovich, Martyn M. Terekhovskii, Alexander M. Shumlianskii and others).[10] The "hospital schools" formed the foundations for the Main Medical School (1786), which was transformed into the Medical-Surgical Academy in 1789. The school provided its students with sound general knowledge of biology. Many famous biologists, such as the anatomists Piotr A. Zagorskii and Vladimir N. Tonkov; zoologists Johann F. von Brandt, Nikolai A. Kholodkovskii; physiologists Ivan M. Sechenov, Ivan P. Pavlov, Vladimir M. Bekhterev; biochemist Alexander Ya. Danilevskii; palaeontologist Karl E. von Eichwald; worked for this school.[11] In the 19th century a number of new schools and research centres were established: the Clinical Institute (1885), the Institute of Experimental Medicine (1890), the Piotr Lesgaft Biological Laboratory (1893), the Women's Medical Institute (1897), the Veterinary and Bacteriological Laboratory (1898), the Psycho-Neurological Institute (1907), and a few others. Professors who worked at these schools and research centres tried to maintain the unity of fundamental and applied knowledge. A number of governmental agencies were directly concerned with biomedical research and were instrumental for its advancement in Russia: the Medical Chancellery and the Medical College in the 18th century, the Medical Department and Veterinary Administration at the Ministry of Interior, the Medical Department at the War Ministry in the 19th century. These bodies established their own academic councils and committees, enlisting many prominent Russian physicians and biologists as their members (James Willie, Karl E. von Baer, Viktor V. Pashutin, Evgenii V. Pelikan, Nikolai I. Pirogov, Sergei P. Botkin, Nikolai V. Sklifosovskii and others).

The institutionalization of research in forestry and agriculture

The Free Economic Society was founded in 1766; its aim was defined as the advancement of applied research (agronomy, forestry and cattle-breeding, among other areas) and its popularization.[12] The 1802 reform of the state administration entrusted the advancement of science and education to several governmental agencies. These ministries and departments established their own academic committees and academic councils responsible for education, research and administration in respective fields. Scholars who served on these committees and councils, particularly the members of the Academy of Sciences, the faculty of the Medical-Surgical Academy and the Imperial St. Petersburg University, which was founded in 1819, were engaged in reviewing various research proposals and projects; they produced reports, instructions and guidelines, served as governmental experts. For example, the Academic Committee for Forestry at the Ministry of Interior was responsible for forest conservation, improvement and the advancement of scientific forestry. In 1811 several schools of forestry were merged together laying the foundations of the Forestry Institute at St. Petersburg.[13]

With the establishment of the Ministry of State Domains (MSD) chaired by Piotr D. Kiselev, its Academic Committee emerged as the leading centre of applied biological research in forestry, agriculture, agriculture-related industries, fisheries

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[7] Kolchinsky, Eduard I, Smagina, Galina I. "Zur Rolle der deutscher Wissenschaftler bei der Entwicklung der Biologie in Russland" In: *Europa in der Frühen Neuzeit. Fortschrift für Günter Mühlpfordt*, Bd 3. Aufbrach zur Moderne. Weimar, Köln,Wien: Böhlau Verlag, 1997, S. 293-312.

[8] Kolchinsky, Eduard I., Sytin, Andrei K., Smagina, Galina I. *Estestvennaia istoria v Rossii (Ocherki razvitiia estestvoznaniia v Rossii v XVIII veke)*. St. Petersburg: Nestor-Istoriia, 2004. 241 p.

[9] Pavlova, Galina E. *Organizatsiia nauki v Rossii pervoi poloviny XIX v*. Moscow: Nauka, 1990, 223 p.; Khartanovich Margarita F. "Akademiia nauk v sisteme gosudarstvennukh i obshchestvennykh nauchnykh uchrezhdenii v pervoi polovine XIX veka". In *Akademicheskaia nauka v Sankt-Peterburge v XVIII-XX vekakh*. St. Petersburg: Nauka, 2003, p.191-214.

[10] Samojlov, Vladimir O. *Istoriia rossijskoi meditsiny*. Moscow: Epidavr, 1997, p. 28-29, 32-37, 40-47.

[11] Belevitin, Aleksandr B. *Professora Voenno-meditsinskoi (Mediko-khirurgicheskoi) Akademii*. St. Petersburg: VMedA, 2008, 616 p.

[12] Kuliebiako-Koretskii, Nikolai G. *Kratkii istoricheskii ocherk imperatorskogo Vol'nogo ekonomicheskogo obshchestva so vremeni ego osnovaniia*. St. Petersburg: Tipografiia V. Demakova, 1897, 18 p.

[13] Red'ko, G.I., Frolov, M.I, Alekseev A.S., Babikov, B.V., et al. *Sankt-Peterburgskaia gosudarstvennaia lesotekhnicheskaia akademiia. Stranitsy istorii: 1803-2003.* St. Petersburg: SPbGLTA, 2003, pp.26-40.

and hunting in Russia.[14] The MSD Academic Council supervised the introduction of new methods of land cultivation on the crown lands; it promoted scientific agriculture and rational exploitation of natural resources, disseminated scientific methods of fighting pests, etc. Many prominent agronomists, cattle-breeders, ichthyologists, soil scientists, entomologists, geologists, climatologists, and experts on game management served on the Council and its various commissions. In 1843 the Forest Department was transferred to the Ministry of the State Domains, and in 1851 the Special Commission for the Study of Fisheries at the Chudskoe Lake and the Baltic Sea was established. Later it organized the expedition of Karl Ernst von Baer and Nikolai Ya. Danilevskii, which examined the state of fisheries of the Ural and Volga, as well as fisheries and seal hunting on the White, Black, Azov and Caspian Seas and the Arctic Ocean. The Council produced four editions of the "*Economic and Statistical Atlas of European Russia*"; these were the earliest maps in Russia, which showed the distribution of soil types, climatic zones, the location of forests, and the state of agriculture in various provinces of the empire. Guidelines for the making of these maps were written by the climatologist Alexander I. Voeikov, soil scientist Vasilii V. Dokuchaev, geologist Alexander P. Karpinskii and other prominent scientists. The entomologist Fiodor P. Koeppen worked on the methods of fighting off pest insects, and the specialist in plant geography Sergei I. Korzhinskii experimented with the cultivation of various grape varieties. The Ministry supervised its own schools –among them the Petrovskaia Agricultural Academy in Moscow and the Forestry Academy in St. Petersburg–, as well as experimental and model farms and estates. In 1892 the Forestry Department organized a Special Expedition led by Vasilii V. Dokuchaev; its objective was to find out the causes and to explore the consequences of the catastrophic draught of 1891, and to propose methods of land improvement. The expedition party included soil scientists, geologists and plant geographers (Konstantin D. Glinka, Vladimir I. Vernadskii, Georgii N. Vysotskii, Piotr A. Zemiatchenskii, Gavriil I. Tanfil'ev and others).

In 1894 the Ministry of State Domains was reorganized into the Ministry of Agriculture and State Domains (MASD) chaired by Alexei S. Ermolov.[15] Its Academic Committee was transformed into an important division; it identified priority research areas in agriculture and exploitation of biological and water resources; it created and reformed research institutions and schools, established agricultural laboratories, experimental fields, provided guidelines and instructions. In 1898-1904 the Committee was chaired by Ivan A. Stebut. The Agricultural Bacteriological Laboratory was subordinated to the Committee. There were plans to establish the Central Institute of Agronomy, which would supervise all experimental fields, farms and stations. The institute would encompass the schools of land cultivation, soil science, chemistry, botany, zoology, agricultural machinery, meteorology, as well as a chemical laboratory, botany and zoology offices, etc. However these plans failed to materialize. Instead, its research and administrative functions were performed by a number of smaller research centres subordinated to the Academic Committee –the Bureau for Entomology (established in 1894, chaired by Iosif A. Porchinskii), the Bureau for Applied Botany (1894, initially chaired by Alexander F. Batalin, and later by Robert E. Regel'), the Consulting Bureau for Zootechnology (1894), the Bureau for Soil Science (1895, chaired by Nikolai M. Sibirtsev; in 1900 it was merged with the Bureau for Agronomy, which had been established in 1897 by Piotr S. Kossovich, changing its name to the Bureau for Agriculture and Soil Science), the Bureau for Meteorology (1897, chaired by Piotr I. Brounov), the Bureau for Industrial Zoology and Fisheries (1899, chaired by Oskar A. von Grimm). In 1901 the Academic Committee created its Academic Commission responsible for the establishment of research institutions in the field of agriculture, their course design, and the supervision of experimental research in agriculture and seed farming. The Ministry of Agriculture and State Domains also encompassed the Mining Department and the Mining Academic Committee, which was concerned with historical geology and palaeontology; it carried out research on fossil florae and faunae, stratigraphy, and was engaged in the exploration of industrial mineral deposits.

Social dislocations and the acceleration of institutionalization

In the period of dramatic social and political dislocations, which coincided with the first two decades of the 20th century, the Academic Committee chaired by Academician Boris B. Golitsyn began to play an even more important role.[16] In 1907 Efimii F. Liskun transformed the Bureau for Zootechnology into the first research institution for cattle-breeding. In the same year the Bureau for Mycology and Phytopathology, and the Bureau for Agricultural Machinery were established, while the Bureau for Particular Problems in Plant Cultivation was created in 1911. The Academic Committee also had its own Bureau for Education, which supervised agricultural schools, and the Commission for Agricultural Experiments. Many leading scientists, such as Ivan P. Borodin, E.F. Liskun, Nikolai I. Kuznetsov, Evgenii V. Wulf, R. E. Regel, Nikolaj A. Monteverde, K. D. Glinka, Vladimir L. Komarov, Dmitrii N. Prianishnikov, I.A. Stebut, G.I. Tanfil'ev among others, served at these bureaus, botanical gardens and experimental stations supervised by the Ministry of Agriculture.

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[14] *Istoricheskoe obozrenie 50-letnei deiatel'nosti Ministerstva gosudarstvennykh imushchestv: 1837-1887. St. Petersburg, 1888, Ch. 1-5; Obzor deiatel'nosti Ministerstva gosudarstvennykh imushchestv (nyne Ministerstvo zemledeliia i gosudarstvennykh imushchestv v tsarstvovanie Imperatora Aleksandra III: 1881-1894.* St. Petersburg, 1901; Gins, G.K., Shafranov, P.A. *Sel'skokhoziaistvennoe vedomstvo za 75 let ego deiatel'nosti (1977-1912 gg.).* Petrograd, 1914.

[15] *Obzor deiatel'nosti Ministerstva zemledeliia i gosudarstvennykh imushchestv*. St. Petersburg, 1894-1905.

[16] *Obzor deiatel'nosti Glavnogo upravleniia zemleustroistva i zemledeliia 1905–1914 g.g*. St. Petersburg /Petrograd, 1907–1915.

After the February 1917 Revolution the Academic Committee for Agriculture was created at the Ministry of Agriculture; it was chaired by Vladimir I. Vernadskii, an ardent advocate of scientism and the chairman of the Commission for the Study of Russia's Productive Forces (KEPS).[17] Former bureaus were renamed to departments: 1) for agriculture and soil science; 2) for machinery research; 3) for meteorology; 4) for bacteriology; 5) for applied botany; 6) for specific problems in plant cultivation; 7) for gardening (its subsection for garden design was chaired by the future President of the Academy of Sciences of the USSR V. L. Komarov); 8) for mycology and phytopathology; 9) for zootechnology; 10) for fish-farming, fisheries and game-keeping; 11) for applied zoology and entomology, 12) for the organization of experimental research; 13) for forestry; 15) for bee-keeping and silkworm breeding; 16) bureau for agricultural education.[18] Many of these departments were at the same time an integral part of the KEPS: it strengthened the links between fundamental and applied research.

In 1922 the Academic Committee for Agriculture was transformed into the State Institute of Experimental Agronomy, headed by Nikolai I. Vavilov.[19] The Institute had units for soil science, applied botany and selection, entomology, mycology and phytopathology, zootechnology, applied ichthyology and scientific game and fisheries management, machinery research, forestry, agricultural microbiology, and a library. Soon Vavilov founded the All-Union Institute of Applied Botany and New Cultivated Plants and became its director. Later both institutes provided the foundations, on which the All-Union Academy for Agricultural Sciences (VASKhNIL) was created in 1929, with Nikolai I. Vavilov serving as its first President[20]. From its early days many members of the VASKhNIL were at the same time full members the Academy of Sciences of the USSR, among them were the leaders of fundamental biological disciplines: the geneticists N.I. Vavilov, Nikolai K. Kol'tsov, Grigorii A. Levitskii, Alexander S. Serebrovskii, the ecologist Boris A. Keller, the soil scientists Vasilii R. Williams, Nikolai M. Tulaikov, the physiologists Dmitrii N. Prianishnikov and Andrei A. Rikhter and others. They always advocated the unity of fundamental and applied research.

Soon Vavilov was compelled to engage in the struggle against Trofim D. Lysenko. Hundreds of selectionists, geneticists, agronomists, plant cultivators, cattle-breeders, soil scientists, foresters and other highly qualified specialists, who had begun their career in the late 19th - early decades of the 20th century, were subjected to political repressions and persecuted. Vavilov and many of his colleagues died in Stalinist prisons. Their extermination in the years of the "Great Terror" ensured the victory of Lysenkoism in the USSR, where it enjoyed absolute dominance in the 1930s-1970s. Many of its advocates are still influential in the applied branches of biology: their leverage accounts for the production of books denigrating Nikolai I. Vavilov and other victims of the Stalinist terror, while glorifying Lysenko and his 'achievements'.[21] These books are published in a massive number of copies.

Conclusions

The institutionalization of applied biological research was under way from the very moment when science was imported to Russia. Many scientists and science administrators took part in this process. The system of research institutions, which emerged in the late 19th - early 20th centuries within the administrative structure of the Russian government, became the basis for the making of institutional infrastructure for agricultural research in the USSR. The VASKhNIL was established upon the initiative of scholars, who after the 1917 Revolution used the Bolsheviks to promote their own pre-Revolutionary designs for a broad network of research institutes in Russia.

The structure of the pre-revolutionary research institutions survived the regime change and became the matrix for the new institutes –the VIR (the All-Russia Institute for Plant Cultivation), the VIZR (the All-Russia Institute for Plant Protection), the Institute of Forest Management, the GosNIORKh (the State Research Institute for Protection of Lake and River Fisheries)–, or they became a part of these institutions as laboratories, bureaus, departments, experimental stations, etc. Many of these institutions considered fundamental research as the basis for solving applied problems.

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[17] Mochalov, Inar I. *Vladimir Ivanovich Vernadskii*. Moscow: Nauka, 1982, p. 217.

[18] *Nauka v Rossii. Spravochnyi ezhegodnik*. Petrograd, 1920, vyp. 1.

[19] Esakov, Vadim D. *Nikolai Ivanovich Vavilov*. Moscow: Nauka, 2008, pp.-142-141.

[20] *Ibid*, pp. 155-165.

[21] Mukhin, Yurii. *Prodazhnaia devka-genetika. Poznanie mira ili kormushka?* Moscow: Izdatel' Bystrov, 2006, 416 p.; Pyzhenkov, Vladimir I. *Nikolai Ivanovich Vavilov: botanik, akademik, grazhdanin mira*. Moscow: Samoobrazovanie, 2009, 136 p.; Ovchinnikov, N.V., Konenkov, P.A., Chichkin, A. *Trofim Denisovich Lysenko – sovetskii agronom, selektsioner, biolog.* Moscow: Samoobrazovanie, 2009, 192 p.

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THE ACADEMY OF SCIENCES EXPEDITIONS OF THE FIRST HALF OF THE 19TH CENTURY AND THE DEVELOPMENT OF PRACTICAL BIOLOGY IN THE RUSSIAN EMPIRE

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Abstract

The first half of the19th century is for the whole world the period of formation of the capitalist relations. At this time Russian science has reached a new stage in its development. Industrial development also puts other purposes and problems before science. The 18th century was a time when the enormous territories of the Russian empire had practically not been studied yet, nor was any finding considered of interest. The problems of a more purposeful approach to research had been put before scientists. Expeditions were an integral part of the Academy activity for promoting its further development and prosperity. They helped to create a data-base for the future development of science.

First of all, expeditions gave wide opportunities for carrying out various researches in the fields of geography, biology, zoology, ethnography, etc. Secondly, expeditions allowed for the improvement of scientific methods. Along the travel, scientists developed their own techniques and methodologies for research, which, after its completion, could subsequently enter into a scientific arsenal.

From the total number of analyzed expeditions (41 expeditions made by the Academy of Sciences, either separately or together with the ministries), 15 led to appreciable breaks in science – of which 7 especially in biology.

Since the first years of existence of the St.-Petersburg Academy of Sciences (1724), expeditions were an integral part of its activity. Through a studying prism of expeditions, we can to retrace the history of development of the outlying districts of the empire, the change in cultural and public life in the country, the growth of consciousness by national minorities.

In the first half of the 19th century across the Russian empire 28 expeditions took place. Outside of empire there were 15 expeditions. From all of them, only 16 were biological expeditions. But almost all expeditions during that time were complex. Scientists going in ethnographic or geological expeditions received numerous instructions concerning biology. According to these instructions, those scientists collected herbariums and made biological observations. Not only have expeditions enriched the academic science with up-to-date materials, but also were given the chance to receive trustworthy information about the nature and population of the Russian empire. Scientific fundamental and applied research methods and a technique for collection gathering were developed and improved during the expeditions. Many of those methods have reached up to now. Expeditions also promoted not only botanizing, but also the development of sciences as a whole, and its separate areas.

From all the variety of expeditions promoting the development of practical biology in the first half of the 19^{th} century, we have chosen the most representative ones, such as Baer's expedition to Nowaja Zemlya in 1837, Middendorf's expedition across Eastern Siberia in 1842-1845 and Shrenk's expedition on the Russian Far East in 1853-1856.

K. M. Baer's expedition to Nowaja Zemlja in 1837

Baer's expedition to Nowaja Zemlya in 1837 was one of the basic stages in studying the Russian North. It begun in May 1837, and its main aims were:

1. Botanical, zoological and geological investigation in the Nowaja Zemlya archipelago.
2. To collect botanical and zoological samples.
3. To make more precise geography maps.

Karl Baer was in expedition with his colleagues:

1. Student of the University of Derpt A. A. Leman.
2. Laboratory assistant of the Zoological museum E. Filippov.
3. Painter H.R. Reder.
4. Attendant Dronov.

In 6 of July 1837, K. M. Baer arrived at Archangelsk. On 19 July the explorers had reached the shores of Nowaja Zemlya. Until 21 August the scientists worked on Nowaja Zemlya. In October K.M. Baer arrived with his companions at Saint-Petersburg.

K. M. Baer's complex research allowed putting a base for Nowaja Zemlya's climatology. Nowaja Zemlya had been investigated topographical, geological and zoological relations. This expedition gave the basic file of knowledge of this region that we have now. Baer's expedition to Nowaja Zemlya represents the first attempt of approach to a modern complex ecologic-biological method for studying territories. K. M. Baer collected 135 plants of the total amount of species, 197, which are growing in Nowaja Zemlya.

A. F. Middendorf's expedition to Eastern Siberia in 1842-1845

Middendorf's expedition in 1842-1845 had a huge value for research of Eastern Siberia. The main aims of the expedition were:

1. To find out the deposit conditions for permafrost.
2. Botanical, zoological, physical and geographical investigations in the region between Turuchansk and river Khatanga.
3. To study the quality and quantity of organic life on the Far North, far from the sea coast.

Companions:

1. Topographer V.V. Vaganov.
2. Forest guard T. Brandt.
3. Laboratory assistant M. Furman.

In January 1843 A. F. Middendorf arrived from Krasnoyarsk in Turuhansk. On 25 February 1843 the researcher arrived at Turuhansk. On 12 August 1843 A. F. Middendorf reached the Taymyr peninsula. In a year on August (1844), the explorer swam in the Okhotsk sea, and only on 20 March did he arrive at Saint-Petersburg.

A. F. Middendorf was the first researcher of such a scientific phenomenon as permafrost. He was the first to collect numerous data about nature and environment of theTaymyr peninsula and the Priamursky region. He opened the Byrranga and Bureinsky mountains ridges. In reports of Middendorf's expedition, the complex characteristics of the nature of the surveyed territory are given. These reports considered detailed data on climate, vegetation and fauna of Siberia.

A. F. Middendorf offered the first scientifically well-founded scheme of ecologic-faunistic division into the districts north and east of Siberia, and in details considered a problem for modern distribution of animals. Middendorf's investigation was very important for practical biology, for example Middendorf defined the borders of various botanical-geographical zones of vegetation.

L. I. Shrenk' expedition to the Russian Far East in 1853-1855.

The expedition of L. I. Shrenk to the Russian Far East in 1853-1856 laid the basis for such a synthetic science as zoogeography. The main aims of the expedition of 1853-1857 were:

1. Botanical, zoological and geographical investigation.
2. To collect the botanical and zoological samples.
3. Investigation of meteorological phenomenon, sea ebbs and high tides.
4. Astronomical definition of a geographical position of the main points of the visited countries.

Companions:

1. Painter V. Polivanov.
2. Laboratory assistant M. Shil.

In 1853 L. I. Shrenk leaved Saint-Petersburg. In the beginning of August 1854 L. I. Shrenk reached the Nikolayev post (the coast of Okhotsk sea) with his companions, and there he founded a storehouse for the investigations. During the long autumn and winter time, they investigated the surroundings of the post and the coast of Sakhalin island.

In the spring of 1855 L. I. Shrenk, together with the other members of the expedition, went by boat upwards on the Amur. In January 1857 L. I. Shrenk arrived to Saint-Petersburg.

L. I. Shrenk, as well as K. M. Baer, suggested considering nature not as an assortment of separate species, but as a complex in which an important role was played not only by animals and plants, but also by geographical and climatic conditions. Accordingly, the studying of separate species was not possible without studying the conditions of their formation, origin and existence.

Also for the first time the researcher revealed and defined such signs of species as their morphological structure and the presence in a definite area. On the basis of such approach, the academician created the zoogeography of the Priamurja region and conducted the biooceanographic research of the Russian seas (especially the Japanese Sea). Shrenk was the first scientist to apply the method of local flora investigation. It was very important for the development of practical biology.

Conclusion

Science differentiation has now practically reached its apogee, but initial division of sciences had begun in the first half of the 19th century. During long-term expeditions, the methods which have subsequently become the basic ways of materials collecting, appeared and fulfilled. Collected data, after their processing and analysis, led to the occurrence of new branches of science, including biology. Thus, expeditions were the areas on which new methods were tested. Russian scientists, in extreme conditions, using imperfect tools, conducted unique researches promoting progress and development of domestic science.

References

MIDDENDORF A.F. (1860-1877) *Expedition to the north and east of Siberia*. Parts 1 and 2. Saint-Petersburg.

NASAROV A.G., TSUTSKIN E.V. (2008) *Karl Maksimovich Baer*. Moscow.

PASETSKII V.M. (1984) *Russian discoveries and investigations in Arctic. First half of the 19th century*. Leningrad.

Russian expeditions for the investigation Siberia and islands. 1734-1862. (1877) Kronshtadt.

SMIRNOV V.G. (2006) *Investigations in the world ocean by Russian war sailors and scientists. 1826-1895*. Saint-Petersburg.

SVENSKE K.F. (1866) *Nowaja Zemlja and its geography, history and industry*. Saint-Petersburg.

SHIRINA D.A. (1983) *The Annals of the Expeditions of the Academy of Sciences to the northeast in pre-revolutionary period*. Novosibirsk.

The Archive of the Russian Academy of Science Saint-Petersburg Branch. P. 129. L. 1. F. 350.

The Archive of the Russian Academy of Science Saint-Petersburg Branch. P. 2. L. 1 – 1838. F. 6.

The State Russian History Archive P. 733. L. 13. F. 273. 1853-1857. Part 1.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 612.

ZEMSTVOS AND NATURALISTS: OBJECTIVES OF APPLIED RESEARCH, METHODS OF FUNDAMENTAL SCIENCE

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Abstract

In their recent works, historians of Russian science emphasise that many projects which were carried out in the field of natural history in the European Russia in the late 19th century, with funding provided by local elected authorities (the zemstvos), could be defined as applied research only in terms of their objectives, but not their methods and results. For example, Vasilii V. Dokuchaev's expeditions to Nizhnii Novgorod and Poltava provinces were very important for genetic soil science; however, they failed to achieve their explicit practical goal of cadastral surveying. In my paper I attempt to analyse some reasons accounting for this tendency.
In the 19th century many Russian naturalists and agronomists sought to prove that the study of local nature was essential for the advancement of agriculture. In my view, their views were a reaction not only to a primitive state of agricultural production in the country but also to the failures in a 'blind' transfer of Western European practices. European education in agrology, as well as European literature in that field proved to be insufficient for transforming rural economy into successful agricultural enterprise. At the same time the tsarist government still refused to acknowledge that agriculture required specialised knowledge. When in the last two decades of the 19th century scientists began to study local natural environments, they were very excited about these projects, as they were convinced that the study of natural history was the first step towards rationalising agricultural production in Russia, while they never questioned their own research design: they did not consider the need to change their methods and research objectives. Many of these projects pursued applied objectives but employed methods borrowed from fundamental science. Therefore their results contributed primarily to the advancement of fundamental science. Thus in the late 19th century we observe a conspicuous gap between primitive methods of agricultural production and advanced state of research that was hardly relevant for everyday agricultural management.

It has been a common place in the history of Russian science to claim that many studies produced by 'outstanding Russian naturalists' were directly linked to the applied problems. Recently some historians have emphasized almost the opposite trend: they claim that many naturalists' studies in the European part of Russia in the late 19th century that were funded by local authorities (the *zemstvos*) can be called 'applied' only if we consider their problems but not their methods and their results. The expedition of Vasily V. Dokuchaev and his team to the Nizhegorodskaya (1882-1886) and Poltavskaya (1888-1894) governments[1] are examples of such phenomena.

In the introduction to the 14-volume "Materials" of Nizhegorodskaya expedition Vasily Dokuchaev[2] declared that one of the reasons why he had accepted the proposal made by the Nizhegorodskoye zemstvo was the urgency of the problem: the aim of the project was to work out a system of land assessment that would ensure fair taxation. However the project output turned out to be a very important contribution to soil science and plant geography –it was not particularly helpful in terms of land assessment.

Materials of Nizhegorodskaya and Poltavskaya expeditions and similar works[3] contained standard descriptions of surveyed territories, as they were conducted by naturalists. Plant geographers focused mainly on natural plant communities, soil scientists concentrated on uncultivated soils. Such objects were interesting for naturalists but not for farmers. The research methods of Dokuchaev team were methods of pure science, and Vasily Dokuchaev regularly stressed this point. Dokuchaev himself, his team and followers were more interested in pure knowledge: how would their principles of classification soils or plant communities work? Were the principles of ‘genetic’ classification of soils, which they had developed in Nizhegorodskaya expedition, applicable to other regions and to other soil types? Dokuchaev and his followers were much less concerned with immediate practical value of their research for local authorities or farmers.[4]

I am not claiming here that scientists consciously ignored practical questions they were commissioned to solve or had cynical intention to ‘satisfy their curiosity at the expense of the state’ (or in this case at the expense of the zemstvos or local landowners, since Dokuchaev’s team also surveyed private estates[5]). I agree with Alexey Karimov[6] when he suggests that Russian naturalists merely assumed: when a thorough academic research was completed its results would immediately answer pragmatic needs of agricultural practice, in other words, their utility would be self-evident.

Throughout the 19th century, Russian naturalists, public figures and agriculturists tried to convince the government that the study of local nature was the first step to make in order to improve backward Russian agriculture. I believe that this attitude was a reaction not only to the backwardness of Russian rural economy, but also to unsuccessful transfer of the European agronomical technologies.

In the 19th century, agriculture as a field of knowledge was still not a scientific discipline but a set of ‘recipes’. In some European countries agricultural technology was relatively advanced but in most cases it was just a collection of good practices accumulated by generations of peasants and farmers by trial and error. Like any other recipes, these rules produced by agricultural practice prescribed what was to be done but usually did not explain why it had to be done in a particular way. Explanations appeared only in the second half of the 19th century with the development of plant physiology, organic chemistry, biochemistry, and other disciplines.

When Russian landowners tried to copy technologies developed in Western or Central Europe, the results were most often unsuccessful. The reasons for these failures were understandable: different environment conditions required the technologies to be modified in order to match different environment.[7] Consequently, in order to adapt European technologies to Russian environment it was necessary to study the environmental conditions of a particular province.

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[1] *Materialy k ocenke zemel’ Nizhegorodskoy gubernii. Yestestvennoistoricheskaya chast’*. St. Petersburg, 1884-1887. Issue 1-14; *Materialy k ocenke zemel’ Poltavskoy gubernii. Yestestvennoistoricheskaya chast’*. St. Petersburg, 1890-1894. Issue 1-16. Dokuchaev’s research group consisted of young geologists, mineralogists, plant geographers and some other researchers, mainly students and graduates of the St. Petersburg University. The expeditions themselves, chemical and physical laboratory tests, the Proceeding publications were sponsored by the Nizhegorodskoye and Poltavskoye Provicial Zemstvos, research travels of some participants (for example of the young botanists – A. Krasnov, V. Aggeyenko and E. Nedergefer) – by St. Petersburg Naturalist Society.

[2] Dokuchaev, V.V. Perface. In: *Materialy k ocenke zemel Nizhegorodskoy gubernii*. St. Petersburg, 1887. Issue 14. P. I-VI.

[3] There we can include not only the expeditions of V.V. Dokuchaev and of his direct disciples, but also some others researchers, for example the surveys produced by the Kazan naturalists A.Ya. Gordyagin and R.V. Rizopolozhensky sponsored by the Kazan Naturalists Society and by the Kazan Provincial Zemstvo in 1888-1890.

[4] Perhaps only one participant of Dokuchaev’s Nizhny Novgorod expedition –N.M. Sibirtsev– seriously collected information on the tillage methods, yields, frequency of crop failures and droughts etc. Later, Sibrtsev stayed to work in the Nizhny Novgorod as a head of the Natural History Museum of Nizhny Novgorod Provincial Zemstvo. Working with Statistical Department of the Zemstvo he discussed the minimum of necessary soil studies for the land cadastre.

[5] For example, Narishkin’s estate “Pady” in Saratov province, Shuvalov’s estate in Voronezh province etc.

[6] Karimov, A.E. *Dokuda topor i solha hodili: ocherki istorii zemrl'nogo i lesnogo kadastra v Rossii XVI – nachale XX veka*. Moskow: Nauka, 2007. 237 p.

[7] See for example: Beketov, A.N. “O yestestvoznanii kak predmete obzchego obrazovania”. In: *Trudi I s’yezda Russkih yestestvoispitateley*, St. Petersburg, 1868. P. 32-39.

Everyone would agree on that: all problems began later. The Russian government acknowledged that the level of agricultural technology in Russia was considerably lower than in Europe. But it had no long-term policy on this matter.[8] From time to time the government made convulsive attempts to improve the situation: it imported technology, together with their carriers (for example, with German colonists), established the chairs of agriculture at the Russian universities, etc. However if we consider the chairs of 'rural economics' at the Russian universities we have to admit that it was only in Dorpat where agriculture was more or less successfully taught before 1840s. In other universities attempts to introduce courses on agriculture failed. Thus, Karl A. Nöldechen (1771 (1772?)-1819), who was the first Professor of rural economics at the Kharkov University (1811-18), turned out to be so inept in this position[9] that after his dismissal the University Council lost any interest in the subject itself and the chair remained vacant for more than twenty years. From time to time some other professors gave lectures on 'rural economics'.[10] Before 1865, when the Petrovskaya Academy of Agriculture and Forestry (nowadays the Timiryazev Agricultural University in Moscow) was created, Russia had no institution of higher education for agriculture; the only exception was a small Agricultural Institute in Gori-Gorky (in present-day Belarus) founded in 1842. A catastrophic shortage of specialists, agronomic training and, especially, research institutions existed until the first decades of the 20th century.

Those few landowners and managers who were trying to improve their estates and farmsteads got no support. Even if by trial and error they discovered a relatively good 'recipe', there was no structure –state, public or commercial– that could help them to promote and improve their practices. The situation did not change until the late 19th century when first agricultural experimental stations appeared in the Russian Empire.

In the 19th century Russia, with its primitive state of agricultural knowledge, 'progressive' public figures, landowners and agronomists respected scientific knowledge very much and expected benefits, which it would bring to agriculture. Under benefits they understood not only increased incomes but other things as well. Russian 'educated public' believed that as soon as local nature of their provinces would be described by scientists, academic research would immediately provide them with exact guidelines, which would enable them to get higher and stable yields without depleting soil and damaging nature; in other words, academic research would furnish them with a recipe for "sustainable development", if we apply modern terminology.

I give a couple of examples to prove these arguments. Firstly, I quote an opinion expressed by Alexander Izmailsky. Alexander Ismailsky (1951-1914) was an agronomist, who graduated from the Petrovskaya Academy. Most of his life he served as a manager on large estates in the Russian steppe provinces. He conducted several series of experiments on measuring soil moisture in different environmental conditions. His experiments and publications proved to be a valuable and timely contribution to research produced by Dokuchaev and his team.[11]

In December 1892 the Moscow Agricultural Society discussed an idea of organizing 'public works for irrigation of Southern and Eastern Russia'.[12] In his letter to Dokuchaev Izmailsky discussed this project and expressed his doubts: "what could be more logical (considering our poverty): to spend money on irrigation and afforestation of tiny plots, or on a broad survey of all environmental conditions of the Russian steppes?"[13] Alexander Ismailsky obviously supported not these small-scale experiments with their practical local implications, but fundamental science.

Another episode. In 1897, the Statistical Committee of the Free Economic Society upon the request of the Russian government discussed the best ways of assessing land value. V.V. Dokuchaev advocated his own approach based on his experience of surveying Nizhny Novgorod and Poltava provinces. In his view, a cadastral survey should start with determining 'natural characteristics of arable land on the basis of its geological, chemical, botanical and zoological characteristics, as well as their links to climate'[14]. This part should be carried out by specialists in soil science without any participation of local population. Only at the second stage of a survey, when local economy is examined, local population should be consulted. In fact, Dokuchaev's program promoted the interests of fundamental science and the governmental

[8] See for example: Sunderland, W. *Taming the Wild Steppe. Colonization and Empire on the Russian steppe*. Ithaca & London: Cornell University Press, 2004. 236 p.

[9] His previous activity in Germany was quite successful: in Berlin and in Pommern he served on the board of management of royal estates, and published some original and translated articles on agriculture.

[10] *Physiko-matematichesky facul'tet Khar'kovskogo Universiteta za pervie 100 let ego suzchestvovania*. 1805-1905. Khar'kov, 1908.

[11] See for example: Izmailsky, A.A. *Vlazhnost' pochvi i gruntovaya voda v svyazi s rel'efom mestnosti i kul'turnim sostoyaniem pochvi*. Poltava, 1894. 234 p.

[12] *Stenograficheskiy otchet o soveschaniyah pri imperatorskom Moskovskom obschestve sel'skogo khozayastva s 18 po 22 dekabrya 1892 goda po obschestvennim rabotam po obvodneniyu yugo-vostochnoi chasti Rossii*. Moskva, 1893.

[13] 'Is perepiski s A.A. Izmail'skim'. In: V.V. Dokuchaev. *Sochinenia*. Vol 8. Moskow: Izdatelstvo AN SSSR, 1961. P. 292-293.

[14] 'Zasedanie statisticheskoi komissii sostoyaschei pri III otdelenii Vol'nogo ekonomicheskogo obschestva 1 noyabrya 1897 goda'. In: *Trudi Vol'nogo ekonomicheskogo obschestva*. 1898. Vol. 1. № 1. P. 4-7.

agencies, which accumulate the data on the whole empire collected on the basis of a uniform methodology, and not the interests of local authorities and landowners in the provinces.

Defending the advantages of scientific approach, as compared to the methods of statistical research, V.V. Dokuchaev was unwilling to accept its limitations and made gross mistakes when he discussed economic issues. Judging from his experience of studying soils in Nizhny Novgorod and Poltava provinces, V.V. Dokuchaev suggested that a cadastral survey had to focus on what he called 'natural productivity' of soils. But Dokuchaev's concept of 'natural productivity', which Dokuchaev extrapolated from physical and chemical characteristics of soil, was very poorly related to the actual fertility of soils. Dokuchaev team conducted no experiments to assess soil fertility. A comparison of actual yields produced on different soils did not confirm Docuchaev's assessment.[15]

Statisticians from the Free Economic Society criticized Dokuchaev's program as serving the interests of the central government rather than those of local zemstvos and landowners.[16] But Dokuchaev's team, probably, was convinced that their long-term interests (working out a uniform, universally applicable method for a comprehensive description of natural resources in different regions of the Russian Empire) were more important than the short-term zemstvo interests (working out guidelines for land value assessment and cadastral surveys). This interest in developing a single standard universally applicable method for a comprehensive description of natural resources became a permanent feature of Russian soil science and plant geography in the 20th century. Which one of several terms is the most appropriate? What is the best scale to use? Which method should become the standard one for describing a plant community in any region of the country? Of course, standardization of research methods was necessary to achieve some progress. However in the Soviet era this emphasis on uniform methodology did a bad service to Russian plant geography and soil science.

Conclusion

In the last quarter of the 19th century Russian naturalists began to work together with local authorities on their projects, which were aimed at the exploration of natural resources. Both parties evidently hoped that their research would equally benefit local agriculture and fundamental science. However when scientists began to work on these projects commissioned by local authorities, they focused on their own research agenda, which was of little or no relevance for practical problems. Scientists produced knowledge, which was not immediately applicable for the needs of domestic agriculture. The sphere of applied research, which mediates between these two extremes – fundamental science and local knowledge, had not yet been created. In the last quarter of the 19th century soil scientists and plant geographers only claimed that their research was of an immediate value for practical purposes. Real cooperation between scientists and local authorities began later – at the turn of 19th and 20th century when first agricultural experimental stations were created, and zemstvos established permanent salaried positions for experts in natural sciences. But even in the early decades of 20th century we could observe a huge gap between the achievements of fundamental science (soil science, plant geography and so on) and a primitive level of agricultural technology in the country.

[15] Lazarevskiy, F.I. [Kriticheskiy razbor glavneishih resultatov pochvenno-ocenochniy issledovaniy v Nizhegorodskoi gubernii]. In: *Trudi Vol'nogo ekonomicheskogo obschestva*. 1898. Vol. 1. Issue 1. P. 7-11.

[16] Richter, D.I. 'Zamechania na pochvenno-ocenochniy proekt professora V.V Dokuchaeva'. In: *Trudi Vol'nogo ekonomicheskogo obschestva*. 1898. Vol. 1. Issue 1. P. 42-59.

DARWIN IN URBAN CONTEXTS, 1859-1930

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 617.

DARWINISM, ART AND LITERATURE IN RIO DE JANEIRO AND SÃO PAULO IN THE EARLY 20TH CENTURY

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Abstract

Since the 1870's, Darwin's theory has been disseminated among intellectuals and scientists in Brazil. The capital city of Rio de Janeiro, just as other capitals in the world, was re-urbanized in the beginning of the 20th century inspired by the reform of Hausmann in Paris. Behind the process of modernizing the city was the initiative of sanitizing the Brazilian capital, what was undertaken under the coordination of the hygienist Dr. Oswaldo Cruz. Among intellectuals, Euclides da Cunha, a military who followed positivist orientation and was adept to Darwin's theory, based his main publication Os Sertões (The backlands) on the interpretation of Darwin's ideas. The book describes the struggle in the hinterland of the country between a group that supported the imperial government and the troops of the newly installed Republican army. The publication praises both the natural settings and the toughness of the hinterland man, contrasting him with the polite but weak urban men, more susceptible to diseases. Literature and urban architecture applied the scientific precepts of Darwinism in order to solve social, political and cultural issues of the city and the country.

Introduction

Our analysis focuses on the theory of evolution in the early 20th century, addressing the racial question central to natural and social sciences, which led to urban intellectual and social problems as manifested in the arts and popular literature.

With the end of slavery in the late 19th century, cities in Brazil were confronted with an exodus of former slaves from the hinterland. At the same time, waves of European immigrants were arriving in the country. It was at that moment that racial issues started to emerge as a fundamental question. In the early 20th century, the racial question as discussed in Brazil and many other countries was imbued with the theory of eugenics, which was seen as a science for the improvement of human heredity, leading to the popularization of eugenicist ideas (Seyferth, 2008: 148).

Our discussion compares Darwinist interpretations of the question of human race as expressed in science, art, and literature. At the beginning of the 20th century, the subject of race gained enormous prominence in Brazil, soon after Euclides da Cunha had published the novel *Os Sertões* (translated into English as *Rebellion in the Backlands*) in Rio de Janeiro in

1902, to great acclaim. Constantly recovered, Euclides da Cunha has become a mythical figure in Brazilian literature.[1] In the 1920s, the Modernist Art Movement emerged in São Paulo, with the mixing of races in Brazil as one of its main themes. This movement also adopted the work of Euclides da Cunha. The confrontation of such different intellectual and cultural productions constitutes the core of the discussions in this work.

A few studies on the racial question have drawn attention to the involvement of politics in the science of the topic, which characterized the first part of the 20th century. According to Chris Manias, race and national types divided the supporters of European Aryanism. The Germans proclaimed themselves a nation based on the idea of the white race, whereas the French rejected the idea that nation and race were linked and claimed diversity in their historical origin (Manias, 2009: 733-757). While in Europe the racial question was linked to differences between ethnic groups, in the United States, a country with a slave tradition, the problem was the discrimination against blacks by whites, against which Einstein was one of those who protested. Speaking at Lincoln University in 1946, he pronounced the famous sentence: “The separation of races is not a disease of coloured people but a disease of white people. I do not intend to be quiet about it”(Jerome, 2004: 628).

Although Brazil also had a tradition of slavery, the racial question there took the form of miscegenation and involved all sectors of society: intellectual, scientific and political. For human sciences in general, in the first decades of the 20th century, race meant eugenics; however, interpretations were not homogeneous. These issues appeared during the First Brazilian Congress of Eugenics, organized by the anthropologist Roquette Pinto, of the Museu Nacional in 1929. For Nancy Stepan, the existence of both a soft and a hard form of eugenics in Brazil within the neo-Lamarckian tradition and the opposition that developed between the neo-Lamarckian racialists and the Mendelian anti-racialists suggested that the inherent logic of science does not determine its social meanings and outcomes (Stepan, 1990: 145).

Race, man and environment in literature

The determinism of man and environment according to evolutionist ideas appeared in Euclides da Cunha’s book *Os Sertões*. The author emphasizes that the book was based on the theory of evolution and, in fact, he uses expressions such as “struggle for life”, “adaptation” and even a neologism in Portuguese: “*mutuar*” (to mutualize) –implying mutual exchanges, in this case between what he called physical (subject to genetic action) and geological (stable) agents– to define the backlands and their inhabitants, demonstrating his knowledge of Darwinian theory[2]. However, while emphasizing determinism in the environment, he mentions Buckle and does not hide his appreciation for that positivist intellectual who praised the powerful action of the environment on society.[3]

Os Sertões tells the saga of the struggle of a group of backlanders (inhabitants of the remote inland area of north-eastern Brazil) against the forces of the newly founded Republican government, which had replaced the Monarchy which had governed the country since its independence in 1822. The group was led by Antonio Conselheiro, a mythical figure whose image is still considered holy today. What is relevant about this work is not really the history of the fight between the group and the army, but, the image which Euclides da Cunha depicts of those backlanders, who gained strength in the “struggle for survival against an inhospitable environment, of dry land in the winter and fertile in the lush summers”, which he describes perfectly. For Euclides da Cunha, the backlanders are descendants of the white men who ventured into the hinterland (*bandeirantes*) and Indians, and he concludes that “the inferior race reacted positively on the superior one”, becoming a symbol of Brazilian nationality or, in the language of the time, “the race” that characterizes Brazil.

Euclides da Cunha’s description of the landscape is magnificent. The impotent land has a tormented appearance, scorched by “external agents”, with stupendous relief and a stunted flora resulting from the climate of torrential rains following long periods of relentless sun. The land is subject to forces which act silently during the only two seasons in the region, causing molecular imbalance. On the almost naked hills and in contorted dry river beds, the flora is a tangle of branches in a representation of the land’s martyrdom. In the alternation of days and nights, the land suffers from the extreme dryness of the air and faces sudden changes in temperature, adding the martyrdom of dilations and contractions, an alternation that becomes worse when the rain falls and closes the cycle of drought.

In an evolutionist view, Cunha states that the incipient region is still preparing for life because lichen still attacks the rock, fertilizing the earth, and a flora of rare resistance tenaciously struggles against the scourging climate.

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[1] See Candido, 2006; Santana, 1999; Martins, 2001; Abreu, 2006; Sousa and Galvão, 2007. In the 1920s, Euclides da Cunha was honored by the anthropologist Roquette Pinto, who named a room in the National Museum of Natural History in his honor. Lima (1999) draws attention to the new intellectual interest in the backlands in the early 20th century.

[2] See Souza, Vanderlei, 2010.

[3] According to Kaat Wils, Buckle, whose ideas were influential in Brazil, was a staunch Comtian positivist historian who believed that civilization was the result of the development of empirical knowledge. His positivist beliefs were seen as being closely related to Marx’s historical materialism (Wils, 1999).

He describes the drought as an intolerable intermittence of extremely hot days and freezing nights: "The naked earth having counterposed in permanent conflict the emitting and absorbing capacity of the materials that form it ... is seared by the sun and absorbs its rays, and multiplies, reflects and refract them in a dazzling reverberation, in which the leafless branches of the defeated flora collapse" (Cunha, 1927: 28).

The flora of the *caatinga* (a dry forest ecosystem of the semiarid region of northeast Brazil) smothers man and subdues him as it spreads over the land in spiny foliage, which explains the original account of the "struggle for life" in the backlands.

Fig. 1: Caatinga: a dry forest ecosystem of the semiarid region of northeast Brazil.

The inhabitant of the backlands is a *mestizo*, or a mix of the white men who set off to explore the interior –the *bandeirantes*– and ended up staying there, and the indigenous peoples, whom they had dominated and enslaved, but whose nature they also took advantage of. Thus the backlander's character evolved, blending the adventurous nature of the colonizer and the impulsiveness of the indigenous people, and influenced by the isolation, which forced him to react to his environment. The Indian was diluted in the backlander and this gave him intimacy with his environment, preventing him from degenerating. Reaffirming the theory of evolution, Cunha further asserts that in the hinterland the struggle for life was wild in nature, forcing people to face the horrors of the drought and the cruel battles with the arid land, compensated by the abundance that came with the return of the rainy season. "The backlander is first and foremost a strong man!" However, Cunha continues, "he is frozen in time, discarded by the general movement of human evolution. He depends on the land."

In his determinism, Euclides da Cunha does not attribute the destruction of the environment to the inferiority of the races, but he accepts polygenism, affirming that the American races are autochthonous, a common idea among anti-Darwinian anthropologists.

Euclides da Cunha's ideas were rediscovered in the 1920s by the modernist art movement in São Paulo, which reoriented the picturesque and the exotic elements of backlands literature that Euclides da Cunha did not encompass.

Anthropophagic art

The Anthropophagy Movement emerged in art and literature as an icon of Brazilian modernism breaking the chains of intellectual colonization, not through anti-Eurocentrism, but through the blending of European culture, which had shaped Brazilian intellectual life, with the cultural expressions of people from the interior of Brazil. The movement was an intensification of the well-known Brazilian modernist movement, which had burst onto the scene in the 1922 Modern Art Week (*Semana de Arte Moderna*), a week of art exhibitions and book readings and launches held in the city of São Paulo. That week marked the beginning of the *Pau-Brasil* (Brazil Wood) art movement, which was later radicalized by the Anthropophagy movement.

The contrast between the two movements is reflected in paintings by Tarsila do Amaral. In her Pau-Brasil phase, natural beauty was painted in a spontaneous, ingenuous, happy way, revealing the Brazilian landscape openly and directly, with no mystery, whereas in her Anthropophagic phase the simple, brutal, barbaric, illogical face of nature was reinforced

(Gotlib, 2003: 149), emphasizing the wild and the barbaric. The Anthropophagite Manifesto was marked by the dichotomies of town and country and of rationalism and magic (Amaral, 2006: 24).

In 1928, in São Paulo, the painter Tarsila do Amaral[4] gave her husband, Oswald de Andrade (who was a writer and one of the leaders of the literary modernist movement) a birthday gift of a painting depicting a strange figure, which they mutually agreed to name *Abaporu*. The word, taken from the Tupi language, meant "anthropophagous": *aba* –man; *poru* – one who eats human flesh. The painting shows a strange, enlarged figure, with an accentuated contrast between its enormous feet and hands and its minuscule head on top. The earthy colour of the figure contrasts with the blue sky and the blazing sun, a reminder of the baking heat of the backlands. Below, there is a lone green cactus, a characteristic plant of that region described by Euclides da Cunha.[5] Thus was created the Anthropophagy Movement.

Fig. 2: *Abaporu*.

The Anthropophagite Manifesto emphasizes the "barbaric" character of the Brazilian cannibalistic or anthropophagous "savage", capable of devouring or digesting the culture of the country's colonizers –not only the Portuguese, but also other Europeans, including all those that emigrated to Brazil, such as Germans and Italians, and also Africans and Asians (Andrade, 1995).

In the words of the literary critic Antonio Candido, the Modernist Movement in its heroic phase essentially released Brazilians from a series of historical, social and ethnic hang-ups, which were triumphantly brought to the surface of literary consciousness. Modernism adopted a philosophy which attributed a constructive, heroic meaning to the crucible of races and cultures located in a harsh environment. That was a view contrary to the colonizing attitude of the 19th century, which extolled the profusion of nature in Brazil and underestimated man, who was seen as barbaric and inferior. Modernism accentuated the rusticity, the dangers, the obstacles of tropical nature and included the *mulato* and the black man as subjects for study and inspiration. "Primitivism is now a source of beauty, and no longer an impediment to the elaboration of culture. This is true in literature, in painting, in music, in the sciences of man" (Candido, 2006: 127).

In the case of Tarsila do Amaral, it seems that Anthropophagism must already have been present in the 1922 movement, since in 1923 she painted *A Negra* (*The Black Woman*) with a refined Cubist technique and Legerian forms in a design which accentuated not only the harsh, illogical imprints with which slavery marked slaves –disproportionate breasts, which nursed the master's children, and enormous hands and feet representing the hard work in the fields– but also the

[4] Tarsila do Amaral lived in France in the early 1920s, where she studied under Fernand Léger and was introduced to the intellectual circle, developing a close relationship with the poet Oswald de Andrade.

[5] Transcending the botanical descriptions given by Euclides da Cunha, the cactus comes to be seen as a representation of backland Brazilianness. It was already being used in Brazilian garden design in the 1920s and the famous landscape gardener Burle Marx used cacti in his first projects in the 1930s (Martins, 2009: 152).

physical features of the black race –thick lips, reflecting a question which dominated the anthropological sciences at that time: the study of the races[6]. It is relevant to emphasize that those were the times of eugenics in the natural and social sciences.

In 1928, in the middle of the scientific discussion about eugenics, Tarsila do Amaral painted *Ovo* (*The Egg*), representing the life cycle, and the large serpent which, according to Amazonian legends, used to appear to demand the rescue of a damsel. The egg represented the genesis of a being that was subject to its environment, represented by the serpent surrounding it.

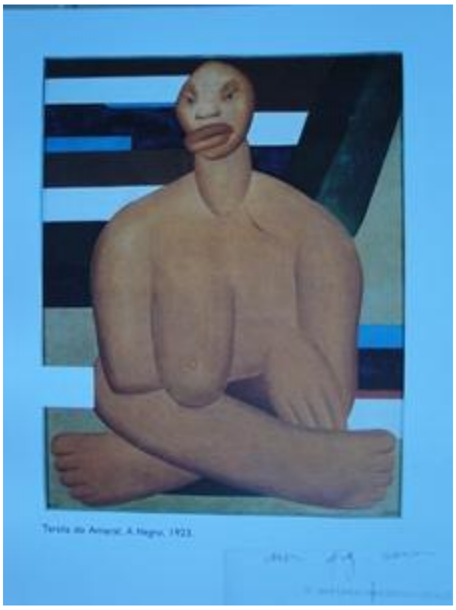

Fig. 3: *A Negra* (*The Black Woman*)

Fig. 4: *Ovo* (*The Egg*)

Soon after *Abaporu* Tarsila painted *Antropofagia*, a painting which crossed *A Negra* and *Abaporu*, "a primordial couple that originated Brazilian culture". The anthropophagy movement implicitly brought with it the issue of immigration, a subject that permeated both politics and science in that decade.

Eugenics and anthropology

In 1929, when the First Congress of Eugenics was held in Rio de Janeiro, the central subject was the immigration policy that the government was putting into practice. In the conference proceedings, Roquette Pinto stated that immigration should not be promiscuous. In his view, individuals should not be submitted to the influences of the environment to the point that their hereditary characteristics were altered; therefore, the selection of migrants had to be rigorous and had to take into account the collective attributes of different populations: their individual conditions, and the mental and moral qualities of each one, which would determine the value of the racial element, Any immigrants with a criminal background should be excluded and subsidized immigration was unacceptable, because poverty was an indication of mental and moral inferiority (Roquette Pinto, 1933:75). Racial differences were not mentioned.

Grounded in Darwinian theory, unlike some eugenicists, Roquette Pinto followed the American Jennings in concluding that heredity was nothing more than the presence of biochemical material. Each gene contains hundreds of chemical bodies which react among themselves, which makes it impossible to control them. Therefore, during development, heredity is a potential that depends on environmental conditions, which means that upbringing plays a key role. Man would be in control of his destiny if the eugenic ideal were in fact feasible: to multiply the best... But who would be the best?

As already mentioned, the question of race was one of the most important scientific issues facing the natural and human sciences at that time. In Brazil there was a belief in the "whitening" of the people, which was a continuation of a

[6] Roquette Pinto, an anthropologist at the National Museum in Rio de Janeiro, defined anthropology at the beginning of the 20th century, unlike the "old anthropology", which wasted a lot of time arguing about the origin of man and other remote subjects, as "modern anthropology", which deals with current issues: "what are the factors of race?" (Roquette Pinto, 1933: 62).

scientific view held in the 19th century. In 1895, the painter Modesto Brocos y Gomez showed his painting *Redenção de Can* (*Redemption of Ham*), which was seen as a representation of the ideology of the whitening of the Brazilian race (Schwarcz, 2003).

Fig. 5: *Redenção de Can* (*Redemption of Ham*)

In 1911, Roquette Pinto attended the First Universal Races Congress in London as assistant to the director of the National Museum, João Batista de Lacerda. During the event Lacerda presented a polemical work entitled *Sur les métis au Brésil* (*On the mestizos in Brazil*), in which he showed the influence of black people on family morals, on the social and political formation of the country, and on the gradual whitening of the Brazilian population. At the event, Lacerda announced that in one hundred years the Brazilian population would be white, based on data from the official 1872-1890 statistics of E. Roquette Pinto (Castro Faria, 1952: 22). This idea had a great impact. The debate continued throughout the 1920s and, for many eugenicists, miscegenation was seen not as a cause of degeneration, but rather of regeneration, because it led to the stabilization of the whitening of the population by natural means.

At the Eugenics Congress of 1929, the theory of natural and "social" selection became the dominant idea and ended up defining even eugenics. Most participants believed that biological crossing accompanied migratory movements, especially in countries like Brazil, where whites, blacks, and Indians lived together (Roquette Pinto, 1933). Roquette Pinto analyzed this question taking the theory of natural selection into consideration. He explained the miscegenation of races using two ideas of selection: biological and social, and he concluded, quoting from Eugen Fischer, that the characters of two races that have crossed remain side by side in the descendants, waiting for selection to prompt one of them to become dominant.

Miscegenation is *combination*. It is absurd to use the expression *a mix of races,* explained Roquette Pinto, still quoting from Fischer. As in a chemical reaction, the substances that cause it are not the same as those that result from it, nor do they retain the same properties. A selective process, he stresses in his Mendelism, is not an infinite transformer of organisms, but it is capable of taking apart phenotypes and giving prominence to genotypes. That is what was happening in Brazil.

> It is an idle and anti-scientific concern to believe that Brazil will some day be inhabited by an anthropological type. Only those who erroneously confuse race and people desire that utopian unity for this country (Roquette Pinto, 1933: 171).

In his view, the problems that were attributed to miscegenation, such as few offspring, susceptibility to disease, a short lifespan, etc., were the result of social selection. The mixing of white and black, like any other mixture, does not cause biological inferiority, but it does result in social problems. Mixed families, with white and black parents, are small, but not due to problems of infertility. Their grandparents had been exposed to the most atrocious social conditions. Although there is selection of the fittest, *mestizos* are still vulnerable to countless diseases (syphilis, malaria, worms, etc.) more than to addictions (drunkenness). *Mestizos* were not constitutionally weak, but they suffered above all from the precarious social conditions to which they were constantly subject. Anthropology proved that people in Brazil needed to be *educated,* not *replaced*. That was one of the conclusions of the First Congress of Eugenics. It should be emphasized that Roquette Pinto's anti-racialism expressed his political position. As demonstrated below, the backwardness of certain communities –the Blacks and also the Indians– was due to the poor social conditions to which they were subject.

Within this same realm of ideas, the sanitarist Belisario Penna claims that it was not race that incapacitated backlanders and *caboclos*[7], but rather epidemic and endemic diseases. Sanitation was eugenics for him. The eugenization of the Brazilian people was not a matter of race, but of hygiene (Stepan, 2005: 172).

Roquette Pinto's book, *Ensaios de Antropologia Brasiliana* (*Essays in Brazilian Anthropology*), in which he disseminated these ideas, was published in 1933, the same year in which Tarsila do Amaral painted *Operários* (*The Workers*). The painting was part of a phase in the artist's life when she was a member of the Brazilian Communist Party. By portraying all the races in the country, the painting seems to satirize Galton's representations of eugenics.

Fig. 6: *Operários* (*The Workers*).

References

ABREU, Regina. (1988) *O enigma de Os Sertões.* Rio de Janeiro: Rocco.

AMARAL, Aracy A. (2006) *Textos do Trópico de Capricórnio: artigos e ensaios (1980-2005)*. Vol. I: Modernismo, Arte Moderna e o Compromisso com o Lugar. São Paulo: Ed. 34.

ANDRADE, Oswald. (1995) "Manifesto antropófago". In *Obras Completas. A Utopia Antropofágica*. São Paulo, Globo.

CANDIDO, Antonio. (2006) *Formação da Literatura Brasileira: momentos decisivos, 1750-1880.* Rio de Janeiro: Ouro Sobre Azul.

CASTRO FARIA, Luiz de. (1952) "Pesquisas de Antropologia Física no Brasil." *Boletim do Museu Nacional*, Abril 20: 1-106.

CUNHA, Euclides da. (1927) *Os Sertões.* Rio de Janeiro: Livraria Francisco Alves – 10ª edição.

GOTLIB, Nádia Battella. (2003) *Tarsila do Amaral, a modernista*. São Paulo: Editora Senac – 3ª edição.

JEROME, Fred. (2994) "Einstein, Race, and the Myth of the Cultural Icon." *Isis*: 627-639.

LIMA, Nisia Trindade. (1999) *Um sertão chamado Brasil.* Rio de Janeiro: Revan, IUPERJ.

MANIAS, Chris. (2009) "The Race prussienne controversy: scientific internationalism and the nation." *Isis*, December: 733-757.

MARTINS, Paulo Emilio Matos. (2001) *A reinvenção do Sertão.* Rio de Janeiro: Editora FGV.

ROQUETTE PINTO, Edgard. (1933) *Ensaios de Antropologia Brasiliana.* São Paulo: Companhia Editora Nacional.

SANTANA, José Carlos. (1999) *Ciência e Arte: Euclides da Cunha e as Ciências Naturais.* São Paulo: Hucitec.

SCHWARCZ, Lilia Moritz. (2003) "O espetáculo da miscigenação." In *A recepção do darwinismo no Brasil*, by Heloisa Maria Bertol DOMINGUES, Magali Romero SÁ, Thomas GLICK, 175-180, Rio de Janeiro, Editora Fiocruz.

[7] A *caboclo* is a person of a mixed Brazilian Indian and European or African ancestry who lives in a rural area.

SEYFERTH, Giralda. (2008) "Roquette Pinto e o debate sobre raça e imigração no Brasil."" In *Antropologia Brasiliana, ciência e educação na obra de Edgard Roquette-Pinto*, by Nisia Trindade LIMA and Dominichi Miranda de SÁ, 147-177. Belo Horizonte, Rio de Janeiro: Editora UFMG, Editora Fiocruz.

SOUSA, Natalia Peixoto Bravo, and Gastão GALVÃO. (2007) "O estigma de uma obra: Euclides da Cunha e suas reapropriações sob o ponto de vista do positivismo e do evolucionismo." *Revista Brasileira de História da Ciência*.

SOUZA, Vanderlei S. (2010) "O naturalismo de Euclides da Cunha: Ciência, Evolucionismo e Raça em Os Sertões". *Fênix – Revista de História e Estudos Culturais*, vol. 7, n. 2: 1-22.

STEPAN, Nancy Leys. (1990) "Eugenics in Brazil 1917-1940." In *The Wellborn Science: Eugenics in Germany, France, Brazil, and Russia*, by Mark B. ADAMS, 110-151. New York, Oxford: Oxford University Press.

WILS, Kaat. (1999) "Les insuffisances historiques du positivisme – Henry Thomas Buckle en Belgique et au Pays Bas." In *Positivisme, Philosophie, Sociologie, Histoire, Sciences*, by Andrée DESPY-MEYER and Didier DEVRIESE, 163-188. Bruxelles, Belgique: Brepols.

APPROPRIATION OF MENTAL MEASUREMENT IN DIFFERENT CULTURAL CONTEXTS

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APROPRIATION OF PSYCHOLOGICAL TESTING IN THE SPANISH PEDAGOGICAL CONTEXT

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Abstract

The paper starts reflecting on the specific problems of history of science when dealing with what is called "the periphery". The author calls for a historical account grounded on a specific local context. To do this it is necessary to start with the use of open and context-sensitive questions that could guide us in order to know why some scientific debates and practices made sense in a certain scientific community or local context, and not others. Thus, the aim of the present paper is to explore the specific functionality psychological testing acquired as scientific practice within the Spanish school system and local pedagogic tradition.
The primary sources studied make us think that the introduction and use of intelligence testing was not so much dictated by a real problem that school teachers detected in their classrooms, but viewed as a medium to legitimize professional pedagogic training. It was used to foster and upgrade the professionalization of Spanish reform pedagogy as scientific undertaking.

Introduction: historiography at the periphery

Mental testing, and specially the measurement of intelligence, has become crucial in defining the role of the psychologist in modern society. Some historical work has already dealt with how this device appeared (Carson, 2007; Fancher, 1985; Gould, 1981; Richarson & Johanningmeier, 1998; Rose, 1979; Sokal, 1990; Wolf, 1969; etc.). These works present a more or less contextualized history on how the ideas about intelligence in relation to the design of new methods of psychological measurement like the intelligence test of Binet and Simon were developed in France and transferred to the United States. Historians like Carson (2007) and Richarson & Johanningmeier (1998) explain the emergence of the mental test in France at the beginnings of the twentieth century and its spreading in North America a decade later arguing that the mental test as technical device emerges within a modern republican society characterized by a certain meritocratic tendency or organization.

But what happened in other places? How was psychological testing introduced in places like Madrid or Barcelona? In the present paper I follow the sensitivity expressed already by Morrell (1985) and, afterwards by other historians of science (see for example Gavroglu et al., 2008; and Nieto, 2008), towards a local history in which the individual and idiosyncratic actions of agents and their choices from the options available to them are as much the stuff of historical change as broader movements. Recently it has been the aim of the STEP group (Science and Technology in the European Periphery) to promote the peripheral factor as historical phenomenon. To express a transfer of ideas and practices they proposed the concept of "appropriation" in order to avoid the idea of passive reception. Now the emphasis lies in a process that confers an active role to the historical scientist on the periphery. Historical actors select, censure and promote certain scientific knowledge and practices, thereby negotiating and using certain strategies of legitimization in consonance with the idiosyncrasy of their local setting. Certain situations and local traditions allow some ways of doing and reflecting things, while simultaneously excluding or inhibiting others.

Following the direction marked by recent history of science I would like to argue for taking even a step further, revising carefully our research questions. Instead of building on questions borrowed from international mainstream historiography, it is necessary to use context-sensitive questions which may help us understand what particular contextual conditions governed and made sense of some scientific debates and practices, and not others. Thus, instead of guiding our inquiry by the assumption, for example, that intelligence testing should have been as important for Spanish pedagogy, as it was in the U.S. during the same period, we first try to understand the specific prevailing local conditions, asking whether psychological testing made sense within the context of the Spanish school system and pedagogic tradition.

Historical research is still rare on how novel (foreign) psychological techniques, like mental testing, were applied at the periphery, specifically in Spanish schools, and on the subsequent exploitation of the results obtained in such testing. On yet another level we need to know how Spanish scholars analyzed, used, and worked with some of the innovative diagnostic devices. The interesting question is not when Spanish scholars started employing modern techniques or who where the first to use these instruments. More fascinating is the question of why certain pedagogues became interested in mental testing and the problem of abnormality in Spain in general. These issues disclose interesting shifts in the role of psychological techniques taking place during the process of their appropriation in the Spanish pedagogical context.

Child abnormality: importing a new human classification

Once schooling became widespread, the “abnormal child” suddenly appeared as a socially threatening entity in the primary classroom. It was a kind of problematic child that supposedly disturbed normal education in public schools, once general schooling had become compulsory for all children from 6 to 9 years and free of charge for the poor. Although this is what many historians explain, when we examine the pedagogical texts published in Spain after the turn of the century, we find that the concept was clearly imported from the North (Del Cura, 2004; Herraiz, 1995; Huertas, 1998 deal with how the interest in abnormal children appeared in Spain).

The meaning of the term "abnormality" was very diffuse, usually defined referring to *“…the ones whose common characteristic is the incapacity, due to their physical and intellectual constitution, to follow normal pedagogical instruction as taught in public schools”* (Cuello, 1909, p. 330). Child mental abnormality became a major issue in Europe between about 1880 and 1910, carving out four basic concerns: speculation about possible causes, methods for diagnosis, and the need for special treatment and education. In Spain the most active promoter of the abnormality problematic in relation to education was Francisco Pereira (Doménch & Corbella, 1997).

Someone who linked his reflections on abnormality to criminology was a professor for law at the University of Barcelona: Eugenio Cuello Calón. He had previously received a grant by the JAE (Junta de Ampliación de Estudios e Investigaciones Científicas) to visit France, Belgium, and Germany from 1908 to 1909. Through his stays abroad he wanted to learn about experimental proceedings in the psychological study of the abnormal child and the problems related to criminal infancy. Once back from his journey, he presented a well-informed report (Cuello, 1909), commenting on recent research in that field (see also Cerezo, 2001). In terms of useful methods for diagnosing mental abnormality in children, Cuello mentioned several objective procedures such as anthropometric measurement, the observation of behaviour, questionnaires, and mental tests. He drew special attention to new mental tests developed in France (Binet & Simon, 1905; 1908). Although he valued quantitative (psychometric) methods because they *“represent a nice medium for analysis and comparison”*, he also advised that they should never be the aim in itself: *“we do not measure just to measure”*, but to determine quantitatively a phenomenon in order to assess its variations and the conditions of its existence (Cuello, 1909, p. 336). In conclusion he insisted on the utility of this research for *“it should not be forgotten that the research of normal and abnormal child psychology has not been developed exclusively to satisfy scientific curiosity but to obtain serious and precise data on which to base education”* (Cuello, 1909, p. 338).

The "abnormal” or “retarded” child became a modern classification for human beings, introduced by Demoor, Decroly, Ley (1904), and others in the framework of a more personalized child-centred pedagogy. As a product of medical classification, thought of as a specific type of human being that was not completely pathologized, a kind of child seemed destined to “exist at the margin” between pedagogy and medicine. Nevertheless, it was a precious “good” courted from several sides, by pedagogues, clinicians, psychologists, and criminologists in an attempt to establish or expand professional expertise.

Cuello’s report shows that besides his legal training he was also well-informed about recent psychological, pedagogical and clinical contributions to child psychology. He returned form his scientific travels with new ideas and techniques, extensive bibliographic references and personal testimonies from visits to various scientific institutions. Moreover, Cuello’s text indicates that it was not the actual abnormal child sitting in the classroom of Spanish primary schools that was causing problems and pressing pedagogues to find a solution. At that time the Spanish school system struggled with other big problems: there were simply not enough public schools and the existing ones were mostly in deplorable shape (see Cuesta, 1994). The problem of abnormality in school interested mainly a minority, the elite circle of the Madrid and Barcelona liberal bourgeoisie. It made sense in the context of a new child-centred pedagogy like the reform project pursued by the Free Institution of Education (Institución Libre de Enseñanza), and other European pedagogic institutions, which called for more individual treatment and education adapted to the special needs and personal traits of each child.

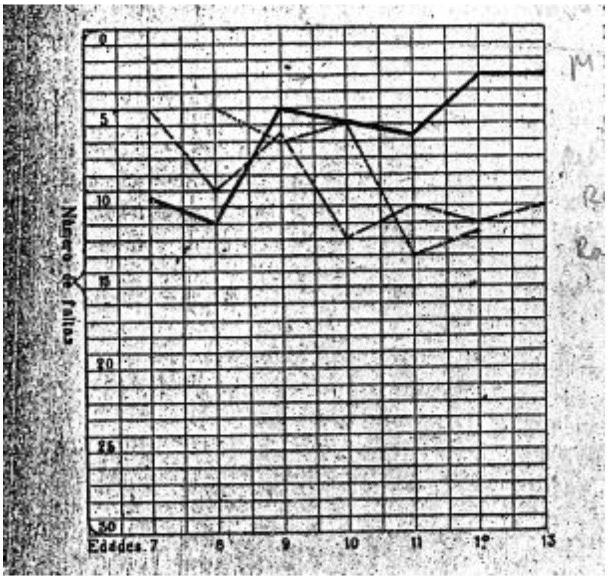
While Galton's passion for quantification bloomed elsewhere in psychology, Cuello indicated in his report that the development of quantitative methods was not on the agenda of Spanish scholars. Instead, the prime goal of progressive scholars, linked to or influenced by the ILE, was educational reform. Thus, psychological methods like mental tests interested Cuello and his colleagues only inasmuch as they were instrumental to their effort to modernize their own educational system.

Mental testing for teacher's promotion

Anastasio Anselmo González Fernández (1870-?), a teacher for experimental and special pedagogy at the teachers' college (Escuela Superior de Magisterio) in Madrid, was eager to diagnose quantitative levels of psychological capacities in children (Domenech & Corbella, 1997).

Already in 1911 González had recommended a massive application of French mental testing in official educational regulations as a procedure for the justification of teacher promotions. If we are to promote certain teachers, he reasoned, that promotion should not be subjective but based on an objective, semi-mechanical procedure. The solution is provided by Binet and Simon in their book on abnormal children (Binet & Simon, 1907): *"The method is simple, the examination [..] does not require more time than an ordinary one; while all subjective elements disappear almost entirely"* (González, 1911, p. 782). González assured that the result would be useful: *"the average of instruction would be obtained for each case, the result of the students of a series of analogue schools; based on this average it would be extremely simple to determine a numbered order in which the corresponding teachers merit ascent"* (González, 1911, p. 782).

The article indicates that he, as a trainer of teachers, instructed his students to apply tests in Spanish schools. The graph below shows the outcome of one such research, studying attention levels of schoolchildren at different schools.



Two student-teachers measured the level of attention in three different kinds of schools in Madrid: a public school, a traditional private school, and what was called a "radical" private school. No names are given but it is highly probable that the traditional private school was a Catholic school run by a religious congregation, while the radical school was probably a "freethinker" school run by anarchists or masons.

The scale on the left goes from 0 errors (top) to 30 errors (bottom), while age increases from 7 (left) to 13 (right). González proudly exclaimed: *"It can be seen that the best results have been obtained by the public school"* (González, 1914, p. 784). Although the children start with a lower attention level in public school, at the age of 7 they quickly surpass the children from other schools showing the lowest number of errors from 9 years onwards (see darker line going upwards).

We find, then, that initially Anselmo González and his trainees did not use psychological testing arrangements to detect abnormal children. He proposed an allegedly objective technique for organizing and prescribing a new merit system for teacher promotion. In this case the introduction of scientific psychological measurements was taken for an objective, apolitical way of comparing different kinds of schools. In his doctoral thesis González had already stressed that one of the greatest influences on the attention span of pupils is the pedagogical method used by the teacher. Therefore he was arguing for higher scores and consequently the faster rise of the public school teacher, while downgrading the pedagogical methods used at private schools (allegedly unable to maintain a higher attention level in pupils).

González's publication shows how pedagogy became a battlefield for the growing social and political dissonance in Spanish society during the *Restoration* period, before World War I. In an attempt to organize adequate education, several social groups had founded their own schools which were competing with each other. In this setting his text testifies his demand for a just distribution of the salary, which seemed to be more pressing than selecting abnormal children.

Mass testing in Spanish schools

A few years later,[1] González worked out a book on mental testing entitled *Diagnosis of Abnormal Children* (González,1914) (see also Cerezo, 2001 and Doménech & Corbella, 1997, some of the few historians that deal with Gonzalez's pedagogical contribution). In the well-informed book he discussed different modern intelligence testing systems.

After describing the various proposals and despite problematic aspects pointed out by Treves, Safioti, and Nayrac at the first International Conference of Paidology in Brussels, he proceeded to advocate Binet & Simon's pedagogical and psychological tests. Referring to the version of 1911 he stated: *"Binet's method satisfies all the exigencies of a good pedagogical diagnostic method, and with the help of it we can obtain a classification of the pupils that allows to send every child to the institution which corresponds to its intellectual level"* (González, 1914, p. 221). The book informs of massive application of the test at Madrid schools, done by his teaching students: *"From the moment Binet's definitive test was published, all the students of the course on abnormal pedagogy at the teacher's college applied it in various schools in Madrid. This has allowed us to obtain a high number of observations because usually each student has studied 200 schoolchildren, which nowadays makes a total sum of more than 20 000 observations"* (González, 1914, p. 212).

While applying the test, Spanish pedagogues like Gonzalez introduced several changes to the tasks included in the Binet-Simon test and its application. One of the first adaptations was to alter task order, exchanging exercises for eight year olds with those for nine year olds. They justify this change by referring to an observation also made by their French colleagues, namely, that a crisis in development takes place at that age, making the correct selection of tasks extremely difficult. Another significant modification was to increase the time limit for tasks.

Nevertheless, acting at the periphery, González was eager to reduce the relevance of these modifications while insisting that *"the basis of the Binet-Simon method subsists, because our modifications affect only details, directed towards a better adaptation of the method to application in our country"* (González, 1914, p. 219). He concluded proudly that the results are completely compatible with those obtained by foreign researchers: *"Thus, there is no disagreement; on the contrary, the results are identical and this amounts to new precision checking of the method for the assessment of child intelligence"* (González, 1914, p. 216).

Conceptual and technical appropriation at the periphery

With the rise of industrialization and the spread of democratic ideas during the nineteenth century life also changed in Spain, at least in some regions and in the cities. While the state was acquiring more responsibility, the topic of education became an issue of debate. From the 1880s on, efforts had been made to establish alternative private schools. One initiative was that of the ILE, promoted by some of the liberal and republican intellectuals. Another was pushed forward by anarchist workers associations. Towards the twentieth century most of the supporters of these initiatives realized that an educational reform of the country could not be based on the founding of private alternative institutions but needed a broader initiative led by the state, organizing public education for all. But the continual blocking of school reform initiatives by some conservative politicians and authorities who feared an emancipation of the "mob", slowed this tendency (Cuesta, 1994).

Still, at the beginning of the twentieth century we find Spanish public schools in a deplorable condition. The overcrowding and unhygienic situation let infectious diseases spread easily; education itself consisted mainly of exercising severe discipline (García Hoz, 1980). These circumstances prompted Spanish physicians to initiate hygiene campaigns while pedagogues fought predominantly for bearable working conditions.[2]

[1] following historians like Domenech and Corbella (1997) it was in 1914 edited again in 1929.

[2] Of course the situation was much better in private schools, especially in the case of recruiting the offspring of the better situated social classes. Secondary education was mainly offered in private elite schools which usually were in the hands of different religious congregations (Jesuits, Franciscans, Benedictines, Capuchins, Aesculapius, etc.).

In this context, scholars started a campaign to detect and separate "abnormal children" from others for reasons similar to those of the hygienic movement: to *purge* the classroom of the infectious and unhealthy *germ* called "abnormality" in an effort to prevent things from becoming socially dangerous. Cuello, for example, said that the abnormal child was not only disturbing at school but also a problem at home because of the *"ease with which they infect other family members with mental abnormality"* (Cuello, 1909, p. 376).

Nevertheless, for half a century Spanish institutions were not prepared to deal with the individuals thus sorted. Molina's research (Molina, 2009) shows that the "Central School for Abnormal Children", founded 1922 in Madrid, was one of the very few public specialized institutions. This implies that using mental testing to separate normal from abnormal children did not make much sense during the first two decades of the twentieth century in Spain. Private Schools were already economically selective and in public and rural schools grouping in the classroom was normally the only option anyway.

The primary sources studied here of Anselmo González make us think that the introduction and use of intelligence testing was not so much dictated by a real problem that school teachers detected in their classrooms, but viewed as a medium to legitimize professional pedagogic training. It was used to foster and upgrade the professionalization of Spanish reform pedagogy as a new scientific community. Accordingly, to enhance psychological expertise in his pedagogical students, González published a textbook in 1921 specifically on psychological testing.

In Madrid, Barcelona, and other big cities, competition was rigorous among private and public schools. The new professional community coming from training in teachers colleges faced competitors. Therefore teachers like González at the public school used the testing method to mark themselves as more professional, scientific, and modern –top quality pedagogues. As part of this effort to distinguish and promote public instruction we find a surprising shift of functionality in the appropriation of the mental testing technique. Anselmo González proposed in 1911 to use Binet and Simon's mental tests as an innovative, rational, allegedly objective and apolitical instrument for officially regulating advancement in teaching careers. Within the Spanish educational context it seemed more pressing for a public teacher to fight for a decent salary by proving the superiority of his pedagogy over that of his competitors from various private schools, than to worry about classifying individual children in terms of normality or abnormality. With his awkward graph in hand, González tried to demonstrate the superiority of pedagogical methods employed in public schools by revealing results of attention level measurement.

With regard to the process of appropriation, the texts of the Spanish authors expressed timid critique and modified the material only slightly, while expressing great respect for the foreign publications. Being in the position to apply a translated version of the French test in Spanish schools let them feel nearer to the researchers they idealized, like Decroly, Binet, and Claparède. By attending international meetings, undertaking scientific travels welcoming visits of foreign researchers, and taking advantage of the access they had to certain scientific literature and instruments, the Spanish pedagogues sought contact with modern science (see also Mülberger, 2008). Paper and pencil tests like Binet and Simon's intelligence test made it possible for Spanish teachers and pedagogues to act as scientists without needing well-equipped laboratories and sophisticated technical know-how. The testing seen as a scientific endeavour easily crossed national and scientific borders. Gonzalez argued complacently that the test had been applied successfully not only in America, England, and Belgium, but in Spain, too. Nevertheless, it was not scientific curiosity that prompted scholars like him to appropriate the test to his local context, but a desire within the Spanish reform movement to legitimize teacher training programs and to upgrade and make more professional the pedagogic occupation.

References

BINET, A. & SIMON, T. (1905). Sur la necessité d'éstablir un diagnostic scientifique des états inférieurs de l'intelligence, *L'Année Psychologique, 11*, 163-190.

BINET, A. & SIMON, T. (1908). Le developpement de l'intelligence chez les enfants, *L'Année Psycholgique, 14,* 1-94.

BINET, A. & SIMON, T. (1907). *Les enfants anormaux.* Paris: Armand Colin.

CAMPOS, R.; MARTÍNEZ PÉREZ, J. & HUERTAS, R. (2000). *Los ilegales de la naturaleza. Medicina y degeneracionismo en la España de la Restauración (1876-1923).* Madrid: CSIC.

CARSON, John (2007). *The Measure of Merit: Talents, Intelligence, and Inequality in the French and American Republics.* Princeton University.

CEREZO, M. A. (2001). *Los comienzos de la psicopedagogía en España (1882-1936).* Madrid: Biblioteca Nueva.

CUELLO CALLÓN, E. (1909). Los procedimientos experimentales para el estudio de la psicología de los niños anormales, *Anales de la Junta para ampliación de estudios é investigaciones científicas,* 3, 6.

CUESTA, P. (1994). *La escuela en la reestructuración de la sociedad española (1900-1923*). Madrid: Siglo XXI.

DEL CURA, M. (2004). Los “niños anormales” en la España del primer tercio del siglo XX: la construcción médico-pedagógica de una nueva categoría infantil. In: PERDIGUERO, E. (comp.), *Salvad al niño. Estudios sobre la protección a la infancia en la Europa Mediterránea a comienzos del siglo XX* (pp. 273-299). Valencia: Seminari d’Estudis sobre la Ciéncia.

DOMENECH, E. & CORBELLA, J. (1997). Aportación al conocimiento de la obra médico- pedagógica del Doctor Anselmo González: Estudios sobre la inteligencia. In: E. DOMÈNECH & J. CORBELLA, *Aportaciones a la Historia de la Psicopatologia infantil* (pp. 183-189). Barcelona: Seminari Pere Mata (Universitat de Barcelona).

FANCHER, R. (1985). *The intelligence men: makers of the IQ controversy.* New York: Norton.

GARCÍA HOZ, V. (1980). *La educación en la España del siglo XX*. Madrid: Rialp.

GAVROGLU et al. (2008). Science and technology in the European periphery: Some historiographical reflections, *History of Science, XIVI,* p. 153-175.

GONZÁLEZ, A. (1911). Problemas de instrucción. *Revista de Educación, I, 12*, 782-784.

GONZÁLEZ, A. (1914). *Diagnóstico de niños anormales.* Madrid: El Magisterio Español.

GONZÁLEZ, A. (1921). *Técnica experimental sin aparatos*. Madrid: Hernando.

GOULD, S. (1981). *The mismeasure of man.* U.S.: Norton.

HERRAIZ, M. G. (1995). *Aproximación a la educación especial española del primer tercio del siglo XX*. Tesis doctoral de la Universidad de Castilla-La Mancha.

HUERTAS, R. (1998). *Clasificar y educar. Historia natural y social de la deficiencia mental.* Madrid: CSIC.

LEY, A. (1904). *L’arrieration mentale*. Bruxelles.

MONÉS, J. (1977). *El pensament escolar i la renovació pedagògica a Catalunya (1833-1938).* Barcelona: La Magrana.

MORRELL, J.B. (1985). Wissenschaft in Worstedopolis: Public science in Bradford, 1800-1850, *British Journal for the History of Science, 18*, 1-23.

MÜLBERGER, A. (2008). Spanish Experiences with German Psychology Prior World War I, *Journal for the History of the Behavioral Sciences, 44, 2*, 161- 179.

NIETO-GALÁN, A. (2008). The History of Science in Spain: A Critical Overview, *Nuncius*, 23 (2), 211-236.

RICHARDSON, R. & JOHANNINGMEIER, E. (1998). Intelligence testing: the legitimation of a meritocratic educational science, *International Journal of Educational Research, 27, 8,* 699-714.

ROSE, N. (1979). The psychological complex: mental measurement and social administration, *Ideology and consciousness, 5*, 5-68.

SOKAL, M. (1990). *Psychological testing and American Society (1890-1930).* EEUU: Rutgers University.

WOLF, T. (1969). The emergence of Binet´s conception and measurement of intelligence: a case history of the creative process, *Journal of the History of the Behavioral Sciences,* 5, 113-134 (part 1), 207-237 (part 2).

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 632.

A TEACHER AS SCIENTIST: CABÓS POPULARIZING PSYCHOLOGICAL TESTING IN CATALONIA

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Abstract

The present paper deals with the intelligence testing applied and promoted by a Catalan teacher called Llorenç Cabós in the 1920s. This teacher of a graded school of Barcelona decided to learn by doing, organizing a series of paper and pencil testings in the classroom. Our research evidences the supportive policy of the local government for such an initiative and how a local daily newspaper referred to the event as a kind of scientific spectacle.

The thesis we defend is that there was no need in Catalan schools to classify students but a clear interest to professionalize teacher's career and to promote certain schools. Testing meant to Cabós to run an experiment. Tests were easy to get and to be used to obtain numeric results. Whatever these numbers meant, Cabós interpreted them as a direct contribution to science.

Introduction

In Spain some pedagogic reform initiatives appeared in several places, especially in the areas in and around the cities of Madrid and Barcelona. As we will see, in Catalonia the specific local and political context left space and even empowered new proposals in the field of pedagogy with a clear interest in psychological issues (see also Cerezo, 2001 and Moreu, 2000). This tendency became more evident in the 1920s, a decade in which new topics like "intelligence" were raised. Our main interest lies in knowing how psychological testing methods were perceived, used and spread in the local Catalan context. Therefore, the paper will focus on the initiative of using psychological measurement in pedagogy pursued in the 1920s by the pedagogue Llorenç Cabós.

We will approach our topic by tracing, in the first place, the political and social context. The twentieth century started in Catalonia displaying strong social conflicts. The first general strike organized in 1902 in Barcelona is a good example for the workers growing reaction towards the new capitalist power of Catalan industry. The tragic week in 1909 and the crisis of

1917 evidenced an extended discomfort, certain instability of the middle classes, and growing strength of the workers movement.

In such a setting, marked by social changes and tensions, we find several groups that viewed school as a medium to enhance their political interests and social values (Cuesta, 1994). One group can be characterized as the conservative, Madrid-oriented Catholic sector, enhancing traditional schooling organized by religious congregations. A second group was the liberal and republican bourgeoisie looking for an inter-confessional school system. These reforms were often strongly influenced by and connected to the *Institución Libre de Enseñanza* of Madrid. In their attempt to reform the educational system they took over the spirit of modern pedagogy based on the child's needs. Some of them defended the right of the child to receive education in their mother tongue, which meant the introduction of Catalan as official school language. A third group of schools underlined the necessity of laic instruction to ensure confessional liberty. Also from the anarchist workers similar initiatives for a school reform were launched, exemplified by Ferrer's "Escola Nova" (Cuesta, 1994).

In this paper we will focus on an impulse for mental testing that came from a Catalan teacher called Llorenç Cabós i Badia, linked to the third, rationalist, group of Ferrer i Guardia. The former worked in one of the public graded schools in Barcelona. The reason why we deal with his work here is twofold: first, he became one of the main popularizers of psychological testing in Barcelona and, second, he was the first to adapt intelligence tests to the Catalan linguistic context.

He was no lonely forerunner but one of the historical actors within a social and institutional network that opened space and even enforced projects dealing with the introduction of psychological testing in the classroom of Catalan schools. At a time of renaissance of Catalan industry and culture, the regional Government (Mancomunitat) was actively trying to improve primary education. In this regard the policy and political intervention developed around 1920 by the regional government and the Barcelona city hall was clearly supportive.

A pedagogue introduces himself into the world of testing

In the Catalan newspaper *La Vanguardia* of the 19th of March 1922 as well as in the pedagogical journal in Madrid "*Revista de Pedagogía*" (1922, I, 4, p. 160) we find an anonymous note informing about the visit of teacher-students to Cabós' school to see a trail in infantile intellectual measurement applied to one case. It was a "public demonstration" only for a group of students trained to become teachers. The newspaper commented that the mental testing presentation was made with great exhibit of skills ("*con gran maestría*") by Cabós, using the Binet-Simon test. The newspaper *La Vanguardia* wrote that in the end "*the visitors remained extremely satisfied with such a practical lesson*" (LV, 19th of March 1922; p. 11).

Cabós was a teacher at a graded school located at the "Paseo San Juan" (nº 43) in Barcelona, interested in psychological testing methods. In 1920 he published the results of a seemingly auto-didactical application of the Yerkes, Bridges and Hardwick test to 100 schoolboys (with ages between 7 and 14 years) (Cabós, 1920). The American scale was a modified version of the Binet-Simon edited in 1915 under the title *"A point scale for measuring mental ability"* by Warwick & York (Baltimore). At that time, several psychological testing systems were sold on the Spanish market[1]. Cabós informed that he had other scales available but that he started applying this one for no other reason that for "having it at hand".

In his report it is clear that the local authorities supported him on this behalf. He thanked the school director and the Cultural Committee of the Barcelona city hall (Comissió de Cultura de l'*Ajuntament de Barcelona*). This Committee, created in 1917 under the direction of Manuel Ainaud i Sànchez, was efficient in ameliorating the public school service in the city by carrying out an ambitious plan that included the establishment of several new well-equipped public schools (called "Grups Escolars"). The report published by this committee in 1916 shows clearly the intention of the city hall to support paidology, understood as modern science of the child. Already in 1911 Eugeni d'Ors, a member of the committee, had recommended Catalan teachers to apply this new technique in order to register and classify retarded schoolchildren. Now that Cabós was willing to work on a trail run, the Committee supplied the school with a substitute teacher for the course 1920-21 permitting Cabós to dedicate his time to the testing. The policy of the city hall of that time clearly enforced paidology as research project and training program for teachers and enhanced local applications and adaptations of intelligence tests.

Before exposing the results in detail Cabós announced his positive impression that "*The Barcelonean child is calculating, ingenious, lively and* mischievous", he stated satisfied (Cabós, 1920, p. 41). After examining 100 children of different ages he could show with the help of tables and graphs, the intellectual superiority of the local infantile population. Here we reproduce one of the tables where he compares his scores with the lower scores obtained by Yerkes-Bridges (Cabós, 1920, p. 44):

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[1] The Yerkes et al. scale cost 2 dollars at that time; for a list of different tests (see Herraiz, 1995).

	Yerkes-Bridges	Nosotros
5 años, puntos...	22	31
6 » »	29	35
7 » »	34	41
8 » »	39	50
9 » »	52	55
10 » »	59	61
11 » »	64	69
12 » »	64	73
13 » »	74	74
14 » »	78	78,5

In 33% of the children he diagnosed a slight backwardness, while 62 % were advanced with regard to their age. Only in 5% of the cases the mental age was the same than the chronological age. These results are surprising because previous attempts to apply intelligence tests had evidenced a lower performance of Spanish children (Comissió d'Instrucció Pública i Belles Arts, 1916).

Cabós' contribution (1920) ended with three conclusive statements. First, he could inform that the application of the metric scale of intelligence is really easy. This statement surely invited other teachers to follow his example. Secondly, in his opinion the application of psychological tests should be made compulsory for graduate schools, where there is enough personnel to take care of this service and therefore not to be applied in unitary schools. This meant that only certain privileged, bigger schools should or could organize massive psychological testing. Although the school service of the city was still deficient in the 1920s Barcelona was proud of some modern (private) "model" schools like the "Escola del Mar" or the "Escola Horaciana". Thirdly, Cabós insisted in the importance of keeping a register of the achieved results for each student.

Expressions like the following: *"acts say more than words"* (Cabós, 1920, p. 44) show his preference for quantitative information and the value he gives to numbers *per se*. This becomes evident in his report where he dedicated more space to expose numeral results than to interpret them. In his opinion: *"the view of these numbers invites the reader to numerous thoughts which should be made by himself"* (Cabós, 1920, p. 43).

The attraction of testing: popularizing psychological testing with the help of public demonstrations

Noticing general interest in his testing and his success of his public demonstration, Cabós promised to complete the demonstration very soon. This time he would apply the Terman test (1916), a popular intelligence test which was a revised version of Binet-Simon's. The most important difference was "mental age" being replaced by "intelligence quotient".

About five weeks later, news on Cabós performances with testing appeared again in the newspaper. Now it was Cabós who went to the teacher's school ("Escuela Normal de Maestros") to give a public demonstration of the use of the Terman test. He applied it to measure the intelligence of a 15 years-old boy. With regard to the tasks included in the test the newspaper comments: "*These vary in number and in difficulty, depending on the age (...). They are accurate and demanding, some did call the attention because of their ingenuity*". All in all, it was clear that "*The experiment resulted* (to be) *very interesting...*" (see Notas in *La Vanguardia*, 2nd of May 1922; p. 5).

In a second publication in the "Journal of Pedagogy", Cabós (1922) analyzed more carefully the performance of his children in each task. Therefore he recognized the linguistic problems of his sample. The task of constructing a sentence using three given words, for example, resulted very difficult, being solved only by 37 % of the schoolboys. The explanation of this result comes right away: "*The language tasks are more difficult for bilingual persons*" (Cabós, 1922, p. 94). The majority of his school children were Catalan or Valencian. They spoke two languages: their mother language and Spanish.

Mental testing was something new to the Catalan public, although there were some schools proud of working with personal "anthropometric profiles", introduced in selected schools like the graded primary schools "Districte VI", directed by the physician Estrany. This school counted with a complete medical support permitting a technical control of the schoolchildren with regard to physical, psychological, and pedagogical variables (Galí, 1979). Another similar school that could count with medical and psychological support was the "Escola Horaciana". Galí informs of a regular psychological control that was carried out in these private schools to know the intellectual age of each child.

It is important to keep in mind that Cabós did not have any formal training as scientific researcher in psychology or in psychological measurement. His audacious decision to "learn by doing" turned him into an attractive example or forerunner, a courted expert during a time in which curiosity and interest for this new scientific way of doing paidology was growing in the local setting. For this reason he received invitations, such as to give a course at the Barcelonan Teacher Association on *"the measurement of intelligence*" (see Notas in *La Vanguardia*, 8th of July 1923, p. 5) and lectures at the Teacher's summer school. The way in which he introduced the intelligence measurement in the first course for teachers shows that for him the application of a mental test meant to run a psychological experiment. Testing was perceived as a way to introduce scientific methods into the field of pedagogy. In order to know what he presented in the second course it is worthwhile to take a closer look at the report of the summer school.

The teacher's summer school: a place to spread mental testing

After his start into psychological testing, Cabós was pushed further into intelligence measurement by the *Federation of Catalan National Teachers* (*Federació de Mestres Nacionals de Catalunya*), which was the entity that asked him to lecture on this subject in the Summer School for teachers of 1922 held in Barcelona (Cabós, 1923; Monés, 1977).

This Federation unified several local teachers associations that had emerged after 1901. Previous to the creation of a teacher's trade union, it tried to supply support to public teachers and cooperated between 1914 and 1923 with the pedagogical reform initiatives launched by the city hall and the regional government (Monés, 1977). One of these initiatives was the Summer School for teachers (Escola d'Estiu) held during this period as an intensive course of one month of duration during school vacations to foster pedagogical training for professionals in the field of education. The school offered an interesting forum for a debate about recent pedagogical tendencies and methods. No wonder topics like abnormality, intelligence, and psychological diagnostic methods were dealt. Surely Claparède's personal visit and participation as invited lecturer in the summer school of 1920 helped to enforce interest in these topics.

Using foreign intelligence tests, Cabós recognized the need to adapt the language test to the Catalan speaking population. In a talk he gave at that teacher's summer school (Cabós, 1923) he informed about an adaptation he had made of the Terman vocabulary test. This test consisted in asking the child to define words like friendship, laughing, and cinnamon counting the number of correct definitions.

Instead of preparing a list of the most common words used by his local population he preferred to translate the list of A.M. Aguayo's collection of Spanish words assembled in Cuba (see Aguayo, 1921) into Catalan. Thus, Cabós explained: *"Taking into account the need of work in Catalan, we have limited ourselves to translate, as exactly as possible, the Spanish Vocabulary of Dr. Aguayo, evidently as a provisional measure, willing to modify it due to the demand of the practical application which will hopefully get extended all over Catalonia"* (Cabós, 1923, p. 55).

Cabós not only adapted the test itself to the local language, he also prepared a booklet for the application of the Terman scale (Quadern-registre) in Catalan language. The text was edited by the Committee for education of the city hall and distributed for free at the teacher's summer school. The handing out of this material together with his explanations on the different tasks of the test was a clear invitation for Catalan teachers to apply intelligence tests in their respective classrooms. He ended his talk insisting in the necessity for the modern teacher to have at hand an "*accurate research instrument*" (Cabós, 1923, p. 59).

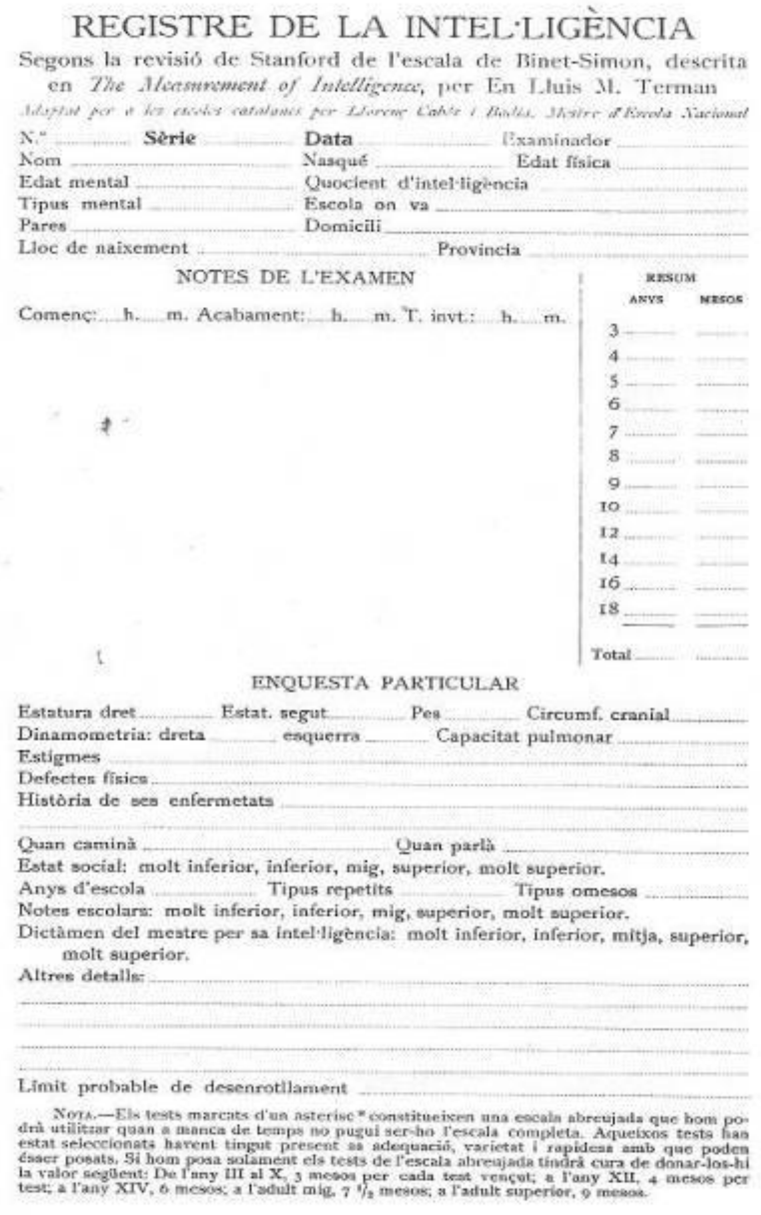
Here we reproduce the first two pages of Cabós' record-booklet for the Terman scale[2].


COMISSIÓ DE CULTURA
ASSESSORIA TÈCNICA

Quadern-Registre de la Intel·ligència
(RECORD-BOOKLET)

Adaptat al català per
Llorenç Cabós i Badia
Mestre d'Escola Nacional

2 We thank Salvador Domènech for sending us this material.



REGISTRE DE LA INTEL·LIGÈNCIA

Segons la revisió de Stanford de l'escala de Binet-Simon, descrita en *The Measurement of Intelligence*, per En Lluis M. Terman

Adaptat per a les escoles catalanes per Llorenç Cabós i Bolós, Mestre d'Escola Nacional

N.º .... **Sèrie** .... **Data** .... Examinador ....
Nom .... Nasqué .... Edat física ....
Edat mental .... Quocient d'intel·ligència ....
Tipus mental .... Escola on va ....
Pares .... Domicili ....
Lloc de naixement .... Provincia ....

NOTES DE L'EXAMEN

Començ: ...h. ...m. Acabament: ...h. ...m. T. invt.: ...h. ...m.

RESUM

	ANYS	MESOS
3		
4		
5		
6		
7		
8		
9		
10		
12		
14		
16		
18		
Total		

ENQUESTA PARTICULAR

Estatura dret .... Estat. segut .... Pes .... Circumf. cranial ....
Dinamometria: dreta .... esquerra .... Capacitat pulmonar ....
Estigmes ....
Defectes físics ....
Història de ses enfermetats ....
Quan caminà .... Quan parlà ....
Estat social: molt inferior, inferior, mig, superior, molt superior.
Anys d'escola .... Tipus repetits .... Tipus omesos ....
Notes escolars: molt inferior, inferior, mig, superior, molt superior.
Dictàmen del mestre per sa intel·ligència: molt inferior, inferior, mitja, superior, molt superior.
Altres detalls: ....
Limit probable de desenrotllament ....

NOTA.—Els tests marcats d'un asterisc * constitueixen una escala abreujada que hom podrà utilitzar quan a manca de temps no pugui ser-ho l'escala completa. Aqueixos tests han estat seleccionats havent tingut present sa adequació, varietat i rapidesa amb que poden ésser posats. Si hom posa solament els tests de l'escala abreujada tindrà cura de donar-los-hi la valor següent: De l'any III al X, 3 mesos per cada test vençut; a l'any XII, 4 mesos per test; a l'any XIV, 6 mesos; a l'adult mig, 7 1/2 mesos; a l'adult superior, 9 mesos.

Conclusions

In the Catalan pedagogical context we find in the 1920s a noticeable sensibility with regard to psychological testing. In an attempt to modernize pedagogy with the use of scientific methods and experimentation, local authorities stimulated non experts like the pedagogue Llorenç Cabós to start applying the new accessible devices of mental testing. In Spain some public entities and institutions already cited in our paper like the city hall and the regional government was clearly supportive.

In the international scientific exchange, mental tests were a very attractive product for two reasons: First, they were cheaper than any of the previous requisites for psychological experimentation based on physiological brass-instruments. Second, also their use was perceived as more intuitive and easy. The results obtained in form of number seemed to stand for themselves as scientific valuable and comparable data. Thus, the project of a modern science as universal undertaking publishing and commercializing affordable technical methods helped Catalan pedagogues to get in contact with modern scientific pedagogy and paidology.

A teacher like Cabós, with no previous psychological training, was able to translate from French or English the tasks or questions to be passed to the school-children and, of course obtained results. Whatever these numbers really meant or represented, it was a way to compare, what he considered scientific results with what other (foreign) researchers had obtained. The act of passing the test became a kind of spectacle that was even mentioned in the daily Barcelona press like the newspaper *La Vanguardia*.

The local city hall made possible the distribution for free of a booklet with instructions, tasks and the register of results translated into Catalan by Cabós. His public demonstrations, his lectures at the summer school and other teacher's meetings, his publications in several pedagogical journals like the Butlletí (also distributed for free to the Catalan teachers, sponsored by the regional government), helped to spread mental testing in Catalan schools.

In our view psychological testing did not start as a result of a local need to classify children by their mental age or IQ in public schools of Barcelona. It seems to have been a way to professionalize the work of the teacher, to turn the teacher into a scientific researcher (see Mülberger's paper in this session). The competition between schools enforced a comparison. Therefore Cabós refers proudly to his modern, graded school and the intelligent Catalan children present in its classrooms. He was eager to show the intellectual superiority of his sample. In the case of the linguistic task in which his children did not perform that well, he excused them, mentioning their bilingualism. At this moment he recognized the need to translate the language task into Catalan. Whereas the pedagogical laboratory under the direction of Dwelshauvers would soon undertake a sampling of the most used Catalan words, Cabós at that time had no other repertoire at hand than a word list elaborated in La Habana.

References

ANÓNIMO (1922). Notas. *La Vanguardia*, 19th of March 1922, p. 11.

ANÓNIMO (1922). Noticias. *Revista de Pedagogía*, año I, nº 4, p. 160.

ANÓNIMO (1922). Notas. *La Vanguardia*, 2nd of May 1922, p. 5.

ANÓNIMO (1923). Notas. *La Vanguardia*. 8th of July 1923, p. 5.

AGUAYO, A.M. (1921). El vocabulario de los niños cubanos, *La Escuela Moderna, 31, 353,* p. 111-122.

CABÓS, L. (1920). Un curso de psicometría práctica en una escuela nacional de Barcelona, p. 39-44. *Inspección médico-escolar de Barcelona*. Barcelona: Tip. La Academia de Serra y Rusell.

CABÓS, L. (1922). Un ensayo de medición intelectual infantil. *Revista de Pedagogía*, año I, nº 3, p. 90-94.

CABÓS (1923). La mesura de la intel·ligència. *Escola d'Estiu, any 1923: resums de l'any 1922, lliçons i conferències, treballs dels alumnes, informació*. Barcelona: Consell de Pedagogia.

CEREZO, M. A. (2001). *Los comienzos de la psicopedagogía en España (1882-1936)*. Madrid: Biblioteca Nueva.

COMISSIÓ d'Instrucció Pública i Belles Arts (1916). *Informe-propuesta de la Comissió d'Instrucció Pública i Belles Arts para la fundación de los laboratorios de psicología experimental y pedagogía*, Barcelona, 23 de Junio de 1916. (document de l'Arxiu històric de la Diputació de Barcelona).

CUESTA, P. (1994). *La escuela en la reestructuración de la sociedad española (1900-1923)*. Madrid: Siglo XXI.

D'ORS, E. (1911). Medida convencional de la inteligencia para la determinación y clasificación de los atrasados, *Revista de Educación*, 1, 2, 106-108.

GALÍ, A. (1979). *Obra completa. Història de les institucions i del moviment cultural de Catalunya, 1900-1936. Llibre II: Ensenyament primari. Segona part*. Barcelona: Fundació Alexandre Galí.

HERRAIZ, M. G. (1995). *Aproximación a la educación especial española del primer tercio del siglo XX.* Tesis doctoral de la Universidad de Castilla-La Mancha.

MONÉS, J. (1977). *El pensament escolar i la renovació pedagògica a Catalunya (1833-1938)*. Barcelona: La Magrana

MOREU, A. (2000). *La Psicopedagogia en España, 1875-1936 : la construcción interdisciplinar de un entorno educativo*. Universitat de Barcelona: Tesis Doctoral.

YERKES, R. M. (1915). A point scale for measuring mental ability, *Proceedings of the National Academy of Sciences of the USA*, February; 1(2): 114-117.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 638.

WHEN AN INSTRUMENT CROSSES BORDERS: MEASURING MIND IN EARLY TWENTIETH-CENTURY FRANCE AND AMERICA[1]

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Abstract

How have modern democracies squared their commitment to equality with the fear that disparities in talent and intelligence might be natural, persistent, and consequential? This talk explores the story of how the American and French republics turned to the sciences of human nature, and specifically particular practices of measurement, to help make sense of the meaning of human inequality. These sciences' exploration of the status and character of human mental differences, it contends, provided a range of political theorists, social scientists, and practical politicians with seemingly objective grounds for interrogating the limits of human equality and developing what could be represented as a justifiable basis for social distinctions. In general, mental philosophers and political theorists on both sides of the Atlantic argued that if the "false" distinctions of wealth or family background or beauty or any of the other accidents of birth could be eliminated, then the "true" ones, those reflecting fundamental aspects of a person's nature, could come to the fore. However, over the course of the nineteenth and twentieth centuries, the specific ways in which each society responded to the evolving sciences of human nature diverged sharply as these nations addressed the problem of balancing equality and difference. This talk will investigate the nature of that divergence and the crucial role that determinations of, and contestations over, ways of assessing intelligence played in both societies, constituting shadow languages of inequality used to help organize educational systems, justify racial hierarchies, classify army recruits, and direct individuals onto particular educational and career paths. It will also explore some of the hesitations about, and resistances to, these practices as they were elaborated and enacted.

"Standardizing the mind is as futile as standardizing electricity," Justice John W. Goff of the New York State Supreme Court proclaimed in 1916, thereby proving himself as prescient about psychology as he was about physics.[2] Electricity, as the folks at Duracell or Eveready could attest, has become a very well standardized object. Indeed, as Simon Schaffer has

[1] This talk is adapted from John Carson, *The Measure of Merit: Talents, Intelligence, and Inequality in the French and American Republics*, 1750-1940 (Princeton: Princeton University Press, 2007).

[2] "No Psychology in Law," *Literary Digest* 53(1916): 405-406 and "Court Bars Binet Test," *The New York Times*, July 19, 1916, p. 7.

argued, one of the great achievements of late nineteenth-century electro-physics was its creation of a "manufactory of ohms," a site where a standardized electrical unit could be produced and converted into a black-boxed instrument capable of being dispatched around the world to regulate electrical practice and the electrical measurements on which it depended.[3] Schaffer's story of the ohm does not stand alone. Work during the last two to three decades on the history of metrology (the science of standards and measures) has again and again found the Schaffer's basic narrative applies, whether to the standardization of the meter as described by Ken Alder or of the lab rat, as recounted by Karen Rader.[4] Various possible standards are proposed, often by rival research groups; technical problems in constructing and making reliable the standard are eventually solved; and the standard that triumphs is then both distributed across a network of relevant practitioners and used to regulate subsequent work in the field. Bruno Latour has in many ways embraced this understanding of standardization as one of his central analytics, describing the development of actor networks with centers of calculation that regulate far-flung practices and ensure that there is calibration throughout the system.[5] It is essentially an imperial story, where a metropolitan institution defines (and often physically maintains) the standard, and then demands that provincials everywhere adopt that standard if their work is to be taken seriously.

This picture constitutes, I think, one of the main meta-narratives that historians of science and other science studies scholars have at our disposal for understanding the development and deployment of scientific instruments and standards. But it is not the only one. A second narrative, drawn one might say more from post-colonial studies, emphasizes less the co-optation of the periphery by the center, and more the adoption, adaptation, and repurposing of artifacts and ideas as they circulate between putative metropoles and provinces.[6] Two very well known examples in science studies of this sort of story are Madeleine Akrich's research on the transfer of technical objects, as she calls them, between Europe and Africa, and Ron Kline and Trevor Pinch's study of the role of the user in shaping the early history of the automobile in rural America.[7] Both emphasize that the wishes and designs of producers, including their imagined consumers and the imagined uses of the artifacts, are only one of the factors shaping what will happen when a technical object is moved to a new locale. Instead, the nature of local conditions, the cultural contexts into which the artifacts are introduced, and the needs/interests of those wanting to adopt the artifacts all have powerful influences on what will be adopted and how it will be used. Rather than the center dictating to the periphery and regulating its actions, this narrative emphasizes the plasticity of techno-scientific objects and the power of local conditions and actors to remake, either subtly or dramatically, the object being transferred as it is adapted to its local environment. It is a story less of empire than of evolution, with emphasis on variation and local adaptations.

This second meta-narrative also resonates with the early history of the Binet-Simon metric scale of intelligence. Created in France by Alfred Binet and Théodore Simon in the period 1904-1911, the Binet-Simon scale quickly spread widely, not only to the United States, but, as the papers in this panel demonstrate so well, to many parts of the world. Importing the Binet-Simon, however, almost always required some sort of local adaptation, if only translating it into the relevant language. And most often many other transformations were made as well. Provincials, in other words, were rarely content to take the instrument as defined in the metropole and to let its structure discipline their activities. Rather they engaged with it creatively, transforming elements to fit local needs and contexts. Such a story certainly holds true for the United States, itself a nation as much on periphery as in the center vis-á-vis Europe even up to the early twentieth century. When the Binet-Simon metric scale came to America, though many psychologists embraced it quickly, they just as quickly began to change it, adapting the instrument to not just its new language, English, but to its new home. And they did so with scant worry about what psychologists in France might think about their endeavors.

Thus, we seem to have two compelling but rather different, if not contradictory, narratives for the dissemination of the Binet-Simon metric scale in the early years of the twentieth century. If one foot lay clearly in the rather imperializing landscape of metrology, of the production of potentially universal standards supervised from metropolitan centers, the other lay just as firmly in the provincializing terrain of evolutionary adaptations and transformations in response to local conditions. In particular, these competing accounts raise two important questions: first, how did local practitioners, in the process of translating, adapting, and often transforming the metric scale, still have confidence that it worked? What convinced them of

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3 Simon Schaffer, "Late Victorian Metrology and its Instrumentation: A Manufactory of Ohms." In *Invisible Connections: Instruments, Institutions, and Science*, eds. Robert Bud and Susan E. Cozzens (Bellingham: SPIE, 1992).

4 Ken Alder, *The Measure of All Things: The Seven-Year Odyssey and Hidden Error that Transformed the World* (New York: Free Press, 2002); Karen Rader, *Making Mice: Standardizing Animals for American Biomedical Research, 1900-1955* (Princeton: Princeton University Press, 2004).

5 Bruno Latour, *Science in Action: How to Follow Scientists and Engineers Through Society* (Cambridge: Harvard University Press, 1988).

6 Dipesh Chakrabarty, *Provincializing Europe: Postcolonial Thought and Historical Difference* (Princeton: Princeton University Press, 2007).

7 Madeleine Akrich, "The De-scription of Technical Objects," in *Shaping Technology/Building Society Studies in Sociotechnical Change*, eds. W. Bijker and John Law (Cambridge: MIT Press, 1992), pp.205-224; and Ronald R. Kline and Trevor Pinch, "Users as Agents of Technological Change: The Social Construction of the Automobile in the Rural United States," *Technology and Culture* 37 (October 1996): 763–95.

its efficacy once it was no longer the "same" instrument as that being deployed in Paris? And second, how were results arrived at within the context of one locale meant to be linked with those in another? Was there interest in the creation of a universal measuring standard, and if not, what was the status of this instrument, and by implication, the field of mental measurement meant to be as a science? In my brief time this morning, I will try to cast at least cast a little bit of light on both of these questions by taking a much too quick look at the early history of the Binet-Simon test in France and America.

When Alfred Binet commenced work in 1904 on the first version of his metric scale, published in 1905, the direct impetus was his appointment to a ministerial commission on children lagging in school. The scale began, as Binet and his colleague Théodore Simon explained at the Fifth International Congress of Psychology in Rome (1905), as a new means of diagnosing idiocy, imbecility, and feeblemindedness (*débilité*), and distinguishing these subnormal types of intelligence from normal minds lacking sufficient training. Their initial goal was to replace what they deemed the "arbitrary" classificatory methods of doctors and educators with a procedure for defining degrees of intellectual deficit that was more objective, precise, and above all scientific. The thrust of Binet's work in the 1905 scale was to remedy this lack of precision by creating a series of tasks that would differentiate unambiguously between the normal mind and the various degrees of subnormal intelligence.[8]

To accomplish this, Binet developed a series of thirty tests arranged from simplest to most difficult. Calibrated through application to "normal" children, the series contained sets of tests that, Binet contended, could be passed by individuals of one level of intelligence, but not of a level below. At its simplest, therefore, the Binet-Simon was constructed as a series of barriers that would stratify a population into its "natural" intellectual levels along the scale of mental development. While Binet originally conceived of the test as limited to subnormals, the test's "genius," as it were, was its equation of subnormal intelligence with arrested normal intelligence and thus its creation of a scale that in principle could apply to normal children as well as the feebleminded. Adhering to a fundamental tenet of French clinical psychology, that the pathological differed only in degree from the normal, Binet imagined the metric scale as a means of determining a precise relation between abnormal states of intelligence and normal intellectual development. "Here, in our studies on children," Binet commented in 1905, "it is not only a comparison that is necessary, it is a physiological, anatomical and anthropological table of standards [barême] to which one must return every time with each new subject to determine in what measure this subject is inferior to the normal."[9]

The crucial word here is "barême," which signified a standard that captured some aspect of what it meant to be "normal" and that could be used to analyze deviations from this normal state. Binet's "barême," however, suggested a second feature as well: the possibility of measuring the degree of deviation from these objective standards. And Binet's scale itself did not just compare diseased with healthy, it also measured the degree of intellectual development. This became particularly apparent in his 1908 revision of the scale, which was comprised of an almost totally new collection of fifty-six tests, arranged in sets according to mental age, and calibrated so that most normal children of the relevant age could pass them while at most half of the age just below could do so. In addition, Binet added to the 1908 scale an explicit procedure for determining a numerical intellectual level, the "niveau mental," and more rigid criteria for a correct response. These changes substantially increased the emphasis on precision and expanded the scale's target population to include normal children as well as subnormals.[10]

As is well known, the reception of the Binet-Simon intelligence scale in France was lukewarm at best. Almost immediately, French physicians proved hostile to a diagnostic technique that apparently challenged their authority, a reaction that pushed Binet to reorient his intelligence work away from medical diagnosis and "toward practical and social questions," as he declared in 1908.[11] Although Binet worked assiduously to identify a range of possible uses for the scale and to promote it to everyone from the Ministry of Education to the French army, the actual uptake of the instrument was modest. Binet's sudden death in 1911 at the age of 54 deprived the scale of its most important champion. While most French psychologists acknowledged that the Binet-Simon was a measuring instrument of some practical value, few found it of more than limited relevance to their own research programs. The orientation of French psychological investigation in the early twentieth century was overwhelmingly toward clinical studies of individual pathology or laboratory experimentation on basic psychological functions, not toward the practical applications of psychological science to social problems. Moreover, most institutions remained content with time-honored methods for identifying the mentally subnormal (the arrièrés), the mentally superior, and those best suited for certain types of training or occupations.

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[8] See Alfred Binet and Théodore Simon, "Sur la nécessité d'établir un diagnostic scientifique des états inférieurs de l'intelligence"; "Méthodes nouvelles pour le diagnostic du niveau intellectuel des anormaux"; and "Application des méthodes nouvelles au diagnostic du niveau intellectuel chez des enfants normaux et anormaux d'hospice et d'école primaire"; all in *L'Année psychologique* 11 (1905): 163–336. For English translations, see *The Development of Intelligence in Children,* trans. Elizabeth Kite (Baltimore: Williams & Wilkins, 1916).

[9] Binet and Simon, "Méthodes nouvelles pour le diagnostic," p. 243.

[10] Alfred Binet and Théodore Simon, "L'intelligence des imbeciles," *L'Année psychologique* 15 (1908): 1–147.

[11] Alfred Binet, "Préface," *L'Année psychologique* 14 (1908): v–vi, quote on p. v.

Outside of France, however, the response to the Binet-Simon metric scale was anything but ambivalent, notoriously so in the case of the United States. The 1905 Binet-Simon scale arrived in America in 1908, one of the spoils of a research junket to Europe undertaken by American psychologist Henry H. Goddard of the New Jersey Training School for Feebleminded Girls and Boys in Vineland. Goddard commenced experimenting with the scale soon after his return, but found it of limited value. When Binet published the 1908 revised Binet-Simon, Goddard at first hesitated to use it, recalling later that it had "seemed impossible to grade intelligence in that way. It was too easy, too simple."[12] Intrigued by the possibility, however, Goddard finally administered the revised instrument to residents at his school, and reported himself to be amazed by the results. The scale, he declared, provided accurate diagnoses of the mental levels of all of the children he had examined. What had taken Goddard and the staff months to determine through long exposure to the subjects, the Binet-Simon was able to reveal in a single testing session.[13]

It would soon be clear that Goddard's experience was by no means unique. By 1916, when Lewis Terman completed the Stanford-Binet, his version of the Binet-Simon intelligence scale, intelligence was already something of an industry in American psychology. Articles either about the Binet-Simon scale or research data generated by the scale filled professional journals; numerous rival versions of the scale competed for clientele and professional dominance; and a great deal of hand-wringing was in evidence about the improper use of the technology by those deemed "ill-trained" to apply it appropriately. American psychologists were infatuated with the test; they adopted with few reservations what their French counterparts found either uninteresting or problematic. In the process, however, they adopted wholesale the version of intelligence—singular, hierarchical, unidimensional—built into the Binet-Simon instrument, as well as a vision of how the scale might best be deployed.

Once Goddard began the process of Americanizing the intelligence test, others quickly followed. Terman and his student Hubert G. Childs began to investigate the 1908 scale in 1910, conducting extensive experiments on schoolchildren in California that would result in their first revision of the scale, published in 1912.[14] Edmund B. Huey praised the test in 1910 as "the most practical and promising means yet made available for determining the fact and for measuring the amount of mental retardation" and produced a translation, as did Fred Kuhlmann in 1911.[15] Other psychologists also jumped into the fray. Although complaints about details of the French scales abounded and almost every psychologist altered the tests at least slightly to meet their own needs, the Binet version of the scale remained dominant. Many psychologists were uneasy about this fact, aware that a measuring instrument designed for Parisian children might be inaccurate for Americans, with their different set of culturally specific background knowledge, a criticism Leonard P. Ayres leveled explicitly in 1911.[16] In 1916 these worries were mostly alleviated with Terman's publication of the Stanford-Binet, his overhauled version of the Binet instrument standardized on an American population.[17] While some psychologists—most notably Robert M. Yerkes—continued to express misgivings about the Binet approach and to propound alternatives, the majority of the testing community adopted the Stanford-Binet as the standard test for measuring intelligence, a position it maintained until well into the century.[18]

With these quick sketches of the very different careers of the Binet-Simon metric scale in France and America in mind, I would like to return to the issues I posed at the outset. On the surface, the story of the Binet-Simon scale does not fit very well with the standard metrological narrative about the development and deployment of standards, the dominance of metropole over periphery. Although Binet and Simon's express purpose was to standardize diagnoses of subnormal intellect, and although they built their equivalent of the standard ohm—their barême based on the performance of Parisian school children—into the heart of the scale, the American adopters of the instrument never felt particularly comfortable with this standard. Many did admit that simply translating the scale was enough to make it work; indeed, recall that Goddard reported himself to be astonished by how accurately the 1908 scale was able to classify the subnormal population in his asylum. Nonetheless, almost all of the American psychologists who began using the Binet-Simon scale tinkered with it, changing

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12 Henry H. Goddard, "Introduction" to Alfred Binet and Théodore Simon, *The Development of Intelligence in School Children,* trans. Elizabeth S. Kite (Baltimore: Williams & Wilkins, 1916), p. 5.

13 Henry H. Goddard, "Four Hundred Feeble-Minded Children Classified by the Binet Method," *Pedagogical Seminary* 17 (1910): 387–97.

14 Lewis M. Terman and H. G. Childs, "A Tentative Revision and Extension of the Binet-Simon Measuring Scale of Intelligence," *Journal of Educational Psychology* 3 (1912): 61–74, 133–43, 198–208, 277–89.

15 Edmund B. Huey, "The Binet Scale for Measuring Intelligence and Retardation," *Journal of Educational Psychology* 1 (1910): 435– 44; and Fred Kuhlmann, "The Results of Grading Thirteen Hundred Feeble-Minded Children with the Binet-Simon Tests," *Journal of Educational Psychology* 4 (1913): 261–68.

16 Leonard P. Ayers, "The Binet-Simon Measuring Scale for Intelligence: Some Criticisms and Suggestions," *Psychological Clinic* 5 (1911): 187–96.

17 Lewis M. Terman, *The Measurement of Intelligence: An Explanation of and a Complete Guide for the Use of the Stanford Revision and Extension of the Binet-Simon Intelligence Scale* (Boston: Houghton Mifflin, 1916).

18 For Yerkes's critique of Binet-style instruments, see Robert M. Yerkes, "The Binet Versus the Point Scale Method of Measuring Intelligence," *Journal of Applied Psychology* 1 (1917): 111–22.

tasks, altering acceptable answers, and generally remaking the scale to fit their own conceptions of what an accurate measurement of intelligence would look like, given the population they were working with. They seemed, in other words, completely unworried about the decoupling of their instruments, and thus of their measurements, from those produced in Paris. Terman's creation of the Stanford-Binet completed this process. Not only did he replace most of the Stanford-Binet tasks with his own, but introduced a new barême as well, his 965 California school children. What was now left to connect the findings produced by the Stanford-Binet in America with those of the Binet-Simon in Paris?

The answer, of course, is not much, though it is also not clear that anyone really cared. With Binet's death there was no central figure in France with sufficient interest and authority to demand that the measurements remain commensurate, that the Parisian standard prevail. Moreover, the kinds of problems that the metric scale was meant to address were in many ways local ones: which students needed special education, which recruits would benefit from advanced training. There was little social need to have these determinations be able to travel and to accord with one another. When that issue did become pressing in the United States, it was to make the evaluations work on a national scale, and quickly the Stanford-Binet with its ability to provide a single measure, the IQ, came to fill that role. For decades after, one of the most important ways of authenticating new mental tests in America would be to ensure that they correlated with Stanford-Binet IQ scores. So perhaps it was the Stanford-Binet that eventually took on the role of standard setter, with IQ becoming a widely employed way of expressing the measured level of a person's intellect.

But with regard to the Binet-Simon, I do not want to suggest that the story of its dissemination unproblematically fits the model of adaptation and local contextualization. Certainly in the U.S. case, and I suspect in others we will hear about today as well, there were clear limits to how much the Binet-Simon could be transformed. In particular, even though its specific standard population did not end up prevailing, its method of using age stratification as a way to assess intelligence and its vision of intelligence as a singular entity that could be numerically gauged did. Almost every American psychologist who worked with the metric scale considered the technique of using age-stratified performance by "normal" children as the way to assess precisely intellectual deficit to be Binet's key innovation, and most marveled at how well that worked. They might not agree on the specific tasks that best correlated with chronological age, but all embraced the method. And, with even less equivocation than Binet, most also embraced wholesale the vision of intelligence—singular, hierarchical, unidimensional— that the 1908 and 1911 scales embodied, as well as his ambitions for the possible social roles that the scale could fulfill. Flexible in some ways, the Binet-Simon scale proved extraordinarily obdurate in others.

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PSYCHOLOGICAL MEASUREMENT IN BRAZIL IN THE 1920S[1]

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Abstract

The paper traces the historical context to show the specificity of the development of Brazilian psychology while discussing the notions of centre and periphery. It focuses on the measuring with psychological tests in the early twentieth century. The great cultural, political, and economic effervescence taking place in the 1920s transformed the experimental laboratories into places where psychological measurement was applied. Additionally, local laboratories of the so called "Normal Schools" were dedicated to prepare teachers for elementary education. The Brazilian Mental Hygiene League is also presented and its function is compared with that of the New School movement in relation to psychological testing. Our analysis seeks to clarify the relationship between the expanded and consistent presence of psychological measurement and the institutional support of the government, which led in many cases to a transformation of psychological measurement in something like a state project.

Introduction

Brazil entered the European universe in 1500, when the first Portuguese ships arrived at what today is Brazil's territory. Colonization left deep marks in the Brazilian way of living and thinking from there on. The production and dissemination of scientific knowledge was quite limited until the early years of the 19th century. It depended chiefly on the importation of foreign works and brief internships in the European territory, mainly, in France, taken by those interested in psychological topics. The elites were satisfied with Brazil's role as an exporter of agricultural and mineral products and as a receiver of ideas as well as manufacturing methods developed in other places. Taking this road, republicanism spread in Brazil since the second half of the eighteenth century. (Fausto,1995). It was up to the intellectuals engaged in the republican cause to reform the State in order to meet the challenges of an economically and culturally backward nation and lead it toward a civilized model of a European nation. In this sense, the reduction and/or elimination of illiteracy was considered fundamental. It is estimated that around 65% of the Brazilian population was illiterate in the early twentieth century, very different from the Argentine situation. (Fausto & Devoto, 2004).

[1] Support: CNPq, FAPERJ, UERJ.

The political and intellectual elite tried to ameliorate education because the educational system was perceived as deficient, in the sense of being archaic, artificial and based too much on memorization and physical punishment (Gondra, 2004). In the year following the proclamation of the republic, new legislation modified the teaching system (Decreto Brazil, 1890).

This brief historical contextualization is important since it points out the specificity of the Brazilian case also in relation to discussion of the notions of centre and periphery: Brazil only takes what is known as a "peripheral" position in the nineteenth century. With the creation of the first university courses a great effort was being made to reproduce the ideas and practices developed in the centre in the most "correct" way, thus seeking to become a "civilized" nation. Psychology was also part of the "group of new ideas" that arrived to Brazil (Romero, 1926): positivism, evolutionism, materialism.

The present paper focuses on texts published in Brazil at the beginnings of the twentieth century that deal with psychological measurement. Psychological measurement was practiced initially, but not exclusively, in laboratories of experimental psychology. The great cultural, political, and economic effervescence taking place in Brazil in the 1920s turned the experimental laboratories into places of psychological measurement, with the new laboratories of the Normal Schools becoming active. Meanwhile also the Brazilian Mental Hygiene League developed an interest in psychological measuring as we will see in our review of the journal "Brazilian Mental Hygiene Archives". The New School movement shared some of the aims of the League and, therefore, was open to psychological testing. Our analysis makes clear the close relationship between the increasingly consistent presence of psychological measurement and the institutional support of the government, which led in some cases to transform mental measurement into a kind of state project.

The first phase of psychological measurement

After the first psychological laboratory had been founded by Bomfim in 1906, some other laboratories would appear, most of them in the "Normal Schools" –institutions for preparation teachers to elementary schools. (Olinto, 1944/2004).

Probably the proposal for an invitation of the psychology professor Clemente Quaglio (1872-1948) was decisive for the arrival of the Italian pedagogue Ugo Pizolli (1863-1934) to Brazil, who in 1914 spent seven months at the School of Anthropology, Pedagogy and Experimental Psychology of the Escola Normal da Praça in São Paulo (Centofanti, 2006; Antunes, 1999). He taught the use of ordinary lab instruments, and argued for the implementation of a "biographical card" in schools. In Pizolli's view the teacher needs to have some psychological knowledge which permits him to distinguish between normal and abnormal children (Centofanti, 2006). The influence of his scholarship would be noticeable. After Pizolli's brief stay, the laboratory of the Normal School of São Paulo began to operate precariously. Monarcha (1999) states that Quaglio used "various measurement methods, among which was the metric scale of intelligence of Alfred Binet and Théodore Simon; he probably was undertaking the first application of the scale in the Brazilian educational field." (Monarcha, 1999, p. 252).

Hygienism –to cure an ill nation, prevention is the best remedy...

But a strong impulse for mental testing came from the Brazilian Mental Hygiene League which started to appear in the 1920s, a decade of politico-cultural effervescence in Brazil. In contrast with the beginning of the twentieth century, in which the Republic was embryonic, in the 1920s the atmosphere was one of criticism of the existing oligarchic republican model. There were revolts, such as the lieutenants' revolt, aiming at transforming the political regime and resulting in the movement known as "Coluna Prestes" – it crossed a good part of the interior of the nation between 1925 and 1927, seeking the insurgence of the people against the existing oligarchic regime. There were cultural reform movements, such as the modernist movement and the founding of the Communist Party of Brazil (1922). There was also a moment of crisis in the coffee industry and thus a moment of economic difficulties for the nation. At this point, the Catholic Church reorganized itself and united its intellectuals around the Dom Vital Center, in 1922 (Fausto, 1995).

In this context the Brazilian Mental Hygiene League (LBHM) was founded in 1923 by Gustavo Riedl, then director of the Psychopath Colony of Engenho de Dentro and a great enthusiast of hygienism, which he got to know on his travels to the United States. In 1923 the League was considered of public service by the Chamber of Deputies and, as of the following year, it began receiving financial assistance from the government.

It united professionals from different fields, members of the educated elite of the time, mainly, physicians, jurists, educators and journalists. Also one year later, in 1924, the Brazilian Association of Education (ABE) was created interested in solving the existing social problems with the help of education. Psychological testing would increasingly appear as a great auxiliary instrument in the project of "civilizing the nation", of introducing Brazil into the lifestyle of occidental civilization (Carvalho, M. A. R., 1994; Schwarcz, L., 1993). As Campos says, we are dealing with an initiative led by a generation of dedicated intellectuals, who tried to *"reinvent the nation, organize it upon new foundations and make it quickly overcome the enormous archaic obstacles that separated it from contemporary civilization"* (Campos, 2004, p. 24). The issue here is to know the "abnormal", those who happen to be degenerated in Morel's sense and, for this reason, difficult the development of the country towards a civilized nation.

In 1925, the League began the publication of the "Brazilian Mental Hygiene Archives", a regularly published periodical and the official dissemination vehicle of the hygienist ideology. There is only one article by a psychologist, "Children's mental hygiene, based on the laws of psychology" by Waclaw Radecki, Chief of Psychology Laboratory of the Psychopath Colony. This is important because Radecki was the only one among those interested in psychology at that time in Brazil that had a formal education –academic and professional– in this new field of knowledge. The Brazilians were either solely self-taught or, at the most, had taken advantage of short training courses in European psychology centers (as Bomfim and Medeiros). It seems to be reasonable to conclude that the Radecki's hiring to the Psychopath Colony is inscribed in the relevance of the psychological knowledge to hygienism.

Despite these names and departments, it is interesting to note that the authors of the first historiographic texts of psychology in Brazil rarely mention this strong presence of "psychology" in the League. Only Olinto (1944/2004) and Lourenço Filho (1955/2004) mention it, yet in a discreet manner.

The systematic use of tests began with the League, since the tests seem to correspond to the main objective: prevention. It is necessary to know the potential, the possible abnormalities –make a diagnosis–, in order to prevent maladjustments, crime, and madness. Criticisms of the earlier use of the tests by Bomfim and Alves head in this direction, since "adaptation of the tests to the Brazilian context" was important (Leme Lopes, 1929: p.70). The "translation" they performed was not enough; an "overhaul" was necessary and "[was] being conducted by the physician Ernani Lopes," future president of the League. Other texts are studies of tests seeking their Brazilian standardization, such as those by Leme Lopes (1930, 1932) and by Lopes (1931). There is much confidence in the scientific nature of the test, as long as it is used in an appropriate technical manner, as well as its ability to predict a student's intellectual aptitudes. These articles achieve an even greater objective: to clarify the need to recognize (diagnosis) the possible abnormalities, thus favouring a hygienic culture in Brazil, as pointed out by Carvalho (Carvalho, A. M. T., 1999).

Didactic support for testing

In the same year the Brazilian Association of Education (ABE) was founded, a book was edited that we consider being crucial for the history of psychological measurement in Brazil: *"The Tests"*, by Medeiros and Albuquerque (1924). He presented a didactic support for dealing with the problems of the various types of exams in commerce and industry. He introduces the "test" concept, its purpose and its importance for standardization. He describes the history of the tests, explain how to use them, while he cites the main ones giving examples. But he also discusses on intelligence, raising the question about how to measure what is unknown?

It is not sure that test applications had been made previously but it is clear that the Binet-Simon test had already been known in several of its forms. Nevertheless, the book by Medeiros and Albuquerque facilitated the dissemination of measurement tests, introducing the concept in fields that still had not acquired the perspective of standardization.

This occurred, for example, in Bahia, where Isaías Alves (1898-1968), who was trained in Law but dedicated to the teaching profession, became the main advocate of mental tests. After reading the book by Medeiros and Albuquerque, he immediately bought the books cited in it (Alves, 1930). His various activities included the translation of the Binet-Simon-Burt scale, which he discussed in his 1926 book *Teste Individual de Inteligencia (Individual Intelligence Test)*, and the proposal of using tests and experiments from other countries. He also standardized Ballard's collective test[2], which he describes in his book *Tests and School Reorganization* (1930).

Alves explains the value not only of diagnosis but also of prognosis of the Binet-Simon test. It not only helps the school to decide about the present but indicates what the student can accomplish in the future. It becomes possible to make a prediction "as to the child's future" (Alves, 1930, p. 31). But he is already conscious about the interaction with the social situation. He adds that a child with an indicative a below average intelligence (like IQ of 85) that "his scientific or social situation will be even lower, if money or protection is not able to give him a position that he will never know how to honour" (Alves, 1930, p. 36). Alves thus seems to be correct that, despite all the praise given to science in official discourses, the cultural custom of "paying favours" (DaMatta, 1985) was prevalent. It means, in Da Matta's analyses, that the Brazilian cultural habit of giving privileges to social positions by means of criteria other than merit, thus creating a culture in which the favour (and the need to pay for it is a constant practice.

Alves goes beyond the applicability of measurement to education and extends it to delinquency: the Binet-Simon test renders a valuable service in the study of delinquents, especially juveniles, who need special treatment [...] An 11-year-old delinquent with an IQ over 90 *"indicates a sure possibility of recovery, if placed in a pure and perfectly disciplined environment,"* [...] but if placed together with other delinquents with IQs of 40-70, there is *"every possibility of aggravating his*

[2] Test created by Ballard in England, considered very economical to use because only sheets of paper and pencil and can be applied collectively. It was used in Barcelona for Mira y Lopez and adapted to Argentina by Croome, Iglesias y Forgiore (Székely, 1946).

moral disorganization, even of becoming a leader of the imbeciles and cretins that make up a large part of the criminal world" (Alves, 1930, pp. 38-39).

Alves' discourse emphasizes the close relationship between the State's interest in the need for a correct prognosis in the scholastic future of the child for the correct investing. The State should invest in the construction of a new man. He even remained linked to the State even during the dictatorship established with the New State in 1937 (Rocha, 2008).

Alves was a scientist reproducing dominant ideas of his time. In 1928 he was responsible for the testing service in public schools like Salvador (Bahia). During his stay in the U.S., when he studied with Edward L. Thorndike, he commented on the results achieved. For his work he obtained his Master of Arts and Instructor in Psychology degrees from the Columbia University Teachers College. In these comments, he explains that, dividing the students into three categories (blacks, mulatoes and whites), he observed low performance on the tests of students considered black (66.1), while the best performance were the ones of the white students (86.6). Mulatoes displayed average performance (73.6). He also verified that the average IQ of the white students rose when the scores of students from private schools were added to those of public school students (this was the case for the Ipiranga High School) (Alves, 1933, p. 18, as cited by Rocha, 2008). In relation to this point, however, Alves did not innovate but repeat Roxo's classification. This physician had proved that the reaction time of alienated blacks was longer than that of whites in his doctoral thesis (Roxo, 1900).

The new school –laic, free, public education

Although since the 1920s there had already been interaction among educators seeking the transformation of education in Brazil, whose most visible consequence was the creation of the Brazilian Education Association in 1924, it was only in 1932 that the proponents of the "New Education" launched their manifesto. Headed by Fernando Azevedo, the Manifesto also boasts other signatories of prominence, including professionals that would later become highly renowned in the psychology field.

Highlighting the necessary laic, free and obligatory nature of education, as well as its public qualities –since it is a social activity–, the foundation for using measurements is in the Manifesto's main proposal: education must cease to be a privilege resulting from the individual's socio-economic situation, and we must recognize each person's right to an education "as far as his natural aptitudes will permit" (Azevedo, 1932).

References

ALVES, I, (1926). *Teste Individual de Inteligencia* (*Individual Intelligence Test*). Salvador: Nova Graphica.

ALVES, I. (1930). *Os testes e a reorganização escolar* (*Tests and school reorganization*). Rio de Janeiro: Livraria Francisco Alves.

ALVES, I. (1933) *Teste de inteligência nas escolas*. (*Intelligence Test at schools*). Rio de Janeiro: Diretoria Geral de Instrução Pública.

ANTUNES, M. A. M. (1999). *A psicologia no Brasil – leitura histórica sobre sua constituição* (*Psychology in Brazil – historical reading about its constitution*). SP: Unimarco/Educ.

AZEVEDO, F. and others. (1932) *Manifesto dos Pioneiros da Educação Nova* (*Manifest of New Education Pioneers*).
Available at www.pedagogiaemfoco.pro.br/heb07a.htm, acessed on May.15.2010.

BRASIL. *DECRETO N. 981 - DE 8 DE NOVEMBRO DE 1890* (*LAW DECREE #981 – DATED NOVEMBER 8TH, 1890*) *Approva o Regulamento da Instrucção Primaria e Secundaria do Districto Federal* (*Approves the Regulation of Primary and Secondary Instruction at the Federal District*).

CAMPOS, R. H. F. (2004). Prefácio (Preface). IN: LOURENÇO FILHO, M. B. *Introdução ao Estudo da Escova Nova* (*Introduction to the study of New School*). Rio de Janeiro: EdUERJ.

CARVALHO, A. M. T. de (1999). Trabalho e higiene mental: processo de produção discursiva do campo no Brasil (Labor and mental hygienics: process of discourse production in the field in Brazil). *História, Ciências, Saúde – Manguinhos* (*History, Sciences, Health – Manguinhos*), VI (1), 133-56.

CARVALHO, M. A. R. (1994). *Quatro vezes cidade* (*Four times a city*). Rio de Janeiro: Sette Letras.

CENTOFANTI, R. (1982). Radecki e a Psicologia no Brasil (Radecki and Psychogy in Brazil). *Psicologia, Ciência e Profissão* (*Psychology, Science and Profession*), 3 (1), 2-50.

DA MATTA, Roberto (1985). *A Casa e a Rua* (*The home and the street*). São Paulo: Brasiliense.

FAUSTO, B. (1995). *História do Brasil* (*History of Brazil*), São Paulo, Edusp/FDE.

FAUSTO, B. & DEVOTO, F. (2004). *Brasil e Argentina: um ensaio de história comparada (1850-2002)* (*Brazil and Argentina: an essay on comparative history*). São Paulo: Ed. 34.

GONDRA, J. G. (2004). *Artes de civilizar: medicina, higiene e educação escolar na Corte Imperial* (*Civilizing arts: medicine, hygienics and school education in the Brazilian Imperial Court*). Rio de Janeiro: EdUERJ.

LEME LOPES, M. B. & Fialho, I. A. (1929) Suggestões para o emprego dos tests (Suggestions for the application of the tests). In: *Archivos Brasileiros de Hygiene Mental* (*Brazilian Archives of Mental Hygienics*), anno II, novembro 1929, #2.

LEME LOPES, M. B. (1930) Pesquisas sobre a memória de fixação (Research about memory fixation). *Archivos Brasileiros de Higiene Mental* (*Brazilian Archives of Mental Hygienics*), III (2):235-240.

LEME LOPES, M. B. (1932). A attenção concentrada explorada pelo test de cancellamento (Concentrated attention explored by cancelling test). *Archivos Brasileiros de Higiene Mental* (*Brazilian Archives of Mental Hygienics*), V (2):41-63.

LOPES, E. (1931) Subsídio para a adaptação brasileira dos tests de Binet-Terman (Subsidies for the Brazilian adaptation of Binet-Terman's tests). *Archivos Brasileiros de Higiene Mental* (*Brazilian Archives of Mental Hygienics*), IV (3): 180-207.

LOURENÇO FILHO, M. B. (2004). A Psicologia no Brasil (Psychology in Brasil). In: MITSUKO, M.A.A. *Histórias da Psicologia no Brasil* (*Histories of Psychology in Brasil*). Rio de Janeiro: EdUERJ. Originally published in 1955.

MEDEIROS E ALBUQUERQUE (1924). *Os Tests* (*The Tests*). Rio de Janeiro: Livraria Francisco Alves.

MONARCHA, C. (1999). *Escola Normal de São Paulo: o lado noturno das luzes* (*Teacher's Training School in São Paulo: the nocturnal side of lights*). Campinas: Editora da Unicamp.

OLINTO, P. (2004). A Psicologia Experimental no Brasil (Experimental Psychology in Brasil). In: MITSUKO.A. *Histórias da Psicologia no Brasil* (*Histories of Psychology in Brasil*). Rio de Janeiro: EdUERJ. Originally published in 1944.

RADECKI, W. and RADECKA, H. (1925). Pesquizas experimentaes sobre a fadiga dos menores trabalhadores nas fábricas – Nota prévia (Experimental researches on the fatigue of underage workers at factories – Previous Note). *Archivos Brasileiros de Higiene Mental* (*Brazilian Archives of Mental Hygienics*), 1 (2): 181-184.

ROCHA, A. C. S. M. (2008). Nem cola nem pistolão: Isaías Alves e os testes de inteligência nas escolas (1926-1942) (No cheating no protection: Isaías Alves and the intelligence tests at schools (1926-1942)). *Vitória da Conquista, Anais do IX Encontro Estadual de História* (*Annals of the IX State Encounter of History*) – ANPUH-BA.

ROMERO, R. (1926). Explicações Indispensáveis, prefácio aos Vários Escritos de Tobias Barreto de Menezes (Essential explanations, preface to Several Writing by Tobias Barreto de Menezes), In: Tobias BARRETO, *Obras Completas* (*Complete Works*), Sergipe, 1926, t. X, p. XXVI. [1st edition 1900].

ROXO, H. (1900). *Duração dos atos psíquicos elementares nos alienados*. (*Psychic duration of elementary acts on the alienates*). Thesis (Medical School). Rio de Janeiro.

SCHWARCZ, L. M. (1993). *O espetáculo das raças: cientistas, instituições e questão racial no Brasil – 1870-1930* (*The spectacle of races: scientists, institutions and racial issues in Brazil – 1870-1930*). São Paulo: Cia das Letras.

SZÉKELY, Béla. (1946). *Los Tests: Manual de técnicas de exploración psicológica*. (*The tests: Handbook of psychological exploration techniques*) Segunda Parte. Buenos Aires: Editorial Kapelusz.

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DE SANCTIS, BINET, AND THE INTELLIGENCE TEST IN ITALY

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Abstract

In recent years, the scientific, cultural and political French context of the first intelligence tests has been the object of systematic historical research (Carson, 2007; Foschi & Cicciola, 2006). Sante De Sanctis (1862-1935) and Alfred Binet (1857-1911), in collaboration with Théodore Simon (1873-1960), introduced their intelligence tests to the scientific community of the time at the Fifth International Congress of Psychology held in Rome in 1905. De Sanctis and his collaborators compared the two tests and used them in the training schools and asylums for children . This paper explores how intelligence testing was introduced into the Italian and, more specifically, Roman context.

In the early 20th century European context, two "experimental" psychologists, namely the Italian Sante De Sanctis (1862-1935) and the Frenchman Alfred Binet (1857-1911), understood the social value of the psychological applications and presented –in 1905 in occasion of the V International Congress of Psychology– the first intelligence tests. The metric scale of Binet and Simon's intelligence test and the Reactives of De Sanctis' test were officially presented during the V International Congress of Psychology held from the 26th to the 30th of April 1905 in Rome. Sante De Sanctis was involved in the organisation, serving as vice-secretary.

Binet and Simon (1905) both authored the paper entitled "*Méthodes nouvelles pour diagnostiquer l'idiotie, l'imbécillité et la débilité mentale*" that was included in the third section dedicated to abnormal psychology. The section was presided by Enrico Morselli (1852-1929). It is necessary to add that Binet and Simon's paper was presented by Henri-Etienne Beaunis (1830-1921). This section also included a paper by De Sanctis, entitled *Su alcuni tipi di mentalità inferiore* [About some types of mental inferiority], which contains the first version of his psychological tests (De Sanctis, 1905).

Binet and Simon's metric scale of intelligence

On October the 4th 1904 Joseph Chaumié (1849-1919), Minister of Public Education and the Fine Arts, created the inter-ministerial commission for the education of abnormal children. The government of Émile Combes (1835-1921) entrusted the presidency to Léon Bourgeois (1851-1925), a mason, lawyer and radical theorist. The commission was established to verify whether or not the laws published by Jules Ferry (1832-1893) on free education (1881), as well as secular and mandatory education (1882), had been applied to abnormal children. Bourgeois called upon Binet to become a member of the commission, which had grown so much that it had been subdivided into several sub-commissions: the first was in charge of carrying out a study on the number of “abnormals” present within the French national territory; the second sub-commission –defined as *sous commission pédagogique des anormaux*– was responsible for studying the measures necessary in order to assure that developmentally delayed and instable children were provided with the benefits of an education. This comprised an analysis of the institutional and pedagogical questions associated with “abnormal” children. Binet was one of the most active protagonists of the second sub-commission, for which he was the author of the final report on abnormal children (see Vial & Hugon, 1998).

The experience gained through the inter-ministerial commission supported the development of the first version of the intelligence test known as *Echelle Métrique de l’Intelligence*. Binet, in collaboration with Théodore Simon, worked together in developing the test. The construction of this instrument was facilitated by the pretext of responding to the social demand created by the schooling of abnormal children. Their low level of performance in normal classes suggested the need for alternative forms of education.

But what was meant by inferior states? What theory of abnormality did Binet adhere to? The author, in particular, invoked a theory of partial development as the cause of abnormality. In fact the development of the abnormal child followed irregular and partial processes that varied from individual to individual. Nevertheless it did not exclude the possibility of substituting the deficits in some sectors with others that were better developed; in consequence a “psychological” intervention intended to increase the deficient development sectors of the abnormal individual was retained to be possible.

It should be noted also that the *differential diagnostic method* developed by Binet and Simon should, in the first place, have established the *intelligence level* –a notion that in 1908 was substituted by *mental age*– of abnormal children, making it possible to place the children in the following objective and verifiable diagnostic categories: idiocy (inferior level of intelligence), imbecility (intermediate level) and weak mind (less severe and lighter level). In the second place, the method made it possible to objectively distinguish between a normal and an abnormal child.

In general, the differential diagnosis is composed of three principal methods: 1) the medical method, 2) the pedagogical method and 3) the psychological method (Binet & Simon, 1905b).

1. The *medical method*, which was very long and laborious, sought to identify intellectual inferiority by means of anatomical, physiological and pathological signs.
2. The *pedagogical method* consisted of the assessment of a subject’s intelligence on the basis of the knowledge acquired by the student in an educational setting.
3. The *psychological method*, which in practice used the Metric Scale of Intelligence, sought to determine the *abnormal* child’s level of cognitive functioning, comparing this level with what was considered normal for the majority of children, with apparently healthy development.

Binet and Simon’s conclusions held that one should consider the medical method only capable of revealing *possible* indications of mental delay. Hence it should only be used to confirm the psychological diagnosis. They also argued that the pedagogical method only managed to demonstrate *probable* signs of abnormality because: *“The intelligence of each of us must solely be judged through intellectual manifestations”* (Binet, 1910, p.17). The two authors’ propensity for the psychological method, specifically the Metric Scale of intelligence, was evident. For them it was the only method capable of objectively establishing the intellectual level of abnormal children, managing to identify *certain* signs of mental retardation on the basis of the degree of the gap in respect to a statistically constructed norm (Cicciola, 2008; see Lombardo & Pedone, 1995 for models of normality and pathology in the history of psychology).

De Sanctis’ reactives

The tests constitute a category of instruments that have for a long time characterized the psychological disciplines and, in part, continue to characterize them today. Yet very few studies exist that seek to reconstruct the origins and the development of these applied instruments. This is also the case for the historiography of these instruments in Italy. This paper will explore the tests developed by De Sanctis, and known as “De Sanctis’ Reactives”, that the author applied above all in the context of his asylum schools founded in 1899 with the aim of grading the mental insufficiency of children and adolescents.

Sante De Sanctis –who was trained at the School of Medicine of the University of Rome *La Sapienza*– was a student of Giuseppe Sergi and a post-doc student at the Salpêtrière of Paris. De Sanctis held the first professorship in experimental psychology in Rome. Historically speaking, he was clearly a leading figure at the beginning of the 1900s within the sciences applied to behaviour in the context of the renewal and the modernization of the city of Rome and the Italian state, which had recently been unified in the institutional form of a constitutional monarchy. There were two additional versions of the test developed by this Roman psychologist –supported by his pupils–presented in 1905. The second version contains only slight modifications in respect to the first and continues to be applied only in the asylum schools. The last version, dated 1914 (see De Sanctis & Bolaffi, 1914), is characterized by an increase in the number of degrees of mental insufficiency and partially includes the modifications proposed by Postowsky, a Russian scholar for which there is only a feeble trace in the current historiographical literature (Ceccarelli, 2002, p. 42; see Cimino & Lombardo, 2004).

In the 1905 version presented at the Congress, De Sanctis introduced his reactives in this manner:

> The series that I propose is applicable to all phrenasthenics, as long as they are not under the age of 7 and as long as they are calm; it is worth noting that this should not be applied if not during *periods of utmost calm and when the subject is in full health* and not when the subjects are in a bad mood, emotionally disturbed, tired or feeling negative, in a tantrum or of a similar spirit. *Choosing the moment for the application of the reactives in order to obtain from the reagent* optimal *responses*; this is the difficulty that only a capable master or an experienced doctor will know how to overcome with full success (De Sanctis, 1905, p. 586).

De Sanctis proposed the following exercises in the first version of his test:

1. Give me a ball (measure of response time);
2. Which ball have you given me? (measure of response *recognition*);
3. Do you see this piece of wood?: well then, find the pieces of wood that are the same as this one in the middle of all of the others that you see;
4. Here's a pencil for you; mark all of the equal pieces of wood that you saw earlier on this on carton (*recognition* and the indication of the figures is achieved);
5. Here are once again lots of pieces of wood of the same form as those that you just indicated on the carton; look at them carefully, then tell me *how many there are*, which of these is the *biggest* of all of them, which one is *farthest* from you;
6. (One places the diaphragm in such a way that the reagent no longer sees the pieces). And now tell me: do you think that the *largest* objects are necessarily the *heaviest*? Are the *most distant* objects really the *smallest*, or do they only appear to be smaller than the closer objects? (p. 586-587).

De Sanctis recommended this series be applied in accordance with rigorous rules aimed to indicate four degrees of mental insufficiency (no insufficiency, mild insufficiency, average insufficiency, high insufficiency).

Amongst the other things the problem of training for teachers emerged. It is not by chance that the teaching of experimental psychology was a fundamental subject in the pedagogical schools proposed by Luigi Credaro (1860-1939), a pedagogue and politician.

The 1914 version was published in *Infanzia Anormale* [Abnormal Childhood] and edited by De Sanctis and his collaborator, a teacher at the Casa di Cura e di Educazione [House of Care and Education) “Villa Amalia” in Rome, named E. Bolaffi. The book describes the three versions of the “battery” of tests developed by De Sanctis, beginning with the 1905 version. In the 1914 version the test was comprised of six sub-tests that constitute a real “operational series” of exams with an established sequence, verbal deliveries and a complete description of the material used.

De Sanctis' test, like that of Binet-Simon, consisted of tasks ordered by increasing difficulty. The substantial difference consists in the different objectives between the two tests: De Sanctis did not have the intention of measuring the normal mental age and comparing it with the pathological. De Sanctis' test measured the intellectual deficit and was capable of taking a snapshot of the difference in the level of mental capacity that could be applied to the healthy and the abnormal in order to verify whether or not there was difficulty in learning that necessitated an intervention involving psycho-pedagogic methods.

Conclusions

Psychological historiography highlighted the role of the approaches involving anthropometric and craniological measurements as precursors to later investigations with the help of mental tests.

As we have seen, 1905 can be defined as the year in which the psychological study of the individual by means of mental tests really started. Binet's test was, in concrete terms, the result of a program of craniological and anthropometric research that had led French psychology to exclude methodologies for measuring intelligence based on the correlation between body measurements and corresponding success in education (Foschi & Cicciola, 2006; Cicciola, 2008). The same Sante De Sanctis first supported and then distanced himself from a positivistic and anthropometric conceptualization that in Rome was represented by his “master” Giuseppe Sergi) (Lombardo & Cenci, 2004).

Both in the Parisian and Roman context, precise transformist conceptions of human nature were also well rooted and it is on these that Binet and De Sanctis constructed a conceptualization of “educability” with the aim of the intellectual recovery of children with mental problems as well as the poor and marginalized. In this sense the mental test in France and

Italy would have primarily come to be used in public education; mainly pursuing an educational and progressive aim. The individualization of "delayed" children was the first step for providing them with a special education that was useful for their training, in order to rebalance them with the help of a psycho-pedagogic intervention. The aim was to give these children a chance although nature had not given them equal opportunities. One notes a difference with other contexts. One is the American in which the tests were used with the aim of eugenic selection rather than to make it possible to offer children a potentially helpful intervention (Carson, 2007). Another is the Spanish, where the tests were proposed to be used for the promotion of teachers (Mülberger, 2010).

The situation in Italy was similar to the experience of the third French republic. The objective was primarily that of making primary education free, open and mandatory for all, including phrenasthenic children or those with other types of behavioural problems. At the beginning of the 1900s the use of mental tests was above all applied in a psycho-pedagogic context. Its use in psychiatric clinics was, from the beginning, above all for precision and calibration. After the first decade of the 1900s the use of mental tests in Italy underwent a sort of progressive medicalization (Lombardo, 2007). Between the 1800 and 1900s the intelligence tests were used in "special schools" (e.g., asylum schools, educators, differential classes) and then rudely entered medical clinics. From the 1920s on the tests were introduced in neuropsychiatric practice with children with the goal of diagnosing "mental retardation".

In the Italian, medical, psychological, and pedagogical cultural context that characterized the entire 20th century, the conviction was held that the test was not a *passe partout* instrument that could be applied like "a thermometer". The test was always used to complete the clinical interview with specific populations and with practical aims. Psychologists and doctors shared the conviction that the tests did not pass the test-retest reliability assessment for the average subject and that this evidence was adequate in order to promote a use of the mental test just in association with a systematic multidisciplinary clinical observation and interview (Ceccarelli, 2002).

References

BINET, A. (1910). Les signes physiques de l'intelligence chez les enfants. *Année Psychologique*, *16*, 1-30.

BINET, A., & Simon, Th. (1905). Méthodes nouvelles pour diagnostiquer l'idiotie, l'imbécillité et la débilité mentale. In S. DE SANCTIS, *Atti del Ve Congrès International de Psychologie tenuto a Roma il 26-30 avril 1905* (pp. 507-510). Roma: Forzani.

CARSON, J. (2007). *The measure of merit. Talents, intelligence and inequality in the French and American republics, 1750-1940*. Princeton: Princeton University Press.

Ceccarelli, G. (2002). *Il testing psicologico in Italia* [*Psychological testing in Italy*]. Urbino: QuattroVenti.

CICCIOLA, E. (2008). Alfred Binet e le prime "misure" dell'intelligenza (1905-1908) [Alfred Binet and the first "measures" of intelligence (1905-1908)]. *Physis, Rivista internazionale di storia della scienza*, *45*, 165-203.

CIMINO, G.P. & LOMBARDO, G.P. (2004). *Sante De Sanctis tra psicologia generale e psicologia applicata* [*Sante De Sanctis between general psychology and applied psychology*]. Milano: Franco Angeli.

DE SANCTIS, S. (1905). Su alcuni tipi di mentalità inferiorei [On some types of mental inferiorità]. In S. De Sanctis, *Atti del V Congresso Internazionale di Psicologia, 26-30 aprile 1905* (pp. 576-587). Roma: Forzani.

DE SANCTIS, S. & BOLAFFI, E. (1914). La gradazione dell'insufficienza intellettuale con il metodo dei reattivi [The gradation of intellectual insufficiency with the method of reactives], *L'Infanzia Anormale*, 7, 153-173.

LOMBARDO, G.P. & Cenci, S. (2004). La concezione differenziale di Sante De Sanctis negli studi di psicologia applicata [The differential conceptualization of Sante De Sanctis in applied psychological studies]. In G. CIMINO & G.P. LOMBARDO, *Sante De Sanctis tra psicologia generale e psicologia applicata* (pp. 159-208). Milano: Franco Angeli.

FOSCHI, R., & CICCIOLA, E. (2006). Politics and naturalism in the 20th century psychology of Alfred Binet, *History of Psychology*, 9, 267–289.

LOMBARDO, G. P. (2007). Historiographic categories in the history of Italian psychology. Sante De Sanctis between psychiatry and psychology, *Rivista di Psicologia Clinica, 2*, 132-139.

LOMBARDO, G.P. & PEDONE, G. (1995). *Normale e patologico nelle teorie della personalità* [*The normal and the pathological in theories on personality*]. Bari: Laterza.

MÜLBERGER, A. (2010). The role of psychological measurement in renewing pedagogy in Spain (1900-1920). *STEP Meeting in Galway*. Unpublished manuscript

VIAL, M., & HUGON, M.-A. (1998). La commission Bourgeois (1904-1905). Documents pour l'histoire de l'éducation spécialisée. Vanves: CTNERHI.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 653.

HISTORY OF PSYCHOLOGICAL MEASUREMENT. RUSSIAN CONTEXT IN THE 1920S-30S

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Abstract

The rising of the experimental research at the turn of the 19th century is mostly focused on new objective methods and the same interest is shared by scholars in Russia as in Europe and USA. The historical period we evoke begins when science has just discovered the child as a major object of study. Medicine, law, and then all the young human sciences seize it (Ottavi, 2001) to build knowledge about human development. At the time where boundaries between sciences were not clearly established, all of them were on the way to investigate in the same new field: the child and his development. However, specificities in the development of psychological and neurological disciplines will appear in Russia with the First World War and then, will increase with the huge social changes induced by the October Revolution in 1917. If the history of psychological measurement concerns also adults, in our approach, we focus deliberately on the psychological tests applied to children and closely connected, like elsewhere, to the question of education. The Soviet psychological measurement is mainly linked to pedology and its singular fate (Fradkin, 1990; Etkind, 1992, 1997). Pedology, also called "child study", supposed to be the solution to solve the numerous problems of education in the new socialist society, was also the first science to be officially forbidden in Soviet Union in 1936. Psychological measurement was indeed massively applied to children in various circumstances which we are going to describe further through samples collected in the journal Pedologija, the official organ of Soviet pedology from 1928 to 1932. Lev Vygotskij's point of view on the matter will contrast this historical context.

A new science for a new society

The educational stakes in the 1920s in Soviet Union were of a rarely equaled large scale. After seven years of international conflicts, revolution, civil war and famine (1914-1921), one priority was the reintegration into society of hundreds of thousands of children and teenagers left on their own (*bezprizorniki*), by these sequential events, in a country itself disintegrated, weakened and in search of marks. A project of fast and radical reconstruction had to be started, compatible with the ambitions of the young socialist nation. Pedology served the socio-political plan with expectations of an efficient education policy. In quest of solutions, it afforded this science a direct experiencing to the creation of a "New Man", linking theoretical research on child development to practice in the educational system. Authorities expected from pedology a fast progress to design a child of a new type according to their ideology. A new science calling for new practices, pedology was probably the discipline that used "objective" psychical and physical measurement on such a large scale. The laboratory for experiments took the size of the nation. The revolutionary situation, the strong social needs for knowledge on the child and perhaps also an overt context for a new discipline are the original components of the educative policy in USSR, during the 1920s. After 1917, all sciences in Soviet Union are reconsidered on the same bases: an objective materialistic dialectical science, fighting against the idealistic, mechanistic roots of traditional science. Education for the new nation must necessarily be distinguished from the tsarist one and from the system in place in capitalistic countries.

A controversial issue

This experimental and pedological adventure in Russia starts mostly with the personalities of Vladimir Behterev (1857-1927) and Alexandre Nečajev (1870-1948) in Petersburg, Gregori Rossolimo (1860-1928) and Mihajil Bernstein (1894-1975) in Moscow. The central place was given to explore qualitatively and quantitatively the child potential. One of the main reasons for this decision, and it is still an interpretation nowadays, was the abusive way the pedologists used the testing. Before the First World War, experimental pedagogy and experimental psychology used to work very closely together and in connection with Western scholars. According to Helene Antipoff, Claparède's Russian assistant in Geneva, before Nečajeff, pedagogical psychology (or experimental pedagogy) was a "terra incognita" in Russia, considered like a ridiculous and harmful matter for Russia. For him, a drastic reform in schools was necessary in connection with experimental psychology. The reception of his first book[1] (1901) was much criticized having connected pedagogy with psychology.

An objective science for Soviet pedology

The independent way taken by Soviet pedology became an originality whereas elsewhere, it was incorporated in other sciences. Russian pedologists agreed on the fact that the science of the child development was an objective science as well as biology, thus rejecting the empirical orientation of the first pedologists, in favor of a subjective, introspective science or in the line of Wundt's experimental psychology. Psychological measurement became a trend for child testing at school to evaluate their intellectual abilities, but also often used to compare children according to their cultural environment and family upbringing. Some of the leading pedologists of the early soviet period like Pavel Blonskij (1884-1941) or Mihajil Basov (1892-1931) expressed both their enthusiasm and fascination for testing methods. Blonskij, in the preface of the 1928 book *Tests: Theory and practice*, envisaged a promising future to the method. He claimed that "Tests are more than a method of control; they are an instrument of rationalization of school". Basov in the same book gave his enthusiastic opinion on the matter: "I think the sometimes sharp criticism of the testing method will bring finally not to the reversal, not towards the abolition of this method, but on the contrary, towards its consolidation and its assertion in the defined borders, where, obviously, its legitimacy and the reason to be applied will be proved"[2].

Russian contribution to psychological measurement

If Binet's tests were very often used by Russian scholars, Rossolimo, mostly known as a neuropathologist created a method of psychological profiles compared at that time worldwide with Binet's method. His test was also used abroad. He conducted his main research on children with psychical diseases and abnormal behavior, studying age specificities in child behavior. In 1911 he opened in Moscow the first institute of child psychology and neurology working on nervous diseases. He determined a direct correlation between behavior and the development of psychical functions. His profiles' method was mainly a quantitative measurement of the various psychical functions of personality. He itemized 22 functions. The results of the test were put on a graphic, representing a curve of a given profile of personality. However, Soviet pedology was not a homogeneous science, and had difficulties to move from a dualistic point of view. For the two main tendencies which were confronted in their theses, the dichotomy between biogenetical and socio-genetical foundations was obvious. One overevaluated the influence of heredity on development of child, whereas the other neglected it by considering that only the transformation of the social environment allows a radical modification of the development of the child, by adaptation. The socio-genetic orientation was supported by the authorities' ideological point of view. Vygotskij's historico-cultural orientation was very marginal, at that time.

[1] *La psychologie expérimentale en rapport avec l'enseignement scolaire*. (1ère éd. I vol. ; 2ème édit. En 2 vol. 1907).

[2] Our translation.

Experimentation and testing through the journal *Pedologija*

This journal was born in 1928, after the first panrussian congress of pedology, with the support of some major politicians involved in education policy, like Nadežda Krupskaja and Anatoli Lunačarskij. *Pedologija* was published until 1932. The description of the contents of *Pedologija* is not exhaustive in the modalities granted for this article, because the whole magazine over five years counts several thousand pages. Chronicles and thematic directory of *Pedologija* constitute a considerable source of precious empirical and statistical information and allow showing concretely what was measured in researches about child. Many empirical researches were of quantitative nature, founded on the analysis of statistical data. Numbers of them were based on questionnaires, Binet's tests and Rossolimo's profiles. The anthropometrical and psychometrical tests were frequently applied, and reflected the biogenetic and socio-genetic points of view. The examples bellow were chosen to illustrate the different types of studies elaborated during this short period in pedology.

Ethnographical type study

The multiethnic dimension of USSR opened an emancipation policy for the minorities. It was developed during the 1920 idealistic period, followed by a realistic bureaucratic control after 1930[3]. In 1922 Russia became the Union of the Socialist Republics, composed by 15 republics and some autonomous territories, the *kraïs*. The Soviet policy on nationalities in the 20s and the beginning of the 30s, was placed under the sign of the *korenizatsija*[4], a short period during which the new political regime tried to reverse the effects of the Russianization undertaken during the Tsarist period on non Russian populations. After 1930, Stalin intensified the Russianization of the minorities, and tried to impose a single culture[5]. In 1930, many scientific expeditions were sent to the Caucasus, Central Asia, Siberia, and Mongolia. The field of scientific research was not limited to ethnography, and opened a field to pedologists in connection with educational psychology. *Pedologija* published a special issue almost entirely devoted to the first research results mainly about women and children of national minorities, (1930, 2). In each case, a Binet-Simon test was applied, sometimes associated to the Rossolimo's psychological profiles test. Anthropometric measurements were also applied to a broad scale with children of the various Republics. Comparative studies with the Russian children were carried out.

Biogenetical type study

S. Kacenelson's and V. S. Brodovskaja's study entitled "The characteristics of the children in the first degree in work school" (*Pedologija*, 1930, 3, 350-368), represents an example of the biogenetic stream of pedology. This is a medico pedological type research, bearing on 1,045 children admitted in the first group of 12 elementary schools (504 boys and 541 girls). The age of the subjects varied from 7 to 10 years. The authors took account of the children social situation; hereditary influences; their physical development; their health condition; their psycho-neurological status (giddiness, speech difficulties, knee jerk, pupil reflex, etc.). The mental age was measured according to Rossolimo's reduced scale. They carried out a compilation of statistics starting from the results of their observations, and drew some of the following conclusions: as the children who enter to the school represent a heterogeneous mass from the biological and social point of view, the composition of the first groups of the elementary school must be only based on one preliminary pedological investigation. In addition, one should not make compulsory teaching starting from seven years of age; however, the seven year old children wishing to attend school could be allowed. For slower children, it was mentioned as essential to create special groups with an individualized teaching.

Sociogenetical type study: Social organization abilities

S. Zalužnyj study (*Pedologija*. 1929 4, 478-489) on child collectives is an example of a sociogenetical type research. Carried out in Ukraine, in 212 elementary and secondary schools, it consisted of testing the class social organization. Each class had to fulfill different tasks successively, such as to elect a delegate for the municipal school conference, to organize quickly small teams for collective works or menage a project of a school festival. According to an official report taken by teachers, marks were attributed to the results on a scale of 5 degrees, the results were showing the progress from class to class: The class, the age, the number of groups tested were the base of an average of marks, an average of variable and an average of time: The coherence of the tests was evaluated by correlation of par and odd tests with the application of the Spearman-Brown's formula. Which deductions can one draw from these tests? The author affirmed that the results gave an account of the children's problems of organization and discipline between them. The children found difficulties for planning

[3] For example, the obligation for all national minorities to put down their origin on their identity papers.

[4] literally: 'rooting'.

[5] One school, one language.

their work by group; groups were usually formed by genders. The author didn't give any precise result, and remained cautious by concluding that it would be necessary to continue the research to study the problem of the disturbing elements.

Vygotskij's specific point of view

Vygotskij is not very known as a pedologist, however the child development became his main topic after 1927. He didn't share his colleagues' enthusiastic point of view on quantitative methods.

Vygotskij, in several texts, expressed clearly that psychology and its methods didn't satisfy him. In the last years of his life, through pedology, he developed a general science where the theory can't be separate from practice. It is in practice that theory could find a validation. In the *Significance of the crisis in psychology* (1927), Vygotskij's diagnosis was explicit: between an idealistic approach following a subjective way, and a mechanistic approach reducing the psychological phenomena to a biological fact, the question was to find a monist and dialectical way, which takes account of the material base of the psychical processes without reducing them to simple biological processes. One major element of the crisis was the difficulty to articulate theory and practice.

In his text on "Pedology and close sciences" (1931), Vygotskij[6] analyzed the reason of this stillborn discipline in Western countries. In Vygotskij's opinion:

> "Pedology should be based on the objective reality of the unique process of development which is its object. It can't be build on the field of the metaphysical, logico-formal point of view of child development which only allows a mechanical association of the different aspects of development, nor on the field of a dualistic point of view of human nature, closing the way to the study of the real unity represented by the process of child development. It is exactly for this reason that pedology as a particular science is almost dead in the West and in America".

If we follow Vygotskij's reasoning, here, the essential basic unit of pedology is the child, composed of multiple internal systems, in his relationships in a particular environment. By taking the child as a whole, according to Vygotskij, it allows a decomposition per unit, by keeping the characteristics specific to the child which are as well internal (hereditary) and external (social milieu), to reach at last the particular complex phenomena which compose them. The starting point cannot be the understanding of isolated facts. For Vygotskij, it was necessary to start from a synthesis to understand its multiple aspects, and he called in question the validity of the interpretation of results obtained by quantitative methods:

> "The element taken separately does not have a value for analysis of a pedological type. For example, to separate hydrogen and oxygen from a molecule, makes lose the characteristics of the unit. Consequently, the core is not the element, but the unit, i.e. the relation of a factor of the milieu with the characteristics of the child. If we find such a unit, it will preserve in it what is present in the development of the language as a whole, i.e. the relationship between factors of the milieu, individual factors, and factors rooted in the characteristics of the child" (1933/34 1996, p. 45).

Vygotskij argued that by the will to show relations between everything, one doesn't explain anyyhing at all anymore. He expressed his mind very clearly:

> "But finally, looking for general links instead of a clear picture, it is an absurd confusion which appeared. All was related to all: memory with capacity, intellectual development with the acuity of sensory functions, growth, understanding, the way of eating, and the success in arithmetic" (p. 58).

For the five-year scientific plan, Vygotskij wrote in 1929 a report on the detailed program planned for research concerning national minorities and preparing with Luria an expedition in Central Asia (1931-1932). In this article, Vygotskij was clearly guarded about the use of tests and their compatibility with cultural diversity. They would owe, according to him, being adapted to the specific context of each country, according to its formal and informal system of education. In his opinion, tests in themselves cannot be objective indicators of the intellectual development. He claimed that the standard tests used in pedology do not have a universal value and can sidestep the issue of a research. He conceived the development of the child in an ecological and cultural context, and recommended a field method to study it. The point was to understand "the child as an integral part and natural product of his specific milieu, in which he lives and develops" (p. 370). It is by taking into account these factors that it will be possible to find some universal characteristics of children development. By considering the child cultural environment, his aim is to evaluate the effects of a general system of education[7] on the development. Luria's expeditions in Uzbekistan to which Vygotskij did not take part for health reasons, aimed to validate this hypothesis of the schooling effects on the higher mental functions (Van der Veer and Valsiner, 1991). The assumption was that a

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[6] All Vygotskij's quotations are our translation.

[7] Unified in USSR for ten years (1919-1929).

qualitative change was to be produced by education, and to be observable in contexts where the development of the higher mental functions were worked out by other cultural means and led to different results. The first study was based on the observation of adults, the second included children. According to Luria[8], that field research was carried out by his 14 collaborators team studying 11 main topics, out of 600 people: 1. thought as a function subjected to historical changes (and namely, the process of word usage, deduction, comprehension of metaphors and symbols, logical thoughts, etc.) 2. the structure of individual psychological process (in particular, perceptions: the perceptions of form and color in connection with visual thought, optical geometrical illusions, drawings, the features of remembering, counting and so forth).

For an estimation of child mental development, Vygotskij chose a qualitative method: the instrumental method he was working on through the historico-cultural theory. We can call it instrumental method, says Vygotskij (1928), "as it is based on the discovery of the instrumental functions of the cultural signs of the behavior and their development, based in experiments on double stimulation" (p. 75). "This method is not only the key to understand the higher forms which appear in the development process of the behavior of the child, but it is also an acquisition for the practice, education and secondary education."

The criticisms of pedology 1932-1936

Pedology was the first science whose destiny was determined by a political resolution (4th of July 1936 Decree), proclaimed illegal and considered as a "pseudo science" without any scientific debate. Books censured, authors persecuted. Some contradictory opinions about the pedologist's role were spread around: a great popularity in schools, in their work in cooperation with teachers, to solve problems of learning (Fradkin, 1990), or just at the opposite manipulation, ideology, with no connection with reality (Berelowitch, 1990; Ewing, 2001). The problem with testing can be explained by the incompatibility between stalinian ideology and the tests revealing some individual differences that could be explained only by a heterogeneous social context, and the socialistic environment was supposed to be homogeneous, without different social classes anymore. Fradkin (1990, p. 204) confirmed this thesis and underlined the virulence of the attacks against pedology, which is explained by the incompatibility of all the scientific approaches whose object was the child development. The tests will be forbidden until 1970, (Avanesov, 2000), and pedology has had no official rehabilitation until now.

Conclusion

After 1936, Soviet Human sciences were extremely reduced. Soviet psychology came back on the international stage with the figures of Vygotskij, Luria and Leontev, about 30 years after Vygotskij's death in 1934. Etkind (1992) was concerned with the effects on a population, when a science is forbidden for such a long time. But we can ask the opposite question. Did Psychology and Human Sciences develop since the period we spoke about in countries where they had the opportunity to progress on child development as a support for a better education with quantitative methods?

References

ANTIPOFF, H. (1913). Nos amis: M. Alexandre Netchajeff. *L'Intermédiaire des Educateurs 11*, 9-13.

AVANESOV, V. (2000). *http://oren-test.narod.ru/history.htm*. Consulted: 11.01.11.

BASOV, M. (1928). Testy: teorija i praktika. [Tests: theory and practice]. *Sbornik*. Nº1. M: Rabotnik Prosveštenija.

BERELOWITCH, W. (1990). *La soviétisation de l'école russe 1917-1931*. Lausanne: L'Age d'Homme.

BLONSKIJ, P. (1928). Testy: teorija i praktika. [Tests: theory and practice]. Predislovije. *Sbornik*. Nº1. M: Rabotnik Prosveštenija.

ETKIND A. (1992). L'essor et l'échec du mouvement païdologique. De la psychanalyse au nouvel homme de masse. *Cahiers du Monde russe et soviétique*. *XXXIII* (4), 387-418.

ETKIND, A. (1997). *Eros of the impossible*. Colorado: Westview Press.

EWING, E. T. (2001). Restoring Teachers to Their Rights: Soviet Education and the 1936 Denunciation of Pedology. *History of Education Quarterly*. 41 (4), 471-493.

[8] Quoted in Vygotskij, L.S., Luria, A. R. (1930/1993). *Studies on the history of behavior: ape primitive, and child*. Hillsdale: Laurence Erlbaum Associates, p. 15.

FRADKIN, F. A. (1990). *A search in Pedagogics*. Moscow: Progress Publishers.

KACENELSON, S., BRODOVSKAJA, V.S. 1930 Karakteristika detej postupajuŝih v pervuju grupu trudovoj školy. [The characteristic of children starting the first degree of work school] *Pedologija, 4*. 350-368.

OTTAVI, D. (2001). *De Darwin à Piaget. Pour une histoire de la psychologie de l'enfant*. Paris: CNRS.

ROSSOLIMO, G. I. (1917) *Psychological profiles*. (2nd ed.) Moscow.

ROSSOLIMO, G. (1922/1929). *L'individualité de l'enfant*. Paris: Félix Alcan.

ZALUŽNIJ, A.S. (1929). Organizovanost' škol'nyh grup. [Organisation in school groups]. *Pedologija, 4*. 478-489).

VAN DER VEER, R., VALSINER, J. (1991). *Understanding Vygotski. A quest for synthesis*. Oxford: Blackwell.

VYGOTSKIJ, L.S., LURIA, A. R. (1930/1993). *Studies on the history of behavior: ape primitive, and child*. Hillsdale: Laurence Erlbaum Associates.

VYGOTSKIJ, L. S. (1927/1999). *La signification historique de la crise de la psychologie*. Neuchâtel et Paris: Delachaux et Niestlé.

VYGOTSKIJ, L. S. (1928). Problema kul'turnogo razvitija rebenka. [The problem of cultural development of the child.] *Pedologija, 1*, 58-77.

VYGOTSKIJ, L.S. (1929). K voprosu o plane naučno isledovatel'noj raboty po pedologii nacionalnih menševst. [The question of a work plan of scientific research on national minorities in pedology]. *Pedologija, 3 (5),* 367-377.

VYGOTSKIJ, L.S. (1931). K voprosu o pedologii i smežnyh s neju naukah. [Pedology and close sciences]. *Pedologija, 3 (15)*, 52-58.

VYGOTSKIJ, L. S. (1933/34 1996). *Lekci po pedologii*. [Lectures of pedology]. Ijevsk: University of Oudmourtie.

SCIENTIFIC AND TECHNOLOGICAL EVOLUTION OF THE GAS INDUSTRY

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THE ADOPTION OF NEW TECHNOLOGIES BY GASWORKS IN SPAIN

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Abstract

The technology of gas production for public lighting and domestic and industrial uses has become obsolete. The replacement of coal gas by natural gas resulted in the abandonment of distillation and physical and chemical depuration technologies in favour of extraction and distribution processes.
The production of coal gas contains the germ of renewal because of the need for frequent changes due to the deterioration of retorts. As a result, iron retorts were replaced by ceramic ones. Their position changed from being horizontal, tilted, and finally vertical. In order to boost production, the retorts were enlarged before being replaced by chambers. Another change in coal gas production concerns heating, i.e. by coke or tar combustion or by gas producer. Subsequently, new methods were introduced in order to obtain town gas by means of mixing blue water gas and carburetted water gas with coal gas. The decade of the 1960s marked a trend towards a new technology of gas production: cracked gas. This process replaced coal by oil as a raw material, giving rise to a cleaner and more chemical system of production.
This paper provides a preliminary approach to the evolution of technologies of gas production at fifty gas works in Spain. To this end, an attempt is made to gather dispersed and heterogeneous information in order to shed light on the evolution of technological change in the cities where this took place.

In August 1842 Barcelona became the first town in Spain to be lit by gas. After Barcelona, gasworks were built in other towns. In 1861 there were 29 factories in operation. Statistics concerning manufactured gas in Spain first became available in 1901. In order to establish the technical characteristics of these factories and their importance in industry, we have analysed the statistical data published between 1901 and 1933 by the Ministry of Finance, and we have reproduced the same analysis using the data for the period 1940-1960. In the latter period, gasworks in Spain may be divided into six groups according to the volume of production. We can observe that there are considerable differences in evolution, i.e. there is a marked decrease in the number of factories and a significant rise in the production of gasworks, especially in Barcelona and Madrid.

This paper is structured around two sections. In the first one we have analyzed what was the evolution of gas technology based on the distillation of coal and we focused mainly on the production phase, i.e. in the ovens. Secondly, we have analyzed the gasworks established in Spain in order to identify which technology had been applied and how this process had happened.

The technology of gas production

The most utilized system of gas production consisted in distilling coal in retorts by heat. The gas obtained was composed of hydrocarbons necessary for lighting, and also of unwanted substances such as hydrogen sulphide, ammonia vapour, water steam and tar.

Distillation in the retort produced coke as a by-product. The gas obtained was purified by condensation, washing and chemical processes before storage in gasholders and distribution to consumers.

The technology of gas production may be divided into six phases: 1) Directly-fired ovens; 2) Recuperative ovens; 3) Ovens of intermittent carbonization: 4) Ovens of continuous carbonization; 5) Water gas production; 6) Cracked gas production.

In this paper, we focus on the first five phases up to the year 1960. All the methods used, in principle, the same technology based on distilling coal gas or similar products.

The first retorts were cylindrical made of cast iron 1.80 m in length and 30 cm in width. They only lasted 8-10 months, after which it was necessary to replace them. In 1828 a patent was taken out on a refractory clay retort that was cheaper and lasted 2-3 years. This retort was subsequently modified by constructing one large retort using refractory bricks. However, the most utilized oven was the one of 3, 5, 7 or 9 ceramic retorts between 2.10 m and 2.70 m long, with a D shaped section, 40 and 55 cm wide, and directly fired by coke combustion.

Coke was the fuel used to achieve the high temperatures required in the distillation of coal. However, in some cases tar was employed. George Anderson designed an oven for using tar and coke. Subsequently, injectors were also employed. The Siemens brothers designed an oven heated by gas generated in a gas producer in which the lean gas was obtained by subjecting the residual coke to a high temperature air stream. The heat produced in the gas producer and in the oven was recovered in a recuperator or a regenerator.

The incorporation of gas producers to heat the retorts was undertaken in two ways. One way consisted in taking advantage of the replacement of the retorts in a directly-fired oven to use the combustion chamber in which to install a gas producer. This was the case of the Parsy oven. The other way involved implementing new technology such as Lachomette ovens. The Parsy oven had a gas producer and a recuperator placed in the small space that was previously used as a combustion chamber. For this reason, the gas producer had to be horizontal and the recuperator small.

The gasworks at Dresden used the Hasse-Didier oven, in which the gas producer was outside the oven and in which the primary air was heated by double walls that contained the ducts. The other model was the Lachomette oven, which was patented in 1889. This oven consisted of a gas producer made of refractory material with a grill placed perpendicular to the gas outlet.

In 1909, the use of tar in a gas producer was endorsed because of its low cost. Hovine designed an oven, similar to the Lachomette oven, equipped with a device that allowed the use of tar. Subsequently, this oven incorporated a tar injector. One example of this was the Echinard oven.

In the last decade of the nineteenth century and in the first decade of the twentieth century three new methods of coal distillation were introduced to meet the challenge of electricity: 1) Ovens of intermittent carbonization in vertical retorts, 2) Ovens of continuous carbonization in vertical retorts, 3) Ovens of inclined chambers.

Traditional distillation ovens were included in the group of ovens of intermittent carbonization. Nevertheless in 1885, in the Compagnie de Gaz de Reims, the idea of fitting 6 m long retorts inclined between 32º and 35º in an oven was conceived in order to facilitate the descent of coal under its own weight.

In 1905, ovens of vertical retorts 5m high were built in Berlin. The oven distilled coal for 10 or 12 consecutive hours. The charging and discharging were carried out mechanically. One example of ovens of intermittent carbonization was the Klönne oven, which was equipped with between 3 and 6 vertical chambers.

In 1907, Dr. Reis in Munich built ovens of inclined chambers that were commercialized by the Klönne Company. These ovens resembled those built in the United States to produce coke, the residual gas being used for lighting. Another type of oven that had horizontal chambers was manufactured by the Koppers and Klönne companies. These ovens consisted of parallelepiped chambers and occupied less space than the inclined chambers of their rivals.

To accomplish continuous carbonization, coal had to be introduced and coke extracted without interrupting the process. In 1903, Harold Woodall and Arthur McDougall Duckham tested continuous carbonization, but the industrial application was not implemented until the end of 1907 owing to the difficulty of obtaining the high temperature necessary for distillation.

In 1908, William Young, John Glover and Samuel West devised in Manchester another oven of continuous carbonization. This model had four retorts about 6 m long in square sections with rounded angles, narrower at the top than at the bottom. The main feature of this oven was that it was heated by ten horizontal ducts (placed one on top of the other) that crossed the retorts transversally.

If water vapour passed over incandescent coal a gas consisting mainly of hydrogen and carbon monoxide was produced. This gas came to be known as water gas. The reaction was endothermic, i.e. heat is absorbed leading to a decrease in temperature, which terminates the reaction. Therefore, water gas producers worked in two stages. One stage, known as blow down, consisted in introducing an air current through the coke to produce blow gas to increase the temperature. The other stage, known as gasification, consisted in passing the vapour through the coke to produce water gas.

There were two kinds of water gas producers: one involved recovering the heat to increase both the air and the steam. The prototype of this producer was the Tessie of Mottay model and was used to produce water gas without carburetting but it could also be subsequently carburetted. The German model Dellwik-Fleischer was based on this type.

The other type involved the use of heat to decompose oils in order to produce carburetted water gas directly. The model was the Lowe producer, on which the Anglo-American Humphrey and Glasgow producer was based. There were other models that required regenerators like the Krammers & Aats from Germany, which produced complete combustion or the Strache from Austria, which resulted in incomplete combustion. The Dellwik-Fleischer producer had a dual circulation of vapour in both directions (up and down) that favoured gas production. The blow air entered from below and was driven by a fan. The Humphrey and Glasgow producer consisted of three elements: a gas generator, a carburettor and a booster heater. During the blow phase, the gas produced has a high of carbon monoxide because some of the air returned to the producer to burn the parts that had not undergone combustion. In the gasification stage blue gas passed through the carburettor under a shower of oil. Other models of water gas producers were Tully from England and Rincker & Wolter from the Netherlands. These models were characterized by the decomposition of the oil that was cast directly over the incandescent coke. Water gas was first manufactured in the USA, where it accounted for two thirds of the gas consumption. However, in Europe its toxicity and its lack of smell were used as arguments by the proponents of coal-gas to prevent its production.

The gas industry in Spain

The evolution of the gas industry in Spain may be divided into five periods from its beginnings in 1842 to the 1960s, when coal gas production was replaced by cracking: 1) Early period (1842-1860). 2) Period of expansion (1861-1890). 3) Period of awareness of the need for technological change (1891-1921). 4) Period of consolidation of technological change (1921-1938). 5) Period of autarchy (1939-1960).

In April 1846, the gasworks in Barcelona was supplied with 10 ovens, two of which were equipped with 5 cast-iron retorts, and 8 ovens of only one retort made of refractory bricks. We suspect that a small technological change was taking place in this gas plant, i.e. iron retorts were being replaced by ceramic retorts. Subsequently, other gasworks in Valencia (1844), Cadiz (1846), Santander (1853), Alicante (1854), Reus (1855) and Valladolid (1858) manufactured coal gas in directly-fired ovens. This demonstrates that the technology using directly-fired ovens of 3 or 5 horizontal ceramic retorts had been adopted in Spain.

The case of the gasworks in Figueres should be noted because the gas was produced not from coal but from fat obtained from washing wool. The manufacture of this gas was known as the composite waste gas system, which corresponded to a patent granted in 1858 to a Frenchman, Alfons Humbert, and to Antonio Escubós from Barcelona. In the explanation of the procedure, the authors made it clear that the patent involved only the production of fatty substances to replace coal but did not involve changes to the equipment used in coal gas production. In 1880 the composite waste gas system was replaced by a new plant outside Figueres using the traditional method. This system, which was patented by Humbert, recalls the method subsequently devised by Enric Hirzel in Leipzig in 1863. Hirzel invented a simple and economical method to separate fat from soapy water when washing wool (which is termed Swint) and began to distil this product. The Humbert method was also adopted in Manlleu and Vic for 200 gas lights but was eventually replaced by coal gas manufactured in directly-fired ovens.

Humbert gas was not the only unconventional method of gas production. There were two other cases: Arbós gas and rich gas. The former was attributed to Jaume Arbós (1824-1882) chemist and priest. Between 1852 and 1867, Arbós took out five patents on the manufacture of gas and on a suction gas producer applied to engines.

Jaume Arbós obtained gas from a producer without retorts using charcoal and other carbonaceous substances.. The Arbós gas was implemented in Vilafranca del Penedes and in Badalona in 1864.

Other cities opted for systems that distilled mineral oils or hydrocarbons. This was the case of Blanes in 1881 where it was installed a Rieber and Gruner device to produce rich gas. This gas was, like that produced by Humbert, a type of oil gas obtained in this case not from fat but from the decomposition of hydrocarbon liquids and shale oils subjected to heat. The town of Palafrugell followed the example of Blanes in 1882. Another factory in El Ferrol using the same Rieber and Gruner method as in Blanes was set up.

We call the second period, the period of expansion because the number of gasworks in Spain underwent a fourfold increase. Most of these gasworks continued to produce gas by directly-fired ovens as in Cordoba, Malaga, Murcia, Premià de Mar, Sant Feliu de Guíxols, Terrassa, Tortosa, Valls, Vilanova, etc. However, there were some gasworks that owing to the high consumption took advantage of the need for retort replacement by implementing more advanced systems that were more cost effective. This was the case of Cadiz and Barcelona. In Cadiz, Klönne installed a new factory with ovens of eight retorts in 1885. In Barcelona, some ovens of inclined retorts of the Coze system were implemented in 1890, for the fist time in Spain, at the gasworks in Barceloneta, which increased production and reduced labour costs.

The fierce competition between gas and electricity led to technological change in the gas industry. Some gasworks built recuperative ovens. This was the case of Mahon, Reus, Sant Feliu de Guíxols and Vilafranca del Penedès.

The largest factories, most of which were owned by the Compagnie Centrale d'Eclairage pour le Gaz Lebon, opted for Lachomette ovens, which were more suited to large gasworks. However, French technology implemented by the Compagnie Lebon was not adopted by Catalana de Gas, which preferred Bueb type vertical retort ovens from the German company Didier. In San Sebastian, where the company had belonged to the City Council since 1893, the Otto ovens, which were based on coke ovens, were preferred.

Reliance on foreign technology did not seem to be the best solution for small gas plants. One example is the gasworks at Valls. In this town, there was a directly-fired oven of 6 retorts. In 1911, Pablo Yvern, son of the owner, incorporated a gas producer and a recuperator designed and patented by himself. This new system had important repercussions for other small factories in Spain. Pablo Yvern Ballester (1879-1950?) had studied Industrial Engineering at the School of Barcelona. In 1901, he went to England to work as an apprentice at the Gas Light & Coke Company of London and to attend some classes for engineers. On his return to Spain, as manager of his factories, Pablo Yvern took out a patent on "Improvements of the heat recuperative ovens heated by gas". He installed his recuperative ovens at the factory in Valls and subsequently at other gasworks on the Mediterranean coast.

In 1938, during the Civil War, the Unified Gas Service of Catalonia (SGUC) made an inventory of gasworks in Catalonia. There were 25 gasworks of which only one, the one in Tortosa, was closed. Of these, there were only 8 that had directly-fired ovens. Twelve gasworks were equipped with recuperative ovens, 8 of which were of the Yvern type while the others used foreign technologies. This bears testimony to the significance of the Yvern recuperative. Lachomette ovens continued to be used at the Lebon factories although the factories of Gràcia, Arenal and Sant Martí had been taken over by Catalana de Gas in 1923. Nevertheless, the technical innovations introduced in these period concerned ovens of continuous carbonization. In Valencia after WWI the facilities were replaced by 5 Woodall-Duckham ovens, which used coal from Asturias. This technology was not restricted to large gasworks as shown by the factory in Reus.

After the Civil War, the ovens of continuous carbonization did not live up to expectations. The West Glover ovens in Reus did not work as expected. More noteworthy was the case of Barcelona, where these continuous ovens had never worked as continuous ovens but always as intermittent ovens. Consequently, in 1942, the Dr. Otto & Co. agreed to replace them, without charge, by other ovens of 5 chambers of intermittent carbonization. However, they were not implemented until 1947 because of WWII. The gasworks in Madrid used Koppers ovens, which were less common in Spain. These ovens were of intermittent carbonization and consisted of horizontal chambers. Some companies opted for less common ovens such as the Parsy oven, which was adopted in Calella, Girona and Vigo or the Hovine oven, which was installed in Jerez de la Frontera. There were few changes in technology between 1943 and 1960. In Granada Lachomette ovens were replaced by Collin ovens.

In the early decades of the 20th century, water gas was developed to be mixed with coal gas. Spanish public opinion in 1874 was not favourable to water gas given the controversy and restrictive decrees in England and France. However, this unfavourable opinion changed at the beginning of the 20th century. In 1902, in Gijón (in northern Spain) it produced water gas in a Dellwitch Fleischer gas producer to mix water gas and 30% coal gas for lighting. In 1908 a Kramers and Aats water gas producer were working in a factory in Cádiz. Nevertheless, the production of water gas took off in Spain in the 1930s and the Dellwitch Fleischer gas producer was the preferred model. This model was implemented at the El Arenal gasworks in Barcelona and subsequently in Sabadell. The Humphrey and Glasgow model was installed in Madrid, while the Kramers and Aats producer was adopted in Malaga. An Otto-Pintsch carburetted water gas producer was installed at the gasworks in Barceloneta in 1934 to complement the Dr. Otto ovens.

The Tully Gas Plant system was adopted in Córdoba in 1920 to produce gas using anthracite. This system, which was known as total gasification, was nothing more than a system that produced carburetted water gas. It was therefore installed at the factory in Reus in 1947 and subsequently in 1960 at the gasworks in Palma de Mallorca and A Coruña. From 1960 until the introduction of natural gas, the water gas producer became the most utilized method to obtain the town gas.

Conclusion

The gasworks in Spain bear testimony to the adoption of foreign technologies and also to the improvements made to these technologies.

In the early period, the most utilized technology consisted of directly-fired ovens of 3 or 5 horizontal ceramic retorts. However, at small gasworks, gas was obtained from fatty substances, mineral oils or hydrocarbons. Humbert gas and the rich gas were examples of this technology.

In the period of expansion, the specific process implemented in small towns continued to be developed by local technicians. This was the case of Arbós gas. Nevertheless, more advanced technologies, e.g. Klönne and Coze were imported to replace ovens whose retorts had deteriorated.

In the period of awareness of the need for technological change, efficiency was increased by the implementation of recuperative ovens. Small gasworks were equipped with recuperative ovens such as the Yvern recuperative, which bears witness to the development of local technology. Large gasworks were supplied with Lachomette ovens built by the Lebon Gas Company. The largest gas plants opted for ovens of vertical retorts.

The technological change was consolidated in the fourth period (1921-1938), during which recuperative ovens, such as the Yvern, began to be implemented in place of directly-fired ovens. Technological innovations arose from the introduction of continuous carbonization at large or medium sized gasworks.

Finally, the period of autarchy (1939-1960) confirmed the failure of the continuous carbonization method. In its place, the vertical retorts with intermittent carbonization were utilized. Other less diffused systems such as Percy and Collin were also introduced. During the first decade of the 20th century, some procedures to obtain water gas were implemented. The must notable example of this was the Tully Gas Plant in Cordoba. Nevertheless, the Otto-Pinstsch water gas producer was the one utilized in Barcelona after 1934 until the introduction of natural gas.

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THE GAS MUSEUM

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Abstract

1992 saw the founding of the Gas Natural Foundation. One of its current lines of action is the sponsoring of cultural activities that help to preserve and disseminate the historical heritage of the gas sector. Through its Gas History Centre, it maintains, classifies and catalogues the company's Historical Archive. Comprising 53 funds and 3 collections, the archive occupies nearly three linear metres and has almost 68,000 individual records, 11,248 of which belong to its photographic collection. One of the aims of the Gas History Centre is to encourage original research and educational projects that will recover the historical elements of the sector. How the gas industry grew, its involvement in civic life, and technological advances in Spain, with particular emphasis on the people and places that have played a part in creating the world we live in today.

In 2006, Gas Natural's Board of Directors and the Foundation's Sponsorship Board agreed to set up the Museum of Gas project. This museum will bring together the Historical Archive and the company's collection of historical pieces containing over 3000 objects, and will work in tandem with the Gas History Centre. Its permanent and temporary exhibition rooms will display the history of gas with a brief overview of electricity, extending to the hybridisation of natural gas with new energies, and future developments. The complex will also house the Foundation's central offices and services.

The Gas Natural Fenosa Foundation is building the **Museu del Gas** in a 19th century listed building, where electricity was produced from gas engines, situated in the Plaça del Gas right in the centre of the city of Sabadell.

The architect who designed the building in 1899 was Juli Batllevell i Arús, a modernist architect very close to Gaudi, and the businessman who commissioned the project from him, was Joan Brujas, proprietor of "La Energia, S. en C.", a company which in 1913 became part of Catalana de Gas y Electricidad, S.A. the old name of Gas Natural Fenosa. Currently, the building is protected by the Sabadell Special Plan for Heritage Protection and the remodelling and part of the new construction, as well as the interior museum design, has been awarded to the Varis Arquitectes studio under the direction of the prestigious architect and museum designer, Dani Freixes.

A long-standing tradition

In 1977, the cultural curiosity of the company, in those days, Catalana de Gas y Electricidad, S.A., made it set up the first Permanent Exhibition on gas in its historical company headquarters at l'Avinguda Portal de l'Àngel 22, Barcelona. That first exhibition presented the historical evolution of gas energy and with the most advanced museum elements of its time showed the characteristics and applications of natural gas as a clean, efficient energy, one which respects the environment. In 2006, almost thirty years later, it closed due to the move of the headquarters of the company to Plaça del Gas, 1, to where the first gas factory in Spain had been.

In parallel, from 1987, they began to conserve and catalogue the voluminous Historical Archive, a testament, since 1843, to the activity and technology of the company, the pioneer in the Spanish market of the development of the gas and electricity industry. More discreetly, the company went on recovering interesting specimens of those antique domestic gas appliances (now known as gas domestics) which would be widely introduced at the end of the 19th and beginning of the 20th centuries.

In 2004, the Foundation assumed the management, conservation and diffusion of the historical heritage of the current Gas Natural Fenosa, creating the Historical Centre as an instrument to vitalise, investigate and spread the historical heritage of the company. The latest studies published are the books *La industria del Gas en Córdoba* (*The Gas Industry in Cordoba*) *(1870-2007)* and *La industria del gas en Galicia: del alumbrado por gas al siglo XXI: 1850-2005*, (*The Gas Industry in Galicia; from Street Lighting to the Gas of the 21st Century: 1850-2005*), a line which will continue with the coming edition of *La familia Gil: empresarios catalanes en la Europa del siglo XIX* (*The Gil Family: Catalan Entrepreneurs in 19th Century Europe*). To complete the challenge, the idea appeared of the Gas Museum project, a unique facility, which fortunately will be able to be inaugurated in 2011.

A corporate museum, but a museum of energy, technology and society too

The Museum's discourse will explain history and the future, energy and the environment, gas and electricity, milestones and persons, technology and society. In short, an extensive stroll along an interesting path which can be divided up according to the following guiding aspects of the museum project:

A space devoted to a corporate museum where the long history of the company will be explained, from the far off days of 1843, as the *Sociedad Catalana para el Alumbrado por Gas* (*The Catalan Gas Lighting Company*) (1843-1912), afterwards *Catalana de Gas y Electricidad* (1912-1987), *Catalana de Gas* (1987-1992) *Gas Natural* (1992-2010), right up to the current *Gas Natural Fenosa*.

A second space will describe the manufacture of gas and electricity in the 19th and 20th centuries, the technologies employed, the occurrence of social and historical changes, without forgetting the evolution of customs and habits brought in by the new inventions and ways of life.

The Museum wants to explain the past, but also to provide tools to act in the present which help to create a good future. The third museum exhibition space will talk about the energies of the future, about the importance of concentrating our efforts to create energy which is continuously available, economical and which has low level of environmental impact.

The top of the building has also wanted to be part of the museum and it will be available for visits. From it you can enjoy an urban view transporting you to the industrial and textile past of the city, but also enjoy the garden and pond fed by the water collected by the tank hidden under the roof tiles.

The temporary exhibition space will be inaugurated with the exhibition, *Juli Batllevell, un gaudinià oblidat* (*Juli Batllevell, a Forgotten Gaudian*), on the life and work of this important modernist architect.

Finally, the building will also be the institutional headquarters and offices of the Gas Natural Fenosa Foundation.

Fig. 1: Virtual image of the Gas Museum.

Preserving the company's Historic Archive

A unique element, which enables value to be added to the project, is the availability of a large Historical Archive which enriches the discourse and the pedagogical and research possibilities related to the Museum. It influences the design and the interior distribution of the spaces, but at the same time ensures the adequate conservation of the documents and images of the activity of Gas Natural Fenosa and of the companies which have made it up through the course of more than 160 years.

Fig. 2: An illustration by Alexandre de Riquer.

A sustainable building

The Museum wants to be an example of a sustainable building. In the rehabilitation and construction, as well as in the initial demolition of the unlisted parts, sustainable criteria have been applied, based on the reduction of the energy requirements, the generation of energy both efficiently and from renewable sources, the efficient use of water, the use of low environmental impact materials and the minimising of waste production.

A dynamic centre, for its immediate surroundings but also at a wide range

The Gas Museum aims to be a dynamic centre, devoted to energy in the interface with the environment and society. We are putting together an agenda of activities where the didactic and cultural offer will reach a wide range of public.

The Gas Natural Fenosa company operates in 23 countries. The Gas Museum website will be a virtual window onto the Museum which will enable the contents, the temporary exhibitions, conferences, workshops and seminars taking place to be at the reach of everyone. It also aims to be a way to accede to the document archive and the collection.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 668.

THE PRODUCTION AND CONSUMPTION OF GAS IN MÁLAGA (1854-2009)

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Abstract

The present work studies the evolution undergone in the production and consumption of gas in Málaga from 1854 –the year of the inauguration of public lighting– to the present day. To this end I have brought together the relevant statistics and information analysis, relating it to those factors which have conditioned the development of this activity, namely: the technology used, the business aspect, the spread of electricity and butane gas, the socio-economic structure of the city and the size of its population. The case of Málaga is particularly interesting due to the fact that it was, and still is, one of the largest urban areas in Spain –81.000 inhabitants in 1860 and 566.000 at present day– in addition, it underwent a notable industrialisation process from the mid-eighteen-eighties to the nineteen-twenties.

Introduction

The present work examines the evolution undergone in the production and consumption of gas in Málaga from 1854 –the year of the inauguration of public lighting– to the present day.

I have drawn up a series of statistics reflecting the production and consumption of gas. Information that I analyze in relation to some of the factors that determined its development: the technology employed, the business management, the extension of electricity and butane gas, the socioeconomic structure and the number of inhabitants.

In Spain, Málaga was among the first cities to have gas. Different factors explain this early start. It was and still is one of the largest cities in Spain –79.000 inhabitants in 1854 and 566.000 today. It underwent a remarkable industrialization process. These factors included its port facilities, and the future construction of railways, which would make coal supplies easier.

The case of Málaga is interesting because it was one of the most industrialized cities from the 1830s to the 1920s. It was due to the benefits from the profitable overseas trade especially based on the export of agricultural products in the last third of the 18th century. This benefitted an important number of industries, especially some of the main textile and iron and steel industries of the country such as Manuel Heredia's foundry. One of his factories was set up in 1833 in Málaga, and the textile industry *Industria Malagueña* in 1846. We can also point to other industries such as the production of containers, chemicals (sulphuric acid…), and agricultural industries (wine, liquor and raisins)[1].

In the history of gas in Málaga, there are three cycles to highlight, depending on the energy employed: 1st. 1854-1968 was the period of the production of gas from coal; 2nd. 1969-1992 the period of the production of gas from oil; and 3rd. 1993-2009, the period of natural gas.

Gas made from coal (1854-1968)

Gas was first used, in 1846, by one of Málaga's most important industries, the *Industria Malagueña*, for its own-consumption. The manufacture for the sale of gas began in 1854.

In 1848 the *Empresa General de Alumbrado de Gas* began to build a gas factory. The *Empresa General de Alumbrado de Gas*, which was founded in 1846 when the English entrepreneurs, Manby and Partington, set up the *Sociedad Madrileña para el Alumbrado de Gas*. This company created *Empresa General Peninsular de Alumbrado por Gas* in the same year, planning to extend its activity to diverse places in Spain, like Valencia, Cádiz, Santander and Málaga between 1846 and 1848. However, it was not possible because of the difficulties that arose from the problems connected with coal supply, the diversity of the activity, and the changes in the economic system. Consequently, the construction of the gas factory in Málaga was never completed.

In 1852, the manager of the *Empresa General de Alumbrado de Gas* Luis Gosse obtained the monopoly of public and private lighting supply in Málaga. It was Gosse, who began the production for the sale of gas in Málaga, first used for public street lighting, in 1854. In the same year the business was taken over by the French group Vautier, which later formed la *Societé pour l' Eclairage de Malaga*, in 1859.

The factory was built on the outskirts of Málaga, in the area of Arroyo del Cuarto, where many other industries were located. This place was suitable because it was near the port and the future railway station of *Ferrocarriles Andaluces*, so, it would make the transport of coal and its waste products easier[2].

As for the city of Málaga, it is worth pointing out its early role as a centre of commerce, which laid the bases for its early industrialization, which began in the 1830s. It was this early industrialization that made the city attractive to the gas company. However, the larger factories chose the production of gas for their own use, and when electricity appeared they changed to this new form of energy, leaving gas behind. In addition, the level of income for most of the population was low, which meant their expenditure on gas was minimal, because it was considered a luxury product. Other problems the company faced were the distance from the coal sources and the expense of transporting coal from the mines of Cordoba.

We can see in Figure 1 that in each historical cycle, there are various stages, conditioned by their endogenous and exogenous factors mentioned above. Thus, between 1880 and 1968 there are the following periods when production fell: 1880-1895; from the first World War to 1921, the latter period subject to sharp rises and falls in production and finishing in a pronounced slump; the 1930s; and from 1951 to 1968. The periods of growth were: the first years of the twentieth century up to the First World War; the Dictatorship of Primo de Rivera (1923-1929); and from 1943 to 1950.

It was a complex process, in which strategy was decisive, especially when faced by challenges. As for example, when during the 1880s and the early nineties of the 19th Century, highly-qualified staff were contracted, the factory was modernized to compete with electricity, sales to private concerns increased, and there was an improvement and the extension of lighting to other zones[3].

Although the period 1900 to 1913 was one of growth, the increase in electricity production was even higher. The two most important electrical companies, the German *Siemens Elektristche Deutsche* and the English *The Malaga Electricity*, increased their production significantly and from 1903 they bought what was necessary from *Hidroeléctrica del Chorro*. In 1913, *Siemens* and *The Malaga Electricity* were the leaders in the lighting business[4].

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[1] PAREJO, Antonio (2009) *Historia Económica de la provincia de Málaga*, Málaga, Diputación Provincial.

[2] FÁBREGAS, Pedro (2003) *La Globalización en el siglo XIX. Málaga y el gas*, Sevilla, Ateneo de Sevilla, 19-24. ARROYO, Mercedes (2006ª) "Actitudes empresariales y estructura industrial. El gas de Málaga, 1854-1929", *Scripta Nova*, núm. 215.

[3] GARCÍA DE LA FUENTE, Dionisio (1984) *La Compañía Española de Gas, S. A., Más de cien años de empresa*, Paterna (Valencia), CEGAS. FÁBREGAS (2003). ARROYO (2006ª). ARCHIVO MUNICIPAL DE MÁLAGA, boxes 58C, 1958-1959, 1961, 1963 and 1964-1966.

[4] *Estadística del Impuesto sobre el Consumo de luz de gas, electricidad y carburo de calcio. Año 1913*.

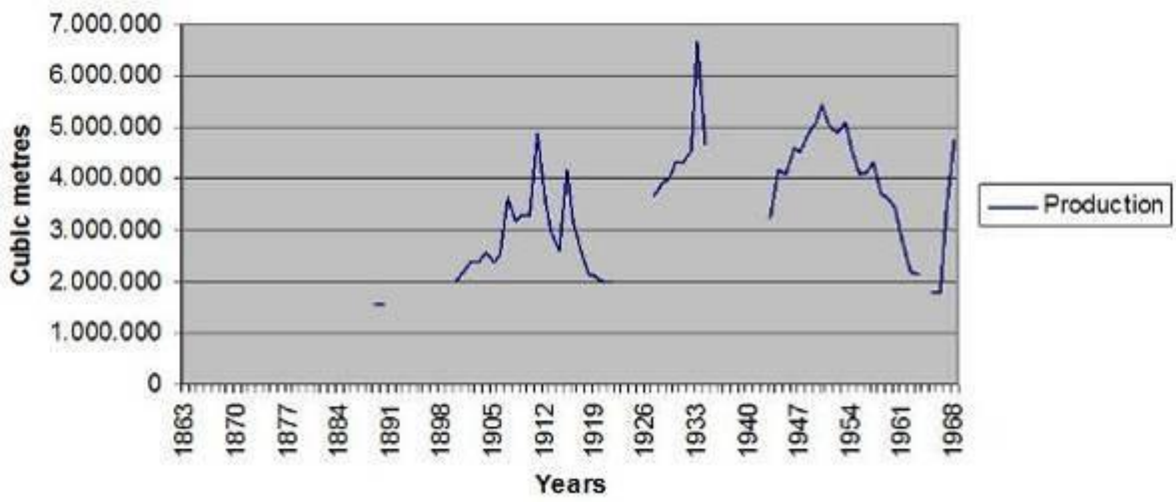

Fig. 1: The production of gas made from coal in Málaga (1863-1968)
Source: Table 1.

The reaction of *Lionesa* was changes to its gas machinery in order to face competition from the electrical companies. In 1912, the gasworks was remodeled. *La Lionesa* bought 6 "Lachomette" furnaces with 54 vertical retorts and other technology that could keep the furnaces hot[5].

In the first years of the 1920s, the prospects of the *Lionesa* were not good, because it had lost its old share in the lighting business. However, in 1923 a new company, *Sociedad General de Aguas de Barcelona,* took control of the *Lionesa* in a period of economic and demographic boom[6], which helped to change the situation in the gas business. From 1923 to 1929, the *Sociedad General de Aguas de Barcelona* made decisive changes to the business, at just the right moment.

The investments of 1924 in the gas system and factories made the service more efficient and helped to increase the number of customers that used gas in their own house for cooking and lighting[7]. They also supplied some factories that had been self-sufficient up to then. According to Arroyo, all this was possible thanks to a business policy of taking on short term and long term risks. The importance that private consumption was acquiring, representing 70% of the income in 1929, helped to strengthen the sales system. Also, the street lighting was extended to new urbanized areas[8].

Sociedad General de Aguas de Barcelona sold *Sociedad para el Alumbrado de Málaga* to its branch *Compañía Española de Electricidad y Gas Lebon*, acquired in 1925. Thus, *Alumbrado de Gas* became a branch of this latter[9].

Regarding the stage from 1951 to 1968, we can see in Figure 1 that it was a period characterized by a crisis: gas production and the number of consumers decreased sharply[10]. In the 1950s, the gas produced by coal couldn't compete, due

[5] GARCÍA DE LA FUENTE (1984), 182-183. FÁBREGAS (2003), 135. HEREDIA, Guillermo; LORENTE, Virginia (2003) *Las fábricas y la ciudad (Málaga, 1834-1930)*, Málaga, Arguval, 82.

[6] In these years, the gross provincial product rose an average of 2.8% a year. The population of Málaga capital grew (38,000 inhabitants more between 1920 and 1930) arriving at 188,000 inhabitants in 1930. PAREJO (2009), 132. Another positive factor that increased the consumption in Málaga was the rise in wages. VELASCO GÓMEZ, José (2008) *La Segunda República en Málaga 1931-1936*, Málaga, Ágora, 33.

[7] Something similar occurred in Lisbon and Córdoba. Arroyo and Cardoso de Matos (2009) "La modernización de dos ciudades: las redes de gas de Barcelona y Lisboa, siglos XIX y XX", *Scripta Nova*, núm. 296. About gas use in houses: ARROYO, Mercedes (2003) "*Gas en todos los pisos*. El largo proceso hacia la generalización del consumo doméstico del gas", *Scripta Nova*, núm. 146.

[8] ARROYO (2001) "Banca, infraestructuras urbanas y estrategias empresariales. La fábrica de gas de Málaga, 1923-1940". In: *Actes del 3er. Congreso de historia catalano-andaluza. Cataluña y Andalucía, 1898-1939*, Barcelona, Ediciones Carena, 297-325.

[9] FÁBREGAS (2003), 138-140. ARROYO (2006ª), 15.

[10] In Málaga, during the period 1951-1968, the consumption by household customers and industry stayed the same, with a slight decrease in the former. From 1952, the number of customers in the city of Barcelona grew. SINDICATO NACIONAL DE AGUA, GAS Y ELECTRICIDAD: (1958) *Estadística comparativa de la industria del gas. Año 1957*, Madrid; (1960a) *Datos estadísticos técnicos de las fábricas de gas españolas. 1950-1955*, Madrid; (1960b) *Estadística comparativa de la industria del gas en España durante los años 1957, 1958 y 1959*, Madrid; *Datos estadísticos técnicos de la industria del gas*, Years 1961-1968, Madrid. ARROYO, Mercedes

to its new competitors –liquefied petroleum gases such as butane and propane gas–; low tariffs charged; the huge number of workers; and the anachronistic infrastructure. All these factors influenced the decline of the business.

The introduction of butane and propane gas represented very strong competitors for coal-based gas and it was the main reason for its decline. In 1953, Spanish refineries began to sell butane and propane gas, and in 1957 the company *BUTANO* was founded. This company's aim was to take advantage of the cuts in electricity supply, which reappeared in 1956-1957[11]. Butane gas was well-received by the population and industry because: it was cheap, it was efficient and easily transported.

In Málaga *Electricidad y Gas Lebon* decided to eliminate coal-based gas. First Málaga used oil as a raw material. At the beginning of the 1960s, it tried to produce gas through a thermic cracking process using light naphtha but it was unsuccessful.

Once again, the gas industry reacted to the new challenge, by finding new systems of production based on oil. It was the company *Catalana de Gas y Electricidad*, now *Gas Natural,* which reorganized the gas industry. As from 1965, this company controlled the *Compañía Española de Gas.*, which was the owner of gasworks in Cádiz and Málaga. In 1968, in Málaga, *Gas Natural, S.A.* set up two Cifuindus plants that used light naphthas in the catalytic cracking process[12]. In Spain, *Catalana de Gas y Electricidad* was the first gas company that had this type of plant in 1956.

Gas made from oil (1969-1993)

Gas manufactured from oil continued until 1993, its main characteristic being a marked increase in production between 1969 and 1977. As we can see in Figure 2, gas production doubled between 1969 and 1977. This coincided with a period of economic expansion in Málaga[13].

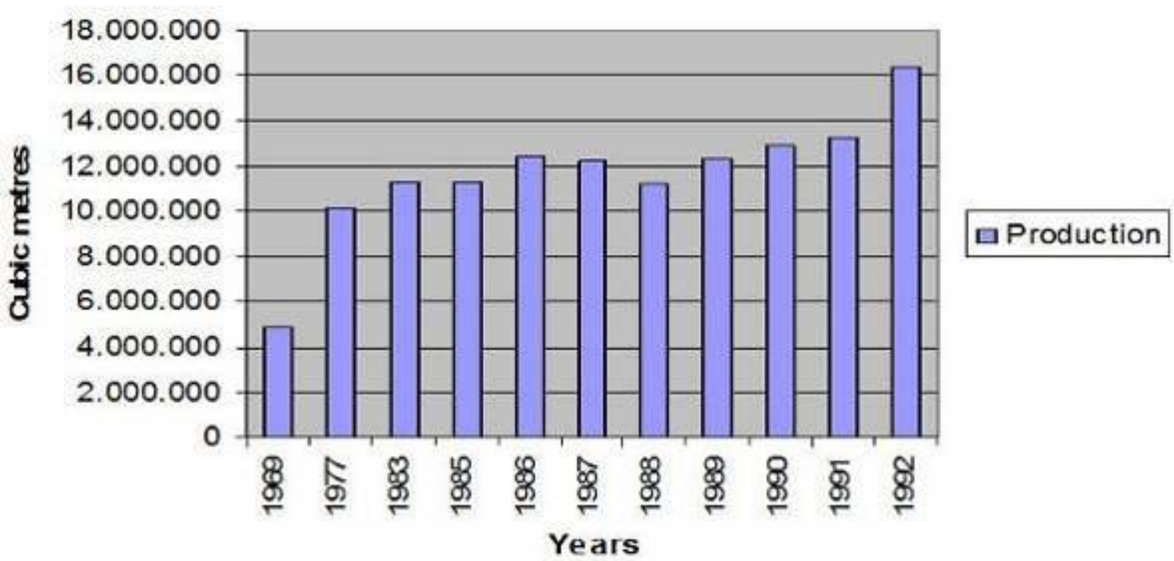

Fig. 2: The production of gas made from oil in Málaga (1969-1992)
Source: Table 1.

The transition to natural gas began in 1992. The previous step was the building of a propane gas plant. It was essential to replace the petroleum gas by natural gas. A process started in Spain in 1969, which came to Andalusia three

(2006b) "Los cambios en el proceso de producción y de distribución de gas en Barcelona y su *hinterland* (1930-1961). Entre el gas de hulla y el gas natural", *Sripta Nova*, núm. 218.

[11] FÁBREGAS (2003), 154-159. SÁNCHEZ GUTIÉRREZ, María Matilde (2006) *La regulación del sector del gas natural*, Valencia, Tirant lo Blanch, 64.

[12] FALGUERAS, Francisco *Una industria centenaria,* Catalana de Gas y Electricidad. GARCIA DE LA FUENTE (1984).

[13] PAREJO (2009), 287.

decades later, specifically in Seville, where the replacement process finished in 1991, carried out by *Gas Andalucía*. In 1987 *Gas Andalucía* was founded. In 1995, *Catalana de Gas*, now *Gas Natural,* became the only shareholder of *Gas Andalucía*[14].

In 1993, the establishment of plants in Málaga and Cádiz, where liquefied natural gas was brought and regasified, put an end to naphtha as a raw material in Andalusia[15].

Natural gas (1994-2009)

We can see that the gas consumption has increased greatly (Figure 3). Between 1994 and 1996 the consumption of manufactured gas was around 13-14 million cubic metres –m^3–, while the consumption of natural gas only represented 1 million m^3.[16] But since 1997, the situation has changed because only natural gas was supplied (73 million m^3 in that year). It was from 2001 that consumption increased in a spectacular way, coinciding with the spread of the new gas system to other different municipalities: the main coastal towns like Fuengirola, Mijas… and also those in the interior such as Ronda and Antequera…

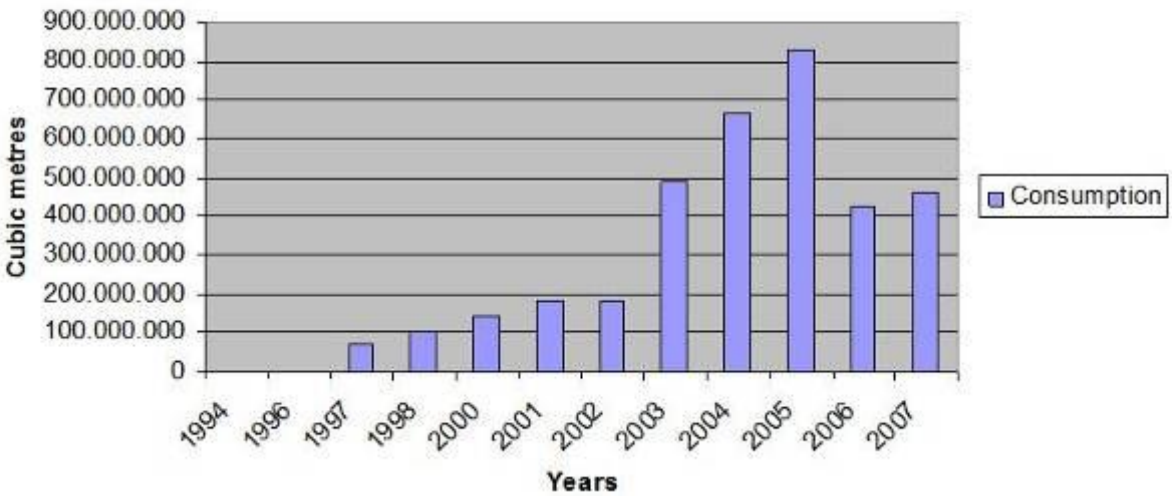

Fig. 3: The consumption of natural gas in Málaga (1994-2007)
Source: Table 1.

Between the years 2001 and 2007, the increase in consumption was really important, reaching, in 2005, the highest figure, 827 million m^3, an increase caused by the rise of the income levels and the building business. This spectacular rise was related to the increase of incomes of the working population and also with the boom in the construction industry. It is a market of more than a million people, exceeding 70% of the population of the province. At present, Cádiz and Huelva are the Andalusian provinces with the highest consumption levels, and Málaga is the 6th province in this context, at a distance from these two.

The increase in the consumption of gas has been possible thanks to important investments in transport and distribution networks, which have allowed its connection to the Iberian network. The natural gas network of *Gas Natural Andalucía* has grown considerably since 2001. It had 268 kilometres in that year and 802 kilometres in 2008. It represents 24% of the regional network, the second province after Seville[17].

The changes coincided with a moment of profound transformation in the sector, due to the liberalisation process started in 1996. From the 1st of January 2003 one could choose between the usual natural gas supplier and any of your choice. In that way there were two markets: a regulated one and one without restrictions[18]. In Málaga, in 2004, 75% of the

[14] GAS ANDALUCÍA (1992-1995) *Informe Anual*. Year 1991-1994.

[15] GAS ANDALUCÍA (1994) *Informe Anual*. Year 1993.

[16] This diagram represents only natural gas consumption.

[17] GAS ANDALUCÍA (1998) *Informe Anual 1997*. GAS NATURAL ANDALUCÍA: (2002) *Informe Anual 2001*; and (2009) *Informe Anual 2008*.

[18] FERNÁNDEZ PARADAS, Mercedes (2009) *La industria del gas en Córdoba (1870-2007*, Madrid, Lid Empresarial, 142-146.

customers chose the regulated option and 25% belonged to the other group. The biggest change occurred at the end of 2008, by which time all the consumers belonged to the free market.

Table 1: Production, consumption, customers and gas network length in Málaga (1863-2008)

Years	Production (m^3)	Consumption (m^3)	Customers	Network length (metres)
1863	1,950,000			
1880	1,594,876			
1882	1,580,110			
1889	1,551,250			
1890	1,551,250			
1893	1,620,600			
1895	1,788,500			
1900	2,020,275			
1901	2,207,520			
1902	2,366,295			
1903	2,380,895			
1904	2,571,060			
1905	2,380,895			
1906	2,518,500			
1907	3,616,785			
1908	3,192,655			
1909	3,288,285			
1910	3,288,285			
1911	4,876,306			
1912	3,551,760			
1913	2,927,869			
1914	2,597,508			
1915	4,155,218			
1916	3,184,960			
1917	2,613,075			
1918	2,157,032			
1919	2,110,988			
1920	2,006,233			
1921	2,006,233			
1923		1,600,000	2,149	
1924		1,800,000	2,567	
1925	3,222,485	2,225,000	5,463	
1926		3,366,950	7,635	
1927	3,686,925	3,686,925		
1928	3,886,275	3,886,275	9,669	
1929	4,014,325	4,014,325	10,390	
1930	4,312.260	4,312,260	11,103	

Years	Production (m^3)	Consumption (m^3)	Customers	Network length (metres)
1931	4,326,546	4,326,546	11,216	
1932	4,539,900	4,539,900		
1933	6,660,964	4,589,400		
1934	4,665,225	4,665,225		
1935	4,676,825	4,676,825	9,140	
1936			8,315	
1938	3,799,000	3,799,000	8,318	100,000
1943	3,260,000		7,399	104,739
1944	4,179,000			
1945	4,095,000			
1946	4,574,000			
1947	4,508,000	3,793,832	8,140	75,000
1948	4,856,000			
1949	5,099,000		8,355	
1950	5,415,000		7,990	115,476
1951	5,012,000			
1952	4,911,000		7,328	
1953	5,089,000			
1954	4,585,000		6,646	
1955	4,106,000	2,955,364		
1956	4,147,000	2,924,854	6,118	116,850
1957	4,326,000	3,017,356	6,208	116,881
1958	3,719,000	2,650,778	5,929	116,983
1959	3,590,000	2,502,668	5,638	116,795
1960	3,441,000	2,315,591	5,096	116,196
1961	2,746,000	2,079,171	4,614	116,056
1962	2,173,000		4,257	116,056
1963	2,138,000		4,178	116,056
1964			3,886	116,056
1965	1,794,000		3,488	116,117
1966	1,788,000		5,886	123,870
1967	3,380,000		5,886	130,701
1968	4,738,000		6,915	
1969	4,890,000	4,068,000	6,697	136,513
1977	10,139,000			
1983	11,269,048	11,269,048	12,807	
1985	11,334,000	10,280,952	12,163	181,000
1986	12,456,000	11,591,429	12,694	
1987	12,293,809	12,293,809	12,879	
1988	11,217,381	11,217,381	13,085	184,900

Years	Production (m³)	Consumption (m³)	Customers	Network length (metres)
1989	12,387,381	12,387,381	13,439	184,500
1990	12,928,571	12,928,571	13,968	195,100
1991	13,283,571	13,283,571	14,569	200,000
1992	16,392,619	16,392,619	14,965	208,000
1993		14,562,381	15,689	211,000
1994		13,578,067	16,552	
1995				198,000
1996		15,144,029	18,316	
1997		73,300,000	19,808	201,000
1998		99,700,000	22,135	212,000
1999				
2000		142,000,000		230,000
2001		182,000,000	32,143	268,000
2002		183,000,000	36,215	302,000
2003		488,000,000	40,566	347,000
2004		666,000,000	47,080	378,000
2005		827,000,000	56,428	489,000
2006		427,000,000	61,742	
2007		462,000,000	66,731	520,000
2008			70,666	

Sources for table 1:

The production in 1863, in GONZÁLEZ, María Ángeles; BRENES, Francisca (1992) "La implantación de la industria del gas en Málaga", *Jábega*, núm. 72, 61-68. 1880, 1882, 1938 and 1943-1969 and 1977, in FÁBREGAS (2003), 63 and 171. 1889-1900, 1906-1907, 1909, 1910, in *Estadística de la Contribución Industrial*. 1901-1905, 1908, 1916-1918, 1920-1921 and 1925, in *Estadística del Impuesto sobre el Consumo de luz de gas, electricidad y carburo de calcio*. 1910-1915, it is an estimate based on the production figures for 1910 and the income obtained from lighting between 1910-1915. 1919, in *Anuario Estadístico de España. Año 1922-1923*. 1927-1935, in MINISTERIO DE INDUSTRIA Y COMERCIO *Estadística general del consumo de carbones.* Years 1933-1935. This source shows the same levels of production and consumption for 1934. The information for 1927-1932 and 1934 refers to the gas sales and they are underestimated. Finally this has been used because the production shown in *Estadística del Impuesto sobre el Consumo…* were lower than those of gas consumption. 1983 and 1986-1992, in MINISTERIO DE INDUSTRIA *Estadística de las industrias del gas*. Years 1983 and 1986-1992. The data for 1983 and 1987-1992 are also underestimated because they refer to consumption. 1985, in MINISTERIO DE INDUSTRIA *Estadística de fábricas de gas. Año 1985*.

The consumption levels in 1923-1925, in ARROYO (2006a). 1926-1935, in MINISTERIO DE INDUSTRIA Y COMERCIO: *Estadística general del consumo de carbones.* Years 1933-1935. 1938, in FÁBREGAS (2003), 189. 1947 and 1955, in: SINDICATO VERTICAL DE AGUA, GAS Y ELECTRICIDAD (1948) *Datos estadísticos técnicos de las fábricas de gas españolas 1930-1947*, Madrid; and SINDICATO NACIONAL DE AGUA, GAS Y ELECTRICIDAD (1960ª). 1956 and 1960-1961, in COMPAÑÍA ESPAÑOLA DE ELECTRICIDAD Y GAS LEBON *Memorias.* Years 1956 and 1960-1961. 1957-1959 and 1964-1969, in SINDICATO NACIONAL DE AGUA, GAS Y ELECTRICIDAD: *Estadística comparativa de la industria del gas*. Years 1957-1959, and *Datos estadísticos técnicos de la industria del gas*. Years 1964-1969. 1983, in MINISTERIO DE INDUSTRIA *Estadística de las industrias del gas*. *Año 1983*. 1985, in MINISTERIO DE INDUSTRIA *Estadística de fábricas de gas. Año 1985*. 1986-1994 and 1996, in MINISTERIO DE INDUSTRIA *Estadística de las industrias del gas*. Years 1986-1994 and 1996. 1997-1998, in GAS ANDALUCÍA *Informe anual*. Years 1997-1998; and 2000-2007, in AGENCIA ANDALUZA DE LA ENERGÍA *Datos energéticos de Andalucía*. Years 2003-2007.

For the customers of 1923-1924 and 1929, in ARROYO (2001). The ones of 1925-1928, 1935, 1938, 1943 and 1949-1950, 1952 and 1954, in FÁBREGAS (2003), 189. 1930-1931, in COMPAÑÍA ESPAÑOLA DE ELECTRICIDAD Y GAS LEBON *Memoria. Ejercicio de 1931*. 1936, in ARROYO (2001). 1947, in SINDICATO VERTICAL DE AGUA, GAS Y

ELECTRICIDAD (1948). 1956-1959, in SINDICATO NACIONAL DE AGUA, GAS Y ELECTRICIDAD: *Estadística comparativa de la industria del gas*. Years 1956-1959. 1960-1969, in SINDICATO NACIONAL DE AGUA, GAS Y ELECTRICIDAD *Datos estadísticos técnicos de la industria del gas*. Years 1960-1969. 1983, 1986-1994 and 1996, in MINISTRIO DE INDUSTRIA *Estadística de las industrias del gas*. Years 1983, 1986-1994 and 1996. 1985, in MINISTRIO DE INDUSTRIA *Estadística de fábricas de gas. Año 1985*. 1997-1998, in GAS ANDALUCÍA *Informe anual*. Years 1997-1998; 2001-2002, in GAS NATURAL ANDALUCÍA *Informe Anual*. Years 2001-2002. 2003-2005, in COMISIÓN NACIONAL DE ENERGÍA *Análisis de la evolución de la demanda de gas natural y el número de consumidores durante el año*. Years 2003-2005. 2006-2008, in COMISIÓN NACIONAL DE ENERGÍA *Información básica de los sectores de la energía*. Years 2008-2009.

Network length in 1938, in MINISTERIO DE INDUSTRIA Y COMERCIO (1942ª). 1947, in SINDICATO VERTICAL DE AGUA, GAS Y ELECTRICIDAD (1948). 1956 and 1958-1959, in SINDICATO NACIONAL DE AGUA, GAS Y ELECTRICIDAD *Estadística comparativa de la industria del gas*. Years 1956 and 1958-1959; and 1967, in SINDICATO NACIONAL DE AGUA, GAS Y ELECTRICIDAD *Datos estadísticos técnicos de la industria del gas. Año1967*. 1985, in MINISTRIO DE INDUSTRIA *Estadística de fábricas de gas. Año 1985*. 1988-1991, 1993 and 1997-1998, in GAS ANDALUCÍA *Informe anual*. Years 1988-1991, 1993 and 1997-1998. 1992, in MINISTERIO DE INDUSTRIA *Estadística de las industrias del gas*. Year 1992. 1995, in Oilgas "Producción y consumo de gas en España en 1995", *Oilgas*, núm. 342. 2001-2006 and 2008, in GAS NATURAL ANDALUCÍA *Informe Anual*. Years 2001-2006 and 2008. The rest of years in FÁBREGAS (2003), 189-190.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 677.

COMMON PATHWAYS USED FOR THE INTRODUCTION OF THE TECHNOLOGY OF GAS IN CATALAN CITIES DURING THE SECOND HALF OF THE NINETEENTH CENTURY. THE CASE OF REUS AND OTHER SIMILAR CITIES

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Abstract

The bourgeoisie of the Catalan town of Reus looked keenly all that happened in Barcelona, the most cosmopolitan and important city in Spain. At the same time, Barcelona looked to the northern cities in Europe, specially London and Paris, longing to be on its same level of modernity by gaining the last technologic innovations that could increase their quality of life.

About the gas industry, Barcelona created their first infrastructures three decades after London and two decades after Paris. As in Barcelona, the Reus bourgeoisie had the control of industry, trade and municipal government, and therefore they could decide on the future of the town. Ten years later, in 1854, the gas industry was installed in Reus. The Catalan bourgeoisie kept nexus between them, and created a channel for the technologic knowledge from developed countries.

This communication tries to establish these common channels for introducing the gas technology in the Catalan towns, and I take the case of Reus as an example because at that time it was the 2nd Catalan town in economic and demographic importance. At the beginning, technologic communications were conducted in England and France. England exported technology, technicians and soft coal. France only exported technology and technicians. But both countries also exported business initiatives. It was only a one-sense communication channel.

Introduction

This communication aims to establish the technological introduction process of the gas in smaller towns and reference cities like Reus and their environment.

In mid 1800s, the gas industry was introduced in Catalonia with British and French capital and technology.

The beginning of the gas industry in Spain was late. It became the second wave of the sector which reached peripheral Europe.

Reus was the second Catalan city in economic and demographic importance, at a hundred kilometres from Barcelona. Reus joined the gas industry in 1854. A year later, it went into modern European light.

The idea that Reus had to look like Barcelona came from the bourgeoisie. A decade earlier, the bourgeoisie from Barcelona had wanted to enjoy the technical advances of cities like London and Paris.

Actors and technological measures that established the Catalan gas industry.

1. Foreign technicians

Not a large number of foreign technicians acted in Catalonia, but they were the first to introduce the knowledge and tools required by the gas industry.

Foreign technicians came mainly from the British Isles, France and Germany.

William Richards built factories in Reus in 1854, and in Tarragona in 1858. The gas meters and other tools were imported from England. Some years before, Richards had been an engineer of the *Sociedad Catalana para el Alumbrado por Gas*, from Barcelona.

Richards is a clear exponent of the nexus between technicians and foreign technology.

2. Foreign companies

The British and French initiatives were established in Spain because the energy market was still virgin. The reason was to be able to vent their financial and technological interests.

The foreign gas company tried to generate orders from participating companies. Thus, these companies could provide an outlet for the production of elements for the gas industry.

Other foreign firms, with a smaller presence in the country, tried to settle in Catalan cities such as Girona, Tarragona and Valls. These companies wanted to settle in this type of medium-sized cities because they fitted their potential business.

The creation of new gas companies generated an open path for technology transfer. The most active representative of this process in Spain was Charles Lebon.

3. Foreign Consulates in Catalan and Spanish cities

This institution protected the interests of the subjects of their respective states.

As an example, we find the relationship between William Richards and Tarragona's British Vice Consul, Edward Bridgman. Vice Consul in 1857, he recommended an English engineer to the City Council.

Richards got the concession to settle the gas lighting in Tarragona. Bridgman became the Manager of Richards' firm, the *Sociedad Tarraconense para el Alumbrado por Gas*.

The relationship between Bridgman and Richards continued in collaborating in the manufacture of gas meters. The application of the invention privileges is an evidence of the economic relationship between them.

In other Spanish cities, as in Ferrol, there were similar cases.

4. Speculation and the added value of gas factories

Existing companies were dedicated to build gas factories to sell them right away and get immediate profits.

Selling gas factories infrastructure was the most conservative economic method in gas industry. William Richards practiced this method of making money. He built factories, in addition to Reus and Tarragona, in Sabadell (1851-1852) and Manresa (1858).

Long term exploitation of the gas enterprise had the risk of benefits irregularity. The economic success of the company depended on fluctuation in coal prices and on demand growth.

Speculation in the construction of gas plants had a positive technological aspect: in each new factory, new technical improvements were applied that appeared within the sector.

5. The profit reason

The profit reason is the primary goal for any capitalist economy. In the case of Catalan gas corporations, there was a target for all shareholders.

This business expectancy acted as an incentive for technological innovation. When you got big profits, research on new applications were accelerated.

The benefits of the Reus company, *Gas Reusense*, were considered as excellent since 1860. The development of the gas company allowed an improvement of production and, in 1898, led to access to the electricity business.

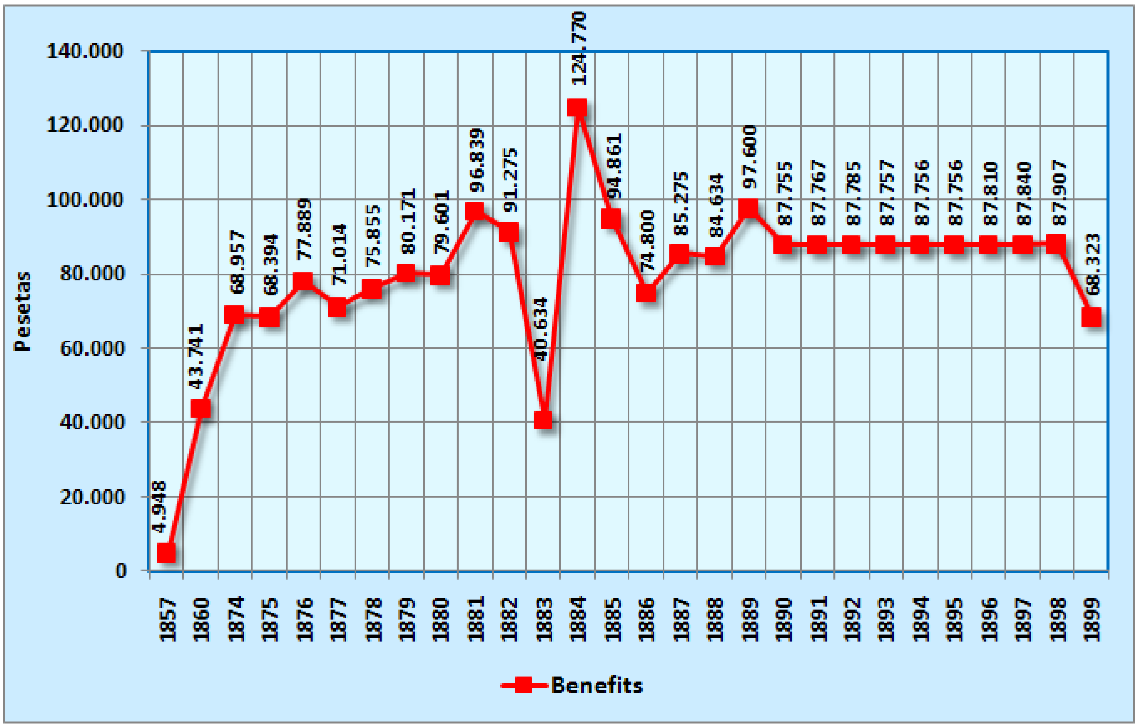

Fig. 1: *Gas Reusense*'s benefits in the period 1857-1899.

6. The Spanish government performance: patents and privileges for invention

In the year 1826, a Royal Decree was established on the patent. This new legislation introduced more categorical rules about invention and introduction of patents.

Despite the protectionist policy of the Spanish State, the gas sector employed foreign capital and patents. This external dependence was due to the lack of its own technological base and to an inability to invest in major industries.

In the Spanish gas industry the kidnapping of foreign patents was usual. The limited capacity for invention and patent creation drove companies to try to improve the copied object. This form of copy served also as a way of technological penetration.

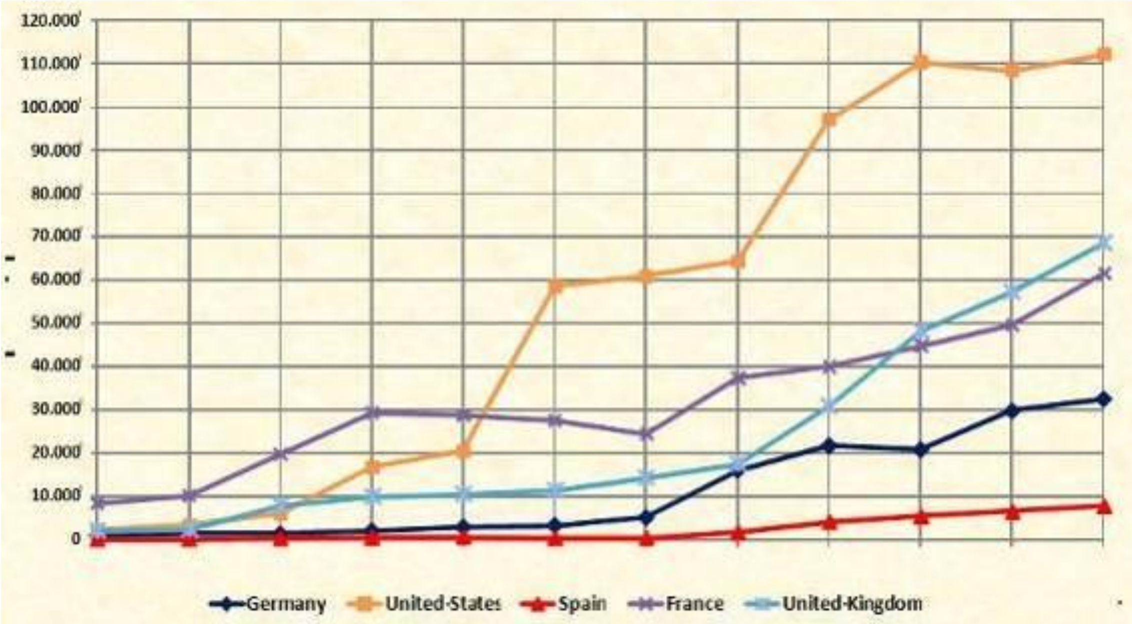

Fig. 2: International Patent Statistics, 1841-1900.

Table 1: Distribution of patent applications as economic activities in Spain, 1851-July 1878.

Economic activity	Patents	% of total period
Capital goods	568	13,4
Textile	539	12,7
Processing of primary products	490	11,6
Services	367	8,7
Chemistry	361	8,5
Iron and steel industry	346	8,2
Construction	229	5,4
Rail	195	4,6
Coal and mining	177	4,2
Gas and lighting	176	4,2
War industry	156	3,7
Paper	125	3
Naval transportation	93	2,2
Access to information	80	1,9
Ground transportation improvements without rails and vehicles in general	79	1,9
Wood	76	1,8
Agriculture and livestock	69	1,6
Communications	52	1,2
Aeronautics and air transhipment	25	0,6
Electricity	21	0,5
Channels	5	0,1
TOTAL	4.229	100

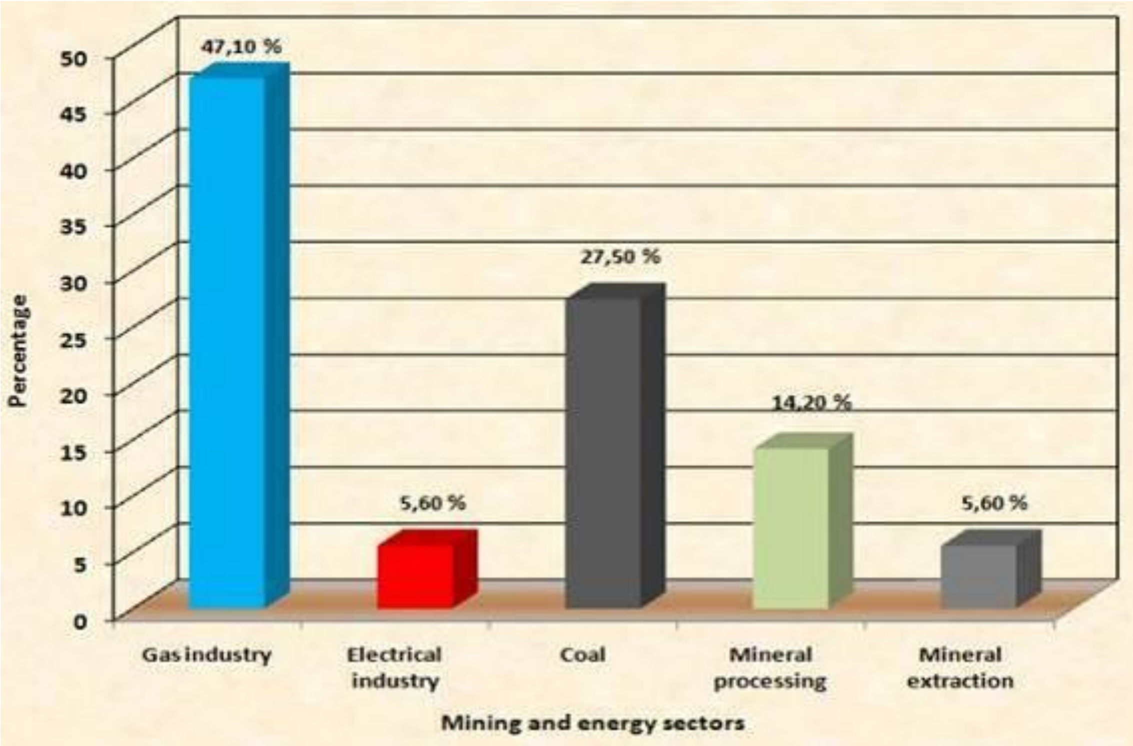

Fig. 3: Distribution of patent applications in the mining and energy sector by subsectors in Spain, 1851-July 1878.

Table 2: Percentage of patents protecting the technology of foreign origin in the economic activities that used this system in Spain, 1759-July 1878.

ECONOMIC ACTIVITY	Presence of foreign technology in the patent system (%)
Iron and steel industry	73,4
Textile	66,7
Equipments	62,1
Chemical	62,0
Construction	60,6
Processing of primary products	58,4
Services	49,1
TOTAL AVERAGE SPAIN	62,7

Table 3: Distribution of patent applications in the gas sector and electricity in Spain, 1851-July 1878.

GAS AND ELECTRICITY		PATENTS	% ON GAS AND ELECTRICITY PATENTS
GAS AND LIGHTING	Gas production (distillation, gas water, gas carburizing).	109	55,33
	Gas distribution (piping, burners, meters, lamps, etc..).	42	21,32
	Lighting by oils, oil or its derivatives.	25	12,69
	TOTAL GAS	**176**	**89,34**
ELECTRICITY	Cables, conductors, insulators.	3	1,52
	Batteries.	7	3,55
	Electricity production.	5	2,54
	Electric lighting.	6	3,05
	TOTAL ELECTRICITY	**21**	**10,66**
TOTAL SECTORS		**197**	**100**

7. City councils

In the case of the cities of Reus and Tarragona, the city councils acted as promoters of companies introducing gas lighting.

They opened an important path for the entrance of technological gas industry and encouraged local capitalists' proposals.

If private elements were essential in the industrial establishment, no less important was the interest shown by local public entities.

The municipalities imposed in the contracts with concessionaire's enterprise the Advancement of Science Clauses. These provisions contributed to technological innovation. Reus explicitly stipulated for the mixed lighting of the city by contract, achieved in 1897.

They also encouraged the industry access to technology by promoting their participation in various international exhibitions held during the nineteenth century.

8. The performance of Catalan industrialists and capitalists

They made trips during the first decades of the nineteenth centuryto the technological heart of the moment, sometimes to buy tooling for the textile industry.

Experiences outside the Catalan bourgeoisie were transmitted within the group itself. In this way, technology was transferred between members of different cities.

The contact between the bourgeoisie of various cities is evidenced by the fact that the shareholders of *Gas Reusense* resided in Barcelona. Macià Vila, a major industrial of the city of Reus, represented the interests of these shareholders.

9. Connections of the bourgeoisie with the country's politicians

General Prim, Pascual Madoz and the Earl of Retamoso, prominent politicians of the age, were part of the shareholding of the company *Fabril Algodonora*, which was ran by Macià Vila.

The friendly relationship between General Prim, a military and progressive politician who became President of the Council of Ministers of Spain, and Macià Vila established a personal affinity.

The correspondence between Macià Vila and General Prim mixed personal issues with economic and political interests of them both. It is a clear example of the complicity between politics and economics in the nineteenth century.

Exchanges between industrialists and politicians were necessary and constant. Politicians required industrial capital to finance their political positions to remain in power. In return, industrialists received valuable information and favourable treatment.

10. The School of Chemistry of the **Junta de Comercio** *and the Industrial Engineering School of Barcelona*

The bourgeoisie emphasized his interest in the country industrial education and the maintenance of free schools of the *Junta de Comercio de Barcelona*. This institution covered the costs of its pensioners studying the natural sciences and industrial arts abroad.

In Spain, the industrial studios were founded in 1850. In 1851 the Industrial School of Barcelona was created, which brought schools and Chairs from the *Junta de Comercio*.

The School of Engineering of Barcelona was the only one responsible for providing Catalonia with the senior technicians that the country industrialization required during the second half of the nineteenth century.

In Reus, the technical direction of the gas factory was changed in 1859. Engineer Richards, who ran the factory, passed all responsibility to a local technician, Josep Simó Amat.

In the 1880s, the School of Industrial Engineering was socially recognized, but since the 60s, these Catalan engineers already controlled some gas plants in Catalonia.

By the end of the century, as seen in the town of Reus, the figure of Industrial Engineering was established both in private companies and public institutions.

11. The **Fomento del Trabajo Nacional** *and the Chambers of Commerce and Industry*

The *Fomento del Trabajo Nacional* was the result of employer associations developed during the nineteenth century, and represented another channel of technology penetration.

The *Fomento* spread technological and economic developments inside and outside the country by publishing their magazine. Also through the celebration of meetings such as those held in Reus and Manresa in 1894.

Among the tasks performed by *Fomento*, some highlights are its involvement with some economical corporations, new technologies and improvements in the gas industry.

Fomento and, later, the Chamber of Commerce, created a kind of industrial education to spread other nations' industrial developments.

Since 1900, the Chambers of Commerce, Agriculture and Industry agreed to become partners of *Fomento*.

The existence of contact between the Catalan gas companies and private institutions was obvious. A good example is the relationship between *Fomento* and the Reus gas company, which exchanged technological knowledge and economical aspects.

12. The **Sociedad Catalana para el Alumbrado por Gas***, of Barcelona*

This company was the mirror in which all other gas companies tried to reflect, especially in Catalonia.

The abroad contacts of *La Catalana* were a way of introducing important technology to the rest of companies.

The relationship of *La Catalana* with foreign technology was initiated at the moment of its creation due to the figure of its shareholder Charles Lebon and the technological elements he imported from others countries.

Through the *Centro Científico Industrial* of Barcelona, *La Catalana* came to know of foreign innovations and prestigious French journals as *The Célébrité* and the *Journal de l'Eclairage au Gaz*.

The participation of shareholders of *La Catalana* in the *Gas Reusense* company created an important link. The correspondence between the two companies on technological and economic items was usual.

The bibliography of *La Catalana* about gas industry consisted on about 40 volumes acquired between 1845 and 1900. This collection became a place of reference for technical consulting.

13. Reus' economic and cultural associations

The *Sociedad de Amigos*, formed by Reus capitalists and industrialists, was born in 1839. This society was the predecessor of *El Círculo*, created by the most important taxpayer of the city in 1852.

Members of *El Círculo* owned most of the means of production and political control of the city. It was a vital centre of business and technology. Some of their members were shareholders of *Gas Reusense*.

There were other economic and cultural associations in Reus, such as the *Centro de Lectura* created in 1859 and the Chamber of Commerce and Industry of Reus.

These institutions also provided innovative technological knowledge during the second half of the nineteenth century.

14. Local technologies and cultural publications

The publications that came out in Reus in the 19th century were another channel of technological knowledge.

One of the most prestigious cultural magazines of the city was the *Eco del Centro de Lectura*. Published by the *Centro de Lectura*, it included articles about gas and its applications.

At the *Centro de Lectura* library there were books about gas technological evolution. In the archive there are still copies of the *Revista Industrial*, from the period between 1861 and 1863, which published all technical innovations –among other disciplines, the gas industry.

***15. Bibliography and subscriptions to foreign journals used by* Gas Reusense**

The bibliography on gas industry appeared intensely from the moment it became an energy sector. But it increased its volume with the onset of electricity. It made the access to foreign technological books easier.

The first books and magazines that Catalan and Spanish gas companies acquired were, basically, French and British. *Gas Reusense* was no exception.

The books found in Reus and Tarragona gas companies were edited in Paris and Liege.

Gas Reusense sought its own channel to gas technology by subscribing to the most important publications published abroad: the French *Journal de l'Eclairage du Gaz* and the *Engineer* and the *Journal of Gas Lighting*, both published in England.

Another path used by *Gas Reusense* to join technology in a safe and efficient way was to become a member of the Gas Manufacturers Association.

16. Catalan technicians take responsibility

The first Catalan and Spanish gasworks were created and driven by foreign engineers, but they were quickly replaced by local technicians.

Since 1860, an important technological assimilation was initiated, and knowledge achieved by local engineers could be considered as sufficient.

In 1880, the gasworks were built by engineer I. Boixaderas in Valls, and it was governed and administered by engineer J. Borras, who were both students of the School of Engineering of Barcelona.

According to the *Anuario de la Minería, Metalurgia y Electricidad*, in Spain in 1900 the gas factories management were directed by more Spanish engineers than foreigners, in a ratio of 9 to 4. The *Anuario* of 1905 established that the ratio of engineers working in Spanish gas industry were 10 to 1.

Conclusions

Cities became the stage for the nineteenth century economic development. A new energy appeared on them for public lighting: gas.

The gas industry in Barcelona (1842), Reus (1854) and Tarragona (1858) needed pathways for technology introduction, and to consolidate itself it also required a number of actors to perform a set of technology activities.

The birth of gas industry in Catalonia and Spain had a foreign origin. The foreign experts introduced the materials and the knowledge that the industry needed.

The source of technical and foreign gas companies were mainly the British Isles and France.

William Richards built factories in Reus and Tarragona with British materials, and he created his own company in the Catalan energy market, that was practically virgin.

In most Catalan cities, the main actors that made gas industry possible were the councils, which acted as promoters, and local industrialists and capitalists, who invested to achieve it.

Macià Vila was the industrial prototype that worked for mutual benefit taking profit from his country's political connections.

Some private institutions collaborated, too, in the introduction of technological innovations in Catalonia. We highlight the *Fomento del Trabajo Nacional* and the Chambers of Commerce.

The reference company for the Catalan gas industry was the *Sociedad Catalana para el Alumbrado por Gas*, of Barcelona. *Gas Reusense* and *La Catalana* maintained contact since the beginning of the activity of the Reus Company. The fact of having some common shareholders was a big help.

Bibliography and foreign magazines were another way for the transmission of knowledge to Catalan cities.

The new techniques required a transmission and consolidation of knowledge, which could only be done in a systematic way at technical schools and universities.

Since 1880, industrial engineers were socially recognized, but before that, as in the case of Reus, the technology transfer process from foreign technicians to local ones had already begun.

The process of technology assimilation in the gas industry could be considered as consolidated in late nineteenth century, when most technicians in industrial factories had already been educated at industrial schools of Barcelona and Madrid.

Sources

ACBC: Archive of Baix Camp (Reus).

ADT: Archive of Diputació de Tarragona.

AGN: Archive of Gas Natural.

AHT: Historical Archives of Tarragona.

BFTN: Library of Fomento del Trabajo Nacional (Barcelona).

OEPM: Spanish Patent and Trademark –*"Oficina Española de Patentes y Marcas"* (Historical Archive).

References

ANGUERA I NOLLA, Pere (1977) *"El Círcol". 125 anys d'una societat.* Reus: Societat "El Círcol".

ANGUERA I NOLLA, Pere (1981) *Economia i societat al Baix Camp a mitjan del segle XIX.* Tarragona: Publicacions del Col·legi d'Aparelladors i Arquitectes Tècnics de Tarragona, Escaire, 3.

ARROYO HUGUET, Mercedes (1996) *La industria del gas en Barcelona (1841-1933).* Barcelona: Col·lecció "La estrella polar". Ediciones del Serbal.

ARROYO HUGUET, Mercedes (2000) *Tècnics i tecnologia de gas a la Catalunya del segle XIX.* Barcelona: Quaderns d'història de l'Enginyeria, volum IV.

BANÚS SANS, José (1954) *Gas Reusense 1854-1954*, Reus.

BRAGULAT, Anna. M. (1989) *Fondo Histórico Revistas, 1877-1970.* Cuadernos de Historia, 4. Barcelona: Grupo Catalana de Gas.

BRAGULAT, Anna. M. (1989) *Fondo Histórico Libros de Gas del XIX.* Cuadernos de Historia, 10. Barcelona: Grupo Catalana de Gas.

CABANA, Francesc (1992) *Fàbriques i empresaris. Els protagonistes de la Revolució Industrial a Catalunya.* Vol 1: *Metal·lúrgics. Químics.* Barcelona: Enciclopèdia Catalana.

CONTRERAS, Adriano (1900) *Anuario de la Minería, Metalurgia y Electricidad de España, 1900.* Madrid: Revista de minera, metalúrgica y de ingeniería.

CONTRERAS, Adriano (1904) *Anuario de la Minería, Metalurgia y Electricidad de España, (con una sección de Industrias químicas).* Madrid: Revista de minera, metalúrgica y de ingeniería.

FÀBREGAS, Pere A. & BRAGULAT, Anna (2005) Arc*hivo Histórico de Gas Natural: un camino iniciado en 1840*. Santiago de Compostela: http://www.usc.es/estaticos/congresos/ histec05/ b21_fabregas_bragulat.pdf

FÀBREGAS, Pere A. (1993) *Un científic català del segle XIX: Josep Roura i Estrada (1787-1860).* Barcelona: Gas Natural SDG, S.A.

FÀBREGAS, Pere A. (2003) *La globalización en el siglo XIX: Málaga y el gas.* Sevilla: Universidad de Sevilla.

FERNÁNDEZ-PARADAS, Mercedes (2009) *La industria del gas en Córdoba (1870-2007).* Barcelona: Biblioteca de Historia del Gas, núm.: 2, Fundación Gas Natural.

FOMENTO DEL TRABAJO NACIONAL (1879) *Estudios Mineros, volumen 3*. Estudio sobre la: *Divisibilidad práctica de la Luz eléctrica. Memoria descriptiva del Sistema Molera y Cebrián.* Barcelona: Imprenta de Leopoldo Domènech.

FOMENTO DEL TRABAJO NACIONAL (1893-1905) *Memorias.*

GARCÍA DE LA FUENTE, Dionisio (1996) *Del Gas del Alumbrado al Gas Natural en Castellón de la Plana. 1870-1995.* Compañía Española de Gas, S.A.

GARCÍA DE LA FUENTE, Dionisio (2006) *Una historia del gas en Alicante.* Barcelona: Biblioteca de Historia del Gas, núm.: 1. Fundación Gas Natural.

GARRABOU, Ramon (1982) *Enginyers industrials, modernització econòmica i burgesia a Catalunya (1850-Inicis del s. XX).* Barcelona: Col·lecció Clio. L'Avenç – Col·legi d'Enginyers Industrials.

GASOL, Josep M. (1984) *125 anys del Gas a Manresa, 1859-1984.* Catalana de Gas y Electricidad, S.A.

GRAU PUJOL, Josep M. & PUIG TÀRRECH, Roser (2007) *Immigració estrangera a Catalunya des d'una perspectiva local: Reus, segles XVI-XX.* Butlletí de la Societat Catalana d'Estudis Històrics, núm. XVIII, p. 93-108.

MARTÍNEZ LÓPEZ, Alberte (2009) *La industria del gas en Galicia: del alumbrado por gas al siglo XXI, 1850-2005.* Barcelona: Biblioteca de Historia del Gas, núm.: 3. Fundación Gas Natural.

MELER, Tomás J. (1887) *Anuario Meler. Guía Oficial e indicador general de la Industria y del Comercio de Barcelona.* Barcelona: El timbre Imperial.

Memòria (1886-1986). Reus: Cambra Oficial de Comerç i Indústria.

MOYANO JIMÉNEZ, F.(2009) *Un model d'empresa energètica local: "Gas Reusense" (1854-1969)*. Tesis Doctoral. Universitat Rovira i Virgili. Tarragona.

NADAL, Jordi, dir. (2003) *Atlas de la industrialización de España, 1750-2000*. Madrid: Fundación BBVA. Crítica.

OLIVÉ SOLANES, Josep M. (2002) *Tarragona i el Gas*, *1844-1865.* Tarragona: Fundació Cultural Minerva, Volum I.

RIBÉ, Ernest & GASCON, Vicenç (1995) *Història del gas canalitzat a Valls.* Valls: Gas Tarraconense, S.A.

ROSÉS, Joan R. (2001) *La difusión de la tecnología extranjera en España durante la Revolución Industrial: El caso de la industria algodonera catalana (1784-1861).* Zaragoza: Congreso de Historia económica.

SÁIZ GONZÁLEZ, J. Patricio (1999) *Invención, patentes e innovación en la España contemporánea.* Madrid: Oficina Española de Patentes y Marcas. Ministerio de Industria y Energía.

SABATÉ MILL, A. (prólogo) (1994) *Tradició gasista. 75 aniversari Gas Penedès.* Barcelona: Gas Penedès.

THOMSON, James (1994) *Els orígens de la industrialització a Catalunya. El cotó a Barcelona (1728-1832).* Col·lecció Estudis i Documents, 50. Barcelona: Edicions 62.

VERNET, Joan & PARÉS, Ramon (Dirs.) (2009) *La Ciència en la Història dels Països Catalans.* Volum III: *De l'inici de la industrialització a l'època actual.* València: Institut d'Estudis Catalans; Universitat de València.

WILLIOT, J. P. (2006) *La diffusion de la technologie gazière francaise dans le bassin méditerranéen: de la construccion des usines à gaz à la mise en place des réseaux de gaz naturel (années 1840-1980),* en MERGER, M. (dir.), *Transferts de technologies en Méditerranée,* París: Presses de l'Université París-Sorbonne, p. 207-219.

MAKING SENSE OF THE AURORA: THE NORTHERN LIGHT IN SCIENTIFIC AND CULTURAL-POLITICAL CONTEXTS

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BETWEEN ASTROPHYSICS AND GEOPHYSICS: THE FRENCH CONTRIBUTIONS TO THE STUDY OF AURORAS AT THE BEGINNING OF THE 20TH CENTURY

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Abstract

Atmospheric researches were initiated in France during the 1860s by the physician and astronomer Jules Janssen, who created for instance the expression "telluric lines" to name the terrestrial part of the solar spectrum. This work was continued at the Meudon Observatory at the end of the 19th century, where Henri Deslandres inferred knowledge of atmospheric phenomena by means of mimetic experiments on cathode rays in vacuum tubes.

We will examine how these atmospheric researches were pursued at the beginning of the 20th century. First, we will detail the work of Charles Nordmann, who tried to make a link between auroras and radio waves emitted from the sun, and experimented at the top of the Mont Blanc to detect those waves. Afterwards, we'll examine the way by which the "French school of molecular diffusion", initiated by Jean Cabannes, a Charles Fabry's student, paid attention to the auroras. In the end, we will focus on the researches of Alexandre Dauvillier in cosmic physics, and his participation in the French expedition of the Second International Polar Year (1932-1933) at the Scoresby Sund, during which he ruined the hertzian and ultraviolet theories of auroras, and confirmed the spectroscopic results of Vegard.

In this way, we will point out the place auroras took in atmospheric lights researches in France (auroras, light of the night, sky, zodiacal light, …), the articulation between field observations and laboratory practices, the collaboration between French and Nordic physicists, and we will question the ways and meanings by which atmosphere was at all once an astrophysical and geophysical object.

At the end of the 19th century, astrophysics and geophysics started to be identifiable and institutionally stabilized fields in France. Nevertheless, some objects were always ambiguous and didn't respect rigid boundaries. For instance, atmosphere was studied all at-once by physicists, astronomers or meteorologists and became a strategic place between heavens and earth. Atmospheric researches became in France a real stake and various styles of researches were observed in their study: atmosphere could be studied because of the possibility it offered to explore the interactions between light and matter; atmosphere became an object allowing analogies and technical transfers between the study of stars and the study of the earth; and atmosphere was also an important part of a new discipline, cosmic physics, seen as an interdisciplinary science making the coordination between astrophysics and geophysics.

In this context, auroras, seen as part of atmospheric researches, were largely studied by French scientists. Our paper aimed first to do a review of the French polar expeditions, during the 19th century and the beginning of the 20th century, where auroras were observed or analysed. The first of them was the 1833 expedition of Jules Poret de Blosseville, during which terrestrial magnetism was studied on the suggestion of François Arago. But auroras became systematically and largely studied during the 1838-1839 *La Recherche* expedition: Bravais tried, influenced by Arago as well, to detect connections between auroras and terrestrial magnetism. If scientific aims were aimed, it must be recognized that a specific political context will have to be explored further. Indeed, Arago and Humboldt had a cosmic thought that was not the same than the scientific and philosophic thought of Gauss and Weber in Germany. If Arago and Humboldt wanted to measure all the magnetic particularities at the surface of the earth, Gauss and Weber preferred an abstract mathematical approach where local singularities were not to be taken into account. This difference between these two methodologies stimulated English reflections about the way to explore the earth, and led to the "magnetic crusade"[1]. The *La Recherche* expedition will have, in this respect, to be studied in this scientific and political context. Indeed, Bravais, charged with the measurement of magnetic parameters and their relation with auroras, seems very ambiguous with the French position and more inclined to adopt the German method when he says:

> "En Allemagne, MM. Gauss et Weber, à la tête de l'association magnétique allemande, montraient par la publication annuelle de leurs *résultats* que l'on pouvait mesurer avec un grand degré d'exactitude les plus petites variations de la force magnétique.[…] Les belles observations de M. Arago sur l'influence lointaine exercée par les aurores boréales laissaient encore indécises la question des petites et incessantes déviations ; l'observateur ne leur attribuait qu'une faible importance, les considérait comme d'inexplicables anomalies et ne s'attachait qu'à comparer entre les moyennes déduites d'un grand nombre d'observations. Mais, en 1838, l'expérience de l'association magnétique allemande prouvait déjà que cette manière de voir n'était pas exacte, que les variations indiquées n'avaient point une origine purement locale, et que la cause inconnue qui leur donnait naissance étendait son action à des distances considérables : ainsi un intérêt tout nouveau devait désormais s'attacher à leur étude."[2]

Thereafter, the South Pole was more explored by French expedition than the North Pole. We can cite the *La Romanche* expedition in 1882-1883, during the first polar year where various scientific researches were made, concerning meteorology, magnetism and auroras, geology, botanic, zoology, hydrographic and oceanographic measures, anthropology, and, as well, the observation of the transit of Venus. In 1893-1894, Calloch de Kerillis, captain of the *Chasseloup-Laubat* stayed in Newfoundland where he particulary investigated the auroras: he imagined a heliodynamic theory of the auroras using as well the 1882-1883 results[3]. For him, the earth was surrounded with atmospheric and concentric rings, centred on the Gauss Pole, and constituted by an auroral matter, non luminous by itself, and put in an isostatic equilibrium under the influence of the terrestrial magnetism. The Earth would be like a comet, with a tail, where a "solar induction" provoked luminous phenomena.

But the most important expedition for the auroras researches was in no doubt the 1932-1933 expedition in Scorseby Sund prepared in the context of the 2nd Polar Year, during which Dauvillier carried out the first important study of this phenomenon. Instead of an electrical theory, Dauvillier used electrons –cathode rays, as named at the beginning of the 20th century– to build an electronic theory named "secondary electrons auroral theory". In his course of cosmic physics, in 1954, he can claim, on the basis of laboratory experiments, observations and measurements made during his polar travels, and on mathematical treatments, that secondary electrons are produced in the exosphere by solar corpuscles, and then excited by primary electrons that gravitate in the terrestrial field: those electrons then go down from this orbit and bombard the earth. For him, auroras are, consequently, a permanent phenomenon, that can be observed everywhere on the earth.[4]

Thereafter, expeditions occurred in 1934-1935 in Abisko, and in 1937-1938 in Trömso. In Abisko, Dauvillier was accompanied by the French scientists Daniel Barbier, Daniel Chalonge and Etienne Vassy: they made atmospheric researches concerning ozone and auroras. In Trömso, René Bernard, from the Institut de Physique Générale de l'Université de Lyon, made spectroscopic researches of the auroras and of the night sky light.

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[1] John Cawood, "The magnetic crusade: science and politics in early Victorian Britain", *Isis*, vol.70, n°4, 1979, p.492-518.

[2] *Voyages de la commission scientifique du Nord en Scandinavie, en Laponie, au Spitzberg et aux Feröe, pendant les années 1838, 1839 et 1840 sur la corvette La Recherche*, Paris, 1838, p.2-3.

[3] CRAS, 1910,t.150, p.1296; *L'Aurore Boréale*, Paris, Ficker, 1911.

[4] A. Dauvillier, *Le magnétisme des corps célestes. Tome troisième. Les Aurores polaires et la luminescence nocturne*, Paris, Hermann, 1954.

During this period, auroras stimulated theoretical questionings, above all after the discoveries of new radiations and new corpuscles at the end of the 19th century. For the physicist Charles Nordmann, auroras resulted of the light created in the terrestrial atmosphere because of the effects of hertzian waves emanated from the sun. In a communication made on November 20th,1903, Nordmann summarizes the various auroral theories to discuss and criticize them. For him, magnetism couldn't be invoked, because of the lack of magnetic perturbations frequently noticed during a great number of polar expeditions: in this way, it can be said that the observations made during the 19th century polar expeditions started to be a useful means to cumulate knowledge about auroras, such as Mairan did in the 18th century. Influenced by the experiments of Birkeland concerning the cathode rays, and by the theory of Arrhenius, based on the hypothesis of negatively charged corpuscles emitted by the sun and producing cathode rays in the terrestrial atmosphere, Nordmann was conducted to disqualify electric currents in the atmosphere as the cause of the auroras. Instead of this, he imagined that auroras are a luminescent phenomenon caused by cathode rays produced in the atmosphere by hertzian waves emitted by the sun. This hypothesis is to be contextualized in Nordmann researches: working first at the Meudon observatory with Janssen, Nordmann defended his thesis on the hertzian waves in 1903 (prepared with Henri Poincaré), and then worked at the Paris Observatory. For him, hertzian waves became a paradigmatic object, allowing the explanation of a lot of cosmic phenomena such as comets, nebulas, etc. For Nordmann, auroras make part of a larger epistemological problem: to explain cosmic appearances by the means of laboratory experiments renewed by the discovery of new corpuscles and new radiations. Larger studies will have to be devoted to describe precisely this analogical method.

Paul Villard is another French physicist who proposed at the beginning of the 20th century a theory explaining the northern lights. As the astrophysicist Henri Deslandres, he used the newly discovered cathode rays to explain such cosmic phenomena as auroras or comets. His theory is based on his work concerning the relations between cathode rays and a magnetic field, and can be called "cathodic electromagnetism"[5]. For Villard, northern lights are produced by the solar illumination of cirrus in the atmosphere, conducing to the production of cathode rays. Interacting with the terrestrial magnetic field, those corpuscles coil themselves in a particular tube trajectory, named *trochoïde*, producing visible rays in the atmosphere. He writes that "il résulte de cette étude que si des rayons cathodiques se produisent dans le champ magnétique terrestre, ils s'enrouleront en une spirale repliée en zig-zag de manière à former autour de la Terre une nappe de revolution limitée au nord et au sud par deux parallèles magnétiques d'intensité décroissante de l'ouest à l'est, et d'aspect tout à fait semblable à celui d'une aurore "[6]: his theory is guided and constrained by the theoretical problem of a mathematical description of the trajectory of a charged particle in a magnetic field, an is then a purely physical problem resolved by a mathematical analysis.

In the tradition instigated by Charles Fabry, physicists such as Jean Cabanne or Jean Dufay explained auroras in the subfield of atmospheric optics, more interested in the study of the atmosphere as an interactive place between heavens and earth, where the interactions between light and matter could easily be measured and observed: auroras, or northern lights, were an atmospheric phenomenon to be studied in the same way than the zodiacal light and the light of the night sky. For Daniel Barbier and Daniel Chalonge, auroras were studied because of spectroscopic researches that allowed to make analogies between stellar, solar and telluric atmospheres. Unlike Villard or Nordmann, who wished to explain natural phenomena by induction of laboratory, and so terrestrial, experiments, Barbier and Chalonge saw the atmosphere as a boundary place, between heavens and earth, and so between astrophysics and geophysics. They often insisted on this methodological aspect: the study of the stars needs precise measurements of the atmospheric absorption, or precise observations of the particles emitted by the sun seen as a singular star, making the study of auroras or atmospheric ozone necessary: "Nous avons entrepris, depuis plusieurs années, des recherches spectrophotométriques sur les étoiles des premiers types spectraux, dans un but purement astrophysique. Nous sommes cependant obligés, pour faire les corrections d'absorption atmosphérique dans l'ultraviolet, de déterminer quelle est, pour chaque soirée d'observations, la valeur de l'épaisseur réduite de l'ozone atmosphérique, et nous obtenons ainsi, accessoirement, des résultats intéressant la géophysique"[7]. Then auroras are not really studied for themselves, but because of the possibility to observe the sun by the means of the detection of solar particles in their interaction with the terrestrial atmosphere: "L'émission de particules électrisées par le soleil pose un problème très intéressant en soi-même et aussi à cause de l'influence qu'elle pourrait avoir sur l'équilibre des couches extérieures du soleil. En outre, la théorie pourrait montrer quel est le rôle de ces particules dans la formation des raies brillantes de certaines étoiles chaudes et des nébuleuses. […] Le seul phénomène qui se présente à

[5] Paul Villard, "Les rayons cathodiques dans le champ magnétique", *Le Radium*, 1906, 3, n°4, 97-106; "Les rayons cathodiques et l'aurore boréale", *Journal de Physique Théorique et Appliquée*, 7,1908, 429-453.

[6] Paul Villard, "Sur l'aurore boréale", *Journal de Physique Théorique et Appliquée*, 4,1908, 1-2.

[7] Daniel Barbier, Daniel Chalonge, "Recherches sur l'ozone atmosphérique", *Le Journal de Physique et le Radium*, 1939, t.10, n°3, p.113.

l'état pur comme dû avec certitude aux particules émises par le soleil est l'aurore polaire. C'est à ce seul phénomène que nous allons demander des renseignements sur la nature des particules et en particulier sur le signe de leur charge et leur vitesse."[8] The same conclusions can be made about the method employed by Jean Cabannes, another student of Charles Fabry, and renowned for his experimental confirmation of the law of Rayleigh concerning the scattering of light by gases in the 1920's: for him, and his students after him, auroras and atmospherical phenomena are observed under the prism of the scattering of light. Indeed, atmosphere is therefore the place of the interactions of light and matter, permitting an easy study of light by the way of zodiacal light, light of the night sky or auroras. During a long part of his career, Cabannes applied recommandations made by Fabry at the beginning of the 20th century: "Le phénomène de la diffusion de la lumière par les gaz, dont la théorie a été faite par Lord Rayleigh, a conduit à l'explication de la lumière bleue du ciel. Je me propose de montrer que la même cause peut être invoquée pour expliquer un certain nombre de phénomènes naturels, et cela en faisant intervenir seulement des gaz dans un état d'extrême rarefaction"[9].

But, if practices are concerned, it will be interesting to focus the attention on Alexandre Dauvillier. For him, auroras were integrated in a new discipline: cosmic physics. Seen as a unitary theory, cosmic physics was built as a science of coordination and as a synthesis between astrophysics and geophysics. Auroras were so explained in the perspective of interactions between electrons projected by the sun, and terrestrial atmosphere. Dauvillier put the emphasis on cosmic physics as a field discipline: for instance, he wrote about Villard that "L'illustre physicien manquait de l'expérience des régions polaires et il sera toujours quelque peu imprudent de proposer une théorie de ce phénomène sans l'avoir longuement observé"[10] . He refused as well a too geometrical treatment of natural phenomena, a too theoretical explanation of them: "La théorie de P. Villard a été discutée par C. Störmer. Comme on le voit, une véritable théorie de l'aurore était édifiée, mais cette conception était trop théorique et les aurores ne présentent pas, dans la réalité, cet aspect géométrique simple."[11]

For Dauvillier, polar expeditions were a necessary step in the learning of a geophysicist because he saw his 'cosmic physics' as a science of adventure. He wrote about the training of students:

> "L'étude des aurores boréales les fera voyager et hiverner dans les régions polaires. Pour ma part, le souvenir de trois hivernages vécus, pendant la longue nuit polaire, en Laponie ou au Grönland, m'enthousiasme encore, et je n'ai pas renoncé au projet, caressé depuis longtemps, qui devait nous faire, le capitaine de frégate Habert, quelques collaborateurs et moi, séjourner toute une année exactement au pôle sud ! A l'exemple de leurs aïeux découvrant, au siècle dernier, le magnétisme du Globe, l'étude des rayons cosmiques leur fera parcourir les océans, gravir de hauts sommets, s'élever dans la stratosphère, en ballon ou en avion, s'enfoncer dans les mines profondes ou plonger en sous-marin. Je ne connais pas de programme d'action plus séduisant, offert à l'activité d'un homme de science, que cet alliage de recherche patiente et réfléchie, d'activité physique et de contemplation de la nature."[12]

He was himself implicated in numerous travels, such as the second International Polar Year in 1932-1933, during which he stayed 14 months in Scorseby Sund. But Dauvillier first travelled in 1931 to Sodankylä (Finnish Lapland) where he stayed few months. In 1934, he worked in Arosa (Switzerland), and returned to polar regions in 1934-1935, for a wintering in Abisko (Sweddish Lapland) with Daniel Barbier and Daniel Chalonge. But these scientific expeditions, characterising a field approach of his cosmic physics, are necessarily accompanied by laboratory experiments. Further researches will have to be raised to question the epistemological specificity of the science developed and promoted by Dauvillier. It can already be said that the lab experiments conducted by Dauvillier try to precise secondary phenomena occurring in the production of auroras: if his auroral theory is based on secondary electrons, the problem is not yet solved because of the lack of understanding of the permanent emission of high-energy corpuscles, of the nature of the exosphere, of the nature of secondary electrons emitted by this atmospheric layer, and of the turbulent regime that exists in those places where impacts between solar and terrestrial corpuscles occur. To precise the effects of these phenomena, Dauvillier was conducted to build an animated model named 'Terrella', allowing the reproduction of auroras according to the time and to the moment in the year. He could find important results with this model, such as the permanence and the rotation of the terrestrial corona, a luminous zone corresponding to the auroral zone, its changing dimension –larger in summer than in winter– and the displacement of the

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8 Daniel Barbier, "L'émission de particules électrisées par le Soleil et la théorie des aurores polaires", *Le Journal de Physique et le Radium*, 1937, vol.8, p.303.

9 Charles Fabry, "Remarques sur la diffusion de la lumière par les gaz", *Journal de Physique Théorique et Appliquée*, 1917, vol.7, .p.7.

10 Alexandre Dauvillier, *Le magnétisme des corps célestes. T.3 Les aurores polaires et la luminescence nocturne*, Paris, Hermann, 1954, p.107.

11 *Ibid*., p.109.

12 Alexandre Dauvillier, *La physique cosmique*, Paris, Flammarion, 1951, p.12.

auroral pole. His model was then used in a pedagogical way: Jean Perrin asked him to build an auroral synthesis device allowing the visualization of the path of the cathodic beam in the magnetic field of the earth.

As a conclusion, it can be said that the French approach of auroras is still to be questioned. Each of the styles seen above articulated various sites: laboratory, mountain observatories, polar and desert expeditions. Auroras allow science historians to go further in the description of the connections between the search of precision and universality by the use of multiple and singular sites[13]. Auroras will have, as well, to be integrated in the study of other boundary objects such as cosmic rays, ozone, zodiacal light, light of the night sky, because of their historical interest in the construction of hybrid sciences as astrophysics, geophysics and cosmic physics, and because of the possibility to know more about the emergence of complexity and interdisciplinarity at this moment.

[13] See for instance: LE GARS, Stéphane, AUBIN David "The Elusive Placelessness of the Mont-Blanc Observatory (1893-1909): The Social Underpinnings of High-Altitude Observation", *Science in Context*, 22,3, 2009, p.509-531.

THE ROLE OF THE HISTORY AND PHILOSOPHY OF SCIENCE AND TECHNOLOGY FOR ANALYSIS OF THE PHENOMENON OF SCIENTIFIC AND TECHNOLOGICAL SCHOOLS IN THE MODERN KNOWLEDGE SOCIETY (WORLD CONTEXT)

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THE HISTORICAL DEVELOPMENT OF LASER TECHNOLOGY AS AN EXAMPLE OF NON-CLASSICAL ENGINEERING SCIENCE[1]

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Abstract

Laser is one of the important inventions of the 20th century. The creation of laser leads to the development of novel technological processes, new branches of industry and origin of the principal new mode of communications, which are very important for knowledge society. It is representative that in the 20th century five Nobel prizes for physics were given for laser investigation. It is well-known that classical engineering sciences are closely connected with natural sciences (especially with physics). At the same time engineering sciences have their own methodology and their own specific mode of knowledge organization –technological or engineering theory. The development of technological theory and practical applications of laser technology were going parallel. But the technological theory in laser technology is based on non-classical physics and can be regard itself as the new type of non-classical technology. This is the history about the non-linear direction from theoretical scientific knowledge to technological applications and the important role of scientific schools.

Formation and development of laser technology as an object of social and methodological analysis is chosen deliberately, because in our opinion laser technology represents a typical example of construction of a non-classical technical theory when “pure” science and engineering experiment are developed simultaneously, mutually enriching each other.

Let us review in more details the way of transformation of quantum physics theoretical postulates into one of the most considerable branches of modern technical theory –the theory of laser and optical-electronic devices and systems.

For the solution of this problem it is necessary to track all the way from emerging of an idea to its practical embodiment in the device or technology. And, in this case it is not a matter of separate inventions, discoveries and innovations, but rather a matter of formation of a highly-developed scientific and technical discipline with all its distinctive features: ascientific and engineering community, developed industrial structures, etc.

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The approach offered by V. Gorokhov was a methodological basis for our research[2]. In this approach, it is important to take into account the external social conditions, apart from the internal logic of scientific development.

The importance of Einstein's ideas for the development of quantum electronics

In the beginning of the 20th century optics already had a rich history. Here we will not review a detailed analysis of the achievements of many great scientists, as they are well-known and described in detail many scientific and popular articles and books written "from within", i. e. by physicists, as well as "from the outside", i.e. by philosophers and methodologists of science.

We will review the origin of the theory of laser and optical-electronic devices and systems from the moment of occurrence of the idea which has subsequently become the key idea for this science and technology.

The basis of this research was brought to science by Einstein[3]. Actually, in 1905 he published an article "About one heuristic point of view, concerning occurrence and light transformations". In this article, the possibility of the existence of light quanta –photons– was considered. Although by then the concept of "energy quantum" was already known enough, introduced by M. Planck, Einstein's idea was negatively accepted by the majority of the leading physicists of that time. In many respects it was connected with the tremendous success of Maxwell's theory and its experimental confirmation by Hertz. In other words, the wave concept was the prevailing point of view on the subject of light nature. Therefore Einstein was exposed to numerous criticisms and sneers. Max Born, friend and colleague of Einstein, wrote "Nobody took Einstein's light quanta seriously" [4]. This citation illustrates the scepticism about light quanta which was general at that time.

However, the possibility of explaining the red threshold of photo effect only by quantum theory of light[5] made all physicists accept these "strange light quanta", at least as a working hypothesis.

Approximately since 1913 N. Bohr's publications about the quantum theory of electronic orbits in atoms and about the origin of spectral lines started to appear; in 1922 the Compton effect was discovered –all of them strengthening the position of photons theory. In 1916 Einstein published a small article[6], which was recognized as basic theoretical work on quantum electronics more than 40 years after its publishing. In this article Einstein indicated that besides spontaneous radiation of energy by molecules or atoms, induced radiation from a compelled substance also existed.

In classical physics it is considered that under the influence of electromagnetic radiation the atom passes from level E1 (low) to level E2 (high), thus there is energy absorption. The raised atom can spontaneously pass to one of the underlying levels of energy, thus radiating a light quantum. Light waves radiated by warm bodies are formed as a result of such spontaneous transitions of atoms and molecules. Spontaneous radiation of various atoms is not coherent. It is considered that the normal condition of atoms is unexcited (E1), and accordingly the majority of atoms are in it. Einstein showed that radiation of a different type is possible –induced radiation, i.e. transition E2 to E1. Thus it is not important how the atom is transferred to higher power level. The light wave is amplified as a result of this transition. Moreover, the induced quantum doesn't differ from an inducing photon: they coincide in propagation direction, in polarization, and in frequency. Electromagnetic waves, formed of such quanta, are considered coherent. This fact was proved by P. Dirac in 1927 [7].

And the last basic theoretical position, which confirms that there can be unlimited number of quanta in one oscillator field, was proved by Einstein in 1925[8].

These are the basic theoretical preconditions of emergence of quantum electronics and laser technique. But it was still quite far from practical realization of these postulates. The majority of physicists of that time regarded the above described postulates as a certainly "impracticable" part of the theory, as a rather curious consequence of the quantum doctrine, as a certain incident. It can be easily explained if we take into account the fact that a medium with inverse density of population (i.e. such medium in which the number of the excited atoms exceeds number of atoms at the bottom power level) does not exist in natural physical conditions. And actually scientists of that time were focused in a greater degree on

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[2] Gorokhov V. G. *The social and methodological analysis of technical sciences*, St. Petersburg, 2009, (in Russian).

[3] Camejo, S. A. *Skurrile Quantenwelt*, Berlin, Heidelberg, New York: Springer, 2006, (in German).

[4] Born M. Einstein and the quantum of light. Moscow, *Physics-Uspekhi* (*Advances in Physical Sciences*), V. LIX,1,1959 (in Russian).

[5] Camejo, S. A. *Skurrile Quantenwelt*, Berlin, Heidelberg, New York: Springer, 2006 (in German).

[6] Einstein A. Zur Quantentheorie der Strahlurig, *Phys. Zs*. 18, 121 (1917). *Mitteilungen d. Phys. Ges*. Zürich, Nr. 18 (1916) (in German).

[7] P. Dirac. The Quantum Theory of the Emission and Absorption of Radiation, *Proc. Roy. Soc*., London, 1927, Ser. A. V. 114, p. 243-265. *http://home.tiscali.nl/physis/HistoricPaper/Dirac/Dirac1927.pdf* (in English).

[8] A. Einstein, Quantentheorie des einatomige idealen Gases, Sitzungsber. *Preuss. Akad. Wiss*. 22, 261 (1924) and 23, 3 (1925) (in German).

problems of another sort. However, there are always a few people who solve a problem considered unpromising by others. Actually, we will speak further about them.

Experimental confirmation of existence of inverse medium and possibility of compelled radiation

In 1928 Rudolf Ladenburg and his pupil Hans Kopferman, while studying dispersion in the electrical excited gases, experimentally observed a negative dispersion in a helium and neon mix that showed the possibility of creating a medium with inverse density of population[9].

Indirect experimental acknowledgement of the possibility of creating mediums with inverse density of population also confirmed the basic possibility of compelled radiation. But Ladenburg worked with the spectroscope and he used to experiment with natural (i.e. spontaneous) electromagnetic radiation and, therefore, he could not estimate prospects of generators of compelled radiation of electromagnetic waves, since compelled radiation was so weak that it could be neglected in favour of spontaneous radiation. This result remained in the investigated area of science and technology only as "a curious fact".

Works of Soviet physicist Valentin Fabrikant became the next period in the development of laser technique [10].

V. Fabrikant graduated from the physical and mathematical faculty of the Moscow State University, and was a pupil of Sergey Vavilov. Basic questions in his sphere of interests were connected with optics of gas discharge and, more precisely, with the phenomenon of electroluminescence and creation of practical samples of luminescent lamps. During this research, Fabrikant became interested in the possibility of strengthening optical radiation at its passage through a gas medium. In 1939 Fabrikant defended his doctoral thesis, where apart from generalization of Ladenburg's experimental results, he also described situations in which induced radiation would prevail over the spontaneous one. Besides, Fabrikant directly specified mediums where it is possible to create inverse density of population, i.e. he specified the mediums which strengthened, instead of absorbing, passing radiation [11].

However, these results had no practical outcome for the achievement of particular objectives; in addition, the Great Patriotic War began, and no special attention was paid to the works of Fabrickant. After the war Vladimir Fabrikant together with the laboratory worker Fatima Butaeva renewed the work on research on optics of gas category and mediums with negative dispersion (1947)[12]. But the works of Fabrikant and his colleagues fully contradicted the dominating approaches of that time[13].

It is easy to assume that such a reaction of guild fellows for a long time postponed recognition of the importance of this research. Scientific magazines refused to publish articles about substances with negative light absorption, and their authors were in an unenviable position.

Actually these incidents represent an impressive example of an inopportune discovery, as well as the influence of the paradigm (in manner of Th. Kuhn) and the academic environment on propagation of scientific ideas.

As a matter of fact, the main operation principles of the instrument (which was afterwards named after the English acronym LASER –Light Amplification by Stimulated Emission of Radiation) were formulated.

The word "laser" was used for the first time by Gordon Guld, post-graduate student of Columbia University. Generally Guld's story is quite interesting, and it could be an example of academic community influence. In November, 1957 he completely single-handedly described and designed an instrument now known as laser. However, without significant authority in scientific circles he was afraid to publish the results in the form of a scientific article. However he notarized all the calculations and drawings. It didn't bring him scientific glory, although on the basis of these records Guld was able to receive a set of patents connected to the usage of lasers that made him rich afterwards.

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[9] Ladenburg R. Dispersion in electrical exited gases, Moscow, *Physics-Uspekhi* (Advances in Physical Sciences), 1934, V. XIV, 6 (in Russian).

[10] Fain, W. M.; Chanin, J. I. Quantenelektronik. *Phzsik der Maser und Laser*, Leipzig: B. G. Teubner Verlagsgesellschaft, 1969 (in German).

[11] Biberman L., Valentin Fabricant (To the sixtieth anniversary from the date of a birth), Moscow, *Physics-Uspekhi* (*Advances in Physical Sciences*), 1967, V. 93, 4 (in Russian).

[12] Fabrikant V. Some questions of optics of the gas discharge, Moscow, *Physics-Uspekhi* (*Advances in Physical Sciences*), 1947, V. XXXII, 1 (in Russian).

[13] Butaev B., Morosov D. Fatima Butaeva: At sources of creation of the laser, Moscow, Science *and life*, 2007, 12. - *http://www.nkj.ru/archive/articles/12350* (in Russian).

Probably, if S.I.Vavilov, who always supported V.A.Fabrikant, hadn't died in 1951 the Nobel committee nomination of 1964 would have included other names. And it is likely that the invention of laser would have happened a bit earlier.

But history doesn't know of a subjunctive mood. Therefore we will review the further development of the ideas which led to the creation of laser technology.

In 1950 the French optician Alfred Kastler offered a method of optical pumping of a gas medium, which later (in 1966) received the Nobel Prize[14].

Despite the impressive success in research of the nature of electromagnetic radiation, the key step in creation of generators of light in optical band wasn't made yet! Not least of all, it happened because almost all researches in this area were led by scientists-opticians, and for them amplifying of optical radiation was represented by a natural part of the optical system, and the basic restriction for the development of these instruments was the dominating paradigm, as this amplifier didn't present any independent value for them.

Creation of the first generators of microwave and optical radiation

The solution for the objective of creating optical radiation generators, which was not yet formulated, came from the adjacent area of physics –radio-spectroscopy.

Radio-spectroscopy is an area in radio physics which was developed by outstanding Soviet scientists L. I. Mandelshtam and N. D. Papaleksi. In their oscillations laboratory in the beginning of 1939 worked A. M. Prokhorov[15]. In 1946 he defended his master thesis, and his doctoral thesis in 1951. The possibility of synchrotrons' usage for generation of millimetre waves was the subject of Prokhorov's research, the results of which were included into his doctoral thesis.

A. Prokhorov began to work in the field of spectroscopy of gases almost simultaneously with research in the area of accelerators physics. An object of research is rotational and oscillating spectra of molecules. It is necessary to note that radio spectroscopy started to develop vigorously after the Second World War. Not least of all, it is connected with the fact that for the research of gas spectra well-developed applied methods of radiolocation and radio engineering were used.

The main challenge for physicists consisted in the impossibility of creating a sensitive enough method to register the absorptive passages in molecules because of molecule concussions in gas and the Doppler Effect, i.e. it was not possible to take sufficiently precise measurements. This matter was worked on by A. Prokhorov and his pupil and colleague N. Basov. Actually, they solved the objective of resolution enhancement of radio spectroscopes. During this work there was the assumption that broadenings of the spectral lines (because of molecule concussions and the Doppler Effect) can be avoided if instead of gas a bundle of molecules was used.

Actually, it was the beginning of quantum electronics development, but it is interesting to note that the idea of a quantum generator emerged as a by-product of the activity which was aimed at the solution of an absolutely different objective.

Two articles (J. P. Gordon, H. J. Zeiger, and C. H. Townes "Molecular Microwave Oscillator and New Hyperfine Structure in the Microwave Spectrum of NH3"[16], and N. G. Basov, A. M. Prokhorov "The Molecular generator and amplifier"[17]) were published in 1954, absolutely independently from each other, with a difference in some months.

These articles were of theoretical nature, and the device –a molecular generator which radiated on wavelength of 1,25 cm, or the ammoniac maser (from English Microwave Amplification by Stimulated Emission of Radiation)– was created independently in 1954 by C. Towenes' group in the US Columbia University and by A. M. Prokhorov and N. G. Basov in the Moscow Physical institute.

So, we will sum up. In 1954 A. M. Prokhorov and N. G. Basov, and C. Townes, J. Gordon and H. Zeiger[18] offered methods of formation of molecular bundles with subsequent sorting of excited and unexcited molecules and passage of bundle of excited molecules through a cavity resonator. Then for the first time it became possible to integrate the idea of

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[14] He received his award "for the discovery and development of optical methods for studying Hertzian resonances in atoms".

[15] All biographical data of Prokhorov are taken from: Aleksandr Mikhailovich Prokhorov. *Memoirs, articles, interviews, documents*, Ed. Shcherbakov IA– Moscow, 2006; Michailova GN, Osiko VV, Scientist of encyclopedic. Experience of collective portrait. On the ninetieth anniversary of Academician AM Prokhorov. – *Bulletin of RAS*, 2006, V. 76, 9, p. 823 – 833; Aleksandr Mikhailovich Prokhorov (on his fiftieth birthday) – *Physics-Uspekhi* (*Advances in Physical Sciences*), 1966, V. 89, 3. – p. 521-525 (in Russian).

[16] *Phys. Rev*. 95, 282–284 (1954) http://prola.aps.org/abstract/PR/v95/i1/p282_1 (in English).

[17] *Physics-Uspekhi* (*Advances in Physical Sciences*), V. LVII, issue 3, 485-501 (1955) (in Russian).

[18] Fain, W. M.; Chanin, J. I. Quantenelektronik. Phyzsik der Maser und Laser, Leipzig: B. G. *Teubner Verlagsgesellschaft*, 1969, 12-13 (in German).

induced radiation and inverse density of population with the ideas representations about resonators, feedback and generation of coherent electromagnetic radiation.

During the same period A. M. Prohorov together with N. G. Basov developed an exhaustive theory of molecular generator and radio frequency radiation amplifier. Then they patented a new method of creation of inverse density of population –optical pump under three-level circuit[19].

Nevertheless, it was 5 years before the creation of generators in optical radiation. There were a number of reasons for that. The first was described by C. Townes in his Nobel lecture very explicitly:

> "Until about 1957, the coherent generation of frequencies higher than those which could be obtained from electronic oscillators still had not been directly attacked, although several schemes using molecular-beam masers for the far infrared were examined from time to time. This lack of attention to what had been an original goal of the maser came about partly because the preliminary stages, including microwave oscillators, low-noise amplifiers, and their use in various scientific experiments, had proved so interesting that they distracted attention from the high-frequency possibilities"[20].

The other two problems of passage of radiation in optical band were mentioned by A. M. Prokhorov in his Nobel speech:

> "One might think that after the successful construction of masers in the radio range, there would soon be made quantum oscillators (lasers) in the optical range as well. However, this did not occur. Those oscillators were constructed only 5-6 years later. What caused such a delay? There were two difficulties. One of them was as follows: at that time no resonators for the optical wavelength range were available. The second difficulty was that no methods were immediately available for gaining an inverse population in the optical wavelength range."

Let us consider firstly the question of resonators. It is well-known that radio engineering started its development from the region of long waves, where resonators were used in the form of self-inductance coils combined with condensers. In that case the size of the resonator is much less than one wavelength. With the development towards short waves cavity resonators were used. They are closed volume cavities. The size of those cavities was comparable with a wavelength. It is quite clear that with the help of such cavities it is impossible to advance into the region of very short waves. In particular, it would be impossible to reach the optical range.

In 1958 it was proposed the so-called open type of cavities for masers and lasers in the region of very short waves. Practically speaking, this is Fabry-Perot's interferometer; however, a «radio engineering» approach made it possible to suggest the use of such a system as a resonator. Afterwards, spherical mirrors were used together with plane mirrors. The size of these resonators is much more than that wavelength"[21].

Alexander Mihajlovich was modest because it was him who offered to use Fabry-Perot's interferometer, widely known in the world of optics, as open resonator in June, 1958. The basic function of this device consists in the selection of radiation depending on the wavelength[22].

For the sake of justice it is necessary to mention that in the same 1958, although in December, A. Schawlow and C. Townes in the magazine *Physical Review* published the article «Infrared and optical masers». In abstract of this article it was specified:

> "The extension of maser techniques to infrared and optical region is considered. It is shown that by using a resonant cavity of centimeter dimensions, having many resonant modes, maser oscillation at these wavelengths can be achieved by pumping with reasonable amounts of incoherent light. For wavelengths much shorter than those of the ultraviolet region, maser-type amplification appears to be quite impractical. Although use of a multimode cavity is suggested, a single mode may be selected by making only the end walls highly reflecting, and defining a suitably small angular

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[19] N. G. Basov, A. M. Prokhorov. About possible methods of reception of active molecules for the molecular generator, *JETP*, 1955, V. 28, 249,250 (in Russian).

[20] Charles H. Townes. *Production of coherent radiation by atoms and molecules*. Nobel Lecture, December 11, 1964, p. 70 *http://nobelprize.org/nobel_prizes/physics/laureates/1964/townes-lecture.pdf* (in English).

[21] A. M. Prochorov. *Quantum electronics*. Nobel Lecture, December 11, 1964, p. 113-114. *http://nobelprize.org/nobel_prizes/physics/laureates/1964/prokhorov-lecture.pdf* (in English).

[22] Kojevnikov A. B., Mokrova M. V. Interview with A. M. Prokhorov, *Questions of history of natural sciences and technology*, 2003, 4, p. 105-127 (in Russian).

aperture. Then extremely monochromatic and coherent light is produced. The design principles are illustrated by reference to a system using potassium vapor"[23].

As to the second difficulty, the way out of it lay on a way of an uncountable set of researches of spectroscopic properties of various materials. Gases (we will recall Fabrikant) and crystals were the most researched at that moment. Therefore the main experiments were made with them.

The first generator of optical radiation was created on the crystalline active medium. Laser was constructed in May, 1960 in the USA by Theodore Maiman. As the active medium it used an artificial ruby crystal (aluminum dioxide with chrome impurity) which was pumped by means of a normal flashlamp, and as the resonator a mirror covering put directly on polished end faces of the crystal was used. Maiman's Laser was impulse and generated a wavelength of 0,6943 microns, i.e. radiated red light[24].

In the same 1960, in December, employees of the "Bell Labs" laboratory Ali Javan, William Bennett, and Donald Herriott showed helium-neon laser where inverse density of population was created by electrical discharge in a low pressure gas compound[25]. It is necessary to note that the construction of this laser almost completely repeated that of Fabrikant, Butaeva and Vudynsky.

All these events meant that the basic boundary was crossed. Physicists and engineers managed to receive radiation generation in optical band. This stimulated radiation differed from the known before spontaneous variety of characteristics (monochromaticity, coherence, directivity and brightness). And these characteristics allow very active industrial use of lasers. It is necessary to note that the creation of new lasers continues nowadays too, but it is already a routine scientific and technical activity.

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In the end of the article we'd like to mention once again that the theory of laser and optical-electronic technology represents a non-classical scientific and technical discipline. It initially progressed not under the influence of a single basic scientific theory, but under the influence of an interdisciplinary area of research, based on the data of various technical and theoretical models. Simultaneously with scientific research industrial research was carried out as well, and both types of researches mutually influenced each other. As a matter of fact, the theory of laser and optical-electronic technology was one of the first scientific and technical theories of the new class, and consequently the analysis of its formation is deemed important for understanding the logic of scientific and theoretical knowledge development.

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[23] A. L. Schawlow and C. H. Townes, "Infrared and Optical Masers", *Phys. Rev*. 112, 1940–1949 (1958) http://prola.aps.org/abstract/PR/v112/i6/p1940_1 (in Englich).

[24] T. H. Maiman, Stimulated Optical Radiation in Ruby, *Nature* 187, 493 - 494 (06 August 1960) (in English).

[25] Bell Labs Historical Contributions to Laser Technology: *http://www.bell-labs.com/about/history/laser/contrib.html* (in English).

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KNOWLEDGE SYSTEMS IN FLUX: SWEDISH PHILOSOPHY OF SCIENCE AND HUMANITIES DRIFT APART. A HISTORY OF THE CHALLENGE OF THE MODERNIST WORLD VIEW

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Abstract

This paper focuses on the concept of objectivity to illuminate the interdisciplinary negotiations between academic subjects in Sweden from the 1960s to the present. The concept of objectivity held a central theoretical space in almost all academic subjects in Sweden during the postwar period. A salient person in the discussion of the 1970s was Gunnar Myrdal who had focused on the relation between values and the social sciences since the 1930s. For Myrdal, value premises worked as a positive normative ideal. In An American Dilemma *he referred to the American democratic "creed" as a substantial inspiring framework for progressive social change to solve American racism. In 1968 Gunnar Myrdal published* Objectivity in Social Research. *Values were presented as obstacles –yet possible to overcome if the researchers openly declared their valuations. Myrdal's book is one of the first examples of how the concept of objectivity became an engaging topic for debate in the academic circles during the 1970s. Myrdal's view fitted into the traditional view to represent science as a vocation –as Max Weber famously expressed it.*

A suggestion is that the concept of objectivity functioned as a "prism" that upheld a shared academic public space between philosophy, natural sciences, humanities and social sciences. Three factors –the dominance of the philosophy of science within philosophy, Gunnar Myrdal's lifelong interest in values and the new left movement's attack on the belief of an objective science– made the question of scientific objectivity to a public and scholarly issue for debate in Sweden and contributed to uphold a shared communicative space.

However, during the 1980s the discussion about objectivity went out of tune. New competing knowledge systems emerged with hermeneutics, discourse theory, marxism and Thomas Kuhn's paradigm theory. They supplied the humanities with new theoretical tools and the vocabularies were drifting apart.

As a result, a gap between analytic philosophy's quest for impartial truth vs. humanities' and social sciences' defense for including history, values and local practices in the analysis emerged. The new generation of people in humanities founded their own philosophies, and neglected the established analytic philosophy. However, in the last decade questions about objectivity have arisen anew, in medical research and climate science. That might indicate that a renewed scholarly public conversation between different academic topics will have space for an alerted philosophy of science.

Introduction

During the last decades philosophy of science and history of science have drifted apart in Sweden. This state of affairs provides the point of departure for this article, which focuses on the intellectual postwar history of philosophy of science in the Anglophone world and Sweden. The specific question is: How did analytic academic philosophy become isolated from the other humanities? One salient feature has been the questioning of relevance of the analytical philosophy of science by the other humanities, particular history of science. According to humanities in general and history of science in particular, the ambition to be a strong scholarly allied to the natural sciences, since the interwar period have resulted in a technical formal philosophy of science. Analytic philosophy of science proceeds its task as sciences would consists of pure rational content; hence not reflecting how much the sciences cognitive content is determined by its practices, let it be laboratory practices or machine availability.

A thesis in this article is that the tension and the resulting gap can be interpreted in terms of conflicting world views, not only in different cognitive content. The logical positivists and Karl Popper shared a belief in modernity's prime virtues; rationality and reason were incarnated in science. Science was the new frontier against old-fashioned obscure metaphysics represented by idealistic history and philosophy.

The article starts with presenting the logical positivism in Vienna and Uppsala, and in particular arguing for that their philosophical framework was intertwined with a belief in modernity. Philosophy in the shape of the Vienna circle was a part of the European modernism which settled the agenda for the twentieth century Western culture. Usually architecture, art and changing social life styles are considered as the prime carriers of modernity. The thesis is that analytical philosophy of science had a "modernist framework" that helps to understand the rise and the still existing gap between analytical philosophy and its counterparts in the humanities. Rudolf Carnap's *The Logical Construction of the World* illustrates this modernist creed. Also Karl Popper –the logical positivists' first combatant– belonged to a modernist world view; they shared the belief in sciences as the highest knowledge standard, distinguished by its rationality. At the famous colloquium in London 1965 the clash in world views was evident. Karl Popper labeled Thomas Kuhn's view on scientific change as "mob-psychology."

During the twentieth century Swedish philosophy was influenced by the Vienna circle as well as its successor analytical philosophy, a development it shared with the US. A strong component in this common framework was the modernist belief in scientific knowledge as value free. In the nineteen sixties new competing knowledge system emerged; hermeneutics, discourse theory, Marxism and Thomas Kuhn's paradigm theory. They supplied humanities with new theoretical tools. Analytic philosophy of science had to leave room for diverging vocabularies with deviating content. The analytic philosophy of science kept to the modernism belief in the sciences as superior form of knowledge, representing the highest available forms rationality and objectivity.

As a result, a gap emerged between analytic philosophy's quest for impartial truth and the humanities' and social sciences' defense for including history, values and local practices in the analysis. The new generation in humanities founded their own philosophies, and neglected the established analytic philosophy. Instead they highlighted that social practices and material conditions were cognitively relevant for understanding the history of science, a view out of tune with modernism. The article ends by discussing what sustains this academic gap that emerged that I claim is a result of a clash between different world views.

The Logical Positivists in Vienna and Uppsala

The first generation of logical positivists shared a conviction that philosophy of science mattered beyond the academy. The position has been stressed in recent research about the logical positivists (Giere & Richardson, 1996; Heidelberger & Stadler, 2002). The core circle of logical empiricists consisted of Moritz Schlick, Rudolf Carnap, Kurt Gödel, Philipp Frank (brother of Joseph Frank), Otto Neurath, Hans Reichenbach, and Carl Hempel. They repudiated parts of the existent academic philosophy, claiming that it represented a collection of mainly non-testable propositions, infused by metaphysical assumptions (Reisch, 2005). The modern task as philosophers of science was to design conceptual tools to evaluate knowledge claims, and hence demarcating scientific knowledge from less certain knowledge. However, the application was not restricted to science areas. The philosophically elaborated criteria of meaning, testability of facts and theories was intended to be applicable beyond the academic circles. Philosophy of science was a firm allied in the urgent contemporary fight against political persecution and antidemocratic movements, present in Europe in the 1920s. The positivists were radicals viewing science as an allied to create a better future. All of them more or less, forged their institutional connection to various cultural and political commitments.

The members of the Vienna circle viewed the natural sciences as a forceful weapon against the reigning obsolete idealism in humanities –and expanded the task of the philosophers to clean philosophy from outdated beliefs in philosophy. In 1929 Rudolf Carnap wrote in the preface to *Aufbau*:

> Whence then our confidence that our call for clarity, for a science that is free from metaphysics will be heard? It stems from the knowledge or, to put is somewhat more carefully, from the belief that these opposing powers belong to the past. We feel that there is an inner kinship between the attitude on which our philosophical work is founded and the intellectual attitude which presently manifests itself in entirely different walks of life; we feel this orientation in artistic movement, especially in architecture, and in movements which strive for meaningful forms [*Gestaltung des menschlichen Lebeen*] of personal and collective life, of education and of external organization in general. We feel all around us the same basic orientation, the same style of thinking and doing… Our work is carried on by the faith that this attitude will win the future (Carnap, 1967).[1]

The quote illustrates how the Vienna circle viewed the value system of science and culture; namely to be closely intertwined. The Viennese philosophers viewed their task to design philosophical tools intended to sort out rational knowledge from irrational metaphysics. According to them Europe more than ever needed criteria for meaning, facts and truth to defend political and social liberty against anti-democratic movements. They were radicals who considered the sciences as their allies to create a better future.

The Uppsala version of the Vienna circle distinguished itself for analytical truth seeking, focus on scientific epistemology and a belief in the accumulation of scientific knowledge and a hostile attitude to metaphysics. In the search for a unifying scientific principle the natural sciences were the allies that detached the philosophers from nationalistic historians. In Sweden Axel Hägerström in the 1920s, well repudiated philosophy professor at Uppsala University, affiliated with the Social Democratic Party, was the first within the academic philosophy to pick up the new radical trends from Vienna circle. He conquered a public space for the Swedish philosophy at the beginning of the twentieth century. Philosophy and the Swedish Social Democratic party shared the spirit of the time: the modernistic and an anti-metaphysical attitude merged and supported by a deep belief in scientific progress.

Hägerström is famous for his "value nihilism", i.e. that judgments about good, bad, right or wrong could not be judged false or true. Values were expression for emotions, not objectively true. They belonged to metaphysics. Hägerström's motto expressed rhetorically his standpoint: "Metaphysics ought to be destroyed" (Cassirer, 1939).

In Sweden's other university, Lund university, logical empiricism was introduced by the journal *Theoria*, founded 1935 (Manninen & Stadler, 2009 p. 69). The following year, in 1936, Otto Neurath wrote a descriptive historical presentation of logical empiricism for the journal, published in the first issue translated into Swedish (Ibid. p. 78.) In both countries, Austria and Sweden, philosophy of science was a part of a social movement to modernize and democratize the present society.

When the Neo positivist synthesis of formal logic and empiricism was questioned by Karl Popper in 1935, the basic problems of epistemology were re-casted. Roughly, instead of verification as the principal scientific methodological tool, falsification was suggested as the main scientific principle. If a proposed theory resisted falsification, it had to be accepted. Yet, a scientific attitude regarded all scientific knowledge as falsifiable in principle; an ultimate truth for science was neither needed, nor desirable (Popper, 1935). Popper distinguished the scientific method as the hallmark for scientific knowledge; the method generated the border between science and other forms of knowledge. However, even if Karl Popper represented an epistemic break with the logical positivists, the continued focus was on formal logic and empiricism. Another strong continuity is the normative ethical framework, to view the sciences and philosophy of science as the modernist challenger against conservatism and antidemocratic movements.

For long Karl Popper was quite unknown outside the established philosophers' circles. His international breakthrough came first in the 1960s. The two key events were, firstly, the positivism dispute which started in Germany 1961, when Jürgen Habermas made Popper to a positivistic icon, and secondly, when Thomas Kuhn's paradigm theory questioned the established story line of history of science, which Karl Popper was a representative for, as most Western philosophers at the time. At the symposium in London 1965 the different world views were distinctly exposed (Adorno, 1976; Lakatos & Musgrave, 1970).

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[1] Quoted from Galison "Construction Modernism: Cultural locations of *Aufbau*,", in Ronald N. Giere & Alan W. Richardson 1996, p. 34.

Philosophical modernism challenged: Kuhn against Popper at the London symposium 1965

One key event that made both Karl Popper and Thomas Kuhn to academic celebrities was the symposium Imre Lakatos arranged in London 1965.[2] The symposium arranged by Lakatos gave Thomas Kuhn's paradigm theory the salient position together with Karl Popper falsificationism. The other participants were mainly philosophers of science, among them Paul Feyerabend, Stephen Toulmin and Lakatos. No historian of science participated. In his talk Kuhn focused on the differences between him and Popper. For a reader of today it sounds astonishing to hear that Kuhn asserted that he and Popper were regarded as a kind of theoretical pair, especially on the logic of scientific discovery. Kuhn sets his task to elaborate the supposed differences. He emphasizes the scientists' commitment to a tradition, which is a crucial difference to Popper. If they would act as Popper claims, science would be in permanent revolution, which would make progress impossible. According to Kuhn, mature science is characterized by puzzle solving within a paradigm and not revolutions which only occasionally occur. Normal science represents genuine science and is characterized by puzzle solving. The puzzle solving is not an extra rational activity, but relies on the skills the scientist is trained to achieve by textbook reading and established laboratory practice. Scientists behave only as philosophers when they "must choose between competing theories" (Lakatos & Musgrave, 1970, p.7). Kuhn asserts that Popper's view of science is taken from those rare moments of revolution. Tyco Brahe is more representative in the history of science than Copernicus (*Ibid*. p. 5).

In connection to this claim Kuhn poses the question whether rationality is a non-historical concept. His example shows that the concept needs an historical contextualization. Kuhn asserts that the astrologers' explanation of their failure was not unscientific and irrational. Astrology was a craft with close resemblance to meteorology and medicine. Its theory "provided a rationale for various craft-rules which governed practice" (*Ibid*. p. 8.). However, still the theory was too wide, and lacked puzzles to solve.

Kuhn addresses the question under what circumstances does a scientist abandon a theory? His answer is forceful and attacks Popper's position. Popper's theory rests on the assumption that theories can be formulated as syntactical judgments. Doubting the possibility of theories in syntactical forms, he accuses Karl Popper to have produced "not logic of knowledge, but an ideology" (*Ibid*. p.15). Refutation of a theory requires it to be "fully articulated logically –in practice "no scientific theory satisfies these rigorous demands" (*Ibid*. p. 16). Sir Karl has "erred by transferring selected characteristic of everyday research to the occasional revolutionary episodes… after ignoring the everyday enterprise entirely" (*Ibid*. p. 19).

What Kuhn is asserting is that a prominent philosopher of science confuses theory with ideology, an astounded comment to a philosopher who had a solid reputation for warning against the political dangers of confusing science and politics, incessantly warning for that other than liberal political commitment represented an unceasing threat to an open liberal society. Another participant, John Watkins, summarized the different positions: "What is genuinely scientific for Kuhn is hardly science for Popper and what is genuinely scientific for Popper is hardly science for Kuhn" (*Ibid*. p. 29).

Stephen Toulmin is stressing the normative dimension of science, indicating that normative and descriptive views of science are unintentionally mixed together. Maybe "philosophy of science is concerned with the questions what consideration *should properly* determine the selection" … and "the psychology or sociology of science … that in fact settles the matter" (*Ibid*. p. 46). A taste of the coming science wars in the 1990s between the later founded "sociology of scientific knowledge" (SSK) and philosophers of science can be imagined.

In his witty and incisive reply Karl Popper rejects Thomas Kuhn's notion of normal science. He attacks the Kuhnian concept of normal science as representing the logic of historical relativism. His argument runs as follows. History of science doesn't support Kuhn's view (*Ibid*. p. 53ff). Normal science is not normal. Kuhn's thesis that sociology and psychology matters most in normal science rest on the wrong view that scientists are logically forced to accept the framework. The wrong assumption is that no discussion is possible between frameworks.

This thesis is logical, not factual. The myth of the framework is the central bulwark of irrationalism. Frameworks can be discussed. Comparisons between different frameworks are possible. Even totally different languages are not untranslatable. According to Popper "we are prisoners in a Pickwickian sense: if we try, we can break out of our framework at any time" (*Ibid*. p. 56). Popper ends by comparing science and religion. Even if intellectual revolution looks like religious conversion, we can evaluate critically and rationally. "Logic of Discovery has little to learn from Psychology of Research; the latter has much to learn from the former" (*Ibid*. p. 58).

Imre Lakatos is still more straightforward about the supposed negative implications of Kuhn's theory. His paper is the most extensive and discusses almost every detail in Kuhn's book, but the beginning sets the critical framework. According to Lakatos, Popper represents the critical rational knowledge standard in the sciences that should pave the way for the rest of

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[2] The proceedings are published in Imre Lakatos & Alan Musgrave (eds.) *The Criticism and Growth of Knowledge.* Cambridge: Cambridge University Press, 1970.

the society. It was a standard with flaws and failures, but the best available in the human world. In the aftermath, his comment on the differences between Popper vs. Kuhn seems almost clairvoyant in its prospective. Lakatos asserts that:

> The clash between Popper and Kuhn is not about a mere technical point in epistemology. It concerns our central intellectual values, and has implications not only for theoretical physics, but also for the undeveloped social sciences and even for moral and political philosophy … truth lies in power. Thus Kuhn's position vindicates, no doubt, unintentionally the basic political *credo* of contemporary religious maniacs ('student revolutionaries')… Kuhn doesn't understand a more sophisticated position … the rationality which is not based on 'naïve' falsificationism (Lakatos & Musgrave, 1970, p. 93).

Contrary to Lakatos' fear, Paul Feyerabend is pleased to find that he and Thomas Kuhn are on the same side, against the Popperians and the rest of the philosophy of science establishment. Feyerabend objects to Kuhn's view on normal science as puzzle solving within a monolithic theory. Normal science is not even a historical fact. He credits Kuhn for putting light on the "conservative and anti-humanitarian element in mature science", but Kuhn "has misinterpreted the relation of this element." Knowledge grows by the "active interplay of various tenaciously held views (*Ibid*. p. 203). In this view, he is not too far from Popper's falsificationism, which requires the possibility of easily emerging new bold hypotheses. Against the claimed rationality among the Popperians, Feyerabend argues that it is "not necessary arguments which cause us to adopt new standards." He writes:

> Moreover, it is likely that catastrophic changes, frequent disappointment of expectations, crisis in the development of our knowledge will change and, perhaps, multiply reaction patterns (including patterns of argumentation) just as an ecological crisis multiplies mutations. This may be an entirely *natural* process, like growing in size, and the only function of rational discourse may consist in increasing the mental tension that precedes *and causes* the behavioral outburst … Does not the occurrence of such a change show that science which, after all, is a part of the evolution of man, is not entirely rational and cannot be entirely rational? (*Ibid*. p. 217).

Feyerabend made the Popperians and the logical positivists to a common target. The failure of contemporary philosophy of science, inherited from the logical positivists, is this sulky insistence of rationality as the sole reigning force in science. In that claim he was one with the young generation who looked for a theory that situated science within societal context. Feyerabend disqualifies Kuhn for that position and reserves it for himself, displaying sensitivity for the spirit of the radical 1960s. In situating Karl Popper in the same theoretical camp as the logical positivists Feyerabend anticipated a dominant track in the 1970s radical student movement, namely "the positivism criticism." In Sweden almost no one outside the philosophers' circles held Popperianism and positivism apart.[3]

None of the other participants denied that science contained sociological aspects. The question was how much a sociological perspective mattered in philosophy of science, not if it mattered at all. The common concluding opinion underlined the secondary importance. Science had a permanently challenging character. What Kuhn did was to challenge this modernist dogma about sciences special progressive and ethical character. The elevated status of science was questioned.

Concluding remarks

The promotion of a scientific world view was an important part of the modernism in the beginning of the twentieth century. Modernism provided the sciences with a general framework that offered a place for sciences in the broader culture, also committed to social justice, political freedom and equality. In Europe these liberal secular values since the Enlightenment have been strongly intertwined with a deep belief in the sciences as a progressive liberating societal force.

The belief in reason –and the accompanied optimistic vision of a social and political justice in a progressive society with help from the sciences was the granted framework for the analytical philosophy of science in Sweden in the 1960s and 1970s. A close companion to the belief in reason and rationality as distinct features of science was the belief in objectivity. Taken together as a companion to the scientific world view, objectivity made a powerful case for the inevitable value of science.

Kuhn's paradigm theory together with Marxism and Foucault's discourse theory supplied the humanities and social sciences with new theoretical tools to challenge the modernist belief in progress, embedded in the analytical philosophical view. The humanities interpretation of Kuhn emphasized epistemic and moral value relativism and questioned the standard definition of rationality as a universal non-biased science property leading to knowledge progress.

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[3] Feyerabend's position is reflecting Jürgen Habermas position and the "Positivismusstreit" that had emerged in Germany 1961, see (Adorno 1976).

The outcome were different epistemic cultures, parallel but not communicating with each other. Philosophy of science and history of science developed in different directions. For history of science it was a successful separation. The research focus shifted from theory and concepts to practice, experiment, and laboratory and context dependence. A new theoretical vocabulary was imported from anthropology, sociology and history, not philosophy of science. Humanities developed their own philosophical legitimacy and competence, independent of analytical philosophy. A result is that no shared public academic exchange zone exists today between philosophy of science and humanities in Sweden.

A conclusion of the above story is that the philosophical content is not decisive for the public relevance or popularity. The emigrants from the Vienna circle lost their cultural sphere when they arrived to the US. Analytic philosophy of science lost its central role for the other humanities when the left winged inspired cultural meanings superseded modernism. Nowadays French continental philosophy is considered more available for the public by virtue of its existential dimension, despite its notorious indistinct style of writing. The popularity might depend on the fact that continental philosophy is tuned with the present world view, globalism in late modern capitalism.

References

ADORNO, T. W. (1976). *The positivist dispute in German sociology* (Repr. ed.). London: Heinemann.

CARNAP, R. (1967). *The logical structure of the world [and] Pseudoproblems in philosophy* (2nd ed.). London,: Routledge K. Paul.

CASSIRER, E. (1939). *Axel Hägerström : eine Studie zur schwedischen Philosophie der Gegenwart*. Göteborg Elander.

GALISON, Peter (1996). "Constructing modernism: Cultural Location of Aufbau." In GIERE et al.

GIERE, R. N., & RICHARDSON, A. W. (1996). *Origins of logical empiricism*. Minneapolis: Univ. of Minnesota Press.

HEIDELBERGER, M., & STADLER, F. (2002). *History of philosophy of science : new trends and perspectives*. Dordrecht: Kluwer.

LAKATOS, I., & MUSGRAVE, A. (1970). *Criticism and the growth of knowledge*. Cambridge [Eng.]: University Press.

MANNINEN, J. & STADLER, F. (eds.) (2009). *The Vienna circle in the Nordic countries*. New York: Springer.

POPPER, K. (1935). *Logik der Forschung : zur Erkenntnistheorie der Modernen Naturwissenschaft*. Wien: Julius Springer.

REISCH, G. A. (2005). *How the Cold War transformed philosophy of science : to the icy slopes of logic*. Cambridge; New York: Cambridge University Press.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 706.

NANOTECHNOSCIENCE: INTERRELATION OF THE BASIC THEORIES, MODERN EXPERIMENT AND NOVEL TECHNOLOGIES[1]

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Abstract

Nanotechnology is at the same time a field of scientific knowledge and a sphere of engineering activity, in other words –NanoTechnoScience– similar with Systems Engineering as the analysis and design of complex man/machine systems but now as large-scale micro- and nanosystems. In nanotechnoscience, on the one hand, explanatory models of natural phenomena are drawn up and predictions of the course of certain natural events on the basis of mathematics and experimental data are formulated as in classical natural science, and, as in the engineering sciences on the other hand, not only experimental arrangements are constructed, but also structural plans of new nanosystems previously unknown in nature and technology.

In nanotechnoscience, on the one hand, explanatory models of natural phenomena are drawn up and predictions of the course of certain natural events on the basis of mathematics and experimental data are formulated as in classical natural science, and, as in the engineering sciences on the other hand, not only experimental arrangements are constructed, but also structural plans of new nanosystems previously unknown in nature and technology.

The operation of nanotheory

The operation of nanotheory is realized by the iteration method (see Fig. 1). First, a specific engineering problem is formulated. Then it is represented in the form of the structural plan of the nanosystem, which is transformed into an idea about the natural process reflecting its performance. To calculate and model this process mathematically, a functional

[1] This article is prepared for the project 09-06-00042 "Technoscience in Knowledge Society" of the Russian Foundation of Basic Research.

diagram is constructed. Consequently, the engineering problem is reformulated into a scientific one, and then into a mathematical problem solved by the deductive method. This path from the bottom to the top represents the analysis of schemes (the *bottom-up* approach)[2]. The opposite direction –the synthesis of schemes (the *top-down* approach)– makes it possible to synthesise the ideal model of a new nanosystem[3] from idealised structural elements according to the appropriate rules of deductive transformation, to calculate basic parameters of the nanosystem, and to simulate its function.

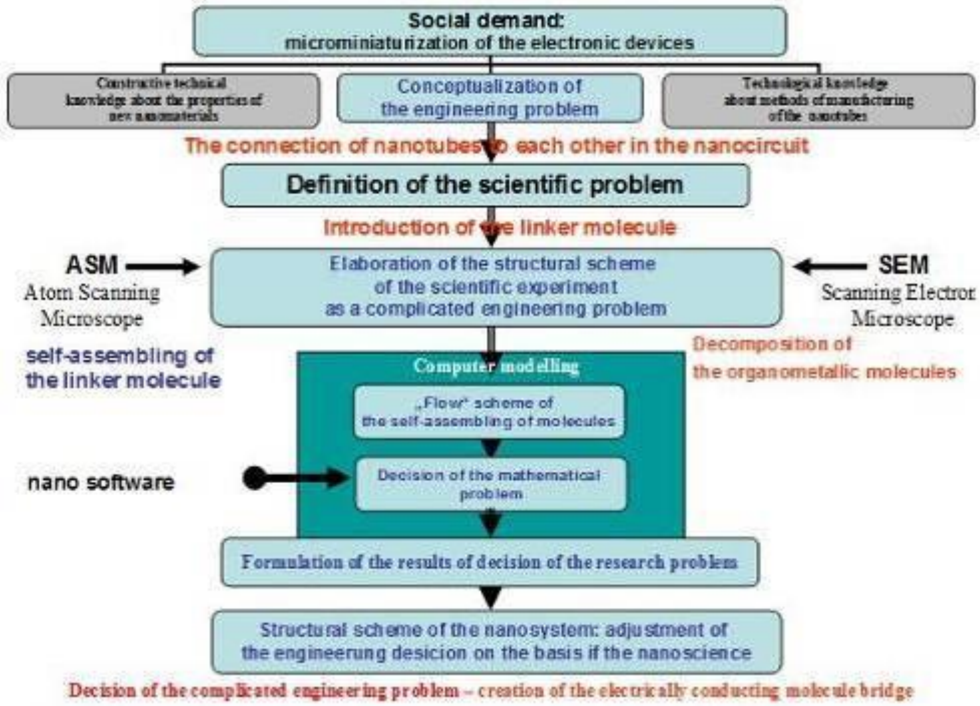

Fig. 1: Operational Algorithm of the NanoTechnoScience Function.

In natural scientific theory, flow charts are of primary importance, but structural plans are not. Both the mathematical apparatus and experiments are, for natural scientists, merely a means of prediction and explanation of the natural processes. For example, Hertz worked in principle as an engineer when he designed new experimental equipment. But he did not intend to find a technical application for his experimental devices. One of the major problems of well-developed engineering theory lies in the "copying" of type structural schemes for various engineering requirements and conditions. Then, the solution of any engineering problem, the construction of any new system, will be supported theoretically. This is the essence of the constructive function of engineering theory (theory in engineering science), its guidance of engineering practice. Its solution is cast into practical-methodical recommendations (for the designer, inventor, production engineer, etc.). A class of hypothetical technical systems must correspond to its abstract objects which have not yet been realised. In engineering theory, therefore, not only analysis is important, but above all, synthesis of theoretical schemes of technical systems.

In nanotechnoscience constructs from various scientific theories –classical and quantum physics, classical and quantum chemistry, structural biology, etc.– are used, whereas, in nanosystems, different physical, chemical and biological processes take place.

[2] For instance, this can be the investigation of "the possibility of connecting nanoparticles in series and in parallel configurations, acting as nanocircuit elements" (see: Alessandro Salandrino, Andrea Alù, Nader Engheta. *Parallel, Series, and Intermediate Interconnections of Optical Nanocircuit Elements Part 1: Analytical Solution*. http://arxiv.org/ftp/arxiv/papers/0707/0707.1002.pdf).

[3] Nanocircuit synthesis can be, for example, a synthesizing nanocircuit elements in the optical domain using plasmonic and non-plasmonic nanoparticles (see: Nader Engheta, Alessandro Salandrino, Andrea Alù. *Circuit elements at optical frequencies: nano-inductors, nano-capacitors and nano-resistors* (2004). http://arxiv.org/ftp/cond-mat/papers/0411/0411463.pdf).

Physical, chemical and biological components of nanosystems

There is, for example, the fabrication of a programmable microfluidic device similar to an electronic computer. "The human body so far is the ultimate 'wet computer' –a highly efficient, biomolecule-based information processor that relies on chemical, optical and electrical signals to operate".[4] Researchers are trying to mimic some of the body's approaches to computing. A special field of research is the development of molecular logic gates (see Fig. 2).

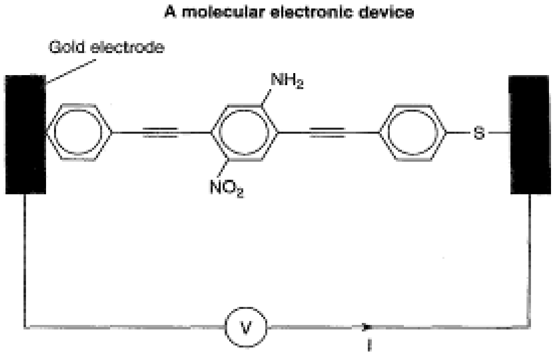

Fig. 2: "Illustration of an electronic switch made of a conducting molecule bonded at each end to gold electrodes. Initially it is non-conducting; however, when the voltage is sufficient to add an electron from the gold electrode to the molecule, it becomes conducting. A further increase makes it non-conducting again with addition of a second electron".[5]

"Already, common logic gates, which are used in conventional silicon circuitry, can also be mimicked at the molecular level. Chemists have reported that a molecular logic gate has the potential for calculation on the nanometer scale, which is unparalleled in silicon-based devices. The general character of the concept of binary logic allows the substitution of electrical signals by chemical and optical signals, which for example opens access to a vast pool of photoactive molecules to be used for the purpose of molecular logic".[6] Such molecular logic gate structures using fluorescence changes have been studied intensively using various inputs: input solutions are routed into "a microfluidic device", which is filled with fluorescent sensor molecules (a fluorescent sensor solution). There the solutions mix and, in certain combinations, switch the fluorescence "output" on or off (Figs. 3-4).

The fluorescence intensity of the pyrazoline derivative **1** is high when the concentration of H+ is low, and vice versa. The fluorescence intensity of the anthracene derivative **2** is high when the concentration of Na+ and/or K+ is high. The emission is low when both concentrations are low. The fluorescence intensity of the anthracene **3** is high only when the concentrations of H+ and Na+ are high. The emission is low in the other three cases. The signal transductions of the molecular switches **1**, **2**, and **3** translate into the truth tables of NOT, OR, and AND gates, respectively, if a positive logic convention is applied to all inputs and outputs (low = 0, high = 1).[7]

These miniaturised chemical analysis systems require only the smallest quantities of reagents. The microfluidic channels are the wires that distribute the information (reagents), while the reaction chambers or mixers are the logical operators. A molecular logic gate in a microfluidic system is based on fluorescent chemosensors by detecting the changes in intensity as a response to various inputs (for example, metal ions). These miniaturised chemical analysis systems are already replacing complex, bulky equipment: Fig. 5.[8]

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[4] M. Berger. *Towards wet computing.* December 5, 2007 http://www.nanowerk.com/spotlight/spotid=3559.php

[5] Ch.P. Pool, Jr., F.J. Owens. *Introduction to Nanotechnology*. Hoboken, New Jersey: John Wiley & Sons, 2003, p. 351. In principle, this electronic switch's mode of functioning doesn't differ from the coherer, an electrical component formerly used to detect radio waves, consisting of a tube containing loosely packed metal particles (filings in Branly's coherer, by Popov´s receiver, or nickel powder by Marconi). The waves caused the particles to cohere, thereby changing the current through the circuit [V. Gorokhov Die Entstehung der Radiotechnik als eine technikwissenschaftliche Disziplin – die Rolle von Ferdinand Braun. In: Wolfschmidt, Gudrun (ed.): *Heinrich Hertz (1857–1894) and the Development of Communication. Proceedings of the Symposium for History of Science*. Hamburg, October 8–12, 2007. 2008, S. 175-201].

[6] M. Berger. *Towards wet computing.* December 5, 2007 http://www.nanowerk.com/spotlight/spotid=3559.php

[7] *Springer Handbook of Nanotechnology* / B. Bhushan (Ed.). Berlin, Heidelberg, New York, 2004, p. 16.

[8] M. Berger. *Towards wet computing.* December 5, 2007 http://www.nanowerk.com/spotlight/spotid=3559.php

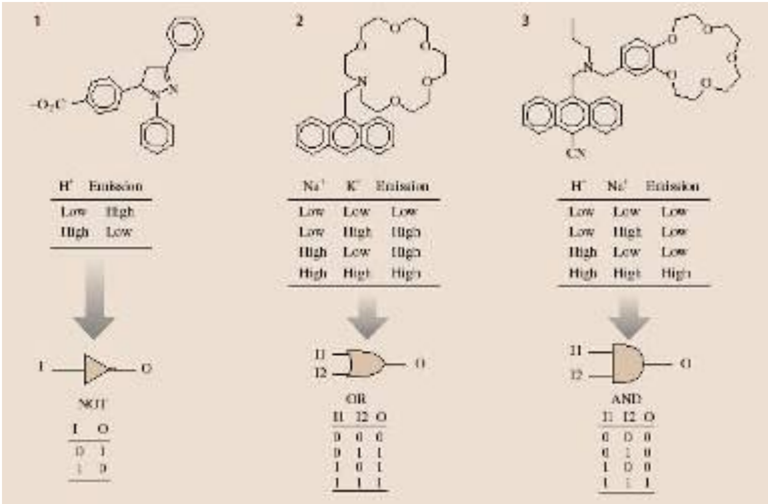

Fig. 3: Molecular Switches and Logic Gates.

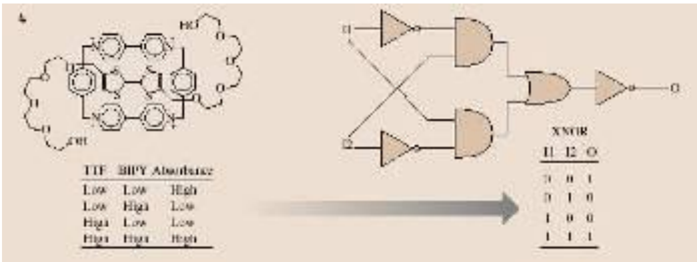

Fig. 4: Combinational Logic at the Molecular Level (The charge-transfer absorbance of the complex 4 is high when the voltage input addressing the tetrathiafulvalene (TTF) unit is low and that stimulating the bipyridinium (BIPY) units is high and vice versa. If a positive logic convention is applied to the TTF input and to the absorbance output (low = 0, high = 1) while a negative logic convention is applied to the BIPY input (low = 0, high = 1), the signal transduction of 4 translates into the truth table of a XNOR circuit).[9]

Biological systems can also be components of electronic circuits derived from large-scale hybrid nanosystems. For example, current flows through a single organic molecule, which permits miniaturizing the electronic device to the single-atom or -molecule scale. A single molecule can be attached to a metallic electrode by using the scanning tunnelling microscope (STM), and can behave electrically like some conventional modern microelectronic components. The sentence, “current flows through a single molecule” actually means that a single electron is transported through an individual molecule. In this case, researchers interpret such molecules as electronic structures or as electronic circuits. These studies have given rise to a new field known as “molecular electronics”. “From the theoretical side, it was clear … that the electronic structure of the individual atoms and molecules plays a crucial role in the electronic transport. Therefore, the theoretical analysis of molecular circuits required a detailed description of the electronic structure of the individual molecules”. Such theories can be different theoretical models of quantum chemistry. But “the first attempts to describe the transport in molecular systems … showed a huge discrepancy with the experimental results, which clearly pointed out the need of further development of these theories”. The concrete theoretical and experimental studies showed that, by fabrication of the metallic electrodes out of Pt, “the catalytic activity of Pt makes the hydrogen molecule an ideal conductor”, inasmuch as the hydrogen molecule can form a very strong molecular coupling between Pt atoms as metallic electrodes. Moreover, these experiments clearly demonstrate that “the transport properties of a molecular contact can not be simply deduced from the properties of the isolated molecules”.[10]

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9 *Ibid*., p. 18.

10 Weber H.B., Cuevas J.C. Charge Transport Through Organic Molecules // Nanotechnology – Physics, Chemistry, and Biology of Functional Nanostructures: Results of the first research programme Kompetenznetz “Funktionelle Nanostrukturen” (Competence Network on Functional Nanostructures) / Th. Schimmel et all. (Eds.). Stuttgart: Landesstiftung Baden-Württemberg. – 2008. - P. 59-64.

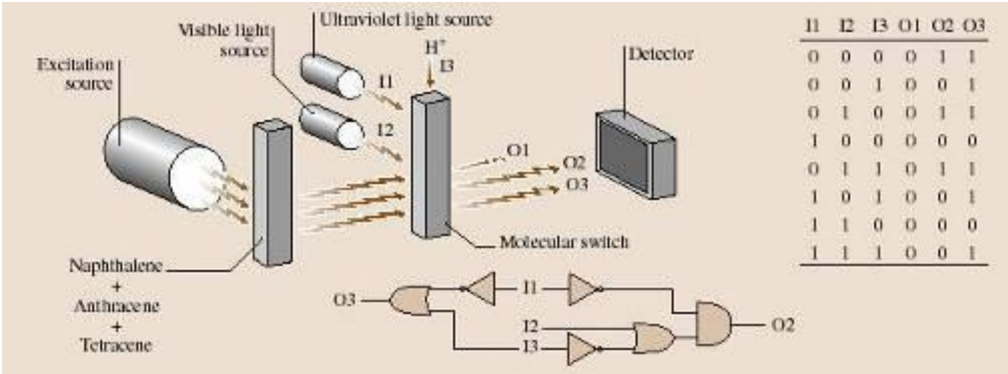

Fig. 5: The excitation source sends three monochromatic light beams (275, 357, and 441 nm) to a quartz cell containing an equimolar acetonitrile solution of naphthalene, anthracene and tetracene. The three fluorophores absorb the exciting beams and reemit at 305, 401, and 544 nm, respectively. The light emitted in the direction perpendicular to the exciting beams passes through another quartz cell containing an acetonitrile solution of the three-state molecular switch shown in Fig. 2.7. Ultraviolet (I1), visible (I2), and H+ (I3) inputs control the interconversion between the three states of the molecular switch. They determine the intensity of the optical outputs reaching the detector and correspond to the naphthalene (O1), anthracene (O2), and tetracene (O3) emissions. The truth table and equivalent combinational logic illustrate the relation between the three inputs and the three outputs. The output O1 is always 0, and it is not influenced by the three inputs. Only two inputs determine the value of O3, while all of them control the output O2.[11]

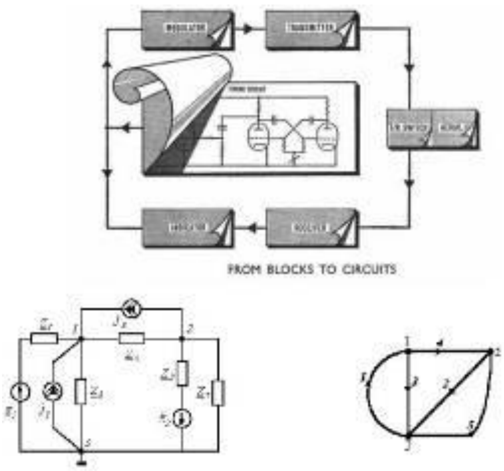

Fig. 6: Schematic block diagram in which each block represents a group of particular circuits (above)[12], equivalent circuits (lower left) and in graph theory (lower right)[13]

From the point of view of radio electronics, it makes no difference how the circuit is constructed (also as a nanostructure). Its blocks and parts can in all cases be represented as the circuits' equivalent to standard electronics components (see Fig. 6).

However one can also construct the circuit on the basis of definite nanosructures (Fig. 7), such as, e.g., a super-heterodyne radio receiver on the nanolevel (see Fig. 8).

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[11] *Springer Handbook of Nanotechnology* / B. Bhushan (Ed.). Berlin, Heidelberg, New York, 2004, p. 21.

[12] Radar Theory. *http://www.radarpages.co.uk/theory/ap3302/sec2/ch1/sec2ch161.htm*

[13] *http://media.karelia.ru/~keip/circuit/theor.htm*

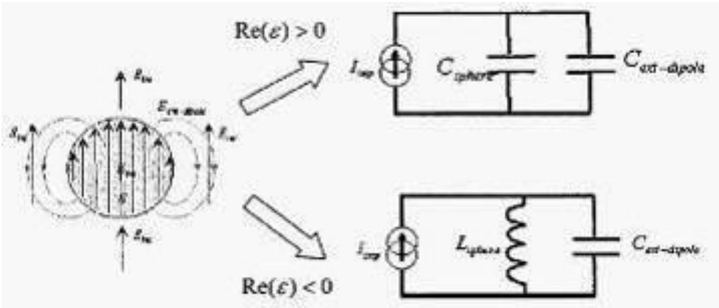

Fig. 7: Optical wave interaction with a nanosphere and its circuit equivalence. When the sphere is made of a conventional dielectric, it can be represented by a nano-capacitor, whereas when it is made of a plasmonic material, the sphere can be represented by a nano-inductor.[14]

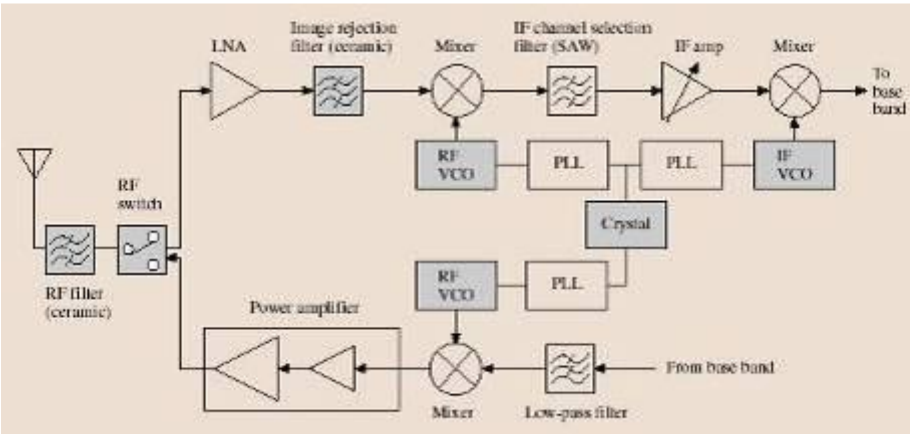

Fig. 8: Schematic of a super-heterodyne radio architecture (VCO = voltage-controlled oscillators, radio frequency (RF) and intermediate frequency (IF), SAM = self-assembled monolayer).[15]

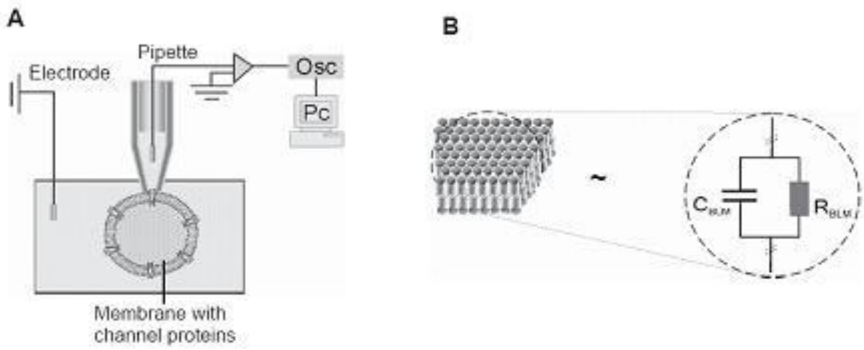

Fig. 9: The dielectric properties of biological membranes enable the study of these membranes and ionic transport occurring through the membranes. (A) Patch-clamping in electrophysiology; (B) Electrical model (right) of a biological membrane composed of packed lipids (left). The membrane is modeled as a gigaohmic resistor, RBLM, in parallel with a weak capacitor (~ 1 /F/cm2), C_{BLM}.[16]

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14 Engheta N., AIU(IJ) A., Salandrino A. Nanocircuit Elements, Nano-Transmission Lines and Nano-Antennas Using Plasmonic Materials in the Optical Domain. In: *IEEE International Workshop on Antenna Technology*, 2005.

15 *Springer Handbook of Nanotechnology* / B. Bhushan (Ed.). Berlin, Heidelberg, New York, 2004, p. 240.

16 A. Dudia. Nanofabricated Biohybrid Structures for Controlled Drug Delivery *Proefschrift Universiteit Twente*. Enschede, Gildeprint Drukkerijen B.V., 2007.

An electronic circuit is here only one of the suitable models for theoretical representation of various natural (electrical, chemical, or biological) processes in the *hybrid nanomachine* (similar to the theory of servomechanisms in which the different constructions of the regulator are considered to be equivalent). One is already discussing a further method of constructing "quasi electronic" circuits with the help of chemical (Fig. 2) or biological (Fig. 9) processes.

In nanotechnology, a nanosystem is often defined as a nanocircuit consisting of standard components (see Fig. 10): "Nanotechnological constructions are to reproduce traditional electronic components (switches, diodes, transistors, etc.) on a nanoscale. One main goal of this effort is to open up new dimensions of data processing, namely through the storage of large amounts of data in the smallest possible space … ".[17]

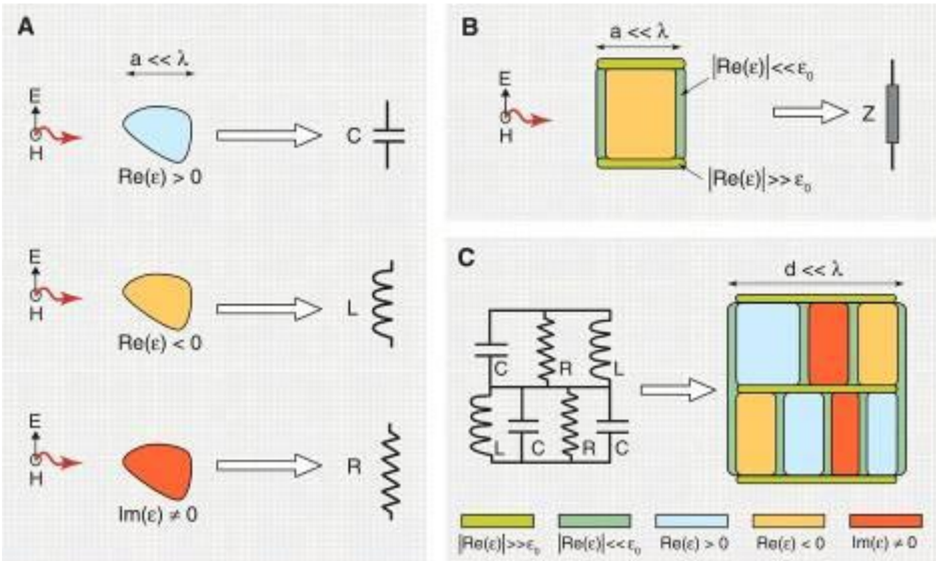

Fig. 10: Subwavelength nanoparticles as lumped nanocircuit elements at optical frequencies, and the collections of such nanoparticles. (**A**) A nanoparticle, with subwavelength size, when illuminated by a monochromatic optical signal, can effectively play the role of a lumped optical circuit element, depending on the permittivity of its material (*2*). (**B**) An optical nanomodule, formed by a material nanoparticle with subwavelength size, covered on its sides by layers of material with a very low real part of relative permittivity and on its two ends by layers of material with a very high real part of relative permittivity. This may perform as an insulated, lumped optical nanoelement with two connecting terminals. (**C**) Illustration of the concept of mn-circuits, several lumped optical nanomodules of (B), arranged next to each other, with a subwavelength dimension. When this mn-circuit is excited by an optical signal, the optical electric fields and displacement currents in these elements are tailored and patterned such that this collection of particles may behave approximately as the circuit shown on the left in a specific frequency band.[18]

In the structure of the nanocircuits we can find many different traditional electronic components ("molecular-scale electronics") realized on the nanolevel with the help of nanotechnology: first, there are electronic elements, second, electronics blocks, and third, large-scale nanosystems, such as, for instance, nanocomputer implemented as "computing at the molecular scale". "Computers of tomorrow could be quite different from what they are today … Computing could become as ubiquitous as electricity … The researchers are also looking at a variety of fabrication processes, from nano-imprint lithography –a kind of production process akin to a traditional printing press–to chemical self-assembly by growing silicon nanowires between electrodes. … Clearly, there's a lot of work to do before nanoscale devices become reality".[19]

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[17] Schiemann G. *Nanotechnology and Nature. On Two Criteria for Understanding Their Relationship* // HYLE - International Journal for Philosophy of Chemistry. – 2005. - Vol. 11. - No.1. - PP. 77-96. - http://www.hyle.org

[18] N. Engheta. Circuits with Light at Nanoscales: Optical Nanocircuits Inspired by Metamaterials. // *Science* 21 September 2007: Vol. 317. no. 5845, pp. 1698 – 1702. *http://www.sciencemag.org/cgi/content/full/317/5845/1698*

[19] *Beyond Silicon: HP Outlines Comprehensive Strategy for Molecular-scale Electronics*. *http://www.hp.com/hpinfo/newsroom/press/2005/050314a.html*

NanoTechnoScience as Nano Systems Engineering

So nanotechnology is, at the same time, a field of scientific knowledge and a sphere of engineering activity; in other words –NanoTechnoScience[20]– similar to Systems Engineering as the analysis and design of large-scale, complex, man/machine systems, but now as micro- and nanosystems. "Nanoscience is dealing with functional systems either based on the use of sub-units with specific size-dependent properties or of individual or combined functionalized subunits".[21] Nanosystems engineering is the aggregate of methods of the modelling and design of various artefacts (fabrication of nanomaterials, assembling technology for construction of comprehensive micro- and nanosystems, microprocessing technology for realizing micromachines, etc.). "Microsystems engineering and nanotechnology are two disciplines of miniaturization in science and engineering, which complement each other. Nanotechnology provides access to so far unused, completely novel effects. Microsystems engineering allows for the development of complete systems solutions due to its highly systemic potentials".[22] Nanosystems engineering, as well as macrosystems engineering, includes not only systems design, but also complex research. Design orientation has an influence on the change of priorities in complex research and of the relation to knowledge, not only to "the knowledge about something", but also to knowledge as a means of activity: from the beginning, *the control and restructuring of matter* at the nanoscale is a necessary element of nanoscience.[23] In the nanoexperiment, the scanning tunnelling microscope is not only an instrument of scientific investigation, but also at the same time a facility for fabricating electrically conducting bridges between an electrode, the nanotubes chosen, computer modelling, and design of the various artefacts. "In order to connect carbon nanotubes to each other, they have to be chemically functionalised"; that is to say, some functionally neutral properties of nanotubes are artificially made functionally specific, for example, electroconductive. "The functionalisation is realized with the help of electron beam lithography". Metalorganic molecules can be used as connections. "The adsorbed organometallic molecules can be deposited with the electronic beam at desired locations to form electrically conducting bridges between an electrode and the selected nanotubes". The linker molecule can also be used as a gate insulator as in the field effect transistor. And the transistor is an important product for the electronics industry (see Fig. 11).

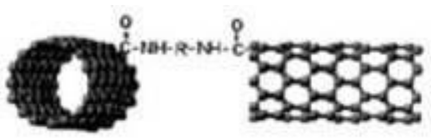

Fig. 11: Schematic drawing of two end-to-side connected nanotubes, where R stands for the linker molecule tripropylentetramin (TPTA) or phenylendiamine (PDA).[24]

In nanotechnoscience, even experimental equipment is a very complicated large-scale system. For its development, design, and fabrication very much time, funds and the work of many scientists and engineers are necessary. The management of such a project is therefore also a complicated systems engineering organisational task. Even alone the coordination of the research groups of physicists and biologists for the investigation and design of the hybrid systems is no simple chore. In addition, the mutual understanding and cooperation of the theoreticians, experimenters, engineering designers, production engineers, and managers who often speak different languages, has to be insured. Technology assessment and risk management, utilisation problems and even nanoethics are also very important. Not only specialists in nanoscience and nanotechnology are needed, therefore, but also generalists with competence for social and methodological reflexion.

So nanotechnology is at the same time a field of scientific knowledge and a sphere of engineering activity; in other words –NanoTechnoScience– similar with Systems Engineering as the analysis and design of complex man/machine systems but now as large-scale micro- and nanosystems.

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20 Baird, D. et al. (eds) *Discovering the Nanoscale*. IOS Press, Amsterdam (2005).

21 Pool Ch.P., Jr., Owens F.J. *Introduction to Nanotechnology*. Hoboken, New Jersey: John Wiley & Sons, 2003.

22 *The KIT Nano- and Microscale Research and Technology Center*. www.fzk.de/fzk/groups/kit/documents/internetdokument/id_059981.pdf

23 Paschen, H., Coenen, Chr., Fleischer, T. u.a. *Nanotechnologie. Forschung, Entwicklung, Anwendung*. Springer, Heidelberg (2004).

24 Roth S., Kern D. *Self-Assambly of Carbon Nanotube Transistors // Nanotechnology – Physics, Chemistry, and Biology of Functional Nanostructures: Results of the first research programme Kompetenznetz "Funktionelle Nanostrukturen"* (Competence Network on Functional Nanostructures) / Th. Schimmel et all. (Eds.). Stuttgart: Landesstiftung Baden-Württemberg. – 2008. - P. 77-94, p. 79.

BIOGRAPHIES OF SPANISH SCIENTISTS DURING THE FRANCO PERIOD

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 715.

THE BIOGRAPHY OF THE SPANISH MATHEMATICIAN TOMÁS RODRÍGUEZ BACHILLER (1899-1980)

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Abstract

The aim of this paper is to present a short biography of the Spanish mathematician and engineer Tomás Rodríguez Bachiller. He won a chair of mathematics in the Faculty of Sciences during the II Republic and lived well with people of diverse political tendencies. He has been considered as an exiled; however, we shall show that this isn't so. After Spanish Civil War (1936-1939), he was suspect by the Franco Regime at first and a file was made, but this fact had little impact in his life. Many of the activities that he conducted in the Universidad Central *in Madrid and other important scientific institutions, and his important work as translator, will be shown.*

Mathematician and Civil Engineer. Professor of the Faculty of Sciences

Tomás Rodríguez Bachiller (TRB) was born in Hong-Kong on November 10, 1899, where his family lived temporarily because of his father's job: Vice-consul of Spain. His father had two daughters, result of a first marriage, and three sons of his second wife.

He did his first studies in Ayamonte (Huelva). TRB was a brilliant student in the *Instituto Cardenal Cisneros* (High School) in Madrid, getting his graduate's degree in 1916. Thereafter, he started two degree courses: first, Exact Sciences (1916-1922) and two years later, Civil Engineering (1918-1924). "As engineer, he could make a living", as his father said to him.

When he was still an engineering student, in 1923, Einstein visited Spain. TRB became popular among his classmates because made some abstracts of this famous scientist's conferences, which were published in the newspaper *El Debate,* and praised by Einstein himself. This episode would lead to funny stories.

Fig. 1: A photograph of TRB.[1]

During the academic year 1923/24, the Faculty of Sciences gave TRB a grant to stay in France to expand his mathematical knowledge. He was in the College of France and the Sorbonne University, met important mathematicians and received some courses: Borel (on elasticity), Picard (on algebraic curves and surfaces), Drach (on contact transformations), Guichard (on differential geometry and Laplace transformation), Cartan (on fluid mechanics), Hadamard (on differential equations following Poincaré), Lebesgue (on "analisis situs") and Vessiot (on partial differential equations and group theory).

As result of this stay, TRB gave a course on "Analisis Situs" at the Faculty of Sciences in the year 1924/25. This was the first course on topology ever taught in Spain. Millán and Ausejo say [1, p. 273]:

> "siguió investigando en temas de topología y geometría algebraica. En su afán por dar a conocer modernos campos de investigación trabajó en una exposición elemental de la topología y en otra sobre el debate intucionismo-formalismo en el campo de los fundamentos de las matemáticas. En marzo de 1926 solicitó una pensión a la Junta para estudiar con Volterra y Fréchet que al parecer no le fue concedida."

He never left the Faculty of Sciences[2] of Madrid and since 1925, he began his academic life as Assistant in this institution, remaining in it until his retirement in 1969. Since October 1932, he held the vacant chair of *Análisis Matemático 3º* (Differential equations) and on June 29, 1935 TRB got the chair of *Análisis Matemático 4º* (Theory of functions), taking up his post on 29 August. He had entered alone to the competitive examination because the other candidate[3] didn't appear in spite of having been previously accepted to take part in it.

On August 27, 1935 he had defended his doctoral thesis entitled *Axiomática de la dimension*[4] (topology) receiving the highest mark. The jury was composed by José Alvarez Ude (president), Francisco Navarro Borrás (secretary), Pedro Pineda, Honorato de Castro and José Barinaga. This means that TRB wasn't yet a doctor when he won his chair, but had to be so before taking up his post.

On August 23, 1929, TRB had been named Engineer of the Corps of Engineers Geographers after winning a competition. However, he was active about nine years: 1929-1932 and 1949-1955. This job gave stability to his new family, as he had married Mª Pilar Pradilla on May 6, 1926. The priest at the wedding had been the philosopher Xavier Zubiri, and Fernando Lorente de No[5] the wedding witness. The couple had three children: Luis, Agustín and Tomás.

---

[1] We thank to F. A. González Redondo for having provided us with this photograph.

[2] You can know about the different Professors in the Faculty of Sciences of Madrid in [12]. Also, you can see in [10] the photographs of some Professors that were in this institution during the first third of the twentieth century.

[3] This candidate was Ricardo San Juan. That same year, San Juan won the chair of *Analísis Matemático 1º* in Salamanca and the next year, he did it with *Analísis Matemático 2º* in Madrid.

[4] A history of the Spanish Doctorate in the first third of the twentieth century can be seen in [5] and [6].

[5] Fernando Lorente de No became Professor of the Faculty of Sciences. See "Homenaje a los profesores depurados por el franquismo" in *Tribuna Complutense,* 28-11-2006.

Before the Civil War, Rodríguez Bachiller had got a chair in the Faculty of Sciences, belonged to the Corps of Engineers Geographers and taught mathematics in private academies[6]. Besides, he spoke several languages quite well: French, English, German and Italian. This allowed him to read and translate foreign books, not just of mathematics but also literary, philosophical, etc, and to enter into relations with scientists from other countries.

Mathematical activities and intellectual environment until 1936

In August 1922, he joined the *Laboratorio y Seminario Matemático* (*Laboratorio*) of the JAE (*Junta para Ampliación de Estudios*). Since then he took care, close to Lorente de No, of guiding the work of younger students. Ausejo and Hormigón[7] say:

> "The LSM [*Laboratorio y Seminario Matemático*] represents the institutional recognition of research as a necessary and sufficient activity for the social justification of mathematicians."

This institution maintained a close relationship with the Italian mathematicians and TRB was one of those responsible for this,

> "… particularmente con Enriques, Castelnuovo y Severi. En el seno del propio LSM es Rodríguez Bachiller quien cataliza esta actividad, introduciendo el estudio de materias que no se impartían todavía en la Facultad de Ciencias: grupos de sustituciones, funciones de variable compleja, geometría diferencial y topología." [8]

As noted, in 1923 he went to France. Shortly after returning, given his link with the Faculty of Sciences and the *Laboratorio*, Bachiller continued his work as a researcher. He had an intense activity in the Spanish Mathematical Society until 1928 through its journal *Revista Matemática Hispano-Americana*[9], participating in it in different ways: reviewing university publications, writing short obituaries and maintaining a correspondence with European mathematicians. In addition, he published some articles such as *La correspondencia biunívoca de Cantor y el teorema de Netto* (1926), *Conjuntos cerrados no densos* (1926), *Sobre el número de dimensiones de un conjunto* (1927) and *Conferencias del Profesor Terradas* (1927).

In 1923 he had attended to the Ninth Congress of the *Asociación Española para el Progreso de las Ciencias*[10], held in Salamanca, and two years later, to the next, held in Coimbra, with a brilliant participation in this last.

Since 1928 he disappeared from the *Sociedad*, its journal and the *Laboratorio* until 1933. The reason was probably an incident he had with Julio Rey Pastor in a session of the *Sociedad*[11]. In 1933 TRB figured again as member of the Board in which José Mª Plans was the director and José Barinaga, deputy director, they both Professors in Madrid.

He had started to collaborate in other cultural magazines as *La Gaceta Literaria* and *Cruz y Raya*. The first was founded by the fascist E. Giménez Caballero. The participation of TRB in it was not effective but appeared in the first issue as responsible for mathematics. This journal was published in Madrid between 1927 and 1932, when it disappeared for political reasons:

> A principios de 1931 Ernesto Giménez Caballero colaboró con Ramiro Ledesma Ramos en el lanzamiento de *La Conquista del Estado,* y al proclamarse en abril la República, las posiciones políticas de Giménez Caballero y su defensa del fascismo, determinaron que todavía más colaboradores se fuesen olvidando de escribir en *La Gaceta*[12].

In 1933, TRB began to collaborate in the magazine *Cruz y Raya, revista de afirmación y negación*. It was founded by the republican José Bergamín, although this publication was open to collaborators of different political tendencies. TRB published a popular article entitled *El concepto de número de dimensiones de un espacio*. Zubiri, his friend and priest at his wedding, was also an important collaborator in it.

---

6 His family remembers, not clearly, that Bachiller became to give preparatory lessons in the famous Academy Krahe (see [8]) in Madrid. Years later, he had his own one.

7 They think that the *Sociedad Matemática Española* (*Sociedad*) and the *Laboratorio* were key agents in the process of the involvement of Spain in the international mathematical mainstream, interrupted in 1936. See [2].

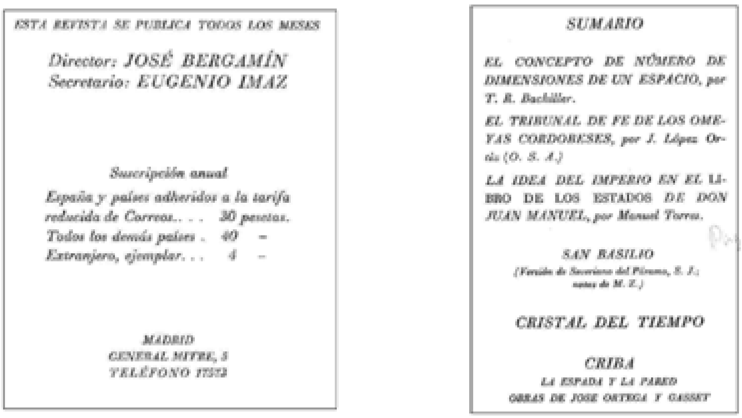
8 See [1, p. 272].

9 Journal published by the *Sociedad Matemática Española*.

10 (AEPPC). You can know the history of the AEPPC in: Ausejo, E., *Por la ciencia y por la patria: la institucionalización de la ciencia en España en el primer tercio del siglo XX*, Siglo XXI de España, Madrid, 1993. This book is cited in [2].

11 See [7].

12 You can see: http://www.filosofia.org/hem/med/m013.htm



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LA IDEA DEL IMPERIO EN EL LIBRO DE LOS ESTADOS DE DON JUAN MANUEL, por Manuel Torres.

SAN BASILIO
(Versión de Severiano del Páramo, S. J.; notas de M. Z.)

CRISTAL DEL TIEMPO

CRIBA
LA ESPADA Y LA PARED
OBRAS DE JOSE ORTEGA Y GASSET

Fig. 2: *Cruz y Raya*.

As his friend Antonio Rodríguez Huéscar noted in 1980[13], Bachiller was a great friend of the social gatherings and indeed he was one of the members of the circle organized around the *Revista de Occidente*, captained by the philosopher José Ortega y Gasset. Also, Rodríguez Huéscar said:

> "Conocí a don Tomás –así le llamábamos, familiarmente, sus amigos más jóvenes– recién acabada la guerra civil, en su tertulia del café Ibiza (en la plaza del Callao), y desde entonces mantuve con él trato ininterrumpido. Fue Bachiller durante toda su vida hombre de tertulia. Concurrió a la de la *Revista de Occidente,* desde antes de la guerra civil, aunque yo no tuve ocasión de encontrarme entonces con él, pues Ortega se reunía con nosotros, a la sazón sus alumnos, en la sede de la revista, en la Gran Vía, a horas diferentes de las de la tertulia ordinaria de «los mayores»."

Probably, these activities were the reason why the winning factious side linked TRB with the losing side after the Civil War. The truth is that he kept at the time, and in the future, good contacts with colleagues of both political tendencies. In fact, his brother Ángel, philosopher and monk that left the Dominican Order in 1935, would be in prison during the war and suffered an "interior exile" in Spain.

His life during the Franco Dictatorship

After the Spanish Civil War, thousands of people had to emigrate to other countries for fear of repression by Franco, which included about a hundred Professors. Some Spanish mathematicians were repressed by the Franco Regime. In this paper, we highlight two: José Barinaga and Fernando Lorente de No. The first[14] had been member of the jury in Bachiller's competitive examination and his doctoral thesis defence, backing him in both cases. Besides, they had been together in the Interim Board of the journal of the *Sociedad* in 1937. Lorente de No had worked with Bachiller in the *Laboratorio* and the journal of the *Sociedad*, but mainly, they were close friends. Finally, Barinaga was a victim of the Franco Regime, as he was separated of his chair until 1946. Less lucky was Lorente de No, who had to give up the university forever.

Bachiller was also suspect for the Regime. On October 10, 1939, he was opened an administrative record and failed to receive half his salary. Two months later, December 22, he was allowed to be on active, but disqualified for occupying managerial and confidence posts. Therefore, he returned to his chair of *Theory of Functions* without interruption until his retirement, except for some periods in which he was in Puerto Rico. He suffered a similar reprisal in the National Geographic Institute on December 22, 1945 because he still belonged to the Corps of Engineers Geographers. Despite not being able to occupy managerial posts, he did so, as we shall see.

About Bachiller's political views, Glick [9] says:

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[13] Antonio Rodríguez Huéscar wrote an article in the newspaper *El País* for Bachiller's death, which occurred on July 9, 1980 in Madrid. See [13].

[14] See [3].

> "Rodríguez Bachiller no tomó parte activa en la política, pero ideológicamente fue demócrata de raíz. Me señaló su susto cuando, estando en Berlín en 1936, fue a saludar al rector de la Universidad, el matemático Ludwig Bieberbach, y éste le respondió con el saludo nazi. En 1941, fue a Roma para seguir unos estudios superiores con Francesco Severi. Era un momento oportuno. "Me sacaron los italianos –diría– de un ambiente hondamente antipático" (recordando la fortuna de Lorente de Nó)."

He had an intense activity in the *Sociedad* again. He was member of its Board in 1941 and returned to work to the journal which had been reorganized and now was published by the *Instituto Jorge Juan*[15]. Bachiller proposed in this journal a lot of mathematical problems, did reviews on books of foreign authors and translated several articles about topology and theory of functions.

But he wanted to research in mathematics, so in 1941 he went to the *Istituto Nazionale di Alta Matematica* in Rome, at his expense, after having been refused a financial aid. There, he gave several lectures on recent results on algebraic surfaces and topological manifolds. On his return, he gave several courses on these topics, current at that time, in the *Instituto*. Also, in 1947, after winning a scholarship[16] to go to the USA for five months, Bachiller was working with Zariski in the University of Illinois (Urbana) and with Lefschetz, Artin and Chevalley in Princeton.

Therefore, he played an important role in the *Instituto*. He gave several courses on topology since 1942, and logic and foundations of mathematics since 1951. For example, in the years 1943/44 and 1944/45 he lectured on "Topological Groups", after intervening in the Congress of the *AEPPC*, held in Oporto in 1942. Although the *Instituto* wanted to publish all these lessons, finally it was not done.

But besides, he was deputy director of the *Instituto* since 1946 until 1949, year in which he replaced José Mª Orts in its direction, remaining ten years as director. During this time, he went to several international meetings such as:

1949. París. Conference about algebra and number theory.

1950. Rome. Meeting held because of Severi's retirement.
Annual meeting of the Italian Mathematical Union.
University of Harvard and MIT in Cambridge. International Congress of Mathematicians (ICM).

1952. Rome. First General Assembly of the International Mathematical Union (IMU).
(Bachiller was representing the Spanish mathematicians).

1955. Princeton. Institute for Advanced Studies.

1956. México. Topology conference.

Despite all these activities, he published few research articles due to a deliberate lack of productivity, according to his friend Rodriguez Huéscar. After the Civil War, he just published a research article, in 1942, entitled "Sulle superficie del quarto ordine contenenti una conica" that appeared in an Italian journal[17]. Also, he is the author of other articles, all them published in the journal *Revista Matemática Hispano-Americana*[18]. However, he directed six doctoral theses:

1940. E. Linés (on probability).

1941. F. Botella and Pedro Abellanas (on differential geometry).

1945. F. Gaeta (on algebraic geometry).

1952. J. R. Fuentes (on logic).

1954. B. Rodríguez Salinas (on differential equations).

Besides this intense activity in the *Instituto* and the lectures on theory of functions (in his chair), he also gave other lessons such as *Mecánica celeste* (of Doctorate, during the years 1945/46 and the next) or *Análisis Matemático 5º* (1947-1957). He gave lectures on *Matemáticas generales* in the Selective Course of the Faculty of Sciences and *Matemáticas* in the *Escuela de Estadística*. He also took part in the *Escuela Oficial de Telecomunicaciones* appearing among his courses: *Fundamentos matemáticos de la teoría de la comunicación* and *Geometría de las redes eléctricas* (in collaboration with A.

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[15] Formerly *Laboratorio*, that belonged to *Consejo Superior de Investigaciones Científicas* (formerly *JAE*). See [11] to know the activities in the *Instituto* during the Spanish Civil War.

[16] Scholarship granted by the *Ministerio de Asuntos Exteriores* (Foreign Office).

[17] *Atti Accad. Italia, Rend. Cl. Sci. Fis. Mat. Natur.* (7) 3, 556–562.

[18] 1941: "Comentarios sobre álgebra y topología", 1, 68–74; 1942: "Sophus Lie", 2, 245–246, "Curso de Topología", 2, 295–297; 1943: "Curso de Topología", 3, 41–51, "David Hilbert" (obituary) 3, 77–81.

González del Valle). Rodríguez Huéscar notes that many students benefited from the high quality and clarity of his teachings[19].

Since already we have indicated, Bachiller had given private lessons in the Academy Krahe. Years later he founded his own academy jointly with J. Abollado Aribau, in which civil engineers received preparatory classes as it can be seen in the following announcement published in the newspaper *ABC*.

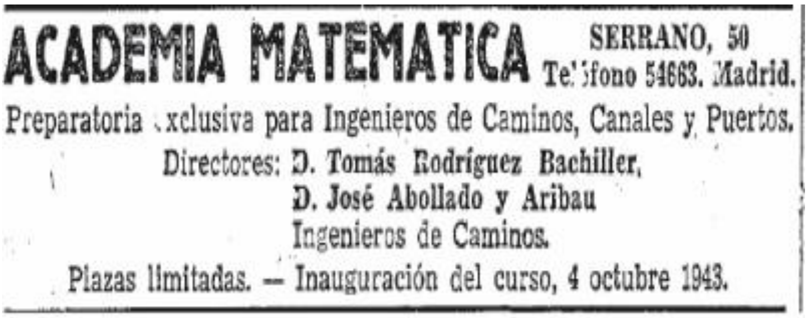

Fig. 3: *ABC*, 23/9/1942.

Because of his knowledge of several languages, the translation of books was an important activity for him, mainly from the mid-50s. So, he translated books such as *Determinantes y matrices* by A. C. Aitken, *Métodos vectoriales aplicados a la geometría diferencial, a la mecánica y a la teoría del potencial* by D. E. Rutherford, *Geometría analítica de tres dimensiones* by W. H. McCrea, *Series infinitas* by J. H. Hyslop, *Lecciones de Geometría Proyectiva* by F. Enriques, and *Lecciones de análisis* by F. Severi, in three volumes. The four first belongs to the British collection *University Mathematical Texts*, whose editors were Aitken and Rutheford. In its Spanish version, this collection, in charge of the publisher Dossat, was directed by TRB.

Fig. 4: Books translated by Bachiller.

He loved travelling, but his travels to Puerto Rico were special for him. His brother Jesús had been born in that country and his mother was buried there after having died in childbirth. In 1954, Bachiller made his first stay in the University of Puerto Rico, in Mayagüez, to teach topology and abstract algebra. This would be done also in successive years. While he stayed in this university, his lessons in Madrid were taught by his former doctoral student J. R. Fuentes. His long absences forced him to leave part of his occupations in Madrid. He left the direction of the *Instituto* and even, in 1959, he was granted a leave of absence for a maximum of ten years (until his retirement) with chair reservation for one[20].

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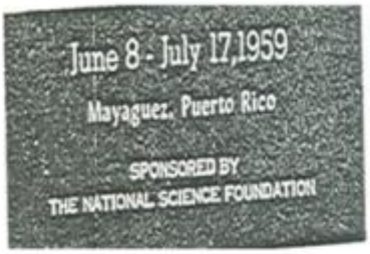
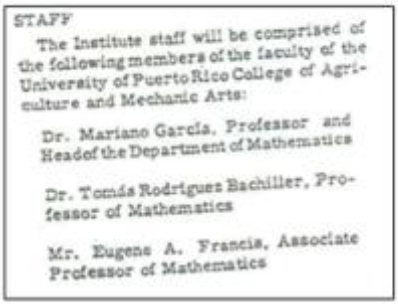
[19] Bachiller was also Professor in the *Escuela de Ingenieros de Caminos* (Civil Engineer School). A. del Campo includes him among "The untypical eccentrics", see [4]. A history of this institution can be seen in [14].

[20] On January 12, 1965, the Rector ordered the opening of a file to Bachiller for not being at work on time.

His taste for social gatherings, practiced before the Spanish Civil War, was able to be continued in Puerto Rico. There, his friend Rodríguez Huéscar noted:

> "Cuando yo me incorporé a la Universidad de Puerto Rico, en enero de 1956, se constituyó allí, con ocasión de mi llegada, y en parte por iniciativa mía -en gran parte, también, por la de Manuel García Pelayo, Gabriel Franco, Alfredo Matilla, Pedro Bravo, entre otros-, otra tertulia, en su mayoría, aunque no con exclusividad, de españoles universitarios, de la que Bachiller -profesor en aquella universidad desde hacía algún tiempo- fue uno de los puntales."




STAFF

The Institute staff will be comprised of the following members of the faculty of the University of Puerto Rico College of Agriculture and Mechanic Arts:

Dr. Mariano Garcia, Professor and Head of the Department of Mathematics

Dr. Tomás Rodriguez Bachiller, Professor of Mathematics

Mr. Eugene A. Francis, Associate Professor of Mathematics

Fig. 5: Professor of Mathematics in Puerto Rico.

These frequent visits to Puerto Rico are the reason why Bachiller has been considered as one more exile, but we have seen that this is not the case. In spite of having a file made by Franco Regime, this had little impact in his professional life. Finally, we have to highlight that he was one of the Spanish mathematicians who most contributed to the importation of modern topology and its introduction into the syllabus of the Degree of Exact Sciences (1954).

References

[1] Ausejo E. & Millán, A. (1989). La organización de la investigación matemática en España en el primer tercio del siglo XX: El Laboratorio y Seminario Matemático de la Junta para Ampliación de Estudios e Investigaciones Científicas (1915–1938), *Llull. Revista de la SEHCYT* 12, 261–308.

[2] Ausejo, E. & Hormigón, M. (2002). Spanish initiatives to bring mathematics in Spain into the International mainstream. In Parshall, Karen H. & Rice, Adrian C. (Eds.), *Mathematics Unbound: The Evolution of an International Mathematical Research Community 1800-1945*. American Mathematical Society, London Mathematical Society, 45-60.

[3] Cuesta Dutari, N. (1966). Don José Barinaga Mata. In Memoriam, *Gaceta Matemática* 18, 63–86.

[4] Del Campo, A. (1999). Los excéntricos atípicos, *Obra Pública* 1, 104–111.

[5] Escribano, J. J., Español, L. & Martínez, M.A. (2007). El Doctorado en Matemáticas durante la II República, *Llull, Revista de la SEHCYT* 30, 51–64.

[6] Escribano, J. J., Español, L. & Martínez, M.A. (2006). El Doctorado español en Matemáticas entre 1900 y 1921, *Llull, Revista de la SEHCYT* 29, 37–50.

[7] Español, L. & Martínez, M.A. (2010). Hacia la matemática abstracta: Tomás Rodríguez Bachiller (1899-1980), *La Gaceta de la RSME* 13, 769–795.

[8] Español, L. & Martínez, M.A. (2008). Un matemático singular: Augusto Krahe García (1867–1930), *La Gaceta de la RSME* 11, 361–382.

[9] Glick, T.F. (1982). In Memoriam: Tomás Rodríguez Bachiller (1899–1980), *Dynamis* 2, 403–409.

[10] González Redondo, F. A., Fernández Terán, R.E. & Fernández Laseca, L. (2007). Los Catedráticos de Matemáticas en la Facultad de Ciencias de la Universidad Central de Madrid durante el primer tercio del siglo XX: una historia gráfica, *La Gaceta de la RSME* 10, 241-260.

[11] González Redondo, F. A. (2001). La actividad del Laboratorio Seminario Matemático de la Junta para Ampliación de Estudios durante la Guerra Civil, *La Gaceta de la RSME* 4, 675–686.

[12] OUTERELO, E. (2009). *Evolución histórica de la Licenciatura en Matemáticas (Exactas) en la Universidad Central*, Facultad de Ciencias Matemáticas de la Universidad Complutense de Madrid, Madrid, 2009.

[13] RODRÍGUEZ HUÉSCAR, A. (1980). Tomás Rodríguez Bachiller, *El País*, 30 de octubre.

[14] SÁENZ RIDRUEJO, F. (1993). *Los ingenieros de caminos*, Colegio de Ingenieros de Caminos, Canales y Puertos, Colección de Ciencias, Humanidades e Ingeniería, nº 47, Madrid.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 723.

FIGHTING ISOLATION: THE MATHEMATICIAN NORBERTO CUESTA DUTARI

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Abstract

This presentation describes the struggle against isolation of Norberto Cuesta Dutari (1907-1989) under the Franco regime. Though he was not close to the establishment and could not profit from a number of opportunities for personal and institutional promotion, he achieved a mathematical production of international quality. His creative papers –dating back to 1943– are still cited in fields like Descriptive Set Theory or Decision Theory. His contribution to pure Mathematics was gathered in the book Matemática del Orden *(1958), a common bibliography item of Set Theory.*
The isolation of Cuesta in places like Ávila, Segovia, and Salamanca was for him a lifelong concern, and not only a matter of geographical distances. He often firmly complained about it, but it is difficult to imagine his vital trajectory anywhere else. After his doctorate in 1943 he tried several times to become a University Professor, but only in 1957 could he obtain a chair in Salamanca. He was fifty, lagging some twenty years behind the Civil War generation: Pedro Abellanas (b. 1914) and Francisco Botella (b. 1915), obtained chairs in their late twenties just after it.
Cuesta promoted the Mathematics degree in Salamanca, started in 1971: Even then, isolation won that battle, but the beauty of his Mathematics still remains.

Dramatis persona

In Volume 2, dated 1942, of the *Revista Matemática Hispano-Americana*, we find four papers under the general title *Números Reales Generalizados*[1], written by a certain Norberto Cuesta. The 1943 Volume contained three more papers[2], the second and third ones are the doctoral dissertation of the author, *Teoría decimal de los tipos de orden*, read in 1943. A remarkable theorem, rediscovered by Sierpinski in 1949 and by Chipman in 1960, is stated and proved on the representation

[1] Cuesta N (1942) Números reales generalizados, RMHA (Ser. IV) 2, 5-12, 62-66, 104-109, 218-225.

[2] Cuesta N (1943a) Construcción de un conjunto ordenado denso y no continuo cuyo número ordinal es ω_α, *RMHA* (Ser. IV) 3, 38-40, and Cuesta N (1943b) Teoría decimal de los tipos de orden, *RMHA* (Ser. IV) 3, 186-205, 242-268.

of linearly ordered sets as binary function spaces. The result has important applications in Decision Theory and Econometrics, and is still cited[3]:

> *"Let (X,p) be a totally ordered set and α the first ordinal number such that $|X|=|\alpha|$. Then (X,p) is order-isomorphic with some subset of the lexicographically ordered function space ($\{0,1\}^{\alpha},<^{lex}$)."*

The obvious finite case shows that the representation may not be unique, and the transfinite case proof was obtained by Cuesta in the post-war isolation of Ávila in Central Spain.

Fig. 1: N. Cuesta around 1970.

It is difficult to establish why Cuesta decided to work in the field of Transfinite Numbers, even he was an early discoverer of (the beauty of) Mathematics, at the age of twelve[4]:

> *"Yo recuerdo haberme hecho matemático, al entender los dos teoremas en que se funda la regla del cálculo de la raíz cuadrada. Era el curso 1919-1920"*

Nevertheless, he first studied Chemistry, where he acknowledged the mathematician Guillermo Sáez as an outstanding teacher. Sáez wrote a dissertation on elliptic functions in 1903 and spent the year 1909-1910 in Leipzig studying with Hölder, and possibly Hausdorff, with one of the first JAE[5] grants. Surely, Sáez's lectures influenced the vocation of Cuesta, who had studied high level Mathematics books before obtaining his Chemistry[6] degree.

Then he moved to Saragossa, completing a degree in Mathematics, and after graduation he was hired as an assistant at Granada University. In late 1937 he was sent to a Secondary School in Teruel, but the Republicans conquered the city in December 1937, he was captured and imprisoned in Valencia. Some months later he was released and served in the Republican Army until the war end. This ex-prisoner condition helped him to obtain in 1940 a Secondary School professorate. We have no information on his mathematical activities at Granada, but he must have been busy with them, for he writes[7]:

> *"Yo había frecuentado mucho, desde que salió el 1929, el bellísimo libro de Sierpinski* **Leçons sur les nombres transfinis***"*

---

[3] Fishburn P (1974) Lexicographic Orders, Utilities and Decision Rules. *Management Science*, 20 (11), Theory Series. 1442-1471. Indurain E (2002) *Matemática del Orden*, Discurso 2002-2003, UNED, Pamplona.

[4] Cuesta N (1977) La conceptuación matemática, IV *Jornadas Hispano-Lusas de Matemáticas*, Jaca 1977.

[5] JAE: *Junta de Ampliación de Estudios*.

[6] Pacheco J (2009), *The mathematician Norberto Cuesta Dutari recovered from oblivion*. Preprint 378, Max-Planck-Institut für Wissenschaftsgeschichte, Berlin: A more detailed biography with a list of Cuesta's publications.

[7] Cuesta N (1978) *Lección académica final*, Universidad de Salamanca.

Cuesta might have considered Set Theory an affordable topic on an individual basis with some basic books and journals, so he started doing research with his readings of Sierpinski and Hausdorff[8]. Post-war isolation in deep Spain kept him away from the influence networks ruling Academia and the Science Agency CSIC[9]. In any case, he did not have to "ask for a research topic", and he always produced independent work of a very personal and original character.

According to Cuesta, if Mathematics could be set-theoretically described, techniques should exist for building large sets, and tools for counting, ordering, classifying, and naming them. He chose to study Orders and their Names, on which he spent some twenty years between 1941 and the early 1960s, addressing questions like:

1. How to build all linear orderings on arbitrary sets: This is his Thesis, where in addition he compared his method with Hausdorff's lexicographic one.
2. How to name these ordering relationships (the *onomastic* problem), and in particular ordinal numbers. This is still an unsolved problem[10].
3. Generalisation to partial ordering relationships. He achieved it in his only paper in English (or something like it) in 1955.

Spanish historical background behind Cuesta's mathematical work

The biography of a Spaniard living between 1907 and 1989 was marked by three episodes: The Spanish Civil War, WW II, and their consequences. Several viewpoints can be adopted in a biographical approach, but here we will consider the influence of several stresses on Cuesta's mathematical output, focusing on the quality of Mathematics, both in absolute terms and in its relation with the prevailing environmental conditions.

The crucial years are the early post-Civil War ones, 1939-1945, when the Franco regime established an idiosyncratic social organisation in "neutral" Spain while WW II ravaged the world. It was a complex mixture: totalitarianism, *élites* and *parvenus* struggling for favour in Franco's environment, desperate Regime efforts to obtain reconnaissance and forgiveness for earlier connivance with the fascist regimes in Italy and Germany, scarce harvests, poverty. Large numbers of unskilled people fled to South America, but there was also an inner mobility from Central and Southern Spain to the North and the Northeast, where, in spite of the Civil War, industry was still concentrated. Political decisions were made in Madrid, in a centralised way inspired on military schemes: Spain was ruled by a mix of war economy and overreaction against almost any policy practised by the Republic between 1931 and 1939.

Going into Mathematics, we begin with a brief overview of the period 1900-1936, split into two sub-periods roughly separated by WW I. During the first one, mathematical curricula in Spain catered mainly to the preparation of Engineering studies and had little to do with mathematical research: Calculus, but little Mathematical Analysis, Mechanics, some Statistics and a dense part of Classical Geometry inspired in *traités* like Rouché-Comberousse's, used for decades.

Some mathematicians obtained JAE grants during its first existence years. Guillermo Sáez was one of them, but the main role was played by Julio Rey Pastor (1888-1962), who spent nearly two years in Germany before WW I and came back to Spain with a panorama of modern Mathematics he started to implement in a frenetic activity[11].

1. Journals and publishing, I

Dissemination of scientific results before WW II was to a large extent a local affair. Most countries had their own journals[12], where national authors published. Publishing in foreign journals happened on occasion of visits or stages or on request by some editor.

The *Sociedad Matemática Española* (RSM, established 1911) started its own journal, and when Rey Pastor founded the *Laboratorio-Seminario Matemático* (LSM, 1915 and 1918) within the JAE, it had its journals, publishing papers on new topics: Mathematical Physics, Relativity, Nomography, Numerical Mathematics, and on Teaching and Promoting Mathematics. The journals were *Revista Matemática Hispano Americana* (series I and II) and *Matemática Elemental* (series I

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[8] The so-called *Leipziger Berichte* from 1906-1907. The 1914 text, *Grundzüge der Mengenlehre* was also studied by Cuesta, but he preferred the *Berichte*.

[9] CSIC: *Consejo Superior de Investigaciones Científicas*,The Mathematics Institute was named after the 18th Century Spanish mathematician Jorge Juan.

[10] A partial negative answer presented to the Academy by Cuesta himself is: Arias de Reyna J (1984) El problema onomástico para los ordinales numerables, *Rev. Acad. Ci. Madrid* 78, 201-203.

[11] Although Rey Pastor started his renovation of Spanish Mathematics in the field of Geometry, this discipline, as well as Astronomy, and some others, were not so directly influenced by his ideas.

[12] Prior to the journal of the RSM there were some other attempts in Spain. See Escribano Ródenas *et al.* in these Proceedings.

and II). Among the newer topics, Rey Pastor introduced the study of divergent series and summation processes in 1928-30: his students Sixto Ríos (1913-2008) and Ricardo San Juan (1909-1969) read dissertations on series summation in the early thirties. Ríos abandoned Analysis after the War and switched to Statistics, but San Juan continued studying series and related matters. Rey Pastor also introduced Tauberian theorems in Spain in 1933, as well as ideas of Topology following the Hausdorff style: G_δ and F_δ sets are defined in the 1941-42 lecture notes on Analysis by San Juan, but with no applications.

2. Survival and continuities after the Civil War

After the Civil War all scientific institutions prior to 1939 were overrun by the *Instituto 'Jorge Juan' de Matemáticas*, a section of the CSIC active until 1982. For years the group concentrated around this Institute governed all official mathematical activity: The few available University positions were unattainable for candidates without the '*Jorge Juan*' approval. Francisco Navarro Borrás (1905-1970), who held the chair of Rational Mechanics at Madrid, was CSIC's man in contests for Mathematics positions –both University and Secondary School ones– from the early forties until the 1960s: The Spanish Official Gazette shows he was almost invariably the head of the examining board.

After the war some Spanish mathematicians (*e.g.* Ríos and San Juan) could continue their work on previous research, or even start other lines (*e.g.* Abellanas and Botella) favoured by the Regime with some funding. But the importance of pure Mathematics was small: Autarchy promoted other Sciences –and related Technologies– like some branches of Chemistry and Physics, Pharmacy and applied Physiology. Applied Mathematics only received some boost when aircraft building was given priority: An *Escuela de Aeronáutica* was created and a number of mathematicians taught there for years[13].

Fortunately, in those years doing pure Mathematics was largely an individual affair, and many mathematicians survived as Secondary School teachers or through private lessons, even some did research on topics where interaction with other mathematicians was not an urgent matter: Cuesta is an example.

With the USA-Spain treaty of 1952, Spain entered the UNO, and scientific policy slowly started to converge with the current standards: internationalisation, collaboration, English as the interchange language. This fact, population increase, and faculty ageing eventually contributed to some of these Spanish outlier mathematicians becoming members of the established community.

3. Journals and publishing, II

Publishing in Spain during the post-war years happened in the *Revista Matemática Hispano Americana* (RMHA), series IV –some enthusiasts had kept it alive (series III) during the war in the Republican Zone. For elementary Mathematics there existed *Matemática Elemental*, also refounded in 1941, and changed into *Gaceta Matemática* in 1949. Both were CSIC journals. The *Revista de la Academia de Ciencias* also contained mathematical papers, as before the war. Nevertheless, *Euclides*, a journal mostly dedicated to students preparing examinations for Engineering Schools, appeared in 1941 with a large Mathematical content: It was founded by José Barinaga (1890-1958), expelled from his chair at Madrid, and José Gallego-Díaz (1913-1965), an engineer and mathematician. *Euclides* survived until 1960, and it provides an example showing the existence of something close to a Spanish mathematical community in the post-war years[14]. The *Revista de la Universidad de Madrid* also offered Mathematics in its section *Ciencias*.

In the early forties some Spanish mathematicians had already published in established international journals: *e.g.* Germán Ancochea (1908-1981) in *Annals of Mathematics* and *Journal für die reine und angewandte Mathematik*, San Juan was also a current author in non-Spanish journals like the prestigious *Portugaliae Mathematica*. Abstracts and reviews of the Spanish production were rapidly available in *Zentralblatt für Mathematik* and *Mathematical Reviews*, started in 1940, so the Spanish production on those years, even with the delay imposed by WW II, is well covered. We have not read any unfavourable comment on Cuesta's mathematics, neither in *Mathematical Reviews* nor in *Zentralblatt*.

Mathematics

Cuesta's main result, the systematic construction of all total orderings[15] on any set, was achieved by firmly believing in actual infinities and by a thorough usage of the well-ordering principle and techniques developed by Steinitz in his 1910 paper on Algebraic Field Theory[16].

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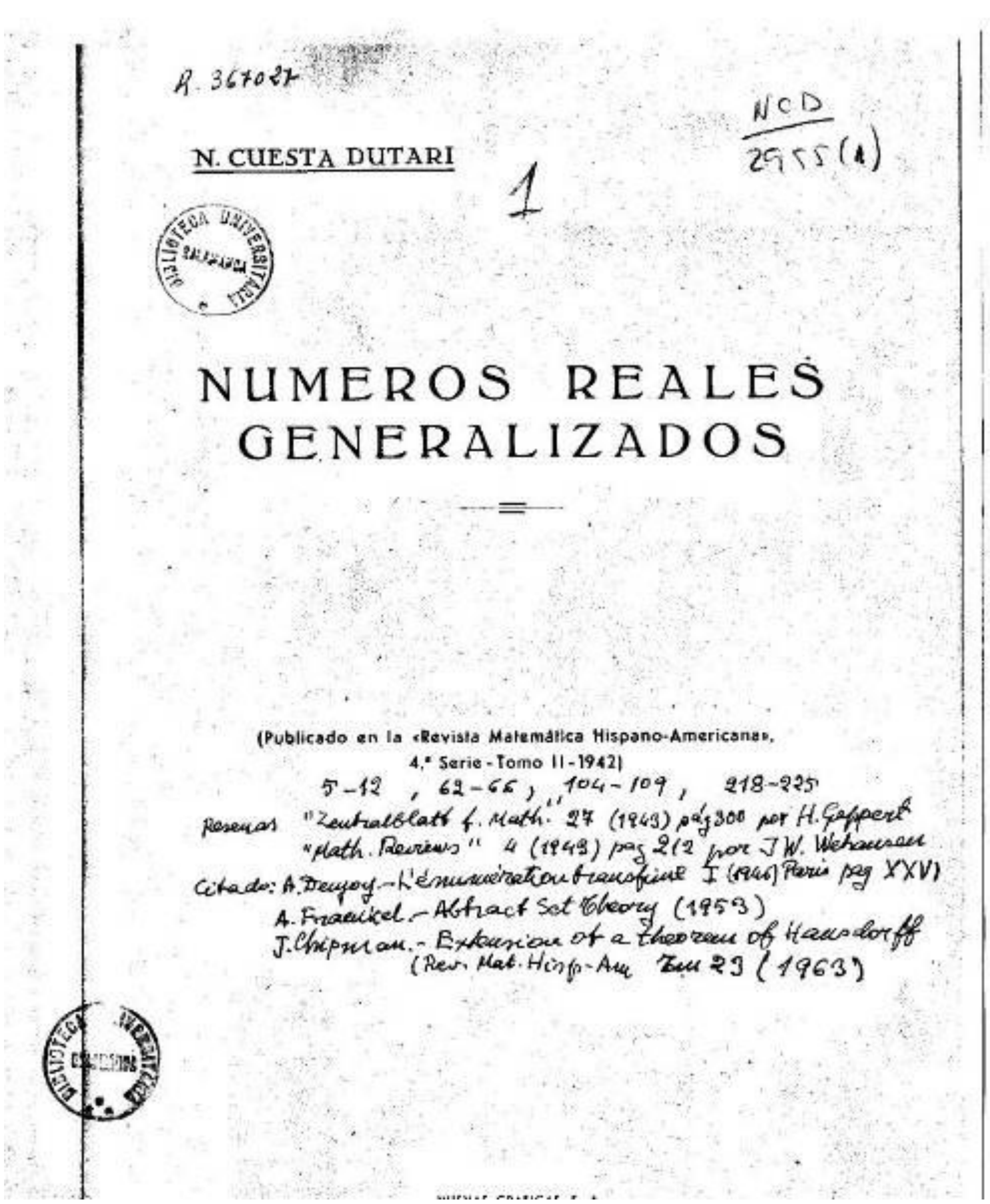
[13] See: San Juan R (1944), Algunas cuestiones de la mecánica de fluidos, *Revista de Aeronáutica*, 46. 16 pp.

[14] See the history of *Euclides* and its role in introducing Gödel in Spain: Olmos P and Vega L (2003) La recepción de Gödel en España, *ÉNDOXA* (*Series filosóficas*) 17, 379-415.

[15] The German Egbert Harzheim, who corresponded with Cuesta in the mid-sixties, acknowledges him as the author of the first book (*Matemática del Orden*) dealing exclusively with order properties. Harzheim E (2005) *Ordered Sets*, Springer-Verlag, Heidelberg.

[16] Steinitz E (1910) Algebraische Theorie der Körper , *J. Reine Ang. Mathematik*, 137, 167-309.



N. CUESTA DUTARI

1

NUMEROS REALES GENERALIZADOS

(Publicado en la «Revista Matemática Hispano-Americana», 4.ª Serie - Tomo II - 1942)

5-12, 62-66, 104-109, 218-225

Reseñas "Zentralblatt f. Math." 27 (1943) pág 300 por H. Geppert
"Math. Reviews" 4 (1943) pag 212 por J.W. Wehausen

Citado: A. Denjoy - L'énumération transfinie I (1946) Paris pag XXV)
A. Fraenkel - Abstract Set Theory (1953)
J. Chipman - Extension of a theorem of Hausdorff (Rev. Mat. Hisp-Am Tom 23 (1963)

Fig. 2: First papers. Observe Cuesta's notes.

1. On algorithms, I

Cuesta did not use the word "algorithm", though he was aware of the possibility of *maquinización*, a word of his invention, in many instances. Moreover, he used the adjective *cronológica* to refer to an auxiliary well ordering needed in his theory. The rationale behind his method comes next.

Let A be any set, and let $a \in A$. Now, well-order A in such a way that a be its first element: This defines a sort of time. Figure out that a occupies some position on an ideal infinitely long supporting line. Then a automatically creates two "vacant places": one in its left side, the other on the right one, yielding a pattern: – a –. Let b be the first element in A – $\{a\}$ and use it to occupy one of those places, say the right one. The result is – a – b –: the vacant place at the right is turned into – b –. Note that there is a decision, choice, or branching (right *vs.* left). Proceed by letting the first element c in A – $\{a,b\}$ occupy one of the three vacant places by changing the chosen empty place into the pattern – c –. Repeat the process.

The choice regarding the place to be changed into – *x* – when introducing any *x* yields a decision tree, within which all possible total orderings of the initial set[17] are represented.

Once the process is over, the problem arises of *naming* that order. Indeed, the order itself could be its name, but this would be impractical by two reasons: First, it would violate the *principle of Borges*[18]; second and worse, isomorphic orderings would receive different names. Cuesta wanted to name order types (isomorphism classes), not individual orders. Therefore he criticised a proposal by Arnaud Denjoy for naming denumerable total orderings. For denumerable ordinal numbers, Cuesta devised this procedure:

Let *S* be a denumerable ordinal number and n_S its name, formed with symbols of some finite alphabet. n_S is an ordinal number complying with Borges' principle: type(n_S)<*S*. Exceptionally, the principle may be violated in some cases; because *e.g.* the empty set needs a name, O=n_O. Ordinals *J* such that type(n_J)<*J* are called *ineffable* and may be needed at some stages, but kept to a minimum. They may appear either by occasional enlarging of the initial alphabet or by recurrence on already existent names. Cuesta's method for naming denumerable ordinal numbers with the initial binary alphabet {0,1} is shown here:

Table 1: Naming denumerable ordinals.

Ordinal	Name	Ordinal	Name	Ordinal	Name	Ordinal	Name
o	**o**	**1**	**1**	ω+1	21	ωx2+1	201
		2	**10**	ω+2	210	ωx2+2	2010
		3	11	ω+3	211	ωx2+3	2011
		4	100	ω+4	2100	ωx2+4	20100
		5	101	ω+5	2101	ωx2+5	20101
		…	…	…	…	…	…
		ω	2(new)	ωx2	20	ωx3	200
Class N1				Class N2			

Boldface: ineffable ordinals. (new) = an alphabet extension

2. On algorithms, II: Inversion of the viewpoint and the proof of "the theorem"

In his construction of total orderings, Cuesta freely used the idea of "filling an empty place" with a given pattern in order to create the order relationship. Now, the viewpoint is inverted, starting with a pre-existent order on some set, considering the addition of elements one at a time, and watching things happen:

Let (*M*,<) be a totally ordered set. A *gap* in it is a disjoint pair (*A*,*B*) of subsets of *M* such that *A*<*B* and *A* ∪ *B* = *M* hold: Now, let *p* be some element which will be used to *fill* the gap: a new set (*M* ∪ {*p*},<) is built under the condition that *A*<*p*<*B*. The insertion of *p* creates two new gaps in (*M* ∪ {*p*},<) "descending" from (*A*,*B*), *viz.* (*A* ∪ {*p*},*B*) and (*A*,*B* ∪ {*p*}). If *v* is a name given to (*A*,*B*), then the two descending gaps could be named, respectively *vi* and *vf*, according to whether the new element was added to the initial (*i*) section *A*, or to the final (*f*) one *B*.

Now, start with (*M*,<)=Ø and consider the gap (Ø,Ø), which will be named O. Then, adding further elements will provide a construction of *binary* names like O*iifiiiif...* This procedure yields the proof of the theorem cited in Section 1.

3. The remarkable 1955 result on the Sierpinski triangle

When shortly after WW II English became the standard for scientific communication, the need of publishing in this language soon became a must. With some delay, Cuesta undertook his only paper in English and gave it to print in 1955[19]. It

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[17] Cuesta N (1958) *Matemática del Orden*, Real Academia de Ciencias, Madrid.

[18] The name comes from J. L. Borges: a King wanted such a detailed map of his kingdom that eventually his geographers presented him a 1:1 reproduction.

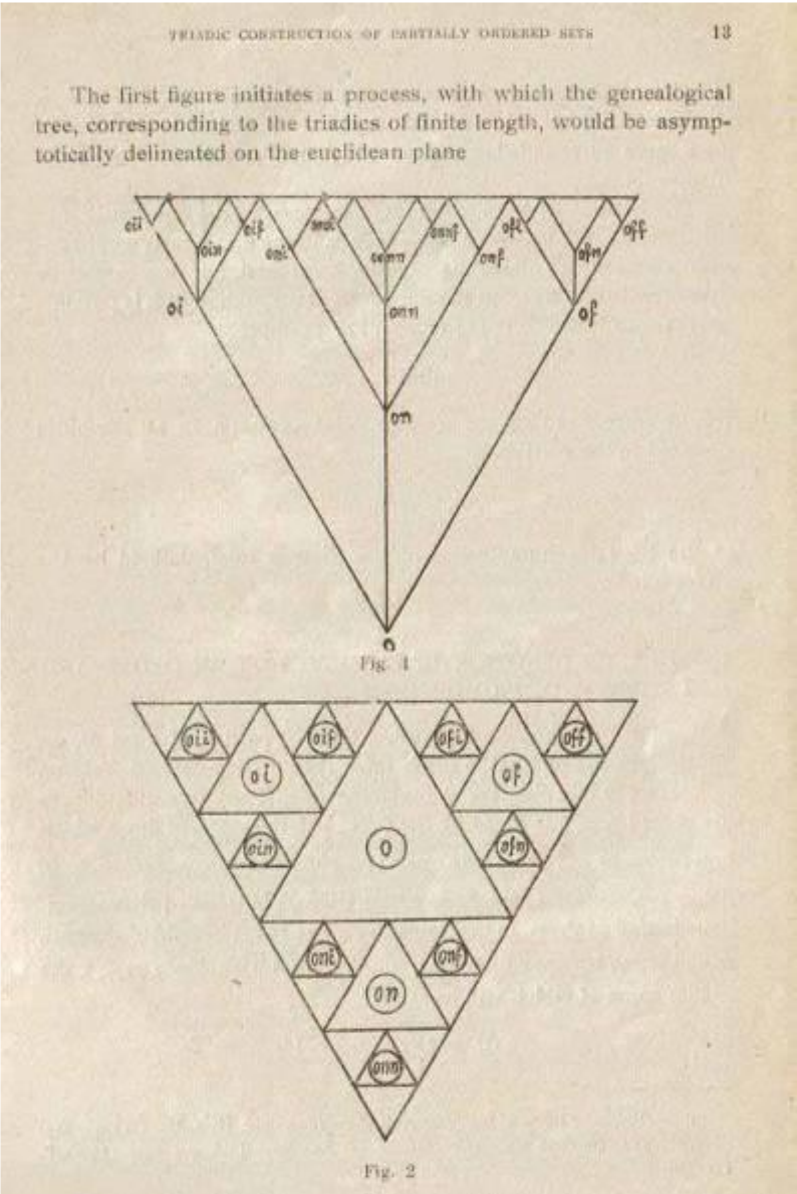
[19] Cuesta N (1955) Triadic construction of partially ordered sets, *Acta Salmanticensia*, Ser. Ciencias, Nueva Serie, Vol. I (4), Universidad de Salamanca.

contains a generalisation of the 1943 algorithm for total orderings to the case of partial orders, by shadowing the binary technique to extend it to *pairs of gaps*:

Let (*M*,<) be a totally ordered set. A *tripartition* of *M* is a triad (*A*,*B*,*C*) of pairwise disjoint subsets of *M* such that *A*<*B*<*C* and *A* ∪ *B* ∪ *C*=*M* hold. One such tripartition may be named τ. Now, let *p* be some element and use it to fill *one* of the two gaps, (*A*,*B*) or (*B*,*C*). The insertion of *p* can be done in any of the forms (*A* ∪ {*p*},*B*,*C*), (*A*,*B* ∪ {*p*},*C*), or (*A*,*B*,*C* ∪ {*p*}). Again a branching process is generated according to the selected choice, and the descending tripartitions will be named, respectively τi, τn and τf, meaning that the new element was added to the initial (*i*) section *A*, to the central one *B* (*n*) or to the final (*f*) one, *C*.

Now, begin with (*M*,<)=Ø and consider the tripartition (Ø,Ø,Ø), named O. Then, adding elements will yield *triadic* names like O*iifnniniiif* ...

This procedure rediscovers the now famous Sierpinski Triangle[20], as shown in the illustrations of Cuesta's original paper: Any denumerable partial order is described by a path joining the centres of ever smaller triangles according to the chosen sequence of *i*'s, *n*'s and *f*'s.


TRIADIC CONSTRUCTION OF PARTIALLY ORDERED SETS 13

The first figure initiates a process, with which the genealogical tree, corresponding to the triadics of finite length, would be asymptotically delineated on the euclidean plane

Fig. 3: Recreation of the Sierpinski Triangle in the 1955 paper.

[20] Sierpinski W (1915) Sur une courbe dont tout point est de ramification, *Comptes Rendus*, 302-305.

A few notes on the 1955 paper translation

This section briefly studies some non-mathematical aspects of the 1955 paper. In a most typical way of his, Cuesta and a French friend of his named Antoine O. Broucke wrote the poor home-made translation constituting the paper discussed in the previous section 3. He soon became aware of the bad quality of the language. As in those years he corresponded with Marshall Stone, we know that Stone wrote him that the text *was not in English*. In figure 4 is a letter sent in 1982 by Cuesta to the author of this paper, regarding a possible new English translation of *Triadic Construction*. The task was not performed, basically because at that time the would-be translator hardly understood the Mathematics in it!

Salamanca A de del 1982
Sr. Dn José Pacheco Castelao

Mi querido amigo: Te devuelvo mi viejo artículo que pusimos en un extraño idioma, "alfamiado" de inglés, entre Broucke y "moi"! Es evidente que ganaría bastante escrito en inglés, y encima por quien, como tú, se lo ha estudiado y puede quizá sacarle algunas extensiones útiles. Yo ahora ya no puedo hacer esto último, pues quiero terminar el libro "de la invención e introducción del análisis infinitesimal en España"; sólo terminándolo, servirá para algo la labor informativa que efectué en archivos, en viejas revistas, y viejos libros.

He revisado las reseñas que de ese trabajo conozco. La del Zentralblat 65 (1956) 284-285 de 24 líneas la hizo Kurepa (es croata, casi de mi edad) inventor de las "ordenaciones ramificadas". Tiene un pequeño error, pues dice se pone I < F cuando algún i y algún f de los respectivos verifican i < f. No tiene eso interés; se ha de verificar i < f cualquiera que sean i y f.

La del Math. Revs 17 (1956) 950 de 21 renglones la hizo Baer (Notre Dame Indiana U.S.A) que me resultó bastante [illegible]. No alude al "inglés" del artículo: Stone me dijo que "no era inglés". Desconozco la de las "Referata Mathematicas" de los rusos, que no sé si se [illegible] encontrar en Madrid, o que quizá tenga Emiliano Aparicio en Bilbao.

Quiero advertirte que yo lo incluí, pero con una redacción muy diferente, en mi "Matemática del Orden" (Rev. Acad. Cien. Madrid Tº 52 y 53, 1959)

A mi me satisface más mi Tesis (Rev. Mat Hisp-Am 1943) allí titulada "Teoría decimal de los tipos de orden" si hoy la publicara, la titularía más expresivamente, "Construcción diádica de ordenaciones totales". En realidad los dos trabajos podrían juntarse en uno, y si tienen algo que aún me satisface, es que la construcción es sistemática, introduciendo los elementos uno a uno y haciéndolo de todas las maneras posibles; por eso se obtienen "todas" en un caso y en otro.

Tú harás con él lo que te parezca más oportuno y yo te lo agradeceré en todos los casos posibles.

Un abrazo muy cordial
N Cuesta

Fig. 4: Letter to the author, on the 1955 paper.

The letter shows how aware Cuesta was of the value of his Mathematics: In addition to the reviews in *Mathematical Reviews* and *Zentralblatt*, he humorously refers to his English as *aljamiado*[21]. Most interesting is the reflection on his beloved 1943 thesis, from which the 1955 paper would be a natural by-product, deserving a joint publication. Cuesta's research on ordered sets, including a newer version of the 1955 paper, is gathered in his 1958 book *Matemática del Orden*, cited above.

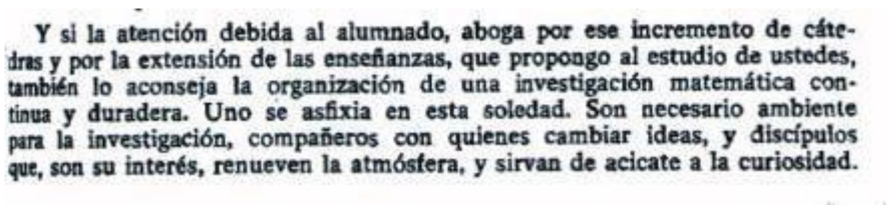
Conclusion: The never-ending isolation

The idea of establishing a Mathematics degree at Salamanca pervaded Cuesta academic life. His personal view, an academia where Mathematics and its teaching would be over any other consideration was an idealised picture of those Göttingen years he much admired and tried to imitate in Seminars he conducted on Monday evenings. For him, scientific policy as an everyday affair meant little, and his Spartan lifestyle could not be easily imposed on other people.

In his 1963 address[22] to the annual meeting of Spanish Mathematicians held at Salamanca, we read his plea for an end of mathematical isolation:

> *"Uno se asfixia en esta soledad. Son necesarios ambiente para la investigación, compañeros con quienes cambiar ideas, renueven la atmósfera, y sirvan de acicate a la curiosidad".*

To understand it, Cuesta was the only Mathematics Professor at Salamanca between 1947 and 1966, with a short interlude in 1955-56. The answer came from the regime evolution in the late 1960s, when, favoured by a series of technocratic Governments, the educational system entered a growing stage in order to cope with an ever larger population and the funding conditions imposed by the World Bank to contribute to the country modernisation, and which led to the 1970 *Ley General de Educación*.



Y si la atención debida al alumnado, aboga por ese incremento de cátedras y por la extensión de las enseñanzas, que propongo al estudio de ustedes, también lo aconseja la organización de una investigación matemática continua y duradera. Uno se asfixia en esta soledad. Son necesario ambiente para la investigación, compañeros con quienes cambiar ideas, y discípulos que, son su interés, renueven la atmósfera, y sirvan de acicate a la curiosidad.

Fig. 5: Final words of the 1963 address.

Without these general environmental conditions, the isolation of Cuesta and the difficulties for his beloved degree in Mathematics would not have been overcome. Indeed many people in other Universities had interests similar to his, but this general picture is a reasonable one.

Table 2: Some new Mathematics studies. Spain, 1963-1971.

Year	University	Promoter	Promoter = School?
1965	Santiago	Vidal	Yes
1967	Seville	Castro	Yes
1970	Valladolid	Martínez-Salas	No
1970	La Laguna	Cascante	No
1971	Salamanca	**Cuesta**	**No**

As he had developed his own idealised picture of actual scientific activities disdaining mundane servitudes like funding struggles, fierce battles around tenures, and other everyday questions, his peculiar ideas on how to do Mathematics crashed with the sudden expansion of the higher educational system after 1970. Cuesta had two students, but only one

[21] *Aljamiado*: Medieval Spanish, written in Arabic characters. Here: the paper was written in Spanish with English words.

[22] Cuesta N (1966) Clima humano necesario para crear una escuela española de matemáticos, *Actas de la* IV *reunión anual de matemáticos españoles*, *Acta Salmanticensia* 6(4), 164-168.

obtained a tenured position and soon moved to Santiago. Cuesta's dream was not followed by academic descendants of his own, but his Mathematics is still there.

Appendix. Publications by N. Cuesta

	Number	Cumulative
Papers in Journals		
Revista Matemática Hispano Americana	39	39
Gaceta Matemática	13	52
Acta Salmanticensia	7	59
Llull	4	63
Revista de la Academia	4	67
Collectanea Mathematica	3	70
Matemas	3	73
El Basilisco	2	75
Revista de Filosofía	1	76
Boletín Biblioteca Menéndez Pelayo	1	77
Zubía	1	78
Proceedings		
Reuniones Anuales de Matemáticos Españoles	9	87
Jornadas Hispano-Lusas	6	93
Books	**8**	**101**
Other	**> 6**	**> 107**

SCIENCE AND NATIONAL IDENTITY AFTER 1945

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 734.

SCIENCE AND NATIONAL PRESTIGE: EARLY DUTCH RADIO ASTRONOMY

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Abstract

In the first post-war decade, the new field of radio astronomy grew steadily in several countries, including Great Britain, the USA, Australia and the Netherlands. The instruments it used (radio telescopes) required a lot of funding. As radio astronomy originated from wartime radar research, and the first radio astronomers were mainly engineers and physicists trained in radar technology, military patronage and funding were self-evident. There were good reasons to fund radio astronomy during the Cold War. It was supposed to stimulate technological innovations that could be used for military purposes. Moreover, big science projects served national prestige. Especially in the USA, the ties between radio astronomy and the military were very strong throughout the 1940s and 1950s.
In my talk, I will analyse the funding of radio astronomy in the Netherlands. The Dutch situation differed strongly from the other countries. It has not been thoroughly studied yet. The Netherlands was the only country where radio astronomers were formally trained as astronomers, and where the research exclusively served astronomical goals. At first sight, there are no clear links with the military or with possible spin-offs. Nevertheless, the government and the Dutch Organisation for Pure Scientific Research (ZWO) provided a huge amount of their science budget for radio astronomy. The question is why –in an era of post-war recovery– they gave radio astronomy such a priority.

Introduction

After the Second World War, a new field of astronomy came into being, namely radio astronomy. This new field arose worldwide, but in this article I will focus on the situation in the Netherlands. More specifically, I will explain how Dutch radio astronomers succeeded in establishing themselves and how they convinced the Dutch government that this novel, uncertain and expensive scientific field was worth funding. To this end, Dutch radio astronomers concluded strategic alliances with industry and public utilities and they used clever argumentation in emphasising practical spin-offs of their research. Moreover, there was the element of national prestige, to which the Dutch government was especially sensitive. The radio astronomical project could contribute to the national prestige of the country and would stimulate national pride. This was both because of the conspicuous instrumentation as because of the fact that radio astronomy was a field in which the Netherlands could play a pioneering role.

The very beginnings

Already during the *interbellum* period, Dutch astronomers had firmly established their position in the international scientific community. Especially Leiden Observatory of the University of Leiden had become an international centre of astronomy. Under the directorship of Willem de Sitter (1918-1934), the observatory was thoroughly reorganised, which led to an enormous increase in quantity and quality of the output.[1]

However, in the Netherlands the problem remained that due to the cloudy climate, the Dutch were severely hindered in making optical astronomical observations. So for observations, they were largely dependent on other countries. But immediately after the Second World War, new opportunities arose. Grote Reber, an electronic engineer and radio amateur from Chicago, was able to detect radio emission from the Galaxy with a self made radio telescope. His findings were published in the *Astrophysical Journal* of 1940.[2] In the Netherlands, this article of Grote Reber came under the eye of the Leiden astronomer Jan Hendrik Oort (1900-1992). Oort immediately realised that Reber's findings could be extremely important for astronomical research. For the great advantage of radio waves was that they were neither hindered by interstellar dust, nor by earthly clouds. The latter made them –contrary to optical waves– very suitable for 'observation' in the Dutch cloudy climate, even during the day. Moreover, Dutch astronomers had been trying to unravel the structure of the Milky Way for several decades. This was in fact the legacy of the Groningen astronomer Jacobus Cornelius Kapteyn (1851-1922), as Oort explained:

> Consistently pursuing the ideal of revealing the general structure of the entire stellar system, Kapteyn laid the foundations for much of the modern work in this line, thereby initiating essentially all the methods used today.[3]

Unravelling the structure of the Milky Way had proven to be impossible by means of optical waves, as only a small part of the Milky way was observable at optical frequencies. Oort realised that the problem might be solved if a spectral line could be found within the radio spectrum.

In 1944, Utrecht astronomy student Henk van de Hulst confirmed that there was a spectral line of hydrogen at 21 cm, well within the radio spectrum. In the interstellar matter of the Galaxy, there was an abundancy of hydrogen, so the hydrogen line would be strong enough to be detected with a radio telescope.

We can hardly overestimate the importance of the discovery of this 21 cm hydrogen line. As radio astronomer Richard Strom stated: 'This discovery would set the agenda for generations of radio astronomers in Holland and elsewhere.'[4]

In the same year 1944, Oort had a plan ready for the building of a radio telescope. He suggested a telescope with a dish of 25 metres, with a receiver built by Philips, the Dutch electronics company. As soon as the war was over, Oort was appointed professor and director of Leiden Observatory, and he judged it the right time to submit his plan to the Dutch government. But because of the high costs, it was impossible for the government to concede his demands. However, Oort would not give up. But was his project viable in the post-war context? Could science be a priority in a country that was devastated by the war?

Towards a knowledge-based economy

In fact, immediately after the war, there was worldwide a very positive attitude towards science. In the US, Vannevar Bush (the director of the Office of Scientific Research and Development) had just written his famous report *Science. The Endless Frontier*, which would ultimately serve as the basis for the creation of the National Science Foundation (NSF) in 1950.

In his report, Bush strongly emphasised 'pure' or 'basic' research, as this would ultimately lead to useful practical applications. For Bush, scientific progress had proven to be extremely valuable during the Second World War: medical applications –based on former basic research– had reduced the death rate in the Army. Radar –an applied science which had its origin in former research in physics– had played a vital role in defeating the Germans and driving back the Japanese.

The Dutch government too believed that science would play a major role in the country's revival. Therefore, Prime Minister Schermerhorn wanted to create a 'braintrust' of researchers, who would cooperate with the industry and the

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[1] Baneke, D., (2010), Teach and travel: Leiden Observatory and the renaissance of Dutch astronomy in the interwar years, *Journal for the History of Astronomy*, XLI, pp. 167-198.

[2] Reber, G. (1940), Cosmic static, *Astrophysical Journal*, 91, pp. 621-624.

[3] J.H. Oort, Problems of Galactic structure, *The astrophysical journal*, 116, 2 (1952), 233-50, p. 233.

[4] Strom, R. (2005) Radio Astronomy in Holland Before 1960: Just a Bit More Than HI, in: W. Orchiston (ed.) *The New Astronomy: Opening the Electromagnetic Window and Expanding our View of Planet earth* (Dordrecht: Springer), 93-106, p. 94.

government to conduct research in several new fields.[5] This eventually led to the creation of the Dutch national 'Organisation for Pure scientific Research' or ZWO. It was provisionally active from 1947 onwards, but only officially launched in 1950. That war could stimulate the emergence of national scientific organisations was well known. As the first director of ZWO, J.H. Bannier stated:

> Wartime fertility in the family of scientific organizations, as witnessed in the United States during earlier wars by the birth of the National Academy of Sciences and the National Research Council, seems to be a universal phenomenon. In a record number of countries World War II has been responsible for the creation of councils for scientific research and science foundations. Just now the happy event has taken place in the Netherlands.[6]

During the discussions about the founding of ZWO, the report of Vannevar Bush was often referred to in stressing the importance of fundamental research for the prosperity of a nation. It was also argued that the new organisation had to subsidise in the first place projects that were of another scale than those that had always been subsidised by the universities. That meant that the organisation had to support in the first place 'large scale research'[7], especially such initiatives that aimed at *national* organisations for new types of research. Further, an emphasis was placed on fields in which the occupied countries were allegedly seriously behind after the war. These were amongst others mathematical research with very fast calculators and nuclear physics. Therefore, before ZWO as a whole was founded, some smaller initiatives were already taken: in February 1946, the Mathematical Centre (MC) a national foundation for mathematics was created; and in May 1946, a foundation for nuclear physics: the Foundation for Fundamental Research on Matter (FOM). These foundations operated under the umbrella of ZWO. The allocation procedure of these foundations was the same as for individual projects, but the difference was that their projects were executed outside the universities or were a joint undertaking of several universities and had a long term character.

Around 1948, the astronomers from the observatories of Leiden and Utrecht were already cooperating informally for some time and now they considered it a good idea to create an organisation that would efficiently coordinate their research at a national level. Therefore, in the autumn of 1948 Oort and his Utrecht colleague Marcel Minnaert took the initiative to found the research group, named the National Foundation for Radio Astronomy or SRZM ('Stichting Radiostraling van Zon en Melkweg'). It was of vital importance that not only astronomers were members of the SRZM, but also representatives of the industry and public utilities. This made the foundation transcend the level of a merely interuniversity organisation and made it a truly *national* organisation. The board included besides astronomers of the observatories of Leiden and Utrecht also representatives of Philips NatLab (the research laboratory of Philips electronics company), of the PTT (the Dutch Post, Telephone and Telegraph Service), and of the KNMI (the Dutch Royal Meteorological Institute).

Gathering allies

The choice of the radio astronomers to include industry and public utilities in the foundation was strategic in several ways. It was partly related to the fact that the origin of Dutch radio astronomy differed from that in other countries. In other countries (such as Great Britain, Australia...) engineers and physicists with a background in war industry –more specifically in radar– took the initiative in radio astronomy. In the Netherlands, the initiative came from professional astronomers. This had the advantage that these astronomers had well defined research questions in mind and they knew very well how to handle their data. On the other hand, it had the disadvantage that they lacked the technical know-how about the building of their equipment. To find a solution to this problem, the Dutch astronomers concluded alliances with industrial partners and public utilities, which could provide them the necessary technical knowledge.

Especially the relation with the PTT deserves attention. Not only were the astronomers interested in cooperation with the PTT, the PTT was interested in cooperation with the astronomers too. This was because of the ionosphere. Part of the program of the astronomers was to measure the radiation of the Sun. Radio radiation of the Sun had an influence on the ionosphere, which in turn influenced the radio communication of the PTT. Knowledge of these disturbances held the promise of improving the PTT's long-distance communication services.

The cooperation with the PTT was very fruitful for the astronomers. As said before, the first plan Oort handed in for his radio telescope in 1945 was not approved, because it was too expensive. The PTT provided an alternative. At the end of the war, they had salvaged several Würzburg radar reflectors, which had been used by the German forces during the War,

[5] Kersten, A.E. (1996) Een organisatie van en voor onderzoekers. De Nederlandse Organisatie voor Zuiver-Wetenschappelijk Onderzoek (Z.W.O.) 1947-1988, p. 10.

[6] Bannier, J.H. (1951) Z.W.O., the Netherlands Science Foundation, *Science*, 113 (2930), 197-201, p. 197.

[7] Reinink, H.J. (1947) De voorgestelde regeling van de Organisatie voor Zuiver Wetenschappelijk Onderzoek i.o., in: *De organisatie van het zuiver wetenschappelijk onderzoek in Nederland. Verslag van het Congres gehouden door het Verbond van Wetenschappelijke Onderzoekers te Amsterdam op 22 november 1947* (Leiden: Verbond van Wetenschappelijke Onderzoekers), 3-11 , p. 7.

and the PTT had brought these to their radio transmitting station at Kootwijk. In 1948, they allowed the radio astronomers to use one of these Würzburgs for their research. The PTT gave the astronomers all kinds of research facilities at their Kootwijk site, because they thought they too would benefit from their research. So thanks to the PTT, the astronomers had their first radio telescope in Kootwijk. It was a much smaller and less sensitive one than Oort had originally in mind (with a dish of only 7.5 metres). But nevertheless they could finally start.

And the cooperation with the PTT offered another advantage. The fact that the radio astronomical research could have practical applications in telecommunication guaranteed that the Dutch government would be more inclined to fund the project. Indeed, supporting Dutch industry was a major government concern in these years of post-war reconstruction. When in 1948, Oort wrote a memorandum for ZWO to get funding, Minnaert from Utrecht Observatory advised him to refer to such practical applications:

> In fact, it is a pity that it says nothing about the practical meaning of the research for the radio service: knowledge of the ionosphere and the disturbances in it; probably the possibility to predict these disturbances. I certainly believe that this consideration would be an argument to give us a grant. Maybe you should add a paragraph to the memorandum on a third page.[8]

This may seem somewhat surprising, because ZWO was founded explicitly as an organisation for pure scientific research. In general practical applications and stimulation of the industry were seen to be important, but pursuing such goals was not the task of ZWO. As Minister of Education, Van der Leeuw had explicitly stated: the task of ZWO was to support entirely free and pure research. However, these words could not hide the fact that ZWO clearly privileged projects of which the practical applications were immediately visible, like nuclear physics.

In 1948, SRZM successfully applied for the first time with ZWO for a start up budget. They received for this first year 13,650 guilders for preparatory radio astronomical research.

Radio astronomy and national prestige

The astronomers made sure that their efforts did not go unnoticed, and they kept the press informed. As soon as observations with the Kootwijk started, a lot of articles appeared in Dutch newspapers, in which we clearly read feelings of national pride and the expectancy that the Netherlands could become internationally important in the field of radio astronomy. Besides, it was emphasised that the Dutch industry would be stimulated by the building of the radio telescopes.

Let us for example take a look at an article that appeared in the Dutch newspaper *Het Vrije Volk* (figure 1).

ASTRONOMEN LUISTEREN NAAR DE STERREN

Nederlanders doen radio-waarnemingen in heelal

Binnen tien jaar belangwekkende ontdekkingen verwacht

De sterrenkunde heeft in het fototoestel nog steeds een machtig hulpmiddel. Hier ziet men de draaiing van de aarde — de schijnbare beweging der hemellichamen — in beeld gebracht: op de lang belichte fotografische plaat hebben de sterren streepjes getrokken. Het vlekje in het midden wijst op de aanwezigheid van een komeet.

(Van onze speciale verslaggever)

In Nederland wordt naar de sterren geluisterd! Op de Veluwe, nabij het radiostation Kootwijk, doen Nederlandse sterrenkundigen sinds kort en voorlopig nog met betrekkelijk geringe middelen, waarnemingen in het heelal met behulp van een bijzondere radio-ontvanginstallatie.

Maar er bestaan plannen voor de bouw van een veel grotere luisterpost. Twee Nederlandse industrieën, Werkspoor in Utrecht en Philips in Eindhoven, zullen de ingewikkelde en hoogst moderne apparatuur gaan maken. Als aan deze plannen misschien volgend jaar uitvoering wordt gegeven, zullen de Nederlandse sterrenkundigen de beschikking krijgen over de beste apparatuur, die er tot nu toe bestaat voor het peilen en het aftasten van het heelal langs de weg der radiostraling, — een nieuwe methode, die pas na de oorlog tot ontwikkeling is gekomen.

Fig. 1: Newspaper article in *Het Vrije Volk*, 18 January 1950.

[8] Minnaert to Oort, March 29, 1948, Oort archives, Box 162, Folder B.

Free translation of the Dutch text in the article:

> *Astronomers listen to the stars. The Dutch make radio observations in the universe. Important discoveries are expected within ten years.*
>
> *In the Netherlands people listen to the stars! In the Veluwe, near the radio station of Kootwijk, Dutch astronomers are making observations of the universe, yet with modest resources.*
> *But there are plans for the building of a much bigger instrument. Two Dutch industries, Werkspoor in Utrecht and Philips in Eindhoven, will manufacture this complicated and ultra modern equipment. If the plans could be executed next year, Dutch astronomers will possess the best equipment in the world to make radio observations in the universe.*

This article emphasises that Dutch companies, such as Werkspoor and Philips, would be stimulated by the radio astronomical project. Moreover, the country would be proud to possess the best equipment in the world.

But the radio astronomers did not yet have their telescope with the 25m dish. In 1949, SRZM presented to ZWO a plan for the larger telescope which would be much more sensitive than the one in Kootwijk. The estimated price tag was around f 200,000[9], which was about 17 % of the ZWO-budget for 1950, being f 1,401,293[10]. The plan was not approved at the first application. One of the reasons for this was probably the poor progress of the Kootwijk observations during the first years. The radio astronomers hadn't succeeded in detecting the famous 21cm hydrogen line yet. When, in May 1951, the astronomers finally did succeed in detecting this line, this detection was extensively covered in the media, for example figure 2.


Kootwijk maakt radio-contact met gaswolken in Melkweg

Ontdekking van Nobelprijs-formaat door Nederlandse astronomen

Fig. 2: Newspaper article in *Het Vrije Volk,* 5 July 1951.

Translation of the Dutch text:

> *Kootwijk makes radio contact with gas clouds in the Milky Way. Discovery of Nobel Prize significance by Dutch astronomers.*

This detection of the hydrogen line –and its extensive coverage in the media– certainly furthered the decision of ZWO to finance the building of the large telescope that same year. In their argumentation, the members of the advisory committee of ZWO emphasised –again– the spin-offs of the radio astronomical research. They argued that the building of this radio telescope was not only of importance for the astronomers, but –because of the involvement of several other sectors– it was important for the education of other scientists and technicians too. Besides, the commission was convinced that radio astronomy was a field in which the Netherlands could be internationally at the forefront.[11]

This decision of ZWO to finance the building of the large telescope was, of course, also covered in the media (see figure 3).

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[9] Minutes of the 6th meeting of SRZM, September 20, 1949, Archives SRZM at Leiden Observatory.

[10] Jaarboek ZWO, 1950.

[11] Kersten, A.E. (1996), *een organisatie van en voor onderzoekers. De Nederlandse Organisatie voor Zuiver-Wetenschappelijk Onderzoek (Z.W.O) 1947-1988*, p. 98-99. Unfortunately, we were unable to fin they original text of the advisory committee.

Nederland krijgt grootste radiotelescoop ter wereld

Heelal te „zien" ondanks wolken

Fig. 3: Newspaper article in *Het Parool*, 22 October 1951.

Translation of the Dutch text:

The Netherlands get the largest telescope in the world. Universe 'visible' despite clouds.

The telescope was built in the woods of Dwingeloo (Drenthe). It was completed in 1955 and became in fact a symbol of national identity when it was officially inaugurated by the Dutch Queen Juliana herself on 17 April, 1956. For several months, it would be the biggest telescope in the world.

Fig. 4: The Dwingeloo telescope.

Fig. 5: Inauguration by the Dutch Queen Juliana, 17 April 1956.

Conclusion

I hope I have made clear that in the successful establishment of radio astronomy in the Netherlands, national pride and national identity were considerations that did matter.

A helpful circumstance was that after and because of the Second World War, there was an increase in national awareness. The country had to be rebuilt and there was a strong belief that science would play a major role in this recovery. Therefore a national organisation for pure scientific research, ZWO, was created. ZWO favoured projects that aimed at a national organisation of science. The Dutch astronomers embodied this goal by creating the SRZM (National Foundation for radio astronomy), in which Dutch scientists, Dutch industry and Dutch public utilities were represented.

The plan of the radio astronomers to build a radio telescope with a dish of 25 metres, which would be the largest one in the world, aroused feelings of national pride, both displayed in and stimulated by the extensive coverage in the media.

The Dutch government and the country were really looking forward to huge discoveries that would be made in a field in which the Netherlands would be internationally at the forefront.

NUCLEAR PHYSICS AFTER WW II

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THE 'ATOMS FOR PEACE' PROGRAM PAVES THE WAY: TURKEY'S FIRST EXPERIENCE WITH A NUCLEAR RESEARCH REACTOR

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Abstract

Turkey joined the Atoms for Peace program which aimed to use nuclear energy for peaceful purposes in 1955. It was the first country signing an agreement with the USA within the frame of the program. As part of the program, Turkey would establish a nuclear research reactor and the USA would provide the required knowledge, training and fission products to build and activate the reactor. The USA would also provide research facilities in American training centres to train Turkish scientists who will study nuclear energy.

In this context, an Atomic Energy Commission (AEC) was established affiliated to the Prime Ministry of the Turkish Republic in 1956. The Commission was assigned to carry on all nuclear activities at national level and to provide coordination at international level. Within the framework of the AEC's 'atomic reactor' project, a reactor of 1 MW named TR-1 was installed in agreement with the American Machine Foundry between the years of 1959-1962. Çekmece Nuclear Research and Training Center (ÇNRTC), which embodied this reactor, started the production of radioisotopes to be used in industry, agriculture, medicine and scientific researches in 1962. To meet the need for radioisotopes, a second reactor of 5 MW named TR-2 was installed in the same building and pool in 1984.

This paper will deal with the following questions by exploring the role and the effect of ÇNRTC in the field of institutionalization of nuclear studies in Turkey: ÇNRTC has worked for meeting Turkey's need for radioisotopes. Besides fulfilling this task, to what degree was ÇNRTC effective in researches and training activities in the nuclear field? Did ÇNRTC contribute to production of nuclear technology? Did it realize effective collaborations with national and international institutions? To what extent did ÇNRTC provide the infrastructure facilities for researches that have been done in the universities? What was the role of ÇNRTC in training researchers and experts in the field of nuclear physics?

As it is known, in 1953, Eisenhower, president of the USA, announced to all the countries of the world, a program that would promote nuclear power for peaceful purposes rather than armament. This is the 'Atoms for Peace' program. Within the scope of the program, the USA would share its knowledge on nuclear power with the allied states under mutual agreements. The Turkish government approached the call and the two allied states entered in a mutual collaboration agreement within the scope of the program on 10th June 1955. The text of the agreement and its appendices were accepted by the Turkish Grand National Assembly on 14th December 1956. According to this agreement composed of ten articles, nuclear power would only be used in the civil area.

The first article of the agreement stipulates information exchange between the two states in the following issues:

- design of research reactors, construction and operation of the same; using those reactors only in research, education and medical treatment,
- health and security issues with regard to operating and using research reactors,
- the use of radioactive isotopes in physical and biological researches, medical treatment, agriculture and industry.

In the present study, the establishment and institutionalization process of the 'Çekmece Nuclear Research and Training Center' (ÇNAEM) will be under focus. This is the first nuclear centre established in Turkey within the scope of this agreement. Our examination will cover its activities in the fields of research and education during its first 20 years. Considering the expectations in its establishment and its present-day situation, the impact of ÇNAEM in the field of nuclear research in Turkey shall be discussed. The organization of ÇNAEM, its activities, its nuclear research reactors and the radioisotope produced in ÇNAEM shall be dealt respectively.

The first nuclear research reactor in Istanbul

Fig. 1: ÇNAEM-Istanbul Küçük Çekmece.

The Turkish government started preparations immediately after the agreement was signed with the USA for establishing a nuclear research centre and providing its scientific and technical infrastructure, and to train the required human power. In 1956, a joint committee of eight members including academicians from Istanbul University and Istanbul Technical University was set upon the request of the government. This committee started working for the establishment of a nuclear reactor on the bank of the Lake Küçük Çekmece, located west of Istanbul. The "Law for the Establishment of Nuclear Power" (No. 6821) enacted in the same year stipulated the establishment of a 'Nuclear Power Commission' associated to the Prime Ministry in Ankara. In 1958, the committee in Istanbul was abolished and all the activities on nuclear power were entrusted to the commission established in Ankara. This Commission consisted of specialists from the public institutions related to mining, bureaucrats of the Ministry of Foreign Affairs and the Prime Ministry, as well as physicists, geologists, chemists, doctors and mining engineers working at the universities in Istanbul and Ankara. This commission took over the nuclear reactor project prepared by the Istanbul committee and gave priority to its realization.

The Nuclear Power Commission decided in 1958 that an atom reactor of pool type with 1 MW power would be set up in Istanbul, in the place previously determined. The building to house the reactor and the other constructions were started in 1959 and completed in 1962. The design and setting up of the reactor was realized by the 'American Machine and Foundry' company. The radioisotopes to be used in the researches were supplied by the USA. The reactor reached its first criticality on 6th January 1962, and started operation on 27th May, 1962.

The organization and the activities of ÇNAEM

ÇNAEM was directed by a manager assigned by the Nuclear Power Commission. The first organization in its early years became restructured and the centre was later composed of the following sections:

1. Research and education

2. Technical services
3. Administrative services

The research and education section was composed of the departments of physics, chemistry, radiobiology and nuclear engineering. The technical services section was composed of the departments dealing with the running and the control of the reactor, the production of the radioisotopes, and departments of electronics, health physics and technical service.

The physics department was mainly composed of research groups in the fields of nucleus physics, plasma physics and solid state physics. The chemistry department had groups in the fields of radiation chemistry, radio activation analysis and analytical chemistry. In the nuclear engineering department researches were conducted on reactor physics, reactor technology, and reactor security. And in the radiobiology department, studies in the fields of cell biology, radioecology and plant physiology were performed.

The technical services section was responsible of the reactor's running and maintenance. Furthermore, it was in charge of assisting research and educational activities. The electronics department was entrusted with the production of measuring devices, and control of measuring operations. The radioisotope department was responsible for the production of qualified radioisotopes to be used in the fields of nuclear medicine, industry and research.

The first nuclear reactor of ÇNAEM: The TR-1

The first reactor of ÇNAEM was named TR-1. It was a pool type research reactor with a power of 1 MW. It was activated in 1962 as stated above. The core (heart) of the reactor was located in the smaller section of the pool of 8.70 meters deep and having 450 cubic meters of water. The wall made of barite concrete was 1.75 m thick. The fuel for the reactor was U-235 enriched. Water was used as inhibitor and cooler and graphite was used as reflector.

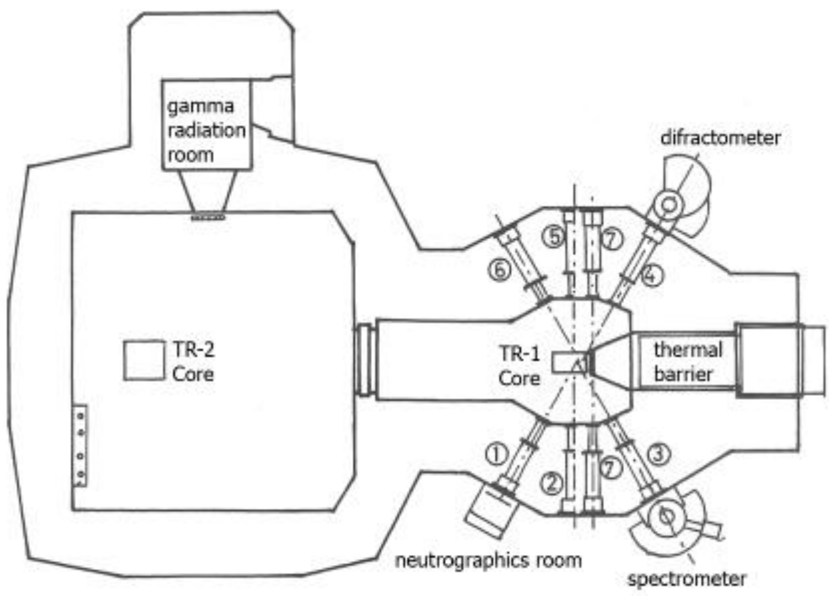

Fig. 2: Plan view of the reactor.

Components of the reactor such as neutrographics room, spectrometer, germanium detector, difractometer and x-ray cameras were used for research and radiation purposes.

The TR-1 reactor, besides producing radioisotopes, was used for research and education in the fields of physics, chemistry and nuclear engineering. The nucleus physics research group studied the structure of nuclei and tested various atomic models through experiments. The plasma group developed new experimental methods in order to measure plasma sizes. For this purpose, a laser laboratory was set up. The reason for focusing on plasma physics was due to the insufficient budget for conducting large size thermonuclear fusion experiments and the lack of trained personnel. Researchers in solid state physics analyzed crystal structures and the interaction of the crystal neutrons. The reactor also was used for researches on radiation chemistry and radiobiology.

Projecting and establishing the second reactor: The TR-2

The setting of a new reactor of 5 MW was envisaged in order to enlarge the scope of applications and increase radioisotope production. A team of four members affiliated to the ÇNAEM were sent to Grenoble Nuclear Research Centre in 1974 and started joint work with French experts. The basic reason for preferring collaboration with a foreign centre in the new reactor project was the need to obtain data to perform neutron and thermo-hydraulic calculations as well as sophisticated computer programs to process these data. The nuclear centre at Grenoble was chosen as the foreign partner because it had a reactor (MELUSINE) similar to the one to be set in Istanbul. The facility provided by the mentioned centre in terms of data transfer to Turkey was another factor for opting to work with Grenoble.

Work in Grenoble lasted for 8 months during which reports, technical specifications and technical drawings were prepared. Besides, the technical details of the new reactor were given in the preliminary project; conditions for repair and fuel renewal of the old reactor (TR-1) were also included. The first reactor was planned to be used only for research after the new reactor became available. Since the new reactor would be set in the building of the old one, the cost of the project would be 50% less. The motto of the Turkish team which prepared the project in cooperation with the French was "do-it-yourself reactor". This project was deemed to be a significant step for the progress of reactor and nuclear technology in Turkey.

TR-1 was stopped on 19th September 1977. In the same year the construction of the new reactor named TR-2 started in the larger section of the pool. 'Belgonuclear', the Belgian company who had secured the tender, completed the construction of the reactor on 10th December 1981. The TR-2 reactor, which was mainly set up to increase and diversify the radioisotope production, was used for the training of the operators. These would be employed in the nuclear power plants to be established in the future for electric power production.

The fuel of the new reactor was enriched U-235. The TR-2, which started to run with full capacity in 1984, was stopped in 1995 to control the earthquake resistance of the reactor building. It has been operated rarely since 2002, and with a power of less than 300 KW.

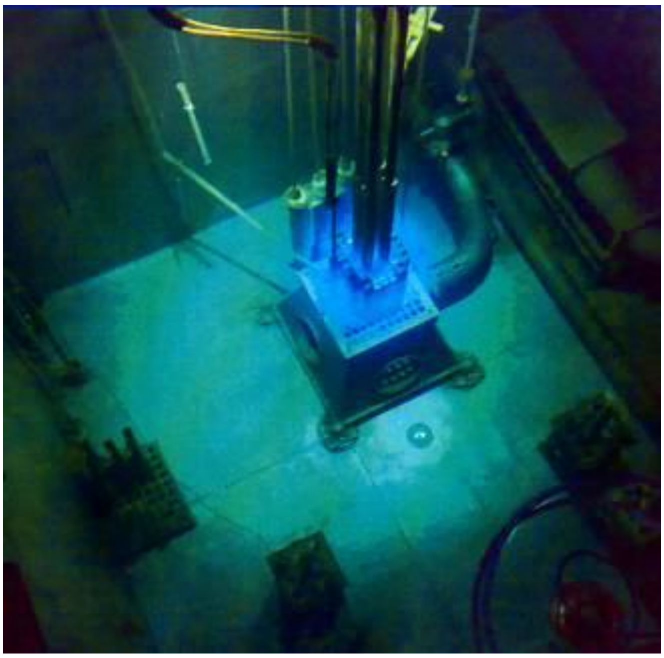

Fig. 3: TR-2 research reactor.

Radioisotope production in ÇNAEM

ÇNAEM, which started producing radioisotope in 1963, was producing 7 different radioisotopes in 1966 and 15 different radioisotopes in 1975:

Table 1: Radioisotopes produced in 1966 (left) and in 1975 (right) using TR-1.

Au-198
Ca-45
Cr-51
Ir-192
Na-24
P-32
Zn-65

Ag-111	Mo-99
Au-198	Na-24
Br-82	P-32
Cr-51	S-35
Cu-64	Sb-124
Ir-192	Tc-99
La-140	Y-90
	Zn-65

Radioisotopes produced were sent to the research centres at universities, hospitals and agricultural centres in Turkey.

Table 2: ÇNAEM radioisotope production from 1971 to 1975.

	1971	**1972**	**1973**	**1974**	**1975**
Nuclear Medicine	89,896	147,335	171,402	178,481	230,154
Industry	22,964	17,400	25,000	30,800	13,600
Research and Training	4,716	0,394	0,305	0,180	0,094
Total	117,576	165,129	196,707	209,461	243,848

As it may be seen in the table, despite the decrease in the field of research, the requirement of radioisotopes in the fields of medicine, agriculture and industry have gradually increased: while the total production of radioisotopes in 1971 was approximately 117 Ci, it became 243 Ci in 1975. The decrease of the demand in the field of research and education, which had already been low, may be caused by the deductions made in the budgets of universities as a result of economic problems.

TR-2 reactor, which began to run in 1981, started its production in 1984 following a control period of 3 years, contributing to meet the deficit of radioisotopes of Turkey. The radioisotopes produced were those having a half-life of a few hours, i.e. with short half-lives.

Following the establishment of ÇNAEM in Istanbul in 1962, attempts of establishing nuclear research centres in Ankara started as well. The "Ankara Nuclear Research and Education Centre" opened in 1967. This was followed by the opening of some nuclear units in the fields of medicine, agriculture and stockbreeding in various state institutions. However, there were no nuclear reactors in these units. The initiatives to set a second reactor in ÇNAEM were concurrent with the construction of a reactor in Istanbul Technical University in 1975. The reactor of the university was named TRIGA Mark-II, and activated on 11th March 1979. This reactor with a power of 250 KW was used for research.

* * *

ÇNAEM was the first nuclear centre in Turkey aiming both research and training manpower. The centre, as well as providing training on the running, control and security of the reactors, offered courses to university students in summer terms.

In addition to the technical work including the production of radioisotopes; manufacturing, maintenance and calibration of the radiation measuring tools, it organized activities to inform the Turkish public about the benefits and damages of nuclear power and radiation. Through the public courses, conferences and publications, ÇNAEM contributed to create awareness in society regarding nuclear power and radiation.

ÇNAEM was in collaboration with national and international organizations from its early foundation: the Nuclear Power Institute (Istanbul Technical University), the Institute of Nuclear Physics Institute (Istanbul University, Faculty of Science), the Faculty of Medicine (Istanbul University), The faculties of Science and Medicine (Ankara University), the

Faculty of Science (Middle East Technical University), the Faculty of Science (Ege University), and the USA Brookhaven National Laboratory.

The studies and researches conducted in ÇNAEM were published annually. Beside these 'activity and progress reports,' researches were published as monographs or in various scientific journals in Turkey or abroad.

The examination of its publications from 1962 up to the present date shows that:

- The top level was achieved in 1972-1973 in terms of the number of publications.
- After the cease of TR-1 reactor in 1977, there is a striking decrease in the publications and reports in foreign languages: From 1977 to 1981, a decrease is observed in Turkish publications as well. There was no publication in 1980 in foreign language and only one report in Turkish.
- As a result of activation of TR-2, an increase was seen after 1981 in the number of publications. Although a significant decrease was seen in the period when TR-2 was in operation (1981-95) Turkish publications continued when compared to the past.

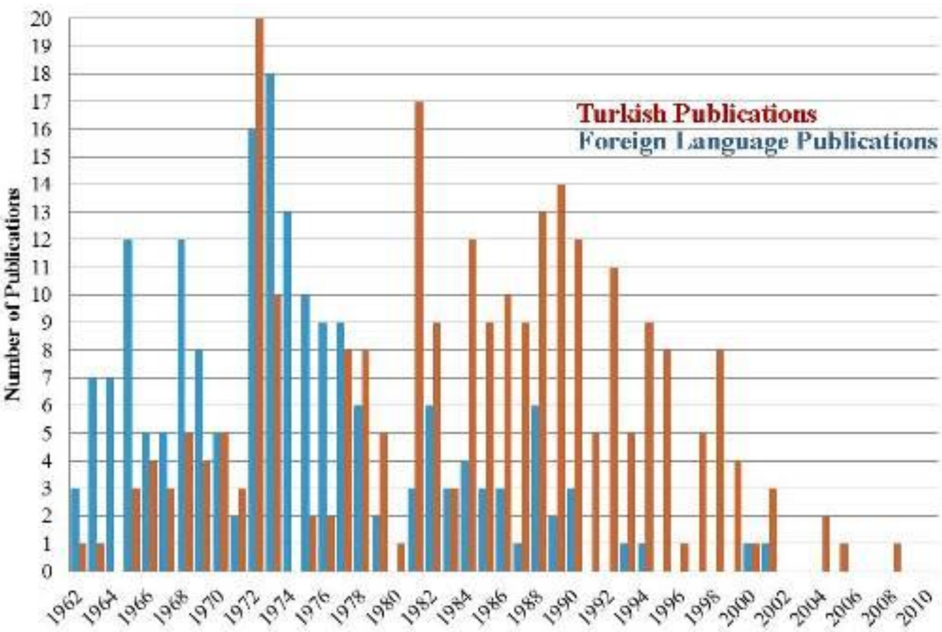

Fig. 4: ÇNAEM publications (1962-2010).

In 1982, together with the issuing of the 'Nuclear Power Law', the Nuclear Power Commission was transformed into the "Nuclear Power Institution of Turkey". This meant a new model in administration, organization and restructuring for nuclear power in Turkey. ÇNAEM was influenced by this reform and underwent new restructuring as well.

The cease of TR-1 after 15 years of functioning led to a decrease in scientific publications and research activities. In addition to this decrease, the setting up of the second reactor TR-2 to produce radioisotopes made impossible the conduct of many experiments that were performed previously in TR-1. TRIGA Mark-II, set up in 1979 in Istanbul Technical University for experimental purposes, seems to compensate experimental deficiency in ÇNAEM.

After joining the 'Atoms for Peace' program, Turkey planned to use nuclear energy in electric power production. The establishment of research and education centres regarding nuclear power was included in the 'First Five Year Progress Plan' (1963-1967). During the preparations of the 'Second Five Year Progress Plan' (1967-1972), the issue was also brought into the agenda but it was postponed until the next development plan period.

Although there have been several attempts to establish a nuclear power plant in Turkey up to date, no energy reactor could be established. ÇNAEM conducted studies and prepared reports on this issue. It provided courses to train experts, and expanded the means of infrastructure. However, there was no opportunity to bring those efforts into action. ÇNAEM, which failed to offer the universities the infrastructure they needed for experimental research, has currently become an institution serving in technical fields such as the production of radioisotopes, the measuring of radioactivity in various fields and the manufacture of radioactivity measuring instruments.

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THE ENTRY OF SPAIN INTO CERN DURING FRANCOISM IN 1961: VALENCIA'S ROLE[1]

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Abstract

The first entry of Spain in CERN was in 1961. At that time, Spain was a dictatorship that started recovering economically from Spanish Civil War (1936-1939) consequences, which devastated the country.
There was only an experimental group in Nuclear and Elementary Particle Physics. It was directed by Professor Joaquín Catalá de Alemany in Valencia. It started their activities in 1950 using the photographic emulsion technique. They established in few time a solid network with the main laboratories and Universities of Europe. The group institutionalized partially in 1958 with the creation of the Centro de Física Fotocorpuscular *in Valencia, funded mainly by the* Junta de Energía Nuclear, *whereas there wasn't a group in this discipline at the rest of Spain. First it was created in Madrid in* Junta de Energía Nuclear *in 1964.*
The entry of Spain in CERN during francoism has been considered as a political issue more than a scientific one. Spain regime suffered from international political isolation until the half fifties. It was easier to participate in that kind of organizations than in other purely political ones. However, we can't forget the existence of this centre in Valencia, though its role was peripheral in the Spanish scientific agenda.
This paper tries to answer four questions. Which role did Valencia's centre play in Spanish CERN entry? What consequences did such entry have in research: staff growth, research lines, travels and stays abroad, collaborations, publications, funding? What relation did the centre have with Spanish Administration? And with Junta de Energía Nuclear*'s group?*
For answering these questions we have looked into CERN Archives among other sources. We must remember

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This work in progress is part of the Ph.D. work by one of us (A. Ceba) on the origins and constitution of Experimental Nuclear and Particle Physics in Spain, directed by V. Navarro and J. Velasco. More details on the origins of Catalá's group in: V. Navarro, J. Velasco, J. Domenech, "La creación de una nueva disciplina científica en España: la física nuclear y de partículas" *Cronos*, 7 (1), 61-86 (2004); "The birth of particle physics in Spain" *Minerva*, 43 (2), 183-196 (2005).

that Spain abandoned CERN in 1969, and it didn't return until 1983. Nevertheless, this abandonment would be an issue in another paper.

Introduction

Spain entered CERN in 1961 under General Franco's dictatorship. It had started recovering from the Civil War (1936-1939) that devastated the country.

The main objective of this paper is to study the background of Spain's entry and its consequences from the perspective of the centre created by Professor Joaquín Catalá in Valencia in 1950, the *Institute of Corpuscular Physics* (IFIC).

The development of Spanish Physics after the Civil War is not yet well known. In a broad sense this is a case study about scientific development in non democratic regimes and the integration of Spanish physics in European projects. The references to Spain in the official history of CERN are minimal[2], and the scarce references in the Spanish bibliography are based in personal reminiscences and JEN documentation[3]. The difficulties of the study of Spanish contemporary science are the bad conservation of sources and/or its wide dispersion. For example the correspondence of professor Catalá is lost. This paper attempts to fill this historiographical vacuum.

Some of the questions which arise in this paper are the influence of the political, economical and scientific situation of Spain in the development of Catalá's group; the interest by the members of IFIC in CERN; the consequences of the entry for their research; and the relations with JEN.

IFIC until 1961

Joaquín Catalá, born in 1911 in Manresa (Barcelona), was professor of Theoretical and Experimental Physics in Valencia from 1945.

In 1949 he obtained a one-year-grant from the Spanish National Research Council (CSIC) to study at the University of Bristol within Sir Neville Mott's group of Solid State Physics. Finding it too theoretically inclined, he moved to Cecil Powell's group, which studied nuclei and particles with the photographic emulsion technique, thanks to his friendship with W.Gibson[4]. Next year Powell won the Nobel Prize in Physics for applying the photographic method and the discovery of mesons in Cosmic Radiation.

Back at the University of Valencia in 1950, he gave the Academic Opening Lecture entitled "The Photographic Technique in Nuclear Physics and Cosmic Radiation", where he complained about the small role given to physics in Spain and justified the creation of his group with scientific, economical and social arguments (for instance, the pacific applications of Nuclear Physics).

The technique allowed laboratories with scarce resources to be competitive with the big American centres which employed huge machines and sophisticated electronic counters, underlining that successes did not always have to be proportional to material resources[5], emphasizing the international character of research in Nuclear Physics[6].

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[2] No references in Volumes 1 and 2. Only one reference in Volume 3 on the vote of the new convention in December 1967. See p. 70.

[3] F. J. Ynduráin, "La Física de Altas Energías en España, in Sánchez Ron, ed., *Un Siglo de Ciencia en España*, (Madrid, Residencia de Estudiantes, 1998) 197-207. M. Aguilar Benítez, "In memoriam, Bruno Escoubés y el inicio de la Física Experimental de Altas Energías en España", *Revista Española de Física*, 13, nº4 (1999) 69-70; P. Pascual Martínez, "Los inicios de la física de altas energías en España," *Arbor: Ciencia, pensamiento y cultura*, Nº 626, (1998), 231-244.

[4] Personal communication to V. Navarro and J. Velasco, 15 May, 2001.

[5] Thanks to Powell and collaborators, emulsions became the dominant technique in European high-energy physics at the beginning of the 1950s. See: "Nuclear Emulsions: The Anxiety of the Experimenter", in P. Galison, *Image and Logic. A material culture of physics*, (Chicago: The University of Chicago Press, 1997) pp. 143-239. On diffusion of the technique to Europe: L. Gariboldi (2006), "Developing a technique for researches in cosmic-ray physics: Nuclear emulsions from Bristol to Europe", in M. Kokowski (ed.) *The Global and the Local: The History of Science and the Cultural Integration of Europe*. Proceedings of the 2nd ICESHS (Cracow, Poland, September 6–9, 2006), pp. 480-486.

[6] "Hemos iniciado en nuestra Facultad modestas investigaciones nucleares, aportando de este modo nuestro pequeño esfuerzo a una obra de mayor envergadura". Catalá (1951), *Op. Cit*, p. XVI.

The use of the photographic technique was an ideal tool for a poor and isolated country like Spain, where a basic food ration book existed until 1952. The Civil War caused a rupture in Spanish science. Some teachers went into exile and others were purged[7]. The institutional structure was modified, focusing in applied research in coherence with the autarchic economical policy. In 1939 the Spanish National Research Council (CSIC) was created as a multidisciplinary research public body, and in 1951 the Nuclear Energy Board (JEN) for developing nuclear energy activities[8].

In September 1950 Catalá set up his group with four Ph.D. students (three chemists, one physicist). Infrastructure was scarce: one microscope, a few emulsion plates, and some photographic materials, in a small room at the Faculty of Sciences. In 1958 they were 19 people, with 5 microscopes available, one of them a high precision Koritska[9].

The main research lines were related to emulsion techniques: nuclear reactions and spectroscopy, pion interactions with emulsion nuclei or techniques related to emulsion chemistry.

Soon it was the most productive group in the Spanish journal *Anales de Física*, quickly obtaining recognition from his colleagues. Catalá was awarded the first Medal of the Royal Society of Physics and Chemistry (RSEFQ) in 1958 for his labour of research, organization and direction[10].

However the research programme presented in the Opening Lecture of 1951 could not be fully carried out. In particular research into Cosmic Radiation, a field where strange particles were a hot topic of the day.

Catalá tried unsuccessfully to join European collaborations inspired by Powell and Occhialini, which made three balloon launches from 1952 to 1954 in Italy to study Cosmic Radiation[11]. It has been said that Cosmic Radiation was the Nuclear Physics of poor countries, but according to Catalá Spain could not even contribute to the launching of balloons[12]. Emulsions analyzed in Valencia were obtained at accelerators and facilitated by foreign laboratories[13].

Cosmic Radiation was unable to compete with the Synchrotrons at Berkeley (Bevatron) and Brookhaven (Cosmotron)[14]. The Italian, Belgian and English groups sent their plates to Berkeley. Catalá tried in vain to collaborate through those groups with the Californian synchrotron[15]. He complained about the great delay of Spain with respect to other countries in a moment where there were a lot of discoveries to be made[16].

Spain and CERN[17]

Around 1950 Nuclear and Particle physics started to be dominated by accelerators. In June 1950 Isidor Rabi presented a resolution in the General Assembly of UNESCO to built European laboratories, one of them for basic nuclear research promoted mainly by physicists Pierre Auger and Edoardo Amaldi.

After difficult negotiations the *Conseil Européen pour la Recherche Nucléaire* was created 15th February 1952. In April it was agreed to design a protosynchrotron of strong focusing of about 30 GeV energy. In 1954 the provisional council was dissolved and the organization renamed European Organization for Nuclear Research (CERN).

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7 On CSIC see: M.A, Puig-Samper (coord.) *JAE-CSIC, cien años de ciencia en España* (Madrid: CSIC, 2007). About repression and depuration see: L. E. Otero Carvajal (dir.) *La destrucción de la ciencia en España. Depuración Universitaria en el franquismo* (Madrid: Universidad Complutense, 2006) and J. Claret, *El atroz desmoche: la destrucción de la universidad española por el franquismo* (Barcelona: Crítica, 2006).

8 See: A. Romero de Pablos, J. M. Sánchez Ron (2001), *Energía nuclear española. De la JEN al CIEMAT* (Madrid: Ciemat, MCYT, 2001).

9 IFIC Archives, Currículum Vitae del Centro de Física Fotocorpuscular, September 1958.

10 Proceedings of RSEFQ, April 13th 1959. Until 1960 they published 63 articles, 6 of them in International journals; 11 theses and they had presented 56 papers in conferences, 8 in international conferences.

11 M. Baldo Ceolin "The Discreet Charm of the Nuclear Emulsion Era", *Annual Review of Nuclear and Particle Science*, 52, 1-21 (2002); C. Olivotto (2006) "The Mediterranean flights and the G-Stack collaboration (1952–1955): A first example of European collaboration in particle physics", in M.Kokowski (ed.) *Op. Cit.* pp. 490-496.

12 These researches were the forunner for *Big Science* in Particle Physics in Europe. See Olivotto (2006), *Op. Cit.* Results were presented in the famous Conference of Bagnères de Bigorre (1953) which Catalá attended.

13 IFIC Archives, Curriculum 1958, *Op. Cit.*

14 From 1946 in Berkeley there was a synchrocyclotron of 0.35 GeV. In 1952 started operation the Cosmotrón of 3 GeV in Brookhaven and in 1953 the Bevatron of 6.2 GeV in Berkeley.

15 Catalá (1957), *Op. Cit.*, p.8.

16 "(…) en Física la mies es mucha, pocos los trabajadores y enorme el retraso que llevamos respecto a los demás" Catalá (1957), *Op. Cit.*, p. 9.

17 See: Armin Hermann; John Krige; Ulrike Mersits; Dominique Pestre (ed.) *History of CERN*, vol. I, (Amsterdam: North-Holland, 1987).

CERN's foundation has been considered both a scientific and a political project, a model for European cooperation. It was a complex process because the actors involved (scientists, science administrators or governments) had different interests[18].

Spain did not participate in the negotiations for the creation of CERN[19]. The initiative was from UNESCO, and Spain did not enter into this organisation until 19 November 1952. The first known reference of an interest is a report by José Maria Otero Navascués (JEN director general) after the First Conference of Pacific Uses of Atomic Energy (Geneva, August 1955), where he considered it necessary to enter into CERN. Catalá, member of the Spanish delegation[20], was coordinator of translation into Spanish for the Conference's Proceedings[21].

In the fifties Spain had progressively improved its international relations. In 1953 it signed the Agreement of Defence and Cooperation with the United States that would facilitate Spain's entry into the United Nations in December 1955. President Eisenhower visited Spain on December 23th 1959.

When the CERN council was consulted in 1956 about Spain and Austria membership, no objection was made[22]. The main difficulty was the financial contribution (letter by CERN director general C.J. Bakker to L. Garcia Llera, Spanish Delegate at International Organisations in Geneva), considered too high by Spain. Bakker answered that the method used for calculating contributions involved net income and not taxable income[23]. The negotiations were slowed down probably due to the Spanish economic crisis. In July 1959 a Stabilization Plan to modernize and liberalize the economy was approved. Meanwhile, Spain became a member of international economic organizations, like the IMF (1958), the World Bank (1958) and the OEEC (1959).

In 1959 Spain joined the International Atomic Energy Agency (IAEA)[24], where from 1959 to 1960 Catalá held the post of Chief of the Scientific Exchange Division. Two members of Catalá's group received grants from the IAEA: José Aguilar for 4 months in 1959 at the University of Birmingham and Eugenio Villar for 6 months in 1960 at the University of Milan.

José María Otero Navascués, now president of JEN, visited CERN in early 1960, a year after the inauguration of the PS accelerator. On his return he submitted a report to the government explaining the advantages of Spain entering CERN. On February 16th the government endorsed his report[25]. The following months saw the negotiation of the special and annual contribution from Spain.

Finally, on December 8th 1960, at the 18th meeting of the Council, Spain joined CERN. The decision was adopted unanimously by the 13 member countries. The special entrance fee was fixed at 2,700,000 Swiss francs to be paid in three years. The annual contribution for 1961 was 2,816,300 Swiss francs for a total budget of 67.7 million Swiss francs[26]. Spain became the fifth largest contributor to CERN at 4.16%, ahead of Sweden (4.10%) and Belgium (4.02%)[27].

IFIC and CERN

Prior to the entry of Spain into CERN, Catalá's group had already established many scientific contacts and collaborations. His old friend and collaborator from Bristol, W.Gibson, was in 1959 the head of the CERN emulsion group.

[18] See John Krige and Dominique Pestre, "The How and Why of the Birth of CERN" in *History of CERN*, vol. I, *Op. Cit.*

[19] Albert Presas has presented at this conference a document from JEN dated 1953 where Otero considered of national interest the entry into CERN. See his paper "*José Otero Navascués (1907-1983), Science for a Regime"*.

[20] See: Romero de Pablos, Sánchez Ron (2001), *Op. Cit,* p. 59. Most of the members were from JEN, National Industry Institute (INI), private companies and the Ministry of Foreign Affairs. Catalá was one of the 12 experts, as "Chief of the Physics Division".

[21] *La utilización de la Energía Atómica con fines pacíficos*. Actas de la Conferencia Internacional de Ginebra; Agosto 1955. XVI Vol. (Ginebra: ONU, 1956).

[22] DIR-ADM-FIN-06 (3), file 1**,** CERN Archives In a confidential letter 969/4/2R from H.L. Verry, Department of Scientific and Industrial Research, UK to Mr. S.A. ff Dakin , 12th April 1960, there are attached some letters dated 1956 from CERN delegates to Sir Ben Lockpeiser, president of the CERN Council. There were not answers from Italy and Belgium, but it was indicated that Sir Ben had oral communications of support form these countries.

[23] *Ibid*, CERN Archives CERN/CC/3621, On February 21th 1957.

[24] An international organisation created on July 29th 1957 in Vienna.

[25] *Ibid* , CERN Archives, Otero to Bakker, February, 22th 1960.

[26] *Ibid*, CERN Archives, CERN/386 "Recommendation to Council on the Accession of Spain", 28th November 1960.

[27] A. Hermann; J. Krige; U. Mersits, Pestre, D., L.Weiss, *Building and running the Laboratory (1954-1965), History of CERN,* vol II, (Amsterdam: North-Holland, 1990), Appendix 3.

In 1960 Eugenio Villar stayed for 6 months at the Institute of Physics Aldo Pontremoli, in the University of Milan, analyzing an experiment done at the CERN PS accelerator[28].

Catalá was one of the five scientists of the Spanish committee that visited CERN in March 1961. All members were from JEN except Catalá[29].

However, Otero wanted Catalá to play a greater role, strongly supporting him for a position as permanent staff member[30]. Otero argued that Catalá was a good candidate because he was the best known Spanish Nuclear physicist abroad, with a position in the International Atomic Energy and large experience in international work. He added that his candidacy would be supported by W. Heisenberg and E. Amaldi, CERN Council members and C.P. Powell, at the time chairman of CERN Scientific Policy Committee.

He also requested that two disciples of Catalá, F. Senent, then at University of Valladolid, and M. Aguilar, from University of Murcia, could come as visiting scientists.

Finally Otero emphasized that Spain's contribution to CERN represented 10% of the budget of JEN, and that Spain had made a great economic sacrifice at a very difficult time when the country was stabilizing its currency. Spanish membership of CERN implied staff training and opportunity to work within a common European enterprise. He would not have sufficient moral force for defending the continuity of Spain in CERN if positions to Spaniards were few or of little importance.

The case was carefully examined. G. Bernardini reviewed Catalá's scientific output and discussed the matter with Dr. Lock, who had worked with Catalá. He considered the most adequate position to be Research Associate rather than Visiting Scientist[31]. Based on that the Director General answer to Otero was that Catalá could not be admitted as a staff member of CERN, but he could come as Visiting Scientist for one year[32].

In 1963 Spain requested a temporary reduction of the quota to 50% for the next three years. There were four reasons: the income "per capita" was below the European average, there were necessary investments in education and research within the country, there were not physicists with experience in high energy and Spanish industry was not competitive in providing machinery or laboratory equipment. These savings would be allocated to train high-energy physicists in Spain[33].

To strengthen his point, Otero argued at a meeting of the Council that Spain only had the emulsion group led by Catalá, and the contribution to CERN represented 35% of the total budget allocated to physics in the country, much higher than the case of the UK, for instance.

The incorporation of Spain into CERN helped to the consolidation and increase of experimental research in high energy physics at the IFIC[34]. In 1965 there were two groups: photographic emulsions and bubble chamber. The first studied fragmentation processes of heavy nuclei by 13 GeV/c protons and negative pions, and the interaction of antiprotons with nuclei of the emulsion. The bubble chamber group had devoted itself to technical problems due to the lack of appropriate equipment such as the instrument for the evaluation of photographs and electronic calculators[35].

New lines of research were created at IFIC[36]: a group dedicated to environmental radioactivity in atmospheric dust and water, publishing a newsletter since 1958 that had been circulating in most foreign centers performing such

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[28] The collaboration involved 17 researchers from Milan, Bari, Bristol and CERN. See: Baldassarre *et alii*. "Observations on the Coherent Production of two Charged Pions by Pions at 14 GeV," *Il Nuovo Cimento*, Serie X, Vol. 21, (1961), 459-468.

[29] CERN PR/7/61, CERN Archives, "Spanish Scientists at CERN". They were Prof. C. S. del Río, Dr. M.A. Vigon, Mr. F. Verdaguer and Dr. A. Tanarro and Prof. J. Català (sic).

[30] DIR-ADM-FIN-06 (3), file 1, CERN Archives. Otero to Prof. Jean Willems, President du CERN. Meyrin-Genève., Madrid, le 15 juin 1961. A large letter of 4 pages, in French.

[31] CERN Archives, DIR-ADM-PERS-02, file one. Memorandum Confidential. Bernardini to Adams (29/06/1961). Bernardini was the scientific director of the Synchrociclotron Division. Lock from 1960 was Joint Group Leader of the Nuclear Emulsion Group. See: "Who's who at CERN", Appendixes, Volume 1 and 2, History of CERN, *Op. Cit.* We thank Xavier Roqué for making these sources known to us.

[32] DIR-ADM-FIN-06 (3), file 1, CERN Archives, CERN/7200 Adams to Otero, April 7h 1961.

[33] *Ibid*, CERN Archives, Letter from Aniel-Quiroga, permanent Delegate in Geneva to Victor F. Weisskopf, Director General CERN, June 6th 1963. CERN Archives.

[34] During the participation of Spain into CERN, 1961-1968, IFIC members published 23 articles, 4 in international journals, and 6 published in *Anales de Física* in English, 16 papers were presented, 1 at an international conference, and 6 theses were defended.

[35] IFIC Archives, "Memoria relativa a las actividades del IFIC desde su fundación", Valencia, May 1965.

[36] Memoria 1965, *Op. Cit.*; J. Catalá; F. Senent, "El Instituto de Física Fotocorpuscular de Valencia", *Arbor*, nº300 (Diciembre), (1970), 82-93.

measurements. They had measured the radioactivity produced by nuclear explosions like those from France in the southern hemisphere. Since 1964 there was a theoretical physics group directed by Professor P. Pascual whose purpose was to assist in theoretical aspects of the work of the other groups, doing at the same time original research into the theory of weak interactions.

IFIC and the Spanish Administration

The struggle for institutional recognition was a constant since the group's creation in 1950. Initially IFIC received most of its funding from JEN thanks to the personal relationship between José María Otero Navascués, President since 1958, and Catalá. JEN was a civilian organization created in 1951, first dependent on Government Presidency and later from 1957 onwards on the Ministry of Industry.

JEN signed an agreement in 1955 with the USA for cooperation on the civilian uses of atomic energy in order to launch experimental reactors within the "Atoms for Peace" programme. The early research focused on designs of nuclear research reactors, neutron activities, design and construction of instruments such as counters and management of radioactive fuel[37].

JEN promoted theoretical physics, high energy physics and even molecular biology. It funded the activity of various external scientific groups. Many professors in Theoretical Physics, Mathematical Physics, Quantum Mechanics and Nuclear Physics were first formed at JEN[38].

JEN was the official interlocutor for Spain in CERN, but it did not have any experimental high energy physics group until 1963 when Salomé de Unamuno, after enjoying a fellowship at CERN, along with her husband Bruno Escoubès, mounted a small laboratory to continue the analysis of bubble chamber photographs previously started at CERN[39].

In 1964 the Institute of Nuclear Studies was created for funding and coordinating research related to nuclear energy[40]. In 1966 the National Committee for High Energy Physics was created within the institute on recommendation of CERN, with the aim of coordinating the Spanish research and becoming CERN interlocutor. The honorary president was Otero Navascués, but the effective one was Catalá.

In 1967, IFIC received a special allowance of about 10 million pesetas of the Advisory Commission of Science and Technology for equipment necessary to analyze bubble chamber events. When Spain quit CERN the material was required by JEN to concentrate activities at Madrid. With the equipment two researchers move from IFIC to JEN, Rafael Llosá and Pedro Ladrón de Guevara[41].

IFIC was from its beginnings an associated group to Institute of Optics Daza de Valdés of CSIC. Funding from CSIC was low; in 1957 it came almost exclusively from JEN[42]. It helped to the precarious economic situation of the group that most of its members were Associated or Assistant professors to Catalá's Chair. It was not until 1961 that the Ministry of Education implemented the Physics degree, making Valencia the fourth Spanish university to have it. Through the Ministry IFIC received in 1963 a 5-year grant for research into low and high energy nuclear physics[43].

Concluding remarks

The Valencia group played an important role in the entry of Spain into CERN, thanks mainly to the very close connection of his leader, J. Catalá, with JEN President Otero Navascués. The entry significantly contributed to the consolidation and increase of the experimental research in high energy physics carried out at IFIC, as can be seen from the indicators of scientific output. Funding, which had came mainly from JEN during the 50's, started to increase as well from the Administration in the 60's. This was a straight consequence of the compromise to impulse high energy physics in Spain according to the negotiations from CERN membership.

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[37] They started to publish in *Anales de Física* en 1954; its production until 1965 was lower than the Valencia group. See: Carlos López, *La Producción Española en Física durante el Franquismo (1940- 1975), a través de los Anales de la Real Sociedad Espanola de Física y Química*, Ph.D. Thesis, Universidad de Murcia, (1986) pp. 792-799.

[38] See: Romero de Pablos, Sánchez Ron, (2001), *Op. Cit.* p. 199.

[39] *Ibid*, p. 205.

[40] *Ibid*, p. 194.

[41] See: Catalá; Senent (1970) *Op. Cit.* p. 82.

[42] Catalá complained in 1956 about centralism and the absence of meritocratic criteria in research funding. See: Catalá (1957), *Op. Cit.* p. 8.

[43] Given on December 9th 1963. University of Valencia Archives. Catalá's file.

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ISOTOPE LANDSCAPES AND LABSCAPES IN PORTUGAL (1952–1962)

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Abstract

Political events led to the appointment of Julio Palacios, professor at Madrid University and an experienced researcher in areas other than nuclear physics, as first director of the Centre for Nuclear Physics Studies, and to the choice of its location at a university research hospital, away from the Faculty of Sciences of Lisbon, in 1952. In this site a laboratory was set where isotope applied research was conducted mainly by physicists.
The same year isotope research was also launched at the National Laboratory of Civil Engineering, in Lisbon, by Armando Gibert, ex-teaching assistant at the Faculty of Sciences of Lisbon, and a researcher with experience in nuclear physics. Together with new laboratory premises, isotope work took place by the seaside at the estuaries of the rivers Tagus and Mondego, and at Póvoa de Varzim harbour.
Location in a hospital influenced the lines of research of the Centre for Nuclear Energy which included chemical manipulation inherent to isotope handling, cancer treatment, and physicians training. As for Gibert, he concentrated on a specific topic –tracing sand movements with radioisotopes. Both groups contributed with papers to the International Conference on the Peaceful Uses of Atomic Energy, Geneva 1955, and published in a small number of foreign journals. However, most of the work was addressed to a Portuguese audience through Revista da Faculdade de Ciências de Lisboa*, in the case of the university group, and* Laboratório Nacional de Engenharia Civil *publications, in the other case.*
This talk uses a comparative approach to contrast different backgrounds, profiles and research agendas of two Portuguese research groups both embracing radioisotopes applications in various labscapes and landscapes settings.

In 1952, seven years after two atomic bombs were dropped over Hiroshima and Nagasaki, Portugal was taking its first steps, at the institutional level, to adopt a post-war nuclear energy program. This nuclear turn was truly an audacious adventure, especially driven by the fact that Portugal possessed uranium ore deposits exported to the USA under a joint agreement with the USA and Great Britain, dating from 1949.

This talk addresses investigations concerning, mainly, radioisotopes manipulation by physicists from the Faculty of Sciences of the University of Lisbon. A group of graduate students from this Faculty selected and led by Julio Palacios (1891-1970), physics professor of the University of Madrid, accepted to exchange the academic space for that of a hospital, the Portuguese Cancer Institute (*Instituto Português de Oncologia*). Another group was headed by Armando Carlos Gibert (1914-1985) who had been awarded a nuclear physics PhD by ETH, in 1946. Until 1948, he was physics assistant at the Faculty, and in 1952 he welcomed a research assignment at National Laboratory of Civil Engineering (*Laboratório Nacional de Engenharia Civil*).

We have to go back to 1947 to understand, on the one hand, the peculiar situation of Palacios, a Spanish full professor holding a post at a Portuguese Faculty of Sciences, and conducting nuclear research at a hospital, and on the other hand, Gibert, a nuclear physicist, finding a research post in a civil engineering laboratory. In fact events of a political nature were the excuse for the dictator António de Oliveira Salazar (1889-1970), to dismiss twenty one university assistants and professors of several faculties, of which three were notorious physics assistants and researchers. Two of these physicists were Gibert and Manuel Valadares (1904-1982), the most prominent researcher in radioactivity and nuclear physics in Portugal, at the time. Furthermore Armando Cyrillo Soares (1883-1950), director of the Laboratory of Physics of the University of Lisbon, retired in response to the physicists' dismissals. Palacios was appointed to fill in the void these dismissals introduced into the physics group of the Faculty of Sciences of Lisbon.

On October 10, 1952 a Provisional Commission for Nuclear Energy Studies (*Comissão Provisória de Estudos de Energia Nuclear*) was launched to pave the way for the Nuclear Energy Board (*Junta de Energia Nuclear*) created in 1954. The aim of the Commission was to furnish it with freshly trained specialists, mainly abroad, in all nuclear areas. Back in 1929, although the country was poor and undeveloped both agriculturally and industrially, an institutional system for supervising university research had been installed surviving on meagre budgets ever since its creation. Under the aegis of this institutional system, at the time of this case named High Culture Institute (*Instituto de Alta Cultura*), the afore-mentioned Commission was charged to implement Nuclear Energy Centres of Studies at the Faculties of Sciences of the three universities of Lisbon, Coimbra and Oporto, as well as at the two Engineering Schools of Lisbon and Oporto. However, the Lisbon Nuclear Physics Centre of Studies was an exception to this location rule.

A physics university group working at a hospital space

The choice of a hospital in exchange for the university space has an easy explanation. In 1924 the Portuguese Cancer Institute was created by the government and after the installation of this hospital its director, Francisco Branco Gentil (1878-1964), sought physicists from the Faculty of Sciences of University of Lisbon to aid in cancer treatment with radon. In 1948 this assignment fell both on Gibert and on Palacios, as first assistant and director of the physics department, respectively. Gibert was a radioisotope enthusiast who had tried to rouse Gentil's interest to this new application in medicine as early as 1946. In 1950, Gibert's efforts bore fruit. In April, he and Palacios travelled to England to visit the British Atomic Energy Research Establishment at Harewell and the Royal Cancer Hospital in London. In September 1952, cancer treatment with radioisotopes started at the Cancer Institute in Lisbon, and in April 1953, a Radioisotopes Laboratory was inaugurated there, headed by Gentil with Palacios' collaboration.

However, these events do not explain completely the location of the Lisbon Nuclear Physics Centre of Studies, because there is evidence that the government would not allow nuclear research to be conducted by physicists at the Faculty of Sciences after 1947. In fact, after his dismissal Valadares was granted political exile in Paris, and the appointment as Maître de Recherches at the *Laboratoire de l'Iman Permanent* at Bellevue. Those were cold war times, when communist influence on French nuclear research scientists was intense. In 1950, Frédéric Joliot (1900-1958) had to resign his top post at the *Commissariat à l'Énergie Atomique*, on account of his political positions as president of the World Peace Council, an organization of Soviet tendency. On the other hand there was Salazar, a ferocious anti-communist ruler, and a British and USA ally in the maintenance of a firm anti-communist control in the Iberian Peninsula. He probably believed he would wipe out all sort of French influence on Portuguese nuclear physics research, politically a very sensitive area in those days, by locating the Centre of Studies outside the Faculty of Sciences, where Valadares still had physics disciples.

In November 1952, when the Provisional Commission for Nuclear Energy Studies appointed Palacios as director of the Lisbon Nuclear Physics Centre of Studies, the choices had been made: the space –Cancer Institute– and the topic – radioisotopes research. Palacios had great experience as an experimentalist, but he was not a nuclear physicist. Originally he was a specialist on cold temperatures and lately his interests had moved on to electrochemistry, while collaboration in cancer treatment opened up for him another line of research in radioisotopes. In 1947, when Palacios accepted the post at the Faculty of Sciences he was facing the great challenge of replacing two important men who had succeeded in launching a research school at the Laboratory of Physics of the University of Lisbon: Valadares, the researcher, and Cyrillo Soares, the director of the Laboratory. Palacios soon realized how hard it would be to accomplish his goal of assembling a research group in a Faculty whose vocation was still to train secondary school teachers. Another problem was the backward university system. The Faculties of Sciences did not yet grant a physics degree, but a joint degree in physics and chemistry which meant that specialisation in physics was nonexistent. As an example, let me mention that modern physics –quantum mechanics and nuclear physics– had to wait for the 1964 reform to be included in the physics syllabus. Despite these drawbacks, Palacios succeeded in recruiting collaborators to start research in electrochemistry: in January 1949, António Manuel Baptista (b.1924), a physics assistant, and a year later Fernando Carvalho Barreira (1928-1976) a chemistry assistant. Soon Baptista would join Palacios at the Cancer Hospital to open another line of research linked to radioisotopes: radiation detectors. In 1952 they published their first article on this topic, in *Revista da Faculdade de Ciências de Lisboa*

(*Journal of Faculty of Sciences of Lisbon*).[1] In 1953 Baptista and Palacios were two of the physicists invited by Gentil to collaborate with him at the Radioisotopes Laboratory.

Activities of the Lisbon Nuclear Physics Centre of Studies

In accordance with the Provisional Commission for Nuclear Energy Studies, the purpose of the Lisbon Nuclear Physics Centre of Studies, in line with all other Nuclear Energy Centres, was "to offer young Portuguese graduates theoretical and practical training enabling them to utilize the many possibilities of phenomena relating to nuclear reactions."[2] In 1953 Palacios and his collaborators Baptista and Barreira prepared the installation of the nuclear physics laboratory at the Cancer hospital with modest equipment; an accelerator was something they could only dream of. The same year Baptista and Barreira were awarded scholarships by the High Culture Institute to train in England. From March to July, Baptista worked with Norman Veall in radioactive measures at the Radiotherapeutic Research Unit of the Medical Research Council in Hammersmith. This training ended with a joint article on "multi-tube gamma counting apparatus for small samples."[3] From July to September, Baptista worked with W. V. Mayneord of Royal Cancer Hospital, University of London.[4] In what concerns Barreira, he spent his 1953 summer participating in a theoretical and practical lessons course on radioisotopes at the Isotope School of the Atomic Energy Research Establishment at Harwell.[5]

In 1954 and 1955 the research group was enlarged with more graduates, originating mainly from the physical-chemical course of the Faculty of Sciences, adding up to thirteen participants in the activities of the Lisbon Nuclear Physics Centre of Studies. A sub-division occurred then, one around Baptista dedicated to radiation detectors and radioactive measures, and another around Barreira with more diversified interests. Studies of iodine-131 and atmospheric radioactivity were predominant, but others such as range determination of beta particles and irradiation with neutrons are also to be found.[6] The Portuguese journal *Revista da Faculdade de Ciências de Lisboa*, a journal dedicated to publishing research work of faculty members, was an easily reachable vehicle for publications written mostly in Portuguese, and sometimes in French and English. This circumstance accounts for *Revista* circulating outside Portugal. Actually, the contents of one of these articles[7] was the object of a controversy between the authors and D. Blanc of the *Laboratoire de Physique Atomique et Moléculaire du Collège de France*. The Portuguese reply found its way to the Italian journal *Il Nuovo Cimento*.[8]

Apart from involvement in research, Palacios and his group also organized training sessions in nuclear physics. Their first activity outside the laboratory aimed at furnishing graduates from the Higher Technical Institute (*Instituto Superior Técnico*) with preliminary notions on nuclear physics before their departure for foreign laboratories. In 1954 and in 1956 the courses lasted two months and the recipients were technicians from the Nuclear Energy Board. In 1956 and 1957, the courses were dedicated to physicians, the first lasting six months and the second a little over two months. Physicians also collaborated with physicists in delivering some of the lessons of these courses.

In 1958 the First Meeting of Portuguese Nuclear Energy Technicians (*1ª Reunião dos Técnicos de Energia Nuclear*) was an important event also for Palacios and his group, as revealed by the report he submitted. Palacios' assessment of the activities of the Centre of Studies he directed was clearly positive. Although the means at his disposal were parsimonious, "in accordance with its goals the young physicists were trained adequately in handling radioactive bodies, as well as in the study of the phenomena taking place inside them."[9] He also stressed the fact that articles were published in foreign journals. At the time Palacios' vision of future development of the Centre was undisturbed by the prospect of a Laboratory of Nuclear Physics

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[1] Julio Palacios, António Manuel Baptista, "A new method for the analysis of radioactive bodies", *Revista da Faculdade de Ciências*, 2nd Series, B–II (1952/3): 49−58.

[2] Julio Palacios, *1ª Reunião dos Técnicos Portugueses de Energia Nuclear* (*1st Meeting of Portuguese Nuclear Energy Technicians*), Lisbon, January 1958, p.1.

[3] António Manuel Baptista, and Norman Veall, "A Multi-Tube Gamma Counting Apparatus for Small Samples", *British Journal of Radiology*, 27 (315) (1954).

[4] António Manuel Baptista, *Curriculum Vitæ*, December 1955.

[5] Fernando Carvalho Barreira, *Curriculum Vitæ*, October 1956.

[6] Most of the articles were published in *Revista da Faculdade de Ciências de Lisboa*, with few exceptions, namely Fernando Barreira, and Manuel Laranjeira, "A graphical Absolute Method for Range Determination of β particles", *Journal of Applied Radiation and Isotopes*, 2 (145) (1957); Talks were also delivered at Iberian Conferences and at the *First International Conference on the Peaceful uses of Atomic Energy, Geneva, 1955.*

[7] António Manuel Baptista, Rui da Conceição Cordeiro, and Júlio Pistacchini Galvão, "Estudo Experimental de Detectores de Cátodo Externo na Região de Geiger-Müller", *Revista da Faculdade de Ciências*, 2nd Series B, IV (1955): 91−104.

[8] António Manuel Baptista, and Júlio Pistacchini Galvão, "Au sujet du Volume Sensible des Compteurs de Geiger-Müller à Cathode Externe", *Il Nuovo Cimento*, X (3) (1956): 647-8. Palacios, *1ª Reunião dos Técnicos Portugueses*, (ref. 2), 10.

[9] Palacios, 1ª Reunião dos Técnicos Portugueses, (ref. 2), 4−5.

and Engineering (*Laboratório de Física e Engenharia Nucleares*) on the way to be constructed in Lisbon, under the Nuclear Energy Board, to house a research nuclear reactor and two accelerators. His arguments were that physicists still needed to be trained in close partnership with the Faculties of Sciences, collaboration with the Cancer Hospital had been productive and should be maintained, and the Lisbon Nuclear Physics Centre of Studies possessed equipment and human resources able to construct radiation detectors which could evolve to industrial scale production.[10]

The real situation, however, was different. By the end of the 1950s it was possible to predict that the Lisbon Nuclear Physics Centre of Studies was to close in the short term. For several reasons Palacios' group began splitting up, slowly, and its leader was the first to set this trend. In October 1953 he returned to his post in the University of Madrid with the understanding that he would work fifteen days per month in Portugal.[11] This situation ended in November 1958 when he stopped collaborating with the Faculty of Sciences[12], retiring in 1961. However, the greatest threat to the group's integrity was the installation of the Laboratory of Nuclear Physics and Engineering, inaugurated in 1961, because members of Palacios' group were natural candidates to posts in this Laboratory and some of them were indeed recruited.

The research work published by the Centre involved investigations, most of them on selected topics: radiation detectors, study of iodine-131 and atmospheric radioactivity, clearly in syntony with a research laboratory aiming at developing physics methods and instrumentation addressed to cancer treatment. Taking into account the scientific unpreparedness of the graduates, we must recognize that Palacios' group made a remarkable effort. These results can be explained not only by Palacios' experience as an experimentalist but also by his commitment to the new task.

Nuclear Physics Applied to Civil Engineering

In 1952, Gibert was invited by Eduardo Arantes e Oliveira (1907-1982), director of the National Laboratory of Civil Engineering, to study the problem of sand drift by sea water. At the time radioactive tracing was a new method first studied by English researchers, based on the production of glass powder with a radioactive isotope, scandium 46, to be mixed with the sand grains. This practice was implemented in 1954. France and Japan followed the English procedure, while Gibert's group located at the civil engineering Laboratory used a different method.[13]

Theoretical research conducted by Gibert showed that radioactive tracers could be deposited on sand grains by an electrochemical process, with the limiting conditions that they should have a minimum half-life of 90 days, and a maximum not exceeding a few months, and that radiation emission should be easy to detect.[14] The choice fell on the radioactive isotope silver 110, with 270 days half-life, and strong gamma-ray emission of about 1.3 MeV. Experimental studies followed closely the theoretical ones in order to develop an adequate method[15]. In 1955, when Gilbert delivered his talk at the First International Conference on the Peaceful Uses of Atomic Energy, he announced the construction of a radioisotope laboratory at the National Laboratory of Civil Engineering. At the time a live trial had not yet been performed.[16]

The first live trial took place later at the river Tejo's mouth, in Cova do Vapor, under unfavourable conditions, aiming at establishing the comparison with the glass powder method. The results of this trial led to the conclusion that, on several parameters, the Portuguese method was more advantageous than the method based on glass powder. It was then decided to hold the next trial north of the port of Figueira da Foz, a city in the centre of Portugal near the mouth of river Mondego[17]. On July 1, 1957, 4,000 kg of sand labelled with 800 mc of silver 110 were dumped at a point of the beach about 2 km north of Cape Mondego. The samples collected in July, August and September enabled researchers to establish the sand drift in

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[10] Palacios, 1ª Reunião dos Técnicos Portugueses, (ref. 2), 15-16.

[11] Session 10 Oct 1953, "Minutes of the Academic Council (Actas do Conselho Escolar)", *AHMCUL* Archives, no. 1443, Book 9, p. 12v.

[12] Session 8 Nov 1958, (ref. 11), *AHMCUL* Archives, nº 1443, Book 9, p. 55.

[13] A. Gibert, "Emploi de Ag-110 dans l'étude du transport du sable par la mer", *Communication nº D35*, delivered at *7th Congress of International Association of Hydraulics Research*, Lisbon, 1957; A. Gibert, "Observation des mouvements du sable sous l'eau au moyen de l'argent 110", *Atome et Industrie, Geneva*, nº IV/C/17/1 (1959).

[14] A. Gibert, "Essai sur la possibilité d'employer Ag-110 dans l'étude du transport du sable para la mer", *Gazeta de Física*, III (3) (1955): 67–72, on 69. (Also reprinted as a *Laboratório Nacional de Engenharia Civil* publication).

[15] A. Gibert, J. Vasconcelos Pinheiro, "Marcação de areias com prata radioactiva e sua identificação em amostras empobrecidas na razão 1:106", *Boletim da Ordem dos Engenheiros*, IV (4) (1955). (Also reprinted as a Laboratório Nacional de Engenharia Civil publication).

[16] A. Gibert, , "Mise en oeuvre de quelques applications des radiations au Laboratório Nacional de Engenharia Civil de Lisbonne", First *International Conference on the Peaceful uses of Atomic Energy*, Geneva, 1955. (Also reprinted as a Laboratório Nacional de Engenharia Civil publication).

[17] Gibert, "Emploi de Ag-110", (ref. 13).

the foreshore in the area of Mondego Cape and of Buarcos Bay, and in this Bay not only in the foreshore but also down to a depth of 6,0 meters at least.[18]

In the 1960s, the technicians of the National Laboratory of Civil Engineering followed a new method based on bone glue developed by Soviet scientists. This new method allowed for a wider choice of radioactive labels than the previous one because it was possible to coat the sand grains with a waterproof membrane of glue which protected the radioactive element. The radioactive isotope selected was Phosphorus-32, with a half-life of 14.2 days, emitting high energy beta radiation, requiring less precaution than gamma-emitting isotopes. This time the purpose was a detailed study of the coastal drift in the Póvoa de Varzim harbour, in the north of Portugal. The method was considered extremely interesting concerning its sensitivity and the quickness in obtaining results.[19]

Concluding Remarks

There is no exact information as to when activities ceased at the Nuclear Physics Centre, nevertheless it can be deduced, by publications and the training abroad of its most important members, that it hardly survived after 1958. This was the year Palacios' link with the University of Lisbon was broken. By this time the Centre had fulfilled the aim of the Provisional Commission for Nuclear Energy Studies: "to offer young Portuguese graduates theoretical and practical training enabling them to utilize the many possibilities of phenomena relating to nuclear reactions".

The research work published by the Centre involved investigations on selected topics which were influenced by Palacios' and Baptista's direct collaboration with the Radioisotopes Laboratory of the Cancer Institute. The Centre's contribution to the training of specialists in nuclear areas helped to pave the way for the Nuclear Energy Board, which came into existence in 1954. At the end of the decade, ex-members of the Centre would actively participate in installing Nuclear Physics and Engineering Laboratory and Calouste Gulbenkian Laboratory of Mass Spectrometry and Molecular Physics. On the other hand, Baptista's appointment at Radioisotopes Laboratory of Cancer Institute, after 1953, meant a great contribution of the physicists to cancer treatment with radioisotopes.

Gibert's experience at the National Laboratory of Civil Engineering was ended by December 1960, also a short lived experience. Today there is no trace of it in the official history of this Laboratory. It seems that Gibert was not successful in implementing a durable radioisotopes practice in sand drift.

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[18] A. Gibert, F. Abecassis, M. Gonçalves Ferreira, J. Reis Carvalho, S. Cordeiro, "Tracing undersea sand movement with radioactive silver", *Second International Conference on the Peaceful uses of Atomic Energy*, Geneva, 1958. (Also reprinted as a Laboratório Nacional de Engenharia Civil publication).

[19] A. Gibert, S. Cordeiro, "A general method or sand labelling with radioactive nuclides", *International Journal of Applied Radiation and Isotopes*, 13 (1962): 41–45. (Also reprinted as a Laboratório Nacional de Engenharia Civil publication).

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 759.

THE AMERICAN INFLUENCE ON THE ORIGINS OF NUCLEAR PHYSICS IN MEXICO

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Abstract

In 1938 arrived to Mexico the expert in nuclear physics Marietta Blau (Austrian physicist), who worked at the National Polytechnic Institute –Instituto Politécnico Nacional*. However, she decided to move to the United States in 1944 without producing any incidence in the development of this field. Years later, several Mexican students returned to Mexico after finishing their doctoral studies in US universities. Two of them promoted nuclear physics at the University of Mexico. Marcos Moshinsky worked in theoretical nuclear physics while Nabor Carrillo promoted experimental nuclear physics acquiring equipment from US companies. In addition, Manuel Sandoval Vallarta, the first Mexican with a Ph.D. from the Massachusetts Institute of Technology, Carrillo and the Mexican physicist Carlos Graef were the first scientists who represented Mexico in the international meetings of nuclear energy.*

From French to American influence. Mexican students graduated in physical sciences in the United States

Several countries have inevitably influenced the development of physical sciences in Mexico. After prevailing during the nineteenth century, the influence of France gradually declined in the first decades of the twentieth century, when due to World Wars I and II the United States began to influence Mexican science. Specifically, the First World War forced young Mexican students interested in science to pursue postgraduate studies in the United States.

Such was the case of the first Mexican Ph.D. in Physics, Manuel Sandoval Vallarta (1899-1977), who graduated from the Massachusetts Institute of Technology (MIT) in 1924. He worked for the MIT for almost twenty years. At the beginning, in Mexico there were no physics degree programs, let alone research institutions. Before long, he became one of the most prominent and renowned scientists in the world, one who had decidedly contributed to the transformation of MIT, which became a major scientific institution in the United States.

The international renown of his work earned in the United States in the 1930s positioned Sandoval Vallarta on the research axis around which physics revolved in Mexico for some years. His talent encouraged university authorities to develop the first physics curriculum and to set up the first physics research center at the National Autonomous University of Mexico –*Universidad Nacional Autónoma de México*, UNAM.

Some Mexican students decided to join him at MIT to carry out their postgraduate studies in physics. When they returned to Mexico, they took charge of the Institute of Physics and other scientific institutions. Among these students were Alfredo Baños Jr. (1905-1994) and Carlos Graef Fernández (1911-1988), who graduated in MIT. Baños returned to Mexico in 1938 to become the director of the first Institute of Physics in Mexico, whereas Graef endeavored to promote the development of experimental nuclear physics (also the relativity field) and the projects of his friend and fellow student, Mexican engineer Nabor Carrillo (1911-1867).

Thanks to Sandoval Vallarta, institutional relations with the United States improved significantly. American experimental equipment began to be imported, collaborative research agreements were signed, US fellowships were promoted among Mexican students, and American scientists were invited to give lectures mainly at the UNAM. Furthermore, collaboration with American physics associations became closer and international congresses were held in Mexico.

When the United States decided to enter World War II, Sandoval Vallarta returned to Mexico. Because of his international prestige, he played a central role at national and international forums that discussed the uses of nuclear energy. In Mexico, he was a member of the National Commission on Nuclear Energy –*Comisión Nacional de Energía Nuclear*, CNEN–, created in 1955, and a scientific advisor and representative of the Mexican government in international events. In Mexico, he was appointed to several important academic and government posts, including Minister of Public Education, Director of the National Polytechnic Institute, and President of the Commission for the Promotion and Coordination of Scientific Research –*Comisión Impulsora y Coordinadora de la Investigación Científica*, CICIC.

World War II strengthened American influence on the development of nuclear physics in Mexico. Some US mathematicians, who disagreed with their country's war policy and wanted to live in Mexico, established contact with local scientific institutions. One of those scientists persuaded and helped Marcos Moshinsky Borodiansky (1921-2009) –Sandoval Vallarta's undergraduate student, and pioneer and promoter of theoretical nuclear physics in Mexico– to apply for a fellowship at Princeton University, where he met some of the best physicists of the world, who because of the war had fled Europe and were working at that institution.

After staying in different European cities that had been devastated by the war, Moshinsky adopted the strategies those countries were implementing to revive science. Some of those strategies included summer schools –which allowed the new generations to meet bright scientists–, development of a physics curriculum for young students, application for fellowships for international postgraduate programs, organization of national and international congresses, and production of rigorously peer reviewed journals.

In a matter of years, Moshinsky became one of the main promoters of physics, particularly theoretical nuclear physics. Among other activities relevant to the development of this field of knowledge in Mexico, he gave undergraduate and postgraduate courses, supervised theses and dissertations, secured fellowships for students who wanted to study in Europe and the United States, invited renowned foreign physicists to participate in international congresses and the like, chaired the Mexican Society of Physics –*Sociedad Mexicana de Física*–, and was the editor of the Mexican Physics Journal –*Revista Mexicana de Física*.

Moshinsky rapidly gained international renown, and put Mexico and the Institute of Physics on the world map of physics thanks to his contributions in group theory, which had remarkable applications in atomic, molecular, and nuclear physics.

European nuclear physicits activity in Mexico

Due to the Second World War and the Spanish Civil War, hundreds of European scientists came to Mexico, but unlike biomedical, biological, and chemical sciences, physics did not benefit much from this influx, despite the arrival of a few European physicists, such as Marietta Blau (1894-1970), renowned Spanish physicist Blas Cabrera (1878-1945), Spanish astronomer Pedro Carrasco Garrorena (1883-1966), and German physicist Thomas Brody (1922-1988). Blau and Brody worked in nuclear physics, while Cabrera focused on disseminating nuclear physics information through science magazines.

To understand the context in which nuclear physics projects were launched, it is important to further examine the Mexican situation in 1938. In the 1930s, the UNAM developed the first physics curriculum. In 1938 the Institute of Physics was founded and Alfredo Baños Jr. (who had recently finished his doctorate studies at MIT under Sandoval Vallarta's supervision) was appointed its director. The Institute staff consisted of four researchers and a research assistant; only the director had a doctorate degree.

At the beginning, the main research lines were cosmic radiation, hydrodynamics and elasticity, biological physics, nuclear physics and radioactivity, spectroscopy and atomic structure, X-rays and molecular structure, astrophysics, geophysics, and soil mechanics laboratory. The cosmic rays project was the institute's most important undertaking; it operated under the supervision of Sandoval Vallarta at MIT, and in Mexico when he was there on vacation.

On the other hand, Mexico was going through important social, political, economic, and scientific changes. President Lázaro Cárdenas (1895-1970) had introduced substantial social, political, and economic reforms, aiming at decreasing

Mexico's dependency on other countries. To that end, he expropriated the assets of some foreign companies to create national entities, among them the state oil company PEMEX. In education, Cárdenas promoted higher technical studies to train technicians and specialists in industry-specific issues necessary to boost the country's economy.

It was in this context that the National Polytechnic Institute (IPN) was created in 1936. The IPN comprised several technical schools that had been founded years before. Among these was the Higher School of Mechanical and Electrical Engineering –*Escuela Superior de Ingeniería Mecánica y Eléctrica*, ESIME–, where Marietta Blau started working in 1939.

Between 1937 and 1942, that policy allowed hundreds of specialists in various scientific fields to become faculty and researchers in different Mexican education institutions, among which the UNAM and the IPN were the most important. Since the UNAM had become autonomous in 1929, a capacity that prevented government institutions from influencing its decisions, Cárdenas founded the IPN within the Ministry of Public Education –*Secretaría de Educación Pública*, SEP.

Of that influx of scientists, only a few were physicists. Hence, they exerted little influence on the development of physics, as compared to fields such as the biomedical, biological, and chemical sciences, to which Spanish exiles contributed greatly. Marietta Blau, for example, came to Mexico but left for the United States some years later. Blas Cabrera, an eminent Spanish scientist, died in Mexico after a few years due to a preexisting condition. Thomas Brody, in contrast, stayed and worked with Marcos Moshinsky in theoretical nuclear physics.

One of the problems facing Blau at the IPN was that the department for which she worked did not have a research budget, as it was only part of an engineering school. Therefore, since she could not live exclusively on her income as an ESIME professor, she had to teach in other institutions. This was a widespread problem among education institutions, and the professors had to teach at several schools to earn a decent income. This allowed her to meet several scientists who taught at both the IPN and the UNAM. Strangely enough, she never gave classes at the UNAM or collaborated with the Institute of Physics, which carried out research in several fields, including nuclear physics.

Marietta Blau was working with Sandoval Vallarta as Head of the Radioactivity Laboratory of the Commission for the Promotion and Coordination of Scientific Research (CICIC). She worked with Sandoval Vallarta at the same time that Nabor Carrillo, but Carrillo was responsible for the project *Estudio del subsuelo de la Cuenca de México* (A Study of the Subsurface of the Mexico Basin). When she decided to go to the United States (in 1944), she didn't know that Carrillo would be the most important promoter of experimental nuclear physics in Mexico.

Nabor Carrillo, main promoter of experimental nuclear physics in Mexico

The main promoter of experimental nuclear physics in Mexico was the Mexican engineer Nabor Carrillo. Because of the Spanish Civil War, he was not able to study with Blas Cabrera –as he had originally intended– but completed postgraduate studies in soil mechanics at the University of Harvard. Carrillo had the opportunity to be present at one of the atomic bomb tests on the Bikini Atoll site in 1946, which convinced him that Mexico had to join the nuclear age.

Coincidentally, Carrillo attended the university where Van de Graaff, the creator of the eponymous particle accelerator, worked. His Ph.D. supervisor was the brother-in-law of one of the owners of the High Voltage Engineering Corporation, a manufacturer and seller of particle accelerators. During a visit to the company's facilities, Carrillo, mesmerized by the potential applications of this equipment in various fields, especially medicine, decided to buy an accelerator for the UNAM –the most costly device in the history of the university.

When Carrillo was appointed President of the UNAM, he already enjoyed international recognition. He was regarded as one of the leading experts in soil mechanics because with a simple method he had been capable of solving the serious problem of subsidence in the Long Beach area. He took advantage of his position at the university to support scientific projects, especially those related to nuclear physics. Also in that capacity, he signed agreements with health institutions to promote the use of nuclear physics in medicine, as the 2-MeV Van de Graaff accelerator –installed in 1952 and the first of its kind in Latin America– and the necessary infrastructure were already in place.

Regarding the use of nuclear energy, Carrillo had other more ambitious projects, which he developed after his tenure as president. With the CNEN, of which he was a Board Member –as was Sandoval Vallarta–, he started negotiations to purchase Mexico's first nuclear reactor. As the cost of the reactor was way over the budget of the UNAM, the largest and best funded university in the country, he decided to seek support from the government, which showed no interest in the project.

This situation improved with the American exhibition "Atoms for Peace," held on the grounds of the Campo Marte military base in Mexico City. Finally, in 1963, a Triga Mark III Reactor was purchased from the American company *General Atomic*. The reactor was installed at the then Nuclear Center of Mexico, known today as the Nabor Carrillo National Institute for Nuclear Research. Unfortunately, Carrillo died prematurely, leaving other projects unfinished.

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References

AZUELA, Luz Fernanda; TALANCÓN, José Luis (1999) *Contracorriente. Historia de la energía nuclear en México (1945-1995)*, México, Plaza y Valdés.

Ciencia (a science magazine), v. V, 1944, 12.

DOMÍNGUEZ MARTÍNEZ, Raúl (2000) *Historia de la física nuclear en México (1933-1963).* México, UNAM, Plaza y Valdés Editores.

ESQUEDA, Enrique; RAMOS LARA, María de la Paz, "Nabor Carrillo, pioneer of nuclear physics in Mexico", in press.

FLORES PALAFOX, Jesús; MONTEÓN GONZÁLEZ, Humberto (1993) *La ESIME en la historia de la enseñanza técnica,* Primer tramo, México, Instituto Politécnico Nacional.

GALISON, Peter L. (1997) "Marietta Blau: Between Nazis and Nuclei", *Physics Today,* 50: 42.

GAONA, E. (1999) *Cáncer, radiación y seguridad radiológica. Medicina nuclear e investigación*, México, Edilibros.

GRAEF FERNÁNDEZ, Carlos (1993) "Origen del desarrollo y uso de los aceleradores de partículas en México", in José FERNÁNDEZ CHAPOU and Alfonso MONDRAGÓN BALLESTEROS, *Carlos Graef Fernández, Obra Científica,* México.

GRAEF FERNÁNDEZ, Carlos (1993) "Primera etapa de trabajo del grupo del acelerador Van de Graaff Tandem en el Centro Nuclear de la Comisión Nacional de Energía Nuclear" (1969), in *Carlos Graef Fernández. Obra Científica* (J.L. FERNÁNDEZ y A. MONDRAGÓN compiladores), México, UAM.

LEÓN LÓPEZ, Enrique G. (1986) *El Instituto Politécnico Nacional. Origen y evolución histórica,* México, Instituto Politécnico Nacional.

MENDOZA, Eusebio (1986 –1st Edition, 1980) *La educación tecnológica en México,* Instituto Politécnico Nacional, México.

MOSHINSKY, Marcos (2003) "Mi vida", *Forjadores de la ciencia en la UNAM,* México, pp. 15-35.

PÉREZ TAMAYO, Ruy (2005) *Historia general de la ciencia en México, en el siglo XX*, México, Fondo de Cultura Económica.

RAMOS LARA, María de la Paz (2008) "La Comisión Impulsora y Coordinadora de la Investigación Científica", *Jornadas Anuales de Investigación 2006,* CEIICH-UNAM, pp. 349-360.

RAMOS LARA, María de la Paz (2006) "Particle accelerators in Mexico", *Historical Studies in the Physical and Biological Sciences*, Volume 36, Part 2, pp. 297-309.

RAMOS LARA, María de la Paz (2006) "La física austríaca Marietta Blau durante su estancia en México (1938-1944)", *Emigración Centroeuropea a América Latina,* Universidad Carolina de Praga, Editorial Karolinum, pp. 157-165.

RAMOS LARA, María de la Paz (2008) "En torno a la Relatividad en la Biblioteca de la Sociedad Científica Antonio Alzate", *La Relatividad en México*, UNAM, México.

STROHAMAIER, Brigitte; ROSNER, Robert (Eds.) (2006) *Marietta Blau. Estrellas de desintegración,* México, Edición del Instituto Politécnico Nacional (realizada por Esperanza VERDUZCO RÍOS).

VÉLEZ OCÓN, C. (1997) *Cincuenta años de energía nuclear en México, 1945-1995*, México, UNAM.

Manuel Sandoval Vallarta. Homenaje (1987) México, Instituto Nacional de Estudios Históricos.

"Nabor Carrillo Flores," Archivos del Instituto de Investigaciones Sobre la Universidad y la Educación. The Nabor Carrillo Flores Fund. UNAM.

Archive Manuel Sandoval Vallarta. Universidad Autónoma Metropolitana Iztapalapa.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 763.

NUCLEAR AND SUBNUCLEAR PHYSICS IN ITALY IN THE SECOND HALF OF THE 20TH CENTURY

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Abstract

Before World War II, two departments were principally active in Italy, known in all the world: Arcetri (near Florence) and Rome. The promulgation of racial laws (1938) and the worsening of work condition caused the departure of most of the best know Italian physicists.
During WW II the remaining part of ex Fermi's group took a very important decision for their future: to avoid any involvement in the war, they left off the nuclear physics studies. They preferred to move towards different fields of research, in which the costs were at stake for a hard knocking Nation from the war.
The years immediatly after WW II were characterized, in Italy, by a very difficult reorganization, also in the fields of physics. The work of reconstruction of Italian physics was undertaken by members of the two principal groups of research presented before the war. In particular, it's right to give attention to Amaldi (Fermi's group) and Bernardini (Arcetri's school); with which a large number of important physicists worked to get ahead to the rebuilding operations.
The availability of some departments with a good level of research was fundamental to continue in the rebuilding works, together with the help of a research institute, the INFN, placed over all the country.
In particular physics, the Italian contribution took place in Frascati's Laboratory building, where there were established an electrosynctrotron –a prototype for more collider generations– and the AdA's accelerator with its follower Adone; in the participation to CERN's building; and in the building of a laboratory in Legnaro, Catania and Gran Sasso.
The winning of the Nobel Prize by Rubbia (1984) for the discovery of the boson vector $W^{\pm}$ e Z^{0} represented the peak of the Italian contribution to particle physics. On theoretical aspects, the main results were Puppi's triangle, the introduction of Cabibbo's angle, and the fourth quarks by Maiani (with Glashow and Iliopoulos).

At the beginning of the 20th century, while European physics was discovering new paths towards new theories, in Italy the situation was very different because there wasn't any attention to the new physics. In Italy there were some good centres in which experimental physics was at a good level, but there were no physicists that gave any attention the new theories such as relativity, both special and general, and quantum mechanics.

This situation changed in 1926, when Enrico Fermi obtained in Rome the first teaching of theoretical physics: it was the start of a new era for Italian physics, an era in which Italy became a cornerstone in the new fields of research, in particular in nuclear physics and cosmic rays physics. At the same time in this period the foundations were put for what would be the (re)building of Italian physics after World War II (WW II).

Together with the work of Fermi in Rome, there was similar work made by Bruno Rossi in Arcetri (near Florence): it's possible to assert that there were two schools for Italian physics in the first half of the 20th century: Rome and Arcetri. And it would be these two schools which would play a fundamental role in the (re)birth of Italian physics after WW II. Obviously, there were in Italy a lot of other situations in which it was possible to study good physics, but these other places were all devoted to experimental physics with particular attention to classical physics.

School of Arcetri – main members	School of Rome – main members
Enrico Persico Gilberto Bernardini Giuseppe (Beppo) Occhialini Daria Bocciarelli Bruno Rossi Guglielmo Righini Beatrice Crinò Giulio Racah Lorenzo Emo Capodilista	Edoardo Amaldi Franco Rasetti Enrico Fermi Bruno Pontecorvo Gian Carlo Wick Ettore Majorana Oscar D'Agostino

A problem of historical studies is its potentially subjective character; it would be a good result to find an objective system to evaluate facts. In this perspective, in my work attention is given to some parameters by which we can evaluate the quality of a physics school: such parameters are, first of all, coincident with the number of articles and prizes obtained by the school. So in this work I have taken into account all the articles written by Italian physicists in the period 1900-1940 and published in *Il Nuovo Cimento*, the principal Italian physics review, in particular in the period before WW II. At the same time the subjective evaluation takes into account the prizes awarded to Italian physicists in the same period, with particular attention to the Nobel Prize and to international prizes available after WW II, such as Dirac medal, Wolf prize, Dannie Heineman prize, Tate medal, etc. Last but not least, the international role played by a school of physics could be measured by the importance of the Congress organized in the country. From this point of view, Italy obtained the right acknowledgment with the Rome Congress in 1931, the first Congress on nuclear physics in which all the best nuclear physicists was present.

It is possible to summarize this work using two graphics in which I give a scheme of the work. As it is the case for all such schemes, also in this case schematization introduces a work simplification, but it is useful to understand the guideline of this work.

* * *

Before starting it is very important to underline that in Italy, in the same years, there were a lot of different realities, much of them very important from the point of view of the work that was made inside: with Rome and Arcetri, also Turin, Milan, Bologna, Naples, Palermo and other Institutes were the place in which experimental work was made in physics. But it's also true to assert that some Institutes were more important than others. In particular, without the work made in Rome and in Arcetri, Italy wouldn't have regained a guide role in physics in the Thirties, and it would be impossible to start the rebuilding work after WW II.

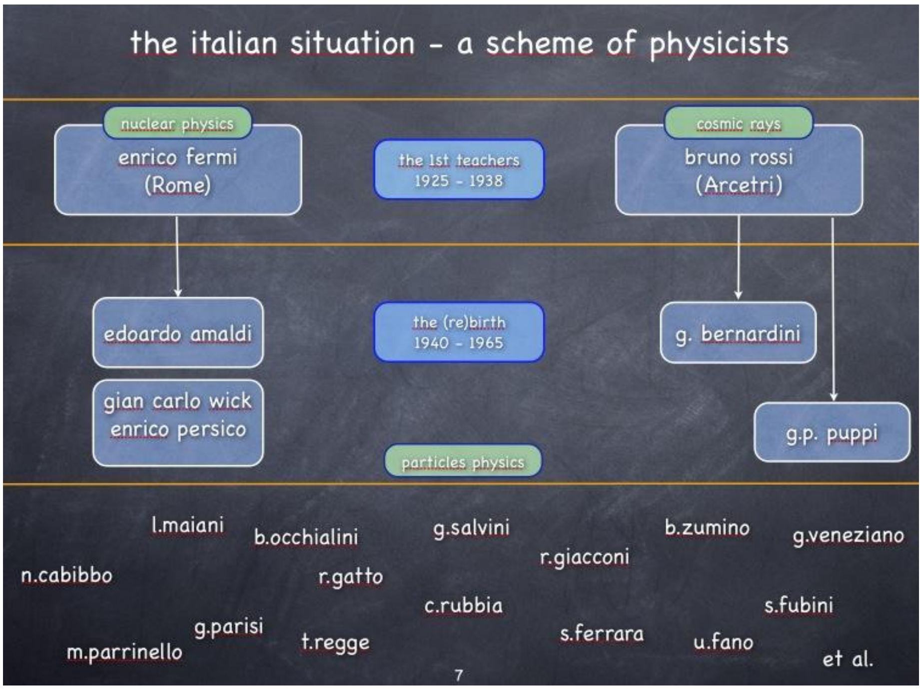
My idea is that there was a first level of teachers of Italian physics, and these teachers were Bruno Rossi and Enrico Fermi. Obviously, these two teachers were working in a complex context, in which other very important physicists were present, such as Orso Mario Corbino, Antonio Garbasso and Enrico Persico. The first two played a fundamental role from the political point of view; in fact, Corbino, who was a physicist, covered a lot of very important roles in the Italian government, and gave a big impulse to the composition of the school of physics in Rome. At the same time, Garbasso played an analogue role in Florence; in fact he was the mayor of the city and from this political charge he helped the daily life of the Arcetri Institute, where Rossi and Persico worked.

For the physicists in Arcetri, Enrico Persico was an important teacher that helped young physicists to improve their knowledge, in particular in the field of quantum mechanics. All the members of the Arcetri's group remembered Persico for the attention he posed in the preparation of his lessons; in fact, two of them recollected Persico's lessons in a book.

It is known that towards the end of the Thirties in Italy the majority of the physicists were forced to leave the country due to the odious racial laws that were promulgated by the Italian fascist regime. So, near the start of WW the Italian physics was in a very difficult situation, with only a few physicists that could decide to stay in their own country. Among them we could remember Edoardo Amaldi and Gilberto Bernardini, each one a member of each of the previously mentioned schools. They will be the cornerstone on which Italian physics will be (re)built after WWII. It's obvious that there were a lot of other physicists in Italy in the same period, and some of them played a very important role, but the polar star of the new start of physics in Italy was due to only some of them. Together with Amaldi and Bernardini, it is right to remember also Gian Carlo Wick and Enrico Persico. The first of these two left Italy near the end of the Fifties, and the second one covered the reverse path in the same period, after a very particular human adventure[1].

In the same scheme also Giampietro Puppi is shown: it's a different, more particular case but also very important for the renaissance of physics in the city of Bologna, the oldest Italian university. Puppi's work was mostly localized in the Department of Bologna, where he arrived after WW II. In that Institute the situation was very difficult, because before Puppi in Bologna worked Augusto Righi and Quirino Majorana, two physicists as good in experimental work as far from new physics[2]. Puppi's work was devoted, first of all, to the reformulation of the research fields on the path of new physics and to the search of new money to guarantee the possibility of organizing a good and long term research work.

All the other names indicated in the bottom of the picture are some of the physicists that continued the work started after WW II.



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In the previous pages it was indicated that the fields of research in Italy before WW II were, in particular, nuclear physics and cosmic rays physics. These two fields represented the base for the development of new fields on physics research in Italy after WW II: elementary particle physics.

The path towards elementary particle physics was characterized by some key points. Among them it's possible to indicate:

- the invention of the coincidence circuit, due to the work of Bruno Rossi in Arcetri towards the end of the Twenties (a work in the field of cosmic rays);
- the discovery of the slow neutron, due to the work of Fermi and others in the middle of the Thirties (a work in the field of nuclear physics);

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[1] In the picture of the (re)building of Italian physics, Amaldi knew that a physicist like Persico was fundamental; so he proposed him to return to Italy from Canada, but due to a dramatic medical reference Persico had to refuse the proposal. After a lot of time and after a new medical check, Persico knew that the first medical information was wrong and his health was good, so he could finally return to his own country.

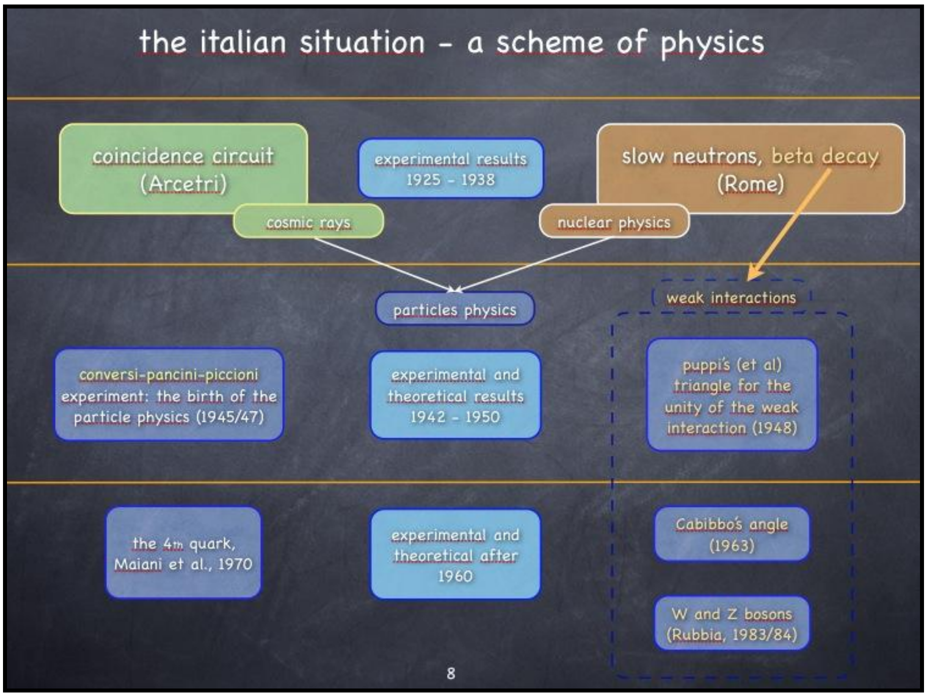
[2] It's to note that the words "new physics" indicate the physics born at the beginning of the 20th century (relativity and quantum mechanics), a physics which was around 30 years old, and on which also a lot of Nobel Awards had focused.

- the experimental work of Conversi-Pancini-Piccioni in 1943 in Rome[3], under the bombing of the Allied force: this is considered the work that starts the era of elementary particle physics.

These last three points represent the base on which the restart of physics will be built; in particular the first results obtained were in the field of weak interaction. Also this is a path that started in the past: one of the first fundamental articles of Enrico Fermi spoke about the β-decay. After this start, in the following years a lot of new articles opened the street towards a very important role played by Italian physicists; in particular the work of Puppi in 1948[4] meant an important goal. After that it is right to remember Cabibbo's work in 1963, a work defined by the Royal Swedish Academy of Sciences as a *"cornerstone of the weak interaction"*[5]

The line started with the cited article by Fermi, and found its end in 1983 with the Nobel Prize awarded to Carlo Rubbia[6] for the experimental discovery of the W and Z vector bosons.

Another very important goal obtained on this path started with the work of the Rome and Arcetri groups, and was the theoretical introduction of the fourth quark, proposed in 1970 by Luciano Maiani and others[7].



* * *

As indicated in the previous lines, one of the feature of this work is to find an objective system to evaluate the quality of the work in physics in a country, or in a part of a country. One system is counting the number of articles divided by topics published on specialized magazines. In the Italian case the first step is to count the number of articles published on *Il Nuovo Cimento* in the period from 1900 to 1940. To deepen the analysis it is possible to divide the period in two steps: the first one from 1900 to 1920 and the second one from 1921 to 1940. This division is not casual, but corresponds to the change that was starting in Italy: the advent of the new physics also in Italy by the work of the forthcoming school of physics.

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[3] Due to the very particular work conditions during the war years, this work was published only in 1947.

[4] At the same time also Tiomno and Wheeler obtained the same result; each of one of them independently.

[5] Class for Physics of the Royal Swedish Academy of Sciences, *Broken Symmetries*, (2008).

[6] The Nobel Prize was awarded jointly to Simon van de Meer.

[7] S.L. Glashow, J. Iliopoulos, L. Maiani (1970). "Weak Interactions with Lepton–Hadron Symmetry". Physical Review D **2** (7): 1285–1292.

JOURNAL	TOPIC	N art.	%
Il Nuovo Cimento 1900-1940	Zeeman effect	9	2%
	Cathod rays	10	2%
	Cosmic rays	14	2%
	Relativity	25	4%
	X rays	35	6%
	Radioactivity	42	7%
	Nuclear Physics	53	9%
	Quantum physics	90	15%
	Electricity	134	22%
	Electromagnetism	184	31%
		414	**69%**
		182	**31%**

JOURNAL	TOPIC	N art.	%
Il Nuovo Cimento 1900-1920	Cosmic rays	0	0%
	Nuclear Physics	0	0%
	Quantum physics	2	1%
	Zeeman effect	6	2%
	Cathod rays	10	3%
	Relativity	11	4%
	X rays	21	7%
	Radioactivity	32	11%
	Electricity	79	28%
	Electromagnetism	125	44%
		286	**100%**

JOURNAL	TOPIC	N art.	%	Δ
Il Nuovo Cimento 1921-1940	Cathod rays	0	0%	**-3%**
	Zeeman effect	3	1%	**-1%**
	Radioactivity	10	3%	**-8%**
	Cosmic rays	14	5%	**5%**
	Relativity	14	5%	**1%**
	X rays	14	5%	**-2%**
	Nuclear Physics	53	17%	**17%**
	Electricity	55	18%	**-10%**
	Electromagnetism	59	19%	**-25%**
	Quantum physics	88	28%	**27%**
		310	**100%**	

It's possible to find some clear indication: a lot of topics, such as Cathod rays, Zeeman effect, X rays, Relativity, show small variation (in rate), while some other topics show big variation in rate, as positive as negative. In particular is interesting to note the small variation of the rate in Relativity (+1%); also the variation of Cosmic rays is small in rate, but it's important to underline that this topic is starting from a null rate.

Other topics show a big change in rate; in particular it is possible to note a big reduction in the rate of Electricity and Electromagnetism, and, at the same time it's noticeable the positive change of Nuclear Physics and Quantum Physics: all this indications tell us that the topics change was started also in Italy, and the effect of this change started to be evident.

* * *

The analysis of publications of Italian physicists before WW II would be deep with graphs and other possible readings of the data. But in a brief article like this it's time to go to the analysis of similar data referred to the period after WW II. Before the war, in Italy the majority of articles were published on a single magazine, but after the war this practice changed. So the first path to walk is to find the right source from which to extract the correct data. For this period I detected four different sources, that I list now:

- The Physical review – the first hundred years
- http://www.springerlink.com/
- http://scitation.aip.org/
- http://publish.aps.org/

The first one is a publication in which all the articles that are considered the most important articles are collected, divided in topics:

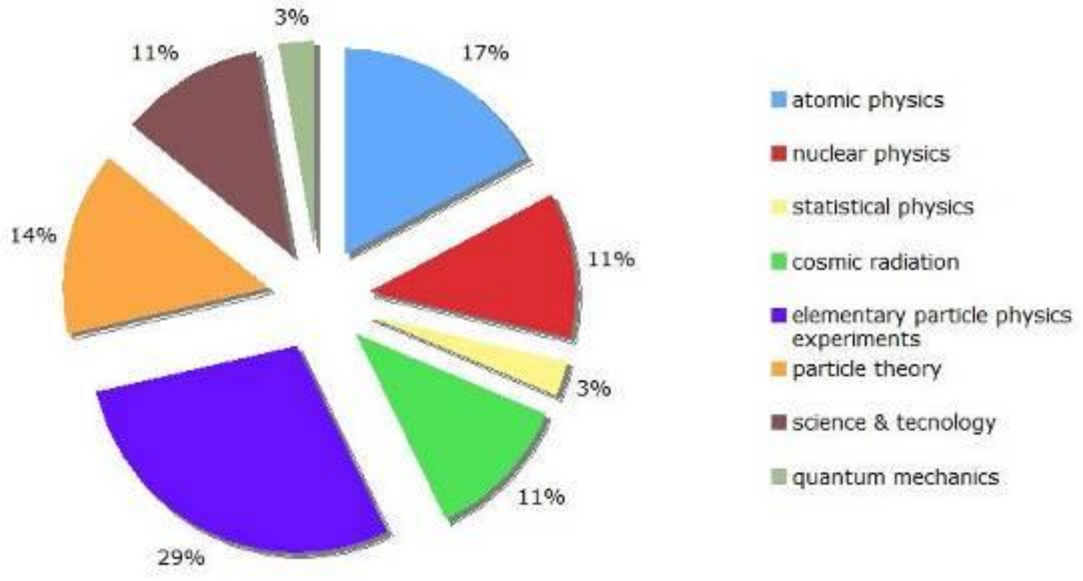
> "*The Physical Review-The First Hundred Years* is a selection of seminal papers and commentaries highlighting the developments in physics and their applications presented in printing and electronic form.
>
> The publication of this collection is sponsored jointly by the American Physical Society and the American Institute of Physics in the celebration of the 100th anniversary of *The Physical Review*".[8]

The other three are internet sites in which a lot of scientific magazines are recollected, such as: *Il Nuovo Ciment, Zeitschrift für Physik* (springerlink.com); *Physical Review Letters*, *Review of Modern Physics*, *Physical Review A – E* (publish.aps.org); *Physics Today*, *Physics Review*, *AIP Conferences Proceedings*, *Review of Scientific Instruments* (scitation.aip.org).

In the case of *The Physical Review* it is possible to extract the data referred to the Italian physicists. Once these data in a table, and then in a graphic, it is possible to discover some interesting information:

Subject	N art.	%
atomic physics	6	17%
nuclear physics	4	11%
statistical physics	1	3%
cosmic radiation	4	11%
elementary particle physics experiments	10	29%
particle theory	5	14%
science & tecnology	4	11%
quantum mechanics	1	3%
TOTAL	**35**	**100%**

[8] *The Physical Review. The first hundred years*, pag V.



Percentage of Italian articles by subject.

It's possible to note that there is a clear prevalence of the topic called "Elementary particle physics experiments", following from the topics called "Atomic physics" and "Particle theory". From my point of view it's a clear result caused by the choice to deepen nuclear physics and cosmic rays before WW II. At the same time there are some other topics with a good rate of articles in the first hundred years of the magazine. In each case it is needed to underline that the period covered by this book starts in the end of the 19th century (at around 1894) and ends in 1994; in other words, within the total number of articles, a lot of them were written before WW II, hence in the period corresponding to the predominance of classical physics in Italy.

* * *

As indicated in the first part of this article, I focused my attention on some of the main physicists involved in the (re)building work of Italian physics. I explained mi choice in the starting part of this work, and now I try to finish this path. To go towards the end of this path I need to analyze the data available from the previously listed sites; in this article I give only an indication of a more complex work that has found its completion in my Ph.D. thesis.

As a starting point it is possible to use the total number of articles written by this four physicists in the period 1932-1996[9]:

Number of articles from 1932 to 1996			
Edoardo Amaldi	Gilberto Bernardini	Gian Carlo Wick	Giampietro Puppi
75	46	37	39

Starting from this data is possible to organize some particular reading; for example, is possible to identify the moment that corresponded to the passage from the nuclear to the particle physics. From the case of Edoardo Amaldi:

[9] The fabulous four are: Edoardo Amaldi, Gilberto Bernardini, Gian Carlo Wick and Giampietro Puppi. The chosen period starts with the publication of the first articles and ends with the last particles paper written by one of the fabulous four.

NUCLEAR PHYSICS	1932	1933	1934	1935	1936	1937	1938	1939	1941	1942	1943	1946	1947	TOT.	%
Naturwissenschaften	1									1	1			**3**	**13,0%**
Zeitschrift für Physik	1	1												**2**	**8,7%**
Proc. R. Soc. Lond.			1	1										**2**	**8,7%**
Il Nuovo Cimento			3								1	2		**6**	**26,1%**
Phys. Rev.					2	1	1	2	1				2	**9**	**39,1%**
Rev. Sci. Instrum.												1		**1**	**4,3%**
TOTAL	**2**	**1**	**4**	**1**	**2**	**1**	**1**	**2**	**1**	**1**	**2**	**3**	**2**	**23**	**100%**
%	**8,7%**	**4,3%**	**17,4%**	**4,3%**	**8,7%**	**4,3%**	**4,3%**	**8,7%**	**4,3%**	**4,3%**	**8,7%**	**13,0%**	**8,7%**	**100%**	

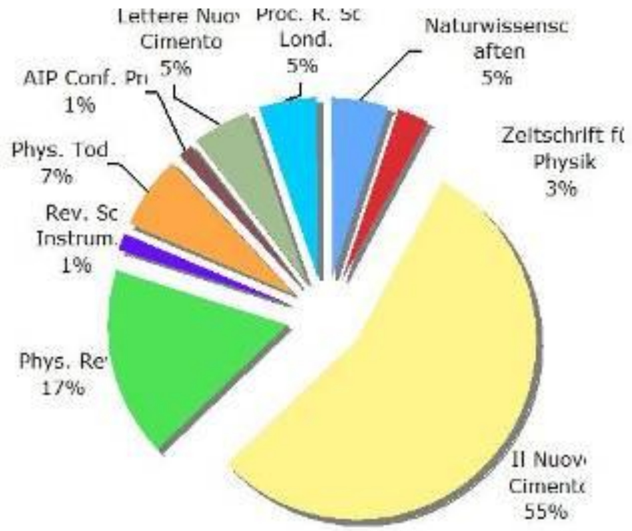
PARTICLE PHYSICS	1950	1951	1952	1953	1954	1955	1956	1957	1959	1960	1961	1963	1970	1973	1977	TOT.	%
Naturwissenschaften					1											**1**	**3,7%**
AIP Conf. Proc.														1		**1**	**3,7%**
Il Nuovo Cimento	2		1	2	5	1	2	1	1	1	1	2	1			**20**	**74,1%**
Phys. Rev.		1					1	1								**3**	**11,1%**
Lettere Al Nuovo Cimento															2	**2**	**7,4%**
TOTAL	**2**	**1**	**1**	**2**	**6**	**1**	**3**	**2**	**1**	**1**	**1**	**2**	**1**	**1**	**2**	**27**	**100%**
%	**7,4%**	**3,7%**	**3,7%**	**7,4%**	**22,2%**	**3,7%**	**11,1%**	**7,4%**	**3,7%**	**3,7%**	**3,7%**	**7,4%**	**3,7%**	**3,7%**	**7,4%**	**100%**	

It's possible to note that the articles written by Amaldi on Nuclear Physics started in 1932 and ended in 1947, with a total number of 23; likewise, the articles published by Amaldi on Particle physics started in 1950, just after the end of the previous articles, and ended in 1977, towards the end of his scientific career. The total number of articles in this case is 27, a number very similar to the previous. The articles referred to the first topics are scattered over a period of 15 years (in particular in the first 10 years, from 1932 to 1941), while the second topics are scattered on a more wide range: 27 years. The same analysis is made in the other case.

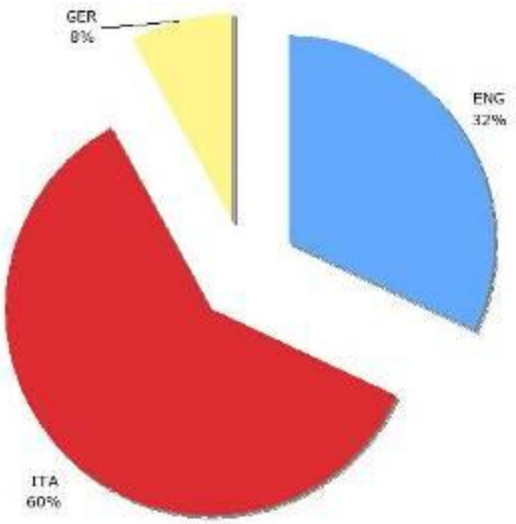
* * *

To end, I'd like to propose a further reading of the articles written in the indicated period. Always in the case of Amaldi, but also in all the other, it is possible to read the number to underline the correlation between the rate of articles and the magazines, and also the rate of articles and its language. In the first case the result can be summarized in a table with the corresponding graphics:

MAGAZINE	N art.	%
Naturwissenschaften	4	5,3%
Zeitschrift für Physik	2	2,7%
Il Nuovo Cimento	41	52,7%
Phys. Rev.	13	17,3%
Rev. Scient. Instrum.	1	1,3%
Phys. Today	5	6,7%
AIP Conf. Proc.	1	1,3%
Lettere a Il Nuovo Cimento	4	5,3%
Proc. Royal Soc. London	4	5,3%
TOTAL	**75**	**100%**



Number of articles by magazines.



Number of articles by speech areas.

There is a wide range of magazines chosen for the publications, with a clear predominance of *Il Nuovo Cimento*. Grouping the data by linguistic areas, there is a predictable predominance of the Italian language, and a good rate also for the English language. German is due to the use of the Rome group to publish in Italian and German. But this use changed after WW II.

WRITING THE HISTORY OF THE PHYSICAL SCIENCES AFTER 1945

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 773.

MEMBERSHIP OF CERN AND THE EUROPEAN SCOPE OF SPANISH PHYSICS[1]

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Abstract

Spain joined CERN on 1 January 1961, to withdraw seven years later on financial grounds. Building on the CERN archives, this paper deals with Spain's first membership of CERN as seen by the physicists and diplomats that handled membership. Beyond the lack of political will and the economic burden that are usually blamed for Spain's withdrawal, I argue that there were "failures of communication" between CERN and Spain, that became apparent in the recruitment of staff, the tender of contracts, and the contribution of member states. Politics and economics were not solely responsible for withdrawal: the long-term R+D policies of Franco's regime, as well as the research outlook of scientists and policy makers such as J. M. Otero, need to be taken into account.

Spain became a member of the European Organization for Nuclear Research (CERN) on 1 January 1961. CERN had been created in 1954 by 12 European countries with the avowed aim of pursuing particle physics research. Spain withdrew in June 1969 with effect from 31 December 1968, to join again in 1983, under the first socialist government of democratic Spain. In spite of its significance for any understanding of research, science policy and international relations during Francoism, Spain's first stage at CERN has claimed little historical attention. The three-volume *History of CERN* reflects the nearly negligible impact of Spanish membership in the 1960s: "Spain" first appears in vol. 2 as a country "hardly represented at Geneva in the mid-1960s", and is listed in vol. 3 as one of the countries that abstained on the new convention in 1967.[2] Spanish high-energy physicists have portrayed the episode as "a paradigmatic example of erratic governmental

[1] Research for this paper has been supported by the Spanish Ministry of Education through project HAR2008-05039/HIST "Bases para una historia de la física en España en el siglo XX". I acknowledge permission to access and quote from the CERN Archives, Geneva (Switzerland), and thank Anita Hollier, CERN archivist, for her assistance; unless otherwise stated, archival references are to files in the CERN archives. I also thank participants in the simposium "Writing the history of the physical sciences after 1945", particularly John Krige, for their comments.

[2] See D. Pestre, "The CERN system, its deliberative and executive arms and some global statistics on how it functioned", in Armin Hermann, John Krige, Ulrike Mersits and Dominique Pestre, *History of CERN. Vol. 2, Building and running the laboratory* (Amsterdam; Oxford: North-Holland, 1990), p. 341–415, on p. 409: "Austria, Denmark, Greece, and Spain […] were hardly represented at Geneva in

policy". They reckon that diplomacy, rather than scientific research, was the main reason to join, and agree with the official line that the mounting contribution made withdrawal inevitable. Even though "membership clearly had a positive scientific impact", it was "badly planned and nothing was done to take advantage of it".[3]

One important work has implicitly challenged the physicists' account. Building on the archives of the Spanish Ministry of Foreign Affairs, Ana Romero de Pablos and José Manuel Sánchez Ron have provided evidence of the Spanish government commitment to the organization through the 1960s. Central to their account is the Spanish candidature for the site of new 300 GeV Super Proton-Synchrotron that CERN started to plan in 1964, and finally built in Geneva. In June 1966, a commission including chairmen of industry, high-rank officials, diplomats, scientists and an embedded journalist, campaigned in CERN for the Spanish site, a visit that was well publicized in the Spanish press. Franco's cabinet came to be split on the candidature: an ad hoc ministerial Commission recommended in September 1967 to remain in CERN as long as the new accelerator was built in El Escorial; for the Ministry of Foreign Affairs, membership was not to be conditioned by the decision about the site; and the Ministry of Finance pushed for withdrawal whatever the case. Romero and Sánchez Ron finally endorse the view that the Spanish economy could not bear the burden of membership.[4]

In this paper, I bring to this story previously untapped sources in the CERN archives. I deal with three prominent issues that had to be balanced against each other: the contribution of member states, on the one hand, and the recruitment of staff and the tender of contracts, on the other. I am concerned with the tacit cultural assumptions that underlined the relations between the Spanish physicists and their European counterparts, and reflect upon their significance for the regime's take on science and technology.[5] Spain's membership entailed high political stakes: it was mediated by the Spanish Nuclear Energy Board (*Junta de Energía Nuclear*, JEN), a governmental agency created in 1951 and presided by Franco's head of general staff and Minister of Air Force, Juan Vigón Suerodíaz (1880–1955). Vigón was succeeded by Navy engineer José María Otero Navascués (1907–1983), a key figure in the reconfiguration of physics in the decades following the Civil War (1936–1939), as the regime sought to enlist modern science in the construction of a totalitarian state based on a national-catholic tradition.[6] Physics and the nuclear sciences became a tool of international legitimation and national pride under military control. What effect, if any, did this reactionary modernist background have on Spain's first stage at CERN? I will argue that Spanish diplomats, together with scientists qua policy makers such as Otero, did not lack political will, but dealt with CERN in terms of national quotas while paradoxically acknowledging this was not the way the organization was run. Ultimately, their institutional and (in Otero's case) personal strategy, rooted in the autarchic policies of early Francoism, did not pay off with CERN the way it had with other international organisms.[7]

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the mid-1960s […] As for Spain, four groups from Valencia, Madrid, Seville, and Saragossa collaborated at CERN in the emulsion and bubble chamber groups"; and John Krige, ed., *History of CERN. Vol. 3* (Amsterdam; Oxford: Elsevier, 1996), p. 70.

[3] Pedro Pascual de Sans, "Los inicios de la Física de Altas Energías en España," *Arbor* 159, no. 626 (1998): 231–39, on p. 234; Manuel Aguilar Benítez and Francisco J. Ynduráin, "El CERN y la Física de altas energías en España," *Revista Española de Física* 17, no. 3 (2009): 17–25, on p. 19.

[4] Ana Romero de Pablos and José Manuel Sánchez Ron, *Energía nuclear en España: de la JEN al CIEMAT* (Madrid: CIEMAT, 2001), p. 203–226; see also Carles Gámez Pérez, "El Grupo Interuniversitario de Física Teórica (GIFT). Génesis y desarrollo histórico, 1968-1976" (MPhil dissertation, Universitat Autònoma de Barcelona, 2004).

[5] On the cultural assumptions of Spanish physicists in the first decades of the century, see Thomas F. Glick, "Dictating to the Dictator: Augustus Trowbridge, the Rockefeller Foundation, and the Support of Physics in Spain, 1923–1927," *Minerva: Review of Science, Learning and Policy* 43, no. 2 (2005): 121–45; *cf*. Sharon Traweek, *Beamtimes and Lifetimes. The World of High-Energy Physicists* (Cambridge, MA: Harvard University Press).

[6] On science and Francoism, see María Jesús Santesmases, "Neutralidades y atrasos: Ciencias y tecnicismo en la España de Franco," in *Actes de la VII Trobada d'Història de la Ciència i de la Tècnica*, eds. Josep Batlló Ortiz, Roser Puig Aguilar and Pasqual Bernat López (Barcelona: SCHCT, 2003), p. 63–78; Antoni Malet, "Las primeras décadas del CSIC: investigación y ciencia para el franquismo," in Ana Romero de Pablos and María Jesús Santesmases, eds. *Cien años de política científica en España* (Bilbao: Fundación BBVA, 2008), p. 211–56; "José María Albareda (1902–1966) and the formation of the Spanish Consejo Superior de Investigaciones Científicas," *Annals of Science* 66, no. 3 (2009): 307–32; Lino Camprubí Bueno, "Los estándares como instrumentos políticos: ciencia y Estado franquista a finales de los años cincuenta," *Empiria*, no. 18 (2009): 85–114; "One Grain, One Nation: Rice Genetics and the Corporate State in Early Francoist Spain (1939–1952)," *Historical Studies in the Natural Sciences* 40, no. 4 (2010): 499–531. General works include Luis Sanz Menéndez, *Estado, ciencia y tecnología en España, 1939–1997* (Madrid: Alianza, 1997), Jaume Claret Miranda, *El atroz desmoche. La destrucción de la universidad española por el franquismo, 1936–1945* (Barcelona: Crítica, 2006), and Amparo Gómez and Antonio F. Canales, eds. *Ciencia y fascismos. La ciencia española de posguerra* (Barcelona: Laertes, 2009).

[7] For a recent synthesis of the literature on Francoism, see Borja de Riquer, *La dictadura de Franco* (Barcelona: Crítica; Madrid: Marcial Pons, 2010). I am elsewhere building an argument on the seizure of a modern scientific discipline such as physics by a reactionary regime, *cf*. Jeffrey Herf, *Reactionary modernism: Technology, culture, and politics in Weimar and the Third Reich* (Cambridge: Cambridge University Press, 1986).

Costs: the contribution of Spain

Spain's contribution to CERN hovered above 50 million peseta (some 2.7 million Swiss franc, around 4 % of the organization's annual budget) between 1961–63. Spain had also paid an initial special contribution of 2.7 million Swiss franc, down from 6.9 million (a 61.2% rebate).[8] In 1963, the Spanish Delegation submitted a proposal to mend the method of calculating the contribution of member states, to take into account the national income per capita, rather than gross national product, and the stage of scientific and industrial development of each member. This prompted a revision of the criteria used as a basis for the calculation of contributions, which were compared with the criteria of other intergovernmental organizations. However, international statistics were "very incomplete" and had not been collected on a comparable basis; moreover, some doubts persisted "about the exact meaning of 'scientific research'".[9] The Finance Committee of CERN discussed in great detail the economic situation of Spain, building on data gathered from the International Monetary Fund and the United Nations, and complementary information on the expenditure on education and research supplied by the Spanish government. The Committee noted that "the first comprehensive study of research expenditure in Spain was made in 1963 in connection with the preparation of the Development Plan" and that there were "no comparable data for previous years". According to the figures provided by the Spanish government, the expenditure on research and development was 1,513.8 million peseta, which were very unevenly distributed between basic research (164 million, 10.8%), applied research (719 million, 47.5%), and development (630.8 million, 41.7%). The distribution by scientific and economic sectors showed the importance granted to Nuclear Energy, Biological and Agricultural Research, and Geology and Mining, which together got 712 out of 1,194 million peseta (2/3 of the expenditure).[10] In December 1963, the CERN Council granted reductions in the Spanish contribution of 50% in 1964, 35% in 1965, and 20% in 1966.[11]

In 1966 Spain asked for a 35 per cent reduction of its contribution over the next triennium, arguing that it would otherwise contribute 7.1 per cent of the country's research and development annual budget (some 35 million dollar, or 0.2 % of its GDP) to CERN alone. The Finance Committee was not willing to adopt a formula to calculate reductions on the basis of national income per capita, such as the UN formula $1/2 \cdot (1000 - x)/1000 \cdot 100$ —where x stands for average national income in US dollars, and which results in 28.2% for $x = 436$ USD, Spain's average income for 1962–64— and instead recommended to follow an ad hoc procedure. The Council finally approved a reduction of 20% for the triennium 1967–69, but even this proved untenable, and in a matter of months Spain was asking for a 28% reduction, implicitly referring to the UN formula.[12] In the final round of negotiations one year later, after Spain had notified its intention to withdraw from the organization unless a "substantial reduction" for the coming five years was granted, the Council decided to accord a reduction of 50 % in the Spanish contribution for the next three years.[13]

While existing accounts suggest withdrawal was a clear-cut decision, the CERN archives actually show it was a protracted affaire. In December 1968 Spain asked the notice of withdrawal to be kept in abeyance and "to remain 'de jure' a member of the Organization, whilst accepting not to exercise her rights". The letter reached CERN too late to be discussed at the 40th Session of the Council, which received it "with great satisfaction", and a decision was postponed until the 41st Session; in March 1969, rising Spanish theoretical physicist Pedro Pascual was "quite optimistic" the situation could be reversed. But a couple of weeks before the Council session of June 1969, Spain communicated its final decision to withdraw with effect from 31 Dec 1968 "due solely to economic and financial reasons".[14]

Benefits: staff and contracts

Contribution meant little in itself: beyond its huge symbolic value, the cost of belonging to Europe's leading scientific organization had to be gauged against tangible benefits, including staff and contracts.

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8 33rd Meeting of the Finance Committee, 13 Jun 1960. Special Contribution from Spain (CERN-FC-430).

9 José Manuel Aniel-Quiroga to V. F. Weisskopf, 6 Jun 1963; 50th Meeting of the Finance Committee, 26 Feb 1963 (CERN-FC-602).

10 55th Meeting of the Finance Committe, 13 Nov 1963 (CERN-FC-639-Rev); quote on p. 4.

11 33rd Session of the Council, 14 and 15 Dec 1963, "The Contribution of Spain" (DIR-ADM-FIN-06 (3) File 2); Romero and Sánchez Ron, *De la JEN al CIEMAT*, p. 218.

12 79th Meeting of the Finance Committee, 18 Nov 1966 (CERN-FC-889); 80th Meeting of the Finance Committee, 13 Dec 1966 (CERN-FC-907); 33rd Session of the Council, 14–15 Dec 1966 (CERN-676); 35th Sessions of the Council, 21–22 Sep 1967 (CERN-735).

13 40th Session of the Council, 18–19 Dec 1968 (CERN-839 and CERN-843).

14 40th Session of the Council, 18–19 Dec 1968, "Summary of Conclusions", p. 4 (CERN-855); P. Pascual to B. Gregory, 4 Mar 1969 (DIR-ADM-FIN-06 (3) File 3); 41st Sessions of the Council, 19–20 Jun 1969 (CERN-880).

Used to share in international organizations as a national representative, Otero thought that Spanish physicists and engineers had to be hired on the principal basis that Spain was a member of CERN.[15] His views run against the organization's policy, which placed professional skill and scientific achievements over nationality. Tensions came to the fore quite soon, and they paradoxically involved CERN's first and higher-rank Spanish staff physicist, Rafael Armenteros (1922–2004), an Spanish exile who had graduated in Physics at Imperial College in 1946. In December 1960, just a few days before Spain became an official member, Armenteros was offered a job at CERN. The problem with Armenteros was not just his Republican past, but also that Spain wanted to have a saying in the recruitment of her nationals. The *Delegado Permanente de España cerca de las Organizaciones Internacionales*, José Manuel Aniel-Quiroga, even suggested that staff members whom they did not regard as Spaniards (i.e. holders of a valid passport) should be listed as 'stateless', clearly referring to Armenteros.[16]

Shortly after joining the organization, and following a suggestion by the Director-General, J. B. Adams, that two or three physicists visit CERN "to study the work going on at our Laboratory and the way in which other Member States collaborate with us", Otero set up a delegation "to discuss the possibilities for Spanish personnel to join the CERN".[17] CERN had meanwhile advertised posts for engineers, physicists, mathematicians and operators in the Spanish press; even though the Spanish Ministry of Work had approved the insets, Otero was not pleased that the JEN had not been told, and asked CERN's president Jean Willems not to take any decision until the Spanish commission had visited CERN.[18] In his report about the visit by the Spanish delegation, Samuel A. Dakin, CERN'S Directorate Member for Administration, noted that "we had none of the anticipated trouble, in the sense that they did not take up the question of methods of recruitment, nor make any mention of Armenteros, except to inquire whether he was yet in post. But they had obviously come with an extremely clear and firm idea as to what Spain was going to get out of CERN, which was at this stage a suitable training for as large a number of her scientists as possible… They reiterated these suggestions after they had made a round of Divisions, when apparently they found a good deal of interest in some of their possible candidates, I presume mainly on the assumption by the people they spoke to that the Spaniards would be fellows or would come outside existing staff ceilings… We promised them nothing, emphasized the competition for fellow and staff posts, but said that we would do what we could to meet the situation".[19]

Later that year Otero expressed his views that Spain's "economic sacrifice" should be rewarded with a group of researchers belonging to CERN's staff, and put forward a senior Spanish physicist for a permanent position. Director-General John B. Adams told Willems that the candidate's experience was "not sufficient in high energy physics to justify a grade which would be acceptable to him", and instead offered a "visiting scientist" position although "he hardly qualifies scientifically for this position". His judgement was based on an appraisal by G. Bernardini: "[the candidate's] papers show a standard physicist. Some of them are approaching technical problems rather lightly, others give the impression of a disproportion between amount of work and data and conclusions derived from them". Adams thought a point could be stretched in this case as long as it was not taken as a precedent, and Willems' final answer was very carefully worded so as to make the post appealing to both Otero and his man. I take this exchange to be extraordinarily revealing of the rather different views on recruitment and scientific excellence held by Otero and CERN's scientists and officials.[20]

The issue was taken up over and over again in the following years, both parts being concerned about the "disproportionately small" number of Spanish staff at CERN. Over the first 18 months of membership, about 40 Spanish candidates were called to selection boards, but the JEN's focus on applied nuclear sciences appears to have backfired, because "although many of the Spanish candidates have experience in nuclear physics or engineering, this is very commonly in reactor work which has little application to the sort of work we do here". By October 1962, however, there were eight Spanish staff members, including 4 scientists or engineers.[21] By mid-1963 the situation of Spain was not unlike that of Sweden, a founding member of the organization, which contributed 4.18 % of the budget (as compared with Spain's 3.36 %)

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[15] Between 1950–66, Otero presided over the Spanish National Council for Physics (*Consejo Nacional de Física*, CNF) that sought to coordinate physical research institutions. He enjoyed international recognition as national representative of Spain in organisms such as the International Union of Pure and Applied Physics (IUPAP), the International Committee on Weights and Measures (CIPM, which he presided between 1968–76), and the International Atomic Energy Agency (IAEA, whose General Conference he presided in 1971). See the hagiographical memoir by Leonardo Villena, "José María Otero Navascués (1907–1983)", *Óptica Pura y Aplicada* 17 (1984): 1–12.

[16] Memorandum by S. A. Dakin of talk with J. M. Aniel-Quiroga, 10 Jan 1961 (DIR-ADM-PERS-02).

[17] Otero to J. B. Adams, 18 Jan 1961 and 11 Mar 1961 (DIR-ADM-PERS-02).

[18] J. M. Otero to J. Willems, 3 Mar 1961 (DIR-ADM-PERS-02).

[19] S. A. Dakin, "Note of the Talk with the Spaniards", 30 Mar 1961 (DIR-ADM-PERS-02).

[20] See Otero to Willems, 15 Jun 1961; Adams to Willems, 29 Jun 1961; Bernardini to Adams, 29 June 1961 (DIR-ADM-PERS-02).

[21] Dakin to Otero, 3 Oct 1962 (DIR-ADM-PERS-02). Staff members included R. Armenteros (Senior Physicist grade 13); D. García Fresca (Senior Engineer grade 11); A. García González (Mechanical Engineer grade 9); and J. Goñi Unzué (Physicist grade 9). See also Otero to Weisskopf, 5 Sep 1962 (DIR-ADM-PERS-02).

and had 13 members of staff, including so-called supernumeraries (as compared with Spain's 16).[22] Spain tried all along, with limited success, to retain some control of the recruitment process, having potential candidates send their CVs to the Spanish Ministry of Foreign Affairs or the Spanish Institute for Emigration, rather than to the organization itself, as was usually the case.[23]

Together with staff underrepresentation, the lack of contracts with Spanish companies played also a role in the decision to withdraw from CERN. How were these seen from the inside, as far as CERN sources allow us to tell? "Failure in communications" between CERN and Spanish industry was suggested by S. Dakin and rejected by Otero.[24] The results of Spanish firms were certainly disappointing: by 1965 they had been awarded contracts for 25,545 Swiss franc, a negligible percentage of the 452 million Swiss franc total of contracts awarded between 1952–65. However, firms in other countries did not fare much better. CERN's policy was based on "competitive tendering" (whereby firms were invited to submit offers), rather than "just return", but there was a gulf between principle and practice: not all contracts were competitive, and submitting an offer was a costly process that was not compensated if the firm failed to win the contract. As J. Krige has shown, the process was not completely rational nor objective, but it was maintained mostly for political reasons, as it represented "an agreement between the member states that no consistent effort be made to distribute contracts between them in relation to their contributions to the budget. The *practical implications* of the 'competitive' policy are thus best understood *negatively*, as meaning that there is no policy of just return in the award of contracts for plan, equipment, and supplies".[25]

Conclusion

Sources at CERN provide a new perspective on Spain's first membership of the organization. Political will was not lacking, but was rather rooted in autarchic practices and attitudes. National representation had granted Spain a place in many an international organism and provided a Navy engineer such as Otero with scientific credentials. Quotas had effectively worked in many instances and would work with the European Space Research Organization (ESRO), which Spain nearly left at about the same time; but they did not work with CERN, which was run on different principles. Otero's loss of influence within the Spanish R+D system by the mid-1960s, as new governmental agencies were created and young, competent physicists were occupying university chairs, may also have played a role.

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[22] "Ingenieurs et physiciens espagnols au CERN", 2 Aug 1963; List of staff 1961–64 (DIR-ADM-PERS-02. Swedish staff had higher grades.

[23] José A. Giménez-Arnau (Spanish ambassador in Geneve) to G. H. Hampton, 10 May 1965; Hampton to Giménez-Arnau, 24 May 1965 (CERN archives, DIR-ADM-PE-02).

[24] Dakin to Otero, 3 October 1962 (DIR-ADM-PERS-02); Otero to Dakin, 23 Oct 1962 (DIR-ADM-FIN-06 (3) File 1).

[25] John Krige, "The contract policy with industry", in Armin Hermann, John Krige, Ulrike Mersits and Dominique Pestre, *History of CERN. Vol. 2, Building and running the laboratory* (Amsterdam; Oxford: North-Holland, 1990), p. 637–677, see Table 11.3 on p. 668, and quote on p. 671; see also the preliminary publication "Which firms got CERN contracts?", *Studies in CERN History* (CHS-24), Geneva, 1987.

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BERTRAND GOLDSCHMIDT AND ATOMIC TECHNO-SCIENTIFIC DEVELOPMENT IN POSTWAR FRANCE

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Abstract

This paper considers the role of biography in the writing of the history of the physical sciences in the postwar, and in particular the history of techno-scientific institutions. It focuses on radiochemist Bertrand Goldschmidt, who played an important role in determining the nature of the Commissariat à l'Energie Atomique's development despite the fact that he was not a policy maker. Rather, as a radiochemist who chanced to specialize in the chemistry of plutonium, he discovered several scientific and non-scientific means to encourage its production in France, and thus influence the general military orientation of the French nuclear program. Goldschmidt was a prolific memoirist and participant-historian, and a look at his writings reveals a noticeably supple view of the postwar nuclear world, despite the narrowness of his technical specialty and the nationalist orientation of the CEA. A similarly broad view is exhibited in the writings and interviews of Goldschmidt's fellow CEA chemist, Jules Guéron. However, for several reasons, Guéron's interests did not influence the CEA's development as did Goldschmidt's, and in fact Guéron left the CEA to become Euratom's first director of general research. This paper suggests that biographical studies incorporating oral histories alongside institutional archives can present a means for framing the development of postwar techno-scientific institutions, and are interesting stories in and of themselves.

Introduction

This paper concerns some aspects of the life, work, and views of Bertrand Goldschmidt, the French radiochemist born in 1912, 33 years old when World War II ended. It explores the role of biography in the writing of the history of the postwar physical sciences. This role is an interesting and complicated one when considered in the light of politically sensitive techno-scientific systems, especially those involving nuclear energy. And this role becomes all the more interesting when one discovers that the actor in question was publicly pronouncing on the state of affairs in the field.

The present study focuses in particular in the earliest postwar years of Goldschmidt's scientific life, tracing aspects of it to the early 1950s, when Goldschmidt became involved in encouraging a French atomic bomb program. Goldschmidt's story is representative of the times. He was a living example of the global reach and nature of postwar atomic science. In the first six months of 1946 alone, on missions related to science, he was all over the map: Chalk River and Ottawa; London and Paris; Washington, New York and San Francisco; the Bikini Atoll. And, like many other postwar scientists, he played a remarkable number of roles (which he for one eagerly sought): radiochemist, engineer, manager, diplomat, government advisor, public educator and propagandist, political observer, a probe to gather as well as give away information. He shared a sense with many other scientists in the atomic field that there were closely associated fundamental shifts in science, technology, politics, and war that could best be understood from a historical perspective and with a scientific background.

Goldschmidt's most important role of all: plutonium specialist, *the* French plutonium specialist. One should briefly review Goldschmidt's background to see how he became one. From a wealthy Parisian Jewish family whose eldest son entered the Ecole Polytechnique, Bertrand Goldschmidt himself entered the Ecole de Physique et Chimie Industrielle (EPCI) in Paris, in 1930. From there, like EPCI graduate Frédéric Joliot, Goldschmidt went not into industry, but to the Radium Institute. He was Marie Curie's last personal assistant.

Goldschmidt completed his doctoral thesis in 1939, and began to teach in the Paris science faculty. After the German occupation of Paris, he managed to get to the unoccupied zone, and then in 1941 escaped France for the US. Eventually, it was arranged for him to join the British Department of Scientific and Industrial Research. It was under these auspices that in July 1942 he was sent to Chicago, to the Met Lab's Chemistry Section, where Glen Seaborg's team was studying the chemistry of plutonium. According to Seaborg's diary, Goldschmidt quickly fit in and was invited into the leadership circle planning laboratory activities. By the end of October, however, Goldschmidt was gone, sent out of the American facility, eventually to join a new Anglo-Canadian team in Montreal. It was in this team, in 1945, that he carried out his first independent work on plutonium: the discovery of a new method of plutonium separation involving the solvent triglycoldichloride.

By the end of 1945, the Anglo-Canadian team had managed to complete an experimental heavy-water reactor and to launch the construction of Canada's first large-scale reactor at Chalk River. Heading back to France from it were not only Goldschmidt, but also Jules Guéron and physicist Lew Kowarski, all deeply involved in its activities, and profoundly changed by their wartime experiences. From the first days of the French atomic program, they earned the ironic label "Canadians", a historic recognition of the techno-scientific expertise they were bringing from the New World to the Old. It should also be noted that three of the French scientists in Montreal, Guéron, Goldschmidt, and Pierre Auger, broke oaths of secrecy to the Anglo-Allies in order to inform Charles de Gaulle about the top secret American bomb project. They were concerned that De Gaulle knew what his British and American allies were up to in order to deal with them rationally; and, a more deeply felt belief, they were convinced that the bomb, and other atomic technologies, were of such importance that the most significant Frenchman of the moment ought to be prepared to see his own country's entry into the atomic world as soon as the opportunity allowed.

Finally, it must be noted that Goldschmidt's view of the new atomic world was as informed by the events of 1946 as by the war. Goldschmidt was repeatedly distracted from his duties in France's Commissariat à l'Energie Atomique (CEA) by his service as a technical advisor to the French United Nations delegation involved in the forlorn attempt to negotiate an international agreement on atomic energy. And in May, he learned that he had been appointed to serve as a French observer at the Bikini atomic tests, taking him away from France for more than two months.

By the beginning of the following year, he had a fully-formed view of the atomic world that he would elaborate over the rest of the decade.

Goldschmidt's atomic worldview

He conveyed his view in public lectures, which he gave to many audiences with evident pleasure for years to come. His message consisted of several overlapping elements, familiar to historians: the themes of the Federation of American Scientists, "no secret, no defense, international". But his own message was not merely an echo of theirs. It contained skillfully chosen details; it was personal; and it sought to describe a view of the atomic world from France, more often than not for French audiences.

It began with the atomic bomb, and its destructive power. Some of what Goldschmidt relied on were popular explanatory devices, like the devastation of Paris by a hypothetical atomic bomb exploded above the Place de la Concorde.[1] He also stated with eloquence the consequences of an atomic war, where atomic bombs might (this comes from 1949) "wipe out…in one blow our cities, our centers full of human lives, from which originate and return all of the links that at present organize our country".

We should not underestimate the effect that witnessing the Bikini tests had on Goldschmidt, even if he did not frequently describe the blasts themselves in his public lectures. When he did, the turn of phrase could be dramatic, and pointed. About the second, underwater explosion, he said to Université radiophonique international listeners in 1949: "The

[1] Goldschmidt, "L'Arme atomique," Talk given at the Sorbonne, May 17, 1946, 8-9. Manuscripts for all talks cited can be found in the archives of the CEA, DRI/B. Goldschmidt M4.13.37, M4.13.38, & M4.13.40. Due to restrictions in the length of this publication, certain details are not cited; please contact the author for page numbers, etc.

water falling back down on the ships from two kilometers high was so full of radioactive particles that the ships would have become veritable radioactive crematory ovens for the crews, had they been there." The immediate military consequences were clear: a D-Day amphibious landing was no longer possible. When he said that in the atomic age naval warfare must be dominated by the submarine, he could easily have had images of the shattered surface fleet at Bikini in his mind.

The essence of the situation as Goldschmidt came to describe it was more than just that "the atomic weapon must render…armies, navies or fortifications outdated", but that it must do so to "war itself". All orthodox military defenses were hopeless (May 17, 1946, Sorbonne.). Only dispersion of forces, and in fact of entire populations, would counter the effects of atomic weapons (Ecole Normale d'Administration, July 8, 1947.). And that was a political act, making the discovery of fission, not really a military, but a political phenomenon, "the most important political factor of our day leaving the laboratory for the foreign departments of all great nations". (Columbia University, May 1947.) Goldschmidt often qualified his remarks as those of the non-specialist: To an audience at the Ecole Normale d'Administration he apologized, "you will excuse me for presenting to you my personal views, that is, those of a chemist and not of a specialist in foreign affairs." The qualification gave him neutral ground to make public statements about global politics. At the same time, his expertise as an atomic scientist gave him the technical understanding necessary to understand the new technological forces influencing the globe, and in his view one must start with the former to get to the latter. And, over and over, for years, the message he chose to deliver was a fundamentally political one, best summed up by the title of the book he proposed to the CEA be translated into French: *One World Or None* (in his rendering: "Le monde sera unifié, ou ne sera pas."—ENA, February 16, 1948). Perhaps it is not surprising that Goldschmidt admitted a great deal of admiration for the Acheson-Lilienthal report, and told an American audience that the French atomic scientists around him did as well.

One thing Goldschmidt told all audiences was that when it came to the bomb, there was no secrecy. Early on, this came with rather clichéd phrasing: "There is no secret about the atomic bomb. The fundamental scientific facts were published and known since 1939; we all know the biggest secret, that the bomb works" (Sorbonne, May 17, 1946.). But the examples with which Goldschmidt illustrated the point indicate both the ingenuity of his argument against secrecy and the personal scientific experience he was drawing on. Goldschmidt noted when in the American Senate the question of secrecy was raised, one witness testified that, when the Americans cut off information to the British, their team in Canada developed a simpler, more complete method of plutonium separation —Goldschmidt's triglycoldichloride. Goldschmidt's view was that the only secrets to speak of were industrial secrets that enterprising countries could arrive at sooner or later (ENA, July 8, 1947).

Just as he believed the American policy of secrecy was mistaken in its conception, he also assumed that it would not work, and sooner or later the Russians would build their own bomb. The first instance of him offering a prediction is from July 1947, at the Ecole Nationale d'Administration.: "in five years perhaps, in fifteen or twenty years surely…" In his view, the American invention had given them a dramatic advantage in world affairs at the end of World War II; the Russian acquisition of the bomb would not lead to equilibrium, but rather a Russian advantage, due to the vulnerability of America's urban concentrations.

As for France, from the summer 1946, Goldschmidt could report that at the UN the French had declared the peaceful intentions of its new atomic program. CEA representatives, Goldschmidt included, qualified that declaration by putting it in the context of the CEA's own meager resources, inadequate for a gigantic atomic bomb program *a l'américaine*. The CEA's interest was, Goldschmidt said, in the production of electricity. He explained to energy-rich Americans at Columbia University in 1947 that in France it would be a blessing to have atomic energy "in regions where the lack of coal is so bitterly felt". Goldschmidt also often mentioned radioisotopes, and even alluded to using radiation present in reactors for genetic modification (Société des Ingénieurs Civils, March 18, 1949).

This view of the atomic world —Goldschmidt's understanding of No Secret, No Defense, International, and even his enunciation of France's declaration before the UN, did not automatically mitigate the possibility of French atomic weapons. To the contrary: in the right circumstances, it could lead the plutonium specialist to consider them the best option for his country.

One must take a moment to consider why Goldschmidt spoke out his view of atomic energy and its consequences to so many different audiences —from France, to Canada, to the US, from university lecture halls to military colleges and civil engineering societies. First, it was his job. Since in particular he was one of the few CEA scientists who had been involved in atomic projects in North America, and since he was one of the three of them working full-time for the CEA (the others being Kowarski and Guéron), he was called upon by various French institutions and civic groups to introduce them to atomic science, technology, and politics. This was especially so after Bikini.

But perhaps the most important reason of all why Goldschmidt took up, and never abandoned, public explication of his view of the atomic world, becomes clear when we examine what he said in 1949 —the year a CEA team led by him purified several milligrams of plutonium. It was in plutonium that Goldschmidt found his strongest public identity and voice. And he seems to have derived great enjoyment from explaining his view of why plutonium was interesting, and why it mattered.

Goldschmidt could not resist describing the incredible difficulties involved in the industrial-scale separation of plutonium. In this Goldschmidt was not just a self-identified plutonium specialist, interested in its industrial-scale production. He was an industrial alchemist. Even before any plutonium had been separated in France, he announced on French radio, "We have entered the era of industrial-scale transmutations" (French radio, March 5, 1949.). A few months later, when the time came to announce his team's success in such transmutations, he returned to the radio to answer a reporter's questions. Goldschmidt's response to a question about plutonium's importance and interest mixed the historical and practical:

> Plutonium's interest resides in the fact that it is a new element that does not exist in nature, and is a creation of man and of the 20th century…..plutonium is a nuclear fuel far more concentrated than uranium and will be the fuel used in future energy-producing atomic machines.

Goldschmidt then told his interviewer that his team of radiochemists had become "the first French alchemists" (French radio, November 23, 1949).

The product of the CEA's alchemy was not gold, but no matter. According to Goldschmidt, it would be the future measure of wealth and power. His March 5, 1949 rendering deserves to be quoted in full:

> Although the first element produced in quantity by modern alchemy might not be gold, this new element, plutonium, will be for the next few decades the sign of power and wealth of the states who possess it, at least if nations are based on a unified world, before the menace this same plutonium represents (French radio, March 5, 1949).

Goldschmidt, then, could judge his own importance to his native country in the light of its plutonium production.[2] He told his radio audience that the ease and speed with which countries could develop "a substantial breeding program" would be the principal markers of atomic energy development (French radio, March 5, 1949). And, he said, the success of such a program depended on the percentage of plutonium recovered from fission products (Société des Ingénieurs Civils, March 18, 1949). His technical roles in the French program were bookends to the entire system. Goldschmidt was responsible for overseeing the production of metallic uranium for reactors; and he was responsible for maximizing the efficiency of the plutonium recovery process.

The uses of plutonium

But for what would France use the plutonium Goldschmidt was so intent on creating?

In 1950-51, the CEA entered a period of crisis. In April 1950, the government dismissed High Commissioner Frédéric Joliot-Curie. Then, in August 1951, his administrative counterpart, Administrator General Raoul Dautry, died suddenly. The CEA was leaderless and rudderless, and its development and early postwar expansion were in doubt.

The crisis in the CEA, along with the Soviet bomb, led to detectable changes in Goldschmidt's assessment of the atomic world. Goldschmidt began to describe the duality of the French atomic program. He had done this implicitly before, in discussions of plutonium. He noted in 1947 that despite the short-sightedness and apparent avarice of the American secrecy policy, the US should not be accused of "extreme egotism", because, after all "nuclear fuel can serve indifferently to produce energy or make bombs" (ENA, July 8, 1947). Now he could apply the point to the French program, which he did on March 13, 1951, significantly at the Institut des Hautes Etudes de Défense Nationale:

> [O]ur Châtillon pile of a few kilowatts, which produces a few milligrams of plutonium per day, and the one at Saclay which will be about a thousand kilowatts and consequently produce about a gram of plutonium a day, are the indispensable steps to the study and construction of a powerful pile, which might be for military or peaceful goals. The plutonium that we isolated at the factory at Le Bouchet is certainly a nuclear explosive, it is peaceful only by its weight, currently 10 milligrams.

To this Goldschmidt added a pithy historical observation about technological development. Twelve years after the discovery of fission, tens of thousands had been killed, entire cities and fleets of retired warships destroyed, and hundreds of international meetings had been held, but atomic energy "has not yet turned the slightest steam turbine, the littlest electric motor".

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[2] Notably, Goldschmidt categorized not only nations but reactors according to plutonium. Experimental piles produced quantities of plutonium on the order of a milligram a day; "medium power" piles made a gram of plutonium a day; and "high power" piles made hundreds of grams a day. To be fair, Goldschmidt added kilowatts to plutonium production: experimental piles approximately a kilowatt, medium-power piles about a thousand kilowatts, and high-power piles hundreds of thousands of kilowatts. Nevertheless, he is the first observer I have come across who classified *all* reactors according to their potential plutonium production (Société des Ingénieurs Civils, March 4, 1949).

At the same time, Goldschmidt also continued to qualify France's aging declaration before the UN, with ever more contortions: "we announced officially in 1946…that we did not have the intention to participate in the atomic arms race from the military point of view, because even if we had the intention to do so, it would not have been very sensible since our effort would only have been 1 to 1.5 percent of the American effort." And he remarked that the British, whose ambitions to produce electric power he admired, and whose program was on a much smaller scale than the American one, were now likely to make a bomb (ENA, October 27, 1951).

Proliferation worried Goldschmidt. It was not just the Russian bomb, though that was worrisome enough. He had already established that any country without a bomb was at the mercy of those who had it: "The factors that hitherto served to establish the hierarchy of military powers (strategic situation, industrial potential, number of men) must now be considered according to the possession or non-possession of the atomic weapon. The only security that exists for less powerful peoples is the will of those who have this weapon." In addition, a country with a hundred bombs could knock out one with a thousand —and any country, overexcited and trigger-happy, could launch a bomb that could start a general war (Sorbonne, May 17, 1946). And as Goldschmidt noted later, one might not even know where the bomb had come from!

Goldschmidt admitted that he could not predict how the French atomic program would fare. He expressed enthusiasm for a European collaboration if France led it; but that collaboration had not been forthcoming. And he admitted as well that he was not sure how successful France's efforts at uranium prospection would be (ENA, October 27, 1951). In Goldschmidt's view, there was increasing uncertainty in the atomic world, and in atomic France.

At just this time, the most important change in the history of the CEA was set in motion. The death of Dautry had created a vacuum that was energetically filled by the young Secretary of State for Atomic Energy, Félix Gaillard. At the beginning of September, Gaillard ordered the CEA's steering committee to formulate a five-year plan that would necessitate vastly increased budgets: the CEA must in a concerted fashion commit itself to a major, long-term goal. By the end of October the committee had the draft of a plan. Its defining feature: an output of kilogram quantities of plutonium by the end of five years.

The plan could hardly have been conceived without the presence in the CEA of Goldschmidt, and over the following four years, Goldschmidt performed several tasks essential to carrying the five-year plan out: identifying the best solvent for the extraction of plutonium (tributyl phosphate), supervising the construction of the plutonium purification pilot plant (by mid-1954 it was treating dozens of irradiated slugs), and choosing the private enterprise for the construction of the full-scale plant (Saint-Gobain). Goldschmidt stepped aside only at the construction of the full-scale plant itself.

The CEA's five-year plan was a fulfillment of Goldschmidt's plutonium ambitions. But Goldschmidt did not limit his interest in plutonium to simply seeing to it that it be produced. He did all he could in his position to make certain it served as fuel in a French atomic bomb. This included providing estimates for the CEA of the expected time and cost of bomb-grade plutonium; sitting on secret bomb program steering committees; and keeping the military at arm's length when they threatened to intrude upon the CEA's own effort to create a bomb program.

His interests as a chemist-engineer-alchemist alone do not account for why he did this. We must make note of his view of the atomic world, and especially the changes it underwent in 1950-1951, the uncertainty mentioned above. The view Goldschmidt came to elaborate suggested that France was better off with the bomb than without it, and that the bomb was indeed within France's grasp, given time. This does not mean that his support of a bomb program was due entirely to this view. A skeptical observer might note that the new administrator general of the CEA, Dautry's replacement, wanted a bomb program; Goldschmidt could best advance his own career, by following in step. And perhaps this is one reason behind Goldschmidt's encouragement of a bomb program. But it is not, I think, the most important.

Conclusion

This brings us back to the question of scientific biography in the postwar.

This story illustrates one argument made for scientific biography in Thomas Hankins famous 1979 article on the subject: Goldschmidt's life appears to be an excellent 'unit' for studying science in certain contexts, especially the technological and global political ones. And it is a particularly apt example for the study of postwar *nuclear* science, especially as a means of organizing a narrative, of a career trajectory and a life before the public. But there is a need for caution. I do not wish to suggest that, as far as the French atomic bomb is concerned, Goldschmidt was The Indispensable Man. And I do not wish to exaggerate Goldschmidt's sense of individual identity; here we see Goldschmidt's public face in particular. And we should always bear in mind that he was part of the CEA. Its identity was his just as he contributed to its.

I should allow Goldschmidt himself a chance to finish. In 1957 he wrote his own brief biography (in the third person), which he supplied to the NATO Defense College where he was to lecture on international atomic affairs. In it, he listed his roles in the CEA at that time: "Director in Charge of External Relations", "French representative on the consultative committee responsible for making preparations for the second atoms-for-peace conference". And, Director of the Chemistry Department: "He is now Director of the Chemistry Department, and is responsible for the chemical cycle of uranium, the preparation of artificial radio-elements, and also, more especially, the extraction of plutonium, that difficult problem for the new modern alchemist." If nuclear histories often juxtapose geopolitics with the minutiae of national technological development, here is a way not just to tie the threads together, but to witness them fused in the form of an influential actor.

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BRUNO TOUSCHEK AND THE BIRTH OF ELECTRON-POSITRON COLLISIONS AFTER WORLD WAR II[1]

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Abstract

Bruno Touschek, born in Vienna in 1921, graduated in physics in 1946 in Göttingen. Among his mentors there were leading scientists of pre-war physics, with whom he collaborated in maintaining alive scientific research during the war. In participating to the reconstruction of physics in Europe, he performed a crucial role in the development of post-war theoretical physics in Italy since mid 1950s. The experience established in participating with Rolf Wideröe to the building of the first European betatron in Hamburg during the war, and in collaborating to the designing and construction of a synchrotron in Glasgow during the late 1940s, as well as his deep knowledge of Electrodynamics, culminated, fifty years ago, in Touschek's proposal to construct a small storage ring for electrons and positrons in the early Spring of 1960, and, after a few months, the much bigger ADONE. The events which led to the approval and construction of AdA, the first matter-antimatter collider, in Frascati National Laboratories near Rome, and the Franco-Italian collaboration which confirmed the feasibility of electron-positron storage rings are recalled.
All this showed that electron-positron colliders could compete with traditional proton machines, proved the feasibility of a new physics and opened the way to higher energy and luminosity. The road to matter-antimatter collisions started in war ravaged Germany and culminated on November 11th, 1974, when the discovery of the J/Psi was simultaneously announced by Burton Richter and Sam Ting. It was confirmed three days later by ADONE, the Frascati Italian accelerator, and signaled the beginning of a new era in particle physics which eventually led to some of the most important discoveries of the Standard model.
Touschek's subsequent work on infrared radiative corrections as needed to extract meaningful physics from high energy electron experiments and his contribution to educate a generation of theoretical physicists to the importance and technical proficiency in resuming light quanta, has also been described. In order to understand

[1] Full article at:
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Bruno Touschek's role in the reconstruction and development of post-War European physics, we also outline his formation both as a theoretical physicist and as an expert in particle accelerators during the period between the time he had to leave the Vienna Staatsgymnasium in 1938, because of his Jewish origin from the maternal side, until his arrival in Italy in the early 1950s. The whole reconstruction has made use of only very recently published material, and especially of archival unpublished documents as well as previously unknown correspondence which include letters to Arnold Sommerfeld and to Touschek's family in Vienna from Italy, Germany and Great Britain.

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WHO DISCOVERED QUARKS?

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Abstract

Quarks are fundamental to our understanding of the universe. The devising of the quark model, its experimental confirmation, and its eventual integration into the "Standard Model" of elementary particles was one of the outstanding achievements of 20th-century physics. It is surprising, therefore, that confusion has arisen about how quarks were actually "discovered", i.e. how experimental evidence led the scientific community to accept the real existence of quarks. A myth has emerged that the existence of quarks did not become accepted until after 1968, and then primarily as a result of the deep inelastic electron-proton experiments at Stanford Linear Accelerator Center (SLAC) in California. This view was bolstered by the award of the Nobel Prize in 1990 to Friedman, Kendall and Taylor. The "SLAC myth" has been perpetuated in textbooks of physics, in Wikipedia, and elsewhere.

However, the contemporary literature shows that the existence of quarks was widely accepted well before 1968, mainly because of data from hadron-hadron scattering experiments using bubble chambers at Brookhaven (New York) and CERN (Geneva). The continuing harvest of such data into the early 1970s rendered the quark model practically unassailable. The SLAC data, while providing valuable complementary evidence, was of secondary importance for establishing quarks in the minds of most scientists at the time.

Suggestions are made about how the "SLAC myth" arose. Philosophical, political and nationalistic factors were surely involved. No doubt there was a desire for SLAC to publicly justify its costs and to distinguish its findings from what had already been revealed elsewhere by the bubble-chamber technique. When a major scientific development has to be conveyed to the public in just a couple of sentences, oversimplification is hard to avoid and a misleading myth may easily be generated.

Quarks constitute one of the greatest scientific triumphs of the later 20th century and are central to our present-day understanding of the microcosm. The quark model has been widely popularised and its essentials are now routinely taught, at least qualitatively, even at sub-university level. One would therefore hope that the story of the discovery of quarks would be well-documented, well-understood and accurately retold.

The work reported here was originally prompted by the feeling that the discovery of quarks is rather poorly dealt with in some of the textbooks and courses that are provided for students today. The topic is often covered very briefly, giving students only a sketchy outline of how the existence of quarks came to be accepted. Arguably, that doesn't matter –as long as they understand the physics of quarks, why worry about the history? But it appears that even the little history that students are taught is surprisingly biased, if not seriously distorted.

It is necessary to begin with a few philosophical remarks. Were quarks invented, discovered, or constructed? It is uncontroversial that the quark model, as a well-developed theoretical hypothesis, was put forward by Gell-Mann and by

Zweig in 1964. It was based on the earlier ideas of Gell-Mann and Ne'eman –the "Eightfold Way" of 1961– and of Sakata and, much earlier still, of Fermi.

The quark model provided the most natural physical explanation for the regularities of the otherwise-bewildering hadron spectrum. However, the experimental data available in 1964 was not sufficient to confirm or refute the quark model convincingly in the minds of all physicists. The discovery of the omega-minus hyperon at the Brookhaven Laboratory was announced in 1964, but it could be argued that it was a prediction of the Eightfold Way symmetry scheme, not of the quark model specifically. And, since quarks could not be isolated and observed as independent particles, scientists with a positivistic frame of mind could claim that the existence of quarks was unproven, and perhaps unprovable and therefore meaningless.

Nevertheless, within a few years the existence of quarks had become generally accepted among particle physicists on the strength of the extensive experimental observations which accumulated after 1964. The acceptance of the quark model on the basis of observational evidence is what is meant in this paper by the "discovery" of quarks.

However, according to another viewpoint, quarks cannot be said to have been "discovered" at all but should be regarded as a social construction, confected collectively by the particle physics community and bearing no necessary relationship to physical reality. Anti-realism had a significant impact on the historiography of science during the late 20th century: in the context of the quark model, Andrew Pickering's *Constructing Quarks: a sociological history of particle physics* (1984) was particularly prominent. Curiously, though, Pickering concentrated on the developments of the mid- to late 1970s (Cushing 1985 and Barlow 2000) as if that had been the time-frame during which quarks were "constructed". Certainly it was the time-frame during which experimental practice shifted overwhelmingly into lepton-beam and storage-ring experiments. It was also the time when the charmed quark became accepted as the explanation for the J/psi particle discovered at Brookhaven and at the Stanford Linear Accelerator Center (SLAC) in late 1974. But, by then, quarks had been widely believed in for the best part of a decade.

What are students taught about the discovery of quarks? In Britain, pre-university (A-level) students studying physics are expected to learn very little about the history. The common A-level specification (EdExcel, 2007) mentions "that nucleon structure can be revealed by high-energy deep inelastic electron scattering", alluding to experiments which took place at SLAC in the late 1960s; but it refers to none of the earlier experimental work.

The book by Tipler and Mosca (2004) is typical of the kind of voluminous, comprehensive and widely-used textbook that covers physics at an introductory and intermediate university level. According to Tipler and Mosca, "The great strength of the quark model is that all the allowed combinations of three quarks or quark-antiquark pairs result in known hadrons. Strong evidence for the existence of quarks inside a nucleon is provided by high-energy scattering experiments called *deep inelastic scattering"* (italics in original). And, "These experiments are analogous to Rutherford's scattering of alpha particles by atoms in which the presence of a tiny nucleus in the atom was inferred from the large-angle scattering of the alpha particles." Thus, Tipler and Mosca strongly suggest that the existence of quarks was accepted because of the evidence from deep inelastic scattering –evidence which only began to appear at the end of the 1960s.

As another example, one may look at Schiller's (2011) online textbook *Motion Mountain*, which says that "The prediction of components inside the protons was confirmed in the late 1960s when Kendall, Friedman and Taylor shot high energy electrons into hydrogen atoms. They found that a proton contains *three* constituents with spin $^1/_2$, which they called called *partons*. The experiment was able to 'see' the constituents through large angle scattering of electrons, in the same way that we see objects through large angle scattering of photons. These constituents correspond in number and (most) properties to the so called *quarks* predicted in 1964 by George Zweig and also by Murray Gell-Mann."

Much the same story may be found in Wikipedia, whose *Timeline of particle discoveries* gives, for the year 1969, "Partons (internal constituents of hadrons) observed in deep inelastic scattering deep inelastic scattering experiments between protons and electrons at SLAC; this was eventually associated with the quark model (predicted by Murray Gell-Mann and George Zweig in 1964) and thus constitutes the discovery of the up quark, down quark and strange quark."

This paper is not based on a comprehensive survey of textbooks, but the accounts given by Tipler and Mosca and by Schiller are typical, and it is clear that a myth has grown up about the discovery of quarks. The myth goes something like this: quarks were postulated theoretically in 1964 by Gell-Mann and Zweig because they provided a physical basis for the "Eightfold Way" symmetry which had been found to describe the hadron spectrum; and the theory was confirmed in 1968 and the succeeding years by electron-beam experiments at the Stanford Linear Accelerator Center (SLAC).

SLAC's own website certainly encourages this view: under *SLAC History* for the year 1968, it lists "First evidence discovered for quarks". And, of course, in 1990 the Nobel Prize in physics was awarded to Jerome Friedman, Henry Kendall and Richard Taylor "for their pioneering investigations concerning deep inelastic scattering of electrons on protons and bound neutrons, which have been of essential importance for the development of the quark model in particle physics."

But what really happened? Personal memory indicates that quarks were widely believed in within a year or two of the publications by Gell-Mann and Zweig, at any rate among the younger generation of physicists in Europe. It would be possible to show this by citing many papers from the period, but the point can be adequately demonstrated by one or two especially prominent examples. For example, the highly distinguished physicist Viktor Weisskopf presented in 1966 a set of lectures at CERN which show that the quark model was then already commonly accepted as the way in which the hadron spectrum should be understood. Such lectures were provided for young scientists and for non-specialists, and as such were not intended to express unorthodox or speculative ideas.

A year later a similar set of lectures was presented at CERN by the MIT physicist B. T. Feld (1967), *The Quark Model of the Elementary Particles*. Over 48 pages, Feld described the quark model in detail. Not only did he catalogue the success of the quark model in explaining the static properties of hadrons (masses and decay widths) but he also mentioned predictions for the ratios of cross-sections for the photoproduction of vector mesons, and also for elastic scattering amplitudes and total cross sections in hadron-hadron collisions, including the Johnson-Treiman (1965) relations. Between them, then, Weisskopf's and Feld's lectures demonstrate that "quarks" were common parlance among theoretical and experimental physicists by the mid-1960s, at least at CERN; and they were retailing knowledge and ideas which were well established at the time and well supported by the experimental evidence then available.

But what sort of experimental evidence was it? During the 1960s, new and improved accelerator technology was making it possible for hadron-hadron collisions to be investigated at increasingly high energies. It was the heyday of bubble chambers, for the invention of which, in 1952, Donald Glaser received the 1960 Nobel Prize in physics. Just as there was no serious competitor to the bubble chamber as an experimental technique at the time, there was no serious competitor to the quark model for explaining the properties of the plethora new particles and resonances that were revealed. Moreover, the quark model provided a framework for understanding the overall qualities of high-energy hadron collisions: the ratios of cross-sections; the slowly-increasing multiplicities of particles produced; the phenomenon of inelastic diffraction; the "leading particle effect"; and the general peripherality of such collisions by which the resulting particles were confined to relatively low transverse momenta.

The quark idea became deeply rooted in the thought processes of ordinary particle physicists. Physicists often used quark-line diagrams to represent interactions, and the "diagrammar" of the quark model could explain why, for example, there is a "duality" in two-body-to-two-body collisions, with hadron resonances being formed by the amalgamation of two colliding particles but also having virtual hadrons exchanged between them. Of course, the quark model was expressed quantitatively as well as qualitatively and diagrammatically: various mathematical formulations were devised, one of the most successful being the Veneziano model (1968).

In reality, therefore, the quark model became accepted because of the weight of experimental evidence that accumulated during the late 1960s, mainly from hadron-hadron scattering experiments carried out using bubble chambers. Brookhaven and CERN were the leading laboratories in the effort, and the discovery of quarks was the collective achievement of a large and dispersed international community of scientists. But there was no single *experimentum crucis*, no individual hero, and no precise moment as which one could say that the existence of quarks had become a certainty.

Of course, this is not to say that the electron-beam experiments at the end of the decade were not of great importance. On the contrary, they probed the structure of the nucleon in a quite different and complementary way, and therefore provided reassurance to physicists that the "quarks" already revealed by hadronic interactions were not some sort of cruel illusion but were likely to be concrete objects in spite of it being impossible to isolate them. The fear that quarks might just be a handy way of thinking had never been far below the surface, and the SLAC data helped to dispel it.

But why did the "SLAC myth" arise? Several different factors may have contributed to it. Institutions such as SLAC generally need to publicise their successes, if only to justify themselves to their funding bodies and to bolster arguments for additional cash in the future. (A quarter of a century later, the cancellation of the Superconducting Super Collider devastated the US particle physics community.) National pride may also have come into it, compounded by a cultural difference which, at least in those days, made Americans more likely than Europeans to promote the significance of their achievements. Rivalry between the West Coast and the East Coast –between SLAC and Brookhaven, and between universities such as Stanford and MIT– may also have been relevant. Furthermore, the award of a Nobel Prize may by itself enhance scientists' and institutions' reputations, ultimately distorting history.

Philosophy may also have played a part. It may not be going too far to suggest that, because of a logical-positivistic philosophy of science, some scientists in the 1960s were inclined to remain sceptical or non-committal about the existence of quarks for many years. Richard Feynman (1969) preferred to refer to "partons" rather than quarks even when he was writing about hadron-hadron collisions. In fact, distinguished West-Coast physicists seem almost to have gone out of their way to avoid mentioning "quarks" –see, for example, papers like that of Bjorken (1969). There were good scientific as well as philosophical reasons for this– the definition of a "quark" was unsettled, the role of gluons was still unclear (although the concept of the gluon had been around for a few years) and a distinction came to be made between "constituent quarks", of which a proton contains three, and "current quarks", of which a proton contains an indeterminate number not less than 3. But whatever the logic, the fact is that a prominent subset of the physics community held back from acknowledging the existence of quarks. So, when the electron-beam data arrived at the end of the 1960s, the appearance of quark-like nucleon substructure could be represented as if it were a new and unexpected revelation.

In conclusion, then: a misleading myth has grown up. Even if quarks were not "constructed" in quite the sense that Andrew Pickering meant, the *story* of the discovery of quarks has been very much a social construction. In fact quarks were not discovered through deep inelastic collisions in California in 1968. Their discovery was a protracted process which began in the middle of that decade, relied predominantly on bubble-chamber observations, and had its centre of gravity in mid-Atlantic.

References

BARLOW, R. (2000). Review of *Constructing Quarks: a sociological history of particle physics* (Pickering, A., Chicago: University of Chicago Press, 1984). Eur. J. Phys., vol. 21, pp. 199-200.

BJORKEN, J. D. (1969). *Asymptotic Sum Rules at Infinite Momentum.* Physical Review, vol. 179, no. 5, 25 March 1969, pp. 1547-1553.

CUSHING, J. T. (1985). Review of *Constructing Quarks: a sociological history of particle physics* (Pickering, A., Chicago: University of Chicago Press, 1984). Philosophy of Science, vol. 52, no. 4, pp. 640-641.

EDEXCEL (2007). London: personal communication.

FELD, B. T. (1967). *The Quark Model of the Elementary Particles* (Lectures given in the Academic Training Programme of CERN). Geneva: CERN Yellow Report 67-21, 24 August 1967.

FEYNMAN, R. P. (1969). *Very High-Energy Collisions of Hadrons*. Phys. Rev. Letts., vol. 23, no. 24, pp. 1415-1417.

JOHNSON, K., and TREIMAN, S. B. (1965). *Implications of SU(6) Symmetry for Total Cross Sections*. hys. Rev. Letts. vol. 14, 189-191, 8 February 1965.

PICKERING, A. (1984). *Constructing Quarks: a sociological history of particle physics*. Chicago: University of Chicago Press.

SCHILLER, C. (2011). *Motion Mountain: the Adventure of Physics*. Vol. V, *Pleasure, Technology and Stars*, p. 157. Online: motionmountain.eu (accessed 24 March 2011).

TIPLER, P.A. and MOSCA, G. (2004). *Physics for Scientists and Engineers* (fifth edition, extended version). New York: W.H. Freeman and Company.

VENEZIANO, G. (1968). *Construction of a crossing-symmetric, Regge behaved amplitude for linearly-rising trajectories*. Nuovo Cimento vol. 57A, p. 190.

WEISSKOPF, V. F. (1966). $SU_2 \rightarrow SU_3 \rightarrow SU_6$ (Lectures given in the Academic Training Programme of CERN). Geneva: CERN Yellow Report 66-19, 24 May 1966.

Wikipedia (2011). *Timeline of particle discoveries*. Online: accessed 24 March 2011.

CROSSING BORDERS
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FROM IDENTITY TO INDISTINGUISHABILITY

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Abstract

It is widely accepted that the papers by Bose in 1924 and by Einstein in 1924-1925 definitely opened the path to the quantum concept of 'indistinguishability'. Since then, distinction in physics between 'identity' and 'indistinguishability' of particles has come to be required.
However, even before the rise of quantum physics, Maxwell, Boltzmann and Gibbs, among others, had to tackle the difficulties and paradoxes that appear when counting identical particles. Later, after Sackur's and Tetrode's first quantum derivation of the canonical partition function of an ideal gas, much confusion arose around its dependence on the number of molecules in the system. Many other physicists, such as Planck, Lorentz or Schrödinger took part in those debates. In 1921, Ehrenfest and Trkal devoted a whole paper to this subject. Clearly, in the days before Bose's seminal work, embroiling and misunderstanding were commonplace.
In considering the emergence of the distinction between 'identical' and 'indistinguishable' particles, we will go through the so-called statistical route to quantum mechanics. The first developments in statistical mechanics became immediately confused and entangled with the irruption of quantum physics. In the first quarter of the twentieth century the mutual influence between both disciplines was profuse.
We will pay special attention to the story of the so-called 'Gibbs Paradox.' We will argue that what is now often known by this name in some textbooks of statistical mechanics did not exist as such in those foundational days.

The statistical route to quantum mechanics in the first quarter of the twentieth century represents a particularly interesting case of interdisciplinary influence. Some of the involved disciplines such as statistical mechanics were practically new, while others, like physical chemistry or thermodynamics, just a bit older. Quantum theory as such was still in a –let's say– embryonic stage

In this communication, I focus on the apparently simplest statistical system: the monatomic ideal gas. In particular, I will focus on the vicissitudes of the concept of identity of its components, the elemental particles, which had its main analytical expression in the dependence of entropy on N, the number of particles.

As for the interdebate, in this particular case, the confussion was so huge, that we can hardly speak of a gradual and reciprocal process of enrichment. At least regarding identity, the wide variety of factors involved in the determination of the N-dependence of the entropy of an ideal gas contributed to the proliferation of arguments and counterarguments, so entangled, that left the question frankly muddled, and far from being clearly understood even after the birth of quantum statistics.

Last month, a more than forty pages article devoted to the Gibbs' Paradox and the statistics of indistinguishable particles appeared in the *Journal of Statistical Physics*.[1] The journal *Entropy* will devote next year a special issue to the Gibbs' Paradox. And these are not isolated cases. For almost one hundred years there has been a continuos flux of contributions, seeking to clarify the question of the identity of the particles with new analysis, new interpretations and new terminology proposals.

Before beginning the story, let me remind some of the results involved in those debates:

- *Nernst Heat Theorem*. Its interpretations, applications, and experimental measures.
- *The quantum hypothesis*. How to quantize non-periodic movements. Extent of the quantum cell.
- *Boltzmann Principle*. Definition of thermodynamic probability.
- How to measure *entropies* in the laboratory.

None of these points were at the time firmly established or with an agreed meaning. In the following I will make a brief review of some of the arguments used when trying to justify the *N* dependence of entropy, in order to establish relations to commonplaces still found in current textbooks and articles. Needless to say, I'm much indebted with previous works on this subject. This communication is based, aside from the original works of the major figures of this story, on the historiographical studies by Desalvo, Darrigol, Monaldi, and Badino, to name only those I have used more often.[2]

Pre-quantum Debate on Identity

Even before the rise of Planck's quanta, physicists had to deal with the statistical treatment of identical particles. Generally speaking, we can say that elementary particles were considered as such in the traditional sense of the word, which could be traced back even to Epicurean atoms. Maxwell wrote, in his *Theory of Heat*, in the ninth edition of 1888, that "The molecules of the same substance are all exactly alike, but different from those of other substances."[3]

Many of the debates that took place on this subject were related to a very famous passage by Gibbs in his "On the equilibrium of heterogeneous substances," in 1876.[4] There, Gibbs pointed out that the expression for the entropy of the mixture of different gases cannot be applied to –let me say– the *mixture* of the same gas.

> ... although the process of mixture [of two different gases], dynamically considered, might be absolutely identical in its minutest details (even with respect to the precise path of each atom) with processes which might take place without any increase of entropy. In such respects, entropy stands strongly constrasted with energy.

Therefore, Gibbs simply stressed the peculiarity of the function that links mechanics and thermodynamics. Maxwell also argued that from a thermodynamic perspective one could not speak of a diffusion within the same gas in the same conditions, that is, within an homogeneous system.[5] Boltzmann saw in the thought experiment by Gibbs just a stroking consequence of the statistical nature of entropy.[6]

This abrupt change of entropy depending on whether the gases in the mixture were the same or different was baptized soon afterwards, as far as I know, by Otto Wiedeburg, in 1894, as 'Gibbs' Paradox.'[7] However, the paradox did not emphasize the statistical nature of entropy, but the fact that with a continuous variation of the microscopic properties came an abrupt change in the macroscopic scale. Anyway, it was stated within a thermodynamic context.

Later, in 1902, in his celebrated book on statistical mechanics, Gibbs did not directly address this issue, but one closely related.[8] When studying mixtures of gases he stated:

> If the particles are regarded as indistinguishable, it seems in accordance with the spirit of the statistical method to regard the phases as identical.[9]

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[1] PETERS (2010).

[2] DESALVO (1992), DARRIGOL (1991), MONALDI (2009), BADINO (2009), BADINO (2010).

[3] MAXWELL (1888), 330. I haven't consulted the first edition, of 1871.

[4] GIBBS (1993), 167.

[5] MAXWELL (1878).

[6] BOLTZMANN & NABL (1903-1921), 519. Their article was finished in October 1905.

[7] WIEDEBURG (1894).

[8] GIBBS (1902).

[9] *Ibid*., 187-188.

However, in some situations, we can be interested in distinguishing particles. According to Gibbs,

> The question is one to be decided in accordance with the requirements of practical convenience in the discussion of the problems with which we are engaged.

Which is quite within the positivist approach of the whole Gibbs' book. In its last paragraph, we find briefly discussed the case of the *mixture* of the same gas. According to Gibbs, this case clearly shows that when dealing with ideal gases there was no escaping: one had to use what he so-called the generic phase, which involves a permutation factor *N!* Otherwise, the entropy of an ideal gas wouldn't be extensive, which is not acceptable. Boltzmann had himself used a similar argument in as early as 1877.[10] To ensure the extensivity of entropy he took away from the logarithm of the probability of a distribution the term $N\ln N$. Like Gibbs, he noted that the entropy constant, which the introduction of this factor modified, was physically irrelevant. Only entropy differences were actually meaningful.

Hence, before the quantum novelties came, no major drawbacks were attached to the indistinguishability of particles. We can approach the problem by noting that in thermodynamics, the variable *N* has a cardinal sense, that is to say, it only refers to the total number of particles, whereas in mechanics, it may as well have an ordinal meaning so it can also refer to the label of the *N*th particle. Statistical approach has to suppress this divergence in the meaning of the variable *N*. Extensivity is only meaningful with a cardinal *N*.

Nernst Heat Theorem and the Quantization of the Monatomic Gas

The formulation of Heat's Theorem by Nernst in 1906 changed the scenario. One of its most accepted interpretations provided physicists with an absolute reference for entropy at zero temperature. The entropy constant was no longer irrelevant. Some physicists thought that maybe quantum physics could provide an analytical expression for the absolute entropy. That was the idea pursued by those who crossed subdisciplinary borders to show new analysis of monatomic ideal gas. In the following I will mention some of the contributions of those days.

The first attempt took place in 1911 and was made by Otto Sackur, a former collaborator of Nernst now at Breslau.[11] Although far from being an expert on that field, his pioneering work soon attracted others more experienced. He obtained an analytical expression for the entropy constant and related it to what Nernst had christened as the "chemical constant," which in turn appeared in the formula of the vapor pressure. Sackur concluded that the value of the elemental quantum region depended on the number of particles of the system. This result was a consequence of imposing the extensivity of entropy. Planck's initial approach to the subject started with an idea very similar to that of Sackur's.[12]

The next significant contribution was by the young Dutch Hugo Tetrode.[13] It is to him that we owe one of the most popular arguments up to now. I will call it the "interchangeability argument." Clearly inspired by Gibbs' book, Tetrode argued that in a system with perfectly equal and interchangeable molecules, in order to obtain an extensive entropy, the total number of microstates must be divided by *N!* Tetrode tied the property of identity with the requirement of extensivity. A good treatment of the first ensured the fulfilment of the second. We must note, however, that this "interchangeability argument" is only valid for configurations where there isn't more than one particle in an elementary cell. If there's two or more particles in a cell, the number of possible permutations is smaller than *N!*

Tetrode's elementary region, unlike Sackur's, did not depend on *N*. However, Sackur would soon publish new researches rectifying his original result. In 1912, he resorted to the factorial of *N*.[14] But then he rectifyed again. In a new paper, when performing the phase integrals, he divided the volume of the system in small boxes of volume V/N.[15] Then, he calculated the total entropy as the sum of individual entropies. Thus, extensivity was obviously ensured.

Another of the main characters of this story is Otto Stern, in 1913 a collaborator of Einstein's. He worked out a new formula for vapor pressure and, in a different article, linked the chemical equilibrium constants with a kinetic treatment *à la Boltzmann*.[16] In the former, he didn't take into account the difference between oscillators and gases concerning the location of molecules in the solid and its lack in gases. Because of that, his analysis yielded an extensive entropy with apparently no *ad hoc* additions. However, Stern only applied quantization to the solid phase.

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[10] BOLTZMANN (1877), 402-403.

[11] SACKUR (1911).

[12] PLANCK (1959), 127-134.

[13] TETRODE (1912).

[14] SACKUR (1912).

[15] SACKUR (1913).

[16] STERN (1913), STERN (1914).

Meanwhile, Tetrode tried to improve his arguments after a heavy critique by Lorentz. The renowned Dutch had pointed out a weakness in Tetrode's method: his use of Boltzmann's Principle concerning the definition of the thermodynamic probability.[17] According to Lorentz, "the way in which this application has been made requires further explanation and justification." Tetrode presented a new paper in 1915.[18] According to him, the new "thermodynamic probability" he introduced did satisfy Lorentz, but the justification of the *N!* remained dark. Tetrode admited it was "a difficult question." A physicist who was also working at Leiden, Obe Postma, defended in 1916 the version of Tetrode in front of that of Planck's, inspired in Sackur's first approach.[19] However, Postma thought that the factorial of *N* wasn't related to anything else than "extensivity condition": since the statistical models had to recover thermodynamics it needed no further justification.

A very similar opinion was that showed by Einstein in a manuscript probably read at the *Deutsche Physikalische Gesellschaft* in February 1916, shortly after having read Tetrode's paper.[20] In that communication, Einstein tried to remove "all confusing paraphernalia" around this subject. He expressed his distrust on the classical kinetic models, which he saw not valid enough as to draw too many conclusions from them.

In a very interesting paper by Trkal and Ehrenfest of 1920 they also critized the existing confusion and developed arguments in accordance with Einstein's.[21] They even questioned the reliability of the vapor-pressure formula method. They pointed that:

> It is not an accident that it is always the same point that remains obscure in these theories, viz., how an expression of the form N^{-N} (Sackur) or $(N!)^{-1}$ (Tetrode) can be forced into the "thermodynamic probability *W*" in order to obtain an admissible value for the entropy.[22]

According to them:

> The law of dependence on *N* can only be satisfactorily settled by utilizing a process in which *N* changes reversibily and then comparing the ratios of the probability with the corresponding differences of entropy.

They deal with gas mixtures, where the *N* dependence of a single monatomic gas cannot be calculated. According to Ehrenfest and Trkal, the impossibility of having processes in which one may double the number of particles of the gas, prevents deriving the dependence on *N* kinetically.

The paper by Ehrenfest and Trkal prompted Planck's response, who after criticism had abandoned his original opinion on the subject. In 1916, he had adopted Tetrode's justification of the factorial of *N*.[23] That is, he had adopted the 'interchangeability argument.' In 1921 he persisted in both his justification of the N factor and in the absolute definition of entropy.[24] Certainly, his arguments in terms of symmetry properties of the whole gas model were more specific. In fact, in one of his successive attemps to justify the argument, Planck had also suggested that it may only be valid in the limit. Soon after that, Schrödinger wrote an article in which he examined different understandings of Boltzmann's principle, reaching up to four different definitions of entropy.[25] He also pointed the weaknesses of the 'interchangeability argument.'

Quantum Indistinguishability to the Rescue?

The scenario changed again with Bose and Einstein's contributions.[26] How did Einstein solve the mess, if at all? I can not dwell too much on details which, on the other hand, have already been discussed elsewhere. What I truly want to emphasize is that Einstein radically changed the approach. In terms of Gibbs' ensembles, and loosely speaking, we can say that he made his developments within the grandcanonical ensemble, rather than in the microcanonical or the canonical, where the problem had been posed for thirty years.

Einstein noted that the new expression for entropy had a property that the classical one very much lacked: it fulfiled both Nernst theorem and the extensivity condition. And all that *ad hoc* factors-free. Comparing classical and quantum cases,

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17 LORENTZ (1914).

18 TETRODE (1915).

19 POSTMA (1916).

20 EINSTEIN (1916).

21 EHRENFEST & TRKAL (1920).

22 *Ibid*., 162-163.

23 PLANCK (1916).

24 PLANCK (1921).

25 Schrödinger (1925).

26 BOSE (1924), EINSTEIN (1924), EINSTEIN (1925a), EINSTEIN (1925b).

he noted that in the former a term N^N had to be removed to obtain an extensive entropy. Although he mentions the 'interchangeability argument,' he doesn't establish any link whatsoever between N dependence and the new statistics.

Despite criticisms, and despite the emergence of the new statistics, the 'interchangeability argument' did not disappear. We find it, for instance, in the book by the American physicist Richard C. Tolman *Statistical Mechanics with Application to Physics and Chemistry*, published in 1927 and signed in August 1926 (over a year and a half after the publication of Einstein's theory).[27] Also in the *Introduction to Chemical Physics* by John C. Slater, from 1939.[28] There we can find an argument repeated in later textbooks: there's no difussion in the same gas because particles are really identical. This "really" was supposed to mean "quantum" identical, not "classical." Schrödinger gave the exactly same solution to Gibbs' Paradox in his *Seminar Lectures* delivered in 1944 in Dublin.[29] However, he did not use the "interchangeability argument," and derived the corrected classical formula for the monatomic ideal gas by performing a mathematical limit, as Planck had already suggested in the twenties. To picture once more time the mess, let me mention that Stern returned to the subject in 1949.[30] He did with a short note on the controversial term in the entropy. Besides ruling out an experimental veredict, Stern claims that "some of the arguments still used can be hardly justified."

Some time in the fifties –I don't know the exact date yet– the confusion got worse, when the term 'Gibbs' Paradox' turned polysemic: a statistical meaning was born. It went on to describe the not-exactly-nice consequences with which one bumbs into when dealing with a non-extensive entropy. This would happen when we use the wrong statistics, that is, classical statistics. Let me note that this new Gibbs' Paradox is not from Gibbs and is not even a paradox. In the stories where this change of meaning can be appreciated, quantum mechanics came to the rescue of classical physics anihilating a paradox that had astonished physicists long before the emergence of quantum mechanics. This attractive reconstruction appears in such valuable books on statistical mechanics as Huang, Reif or Pathria, among others.[31]

I'll finish with a quote of one of these modern textbooks. Kerson Huang's *Statistical Mechanics*, from 1963, reissued in 1987. He justifies the introduction of the factorial of *N* resorting to quantum inherent indistinguishability. There, we find a mixture of classical devices, like the phase-space, and quantum mechanical concepts. However, Huang loops the loop before closing the chapter:

> The foregoing discussion contains the correct reason for, but it is not a derivation of, the "correct Boltzmann counting," because in classical mechanics there is not a consistent way in which we can regard the particles as indistinguishable. In all classical considerations other than the counting of states we must continue to regard the particles in a gas as distinguishable.[32]

Then, although the correct result must be obtained performing a mathematical limit, the correct reason remains that proposed by Tetrode's.

> According to these commonplaces, from classical identity to quantum indistinguishability there's only a factorial of *N*.

References

BADINO, M. (2009): "The old couple: Boltzmann, Planck and the application of statistics to physics (1900-1913)." *Annals of Physics* 18, 81-101.

BADINO, M. (2010): "Pursuing an idea: Planck's quantum theory of ideal gas." German Translation in Dieter Hoffmann (ed.), *Max Planck und die moderne Physik*, 2010, Springer, Berlin.

BOLTZMANN, L. (1877): "Über die Beziehung zwischen dem zweiten Hauptsatze der mechanischen Wärmetheorie und der Wahrscheinlichkeitsrechnung, respective den Sätzen über das Wärmegleichgewicht." *Akademie der Wissenschaften*, *Vienna*, *Sitzungsberichte*, *Abteilung II*, 373-435.

BOLTZMANN, L. & NABL, J. (1903-1921): "Kinetische Theorie der Materie." In *Encyklopädie der Mathematischen Wissenshcaften.* Band V, 1. Teil, No 8, ed. A. Sommerfeld, 493-557, Teubner, Leipzig.

BOSE, S. N. (1924): "Plancks Gesetz und Lichtquantenhypothese." *Zeitschrift für Physik* 26, 178-181.

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[27] TOLMAN (1927), 314.

[28] SLATER (1939), 126-129.

[29] SCHRÖDINGER (1946), 58-62.

[30] STERN (1949).

[31] HUANG (1963), 140-142; REIF (1965), 243-246; PATHRIA (1972), 24-29.

[32] HUANG (1963), 142.

DARRIGOL, O. (1991): "Statistics and combinatorics in early quantum theory, II: Early symptoma of indistinguishability and holism." *Historical Studies in the Physical and Biological Sciences* 21:2, 237-298.

DESALVO, A. (1992): "From the chemical constant to quantum statistics: A thermodynamic route to quantum mechanics." *Physis* 29 (2), 465-537.

EHRENFEST, P. & TRKAL, V. (1920): "Deduction of the dissociation equilibrium from the theory of quanta and a calculation of the chemical constant based on this." *Proceedings of the Amsterdam Accademy* 23, 162-183.

EINSTEIN, A. (1916): "On the Theory of Tetrode and Sackur for the Entropy Constant." Dated by the editors on the assumption that this writing is related to a lecture delivered by Einstein at the *Deutsche Physikalische Gesellschaft* at the meeting of 14 January 1916. In A.J. Kox, M.J. Klein, R. Schulman (eds.), *The collected papers of Albert Einstein*, *vol. 6*, *The Berlin years: Writings, 1914-1917*, Princeton University Press, Princeton, 250-262.

EINSTEIN, A. (1924): "Quantentheorie des einatomigen idealen Gases." *Preussische Akademie der Wissenschaften, Physikalisch-mathematische Klasse (Berlin). Sitzungsberichte*, 261-267.

EINSTEIN, A. (1925a): "Quantentheorie des einatomigen idealen Gases. Zweite Abhandlung." *Preussische Akademie der Wissenschaften (Berlin), Physikalisch-mathematische Klasse. Sitzungsberichte*, 3-14.

EINSTEIN, A. (1925b): "Zur Quantentheorie des idealen Gases." *Preussische Akademie der Wissenschaften (Berlin), Physikalisch-mathematische Klasse. Sitzungsberichte*, 18-25.

GIBBS, J. W. (1902): *Elementary principles in statistical mechanics*. Yale University Press, Yale.

GIBBS, J.W. (1993): "On the equilibrium of heterogeneous substances". In *The scientific papers of J. Willard Gibbs*, vol. 1, Ox Bow Press, Woodbridge, 55-349.

HUANG (1963): *Statistical Mechanics*. John Wiley & Sons, New York.

LORENTZ (1914): "Some Remarks on the Theory of Monatomic Gases." *Proceedings of the Amsterdam Academy* 19, 737-751.

MAXWELL, J.C. (1878): "Diffusion." *Encyclopedia Britannica*, ninth edition, vol. 7, 214-221.

MAXWELL, J.C. (1888): *Theory of Heat*. Longmans, Green and Co, London and New York.

MONALDI, D. (2009): "A note on the prehistory of indistinguishable particles." *Studies in the History and Philosophy of Modern Physics* 40, 383-394.

PATHRIA, R.K. (1972): *Statistical Mechanics*. Butterworth-Heinemann Ltd, Oxford.

PETERS, H. "Statistics of Distinguisgable Particles and Resolution of the Gibbs Paradox of the First Kind." *Journal of Statistical Physics* 141, 785-828.

PLANCK, M. (1916): "Über die absolute Entropie einatomiger Körper." *Preussische Akademie der Wissenschaften (Berlin), Physikalisch-mathematische Klasse. Sitzungsberichte*, 653-667.

PLANCK (1921): "Absolute Entropie und chemische Konstante." *Annalen der Physik* 66, 365-372.

PLANCK (1959): *The Theory of Heat Radiation*. Dover, New York. This is an unaltered republication of the English translation of the second German edition of 1913.

POSTMA, O. (1916): "Entropy and Probability." *Proceedings of the Amsterdam Academy* 18, 1048-1056.

REIF, F. (1965): *Fundamentals of Statistical and Thermal Physics*. McGraw-Hill, New York.

SACKUR, O. (1911): "Die Anwendung der kinetischen Theorie der Gase auf chemische Probleme." *Annalen der Physik* 36, 958-980.

SACKUR, O. (1912): "Die Bedeutung des elementaren Wirkungsquantums für die Gastheorie und die Berechnung der chemischen Konstanten." In *Nernst Festschrift*, Knapp, Halle, 405-423.

SACKUR, O. (1913): "Die universelle Bedeutung des sogenannten elementaren Wirkungsquantums." *Annalen der Physik* 40, 67-86.

SCHRÖDINGER, E. (1925): "Bemerkungen über die statistische Entropiedefinition beim idealen Gas." *Preussische Akademie der Wissenschaften (Berlin), Physikalisch-mathematische Klasse. Sitzungsberichte*, 434-441.

SCHRÖDINGER, E. (1946): *Statistical Thermodynamics*. Cambridge, Cambridge University Press.

SLATER, J.C. (1939): *Introduction to Chemical Physics*. McGraw-Hill, New York.

STERN, O. (1913): "Zur kinetischen Theorie des Dampfdrucks einatomiger fester Stoffer und über die Entropiekonstante einatomige Gase." *Physikalische Zeitschrift* 14, 629-632.

STERN, O. (1914): "Zur Theorie der Gasdissoziation." *Annalen der Physik* 44, 497-524.

STERN (1949): "On the term k ln n! in the entropy." *Review of Modern Physics* 21, 534.

TETRODE, H. (1912): "Die chemische Konstante der Gase und das elementare Wirkungsquantum." *Annalen der Physik* 38, 434-442 (Also: "Berichtigung." *Annalen der Physik* 39, 255-256.).

TETRODE, H. (1915): "Theoretical determination of the entropy constant of gases and liquids." *Proceedings of the Amsterdam Academy* 17, 1167-1184.

TOLMAN, R.C. (1927): *Statistical Mechanics with Applications to Physics and Chemistry*. Chemical Catalog Company, New York.

WIEDEBURG, O. (1894): "Das Gibbs'sche Paradoxon." *Annalen der Physik* 289/12, 684-697.

INTERNATIONALISM OF PHYSICS DURING THE 1920S AND 1930S: FORMATION OF A EUROPEAN RESEARCH NET AND ITS IMPACT IN THE DEVELOPMENT AND DISSEMINATION OF THE EUROPEAN SCIENTIFIC CULTURE

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UNRAVELING THE NATURE OF COSMIC RAYS. BRUNO ROSSI AND THE SPREAD AND DEVELOPMENTS OF EXPERIMENTAL PRACTICES AND SCIENTIFIC COLLABORATIONS IN COSMIC-RAY PHYSICS BEFORE WORLD WAR II

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Abstract

During the late 1920s/beginning of the 1930s, particularly in connection with the birth of Enrico Fermi's and Bruno Rossi's research groups in Rome and Florence, strong relationships were established between Italy and other European countries such as Germany, Great Britain and France, as well as with some physicists of the US scientific community. Bruno Rossi, an outstanding scientist in the study of cosmic rays and the pioneer of this research field in Italy since the early 1930s, is a prominent example in this sense. His friendship with Fermi, Bothe, Blackett, Heisenberg, Bethe, was instrumental for the exchange of knowledge about experimental practices and for theoretical discussions. Rossi was also successful in attracting the interest of physicists such as Arthur H. Compton and Louis Leprince-Ringuet on the problem of cosmic rays. The ties established during the 1930s and the solidarity of many members of the international scientific community gave Rossi the courage to begin a new life in a new world when he was obliged to leave Italy after the enactment of fascist racist laws in 1938 and were instrumental for his later contribution to the rebirth and development of European physics after the war.

Cosmic-Ray research becomes a branch of physics

Between the end of the 1920s and the beginning of the 1930s, as Enrico Fermi (1901-1954) was building up a research group in Rome which was preparing to explore the nucleus, Bruno Rossi in Arcetri, near Florence, was eager to begin work on some experimental project addressing itself "to the fundamental problems of contemporary physics."[1] Then, in 1929, a paper by Walther Bothe and Werner Kolhörster on the nature of the extraterrestrial penetrating radiation appeared,[2] which was "like a flash of light revealing the existence of an unsuspected world, full of mysteries, which no one had yet begun to explore."[3]

At that time, the problem of the nature of cosmic rays did not actually attract general attention, probably because of the widespread belief that the astonishingly penetrating cosmic rays could not be anything else but gamma-rays of very high energy. Bothe and Kolhörster had used the brand new Geiger-Müller counter, which had an improved sensitivity and performance, if compared with other kind of counters and with ionization chambers. Their innovation was to place two Geiger-Müller counters one above the other, with a 4.1-centimeter layer of gold between them and then record the coincidences between the two counters. From this arrangement they argued that coincidences could be produced only by individual ionizing particles crossing both counters, which they thought to be high energy electrons.

Bothe and Kolhörster's experiment, in providing evidence of the enormous potential of the coincidence method, represented the very first attempt to determine the nature of cosmic rays, and contributed to focus the physicists' interest on the radiation found at the place where measurements were made. Both the novelty of the research topic and the low cost of the research tools were the key ingredients of Bruno Rossi's excitement. In just a few months he built his own Geiger-Müller counters, devised a new method for recording coincidences,[4] and began some experiments. The Geiger-Müller technique which Rossi introduced in Italian physics would play a crucial role in the well-known discoveries made by Enrico Fermi and his group in Rome in 1934.[5]

Internationalism and co-operation

In the meantime Rossi had written to Bothe, telling him that he would have liked to spend some months in his laboratory. Bothe's answer was a positive one, so Bruno Rossi left Arcetri in the late 1930 spring thanks to a grant of the Italian National Council of Research. At the time Germany was nearly a paradise for Italian physicists. Fermi himself had been in Göttingen and Leyden after getting his laurea in 1922. Berlin was then the heart of modern physics, and the 24-year-old Rossi had the occasion of meeting Albert Einstein, Max Planck, Otto Hahn, Lise Meitner, Max von Laue, Walther Nernst, Hans Geiger, and other prominent physicists of the time. In particular, he became acquainted with Werner Heisenberg, and with Patrick Blackett, the latter visiting from England; with both he established a friendly relationship. Blackett was a recognized expert on cloud chambers, so that Rossi asked him about the possibility of sending his collaborator Gilberto Bernardini to Cambridge in order to learn this important technique. Blackett's wife was Italian, one more reason to form close friendship with his Italian colleagues.

Rossi's experiments in Berlin provided a more direct and sharp proof of Bothe and Kolhörster's conclusion, but went further in finding the first hints of the phenomenon of a secondary radiation generated by cosmic rays in lead shields. It is to be remarked that Bothe must have been very confident in the young Italian physicist, so much as to reveal him his own secrets for preparing reliable Geiger-Müller counters.

When Rossi was in Berlin, Bothe and Kolhörster went for an expedition to the North Sea and the northern Atlantic Ocean to study the dependence of cosmic rays intensity on the geomagnetic latitude, even if with a negative result. The existence of this geomagnetic effect, connected with the charged nature of cosmic ray particles, had in fact not been definitely confirmed yet. Bothe made him aware of the extensive work that the Norwegian geophysicist Carl Störmer and his pupils had carried on for years on the very complicated mathematical problem of the motion of charged particles in the field of a magnetic dipole, a good approximation to the Earth's magnetic field. Störmer was interested in the trajectories of particles approaching the Earth in the direction of the Sun, but Rossi found out that the answer he was searching for was contained in a formula derived by Störmer. On July 3, during his stay in Berlin, Rossi sent a short note to the *Physical Review* where he arrived at the following conclusion: besides the already expected latitude effect, a second phenomenon

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[1] Rossi, *Moments in the Life of a Scientist* (Cambridge University Press, 1990) p. 6.

[2] W. Bothe and W. Kolhörster, "Die Natur der Höhenstrahlung," *Die Naturwissenschaften* 17 (1929), 271–273; idem, "Das Wesen der Höhenstrahlung", *Z. Phys*. 56 (1929) 751–777.

[3] Bruno Rossi, *Cosmic Rays* (McGraw–Hill, 1964) p. 43.

[4] Bruno Rossi, "Method of Registering Multiple Simultaneous Impulses of Several Geiger's Counters," *Nature* 125 (April 26, 1930) 636.

[5] Matteo Leone, Angelo Mastroianni, Nadia Robotti, "Bruno Rossi and the Introduction of the Geiger-Müller Counter in Italian Physics: 1929–1934," *Physis* 42 (2005) 453-480.

must exist, the East-West effect, which would be revealed by an asymmetry of the cosmic-ray intensity with respect to the plane of the geomagnetic meridian, with more particles coming from East or West, depending on whether the particle charge was negative or positive.[6]

Rossi must have discussed this topic with Werner Heisenberg, during one of the latter's frequent trips from Leipzig to Berlin, because, on August 19, 1930, Heisenberg wrote him a letter asking to summarize Rossi's calculations on the motion of a high-energy electron in the Earth's magnetic field. Rossi answered only on September 13, during the last days of his German sojourn. After a series of detailed passages, Rossi arrived to the same formula contained in his article on the *Physical Review*. A trace of the Rossi-Heisenberg dialogue on cosmic rays is contained in a series of letters now preserved in the Archives of Padua University (not accessible at the moment) which have been published in 1993.[7] As a theoretical physicists at the frontiers of quantum mechanics, and involved in the nascent quantum electrodynamics, Heisenberg had immediately become aware of the interesting new perspectives opened by the latest experiments on the nature of cosmic rays.

As soon as Rossi was back in Arcetri, he began to work searching the proof of the existence of the East-West effect, which could provide a completely different proof of the corpuscular nature of the cosmic radiation as well as an indication on the sign of the charge. However, within the errors, the experience gave a negative result. Being aware that the asymmetry would become pronounced only at low geomagnetic latitudes and at sufficiently high elevation to afford the observation of cosmic rays of comparatively small energy, Rossi planned an expedition to Asmara, the capital of the Italian colony in Eritrea, a town rising at an altitude of 2370 m, and at a geomagnetic latitude of 11° 30' N.

On November 19 Rossi wrote to Heisenberg in reply to the latter's letter of October 6. Rossi apologized for the great delay, and said that only in the previous days had he been able to look at Heisenberg's notes "on the nature of the Ultrastrahlung". He compared his results on the "forbidden regions" with Heisenberg's ones observing that their results were nearly coinciding, even if they had been obtained by different methods. Rossi announced, too, that he was going to publish a preliminary article on the work carried out in Germany.[8]

By the summer of 1931 Rossi had performed a series of experiments (deviation with an electromagnet and study of intensity of cosmic rays with a counter-telescope at different inclinations to the vertical line) aiming with different means at studying the nature and behavior of the cosmic rays.[9]

In the meantime, from May 20 to May 22, 1931, the first international −but European in character− conference on nuclear physics was held at the Eidgenössische Technische Hochschule in Zürich. Rossi had the occasion to meet Marie Curie, Frédéric Joliot, George Gamow (who delivered the opening lecture), Patrick Blackett, Lise Meitner, Bothe, Rudolf Peierls ,Wolfgang Pauli, Maurice De Broglie and Louis Leprince-Ringuet. At the time they were working in Paris on the artificial disintegration of elements, and like James Chadwick were performing their researches using the valve counter invented by Heinrich Greinacher, as a mean of detecting the passage of protons generated during the disintegration process. From the acquaintance with Leprince-Ringuet, and the interest in the new technique of the valve counter the idea of a visit to Paris aroused, which took place during the following year 1932, thanks to a Volta grant provided by the Accademia d'Italia. It was an important occasion for the exchange of knowledge about counters, and especially in arousing Leprince-Ringuet's interest in cosmic rays: "Le séjour de Rossi fut des plus fructueux: par un echange bien naturel, le laboratoire du duc de Broglie fut initié aux compteurs d'eléctrons et aux coïncidences qui permettent de reconnaıtre les rayons très pénétrants au milieu d'autres, plus mous, et definir leur direction. C'était la porte ouverte vers le monde merveilleux des rayons cosmiques [...]".[10] Rossi did not use the whole grant: he anticipated his return to Italy in order to participate to a competition for a chair of experimental physics. This proved a most lucky circumstance: when he was forced to emigrate in 1938, after the enactment of fascist laws against Italian jews, he requested to complete his stay abroad and was thus able to get some money and the visa on his passport in a short time. After the war, when Rossi was at the Massachusetts Institute of Technology, a tight collaboration was established with Leprince-Ringuet's group at the École Politechnique.

That same 1931, an international conference on nuclear physics was organized in Rome by Enrico Fermi with the support of Orso Mario Corbino. The meeting, to which prominent American physicists such as Robert Millikan and Arthur Compton participated, was held from October 11 to 17. The presence of the leading brains engaged at the frontiers of physics research in the field of nuclear physics and cosmic rays gave the event enormous importance, and put the seal on the new disciplinary identity, helping the young Italian physicists to familiarize with current problems. A well known

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[6] B. Rossi, "On the magnetic deflection of cosmic rays, " *Phys. Rev*. 36 (1930) 606.

[7] G. Gembillo, "Un carteggio inedito tra Werner Heisenberg e Bruno Rossi", *Scienza e Storia* 9 (1993)113-122.

[8] A more complete article summarizing his researches in Germany appeared at the beginning of 1931: B. Rossi, "Über den Ursprung der durchdringenden Korpuskularstrahlung der Atmosphäre," *Z. Phys*. 68 (1931) 64-84.

[9] B. Rossi, "Magnetic Experiments on the Cosmic Rays," *Nature* 128 (3225) (1931), 300-301; idem, "Measurements on the Absorption of the penetrating corpuscular Rays coming from inclined Directions," *Nature* 128 (3227) (1931) 408.

[10] L. Leprince-Ringuet, *Noces de diamant avec l'atome* (Flammarion, 1991), p. 300.

photograph taken during the Conference shows the group of physicists gathering on the stairs in front of the Physics Institute of Via Panisperna. Among the others Marie Curie, Louis Brillouin and Jean Perrin from France; Arnold Sommerfeld Paul Ehrenfest Werner Heisenberg, Walther Bothe, Hans Geiger, Otto Stern, Peter Debye from Germany and Holland; Francis W. Aston, Patrick Blackett, C. D. Ellis, R. H. Fowler and Nevill Mott from Great Britain; Niels Bohr from Copenhagen; Wolfgang Pauli from Zürich; Arthur Compton, Samuel Goudsmit and Robert Millikan from the US. Ernest Rutherford was the great absent, so that a telegram was sent by the group. The fourth day of the conference was dedicated to the problem of cosmic rays and Fermi invited Rossi to give an introductory speech. He made a detailed and cogent discussion, also explaining the reasons why he thought that Millikan's assumption, according to which cosmic rays were "ultra-gamma radiation" resulting from the synthesis of elements in the Universe, could not be correct. Millikan did not like Rossi's discourse; he considered him only an arrogant young man and for a number of years thereafter chose to ignore his work altogether.

On November 11, immediately after the conference, Heisenberg wrote again to Rossi: "I still remember the Roman days with joy, the congress was really one of the best and instructive I ever participated until now." Heisenberg, who had much reflected on these topics during the previous months and after the meeting, asked for Rossi's opinion: "I have lately made some calculations on cosmic radiation and I collected the formulas in a small manuscript. I would be very grateful if you could be so kind as to read it and write me how much its content is trivial, well known, and how much wrong, and then send it back. You probably will laugh at the theoretician's list of wishes [...]". On December 8 Heisenberg asked Rossi to send back his manuscript because he needed it for a discussion, and after 4 days he wrote again thanking him and mentioning something about the final draft of Rossi's article on the absorption experiment in 1 meter lead: "I am really burning with interest in the manuscript you announced."[11] A series of letters exchanged with Heisenberg between December 8, 1931, and March 21, 1932, are discussing Rossi's experiments on the absorption in 1 meter lead, and especially on the secondary radiation generated in the metal screens above the counters. According to what Rossi told later, a paper reporting on this effect was rejected by *Die Naturwissenschaft*, and was accepted by the *Physikalische Zeitschrift* only after Heisenberg had vouched for its credibility.[12] On February 10 Heisenberg thanked Rossi for his "new work" which he had "studied with great interest". Heisenberg also told how curious he was about the "angular distribution of secondary particles". That same spring Heisenberg published a long article discussing cosmic rays where Rossi's experiments were widely quoted.[13]

In the meantime Hans Bethe, who spent some time in Rome with a Rockefeller scholarship, wrote enthusiastically about Fermi to Arnold Sommerfeld. He was going to spend the rest of his Rockefeller fellowship in Cambridge, and was very sorry for having to leave Italy: "The stimulus I have here by Fermi, is larger by orders of magnitude [...] Dirac is well known for speaking only one word per light year, and the other people in Cambridge are far from having the general view of the quantum theory that Fermi has."[14] At that time Bethe had just published a landmark paper on the interaction of radiation with matter.[15] He disagreed with Millikan's views on cosmic rays, but at the same time he regarded Rossi's researches with great favor, and during his stay in Italy he visited Rossi in Florence. They were nearly the same age, and it was easy to establish a human relationship, which later proved crucial in Rossi's life. Actually, in being closely bound to the nascent quantum electrodynamics, the archetype of quantum field theories, Rossi's work was followed by theoreticians such as Heisenberg, Fermi, Bethe, Heitler and Bhabha. The latter, too, spent some time in Rome with Fermi during the 1930s, and well knew Rossi's work which widely quoted in his articles. Later, Bhabha and Heitler used Rossi's data to establish a satisfactory quantitative agreement with their theory of the electromagnetic cascade.

From co-operation to collaboration

Rossi's talk on cosmic rays during the Roman conference had stimulated Arthur Compton and provided the initial motivation for the latter's new research program in cosmic rays. Going back to the US Compton thoroughly studied Rossi's talk at the Rome Congress, as well as other recently published cosmic ray works, and decided to change his main research interest. He organized a worldwide cooperative project involving tens of physicists (with all investigators using identical large ionization chambers) eventually demonstrating that the intensity of cosmic rays is correlated to the magnetic rather than geographic latitude. This was one of the first examples involving so many physicists all over the world working at the same research project.

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[11] B. Rossi, "Absorptionsmessungen der durchdringenden Korpuskularstrahlung in einem Meter blei,'' *Die Naturwissenschaften* 20(4) (1932) 65.

[12] B. Rossi, "Nachweis einer Sekundärstrahlung der durchdringenden Korpuskularstrahlung," *Phys. Z.* 33 (1932) 304--305.

[13] W. Heisenberg, "Theoretische Überlegungen zur Höhenstrahlung," *Ann. Phys*. 405 (1932) 430-452. On his side, during this period Rossi thanked Heisenberg, Fermi and Bothe for discussions in his articles.

[14] H. Bethe to A. Sommerfeld, Rom, April 9, 1931 in M. Eckert and K. Märker (eds.) *Arnold Sommerfeld Wissenschaftlicher Briefwechsel*. Band 2: 1919-1951 (Deutsches Museum - GNT Verlag 2004), pp. 322-323.

[15] H. Bethe, "Zur Theorie des Durchgangs schneller Korpuskularstrahlen durch Materie," *Ann. Phys*. 5 (1930) 325-400.

In September 1933 Rossi and his collaborator Sergio De Benedetti went to Eritrea to carry out the planned experimental tests on the existence of the East-West effect. During the journey from Spalato to Massaua, which took about a week, they measured the dependence of cosmic-rays intensity using a ionization chamber provided by Compton thus participating to his great enterprise.

When Rossi and De Benedetti arrived in Eritrea and observed the existence of a difference in the intensity of cosmic rays between East and West, experiments proving this effect had already been announced in two letters sent to the *Physical Review* by Luis Alvarez and Compton, as well as by Thomas Johnson. The study of the asymmetry predicted by Rossi since the summer of 1930 was now confirming the corpuscular hypothesis of the nature of the primary radiation and revealed that the primaries were largely positively charged.

Compton's interest in cosmic ray research was the beginning of a reciprocal liking between the young Italian physicist and the more mature American Nobel Prize winner which would prove important after a few years, when things turned difficult and Rossi was forced to leave Italy in 1938.

In 1931, Rossi had met again Blackett in Zurich and in Rome, and they certainly reinforced the idea of a collaboration. Blackett's interest in nuclear physics and cosmic rays was triggered by the arrival in Cambridge of Rossi's young and brilliant collaborator Giuseppe Occhialini in the spring of 1931. Occhialini brought in Cambridge Rossi's ideas on cosmic rays and especially the technique of coincidence counting with the Rossi electronic circuit. The cross-fertilization between Rossi's methods and Blackett's extraordinary competence in cloud-chamber techniques represented the beginning of a new era in the physics of detectors. At the moment it provided a clear evidence for the existence of the secondary "showers" observed by Rossi and the proof of the existence of the electron-positron production implied by Dirac's relativistic theory of the electron. After the war, in 1947, Occhialini collaborated with Powell's group to the discovery of the pion. These two scientific adventures have by and large been explored by historians. Here it will suffice to remark that the strong tradition of collaboration established between Italy and Great Britain became very important after World War II, when European physicists became aware that, in order to tackle the new tasks of reconstruction and of competition with the mighty community of American physicists, they had to organize in international groups. Occhialini's sojourn in England is a typical example of a strategy implemented by Italian physicists to learn new experimental practices and theoretical tools outside the restricted national reality, as well as to experience a richer scientific context. During the 1930s Germany (Berlin, Hamburg, Leipzig, Göttingen, Heidelberg, Frankfurt), Great Britain (Cambridge, London), France (Paris), Denmark (Copenhagen), Sweden (Stockholm, Uppsala), Switzerland (Zurich) attracted Italian physicists slightly younger than Fermi.[16]

When Rossi and his wife Nora were obliged to leave Italy on October 12 1938, Rossi was offered a position in Manchester by Blackett, who had just succeeded W.L. Bragg as the Langworthy Professor of Physics. After a short stay in Copenhagen with Niels Bohr ("The human interests, the lively intellectual climate, the sane vision of political events that were the essence of the 'Spirit of Copenhagen' went a long way toward clearing our minds, and strengthening our confidence in the future"),[17] Rossi and Nora moved to Great Britain in the following December. In Manchester, where he remained until the early summer of 1939, Rossi found A. C. B. Lovell, G. D. Rochester, J. C. Wilson, and Lajos Jánossy, a refugee from Hungary. Rossi's arrival in Manchester was instrumental in triggering a brand-new program on cosmic ray researches. During that period, Rossi's tie with Blackett transformed in a lifelong friendship and a collaboration in particle physics.

In June 1939 Rossi and his wife Nora left for the United States. Rossi had been invited by Compton to participate to an international congress on cosmic rays to be held in Chicago. After the meeting, thanks to Compton, Rossi received an offer of a research associate post from the University of Chicago paid by the Committee in Aid of Displaced Foreign Scholars. By accepting this position he made a final decision to remain in the United States and became one of the about one-hundred physicists who immigrated to the United States between 1933 and 1941 fleeing Nazi Germany and Fascist Italy. Later Hans Bethe had a clue role in recommending that he be appointed to fill a vacant associate professorship at Cornell University in Ithaca, New York, where he remained until the spring of 1943, when he moved to Los Alamos and was involved in the Manhattan project.

After the war Rossi accepted a position at MIT, but constantly supported Italian physicists inviting them to work at the Laboratory for Nuclear Science. His group at MIT flourished, becoming internationally renowned and attracting researchers from all over the world. By that time a marked shift had occurred in the center of gravity of physics research towards the United States, where big science was already a reality. Collaboration became a vital necessity for European physicists. Relationships established before the war were at the root of the rapid reorganization and flourishing of post-war physics in Europe. Typical in this sense were cosmic ray researches with balloons during the period 1952-1955. They prepared physicists to work on large-scale international projects, setting the stage to the world-scale networks which would soon characterize research at the newly founded European laboratory for nuclear research (CERN) in Geneva.

[16] I owe to Matteo Leone a clear scheme of the centers visited by Italian physicists during the 1930s.

[17] B. Rossi, *Moments*, cit., p. 41.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 804.

"IF A LOT OF RADIUM WOULD BE SUFFICIENT TO MAKE IMPORTANT DISCOVERIES". VIENNA AS A NODE IN THE NETWORK OF EUROPEAN ATOMIC RESEARCH CENTRES

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Abstract

The paper depicts Vienna as a node in the network of European atomic research centres in the interwar years. On the one hand, it sheds light on the strategy employed by Austrian physicists to maintain a position in the network of European research laboratories, which had been dominated since the early 20th century by the French and British schools. Most notably the lending of radioactive substances to radioactivists at home and abroad will be considered. On the other hand, the paper focuses on the scholars visiting Vienna in the 1920s and early 1930s: Which countries did they come from? What impact did their stay have on the local situation, as well as on their further career abroad? Among the factors influencing scholarly mobility, the role of the International Education Board in promoting Vienna as a research centre of atomic physics is studied in-depth.

The situation before World War I

The first institute entirely devoted to the research of radioactive phenomena was founded in Vienna and not in Paris, as one might have expected. On the occasion of its opening in 1910, the Institute for Radium Research received three grams of radium. It had been produced by order of the Austrian Academy of Sciences which thus initiated the formation of an Austrian radium industry. This industry was based on the exclusive exploitation of uranium-rich Bohemian silver mines in St. Joachimsthal. With the Austro-Hungarian government monopolizing the radium market from 1908 on, radium prices skyrocketed due to an ever increasing medical demand.[1] As the Institute maintained good relations with the local radium industry, Vienna had a competitive edge against other laboratories. Before the beginning of the war, it became an important node in the international network of radioactivity research. This network was based on the exchange of radioactive preparations and apparatus, as well as on the circulation of researchers among the centres in Paris, Berlin, Manchester, and

[1] M. Rentetzi, The U.S. Radium Industry. Industrial In-House Research and the Commercialization of Science, *Minerva*, 46 (2008), 438.

Vienna.[2] Between 1913 and 1922, 19 foreign scientists worked as guest researchers at the Radium Institute. The majority came from the German-speaking countries, some from Scandinavia. Occasionally the Institute welcomed visitors from Belgium, Britain, and from overseas.[3] A considerable part of the visitors hailed from those Habsburg crown lands where conditions for radioactivity research were less favourable than in Vienna.[4]

The situation after the war

The war left the scientific field of radioactivity in a state of devastation equal to that of the military battlefield. Belonging to the war enemies of the Entente powers, Austrian scientists along with their German colleagues were banned from international scientific organisations. Nevertheless, they lost less credit among their peers than the Germans. Despite economic, social and political hardships in the wake of the war, Austrian radioactivists soon tied in with old networks. Their countless appeals to re-enact the 'apolitical' international scientific community of the pre-war period were successful mainly in Britain and the United States.[5] Marie Curie, on the contrary, did not actively seek to revive contacts with Austria until 1923.[6] From the financial viewpoint, the Physics Institutes in Vienna faced dire straits. However, when IEB representatives visited the Radium Institute in the mid-1920s, they assessed the value of radioactive stocks at 200,000 US-dollars, and they had good reason to believe that Vienna disposed of more than enough material to continue scientific work.[7] The Institute had in fact no rivals in terms of access to radioactive materials, except for the Laboratoire Curie in Paris.

Notwithstanding that the Radium Institute's regulations prohibited the hiring out or selling of radioactive parent substances to third parties, Ernest Rutherford bought the radium he had borrowed from the Austrian Academy back in 1908. The money, paid in several instalments, helped the Institute get over its financial difficulties during the 1920s. The Cavendish Laboratory in Cambridge in turn disposed of a comparably large amount of radium which enabled Rutherford to open the new research field of atomic disintegration. The ban was lifted a second time when in 1925 Marie Curie received a gram of ionium-thorium oxide as a loan from the Viennese.[8] Following the rules of a reputation-based exchange economy, Curie was prefered to other less renowned colleagues.[9] As we shall see, the Institute expected advice how to make strong polonium sources in return for the service. There was no such ban on selling or lending out radioactive decay products such as radium 'emanation', rare earths preparations,[10] and radioactive rock samples.[11] The major part of preparations went to the German-speaking radioactive community, Scandinavia, and the Netherlands. These countries became the focal point for Austrian radioactivists who wanted to escape the poor conditions in their home country immediately after the war.[12]

The role of the International Education Board

Apart from the abundant stock of radioactive materials inherited from pre-war times, there were also institutional factors that helped Vienna regain its status in the 'radioactive' network. The International Education Board (IEB) played a pivotal role in promoting the city as a centre for atomic research during the 1920s. According to John D. Rockefeller's will, the money entrusted to the IEB should be spent on the promotion of the educational system all over the world. Wickliffe Rose, director of the IEB from 1923 to 1928, ultimately aimed at fostering the industrial and agricultural education in the newly founded states of Central and Eastern Europe. As larger aims of social reform turned out to be unattainable, Rose

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[2] J. Hughes, "The Radioactivists. Community, Controversy and the Rise of Nuclear Physics", unpublished Ph.D dissertation, University of Cambridge, 1993, Chapter 1.

[3] Meyer to Dopsch, 8 January 1923, Stefan Meyer Papers, Archive of the Austrian Academy of Sciences Vienna.

[4] See on the situation in Prague: Hönigschmid to Meyer, 15 November 1912, Meyer Papers, and on the migration of Hungarian physicists to Paris and Vienna G. Palló, "Skepticism and Interest: the Hungarian Reception to Radioactivity", in: D. Hoffmann (ed.), *The Emergence of Modern Physics. Proceedings of a Conference Commemorating a Century of Physics Berlin 22-24 March 1995*, (Pavia: Univ. degli Studi, 1997), 128.

[5] Boltwood to Meyer, 26 February 1915, and Lind to Meyer, 18 January 1921, Meyer Papers.

[6] Curie to Meyer, 1 May 1923, Meyer Papers.

[7] A. Trowbridge, Memorandum of conversation with S. Meyer and K. Przibram, 26 March 1925, IEB, Series 1.2, Box 25, folder 360, Rockefeller Archives Centre Sleepy Hollow.

[8] Curie to Meyer, 30 December 1925, Meyer Papers.

[9] Meyer to Sächsische Landeswetterwarte, 9 September 1925, Meyer Papers.

[10] Hahn to Meyer, 26 March 1924, Meyer Papers.

[11] Meyer to Hönigschmid, 21 February 1920, Meyer Papers.

[12] Kamerlingh Onnes to Meyer, 26 June 1920, Meyer Papers; Meyer to Oséen, 27 October 1920, Oséen Papers, Center for History of Science Archive Stockholm.

rested his hopes on the training of a small and highly educated elite.[13] The national elites should be trained in regional centres supported by IEB. While the IEB spent large sums on founding new laboratories in the United States, contributions were made primarily toward already existing European laboratories. Unlike Cambridge, Paris, or Berlin, Vienna never featured on the IEB's lists of the world's outstanding physical laboratories.[14]

Augustus Trowbridge, at the time head of the IEB's science department, advanced the idea of promoting international migration of promising young scholars with the aid of scholarships and grants-in-aid, supplementing the efforts of national governments. In line with Rose's strategy to promote Central and Eastern European countries, Trowbridge assigned a special task to the Radium Institute in Vienna. In the spring of 1925 he explained to his fellow trustees: *"It is A*[ugustus]*.T*[rowbridge]*.'s own impression that it* [the Radium Institute] *is a case of a good institution, fairly well equipped with a splendid position to serve the entire South-East of Europe, as a center at which good research work in an important branch of modern physics is being done, but being done under what to us westerns would seem almost impossible working conditions for the lack of almost everything we are accustomed to have in such an institute.* […]*; most of the students are from the lower Danube regions, where conditions are still worse, where even the foreign journals are not available, as they are here, thanks to the Institute's being able to effect exchange through its publication.* […] *there is no lack of advanced students who seem somehow to be able to live in Vienna and carry on work; It seems* [...] *that for the next year or two, it would be better not to grant fellowships to Hungarians, Czechs, Poles or Bulgarians for the study of physics in Western Europe, but rather let them seek what is still the natural center, Vienna, where they can partially support themselves while they carry on their work.* […] *The Radium Institute of Vienna ought to be a natural feeder from South-eastern Europe to the laboratories of Madame Curie and Sir Ernest Rutherford.* […] *the Institute is serving a very valuable purpose in keeping alive a spirit of the value of research in a pure science in a country which is the natural cultural center of a vast region."*[15]

Contrary to what Trowbridge and his fellow trustees had expected, the majority of foreign radioactivists visiting Vienna during the early 1920s did not hail from Central and Eastern European countries. By contrast, there was a considerable inflow of visitors from Western and Northern Europe to the Institute. Vienna became a *"new Mecca"*[16] for young Scandinavian, Dutch, and German radioactivists, to no small part because of the country's decayed economic circumstances. Runaway inflation made a stay in Austria affordable for researchers from countries with relatively stable monetary systems. Besides, formerly neutral countries such as Sweden or Denmark hardly made any political reservations against Austria.[17] In scientifically backward countries such as Hungary, Poland, Spain, or Czechoslovakia, fellowships were provided to enable the junior scientific staff to get a training in important research centres abroad.[18] Among scientists from the successor states of the Habsburg monarchy, however, cultural and political alienation contributed to the dwindling attraction of the Radium Institute. Many a man tried instead to reactivate contacts dating from pre-war times to Britain, France, and Germany. František Běhounek, for example, co-founder of the state-run radiological institute in Prague, made contact with Marie Curie in Paris. Stanisław Loria, Marian von Smoluchowski's successor on the chair of physics at the University of Lviv, addressed his former supervisor, Rutherford in Cambridge.[19]

There is some evidence that the Laboratoire Curie in Paris attracted a larger part of scholars from Central Europe. Marie Curie not only strongly promoted the training of young scientists from these countries.[20] She also maintained close affiliations with a number of home and foreign philanthropic foundations. There was thus sufficient prospect that the Czech, Polish, or Hungarian scholars would be awarded a grant at her laboratory.[21] What was more, the Radium Institute clang to its long-established policy to first and foremost welcome post-doc and senior scholars from abroad. Its director Stefan Meyer unwarily made the case for his Institute, pointing to the great number of scholars who had worked in Vienna and who had come to take up a professorship in the meantime. Most of the former visitors held professorships either in Austria, or in the successor states of the Habsburg monarchy.[22] The IEB's initial strategy to promote the Institute as a training centre for Central and South Eastern European scientists worked out only partially, however. On the occasion of a trip to Vienna in the

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[13] R.E. Kohler, Science and Philanthropy: Wickliffe Rose and the International Education Board, *Minerva*, 23 (1985), 76-77, 84-85, 94.

[14] List of the world's outstanding physical laboratories, 3 October 1923, and list of outstanding European and American workers in the field of physics and list of science centers in Europe, 3 May 1927, RAC, IEB, series 1, subseries 1, folder 144 and 146.

[15] A. Trowbridge, Memorandum of conversation with Stefan Meyer and Karl Przibram, 26 March 1925, RAC, IEB, series 1.2, box 25, folder 360.

[16] Hevesy to Meitner, 22 June 1921, Meitner Papers, Churchill College Archive Centre Cambridge.

[17] Knaffl-Lenz to Meyer, 1 May 1921, Meyer Papers.

[18] Trowbridge to Rose, 8 June 1926, RAC, IEB, series 1.1, box 24, folder 345.

[19] Běhounek to Curie, 18 September 1921, Musée Curie Historical Archives Paris; Loria to Rutherford, 29 March 1921, Rutherford Papers, Cambridge University Library.

[20] D. Pestre, *Physique et physiciens en France, 1918-1940, 2. éd.* (Paris: Éd. des Archives Contemporaines, 1992), 157, 163.

[21] Curie maintained close contacts with a number of smaller French foundations, in addition to the Carnegie-Curie-Foundation and the IEB. A. Schürmann, Marie Curie und ihr Laboratoire: Frauenförderung avant la lettre?, *Feministische* Studien, 24 (2006), 1, 36.

[22] List of Professors who had formerly been working at the Radium Institute, November 1927, Meyer Papers.

mid-1920s, Trowbridge noticed *"a great number of young scientists from all through the Balkans* […]*, working at the various institutes connected with the University"*[23] It is peculiar to the IEB's failed strategy that not a single beneficiary of its fellowship or grant-in-aid-programme working in Vienna hailed from Central or South Eastern Europe. The majority of scientists, among them a remarkably high amount of women, either worked without being paid, or received smaller grants from the German Notgemeinschaft, or other private donors.

Among the foreigners who came to Vienna in the early 1920s was Hans Pettersson. Even though many Swedish laboratories were comparatively well equipped, Pettersson found the necessary radioactive sources he needed for his work at the Radium Institute.[24] The Swedish physicist set up a small group of scientists to investigate the process of artificial disintegration of atomic nuclei. The Viennese group grew in size to almost 30 persons in the 1930s, including chiefly scientists from Austria, but also from Bulgaria, Hungary, Germany, Sweden, and the United States. Placing high hopes on Pettersson's stimulating effect, the IEB provided a sum totalling 69,725 US-Dollars by 1935 for atomic disintegration research in Vienna.[25] This sum was far below the money given to the Cavendish, or most of the American universities. However, Curie's laboratory in Paris, or the lab of Heike Kamerlingh-Onnes in Leiden received less money than the Viennese. The German Notgemeinschaft, too, granted rather generous aids for the purchase of apparatus, and it provided fellowships. Apart from German and US-funding, Pettersson succeeded in raising funds from Swedish philanthropic societies and patrons.

The Viennese atomic research group was highly productive: During the first six years they published 50 papers, one monograph on artificial disintegration, and several comprehensive reports. Their experiments gave rise to a lengthy and heated scientific controversy with the Cavendish over the fissionability of light elements.[26] As the group became more and more visible within the scientific community, the IEB's objective in supporting the Radium Institute gradually changed. In 1926, its officers came to the conclusion that *"in radio-active work* [there is] *a real need of obtaining more data on which to develop further theoretical considerations. If there is only one center, provisional hypothesis is likely to result in the field not being thoroughly covered, whereas the existence of two centers, not in complete harmony as to facts nor theory, leads to a more careful criticism of the work of each group and is extremely important for the development of subjects"*.[27] Pettersson and his co-workers came off as the losers of the controversy, and a rather negative image of the group's alleged policy of spreading erroneous results persisted among their international peers. Nevertheless, the IEB decided to continue supporting atomic research in Vienna. The funding was in line with the Board's general policy to encourage competition among various laboratories, in order to advance and to counterbalance the overpowering Cambridge school: *"In* [Cambridge] *Physics there seems to be a slightly unhealthy feeling of superiority, which is due, of course, to the fact that for the past twenty-five years the English have certainly led in that field. They are perhaps a trifle too 'smug' and they are certainly a bit too 'inbred'."*[28]

While the outcome of the controversy did not provoke the Americans to stop their funding, it certainly made it harder for Viennese scholars to pursue a career in science abroad. Pettersson was not the only one who had a hard time continuing his career in Sweden after his return in 1930. As the Austrian physicist O.R. Frisch –who had set off for research work in Berlin earlier– recalled: *"My supervisor, Karl Przibram, told me with sadness in his voice, 'You will tell the people in Berlin, won't you, that we are not quite as bad as they think?' I failed to persuade them."*[29] Apart from Pettersson, none of the physicists involved in atomic research in Vienna during the interwar period gained enough reputation to be considered for a professorship abroad.

New industrial ties and the Viennese polonium network

The fact that the United States became the world's largest provider of radium had put Austria's powerful position into question well before the end of the war. Moreover, with the demise of the Habsburg Empire, Austria no longer ruled over the Bohemian mines. Carl Ulrich, the director of the Imperial radium factory in Joachimsthal, was fired in 1918. Working as an unpaid researcher at the Radium Institute, he became crucial in re-building ties with the radium industry. The Union Minière du Haut Katanga, a Belgian mining company, had begun to exploit uranium ores in the Katanga region in 1921. Well aware of the Institute's excellent relations with the Austrian radium industry, the company hoped to get advice on the industrial

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23 Trowbridge to Rose, 2 April 1925, RAC, IEB, series 1.2, box 25, folder 360.

24 Pettersson to Meyer, 4 June 1922, Meyer Papers.

25 IEB appropriations arranged in order of magnitude of total support given to the various projects, [ca. 1935] RAC, RF, RG 3.1, series 915, box 1, folder 2.

26 R.H. Stuewer, "Artificial Disintegration and the Cambridge-Vienna Controversy", in P. Achinstein and O. Hannaway (eds.), *Observation, Experiment and Hypothesis in Modern Physical Science* (Cambridge, Mass. and London: M.I.T. Press, 1985), 239-307.

27 Trowbridge to Rose, 2 November 1927, RAC, IEB, Series 1.2, box 25, folder 360.

28 W. Rose, Memorandum, 8 June 1926, RAC, IEB, Series 1, Subseries 1 Box 24, folder 345.

29 O.R. Frisch, "How It All Began", *Physics Today* 20 (11), November 1967, 44.

production of radium. Indeed, with the aid of Viennese researchers the company began to produce radium in 1922, and it soon drove the American producers out of the market, securing a monopoly for almost ten years. Even though the Union Minière entertained much closer industrial relations with the Curie family later on, it sold rather large amounts of radioactive material to the Radium Institute at production costs or even provided it for free in exchange for expert advice.

Belgian shipments to Vienna included basic raw material for the production of polonium. Already in 1918 the Radium Institute had started to produce polonium from Austrian radioactive material at small scale and at irregular intervals.[30] The capacity of the early polonium preparations was however limited.[31] Given their short half-life, a continuous production was necessary. With the methods known at the time, this was a tedious task. Besides, a lot of residue input was necessary. Polonium sources took on greater significance in the mid-1920s, after the Viennese physicist Ewald Schmidt had discovered their suitability as α-ray emitters for experiments on atomic disintegration. In order to save their own resources, the Viennese obtained a strong preparation from the Curies in exchange for a gram of ionium-thorium oxide. Contrary to the Viennese, the Curies decided against partitioning their polonium stocks on several small plates. Their ultimate aim was to concentrate the material available in one strong preparation.[32] Inviting the Hungarian radiochemist Elisabeth Rona to Paris, they acquainted her with the French method of concentrating and purifying polonium from radium D solutions.[33] Back in Vienna, Rona refined Curie's method, and before long the Viennese outperformed Paris in producing the strongest polonium preparations. [34]

The new sources allowed for crucial atomic disintegration experiments at the Radium Institute.[35] With a number of European laboratories entering the field of atomic research in the aftermath of the Vienna-Cambridge controversy, the demand for strong polonium sources grew. Whereas James Chadwick of Cambridge used a strong polonium preparation made from radon capsules he had obtained from a hospital in Baltimore,[36] many other atomic physicists had insufficient access to polonium. Unsurprisingly, requests for a share of the Viennese stock became more frequent in the late 1920s.[37]

The Radium Institute distributed its supplies rather strategically. In order to give the Viennese group a leg-up, only the weaker preparations were sent to colleagues abroad.[38] Those scientists who were ready to contribute to solving the controversy were most likely to get a polonium preparation. One of the first was the Institute of Theoretical Physics in Halle, Germany. By means of a Viennese polonium source, Heinz Pose showed the existence of discrete energy levels in the nucleus, while studying the nuclear reactions of aluminium nuclei bombarded with α-particles. He was the first to describe the effect of resonance transformation in a nuclear process.[39] The strong Viennese polonium sources thus became the basis of a new exchange network, opening doors for cooperation between the Austrians and their colleagues abroad.[40]

From feast to famine – Nuclear research in Austria during the 1930s

The discovery of artificially induced radioactivity in 1934 signalled the end of a period in which research was constrained by the scarcity of radioactive ores. As natural radioactivity gradually disappeared from view as a source for and object of research, radium tended to lose its capital value within the scientific community, which decreased at the same time its strategic value for the Austrians. With the gradual introduction of large scale apparatus into nuclear physics, scientists became more and more dependent on state funding and private philanthropy in order to gain access to the new tools. Meanwhile, the Rockefeller Foundation had changed its policy, focusing financial support on the life sciences and selected areas of geography. In Vienna, neither the technical equipment available, nor the scientific specialisation of the nuclear research group members permitted a reorientation towards the new funding policy in the short term.[41] The German

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[30] S. Meyer, Memorandum re further processing of radioactive substances, 5 June 1918, K.k. Ministerium für öffentliche Arbeiten XVII 1918.

[31] Meyer to Vattenfallsstyrelsen Kraftverksbyran Stockholm, 9 March 1921, Meyer Papers.

[32] S.R. Weart, *Scientists in Power* (Cambridge, MA: Harvard University Press, 1979), 41.

[33] Curie to Meyer, 26 May 1926, Meyer Papers.

[34] Karlik to Meyer, 11 September 1931, Meyer Papers.

[35] Pettersson to Jones, 24 July 1933, RAC, RF, RG 1.1, series 705D, box 3, folder 25.

[36] J. Chadwick, "Some Personal Notes on the Search of the Neutron", in: *Proceedings of the 10th International Congress of the History of Science, Ithaca 26 VIII 1962-2 IX 1962* (Paris: Hermann, 1964), 159-162.

[37] Jaffé to Meyer, 10 April 1928; Ising to Meyer, 21 February 1929; Grainacher to Meyer, 30 April 1929; Raudnitz to Meyer, 30 October 1929, Meyer Papers.

[38] Meyer to Hoffmann, 8 March 1929, Meyer Papers.

[39] Smekal to Meyer, 7 March 1931, Meyer Papers.

[40] L.W. Jones' trip to Germany, Czechoslowakia, Hungary, Austria & Italy, 14 April 1930, RAC, RF, RG 12.1, box 64.

[41] S. Fengler, "Wüstentrockenheit auf dem Gebiet der Atomzertrümmerung". Zur politischen Ökonomie der österreichischen Kernforschung in der Zwischenkriegszeit, in: S. Fengler and C. Sachse (eds.), *Kernforschung in Österreich – Wandlungen eines interdisziplinären Forschungsfeldes, 1900-1978*, (Vienna: Böhlau, 2011), forthcoming.

Forschungsgemeinschaft likewise cut back its funding of Austrian science after 1933, mainly for political reasons. With Pettersson's return to Sweden, Swedish philanthropists also stopped their funding. The Austrian ministry of education took the withdrawal of funding from abroad as an excuse to suspend further support for nuclear research. Compared to other centres of nuclear research at the time, the Viennese group lacked an influential spokesman to speak out against the drastic measures. The lack of funding, as well as the withdrawal of key personnel created a grave crisis for Austrian atomic research from the mid-1930s on. The Viennese high hopes of catching up with international developments within the German nuclear research programme after 1938 were soon dashed. In the course of the war, Austrian nuclear science became once and for all marginalized.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 810.

THE ROLE OF PHYSICISTS IN THE «REBIRTH OF A SCIENTIFIC MOVEMENT» IN PORTUGAL DURING THE INTER-WAR PERIOD[1]

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Abstract

The advent of the Republic in 1910 brought with it important reforms to higher education in Portugal. In spite of severe political and financial difficulties of the next two decades, Portuguese universities financed the graduation studies of few young teachers abroad. Despite these relevant initiatives the Portuguese republican government was not successful in creating a national institution capable of promoting and funding the organization of scientific research in the country. Only in 1929 did the government set up this kind of institution, the Junta de Educação Nacional *(JEN) [«National Board of Education»], with the aim of funding research centres in the universities and of providing grants for students to continue their advanced studies both at home and abroad. In the thirties, an important group of young scientists continued their studies in renowned European universities and laboratories where some of them attained their PhD degree. Paris University was the most sought after university for post-graduation in Chemistry, Mathematics and Physics and the Curie laboratory received an important group of Portuguese students during this period. After four years abroad and having established important international scientific links, some of these young scientists returned home and played an important role in their universities promoting the* rebirth of a scientific movement. *This lasted until the end of the Second World War, when several of them were dismissed from their positions because of their active opposition to Salazar's dictatorship.*

[1] This work is financed by funds FEDER through the Operational Competitiveness Factors Program (COMPETE) and national funds through FCT (Foundation for Science and Technology) by the project HC/0077/2009.

The Iberian Peninsula: Portugal and Spain

The two countries in the Iberian Peninsula saw the end of the nineteenth century with the bitter taste of the political humiliation.

In Portugal, after the shameful British ultimatum made following the Berlin conference, an attempted coup to install a Republic took place in 1891 and was defeated; the Portuguese monarchy was going through a period of slow death. This political decline of the monarchy was associated with the political, economic and financial crisis felt all over the country. There was a strong belief that a Republic was imminent and that only the republican spirit could contribute to the complete regeneration of the country, both intellectually and historically.

In the same period, Spain was deeply wounded because of the humiliating military defeats inflicted by the United States and consequent loss of its colonial empire in America and Asia. The country sought to respond to this situation of political, economic and social deep depression. As a consequence of this reaction, a very strong intellectual and civic responsibility movement was born with the main purpose of promoting a profound modernization of Spanish culture, especially at a scientific level. This movement, supported by eminent intellectuals (Ramon y Cajal (1852-1934), Nobel Prize winner in physiology in 1906) stimulated the creation of the *Junta para Ampliación de Estudios* (JAE) [«Board for Advanced Studies»], a very important institution which was responsible for the major transformations in the Spanish scientific renewal. United in humiliation and defeat, albeit suffering different effects (Portugal did not lose its overseas territories), both nations tried to find a remedy for the crisis.

The impact of the JAE (*Junta para Ampliación de Estudios y Investigaciones Científicas*) on the Iberian cultural world was also considerable. In Portugal, scientists and scholars followed JAE's path closely. The Spanish institution was created in Madrid on January 15, 1907; seven months later, the Portuguese government organized a system of scholarships to finance studies abroad, but was unable to transform this initiative into a permanent new organization. The government decree states: «similarly to what several modern nations have done, with proven and enviable success, we will seek to take advantage of the educational experience of more advanced countries in Europe, by sending a large number of Portuguese students to their schools »[2].

The transformation of Portugal into a Republic in October 1910 triggered a number of important initiatives in the educational field. In 1911, the University of Coimbra (the oldest and only university in Portugal over the past centuries) was reformed and the institutions of higher education in Lisbon and Porto were reorganized on a new basis: two universities were founded in the same year in those major cities. Republican reformists assigned the universities a definite number of goals. Among them, the promotion of scientific research, the systematic study of problems regarded as of national interest, and the transmission of culture directly from the universities to other sectors of the population. In spite of financial difficulties, the Portuguese universities supported the graduation studies of few young teachers abroad and some of which went to important European laboratories. Despite these important modifications, the Portuguese government was unable to organize an institution similar to the Spanish JAE with the purpose of coordinating the national scientific policy. It is worth noting that all the plans to reform the universities were implemented in an environment dominated by the old corporation of full professors who kept their old chairs in the new universities.

After the end of the First World War, in 1918, and during the next decade there were several attempts to create a scientific institution capable of promoting a pedagogical and scientific renewal. In 1924, the minister of education, António Sérgio (1883-1869)[3], submitted to the Parliament a diploma to create the *Junta de Orientação dos Estudos* [«Board for Studies Orientation»]. As in previous attempts, the Parliament did not approve the project because of financial restraints and of the differing approaches of the various political groups towards education problems.

Between 1916 and 1924, several Portuguese teachers and students went to Madrid to visit JAE[4] and to collect information about its methods and policies.

Medical doctors were the dominant force in the Portuguese scientific community and it is worth noting the importance of this group and its enthusiastic action for the creation of an institution similar to the Spanish JAE. Two of them were of particular relevance: Augusto Celestino da Costa (1884-1956), full professor of the Faculty of Medicine in Lisbon and his

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[2] In government decree of 31 May 1907.

[3] A central personality of the Portuguese intellectual group during de first Republic, he introduced in Portugal modern ideas concerning education, history and philosophy. He was an "intellectuel engagé" in the Portuguese first half of the 20th century and his ideas and action influenced several generations of intellectuals.

[4] Costa, A Celestino da (1918). *A universidade portuguesa e o problema da sua reforma*. Lisbon. Raposo, L. Simões (1929). Considerações Gerais, in *Relatório dos trabalhos da Junta Lisboa*.

scientific disciple, and Luís Simões Raposo (1898-1934); both were Ramon y Cajal admirers and had scientific and institutional contacts with him[5].

«Junta de Educação Nacional» – JEN

After the 28th of May 1926, with the country under a military dictatorship with no plans to innovate an education system and suffering from a severe funding restrictions, the Minister of Education, following the orientation of his predecessor, approved in the promulgation of a Decree that created the *Junta de Educação Nacional* (JEN) [«National Board of Education»]. The government assigned the indispensable financial resources for its operation. The main important contributions of «JEN»'s program were «to found, improve or subsidize institutions devoted to scientific research (…) to organize and monitor a grants service to be used in Portugal and abroad (…) to promote locations for the scientific work of the ex-grant holders»[6]. The new *Junta de Educação Nacional* (JEN) was officially created in 1929 with Simões Raposo as its Secretary. Following the Spanish JAE example, the new Portuguese institution concentrated initially on offering research scholarships in Portugal and abroad, with the further goals of promoting scientific research at the higher levels of university education, and the transfer to Portugal of the technologies needed for a better management of the country and its colonies. It also planned to support existing research centers, encourage scientific publications and to promote wider cultural exchanges with foreign countries. The JEN policy of sending a large number of students abroad was an implicit recognition of the inability of the Portuguese universities to produce the graduates Portugal needed to work in pure and applied sciences.

In the beginning of its activity, JEN gave some funding to «institutions, public or private, whether or not incorporated in universities or Technical Schools, provided these institutions showed: (…) that their practice was directed exclusively to scientific research (…) that they had good results in recent years, and that their work was cited or earned the appreciation of national and international experts». The physics laboratory of the University of Lisbon was one of the first group of institutions that received JEN's funding. In 1931, the board of JEN declared that «in future it [JEN] will create or develop laboratories or other centers devoted to new special studies, not yet available in the country, where grant holders recently arrived from abroad can work»[7]. Indeed, some years later, JEN organized several scientific research centers, in the fields of mathematics, natural sciences, philology and history that were integrated in the three Portuguese universities. These research centers fundamental objective was to support the work of returning grant students from abroad and also to receive scholarships to pursue research and graduation. The JEN board was aware that the country was in a «precarious situation, mainly in industry and agriculture, which are technically very poor, and that a sound technical preparation of engineers and agronomists will be a necessary condition to promote the progress of the Portuguese economy». The returning grant students contributed in a very special way to the emergence of new scientific structures that supported technical and scientific research.

Although up to 1934-1935 JEN budgets were, on relative terms, quite limited, they supported 173 scholarships abroad (see table 1[8]). Natural sciences (Biology, Chemistry, Geology, Physics) and Mathematics students was the group that received more scholarships (38 and 50), Medicine and Biomedical sciences deserved the second place with 25 and 30 scholarships. France was the preferred country with 34 and 38 grants, followed by Germany and the United Kingdom; Belgium, Italy, Spain, Switzerland received also Portuguese grant holders. Also a number of small grants were given for shorter stays abroad, of up to three months. During the same period JEN also awarded 90 grants (for periods over than one year) to be used in Portuguese institutions: the first place in this distribution was for medicine with 43 and the second one was for natural sciences (Biology, Geology, Physics and Chemistry) with 40 grants[9].

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[5] *Ibid*.

[6] JUNTA DE EDUCAÇÃO NACIONAL. (1931). *Relatório dos trabalhos efectuados em 1928-1929*. Lisboa

[7] JUNTA DE EDUCAÇÃO NACIONAL. (1932). *Relatório dos trabalhos efectuados em 1930-1931*. Lisboa.

[8] Gomes, Emília Vaz, Fitas, Augusto J.S. e Nunes, Fátima. (2009). The Rebirth of a «Scientific Movement» and the Foundation of the National Board Of Education [Junta De Educação Nacional] During The First Period Of Dictatorship Regime In Portugal, *XVIIIe Congrès International d'Histoire des Sciences et des Techniques (Budapeste).*

[9] Gomes, Emília Vaz, Fitas, Augusto J.S. e Nunes, Fátima. (2009). The Rebirth of a «Scientific Movement» and the Foundation of the National Board of Education [Junta de Educação Nacional] During The First Period of Dictatorship Regime in Portugal, *XVIIIe Congrès International d'histoire des Sciences et des Techniques (Budapeste).*

Table 1.

	Scientific domains	Number of grants	France	UK	Germany	Other countries
Grants up to one year	Sciences (Astronomy, Biology, Chemistry, Mathematics, Geology and Physics)	38	17	11	11	13
	Agronomical Sciences	7	2	4	0	1
	Engineering	10	3	2	3	2
	Medicine and Biomedical sciences	25	11	2	10	16
	Other	2	1	0	0	1
	Total	**82**[10]	**34**	**19**	**24**	**33**
Grants for periods over than one year	Sciences (Astronomy, Biology, Chemistry, Mathematics, Geology and Physics)	50	24	12	10	3
	Agronomical Sciences	4	0	2	0	2
	Engineering	6	4	0	2	0
	Medicine and Biomedical sciences	30	9	3	16	3
	Other	1	1	0	0	0
	Total	**91**	**38**	**17**	**28**	**8**

The «rebirth of a scientific movement»

In the thirties, an important group of young researchers furthered their studies abroad in renowned European universities and laboratories, where some of them were awarded their PhD degree. the University of Paris was the most sought after school for post-graduation studies in Chemistry, Mathematics and Physics. During this period the Curie laboratory received an important group of Portuguese students. Table 2 lists the Portuguese researchers who worked in the Curie Institute[11], also young physicists who studied in other European institutions and the very special case of a Mathematician who gained his PhD in Paris.

The physicist Manuel Valadares (1904-1982)[12] and the mathematician Antonio Aniceto Monteiro (1907-1980)[13] are two paradigmatic examples of a very special attitude in changing the scientific environment when returning to their Universities in Portugal. They were the main players in the «*rebirth of a scientific movement*»[14] which was characterized by a full-time dedication to the research beyond the allotted time for classes, when researchers were university teachers, the maintenance and strengthening of ties with the international scientific community, the publication of their scientific research results in international journals and by a strong commitment to encourage other young researchers with the purpose of creating a research group (replicating their own practice abroad).

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[10] There were some cases where the same grant was for two countries.

[11] Archives de l'Institut du Radium (Paris).

[12] Salgueiro, Lídia, (1978), Vida e obra de Manuel Valadares, *Gazeta de Física*, vol.VI,2-12; *Portugaliae Physica*.13 (1982): 121-122, I-VII.

[13] Fitas, A.J., (2008), As relações entre António Aniceto Ribeiro Monteiro e a Junta de Educação Nacional ou um bolseiro português na cidade de Paris (do Outono de 1931 à Primavera de 1936), *Boletim da Sociedade Portuguesa de Matemática*, Número especial AARM: 89-128.

[14] This «*movement*» extended to other domains of pure and applied sciences, although this study only relates to physics, mathematics and chemistry.

Table 2.

NAME (Born/Univ./ Grant period)	**PhD dissertation**
Herculano Amorim Ferreira (1895/Lisbon/ 1929-31)	Scientific work in Imperial College on Opticks and **PhD by the University of Lisbon**.
Amaro Monteiro (1898/ Lisbon / 1937-8)	Scientific work in Paris (lab. Maurice Curie) and **PhD by the University of Lisbon**.
Branca Edmée Marques (1899/ Lisbon / 1931-5)	**PhD by the University of Paris**. (supervisors Mme Curie e J. Debierne). N*ouvelles recherches sur le fractionnement des sels de Barium radifère.*
Manuel Valadares (1904/ Lisbon / 1930-3)	**PhD by the University of Paris**. (supervisors Mme Curie). *Contribution à la spectrographie, par diffraction cristalline, du rayonnement gamma*.
António da Silveira (1904/ Lisbon / 1929-32)	Scientific work in Collège de France (lab. Paul Langevin), he did not get his PhD.
Manuel Teles Antunes (1905/ Lisbon / 1933-5)	**PhD by the University of Madrid**. (supervisor Miguel Catalan). *Estructura del esoectro del cobalto neutron*.
Aurélio Marques da Silva (1905/ Lisbon / 1935-8)	**PhD by the University of Paris**. (supervisor F. Joliot): *Contribution à l'etude de la materialization de l'energie*.
João Almeida Santos (1906/ Coimbra/ 1930-5)	**PhD by the University of Manchester.** (supervisors: W.L.Bragg e J. Welsh) *An X-ray investigation into the structure of anhydrous cobaltous chloride, CoCl2 – at room and very low temperatures – and caesium, rubidium and thallium salts of certain 12-heteropoly acids*.
António Aniceto Monteiro (1907/ Lisbon / 1933-6)	**PhD by the University of Paris**. (supervisor: Maurice Fréchet). *Sur l'additivité des noyaux Fredholm.*
Armando Gibert (1914/ Lisbon / 1942-6)	**PhD by the Suiss Federal Polytechnic School in Zurich.** (supervisor Scherrer). *Effect de la temperature sur la diffusion neutron-proton*.

These two young scientists started this movement with a very specific task: to re-unite in the country their fellow colleagues who had studied in Paris with scholarships. This group, the so-called «Physics, Mathematics and Chemistry Group», operated as a private Association whose members were young scientists that had furthered their studies in Paris and senior professors of Portuguese universities that were really interested in promoting scientific research (a very small group of iconoclast full professors). This scientific association promoted in Portuguese universities, open courses on contemporary physics and mathematics topics that were not taught in university graduation courses. This initiative caused a strong perturbation in the conservative Portuguese *academia*[15].

It is important to compare the activities of these two young «scientific agitators» after their return to Portugal which is chronological and briefly shown in table 3[16].

The physics group in Lisbon

Of all these young researchers, the physicist Manuel Valadares is undoubtedly one of the main characters in this story. In a very meticulous and organized way and with total dedication, Valadares built the first Physics Laboratory Group for experimental research in Portugal. An enormous tenacity, the use of a strategic plan and with an excellent talent for organization characterized his work. He never complained about the deficient conditions in which he had to carry out his research work, instead he always tried to do high quality and up to date research in the real conditions he had to face. The strategy he developed with total agreement of the full professor of Physics Lab in Lisbon[17] was, in his own words, the

[15] Gil, Fernando Bragança (2003). Núcleo de Matemática, Física e Química: uma contribuição efémera para o movimento científico português, *Boletim da SPM*, 49: 77-92.

[16] Fitas, Augusto J. S. and Videira, António A.P. (2004). *Cartas entre Guido Beck e Cientistas portugueses* (Introdução e organização). Lisboa: Instituto Piaget.

[17] Armando Cyrilo Soares (1883-1950).

following: «The experience I gained abroad showed me that it was the specialized research laboratory that gave higher yield, particularly in research centers that had limited funding and insufficient scientific staff»[18].

Table 3.

	Antonio Aniceto Monteiro	**Manuel Valadares**
1937	***Portugaliae Mathematica*** *(Portuguese international Journal on mathematics. Ongoing publication to the present day)*	Research group on Physics strongly related with the research he had done in Paris.
	The **Physics, Mathematics and Chemistry Group**, a private Association of young researchers and senior researchers really interested in promoting research in pure science…	
1938	Organization of the Mathematics Seminar in Lisbon.	Research group on Physics, strongly related with the research he had done in Paris.
1939	***Gazeta de Matemática*** *(A mathematics journal publishing papers on the teaching of mathematics and on topics such as contemporary mathematics, applied mathematics —all aimed at the introductory-level teacher; ongoing publication to the present day)*	
1940	**Mathematics Seminar becomes the Mathematical Research Center of Lisbon supported by JEN/ IAC.** Five young Mathematicians who started working in this Center went abroad with scholarships (Zurich and Rome).	**The research group on Physics becomes the Physics Research Center of Lisbon supported by JEN/IAC.** Armando Gibert, a young researcher of Valadares' group got a scholarship to study in Zurich.
1943		***Portugaliae Physica*** *(Portuguese international Journal on physics. Ongoing publication to the present day)*
1945	**António Monteiro leaves Portugal for South America.**	
1946		***Gazeta de Física*** *(A physics journal publishing papers on the teaching of physics and on topics such as contemporary physics, applied physics —all aimed at the introductory-level teacher; ongoing publication to the present day)*
1947		**Manuel Valadares leaves Lisbon for Paris**

After returning from France in 1933, Valadares tried persistently to keep strong links with the Curie laboratory and to broaden his connections with the scientific international community network. The scientific results of his research group were always submitted to French scientific journals. In 1940-41 he moved to Italy where he carried out research at the «Instituto di Volta» –Pavia, with Rita Brunetti, from February to May 1940– and in the «Laboratório di Fisica dell'Istituto di Sanitá Pubblica» –Roma, with G. C. Trabacchi, from june 1940 to May 1941. In Rome he made contact with the Amaldi group and established important links with Italian physicists[19].

After 1940, as senior members of a Research Centre financed by the JEN/IAC[20], Valadares and Aurélio Marques da Silva (1905-1965) (another PhD from Paris who worked with Frederic Joliot (1900-1958)) supervised the PhD dissertations of four young assistant professors in three years –*Lídia Salgueiro* (1917-2009) from the Physics Laboratory in Lisbon, *Marieta da Silveira* (1917-2004) from the Chemistry Laboratory in Lisbon, *Carlos Braga* (1899-1982) and *José Sarmento* (1899-1986) from the Physics Laboratory in the University of Oporto–, a very remarkable work, indeed. Because of the communication difficulties during the war and the need to inform the scientific community of their scientific results, Valadares and the group

[18] *In* Salgueiro, Lídia, (1978), Vida e obra de Manuel Valadares, *Gazeta de Física*, vol.VI,2-12.

[19] Gaspar, Júlia (2009). *A investigação no Laboratório de Física da Universidade de Lisboa (1929-1947)*. Lisboa: CIHUCT.

[20] After 1936 the JEN had a new designation «*Instituto para a Alta Cultura*» (IAC) [Institute for High Ciulture].

from the Lisbon Physics Laboratory edited in 1943 the *Portugaliae Physica*, a relevant initiative at international level. Table 4[21] summarizes the scientific activity of the Research Centre in Physics, University of Lisbon, in the period of 1930 to 1946.

Table 4.

Years	Scientific papers			PhD	
	National Journals	Intern. Journals	TOTAL		
30-34	9	6	15	2	Herculano Amorim Ferreira, Manuel Valadares
35-38	8	10	18	2	Manuel Teles Antunes, Aurélio Marques da Silva
39-42	16	11	27	1	Amaro Monteiro
43-46	25	5	30	5	Lídia Salgueiro, Marieta da Silveira, Carlos Braga, José Sarmento, Armando Gibert
Total	**59**	**32**	**90**	**10**	

During the war, this Research Centre made serious efforts to support the stay in Portugal of Physicists from France, Italy and Austria who were escaping from the Nazi occupation: Salomon Rosenblum (1896-1959), Guido Beck (1903-1988) and Sergio de Benedetti (1913-1994). The Curie Laboratory in Paris was the link of these three physicists to Portugal, especially in their relationship with two young researchers Mário Silva (1901-1977) (from the University of Coimbra who was in Paris from 1925 to 1929 with a grant of this university) and Manuel Valadares (from the University of Lisbon).

Salomon Rosenblum had a close relationship with both of them (Mario Silva invited him to come to Portugal in 1933) and he had done research work with Valadares in Paris. Beck has been in France since 1937 and during his escape he was informed by French colleagues about the possibility of getting a position in Portugal. Beck had sent a plea for help to Mário Silva in Coimbra through a contact in Paris. During his stay in Portugal he started a cooperation with Valadares (Lisbon Physics Research Centre) and they both helped a small group of physicists and mathematicians from Oporto University to start with the Oporto Theoretical Physics Seminar activity[22]. Sergio de Benedetti, an Italian of Jewish origin working at the Paris Institute of Radium, also sought refuge in Portugal. Physicists and mathematicians from Coimbra, Lisbon and Oporto universities tried to get funding to help these scientists stay Portugal. However, the university was generally resistant to change, extremely reticent in its dealings with foreigners and therefore notorious for its lack in receptivity to academics from abroad bringing new ideas into Portugal. This also explains why the Portuguese academic community never made a great effort to ensure that a warm welcome was extended to renowned scientists fleeing to Portugal.

An unhappy ending...

As we have pointed out, the Portuguese university was generally resistant to change and extremely reticent in its dealings with foreigners who brought new ideas to the country. The great part of Portuguese Academia ostensibly showed a similar attitude towards the young researchers who arrived from a stay of several years abroad. Very few full professors of Portuguese universities were really interested in promoting scientific research and also in supporting new initiatives. This general attitude of the university and the political repressive response of government to the general hope of a shift to a democracy explained the end of the story.

After arriving in Portugal holding a PhD, António Aniceto Monteiro was unable to secure a lecturer position in a Portuguese university because he refused to sign the anticommunist declaration which was compulsory for a civil servant (a very unusual and courageous attitude). In 1945, the Brazilian government offered him a position in Rio de Janeiro and he left

[21] Gibert, Armando (1950). O Centro de Estudos de física da FCL. *Gazeta de Física*. 2(4):86-89.

[22] Fitas, Augusto J. S. and Videira, António A.P.(2007). Guido Beck, Alexandre Proca, and the Oporto theoretical physics seminar. *Physics in Perspective*, 9(1): 4-25.

Portugal. The five young Mathematicians, who had started working with him and then went abroad with scholarships, were unable to reopen the Mathematical Research Centre of Lisbon when they returned to Portugal after the war.

The university never gave Manuel Valadares a position of full professor during his near fifteen years of scientific activity in the laboratory of physics; beyond the scientific papers he published in scientific journals, he was the author of a book which appeared, in 1935, in a prestigious French publisher[23]. For Valadares, like for Monteiro, to be a scientist was a cultural attitude towards the world and life, an attitude supported in some philosophical ideas. Firstly, a unitary representation of the world in which science has a fundamental influence on the material, spiritual and moral evolution of humanity; secondly, the supremacy of reality over the ideal, of the objective over the subjective, and of scientific knowledge over the metaphysical; thirdly, the firm belief in man's capabilities and in the power of his rationality. A set of ideas that opposed the political and social orientation of Salazar's *Estado Novo* («New State»)…

After the war Manuel Valadares was forced by Salazar's regime to interrupt his scientific career at Lisbon University, he abandoned Portugal and started doing research at the Joliot Laboratory in Paris. In spite of the distance between Paris and Lisbon, Manuel Valadares continued to participate in the scientific life of the Lisbon laboratory through permanent contact with Lídia Salgueiro, one of his former PhD students and then professor at the Faculty of Sciences in Lisbon.

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[23] Valadares, Manuel (1935).*Transmutation des éléments par des particules accélérées artificiellement*. Paris: Ed. Hermann et Cie.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 818.

ALEXANDRE PROCA OR THE INTERNATIONALISM OF PHYSICS DURING THE TWENTIES AND THIRTIES

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Abstract

Alexandre Proca (1897-1955) is one of the prominent figures of physics in France between the two wars. He has been a key player in efforts to give to the theoretical physics research a prominent place in the country. In France, as shown by historical studies, physics research was at that time largely dominated by the experimental tradition. Proca also occupies a place in the history of quantum field theory. Author of one of the first doctoral thesis in relativistic quantum theory in France, he proposed the equations that bear his name, Proca equations, which describe particles of spin 1.

From Bucharest to Paris, from engineering sciences to theoretical physics, from theoretical work to the activity of organizing research, Proca followed a singular trajectory whose strongest meaning appears in the light of his "internationalism" (linguistic, intellectual and geographical), but especially in the light of the general scientific internationalism that develops at that time. This is what we intend to show in our presentation.

Proca illustrated in the hard political, intellectual and scientific French context in the thirties and very concretely in the specific context of Parisian world of research. It is marked by great figures (Paul Langevin, Marie Curie, Jean Perrin, Emile Borel ...) and by the emergence of the new Institut Henri Poincaré. Deriving from internationalist projects of the Rockefeller Foundation, the Institute H. Poincaré, where Proca conducts his activities, is devoted to the development of theoretical physics. We will consider these facts to better understand Proca's trajectory and, more general, the ways in which scientific internationalism materializes at European level and in the inter-war period.

Alexandre Proca (1897-1953) is one of the leading figures of French physics of the first half of the twentieth century. Like Jacques Solomon, Leon Brillouin and Louis de Broglie, Proca instigated theoretical research in relativistic quantum theory, especially during the 'thirties. From a point of view relevant to the subject of this symposium, the "internationalism in science", Alexandre Proca is a very interesting individual with a unique itinerary. He is among those scientists who, during the first portion of the twentieth century, chose to foster a career outside their country of origin. Proca was born in Bucharest. A Romanian intellectual declared that Proca belongs to those "Romanian talents who, between the two World Wars, made Paris their second home." In fact, he thought he had something to say in theoretical physics, to use a concise phrase he used to rationalize his migration and his decision to live in France.

What were the reasons for Proca's migration to France? On what political and/or intellectual context was the migration based? Under what conditions did Proca leave his country? What were his intellectual interests? How did he make his way in France? It is on matters of this nature that I will focus this presentation. Questions of this sort shed light on scientific internationalism. I will make some remarks regarding this subject at the end of my presentation.

Let me evoke first of all Proca's life in Romania. It revolved around a technical and practical world. Initially, Alexandre Proca was an engineer, having achieved a degree in the electromechanical section from the Polytechnic school of Bucharest, in the year 1922. With his engineering degree, Proca went to work in the private sector. This allowed him to attain experience abroad, including a trip to the USA in order to visit factories. Proca also had the opportunity to pursue a variety of supplementary scientific activities: in the same Polytechnic school he worked as a teaching assistant. He also collaborated with a local scientific journal. Finally, he was attracted by theoretical work in physics. Between 1920 and 1922 he gave a series of lectures on the theories of special relativity and general relativity. We mention in this context that he was, in particular, well informed about the debate that relativity had generated in France. The debates among the Parisian intelligentsia included the physicist Paul Langevin, the mathematician Paul Painlevé, and the philosopher Henri Bergson, to name only a few. These talks on relativity theory are significant for Proca's itinerary. The reputation that the physicist reached at this time is owed in part to it. This is easily understood. In his complete works published in French and edited in 1988, the author not only describes practical aspects of his theoretical insights, but reveals himself to be a talented promoter of science.[1]

These events at the beginning of Proca's scientific life were near the end of his residence in Romania. In 1923, while he was in rather favourable circumstances, Proca left the country for France. The specific conditions under which he left will not be described here. To understand Proca's migration we should look at the intellectual relations between Romania and France. We do not know if Proca had been to France beforehand. What it is clear, however, is that this country was for him a logical destination. Scholars note that in the early twentieth century Bucharest was called "Little Paris". Historians refer more generally to the strong links existing between France and Romania. It seems that there was a long history of cultural and economic relations that have roots in the sixteenth and seventeenth centuries. At the time of the Enlightenment and much more intensely so during the Romantic era, specialists note, many Romanians made their studies in France. The engineering schools in Paris, especially, welcomed young Romanian students. Proca himself was the son of a former student of the School of Arts and Manufacturing. The experts also speak of the French influence on public institutions in Romania. Historians show that this influence is revealed in all intellectual manifestations, and draw attention to the fact that the French language and literature was deeply influential. They spoke of the existence in the early twentieth century, of the Romanian political and intellectual elite, who were French speaking and Francophile. Proca himself spoke French, as well as German and English.

Thus, by coming to France, Proca ascribed to an apparently well established practice. Other aspects, which do not relate to the links between two countries, but rather refer to French science at that time, were also likely to make Proca's migration to France comprehensible. At the very beginning of the 1920s, France seems, potentially at least, to be a destination object for people who were looking for intellectual landmarks, and especially for new or *avant garde* scientific landmarks. Historians record the prestige of French mathematics between the two World Wars. In physics, there were in Paris a number of personalities who had reached international reputation: Marie Curie, with her laboratory for the study of radioactivity; Jean Perrin, discoverer of the atom in 1910 and who was at the Sorbonne's Physical Chemistry laboratory; Paul Langevin, Professor of Experimental and theoretical physics at the Collège de France, and director of the *Ecole Supérieure de physique et de chimie*; he also lead a laboratory devoted to ion physics at the Collège de France. Finally there was Maurice de Broglie and his X-ray laboratory. All of them were working in the field that Paul Langevin –when participating in scientific meetings such as "Conseils Solvay"– called "the new physics" (*La nouvelle physique*). It was at the time before the First World War. Now, how much was Proca influenced or attracted by these personalities? The question remains unanswered. It is clear, however, that it was with them that the young Proca went to look for a place of work in Paris in the second half of the 1920s.

Proca's Parisian experience. The beginning

We evoke Proca's Paris experience in order to consider his possibility of success at becoming a theoretician. It is apparent that Proca fulfilled the conditions required for being accepted into the Parisian scientific world, or at least those imposed by Marie Curie. To my knowledge, Proca did not have a scholarship. Even though he was trained as an engineer, Proca was nevertheless obliged to start his studies *a Licence ès Sciences* at the Sorbonne. At the end of these studies, from his own testimony, he came to visit Marie Curie, asking her to permit him to join her laboratory. She accepted him personally. It was around 1925. Due to Proca's training and experience in Romania, we can rationally imagine the positive impact his advanced skill set had on Marie Curie. Nothing suggests that he proposed to Marie Curie a specific project.

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[1] G. A. Proca, *Alexandre Proca.Oeuvre Scientifique Publiée*, S.I.A.G., Rome, 1988.

In the laboratory of Marie Curie, he performed experimental work on beta decay, among other activities. Compton's theory of gamma ray scattering had recently been published, which attracted the interest of Marie Curie. But Proca's scientific activity extended beyond his work with Marie Curie during his early period in Paris. Was it simply a case of someone who was looking for new subjects? Proca focused temporarily on probability theory and its applications taught by Emile Borel at the Sorbonne faculty of sciences. Proca had probably enrolled in this course. This gave rise to Proca's publication of the paper: "On an extension of the concept of probabilities."[2] He maintained very close ties with his homeland; therefore, this paper appeared in a Romanian journal of mathematics. A little work on the theory of the photon illustrates another subject of interest of Proca at the time. It appeared in a collection edited by physicist Léon Brillouin.

A more general statement on the environment in which he lived in Paris explains the diversity of Proca's interests and their nature. Proca attended the faculty of sciences, but in addition he was increasingly divided between the laboratory of Marie Curie and the Collège de France. Proca constantly fed his penchant for theoretical reflection. At Collège de France, as mentioned above, Paul Langevin was teaching theoretical physics. Proca probably attended Langevin's lessons given between 1926 and 1930: "La structure des atomes et leurs propriétés magnétiques et optiques"; "La structure de la lumière et les quanta"; "Les échanges entre la matière et le rayonnement"; "Le magnétisme: récents progrès théoriques et expérimentaux". As with all the physicists who attended them, Langevin's lectures at Collège de France were very important for Proca. Because of the brilliant subject matter, Langevin's lessons attracted the whole of the Parisian élite. Thus Proca had the opportunity to come into contact with such luminaries as Leon Brillouin, Jacques Solomon, Francis Perrin, and Louis de Broglie. Foreign visitors also attended Langevin's lectures, including the physicist Leon Rosenfeld, who around 1929 was beginning a collaboration with Niels Bohr in Copenhagen. Proca met Rosenfeld, too.

So this is an illustration of the possibilities open to a young man who wanted to become a theoretician in Paris. There was not, however, an institutional and academic structure devoted to theoretical physics. In this regard it is interesting to mention how major figures in France at that time become theoreticians. Louis de Broglie had begun in the laboratory of his brother Maurice. Francis Perrin had managed to complete his PhD thesis in an almost pure mathematical environment while working with Emile Borel. Langevin's son-in-law, Jacques Solomon, went to Copenhagen to work with Niels Bohr's collaborator: Leon Rosenfeld. Fortunately for Proca, Institut Henri Poincaré opened its doors in 1929. There, he would become a theoretician.

Proca at the Institut Henri Poincaré

As it is known, Institut Henri Poincaré owed its existence to the philanthropy of the American, John D. Rockefeller. This creation was part of a wider project in mathematics in Europe. Negotiations between the founders had taken place in Paris during the second half of the 1920s. The mathematician Emile Borel was one of the most important founders. Borel attributed to the institute the role that it would play in France: to promote the development of theoretical physics and the "*Calcul de probabilités et ses applications*". Emile Borel was motivated by his interest in physics and applied mathematics. According to agreements reached between the Rockefeller Foundation and the University of Paris, academic positions were created. Thus Louis de Broglie became one of its first faculty members, as professor of theoretical physics, moving from his brother's laboratory to the "Institut Poincaré". About Proca and his arrival at the institute we have only a few details. It seems that it was Marie Curie who advised him to join the institute. We should recall that it was she who introduced him to physics research in Paris. The "Institut Poincaré" would allow him to achieve his aspiration of becoming a theoretical physicist. He began under the auspices of Louis de Broglie to prepare his doctoral thesis on the theory of Dirac. He defended his thesis in 1933. Subsequently, he led the research in relativistic quantum theory that culminated in 1937 with the "Proca equations".

It was intended from the beginning that a series of lectures and conferences by prestigious foreign guests were regularly to be made at the new Institute. Albert Einstein, unable to appear in 1929, nevertheless came in 1930, in which he gave a series of lectures on relativity and the unified field theory. Proca was commissioned to translate them. They were published in French in the *Annales de l'Institut Poincaré*. This journal had just been created. The most distinguished figures in theoretical physics came to the Institute: Erwin Schrodinger preceded Albert Einstein; Niels Bohr, Wolfgang Pauli, Leon Rosenfeld, and Robert Millikan came later.

Now, we discuss Proca and the whole of activities conducted at the Institute during the thirties. It is difficult to know exactly what role Proca played, in particular regarding the general organization of courses. In contrast, we know from a correspondence between Proca and Leon Brillouin, that he was heavily involved in the publication of the *Annales*. Editing of the journal required language skills and Proca had them. From another perspective, the lectures at the Institute and the publication of the *Annales* were for Proca an opportunity to establish contacts with foreign physicists, for example, W. Pauli and E. Schrödinger. In some cases, these contacts became deep and enduring, as we see from Proca's correspondence. The letters exchanged between Proca and Schrödinger discuss such diverse topics as Proca's translation of his wave

[2] See reference 1.

mechanics, purely scientific questions, and so on. Proca's correspondence with his fellow physicists is rich, and this remains largely unstudied by historians. Proca's scientific life at the Institute, though very productive, was far from representing the whole of activities conducted by him during the thirties.

Concluding remarks

It is clear from a general point of view of Internationalism, that Proca's itinerary raises –first and foremost– the questions of geographical borders, of migration of persons, of transfer of skills and knowledge, and of opportunities for candidates through movement. However, was Proca himself heavily influenced by the internationalism of science? Should we place him rather in the context of international migration, in light of awareness of new opportunities for travel offered by the emergence of new means of transportation? I remind you only in relation to this question that it was at the turn of the twentieth century that this movement was first manifested. From another less general point of view, let me make this observation: at the beginning of the twentieth century, France and Romania were two countries that after a long history of inter-connectedness had moved closer together. It made favourable Proca's mobility.

Proca, initially trained as an engineer, wanted to contribute to theoretical physics and chose France as the best place to advance his career, but his goal could not be realized immediately. Conditions favourable to theoretical work really did not appear until the thirties with the creation of the Institut Henri Poincaré. Until this time, he worked in Curie's laboratory and concurrently attended the lectures of Langevin at the College de France. Certainly these experiences prepared him professionally, but there is more to it than that. In terms of scientific openness, they demonstrate the great possibilities of Collège de France and Laboratoire Curie. These were places open to outside countries, almost without frontiers. Let me mention that besides Proca, Portuguese students in the late 1920s and the Soviet physicist D. Skobeltzyn in 1929, to mention only foreign visitors, shared with Marie Curie the scientific and intellectual atmosphere of her laboratory. This benefitted Proca. A recent study about the scientific activity in Portugal, more precisely in Porto and during the Second World War, mentions the encounter between Proca and Portuguese students at Marie Curie's laboratory. We see how significant this encounter was. Proca had failed to enter the United States, but he did seek refuge in Portugal. The links that Proca established with some of these Portuguese students helped him to stay in Porto.[3]

The "Collège de France" and especially the "Institut Poincaré" placed Proca in an international environment. In this context he was a major player in the "internationalist vision of science". He learned to organize himself in international teaching. In 1946, immediately after the war, Alexandre Proca put in place at the Institut Henri Poincare a seminar in theoretical physics, the Proca seminar, which was a huge success. His "internationalist" dimension allowed a whole generation of young graduates to become familiar with the theoretical research in nuclear and particle physics that dominated the scene and continued to evolve. Indeed, presentations on meson theory given by a number of distinguished theoretician guests from abroad such as Yukawa or Peierls, inspired new French theoreticians who dominated science in France in the 1950s and 1960s.

Proca's relationship with the Romanian academic environment had doubtlessly never broken. But, in fact, he did not have time to organize big projects with his country. He died too early. Finally, his Romanian origin caused him problems during the War. In France, this prevented him from teaching, but this will not be elaborated upon here.

[3] A. J. dos Santos Fitas and A. A. Passos Videira, Guido Beck, Alexandre Proca and the Oporto Theoretical Physics Seminar, *Physics in Perspective*, vol. 9, n° 1, 4-25, 2007.

BEYOND THE MOLECULAR VISION: UNROMANTIC PERSPECTIVES ON THE HISTORY OF BIOPHYSICS

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SEEING THE INVISIBLE. THE INTRODUCTION OF ELECTRON MICROSCOPY IN GREAT BRITAIN

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Abstract

During the twentieth century, life sciences experienced notable progress thanks to the development of new and improved instruments coming from the field of physics, such as the electron microscope. However, life scientists originally proved very reluctant to incorporate this new technique into their research. In fact, the development of such a new instrument was a consequence of a general concern in the scientific and industrial analysis of materials. In Britain, it was considerations of the potential uses and applications of the new technique, together with the war context, what highly influenced the development and consolidation of electron microscopy, promoted by a new community of scientists engaged in the study of both organic and inorganic structures.

Introduction

The origins of electron microscopy date back to the late 1930s and 1940s, when it developed around a new and promising instrument with many potential uses and applications in different fields engaged with the study and understanding of microscopical structures. In recent years, historians of science such as Nicolas Rasmussen, Bruno J. Strasser, and Falk Müller have dealt with the successful history of the development of electron microscopy in North America and continental Europe.[1]

In Britain, it was only during World War II that the interests of physicists, engineers, life scientists, businessmen, and science administrators converged towards and favoured the introduction, promotion, and support of electron microscopy. This was partly a consequence of specific scientific, political, and economic aspects and interests that one may recognize in

[1] See, for example: N. Rasmussen, *Picture control. The electron microscope and the transformation of biology in America, 1940-1960* (Stanford, 1997); B. J. Strasser, *La fabrique d'une nouvelle science. La biologie moléculaire à l'âge atomique, 1945-1964* (Florence, 2006); and F. Müller, "The birth of a modern instrument and its development during World War II: Electron microscopy in Germany from the 1930s to 1945", in A. Maas and H. Hooijmaijers (eds.), *Scientific research in World War II: what scientists did in the war* (London, 2009), 121-46.

the context of warfare, including the promising industrial applications of the new microscopes and the successful investigations on bacteria and other biological specimens reported from Germany. All these discourses and approaches favoured the importation of several electron microscopes to be used in such a wide range of investigations and the formation in the 1940s of a new community of scientists who promoted scientific studies under the new instrument and unified techniques of specimen preparation and imaging.

The electron microscope represented a merger of interest between several features such as academic work in different fields, industrial and economic concerns, and political aspects related to the war context. The aim of this paper is to unveil the difficulties surrounding the process of introduction and appropriation of electron microscopy in Britain during the late 1930s and early 1940s. In doing so, I hope to show how the introduction of the electron microscope to scientific research in the country was encouraged by and benefited, not merely a group of life scientists interested in a molecular vision of life, as is generally acknowledged, but a new broad community of academic and industrial scientists concerned with the study of both inorganic and organic material properties and structures.

Early users

The development of electron microscopy is linked to the names of two engineers from the Technische Hochschule of Berlin: Max Knoll (1897-1969) and Ernst Ruska (1906-1988). Using some of the methods and apparatus already developed to some extent for the purpose of cathode-ray oscillography, Knoll and Ruska were the pioneers of the construction of an electron microscope in Germany in the early 1930s. The publication of their results, though still very modest, opened the doors for the development of several independent electron microscope projects, which flourished during the mid 1930s in countries like the USA, Canada, Belgium, France, and the Netherlands.

In Britain, the physicist Louis Claude Martin (1891-1981) began the construction of a combined electron and light microscope at the Imperial College of Science and Technology in London, soon after the German engineers reported their first results. Martin wrote to the prestigious physician and pharmacologist Edward Mellanby (1884-1955), Secretary of the Medical Research Council (MRC), to obtain this institution's financial support. In his first letter to Mellanby, echoing some results obtained in Brussels, Martin pointed out how biological structures could be studied this way 'by means of differential impregnation with compounds of heavy atoms such as osmium'.[2] The use of osmium to fix the vegetal tissues, however, was known to completely destroy the cells. Therefore, Mellanby refused to get the MRC involved in this project to build an electron microscope, considering it to merely be a physical project to be funded, in any case, by the Department of Scientific and Industrial Research (DSIR).[3] In fact, even Martin himself, who had underlined the possible application of this new instrument to the study of biological specimens, admitted in his report that there was no real evidence that the new instrument was of any biological use.[4]

The two-stage electron microscope designed by Martin was finally constructed in Manchester at the Research Department of the Metropolitan-Vickers Electrical Company, which was ultimately involved in the project and undertook the manufacture of the instrument at its own expense.[5] Metropolitan-Vickers was one of the very few British companies capable of producing scientific instrumentation requiring demountable vacuum apparatus. On completion, the electron microscope was transferred from Manchester to Imperial College.

Originally conceived to enable a critical comparison between light-optical and electron microscope images,[6] the electron microscope was used to gain a better understanding of its performance problems, in order to suggest new technical developments to improve the use of the instrument in actual microscopy. In fact, Martin and his team were mainly involved in testing different aspects such as the effects of aberrations produced by the electromagnetic lenses and the adjustments required to centre the electron optical system.[7] Various types of specimen were employed during these tests, mainly thin aluminium and solver films.

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[2] Draft of a confidential report to the Department of Scientific and Industrial Research, 8 Jan 1935, The National Archives, Kew (hereafter NA), File FD 1/1702.

[3] E. Mellanby to L. C. Martin, 13 Dec 1934, NA, File FD 1/1702.

[4] Draft of a confidential report to the Department of Scientific and Industrial Research, 8 Jan 1935, NA, File FD 1/1702.

[5] L. C. Martin, R. V. Whelpton and D. H. Parnum, 'A new electron microscope', *Journal of Scientific Instruments*, 14 (1937), 14-24.

[6] E. Ruska, *The Early Development of Electron Lenses and Electron Microscopy* (Stuttgart, 1980), 64.

[7] See: L. C. Martin, D. H. Parnum and G. S. Speak, 'A Report on Experimental Work on the Development of the Electron Microscope', *Journal of the Royal Microscopical Society*, 59 (1939), 203-16; and L. C. Martin, 'The Optics of the Electron Microscope', *Journal of the Royal Microscopical Society*, 59 (1939), 217-31.

The electron microscope, which occasionally achieved a resolution of about 100 nanometres with favourable test specimens (at a maximum magnification of 2000),[8] was moved in 1940 from Imperial College to the National Physical Laboratory (NPL), where it was further developed. However, even though it ultimately performed well and produced satisfactory results, this electron microscope never got past the experimental model stage.[9]

Sceptical attitudes and the war context

Despite the interest of some individuals in developing electron microscopy for biomedical applications, dismissive attitudes towards the use of the electron microscope for biological work were relatively common in most European countries during the late 1930s. The main objections were related both to the sense and value of the imaging of microscope structures below the limit set by the wave nature of light in specimens and to the possibility of attaining such a goal with an electron microscope. Biologists believed that even if such microscopic structures existed at all in living tissue, they would alter so quickly that it would not be possible to observe them directly in an electron microscope image. They also doubted that sufficiently informative relationships could be established between such structures and the function of the tissue, such as for example metabolism, breathing or nerve conduction. As Ruska wrote, there was 'an air of resignation that had developed over the course of the previous 50 years mainly because one could not see such fine structures at all in the light microscope'.[10]

The potential uses of the electron microscope, together with the publication of the first results for biological applications, helped transform reluctant ideas that many scientists had about the new technology. But it was not the only reason. At least in Great Britain, the war context was also crucial. The outbreak of the war had brought new concerns and fears to the forefront of scientific research, such as the possibility of chemical and biological warfare. Therefore, it is not difficult to imagine the sense of alarm that spread not only amongst scientists, but also politicians, when the British bacteriologist William Whiteman Carlton Topley (1886-1944) reported in the summer of 1940 to the Ministry of Health and to his colleagues at the MRC the first results for biological research (in particular photographs of bacteria) carried out at the Robert Koch Institute in Berlin with an electron microscope made by the German firm AEG.[11]

Germany was leading the development of biochemical weapons thanks to a secret and decentralized research organization set up during the Weimar Republic that attracted considerable funding during the Nazi era.[12] Such a threat definitively favoured a rapid change in the attitudes of British politicians and scientists to the development and potential uses and applications of the electron microscope.

Charles Galton Darwin (1887-1962) was designated to negotiate on behalf of the British government the supply of seven electron microscopes made by the RCA on lend-lease terms through the British Ministry of Supply. Once the agreement was ratified, seven electron microscopes were delivered to the NPL. They were expected to provide immediate benefits in the study of the microscopical structure of both organic and inorganic material in the particular context of the war years. Once the agreement was ratified and delivery of the seven electron microscopes to the National Physical Laboratory (NPL) was arranged, the Scientific Advisory Committee of the War Cabinet began to produce a tentative priority list for allocating these instruments. The Scientific Advisory Committee began in March 1942 its preliminary discussions on the issue, having decided by then that the first microscope to arrive had to be set up at the NPL,[13] where the experimental model of electron microscope designed by Martin and built by the Metropolitan-Vickers had been employed in tests for metallurgical purposes.

The metallurgical research at the NPL had been deemed of immediate importance with the outbreak of the war, as it was particularly concerned with the structure and failure of metals in practice. Any metallic material that failed in service was

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[8] R. Reed, 'Some Recollections of Electron Microscopy in Britain from 1943 to 1948', in *The Beginnings of Electron Microscopy*, edited by P. W. Hawkes (note 15), 483-500 (484).

[9] F. W. Cuckow, 'The electron microscope and its applications', *Proceedings of the Physical Society*, 57 (1945), 564-7.

[10] E. Ruska, *The Early Development of Electron Lenses and Electron Microscopy* (Stuttgart, 1980), 65.

[11] For the correspondence with the Ministry of Health, see: P. G. Stock to W. W. C. Topley, 16 Jul 1940, NA, File FD 1/1702; W. W. C. Topley to P. G. Stock, 19 Jul 1940, NA, File FD 1/1702; and W. W. C. Topley to P. G. Stock, 28 Aug 1940, NA, File FD 1/1702. On his report to his colleagues of the MRC, see: H. H. Dale to W. W. C. Topley, 22 Aug 1940, NA, File FD 1/1702.

[12] For example, on the development of extremely toxic chemical weapons produced on a large scale and made available for deployment during these years, see: F. Schmaltz, 'Neurosciences and Research on Chemical Weapons of Mass Destruction in Nazi Germany', *Journal of the History of the Neurosciences*, 15 (2006), 186-209.

[13] E. Appleton to L. Bragg, 9 March 1942, Royal Institution of Great Britain, The Papers of Sir Lawrence Bragg 1890-1971 (hereafter RI MS WLB), Box 41B, Folio 188.

examined by the metallurgy division, the causes for failure being determined as: steel plates perforated by bomb fragments, steel gas cylinders, heavy naval shell, industrial welding, cutting tools, gun barrel broaches, etc.[14]

New imported microscopes

By the end of 1942, five more electron microscopes had been delivered to the following institutions: the Chemical Defence Experimental Station in Porton, a defence research establishment in the Ministry of Supply; the Shirley Institute, belonging to the British Cotton Industry Research Association, in Didsbury; the NIMR premises in Hampstead, North London; Rothamsted Experimental Station, an agricultural research institution in Harpenden; and the Cavendish Laboratory in Cambridge.

As a consequence of the importance given to the applications of the electron microscope to the war efforts, consensus had been soon reached on the decision to send one of the first RCA microscopes that arrived at the NPL to Porton. It was in Porton where different biochemical aspects of war were studied, particularly those of chemical and biological warfare.[15] In particular, intelligence on the chemical warfare capabilities of the Axis powers was scrutinized and new chemical weapons and munitions were developed and trialled. Chemical agents such as phosgene, mustard gas, and different tear gases were industrially produced and weaponized under conditions of secrecy and urgency.[16] Furthermore, practical evaluation of biological warfare had been arranged at Porton by September 1940 under the leadership of one of the foremost microbiologists of the period, Paul Gordon Fildes (1882-1971), who set up a top secret group to undertake these tasks.

The early interest shown by William Whiteman Carlton Topley (1886-1944) in the use of the electron microscope in biomedical work had also made the MRC an ideal and obvious candidate.[17] The electron microscope was finally set up at the National Institute for Medical Research (NIMR) in Hampstead. This may be understood as part of the major effort initiated in the 1920s by the MRC to promote research in what was to become the field of virology, mainly developing new methods to learn more about viruses-ultramicroscopic organisms whose dimensions appeared to lie between those of the smallest bacteria and the largest chemical molecules.

Virology was, indeed, one of the fields that were expected to benefit most. But the identification of certain particles seen in the electron microscope, such as virus particles, was not easy at all. The RCA electron microscope sent to Rothamsted Experimental Station was expected to contribute to the study of the fundamental nature of virus diseases in plants. This field was of increasing interest here during the 1930s, mainly as a consequence of the appointment of the virologist Frederick Charles Bawden (1908-1972) and the biochemist Norman Wingate Pirie (1907-1997), who were especially interested in the study of the physicochemical properties of viruses. Both scientists contributed to the development of electron microscopy in Britain through the spread of new specimen preparation techniques and the improvement of a whole set of new practices regarding the interpretation of electro-micrographs.

The University of Leeds also applied for one of the electron microscopes, which the Scientific Advisory Committee had decided to devote to medical purposes, excluding any other possibilities.[18] It was originally intended for use both in its Medical Department and in the research by William Thomas Astbury (1889-1961) into the constitution of proteins.[19] However, the program was rather weak from the medical point of view and, on some points, showed "elementary misunderstandings of what the electron microscope can be expected to do".[20] It was the name of Astbury, whose X-ray work on the structure of fibres and proteins had mainly contributed to his great reputation, what made Leeds the best option.[21]

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[14] E. Pyatt, *The National Physical Laboratory: A history* (Bristol, 1983), 135.

[15] On the medical research pursued during the war on chemical warfare problems, see: F. H. K. Green and G. Covell, *Medical research* (London, 1953), 328-38.

[16] See: G. B. Carter, *Chemical and biological defence at Porton Down 1916-2000* (Norwich, 2000).

[17] E. V. Appleton to E. Mellanby, 13 May 1942, NA, File FD 1/1702.

[18] E. V. Appleton to E. Mellanby, 9 November1942, NA, File FD 1/1702.

[19] K. Merling to E. Mellanby, 6 August 1943, NA, File FD 1/1702; E. Mellanby to K. Merling, 1 July 1943, NA, File FD 1/1702; E. Mellanby to M. J. Stewart, 1 July 1943, NA, File FD 1/1702; M. J. Stewart to E. Mellanby, 2 July 1943, NA, File FD 1/1702; and E. Mellanby to K. Merling, 1 July 1943, NA, File FD 1/1702.

[20] H. H. Dale to E. Mellanby, 23 June 1943, NA, File FD 1/1702.

[21] E. Mellanby to H. H. Dale, 22 June 1943, NA, File FD 1/1702; and M. Stewart to E. Mellanby, 10 May 1943, NA, File FD 1/1702.

Work with the electron microscope was expected to serve as a complement to the X-ray analysis of different materials, including bacterial flagella.[22] In fact, Astbury was very keen to explore how the new technique could be used to solve problems on the border-line between structural and physical chemistry, and fundamental physiological problems.[23]

Electron microscopy also attracted the attention of those involved in studies of the shape and structure of fibres – which had been developed in the late nineteenth century and the early twentieth century by means of light microscopy.[24] Subsequently, the members of the Scientific Advisory Committee decided to provide one of the RCA's electron microscopes to the British Cotton Industry Research Association, an institution founded originally in 1919 to research new technologies of cotton production. This decision was particularly motivated by the stimulus created during World War II for high-performance textile production. In fact, part of the work at the Shirley Institute, where the microscope was to be allocated, had developed into new research, such as the production of fine cotton nylons as a substitute for silk in parachute fabrics.[25]

Good examples of the interest that the new electron microscope awoke outside the British academic world may also be found in the different firms that travelled to Cambridge in the 1940s in order to see the RCA electron microscope in operation. For instance, the United Steel Companies Ltd. visited Cambridge to inspect the new instrument and see what applications of the new technology they could benefit from.[26] Others, like Tootal Broadhurst Lee Company Ltd.,[27] showed a great interest in the potential usefulness of electron microscopy in the examination of fibres.[28]

The story of how the RCA electron microscope arrived in the Cavendish Laboratory provides further evidence of such an industrial interest. Since 1937, the Cavendish Laboratory had been run by the Nobel Prize winner in Physics William Lawrence Bragg (1890-1971). Bragg was engaged in X-ray investigations of different structures, including proteins, and was keenly interested in the application of new forms of microscopy to the interpretation of nature –having even suggested some ideas for an X-ray microscope a few years earlier. The new electron microscope was expected to provide more accurate images of microscopic specimens at higher magnifications.

Bragg began formal enquires about the possibility of purchasing an electron microscope after a meeting of the Panel of the Iron and Steel Federation, which funded and supervised the work in Cambridge on the study of the structure of metals.[29] At this meeting, held on 4 March 1942, the chemist Henry Stafford Hatfield (1880-1966) pressed for the continuation of the long-term research pursued in Cambridge and the short-range war-effort work for the Ministry of Supply. In this sense, he publicly argued for the convenience of acquiring an electron microscope.

One day later, Bragg wrote to Edward Appleton to obtain some of the information on the instrument and ask about the possibility of purchasing one of the electron microscopes produced by the RCA for the Cavendish Laboratory.[30] Appleton was one of the members of the Scientific Advisory Committee that, at precisely that time, were in preliminary discussion about the allocation of the electron microscopes secured under a lend-lease agreement.[31] The electron microscope was sent to Cambridge late in 1942.

Conclusion

The introduction and development of the electron microscope in Britain was encouraged in the late 1930s and early 1940s by a new and heterogeneous community of scientists interested in the structural study of both inorganic and biological material. This new community included not only experimental physicists and biomedical researchers, but also material

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22 See for example: W. T. Astbury to A. V. Hill, 17 November 1948, Churchill Archives Centre, Churchill College, Cambridge, File AVHL II 4/5.

23 H. H. Dale to E. Mellanby, 23 June 1943, NA, File FD 1/1702.

24 T. G. Rochow and P. A. Tucker, *Introduction to microscopy by means of light, electrons, X rays, or acoustics* (Springer, 1994), 113.

25 For a brief history of the Shirley Institute, see: M. Sawbridge (ed.) *et al.*, *The story of Shirley. A history of Shirley Institute, Manchester, 1919-1988* (Manchester, 1988).

26 A. H. Jay to W. L. Bragg, 24 July 1944, RI MS WLB, Box 60B, Folio 251; W. L. Bragg to A. H. Jay, 4 August 1944, RI MS WLB, Box 60B, Folio 252.

27 Tootal Broadhusrt Lee was a large cotton firm based in Manchester that by the outbreak of World War II had spinning, weaving, and yarn dyeing factories in Bolton, as well as factories in Newton Heath weaving silk and wool and producing handkerchiefs and ties. See: L. Richmond and B. Stockford, *Company archives: the survey of the records of 1000 of the first registered companies in England and Wales* (Gower, 1986).

28 R. P. Foulds to W. L. Bragg, 30 April 1942, RI MS WLB, Box 49B, Folio 12; W. L. Bragg to R. P. Foulds, 2 May 1942, RI MS WLB, Box 49B, Folio 13.

29 See: J. G. Crowther, *The* social *relations of science* (New York, 1941), 488.

30 W. L. Bragg to E. Appleton, 5 March 1942, RI MS WLB, Box 41B, Folio 186.

31 E. Appleton to W. L. Bragg, 9 March 1942, RI MS WLB, Box 41B, Folio 188.

scientists and engineers, as well as industrial scientists working in different fields such as metallurgy, pottery, textiles, etc. Some of them were particularly concerned with problems on the frontiers of structural physics, physical chemistry, and fundamental physiology. But in order to understand the process of appropriation and development of electron microscopy in Britain, it is needed a slightly more complex picture in which a whole set of actors, included some outsiders to the academic world and with explicit different agendas, played a crucial role. In particular, the war context seems to have played a crucial role in such a process.

It is difficult to clearly distinguish the limits of professional, academic, industrial, and political dimensions if dealing with the introduction and appropriation of novel scientific techniques and apparatus such as the electron microscope. Moreover, the establishment and later commercialization of the electron microscope in Great Britain required a first step, characterized by the interaction not only of academic scientists from different fields, but also industrial engineers, politicians, private firms, businessmen, etc. All of them were captivated by aspects such as the huge potential that this new instrument seemed to have and an increasing curiosity on what could be expected from it.

The common place provided by this instrument favoured a continuous dialogue between different actors with their own agendas, which included concerns related to aspects such as the acquisition of natural knowledge, the legitimacy for scientific authority, the potential technological applications, and economic benefits. It was the way in which the whole system was consolidated what led to the final success of electron microscopy.

CIRCULATIONS
IN THE NEUROSCIENCES

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 830.

MUTATION CARRIERS: US LABORATORIES LED BY SPANIARDS IN THE EARLY 80S

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Abstract

*The history of the initial protocols to transfer viral genetic products inducing tumors was greatly facilitated by the availability of continuous cell line of highly contact-inhibited cells (NIH/3T3). During the 70s the teachings of those involved in this research reached some of the Spanish biologists recently arrived in the US. And in the early 80s a significant group of Spanish oncologists developed laborious techniques for the detection of oncogenes, involving the gene cloning of regions where there are aberrations, sequencing and identification of structural genes in the affected loci, and then determination of their role in cancer. Pellicer worked from 1976 to 1980 in cell proliferation and cancer at the Columbia University's Medical Center, College of Physicians and Surgeons. In 1979, Manuel Perucho went to a Gordon Conference on Immunology in New York. Ángel Pellicer communicated him his transfection results. This was why Perucho moved away from Berlin Max Planck Institut für Molekulare Genetik to the US CSHL, and later to Stony Brook. Also in 1979, Mariano Barbacid, from the Laboratory of RNA Tumor Viruses, NCI, Bethesda, went to New York, where Ángel Pellicer communicated him his transfection results. These three molecular oncology research leaders met with oncogenes. After the initial observation that a significant proportion of the tumors scored positive in the fibroblasts used –the NIH 3T3 cells– and that the genes responsible belonged to the ras family, Ángel Pellicer isolated the two main murine genes responsible for the phenotype, N-*ras*, in results presented in 1984. The following year he sequenced the complete coding region of N-*ras*, and suggested evidence for the spectrum of activated mutations in different mouse strains and by different agents. A number of methodical factors affect this historical series of performances. Concept symbols derived from the display of the graphical distribution along time of the concurrent performances reveal the successive shifts of their authors from the Columbia University to the Cold Spring Harbor Lab and the Frederick Cancer Research Center.*

Introduction

In a first approach, the integrated interaction of the viral and cellular genomes is due to L.A. Zilber (1894-1966),[1] the theory of oncogenes was originated by R. Huebner (1914-1998) and G. Todaro (1937-),[2] H. Temin (1934-1994) [3] elaborated the theory of the provirus, D. Baltimore (1938-) isolated the enzyme transcriptase inverse, and M. Barbacid (1949-) activated the first oncogene.[4] The most accurate reconstruction from the true ancestral cancer immunologist Zilber may take into account the importance of the use of cell lines free of sarcoma virus in mutagenic experiments.

In 1969 the third line 3T3 was segregated from the mice embryos in the Swiss/NIH line. This line was to be called NIH 3T3 (after the name of the National Institute of Health where they proliferated). One of the authors was Todaro.[5] The cells gave 20-25 steps to be cloned, until achieving the clone (clone 6) with a particularly high plating efficiency and low saturation density.[6] Later the NIH 3T3 was used mainly to study the viral transformation in tissue- and cell- cultures. The selection of this line for transfection experiments is conditioned by a high efficiency in the integration and fixation of the exogenous DNA in the genome of the NIH 3T3 cells.

The considerable interest that the discovery that an ample variety of tumor cells contained activated *ras* genes had, was detectable through gene transfer in NIH 3T3 cells, and displays the work by Barbacid in August 1983.[7]

This work was presented to the US Natl Acad Sci on April 1983 by the 1953 Nobel Prize Severo Ochoa, who also selected to present there on July 1985 a development on human adenocarcinoma cells of previous NIH 3T3 results, associated to the detection of point mutations authored by M. Perucho.[8] [9] The point mutation malignant acquisition responsibility discovery was the reason after which Barbacid was awarded in 1988,[10] but a carcinogenic induction of *ras* oncogene activation was also obtained under the responsibility of A. Pellicer, being I. Guerrero his postdoc.[11]

Ángel Pellicer, professor in the NYU Dpt of Pathology from 1980, worked with animal model systems of carcinogenesis, by using *ras* genes because of their great clinical relevance. Constantly in contact with Spain through the postdocs training, he admitted Isabel Guerrero as a Fellow of the Spanish High Research Council in 1983.[12] The discovery was that a significant proportion of the tumors scored positive in the NIH 3T3 focus forming assay, and the genes

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[1] Silber, L. (1923) "Ueber das Wesen der Weil-Felixschen Reaktion". *Centralbl. f. Bakt. etc.* 1. Abt. Originale. Bd. 89. Heft 7/8. pp.250-259, where it is proved the serological modification experienced by the bacteria Proteus vulgaris, hereditarily transformed into a variant that is agglutinated by the serum of the guinea pig infected by louse-borne typhus virus. A precedent of the experiments by Griffith on the transformation of pneumococci by way of transfer from a virulent culture. See Griffith, F. (1928) "The Significance of Pneumococcal Types". *Journal of Hygiene*, Vol. 27, Iss. 2, p. 113.

[2] Huebner RJ, Todaro GJ. (1969) "Oncogenes of RNA tumor viruses as determinants of cancer". *Proc Natl Acad Sci USA*, Vol. 64, Nº 3, pp. 1087-1094.

[3] Herzberg, M.; Revel, M. (1973) *Atlas de biología molecular*. Barcelona, Omega.

[4] Garte, S.J.; Hochwalt, A.E. (1989) "Oncogene activation of experimental carcinogenesis: the role of carcinogen and tissue specificity". *Environmental Health Perspectives*, Vol. 81, pp. 29-31.

[5] Todaro and Green were the precursors of 3T3 cells. Todaro, G.J.; Green, H. (1963) "Quantitative studies of growth of mouse embryo cells in culture and their development into established lines". *Journal of cell biology*, Vol. 17, Iss. 2, pp. 299-313.

[6] Jainchill, J.L.; Aaronson, S.A.; Todaro, G.J. (1969) "Murine sarcoma and leukemia viruses: assay using clonal lines of contact-inhibited mouse cells". *Journal of Virology*, Vol. 4, Iss. 5, pp. 549-553. (p. 550.)

[7] Santos, E.; Reddy, E.P.; Pulciani, S.; Feldmann, R.J.; Barbacid, M. (1983) "Spontaneous activation of a human proto-oncogene". *Proc Natl Acad Sci USA*, Vol. 80, Nº 15, pp. 4679-4683.

[8] Winter, E.; Yamamoto, F.; Almoguera, C.; Perucho, M. (1985) "A method to detect and characterize point mutations in transcribed genes: Amplification and overexpression of the mutant c-Ki-*ras* allele in human tumor cells". *Proc Natl Acad Sci USA*, Vol. 82, Nº 22, pp. 7575-7579.

[9] In 1981, Perucho provided a powerful technique for efficiently transforming NIH 3T3 cells, see Perucho, M.; Goldfarb, M.; Shimizy, K.; Lama, C.; Fogh, J.; Wigler, M. (1981) "Human-tumor-derived cell lines contain common and different transforming genes". *Cell*, Vol. 27, Iss. 3 Pt 2, 467-476.

[10] The "cancer Nobel" was attributed (in 1988) by the Swiss Josef Steiner Foundation to Barbacid "for providing evidence that the interaction of a carcinogen with a *ras*-proto-oncogene creates the event that initiates tumor formation". Reddy, E.P.; Reynolds, R.K.; Santos, E.; Barbacid, M. (1982) "A point mutation is responsible for the acquisition of transforming properties by the T24 human bladder carcinoma". *Nature*, Vol. 300, pp. 149-152. For a meticulous figure depicting the importance of this last paper see Morange M. (1997). "From the regulatory vision of cancer to the oncogene paradigm, 1975–1985". *J. Hist. Biol.*, Vol. 30, pp. 1–29. (Fig. 1, p. 21).

[11] Guerrero, I.; Villasante, A.; Corces, V.; Pellicer, A. (1984) "Activation of a c-K-*ras* oncogene by somatic mutation in mouse lymphomas induced by gamma radiation". *Science*, Vol. 225, pp. 1159-1162.

[12] Other postdoctorals with prof. Pellicer have been Claudette Boni, Alfredo Villasante, Javier Leon, Elizabeth Newcomb, Joan Berman, Timothy Thomson, Montserrat Corominas, Ramon Mangues, Renata Schiavo, Guillermo Saez, Rosario Oliva, Javier Santos, Antonio Garcia-Espana, Juan Sanchez-Arias, Marcos Malumbres, Neus Ferrer, Su-Ying Lu, Quixia Lan, Inmaculada Hernandez, Yixing Li, Ignacio Perez de Castro, Laura Martello-Rooney, Marta Benet, and Raffi Suzme.

responsible belonged to the *ras* family. After the initial observation, the two main murine genes responsible for the phenotype, N-*ras* and K-*ras* were quickly identified. The following year Pellicer sequenced the complete coding region of N-*ras*, identified the activating mutation and indicated that in some tumors there was loss of the normal allele, suggesting that the wild type copy (proto-oncogene) was exerting some restraining effect on the action of the activated version (oncogene). Further studies continued providing evidence for the spectrum of activating mutations in different mouse strains and by different carcinogenic agents.

The Spanish team with which M. Barbacid solved the question that served to establish the molecular basis of human neoplasia was formed by E. Santos, D. Martín Zanca, and V. Notario. The Spanish research group conducted by A. Pellicer involved V. Corces, P. Calzada, A. Villasante, and A. Pellicer. M. Perucho, working with a team that included as Spanish collaborators C. Almoguera, C. Lama and J. Jordano, afforded the characterization of human and murid cellular oncogenes and provided a method to detect mutations in transcripted genes. A previous communication to this conference reported on the constitutional history of these three groups.[13]

But all these experiments arrived to function just because of the fortunate circumstance that the NIH 3T3 cells were not normal at all, but that they already had accumulated a great number of genetic alterations subjacent to the gradual process of neoplastic transformation. They needed one last push, provided by the dominant oncogene.

Scientific agenda of Ángel Pellicer

The Spanish immunological tradition, on the base of technical applications of these cells, started with J. Salas,[14] the doctoral advisor of M. Perucho, on DNA binding proteins from hamster fibroblasts, normal or transformed by oncogenic retroviruses. Salas was involved during his post-doctoral fellowship at NYU, in the early seventies, with H Green, the creator with G.J. Todaro of this cell line.[15]

In the mid-eighties, Ángel Pellicer's research agenda is represented in Fig. 1.[16] The general topology of this picture differentiates the upper zone, methods used along the researches, from the lower zone, aims and results obtained. As observed by the colors used, all the articles were the result of research supported by governmental programs. Criteria of gene purification and cell-transformation observation on animals, like mice, served to experiment with mutations in oncogenes. A fainter intensity in the visualization of terms, as the corpus is associated with dates, reflects superimposed information that reflects less disseminated researches. The structure of the tree is hidden in the center, but the clades obtained at the two tips of the main branch substantially shows the chemics vs. radiation carcinogenic procedures.

With Ángel Pellicer [17] it must be said that "until 1977, the methods of gene transfer were successful only when DNA of low complexity was used", and that "subsequent improvements made the transfer feasible using total vertebrate and mammalian". Pellicer's historical three viral positive timidine kinase colonies were obtained by using the mouse Ltk- cell line, one of the cell lines at this date that possessed exogene DNA incorporation and subsequent stabilization efficiency high enough to allow gene transfer by using total cellular DNA, with the efficiencies reached by Michel Wigler's modifications of the original method. The other line was NIH 3T3. The experiment by Pellicer conclusively attained the theoretical efficiencies of the minimum value in need to transfer a single copy gen; and in contrast with the first successes of the sixties this experiment was reproducible.[18]

A result gene transfer by using total mammalian DNA was obtained by Pellicer in the Columbia University in 1978.[19] Predoc relationships in Madrid were crucially extended by Pellicer at Columbia on transfection protocols. As in 1979, Pellicer received Barbacid from Bethesda and Perucho from Europe in New York, and he transcripted for both his friends the

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[13] Currás,E.; Wulff Barreiro, E. (2008) "Integration in Europe of human genetics results obtained by Spaniards in the USA: a historical perspective". *Scientometrics*, Vol. 75, Nr. 3, pp. 473-493.

[14] Pollack, R.; Salas, J.; Wang, R.; Kusano, T.; Green H. (1971) "Human-mouse hybrid cell lines and susceptibility to species-specific viruses". *Journal of Cellular Physiology*, Vol. 77, Iss. 1, pp. 117-119. Salas, J.; Green, H. (1971) "Proteins binding to DNA and their relation to growth in cultured mammalian cells". *Nature: New biology*, Vol. 229, pp. 165-169.

[15] On the laboratory of Howard Green as where George Todaro developed the 3T3 lines, see Teebor, G.W. (s.a.) "History of the Department of Pathology of the New York University School of Medicine".

[16] A maximum likelihood (ML) analysis was carried out with *Treecloud*, applying semantic proximity algorithms, see Gambette, P.; Véronis, J. (2009) "Visualising a text with a Tree Cloud". *IFCS'09: International Federation of Classification Societies Conference*.

[17] Pellicer, A. (1986) "Gene purification by transfection methods". In: Kucherlapaty, R., ed. *Gene transfer*. Plenum Press, p. 264 (ref. in vid. nt. 37).

[18] Wigler, M; Pellicer, A; Silverstein, S; Axel, R. (1978, Jul.) "Biochemical transfer of single-copy eucaryotic genes using total cellular DNA as donor". *Cell*, Vol. 14, Nr. 3, pp. 725-731.

[19] Perucho, M. (1994) "La transformación genética de eucariotas superiores y su influencia en el descubrimiento de los oncogenes humanos". In: Casadesús, J.; Ruiz Berraquero, F. (eds.) (1994) *Descifrar la vida*. Sevilla, Universidad. pp. 261-282.

"ownership" of the transfection experiments in the NIH 3T3 cell lines. The next year (1980) came with Pellicer as tenured professor at the NYU Dpt. of Pathology, a tenure in the NYU at Stony Brooks for Perucho and a new cellular and molecular biology lab at NCI with Barbacid at its head.

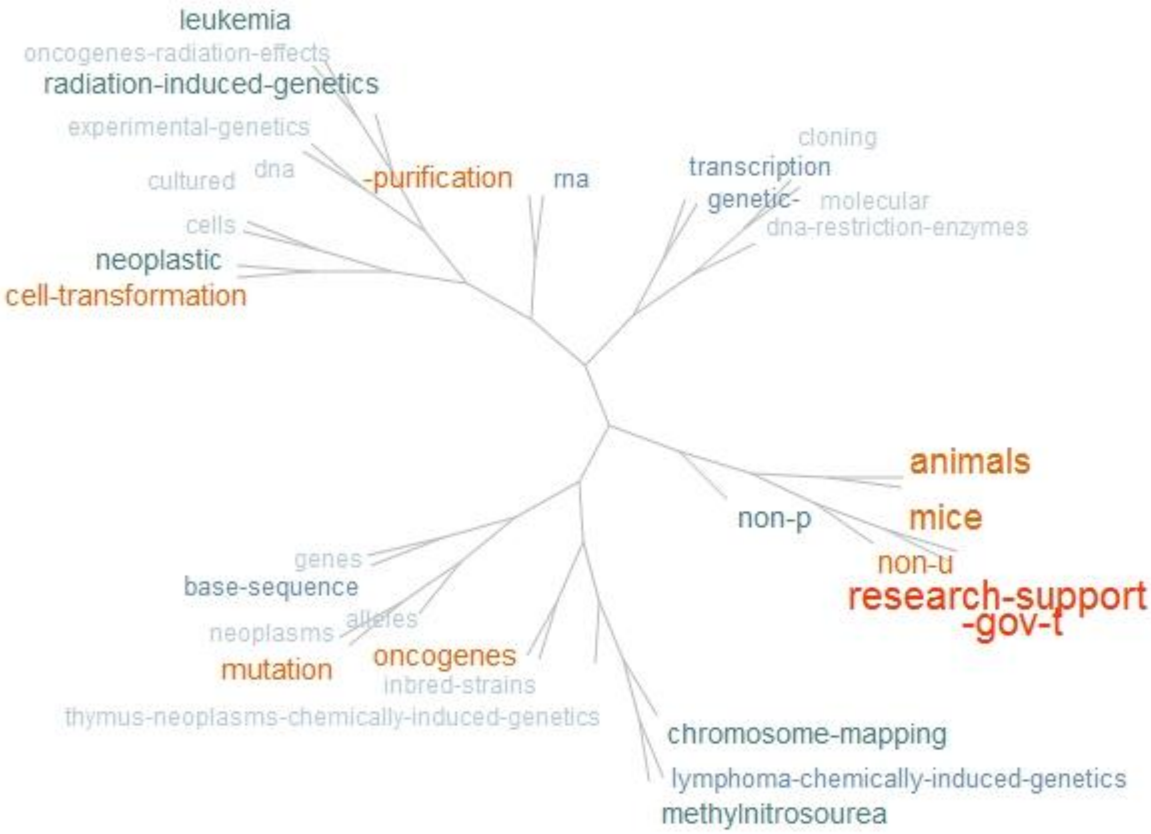

Fig. 1: Maximum likelihood unrooted tree of the tumors scored positive and the genes responsible of it, discovered by Ángel Pellicer.

Once they knew what they wanted from Pellicer, in 1979, how to make growth fibroblasts after successive transfer associated with malignant properties in vivo by using NIH 3T3 cells … all three molecular oncology research groups led by Spaniards decided to identify and isolate human oncogenes through the use of transfection techniques.

In 1982, both the groups of Mariano Barbacid and Eugenio Santos within the National Cancer Institute and the groups of Michael Wigler and Manuel Perucho at the Cold Spring Harbor Laboratory found the oncogene of the T24 line of human bladder carcinoma cells. For his part, Pellicer decided to work in the identification and isolation of murine oncogenes also using transfection techniques in his own lab at NYU Dept of Pathology from 1980.

In the heat of the argument developed by Szybalski,[20] and through his involvement in the integration by cotransformation of all the donor cell genome into recipient chromosomes, the question of whether or not spontaneous neoplastic transformation of 3T3 cells were responsible for unstability by transfection led to met oncogene.

Created in the context of 3T3 cells and at the same time, as a reference scenario for how experiments can "go",[21] 3T6 cells (a line established by transfer of 3 x 10^5 cells to new capsules each six days), were used in 1980,[22] in an ongoing

[20] In 1964, Szybalski reported, by working with a single cell derivative of the Detroit-98 line, originally obtained by nonmalignant human sternal puncture, that the mutant phenotype, the obtention of malignant clones, is determined by the tissular differentiation specific conditions. See Szybalski, W. (1964) !Chemical reactivity of chromosomal DNA as related to mutagenicity: studies with human cell lines". *Cold Spring Harb Symp Quant Biol*., Vol. 29, pp. 151-159.

[21] Knorr-Cetina, K.; Amann, K. (1990) "Image dissection in natural scientific inquiry". *Science, Technology and Human Values*, Vol 15, Iss. 3, pp. 259-283.

study of molecular genetics conducted since 1978[23] at the Molecular Biology Center, Autonomous University in Madrid. The Jesús Ávila group worked in the characterization and analysis of the neuronal proteins associated to a component of the citoskeleton, the microtubules.[24] The 3T6 cell line was treated with the help in chromatin preparation of I. Guerrero.[25]

A systemic interpretation of this scientific community

A strictly increasing process defines the cumulative monotonicity of scientific authority in terms of single-peakedness. It is what can fix the attention on the close competition in the race for oncogenes between the three core members of this Spanish community in the US, Barbacid, Pellicer and Perucho. This relation between individual careers consists in being continually approached, as source authors and as cited authors, by the dynamics of the scientific community.[26] A number of interacting processes can be advanced, each embedded in Fig. 2.

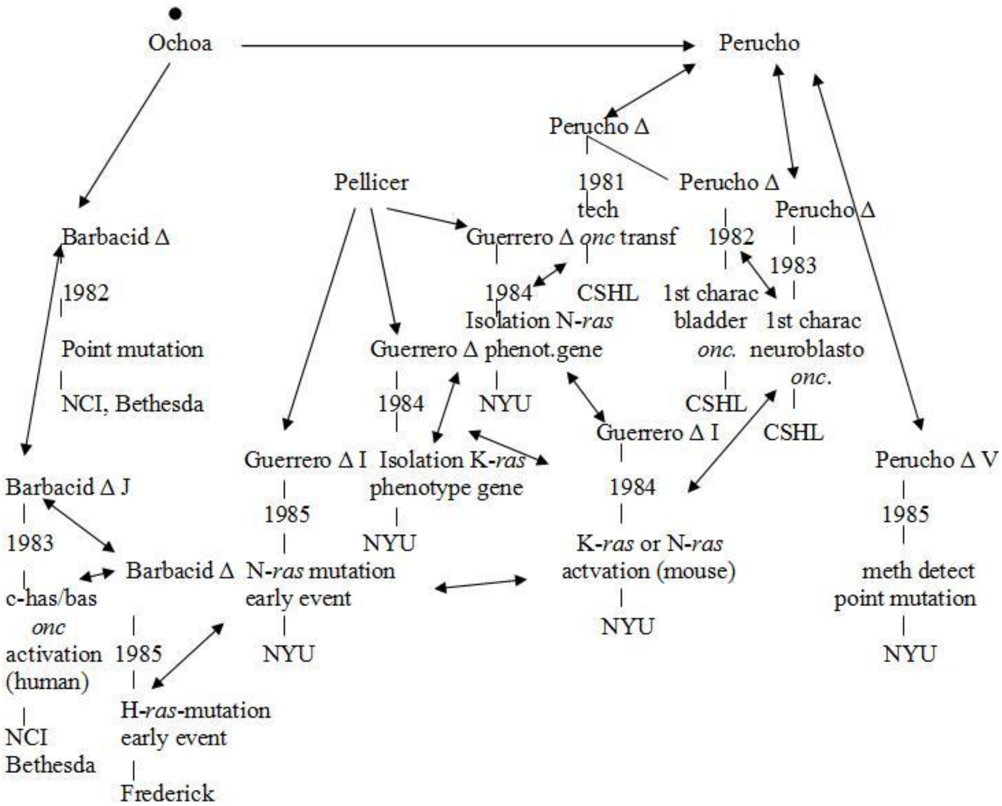

Fig. 2: Schematic view of the way in which Spanish researchers produced mirror symmetrical results in US universities and scientific organizations. Among the three research teams racing for oncogenes. *V*'s and *J*'s symbolize the articles that were presented by S. Ochoa to US Natl Acad. *I*'s illustrate the track of the M. Heidelberger presented group. The small deltas, *Δ*, represent NIH 3T3 cell cultures used to obtain cleavage. Two SCI research fronts are present, 83-1740 "Oncogenes and the genetics of human cancer; viral

[22] Villasante, A.; Corces, V.G.; Manso-Martinez, R; Avila, J. (1981) "Binding of microtubule protein to DNA and chromatin: possibility of simultaneous linkage of microtubule to nucleic-acid and assembly of the microtubule structure". *Nucleic Acids Research*, Vol. 9, Nr. 4, pp. 895-908.

[23] Wiche, G.; Corces, V.G.; Ávila, J. (1978) "Preferential binding of hog brain microtubule-associated proteins to mouse satellite versus bulk DNA preparations". *Nature*, Vol. 273, Nr. 5661, pp. 403-405.

[24] Microtubules associated to neuroblastoma cell differentiation, otherwise useful for centromeric localization. see human artificial chromosome new gene therapy at http://www.garfield.library.upenn.edu/histcomp/human-artificial-chromosomes/

[25] Non-histone proteins in this sequence of chromosomal regions, chromatin (from D. melanogaster embryos), was her dissertation subject at the Autonomous University of Madrid, defended on January 1, 1981. Guerrero research program has been concerned mainly with D. melanogaster with the exception of the three years that she spent at NYU Dpt of Pathology.

[26] Price, D.J. de S. (1976) "Studies in scientometrics, part 2: The relation between source author and cited author populations". *International Forum on Information and Documentation (Moscow)*, Vol. 1, Iss. 3, pp. 19-22.

transforming genes and their DNA structure" (left side), and 84-4046 "Characterization of human and murine cellular oncogene" (right side).
Abbreviations: CSHL –Cold Spring Harbor Laboratory; NYU –Department of Pathology, New York University Medical Center; NCI –Laboratory of Cellular and Molecular Biology, National Cancer Institute, Bethesda, Maryland; Frederick –Developmental Oncology Section, Frederick Cancer Research Facility, Maryland.

Two research fronts are present here, 83-1740 "Oncogenes and the genetics of human cancer; viral transforming genes and their DNA structure", and 84-4046 "Characterization of human and murine cellular oncogene".

Negative differences separate the two major groups of scientists that define the population of authors inside the program. It means that the problem requiring much more authority than they had assigned, continual competitive behavior linked their productions. Although they did not publish so much in all these years, they were also consistent with the modelled research program. They were "underqualified" as source authors, although "fair" as cited authors. They are Guerrero and Pellicer.

On the question of how much affects individual scientists or its constituent parts (groups of scientists) in this demography, the fact is that only a woman (Guerrero) is allocated to treat the program. This knowledge must be described as an insularity that requires a distortion of the original structure before its narrative can be fitted neatly into place.[27] Figure 2 provides a sketch of this social power process.[28]

Conclusion

The competing oncogene theory, by Robert Huebner and George Todaro, hypothesizing a genetic rather than an infectious mechanism for cancer, and Temin's evolutionary protovirus hypothesis were within the scope of the biology performed in Spain from the early seventies, when José Salas was involved during his post-doctoral fellowship at NYU, with H Green, the creator with GJ Todaro of the NIH 3T3 cell line. Also tumor virology before 1965 had in Francesc Duran-Reynals (Barcelona, 1899 - New Haven, 1958) one of the major figures in the attempt to establish the connection between tumor virus and ordinary viruses. And so it should not be assumed that the viral-genetic concept, which develops the insertion of a viral genome into cellular one, did not exist as early as in the 1920s. It follows that when a new molecular concept of tumor growth was advanced describing cancer as dysregulation of individual genes, a unifying concept of genetic cell abnormalities as a central determinant of cancer emerged from several Spanish leaders of US laboratories in the early 1980s.

To be true, in terms of exogenous and endogenous factors which do trigger cellular oncogenes, the new carcinogen theory worked with a cell line (NIH 3T3) with a good number of genetic alterations subjacent to neoplastic transformation. As trained in chromatin patterns, Isabel Guerrero managed to be in profound understanding of 3T3 cells in Spain, through her involvement in the molecular biology of a component of the citoskeleton, the microtubules. By the hope that in spite of the bias introduced in the analysis by the method of detection, derived from the hazardous amount of previous infection behind NIH 3T3 cells, she integrated A. Pellicer lab in the NYU, in the race for oncogenes that occurred in the mid-eighties. To work under the assumption that 10-15% of all the human tumors analyzed contained activated ras oncogenes. That indicated that it constituted an important biological event and that its relevance was not overemphasized by the selection of experimental systems.

Two of the main competitors were M. Barbacid, a Spanish chemist that started his virologist work with S. Aaronson, and M. Perucho who had work with M. Wigler in plasmid rescue and gene transfer.

The selective controversy surrounding the NIH 3T3 cells made them a revealing tool perceived as a frontier associated to malignant properties in vivo. In connection with signaled results from the labs of Barbacid at Bethesda and Perucho at Stony Brook, after previous successes well reported by S. Ochoa at US Natl Acad of Sci. Under the auspices of Pellicer a unique role for oncogene N-*ras* in mammals hematologic malignancies was suggested. So answering the question of whether it were the cultivated cells that activated their gene after transfection while a different oncogen was operative in primary tumor cells.

By providing additional evidences in favor of *ras* gene activation in early stages, Pellicer supplied robust support to the hypothesis that *ras* mutation is the primary event in carcinogenic models. He also reported on thymic lymphomas in favor

[27] Price, D.J. de S. (1981) "The development and structure of the biomedical literature". In: Warren, K.S., ed. *Coping with the biomedical literature: a primer for the scientist and the clinician*. Praeger, pp. 3-16.

[28] Nowakowska, M. (1984) *Theories of research*. Seaside, California, Intersystems publications. (see pp. 164-169, 'Reference frames').

of genetic arguments linking the mutated version of *ras* genes with tumor development and against claims that *ras* genes are activated by truncation and not by point mutation. As contributing in favor of γ-radiation as a source of oncogene mutations he crucially intervened in the debate on mutation specificity for a carcinogen.

The inestimable position of Spanish biologists, in touch with NIH 3T3 cells from its inception, made them technically robust in the search for *ras* oncogenes. A medical doctor, Ángel Pellicer, activated and characterized the genes involved in point mutations by using viral transforming genes obtained through transfer methods developed by himself, first in Columbia University and later in the Dpt of Pathology, NYU. The responsibility for the initiation or progression of tumors, and the issues of mutation specificity have been his successful path to solve the dialectic between the chemist and the biologist in the first steps of molecular oncology.

References

CURRÁS, E.; WULFF BARREIRO, E. (2008) "Integration in Europe of human genetics results obtained by Spaniards in the USA: a historical perspective". *Scientometrics*, Vol. 75, Nr. 3, pp. 473-493.

GAMBETTE, P.; VÉRONIS, J. (2009) "Visualising a text with a Tree Cloud". *IFCS'09: International Federation of Classification Societies Conference.*

GARTE, S.J.; HOCHWALT, A.E. (1989) "Oncogene activation of experimental carcinogenesis: the role of carcinogen and tissue specificity". *Environmental Health Perspectives*, Vol. 81, pp. 29-31.

GRIFFITH, F. (1928) "The Significance of Pneumococcal Types". *Journal of Hygiene*, Vol. 27, Iss. 2, p. 113.

GUERRERO, I.; VILLASANTE, A.; CORCES, V.; PELLICER, A. (1984) "Activation of a c-K-*ras* oncogene by somatic mutation in mouse lymphomas induced by gamma radiation". *Science*, Vol. 225, pp. 1159-1162.

HERZBERG, M.; REVEL, M. (1973) *Atlas de biología molecular*. Barcelona, Omega.

HUEBNER, R.J.; TODARO, G.J. (1969) "Oncogenes of RNA tumor viruses as determinants of cancer". *Proceedings of the National Academy of Sciences of the USA*. Vol. 64, Nº 3, pp. 1087-1094.

JAINCHILL, J.L.; AARONSON, S.A.; TODARO, G.J. (1969) "Murine sarcoma and leukemia viruses: assay using clonal lines of contact-inhibited mouse cells". *Journal of Virology*, Vol. 4, Iss. 5, pp. 549-553.

KISSELEV, L.L.; ABELEV, G.I.; KISSELJOV, F. (1992) "Lev Zilber, the personality and the scientist". *Advances in Cancer Research*, Vol. 59, pp. 1-40.

KNORR-CETINA, K.; AMANN, K. (1990) "Image dissection in natural scientific inquiry". *Science, Technology and Human Values*, Vol 15, Iss. 3, pp. 259-283.

MORANGE, M. (1997). "From the regulatory vision of cancer to the oncogene paradigm, 1975–1985". *Journal of the History of Biology*, Vol. 30, pp. 1–29.

NOWAKOWSKA, M. (1984) *Theories of research*. Seaside, California, Intersystems publications.

PELLICER, A. (1986) "Gene purification by transfection methods". In: Kucherlapaty, R., ed. *Gene transfer*. Plenum Press.

PERUCHO, M.; GOLDFARB, M.; SHIMIZY, K.; LAMA, C.; FOGH, J.; WIGLER, M. (1981) "Human-tumor-derived cell lines contain common and different transforming genes". *Cell*, Vol. 27, Iss. 3 Pt 2, pp. 467-476.

PERUCHO, M. (1994) "La transformación genética de eucariotas superiores y su influencia en el descubrimiento de los oncogenes humanos". In: CASADESÚS, J.; RUIZ BERRAQUERO, F. (eds.) (1994) *Descifrar la vida*. Sevilla, Universidad. pp. 261-282.

POLLACK, R.; SALAS, J.; WANG, R.; KUSANO, T.; GREEN H. (1971) "Human-mouse hybrid cell lines and susceptibility to species-specific viruses". *Journal of Cellular Physiology*, Vol. 77, Iss. 1, pp. 117-119.

PRICE, D.J. de S. (1981) "The development and structure of the biomedical literature". In: WARREN, K.S., ed. *Coping with the biomedical literature : a primer for the scientist and the clinician*. Praeger, pp. 3-16.

REDDY, E.P.; REYNOLDS, R.K.; SANTOS, E.; BARBACID, M. (1982) "A point mutation is responsible for the acquisition of transforming properties by the T24 human bladder carcinoma". *Nature*, Vol. 300, pp. 149-152.

SALAS, J.; GREEN, H. (1971) "Proteins binding to DNA and their relation to growth in cultured mammalian cells". *Nature: New biology*, Vol. 229, pp. 165-169.

SANTOS, E.; REDDY, E.P.; PULCIANI, S.; FELDMANN, R.J.; BARBACID, M. (1983) "Spontaneous activation of a human proto-oncogene". *Proceedings of the National Academy of Sciences of the USA*, Vol. 80, № 15, pp. 4679-4683.

SILBER, L. (1923) "Ueber das Wesen der Weil-Felixschen Reaktion". *Centralblatt für Bakteriologie, Parasitenkunde und Infektionskrankheiten* 1. Abt. Originale. Bd. 89. Heft 7/8. pp. 250-259.

SPENCER, M.; DAVIDSON, E.A.; BARBROOK, A.C.; HOWE, C.J. (2004) "Phylogenetics of artificial manuscripts". *Journal of Theoretical Biology*, Vol. 227, Iss. 4, pp. 503-511.

SZYBALSKI, W. (1964) "Chemical reactivity of chromosomal DNA as related to mutagenicity: studies with human cell lines". *Cold Spring Harbor Symposia on Quantitative Biology*, Vol. 29, pp. 151-159.

TEEBOR, G.W. (s.a.) "History of the Department of Pathology of the New York University School of Medicine".

TODARO, G.J.; GREEN, H. (1963) "Quantitative studies of growth of mouse embryo cells in culture and their development into established lines". *Journal of cell biology*, Vol. 17, Iss. 2, pp. 299-313.

VILLASANTE, A.; CORCES, V.G.; MANSO-MARTINEZ, R; AVILA, J. (1981) "Binding of microtubule protein to DNA and chromatin: possibility of simultaneous linkage of microtubule to nucleic-acid and assembly of the microtubule structure". *Nucleic Acids Research*, Vol. 9, Nr. 4, pp. 895-908.

WICHE, G.; CORCES, V.G.; ÁVILA, J. (1978) "Preferential binding of hog brain microtubule-associated proteins to mouse satellite versus bulk DNA preparations". *Nature*, Vol. 273, Nr. 5661, pp. 403-405.

WIGLER, M.; PELLICER, A.; SILVERSTEIN, S.; AXEL, R. (1978, Jul.) "Biochemical transfer of single-copy eucaryotic genes using total cellular DNA as donor". *Cell*, Vol. 14, Nr. 3, pp. 725-731.

WINTER, E.; YAMAMOTO, F.; ALMOGUERA, C.; PERUCHO, M. (1985) "A method to detect and characterize point mutations in transcribed genes: Amplification and overexpression of the mutant c-Ki-*ras* allele in human tumor cells". *Proceedings of the National Academy of Sciences of the USA*, Vol. 82, № 22, pp. 7575-7579.

REPRESENTATIONS OF SCIENCE AND TECHNOLOGY IN THE EUROPEAN DAILY PRESS

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 839.

EPIDEMICS IN THE NEWS: HEALTH AND HYGIENE AS SEEN BY THE DAILY PRESS IN PERIODS OF CRISIS

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Abstract

As part of a larger project focused on producing a History of the Popularization of Science and Technology in Portugal, my aim is to find out how scientific knowledge reached the common people, using newspapers as the main source of information. This research is being developed in the project «Reading newspapers: An open window to representations of Science and Technology in the Portuguese Press (1900-1926)». Considering the population's limited access to written material, nevertheless each newspaper could be read daily by an estimate 30,000 people in Lisbon, which places this source as probably the most widespread vehicle to divulge the latest scientific news at the time to an unspecialised audience.

In times of health crisis, newspapers are particularly important sources to access the type of information and advice given to the public and the sanitation measures taken by the authorities. In this paper the goal is to compare several epidemics which scourged the world during the nineteenth and twentieth centuries, by analysing their impact in Portuguese newspapers. It is also our goal to contribute to a larger conscience of the reality of everyday life. Social historians have studied the *History of Private Life* and have produced important books on the subject, especially since the works of Philippe Ariés and Georges Duby in 1985. But this research on the popularization of science hopes to improve these studies, with our findings on how and when technologic discoveries and scientific knowledge were first introduced into people's lives and the way they came to be used by society. Regarding disease, hygiene and epidemics, we hope our contribution on the subject shall help us realize the real difficulties posed on the life of people of every class. Whoever lives in our contemporary "civilized" society probably has no idea of life without vaccines and penicillin, and children dying as a part of everyday life, without even the right to mourning or sadness.

The following mainstream newspapers were thoroughly researched, in order to transcribe all the news on science and technology:

Lisbon:

- *Século*, 1855 (another newspaper with the same title was published from 1881 to 1977).

- *Diário de Notícias*, whose purpose was "to be of interest to all classes, accessible to all purses and understood by every level of intelligence", 1864.12.29 to present.

Oporto:

- *O Comércio*, 1854.06.02 to 2005.07.30. On 1856 its name was changed to *O Comércio do Porto*.
- *O Eco Popular*, 1847 to 1860.

No doubt, in late nineteenth century Portugal, society references, whether they were political, economic of scientific, originated in the media and in the people who wrote them. And all these newsmen shared a tendency for rational and clear thought, as well as an emphasis on science and knowledge with practical utility, and an optimism regarding mankind's capacity to dominate nature and understand the world.

Simultaneously, the scientific community understood the need to divulge its recently acquired knowledge. As scientists were becoming professionals and boundaries were established between them and the public, they also realized that the recognition of their work was necessary for the recognition of their own new professional status. Technological improvements were meant to be used by a large number of people, otherwise they were meaningless. Therefore, they had to be divulged to a wide audience. Regarding medicine, and particularly in times of epidemics, divulging information became a matter of survival, for prevention was the most important means for dealing with a health crisis. Throughout the nineteenth century, the time of the great pandemics, the hygienist discourse was used as the prime way to bring medicine into public life. And authorities used this new resource to fight epidemics, introducing it into official reports which were published in generalist newspapers and pamphlets. Otherwise they would have lost the battle.

Our research focuses on collecting news and advertisements that reveal the scientific knowledge of the time. A large database was built and the news and ads on science and technology were classified according to a classificatory grid, which includes the following themes: Science; Science Education; Exhibits / Conventions; Museums; Personalities /Individuals; Publications on science; Accidents, risks and anomalies; Sanitary science / hygiene, health and medicine; Scientific Institutions / Societies; Technology / Innovation; Travels/ Scientific Expeditions.

Advertisements were also an important source of information, because they were fundamental for educating this early audience. Advertising is an element of progress, of economic and social development. It orients, it supplies information, and it spreads culture, hygiene habits, elegance and good taste. It generates markets, increases demand, intensifies production and lowers prices.

Oporto was particularly affected by epidemics. In the nineteenth and early twentieth century the second Portuguese town presented special conditions for the spread of epidemics, because it was suffering a process of industrial growth and it had a mobile population living in the worst possible conditions as far as hygiene was concerned. There were some neighbourhoods called "islands" where there were concentrations of factory workers which rotated on the same bed and the filth and misery were considered extremely dangerous on all official reports. Every news described Oporto as an infected city with deplorable sanitary conditions.

In sum, these were the major epidemics of the time in Oporto and the amount of scientific knowledge about them (table 1).

The cholera **morbus epidemic** in 1855 (which spread throughout the entire country and was followed by yellow fever in Lisbon in 1857), the **bubonic plague** in 1899, in 1918 **typhus** and **Spanish flu**, and later in the 1980s the AIDS epidemic. In 2009 the H1N1 flu was declared a pandemic and there were prevention measures being encouraged and enforced everywhere, but the main issue remains the same: **hygiene**. This an especially important concern now that penicillin is facing multi-resistant bacteria and has proven ineffective with viruses. Prevention measures were always fundamental and both in the nineteenth and the twentieth century the scientific community has tried hard to educate the people, through the press, repeatedly providing important information. Teaching people how to prevent disease by explaining hygienic procedures has been a constant concern in the press and other more recent media in periods of health crisis. News on the subject reveals the state of the art of medical science in each of those periods. It also reveals the importance given by health authorities and journalists to the publication of the most recent discoveries and adequate hygiene procedures to prevent the spread of the epidemics. This is an important subject that can contribute to the debate on the dissemination of science and technology and assert the place that Portugal, a peripheral country, occupied in the European and North American scientific community in the last two centuries. It was not that far apart from the so called more developed countries, because it was an active participant in every international sanitary conference and because newspapers reveal that Portugal was up to date with every scientific and technological development. As least in knowledge, even if sometimes it was not properly applied.

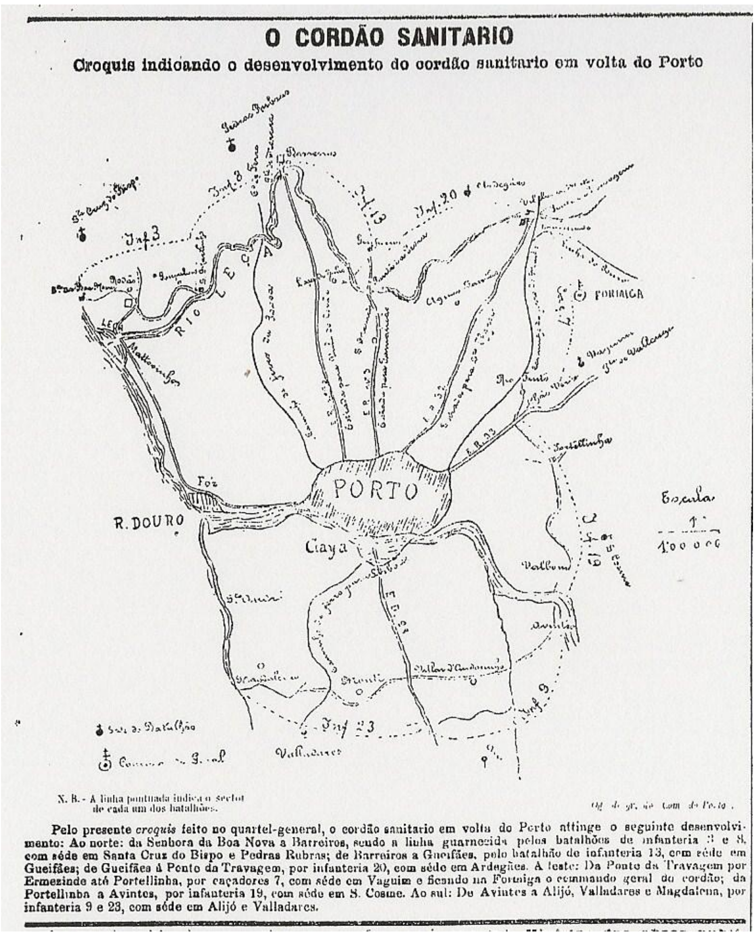
We can observe an increase in the scientific knowledge, particularly in the causes for the transmission of the diseases, and the prevention measures. Nevertheless, treatments were still scarce and the mortality was huge, even when the virus, bacteria and bacillus were identified.

Regarding bubonic plague in 1899, authorities established a sanitary cord around Oporto (see figure 1).

Table 1.

Oporto's Epidemics	Cholera morbus	Bubonic plague	Typhus	Spanish flu	Smallpox
Date	May 1855 – 1857	June 1899 – January 1900	December 1917 – August 1918	June 1918 – December 1918	June 1918 – December 1918
Death toll	22.700 all over the country	111, out of 326 cases	278, out of 2.781 cases	59.000, but later studies indicate up to 135.257	Unknown
Mortality rate	45 per cent	34 per cent	10 per cent	9.8 per thousand of the entire population	Unknown
Causes	Unknown	Bacillus discovered by Yersin and Kitasato Shibasaburō	Bacteria of exanthematic typhus, identified by H. T. Ricketts and Stanislaus von Prowazek	Partly identified, its virus was still ignored	Smallpox virus, identified by Edward Jenner
Supposed causes	Misery, poor hygiene, excessive behaviour, bad eating habits, miasmas, terror	Poor hygiene, misery, fleas, mice, “a microbe”, excessive behaviour	A virus transmitted by lice, lower classes, poorly housed and fed, particularly beggars and criminals	A bacteria, misery, lack of food	
Transmission	Unknown, contagion denied, food, water	Fleas, from mice; through the skin, the nose and the mouth	Lice	Air; First World War, military and farm workers migrations	Air, skin
Sanitary measures	Isolation of the sick and of the entire city, quarantine, hygiene, special hospitals, markets prohibited, schools closed	Isolation of the sick and the entire city, hygiene, special hospitals, markets prohibited, disinfection of buildings and clothes, compulsory baths, sanitary visits with the police, travelling and passengers inspection, children hunting for mice, face mask	Removing lice, body, clothes and house hygiene and disinfection; compulsory public baths and house burning. Isolation of patients and their family and neighbours; house calls (sanitary visits) by health delegates and the police, schools closed.	No known prophylactics, general hygiene and medical assistance, isolation in hospitals. Schools closed, no exams in the universities, markets and fairs prohibited, compulsory notification of the disease. Individual prophylactics: mentholated or salted gargles. Prescriptions for the poor are free. Charitable and well off members of the communities should be encouraged to create private care comities.	Vaccination

Oporto's Epidemics	**Cholera morbus**	**Bubonic plague**	**Typhus**	**Spanish flu**	**Smallpox**
Treatments	Specific treatment unknown. Spirit of camphor, scrubbing the patients with warm salt water, spirit drinks, spearmint, iodine, leeches	Yersin serum	Unknown. For removing lice: petroleum, turpentine, benzene applied on the body; burning sulphur was used for clothes and whitewash for houses.	Specific treatment unknown. Drugs: aspirin, quinine, ammonia, purgative salts, caffeine, camphor oil, mustard and flax seed, borage tea, sodium sulphate in lemon juice with magnesium, anti-pneumococcus serum, sodium benzoate, tolu balsam, benjoim, intravenous injections of glucose, sugar and proper nourishment.	Unknown


O CORDÃO SANITARIO

Croquis indicando o desenvolvimento do cordão sanitario em volta do Porto

N. B. - A linha pontuada indica o sector de cada um dos batalhões.

Pelo presente *croquis* feito no quartel-general, o cordão sanitario em volta do Porto attinge o seguinte desenvolvimento: Ao norte: da Senhora da Boa Nova a Barreiros, sendo a linha guarnecida pelos batalhões de infanteria 3 e 8, com séde em Santa Cruz do Bispo e Pedras Rubras; de Barreiros a Gueifães, pelo batalhão de infanteria 13, com séde em Gueifães; de Gueifães á Ponte da Travagem, por infanteria 20, com séde em Ardegães. A leste: Da Ponte da Travagem por Ermezinde até Portellinha, por caçadores 7, com séde em Vaguim e ficando na Formiga o commando geral do cordão; da Portellinha a Avintes, por infanteria 19, com séde em S. Cosme. Ao sul: De Avintes a Alijó, Valladares e Magdalena, por infanteria 9 e 23, com séde em Alijó e Valladares.

Fig. 1: “The sanitary cord”, *O Comércio do Porto*, 1899.08.26.

During this period many houses were burnt down. Compulsory baths were established for the lower classes, in public baths that were built for that purpose, and children were encouraged to hunt for mice, to deliver them in police precincts, and they were paid for them: "big rats 20 cents, small mice 10 cents each".

As in other earlier epidemics, many scientific international reports on the disease were transcribed and commented upon in the newspapers.

And one Lisbon doctor even invented a prophylactic mask to avoid the transmission of the plague through the nose and the mouth (see figure 2). It was supposed to be used by doctors and nurses dealing with plague patients and the device should be covered by cotton and gauze, imbibed in an anti-septic solution. This was quite an innovation for it was only in 1918, during the influenza, that the use of face masks became popular in Europe.


o algodão e a gaze caem sem ser preciso tocar-lhes, mandando-se queimar, no passo que as hastes metallicas se desinfectam logo a uma

Mascara prophylactica de metal

Quadrado d'algodão revestido exteriormente de gaza embebido n'uma solução anti-septica

Mascara em posição

chamma d'alcool, de que o observador deve estar munido.

O engenhoso apparelho, cuja simplicidade corresponde á efficacia, acha-se em exposi-

Fig. 2: *Diário de Notícias*, 1899.08.24.

Spanish doctors went to Oporto to study the disease and wrote reports that describe the entire work done during the epidemic. Their journey was also reported on the news. They were Jaime Ferran, Frederico Viñas y Cusi, Rosendo de Grau and Federico Montaldo. Regardless of the knowledge of the disease, and the existence of the Yersin serum, that came from the Pasteur Institute in Paris, one of the main doctors who were handling the crisis died from the plague: Câmara Pestana, the Director of the Bacteriological Institute.

In 1918 there was still a typhus epidemic when the first cases of influenza arrived in Portugal from the Spanish border. The first news in Portuguese newspapers concerned the disease in Spain, which hit King Alfonso XIII and the Portuguese ambassador in Madrid Dr. Egas Moniz, who later won the Nobel Prize of Medicine (1949). The first report by Professor Ricardo Jorge warned people about the speed of the contagion. There were no known effective prophylactics, other than general hygiene and medical assistance to sick people, preferably isolated in hospitals.

Ads reflected hygiene concerns, with high incidence on drugs, soaps and disinfectants and even wool coats to avoid the cold, all of them with large titles with the words "flu" and "epidemics" on them. For example, there was a cough syrup which was usually advertised as good for cough, for tuberculosis patients, and others. Now it was good for the flu and bronchitis and pneumonia.

The epidemic hit its peak on October 1918. Even though newspapers continued to report how benign it was in the cities, in order to prevent panic, letters from the correspondents on the countryside, published on the same page, described the amount of orphans and the "painful", "dreadful" and "frightful" disease, together with "extreme misery" and "prolonged suffering".

It is also important to notice that private assistance to the victims and their families was one of the major sources of social welfare at the time, and newspapers were an important factor on that matter. *Diário de Notícias* was able to gather 12,000$000 in two weeks, after having already received 80,000$000 on the two previous months for the prisoners of war. Public figures called rich people to charitable actions and advised them to create committees and solve the problems. And they contributed with their own money for actions that the State could not provide.

At the same time, smallpox had been there, but its intensity increased in June all over the country, particularly in the north. The big concern was vaccination. In 1918 vaccination was not compulsory by law, but was compulsory in practice, because people could not work or go to school without taking it, a rule which still applies in the present. It was posted in every neighbourhood that "no individual over 8 years old may go to school, or work in a workshop, factory, shop or industry, take an exam or public office, without proof of vaccination, revaccination, or having suffered a smallpox assail in the last 7 years. School principals and industry or commerce directors are responsible for enforcing this law, or they shall be fined".

The vaccines were administered for free in health delegations all over the country, and also by the Red Cross. With the 1918 epidemic many new vaccination facilities were created and it was also administered in schools, asylums, prisons, police precincts and other gathering places.

However, these periods of epidemics were not so different from the rest of the time. And this can also be found on the news: sanitary bulletins were printed daily on newspapers and they listed the cases that were reported to medical authorities (which were certainly just a small fraction of the total amount). An average citizen, in the course of his or her existence, would most certainly suffer one or more of the endemic diseases that assailed and killed people until the mid-twentieth century, when vaccines and penicillin were made available to most of the western hemisphere: smallpox, leprosy, tuberculosis, syphilis, typhoid fever, malaria and other intermittent fevers, poliomyelitis, rubella, measles, diphtheria, scarlet fever, tetanus, meningitis, whooping cough and the itch (*sarna*). And also the seasonal influenza, gastritis, and the occasional hydrophobia (rabies), leishmaniosis, lyme disease (transmitted by ticks), brucellosis (transmitted by sheep) and anthrax, formally known as carbuncle (often transmitted by cattle). To these we can add the so called social pathologies, such as alcoholism, which weakened the race (Correia 1938).

Even in normal times there were regular news and ads concerning all the other diseases, such as tuberculosis and its treatments, sanatoriums and spas, research and new drugs, and many others. Therefore, throughout the entire period that is under investigation, news and ads on health and hygiene always had the highest percentage among science and technology news and ads, and reports on diseases and treatments were prevailing.

Therefore we can imagine the fear people lived in. Not only in periods of epidemics, but in everyday life. In the newspapers we find reports on panic attacks and terror caused by the epidemics, generally because of the unknown factor, but also reports on wild dog and wolf attacks, which passed on rabies and entire families ended up dying. This was surely the cause for our mythology on wolves and children and the fear associated: not exactly wolves eating children, but wolves and wild animals transmitting rabies. During the cholera epidemic, terror was even considered one of the causes of the disease.

Hospitals were a particularly frightful place and there were people who chose suicide over going to the hospital, when authorities came to take them to isolation. During the 1899 plague there were riots in the streets when the police and the army were called to force the sick people and their relatives and neighbours to go to the hospital. And during the typhus epidemic in Oporto, a servant at a hotel was diagnosed with the disease and was told he was meant to go to the hospital. He was so horrified that he shot himself in the head.

Concluding remarks

I should like to conclude with a reference to hand washing, after having showed the interest in face masks:

1. As early as 1854, in the beginning of the cholera epidemic, the use of gloves by doctors is referred to as nonsense.
2. During the bubonic plague epidemic there is only one reference to hand washing on official reports. One doctor, a surgeon of the municipal guards, advises the military personnel that in case of contact with a sick person with the suspicious disease, one should immediately proceed to a long hand wash with hot water and soap, followed by disinfection with vinegar and lemon.
3. The Portuguese consul in China, speaking from his experience on a country where this disease was endemic, advised people not to shake hands with everybody and wash them often with energetic disinfectants (*Diário de Notícias*, 1899.08.21).
4. This was also the first time ads wrote about contaminated objects and the need to wash hands with a bar of soap that disinfects them. There is no doubt that advertisements had an important educational value for newspaper readers, in a time when not even doctors put hand washing as a priority of disease prevention in official bulletins.
5. In 1899, an official report by Dr. Ricardo Jorge: hands shall be washed with a disinfecting solution; if there isn't any, hot water shall be used, with soap, or they shall be immersed in vinegar or alcohol. Do not forget meticulous cleaning of nails with a brush. Clothes shall be disinfected with lye or copper sulphate.
6. In 1918 hand washing was still not a priority. During the typhus epidemic there were compulsory public baths and clothes disinfection with burning sulphur. But the only reference to hands is found on a medical report which includes a list of disinfectants and a reference to applying lotions on the most exposed parts of the body, such as hands and feet. Those lotions should be made of mentholated vaseline and camphor oil.
7. The only reference made to hand washing during the Spanish Flu was not by Professor Ricardo Jorge, the National Director of the Health Department, in his official reports, but by Dr. Giraldino Brites, an attendant at Lisbon's University Hospital, who interviewed Dr. Raul de Carvalho, the head of the laboratory of the same hospital, and described a few prophylactic measures for the flu, which included good nourishment and hand disinfection before meals with a few drops of formaldehyde in water or any antiseptic soap.

And this resumes, on a 6,500 entries database and a 60 year time span, the references to hand washing. Hygiene still had a long way to go in 1918.

Also interesting in this research is the persistence, in all epidemics, of the behaviour factor: disease as punishment for excessive and unruly behaviour. This is a religious inheritance which science absorbed and still does not deny, since it has incorporated it into its speech. It manifested again during the AIDS epidemics in the eighties and even during the 2009 flu. Poor hygiene and bad behaviour always seem to be associated with disease and there is always guilt associated. Presently, with the overweight epidemic there is also the bad behaviour factor, and even with cancer. Science knows there are many other factors which are as important to those diseases as social behaviour, such as genetics and the environment, but people are still bombarded with behaviour and guilt, and most of the time, like with cholera or the plague, they don't even know how they got sick…

References

ACKERKNECHT, E. H. (1948), "Anticontagionism between 1821 and 1867", *Bulletin of the History of Medicine*, 22, pp. 562-593.

ALMEIDA, António Ramalho de (2006), *O Porto e a Tuberculose. História de 100 Anos de Luta*, Porto, Fronteira do Caos Editores.

ALMEIDA, Maria Antónia Pires de (2008), "Percursos de pobreza em meio rural: as mulheres, a doença e o aborto", *in* André FREIRE (ed.), *Sociedade Civil, Democracia Participativa e Poder Político. O Caso de Referendo do Aborto, 2007*, Lisboa, Fundação Friedrich Ebert, pp. 21-39.

BALDWIN, Peter (2005), *Contagion and the State in Europe, 1830-1930*, Cambridge, Cambridge University Press.

BAUER, Martin W. (2009), “The evolution of public understanding of science – discourse and comparative evidence”, *Science, technology and society*, 14 (2). pp. 221-240.

BAUER, Martin W. and Massimiano BUCCHI (eds.) (2007), *Journalism, science and society: science communication between news and public relation*, Routledge, London, UK.

BENSAUDE-VINCENT, Bernadette (2009), “A Historical Perspective on Science and Its ‘Others’”, *Isis*, 100, pp. 359-368.

CALMETTE, A., A. SALIMBENI (1899), “La Peste Bubonique – Etude de l’Epidémie d’Oporto en 1899”, *Annales de l’Institut Pasteur*, pp. 865-936.

CORREIA, Fernando da Silva (1938), *Portugal Sanitário (Subsídios para o seu estudo)*, Lisboa, Ministério do Interior – Direcção Geral de Saúde Pública.

FERRAN, Jaime, Frederico VIÑAS y CUSI, Rosendo de GRAU (1907), *La Peste bubónica: memoria sobre la epidemia ocurrida en Porto en 1899*, Barcelona, Tip. Sucesor F. Sanchez, 1907.

GARNEL, Rita (2009), “Portugal e as Conferências Sanitárias Internacionais (Em torno das epidemias oitocentistas de cholera-morbus)”, *Revista de História da Sociedade e da Cultura*, 9, pp. 229-251.

GOMES, Bernardino António (1866), *Aperçu historique sur les épidémies de choléra-morbus et de fièvre jaune en Portugal, dans les années de 1833-1865*, Constantinople, Imprimerie Centrale.

HAMLIN, Christopher (2001), “Contagion and the State in Europe, 1830-1930 (review)”, *Bulletin of the History of Medicine*, Volume 75, Number 1, pp. 137-139.

JORGE, Ricardo (1885), *Hygiene social applicada à Nação Portuguesa: conferências feitas no Porto*, Porto, Civilização.

JORGE, Ricardo (1899), *Demographia e hygiene da cidade do Porto: clima-população-mortalidade*, Porto, Repartição de Saúde e Hygiene da Câmara.

JORGE, Ricardo (1919), L*a grippe: rapport préliminaire présenté à la commission sanitaire des pays alliés dans sa session de mars 1919*, Lisbonne, Imp. Nationale.

JORGE, Ricardo (1920), *Le typhus exanthématique à Porto, 1917-1919: communication faite au Comité international d'hygiéne publique dans sa session d'Octobre 1919*, Lisbonne, Imp. Nationale.

MARTINS, Roberto de Andrade, Lilian Al-Chueyr PEREIRA MARTINS, Renata RIVERA FERREIRA, Maria Cristina FERRAZ DE TOLEDO (1997), *Contágio: história da prevenção das doenças transmissíveis*, São Paulo, Editora Moderna.

MONTALDO, Federico (1900), *La peste bubónica en Oporto (Portugal) 1899-1900: hecho epidemiográficos e investigaciones clínicas recogidos personalmente y anotados por el Doctor F. Montaldo... que asistió en la epidemia, durante tres meses, como Delegado Médico del Gobierno de España: memoria oficial*, Madrid, Establ. Tip. de Portanet.

ROCKWOOD, Charles and O’DONOGHUE, Don (1960), *The Surgical Mask: Its Development, Usage, and Efficiency. A Review of the Literature, and New Experimental Studies*, AMA Archives of Surgery, 80 (6), pp. 963-971.

ROSENBERG, Charles E. (1987), *The Cholera Years: The United States in 1832, 1849 and 1866*, Chicago and London, The University of Chicago Press.

SOBRAL, José Manuel, Luísa LIMA, Paula CASTRO e Paulo SILVEIRA E SOUSA (orgs.) (2009), *A Pandemia Esquecida. Olhares comparados sobre a Pneumónica 1918-1919*, Lisboa, Imprensa de Ciências Sociais.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 847.

ARCHAEOLOGY IN THE PORTUGUESE PRESS: A PRELIMINARY APPROACH

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Abstract

During the second half of the 19th century, Portuguese archaeology was scarcely represented in national daily press, compared to the number of articles dealing with news from other countries, informing about archaeological discoveries in Greece, Egypt, and ancient Mesopotamia, etc., as they involved the Old Testament, and the basis of European culture.
However, during the second decade of the 20th century things seemed quite different. That is why we will analyse Portuguese newspapers in order to understand this phenomenon, and the reasons for the identified differences.

"Scientific progress resides mainly in the evolution of its problems."[2]

Initial context

The climate of political instability experienced in Portugal during the 1820s drove young intellectuals into voluntary exile until the liberal regime was finally affirmed. Their preference was for capital cities offering instruments, equipment and an environment essential to academic growth. Thus they chose London and Paris, where they were imbued with influences they would never forget, and from which they attempted to transpose to Portugal the procedures they believed stimulated the progress of nations. This was particularly important in the country because Portugal had drowned in bloodshed for many

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[2] Popper, K. (2009) *O mito do contexto. Em defesa da ciência e da racionalidade*. Lisboa: Edições 70, p. 250.

years due to a long series of events starting with the invasion by French troops, a situation worsened by the transfer of the Royal Court to Rio de Janeiro. Other serious events followed, among which was the presence of British forces, the loss of Brazilian gold and the start of a long period of fratricidal conflicts that undermined certainties, beliefs and sentiments, thereby injuring and dividing Portuguese society.

Liberalism was eventually established, and consequently those banished intellectuals returned, having lived in the centres of nineteenth-century culture, nurtured by industrialisation. They encountered a country that faced a very deep crisis, but they also found a land full of hope in a world transformed by new ideological standards. Ideas were discussed, experiences were exchanged and projects were nurtured. However, they returned to a nation afflicted by intrusions, internal discord, anxiety and confusion; a land ravaged by constant pillaging, estranged families, anti-clerical uprisings and the abandonment of properties by absolutist opponents. They had to join forces, inspire willingness and merge desires. It would be the only way to reconstruct buildings and open bridges to the future. There was no shortage of synergy, particularly among those who arrived determined to make the country more European without losing the characteristics that made it a unique Western chess piece strengthened by a vast network of roads and means of transport.

The initial enthusiasm and high spirits soon faded. Portuguese financial problems had exhausted all the government programmes. It is true that roads and streets had been built in order to enhance the new sociability. There were also new laws governing education. However, no fundamental comprehensive reform had been implemented. Of course, there was interest in doing so, and in that regard various projects were devised. People with knowledge in different areas organised meetings, and decisions were made. Priorities were established. However, very little was done due to the difficult state of the country, in which foreign debt had to be settled lest its sovereignty be forever lost. Amid this atmosphere, buzzing with innovations but lacking immediate perspectives, the cultural aspects diminished due to shortage of basic human and material resources for implementing drafted and printed programmes indicating their urgency. Among their authors were intellectuals who understood the problems involving science, technology, arts and literature encountered in foreign countries and who hailed the preservation of monuments to be bequeathed to future mainstays of the liberal agenda. Articles had been published in homeland newspapers since the early 1830s, defending the institutionalisation of this area governed for a long time by other European or, more specifically, French terms. In fact, the *Commission des Monuments Historiques* did some creditable and exemplary work, combining additional knowledge such as drawings and photographs, engineering, architecture, art history and archaeology.

Contrary to what the positivist Europe was doing, Portugal had so far ignored the importance of the past as an anchor point of the present and a cornerstone of the future. This was understandable, since it was not essential to strengthen political frontiers and forge a national identity. Unlike most European territories, the country had no need for the justification of its geographical boundaries other than its own historical development free from the struggle for independence. It comprised a territory, a people and a destination that could be expressed in personal images, uniqueness and identifying symbols. This was complemented by the late arrival of the romantic ideas, the first driving force of nationalistic sentiment in a Europe that had been swept up in the Napoleonic code of ambition.

First tests and disillusions

Despite the government's apparent disinterest and inertia, heritage gained followers, not only among those of an academic and literary bent but also among local and regional powers, which saw it as a means of affirmation among the people they represented, thereby extracting dividends (mainly financial) through cultural tourism. Regardless of these more pragmatic aspects, some people reflected on and prepared national action. This effort was supported by the most informed intellectuals and was marked by the strengthening of historical and artistic studies. It was, however, equally attributable to the public recognition (not simply erudite) of a new scientific discipline, which was essential as a means to scrutinise the most remote ancestors of a particular territory or country. (In fact, archaeology had steadily gained ground since the eighteenth century and rapidly gained scientific acceptance in the following century, when it extracted methods from complementary knowledge likely to achieve its final goal following the analysis of materials, including the reconstitution, although partial, of the history of human communities studied.) However, the official recognition of the existence of prehistoric times opened the door to the subject with which we are now familiar, in the year the historiography of this special subject considers its *annus mirabilis* (1859), being precisely the year in which Darwin's key work, *The Origin of Species,* was published. This recognition was surrounded by considerable controversy from the apparent cataclysm caused to Christianity and belief in biblical texts, being directed toward a different path of knowledge of the past based on evolution, including anatomy. Organising debates in the form of academic meetings, articles, papers, national and international trade fairs, the recognition of prehistoric times warranted the adaptation of field work and office methods that clarified it, thus justifying its reception by universities that transformed it into a central subject. It is essential, since it covered the need to affirm ideas, particularly by those who hoped to stand their intentions on past records, using them to indicate hegemonic and border-claim programmes. As part of the new prehistoric studies, archaeology seeped into the daily lives of the more judicious, establishing new national and regional policies.

A group of intellectuals from the city of Setúbal took on an innovative and monumental project in order to demonstrate their understanding of archaeology's relevance in arousing local spirit and encouraging regional self-esteem in the capital city, Lisbon, which would unfortunately continue to ignore skills accumulated over the centuries across the interior of the country it administered. At the end of the 1840s, it founded the *Sociedade Arqueológica Lusitana* (Lusitanian Archaeological Society) (SAL). Its members, amateurs and liberal professionals hoped to examine Roman ruins, but the project only lasted a few years. The scarcity of financial resources, the apathy of the powers in Lisbon and the distance from the capital obstructed its progress. This scenario was apparently contradicted by the impact of the matter in the local and countrywide press. Despite the illiteracy rate, the press managed to give accounts of current events. Excavation logs were published, articles of opinion were printed, and local people were asked to participate. The SAL increased people's curiosity regarding archaeological matters, albeit in a way that was limited to classical themes.

End-of-century expectations

Archaeological articles should be printed in other publications, but the question was which ones. Portugal was seriously lacking in terms of epistemology. There were projects, of course, but they lacked the structure that would allow them to survive individual commitments, such as those of pioneers in prehistoric research. However, due to the absence of a sustained plan and internal structure, by which they failed to occupy university and school auditoriums, their affirmation, was slower and more oblique. It required support from individual and private institutions.

So, where should they be published? Seeing that there was no magazine on this science, the *Real Associação dos Arquitectos Civis e Arqueólogos Portugueses* (RAACAP) (Royal Association of Portuguese Civil Architects and Archaeologists) (1863) preferred it, since it was the most unprotected science in the country. This started in 1871, following participation in the Paris Universal Exhibition (1867), the inauguration of the *Musée des Antiquitées Nationales* and the third session of the *Congrés International d'Anthropologie et d'Archéologie Préhistorique* (CIAAP) (Bologne, 1871). With an archaeological museum (1864), the RAACAP collected materials representing the most remote past of the region known as Portugal and other places. Additionally, it altered its *Bulletin.* Dedicated almost exclusively to the realm of architecture and fine arts, it began to include archaeological studies and became the first national purveyor. This was how the RAACAP made a difference in this small, specific and young scientific community: by leading a process likely to honour it and recognise it as a paradigm of investigation or, at least, scientific dissemination.

Thanks mainly to the RAACAP, the number of archaeological enthusiasts increased –particularly with respect to prehistoric studies– due to the novelties introduced and the perspectives of discovering the ancestral legacy of each territory. Virtual discernments and discerned realities focused the attention of intellectuals from several cities in the country, causing people to gather together in highlighted nuclei in the belief that archaeology would strengthen local and regional sentiments, thus legitimising unity through diversity. Regardless of these restrictions, Lisbon hosted the ninth edition of the CIAAP during an extremely meaningful year (1880) for its nationalist stimuli. The event brought together famous European names in the sciences of anthropology and archaeology. This gave Portugal a rare opportunity to exhibit work performed in these two fields of knowledge. Great efforts were made. The first archaeological chart of the land (the Algarve) was compiled. Excavation projects were sped up, collections were made and a whole social programme was created, which was remembered for many years after the work was officially concluded. The importance of this international meeting was recognised by learned society of the time, and daily records of the sessions, officially opened by royalty, were published in the press.

This increased the hope that institutional support would be received on behalf of archaeology's introduction into the university sphere. It was certain that the presence of so many names recognised abroad would persuade the national authorities in this regard. However, despite the undeniable success of the encounter, the government remained distant. Contrary to expectations, unmatched efforts to publicise the event were basically thwarted, and of course the press alone could not successfully establish archaeological science among the population. This is to say that the country was not structurally prepared to receive it. Logically, the press did not have the power to influence decision-making, and this fact was also evident in weak scientific intellectuality.

Contrary to what was expected by the Portuguese organisers of the ninth CIAAP, archaeology was not officially recognised; instead, it remained outside the spheres of university life and academia. Consequently, it continued to be a subject of study among erudite private associations, which endeavoured to keep pace with collections and museums, some of which were municipal. It was also on a regional basis that enthusiasts of this science found the considerable support they needed; a manner of assistance that was consolidated from large-scale landowners, shop owners and literate industrialists who, concerned with cultural and scientific issues and strengthened by their personal prestige, could acknowledge their role in those areas.

Despite the sense of dissatisfaction, the attention of newspaper readers grew in regard to the new area. This was how the first seeds were sown. However, there was still a long way to go. Nevertheless, unexpected novelties arose in benefit to archaeology. The preservation of archaeological sites was discussed at the ninth CIAAP. The matter was also discussed in countrywide forums, where pages of publications were written without obviously reflecting on specific measures. In response to protests that had been voiced since the 1830s in favour of safeguarding heritage, the government asked the RAACAP to list the ancient structures it considered worthy of classification as "national monuments." This was a step forward. Furthermore, it honoured the RAACAP by recognising its capacity to fulfil this mission. The extensive network of corresponding members established over the years allowed it to provide a quick, assertive response to the government's request. Early in 1881 it presented the report, distributing structures into six general categories, the last of which was archaeology. This was the first time Portugal included archaeology as a topic of heritage. Accordingly, it justified the establishment of the National Monument Commission (NMC), a body that would unfortunately do very little due to insufficient staff and funding.

A novel aspect of the document was the fact that the sixth section almost exclusively comprised prehistoric specimens, mainly representing the megalithic culture. This should not come as a surprise, since it is still a key topic in European archaeological circles, particularly given the political importance accorded by local, regional and national ambitions. Portugal, however, had no autonomist or regionalist problems, nor were there any autonomist aspirations; but researchers could not ignore the most recurrent matters discussed, and thus they established specific work groups. On the contrary, they felt that they should include them in order to avoid isolation and the loss of precious relationships established in unison with other scientific assemblies. By covering them, methods (but not theories) were copied. They emphasized the so-called unique characteristics of artefacts encountered in dolmens, such as engraved schist plates. This is found to have been illustrated in national newspaper banners and headlines. Although it was a topic for newspapers, privately launched specialist publications appearing at the end of the nineteenth century explored it in greater detail.

Concurrently, the Portuguese Ethnographic Museum was founded (1893) through personal effort and political and social capital, the latter of which was frequently evident in general press. This occurred mainly from 1906, the year in which it opened its doors to the public, making it the first institution of its kind to house archaeological and ethnographic materials presumably representing "Portuguese mankind" and associating it with land that had been occupied since the Neolithic era. Thus the end of the nineteenth century opened a new chapter in Portuguese archaeology. The foundation gained form with legal documents from the end of the constitutional monarchy, protecting archaeological sites on public land and making more difficult the sale of artefacts abroad. Meanwhile, the opening of new museums in various inland cities of Portugal was announced, many of which contained archaeological collections. Some of them arose from excavations conducted within their administrative borders, which multiplied local and regional societies whose work was mainly dedicated to such effort. This scenario, however, did not last long enough to make archaeology a solid science. Many protagonists died or passed away. The once rapidly growing impetus, traces, projects and basic connections for their pursuit disappeared and were only recovered years later by local and regional politicians.

Republican achievements

The instauration of the Republican regime (1910) renewed expectations. It was what had been indicated by the academic project that emerged near the end of the preceding century, bearing in mind academic training in the form of printed speeches, debates and intellectual opinions from the first ranks of the new regime. It appeared that the Republic would protect heritage, science and technology as the buttresses of its philosophy. Specific laws were published, covering national museums on an agenda marked by positivism and conviction in regionalist politics. A lot was done, but there was still much more to do. The country was not politically stable. Cities did not have enough specialists in architecture, art history and archaeology to satisfy the programme that, not long before, had been outlined by the Art and Archaeology Councils. Nevertheless, regionalisation achieved the desired goals. Among them was the creation of museums in different places, many containing archaeological collections that encouraged the publication of journalistic articles. However, only some actually were. Generally, it was as if the newspapers –particularly the daily ones– ignored archaeology or put it into second place during the first years of the Republic. This was not the case with the local and regional papers, obviously due to the curiosity and interest they satisfied.

It is important to remember the profound instability experienced between 1910 and 1928, when journalists focused on political, economic and social aspects. Even so, archaeological digs, temporary exhibitions on the subject and speeches by the director of the PEM, José Leite de Vasconcelos (1858-1941) were reported, thereby strengthening his position within the limited framework of specialists in such a specific topic. The truth is that he became an icon of our archaeology and ethnography, and articles were often published concerning the work being performed, mainly when he travelled throughout the country in search of objects for the museum for which he was responsible. This also occurred when he travelled to distant locations in order to participate in international scientific congresses, meet famous representatives of the subjects he had nurtured. Therefore, it is imperative to ask whether the daily press was fascinated by his activity or his personality. What aspects attracted their attention and what attracted readers? Regardless of the answer, a procedure applicable to other people was copied. For the first time, the press was repeatedly following the path of a leading archaeologist. Once in the press, Leite started to exist for society, strengthening and supporting the institution he managed. As a whole, this news benefited science by spreading it, however indirectly it might have done so.

The *Estado Novo* and its ideas

It seems that only with the totalitarian *Estado Novo* regime articles on archaeology started to appear in abundance. Why is this so? Perhaps it is because the subject was institutionalised right after its inclusion in university programmes, at least in Lisbon. Moreover, the transition from the nineteenth century to the twentieth century witnessed the launches of magazines often containing articles on archaeology, some of which were accompanied by pictures and photographs. By elucidating underlying ideas, a significant part of these publications were called *Portuguese Land*, *Lusitanian Journal* and *Lusitanians;* titles that strongly linked Portugal to ancient Lusitania, whose warriors fought bravely against legions from Ancient Rome. It was an understandable connection, in light of exogenous ambitions regarding Portuguese overseas territories. It was time to recover national pride and strengthen identity. Therefore, the "New State" included archaeology on its agenda, just as it did with respect to most scientific fields. Even though the country did not need it to legitimise frontiers, increasingly articles on archaeology appeared in the newspapers. This was probably due to the growing numbers of wealthy individuals, having participated in Portuguese university training, focused on it. It was also due to the proliferation of erudite societies supported by the Portuguese Archaeology Association.

This warranted the need to organise and supervise archaeology. The second director of the PEM, Manual Heleno (1898-1970), was essential for internal archaeology, being on a par with powerful names in Oporto, Coimbra and Lisbon. Contrary to the nineteenth-century historiographical tendency to settle in the Portugal of the Middle Ages, M. Heleno based it on the Neolithic era, or more specifically the megalithic period. Thus he dedicated most of his life to demonstrating it. This was such an important issue for the New State programme that it became a journalistic *pièce de resistance*, as his digs, their results and interviews with the person responsible for them often appeared in the papers. It seemed that archaeology had finally found its path among the populace, or perhaps they discovered archaeology. To culminate the long process of the institutionalisation of this science in the country, there appeared many more studies of its main followers, printed in Portugal's leading newspapers and magazines. It was as if archaeology had been accepted by society as a whole, especially the literate, occupying universities and schools and contributing to local finance through tourism. This acknowledgment was well established in the First National Archaeology Congress (1958), with the presence of foreign leaders.

Reflections and suggestions for study

The daily press started to become more interested in archaeology when it became politically and economically important as a pillar of so-called cultural tourism. This is only one side of the issue, however. It is important to scrutinise newspapers and magazines, and to analyse the quantitative and qualitative differences of the content published in the national, regional and local press. Topics should be identified and the importance of archaeology understood for the sake of clarifying identities and the economic progress of each territory, doing so through geographical comparison. Moreover, it is important to compare the number of articles on archaeology with those pertaining to other sciences, so as to obtain a closer view of its true political and social role. This assessment should contemplate the types of articles published, but more specifically it should consider whether they were scientifically credible or mere descriptions of archaeological episodes. Various other aspects should also be considered, such as the question of which pages of the newspapers and magazines the articles were published. Was it the first page? If so, which types of articles were printed? Was there a difference between the quantity, quality and location of articles in the national, regional and local press outlets? Who were their authors? Were they journalists with scientific knowledge? Was their content reviewed by specialists? Were archaeologists questioned by journalists in regard to specific information? Did periodicals manage to certify any archaeological praxis?

Clearly, there are more questions than answers, at least for now.

References

AYARZAGÜENA SANZ, M. (2004a): "El nacimiento de la Arqueología científica en España". *Pioneros de la Arqueología en España. Del Siglo XVI a 1912*, M. Ayarzagüena Sanz and G. Mora (eds.): 75-77.

COITO, L. C.; CARDOSO, J. L.; MARTINS, A. C. (2008): *José Leite de Vasconcelos. Fotobiografia*. Lisboa: Museu Nacional de Arqueologia/Editorial Verbo.

GRAN-AYMERICH, È. (1998): *Naissance de l'Archéologie Moderne*. 1798-1945. Paris.

MARTINS, A. C. (2003): *Possidónio da Silva (1806-1896) e o Elogio da Memória. Um percurso na arqueologia de oitocentos*. Lisboa, AAP.

MARTINS, A. C. (2004): "Património Histórico-cultural: a emergência das (re)formas (do Liberalismo ao Republicanismo). 2ª parte. *Património. Estudos. 6*. Lisboa: IPPAR: 109-125.

MARTINS, A. C. (2005a):*A Associação dos Arqueólogos Portugueses na senda da salvaguarda patrimonial. Cem anos de (trans)formação. 1863-1963*. Lisboa.

MARTINS, A. C. (2005b): "As ruínas de Tróia (Portugal) e o despertar da Arqueologia clássica no Portugal de oitocentos. In CABRERA VALDÉS, V.; AYARZAGÜENA, M., eds. lits. - *El nacimiento de la prehistoria y de la arqueología científica*. Madrid: Sociedad Española de Historia de la Arqueología, (*Archaia; 3-5*): 65-85.

MARTINS, A. C. (2008a): "«A Oeste nada de novo»?". *Estudos Arqueológicos de Oeiras 15*: 233-291.

MARTINS, A. C. (2008b): "Nineteenth-Century Protohistory at the Portuguese Association of Archaeologists: Fancisco Martins Sarmento (1833-1899) and national identity". In N. Schlanger, and J. Nordbladh, eds., *Archives, Ancestors, Practices. Archaeology in the Light of its History*,: 287-303. Oxford.

MARTINS, A. C. (2009a): Archaeology and Political Agendas: the Making of Nationalism in Portugal (19^{th}-20^{th} Centuries). *Abstract book International Congress Ideas of / for Europe*. Chemnitz.

MARTINS, A. C. (2009b): O Megalitismo no discurso arqueológico português. Entre o Liberalismo e o Estado Novo: uma primeira e sumária mirada. *Estudos Arqueológicos de Oeiras* 17: 607-16.

POPPER, K. (2009): *O mito do contexto. Em defesa da ciência e da racionalidade*. Lisboa. Edições 70.

TRIGGER, B. G. (1992): *Historia del pensamiento arqueológico*. Barcelona.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 853.

COMPARING THE PUBLIC PERCEPTIONS OF SCIENCE AND TECHNOLOGY IN THE GREEK AND THE PORTUGUESE DAILY PRESS: THE CASE OF THE RETURN OF HALLEY'S COMET (1910)

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Abstract

In this paper we present the results of a comparative study of the return of Halley's Comet in 1910, as presented in the Greek and the Portuguese generalist press. Our work is based on a common bottom-up approach offering a complete survey of all relevant news published in two different newspapers in each country: the Greek newspapers Embros *(Athens, 1896) and* Skrip *(Athens, 1893), selected for their wide circulation because at the period under examination newspapers had no clear political orientation, and the Portuguese newspapers* Diário de Notícias *(Lisbon, 1865) and* Diário dos Açores *(Azores, 1870), chosen on the basis of their broad ideological scope and different geographical locations. We offer some conclusions, which explore the contrasts and similarities of how the return of Halley's Comet in 1910 was reported in both countries.*

Introduction. Comparing public perceptions of science in the Greek and Portuguese press

Despite the renewed interest in the role of mass media in communicating science and technology to non-specialized audiences, historians of science have paid scant attention to the daily press as an object of research *per se*. Although the fields of media studies and cultural history have long recognized that newspapers are crucial in the making of public opinion, in the history of science we are just beginning to establish the nature of newspaper coverage and to account for its importance.

Recent studies have shown that the daily press can be seen as a particularly suggestive source for the public images of science and technology and the public perception of their role in society. Moreover, the extent to which science and technology are topics that permeate the entire newspaper and are not limited to specific columns dealing with "science popularization", is indicative of the important role science and technology played in the formation of modern societies. To what extent can newspapers be seen as privileged media for the examination of the cultural meanings of science and technology in different local settings? How do the main political, social and cultural features of a period influence the public discourse about science and technology, and how in turn do discussions about science and technology in the public sphere transform the ideological, social and cultural formations of each historical period?

In this paper we present the results of a comparative study of the return of Halley's Comet in 1910, as presented in the Greek and the Portuguese generalist press. Our work is based on a common bottom-up approach offering a complete survey of all news published in two different newspapers in each country: the Greek newspapers *Embros* (Athens, 1896) and *Skrip* (Athens, 1893), selected for their wide circulation since at the period under examination newspapers had no clear political orientation, and the Portuguese newspapers *Diário de Notícias* (Lisbon, 1865) and *Diário dos Açores* (Azores, 1870), chosen on the basis of their broad ideological scope and different geographical locations. While the one was published in the capital city, had a very wide circulation and no clear political orientation, the other was issued in one of the Azorean islands located in the Atlantic and was associated with the monarchic intelligentsia linked to the Partido Progressista.

Besides methodological commonalities, that is, besides having based our research on the complete survey of all news appearing in successive days in all newspapers, we initially decided to focus on the first decade of the 20th century because both countries were undergoing turbulent political times, and similar journalistic trends.

In Greece, the first decade of the 20th century is indicative of a broader historical period, characterised by a programme of modernisation of the country as well as an expansive national policy. The latter was, nevertheless, severely compromised after the defeat of Greece in the Greek-Ottoman war of 1897, the imposition of the International Financial Control in 1898, and the intensification of Balkan nationalisms. The political instability of the period culminated with a military revolt in 1909, which was also supported by large parts of the population and trade unions, all asking for the military, social and political reform of the country. The military revolt brought, in 1910, into power the Cretan politician Eleftherios Venizelos, who presided over a vigorous program of constitutional, military and social reform and represented the hegemonic national ideology until the defeat of Greece in the Greek-Turkish war in 1922.

Portugal, on the other hand, lived the difficult aftermath of the British Ultimatum and the Berlin Conference (1895) at a national and international level. The international credibility of Portugal was severely damaged to such an extent that colonial matters played a part in the overthrow of the monarchical by a republican regime in 1910, precisely the year of Halley's passage. Therefore, the implantation of the Republic on October 5 brought hope in a new society built on democratic, socialist-oriented, anti-clerical, nationalistic and colonialist values. While some of the former characteristics are easily understood as a reaction to monarchical values, nationalism and colonialism were a response to the outcome of the Berlin Conference and the Portuguese reactions to secure its African colonies. Ideologically, they had the support of many intellectuals who praised the gospel of positivism and endorsed scientism, mostly as an initial reaction to a society formerly dominated by religion, and subsequently as a more or less articulated philosophical project.

Regarding journalistic trends, both countries were undergoing similar processes, not only in terms of the printing technologies used, which accounted for the broader and cheaper dissemination of newspapers, but also in relation to new journalistic genres, replacing opinion articles for more "neutral" news, which left to the readers the task of forming their own opinion. Finally, both countries had a high illiteracy rate, around 50% for Greece and 80% for Portugal. However, this very characteristic makes newspapers, through multiple and oral readings at various public places, a privileged source to survey the public opinion on science and technology matters.

As in both Greece and Portugal "locality" often guided the selection of news and, furthermore, shaped their presentation, we selected the return of Halley's Comet on May 1910 as a particularly suitable case study on which to base this comparative exercise. On May 18, 1910 Halley's Comet was due to make its periodic passage near the Earth. Spectroscopic analysis had shown that the tail of the comet consisted of poisonous gases and extensive news coverage in the daily press spread panic around the world, reporting that the Earth would travel through its tail. The number of news on the comet was roughly the same in both the Greek and Portuguese newspapers surveyed (around 90) and in both cases most of them appeared on the month of May.

The return of Halley's Comet as seen through the Greek press

A year before the return of Halley's Comet, the journalists of the Athenian newspaper *Embros* had predicted that its passage would turn into a major journalistic event. Indeed, *Skrip* and *Embros* covered the event in a similar way with more than 40 articles each. Brief articles in the middle pages announced the latest news concerning the orbit of the comet from various observatories around the world. Longer ones traced the previous appearances of the comet and linked them to great, mostly destructive, historical events. Almost the majority of the translated articles came from French newspapers, and many of them were extracts of texts written by the acclaimed populariser and amateur astronomer Camille Flammarion. These articles, sometimes accompanied with illustrations, aimed at giving the public a general idea about the nature of comets and their orbits and informed about possible threats to the world. On various occasions both newspapers published interviews of Dimitrios Aiginitis, Professor of Astronomy and Meteorology at the University of Athens and Director of the National Observatory, and other members of the Greek scientific community. They also reported in detail public events, such as public lectures on the comet or even theatrical plays inspired by the comet.

Halley's Comet was an exceptional astronomical event with multiple repercussions in people's daily life. Newspapers seem to have been the most important medium through which information concerning the comet was made known to a wide public. Through newspapers' diverse types of articles about the comet, the Greek public was familiarised with astronomical terms and notions, instruments such as telescopes and spectroscopes, as well as sites of astronomical research. Popular science articles on Halley's Comet were avidly read, and then discussed in coffee shops, taverns or other public spaces. As the chronicle-writer of *Embros* observed, the comet left behind a great number of "astronomers" who continued to discuss their theories over a cup of coffee. People gained also first-hand experience with at least one aspect of astronomical practice –observation– during the nights when they stayed awake in order to observe the strange visitor of the sky over their heads.

Despite astronomers' accurate prediction of the return of Halley's Comet on May 18, contradicting information concerning possible threats to the planet put at stake and reinforced, at the same time, the trust in experts. Journalists used scientific loanwords in order to enhance the authority of their writings, but also to render their articles spectacular and attractive. Although most astronomers were reassuring concerning the possible threats posed, the reporting of the poisonous constitution of the comet's tail as well as its unconceivable length (ranging between 23 and 25 million kilometres) were sensational enough to create an ambiance of anxiety. Most chronicle-writers found the effect of the news on the public rather amusing. They took the opportunity to walk among the excited Athenians, who camped on the hills of the capital so as to observe the comet, and reported on a daily basis the most entertaining dialogues. Although critical of "end of the world" scenarios, journalists insisted on writing about them in order to provoke sensation among their readers. This choice was dictated by, and at the same time fed, the main topic of discussions taking place in public spaces.

May 19, when everybody woke up safe and sound, marked the beginning of a new series of articles that criticised not only the credulity of the lay public, but also the unreliability of the astronomers. Journalists blamed foreign astronomers for the dissemination of false information that created panic and in some cases led people abroad to take their own lives. They also implied that most Athenians experienced the passage of the comet in a climate of euphoria, because of their own calm stance when reporting the news. Although journalists were harsh in their criticism of foreign astronomers, when it came to the local community, they showed respect and reproduced uncritically the reports of the National Observatory. At the same time, both newspapers served as a forum for local scientists, who portrayed their science as a "positive" one, self-fashioned themselves as experts and enhanced their role in Greek society. Through these interventions, the newspapers gained also the credit of being a legitimate vehicle of scientific knowledge.

The return of Halley's Comet as seen through the Portuguese press

The two Portuguese newspapers selected covered extensively the passage of Halley's Comet, but there are interesting differences between the two. The amount of news by the capital newspaper (67) is roughly the triple of the news by the islander newspaper (25). In both newspapers news on the comet was given a prominent place and were evenly distributed throughout the newspaper (first five to seven pages out of ten, in the capital city newspaper and first two pages out of four, in the islander newspaper). News on the comet were also unevenly distributed around the year, with a peak in May, when the comet was supposed to be closest to Earth (18-21 May), and disappearing abruptly after its passage. Furthermore, while in the Portuguese Azorean newspaper news were not located in special science sections and were never illustrated, probably due to printing limitations, in the capital city newspaper they were at times located in a science section called "Scientific Chronicles," or in special sections not necessarily related to science such as "Chronicles from Porto," and were often accompanied by figures.

In both newspapers, a substantial fraction of news centred on giving readers practical information on the comet, including the characteristics of its trajectory, its constitution (nucleus, wig and tail), its effects on Earth (mechanical, calorific and chemical), and its history, as well as visibility conditions or varied information stemming from different observatories across Europe. Like in Greek newspapers, they were often based on the expert opinion of foreign popularisers of astronomy such as Flammarion or his friend in Portugal, Théophile Moreux. Contrary to Greek journalists, Portuguese journalists did not place so much emphasis on the possible threats of Halley's passage on humanity. References to the possible dangerous, or even deadly, effects of cyanogen and other gases were counteracted by articles by some Portuguese scientists who explained that spectroscopy enabled to identify the composition of the comet's tail, but could not calculate its density, so that there was no evidence that gases existed in such quantity as to become a dangerous threat to life on Earth.

The importance conveyed by national/local astronomers or scientists is also a crucial characteristic of both Portuguese newspapers: the islander newspaper gave prominence to local/Azorean scientists, and the capital newspaper assumed strongly its role as a national newspaper and included more encompassing news. In fact, the Azorean newspaper reproduced, in successive articles, a conference about the comet, which was delivered by the well-known amateur scientist Colonel Afonso Chaves. More than its timeliness, the comet's return was addressed due to the association of astronomical events to weather and earthquake prediction, a problem of greater relevance to the islands, and especially to their scientific elite. Chaves used the newspaper as a communication channel to win the local elites for the agenda of the scientific community, whose aim was to render Azores an active node in an international network of meteorological and seismological observatories. The newspaper also drew attention to another Azorean astronomer, lieutenant Melo e Simas, who delivered a speech on the comet in the context of the newly founded Academy of Sciences of Portugal.

Contrary to the Azorean newspaper, which was more focused on particular kinds of news and topics, in Lisbon, newspapers covered different typologies of news: astronomical information on the comet both from foreign or local scientists, science popularization articles, description of various reactions of the population in Portugal and abroad, news issued by a recent scientific society which emerged in association with the blossoming republican movement, questions asked by readers or observations sent by them, announcements of different social events related to the comet, advertisements on national or foreign books about the comet, or advertisements on a special anti-comet powder supposedly validated by the scientific community; and, finally, literary and satirical texts sent by locals to the newspaper using the comet as pretext to enrol in various social and political criticisms.

Portuguese scientists appropriated the comet's passage to enrol in popularization of science. The importance of popularization as a means to educate the working class and the population at large reflected the endorsement by committed intellectuals' of positivism and scientism as antidotes to a society so far dominated by religion and the Catholic Church. Initially a reaction to a retrograde society, this trend gave way to a more or less articulated philosophical project, which materialized, for instance, in the creation of the Academy of Sciences of Portugal, in 1907. Spokespersons of the Academy used science to fight against uneducated people's fears and superstitious beliefs.

If many scientists used the capital newspaper as an outlet for their popularization of science activities, some astronomers used it also as a means to push forward their scientific agendas, and specifically to gain advocates for astrophysics. Costa Lobo, for instance, made a plea for the importance of this emerging sub-discipline, explained its association with new techniques such as spectroscopy and photography and argued for the need to buy appropriate instruments. Besides talks by well-known scientists, the reader was also informed about talks taking place in workers' or socialist associations by less well-known speakers (Inocêncio Camacho), clearly partaking of the spirit of the time, that is, the movements of "education for all".

One of the most revealing traits of the Portuguese newspapers is the prominent role played by the local population in informing journalists of the activities taking place in their localities concerning the comet, and the reactions of the population that ranged from fear, terror and suicidal attempts to celebrations and the organization of various social events. Often readers offered satirical and literary descriptions of real or imagined comet-related situations, such as a politically and socially oriented critical text of a telegram supposedly sent by the Comet itself while passing above Portugal. The active role of the population was such that readers made suggestions or even criticized journalists for publishing sensationalist news in order to increase their audience instead of enlightening the population by publishing digests of scientific information. The contrasting role of journalists and journalism as seen by readers was also paralleled by the dual role played by priests and the Church. Priests were portrayed in two opposite ways: as those who usually reinforced public fear about the "end of the world" as signalled by the comet's passage, or, exceptionally, as people who could use their sermons in a constructive way by enlightening their audience as to the effects of the comet.

Despite the wealth and variety of news issued by Lisbon newspaper, one should realize that this did not subscribe to any specific scientific agenda of popularization as the Azorean newspaper did. This is clear when one contrasts percentages of news on the comet relative to all news on science, technology and medicine in both newspapers (27.6% for *Diário de Notícias* compared to 7% for the Azorean newspaper), meaning that *Diário de Notícias* simply registered scientific events as they emerged, depicted reactions to them, and gave a place to national scientists who used it as an outlet for their popularization agendas. On the contrary, in the Azorean newspaper the agendas of the local scientific elite were, almost permanently, at the core of the newspaper's contents.

Comparative approach at stake: What did we gain? What do historians gain?

The importance of "locality" in shaping scientific news is evident from the dissimilarities found between Greek and Portuguese newspapers, as well as between Portuguese newspapers. Certainly the choice of two Greek newspapers from Athens, and two Portuguese newspapers from two different cities, one in Portugal mainland and the other in one of its islands, is at the core of the asymmetric results obtained when comparing Greek and Portuguese newspapers. Furthermore, in contrast to the Lisbon newspaper, in the Azorean paper, news on science appeared regularly in accordance with the interests of local intellectuals and scientists who did not need the occurrence of spectacular natural events to have their voices heard.

Similarities among all newspapers are probably tied to international trends. In fact, newspapers in both Greece and Portugal extracted news from the foreign press (especially French) and published opinions of popularisers such as Flammarion, whose presences was regular whenever astronomical phenomena were involved. However, news from abroad was appropriated in contrasting ways. The belief that the poisonous composition of the comet's tail was a real threat to life on Earth was more intense in Greek newspapers than in the Portuguese. The respective assessment of some foreign astronomers' claims from both Greek and Portuguese journalists differed. Greeks but not Portuguese journalists blamed the foreign astronomers for the panic created, while both Greek and Portuguese considered local astronomers a very reliable source.

In both countries, despite journalists' references to possible dangers from the comet's passage, the local scientific community downplayed such a danger and provided reassuring information. All newspapers gave astronomers' ideas a prominent place. However, the impact of the scientific community seems to have been much stronger in the Portuguese than in the Greek case. This difference has probably to do with the strong agendas of Portuguese scientists, who took the opportunity to use the daily press as means of their engagement in the popularization of science and their fight against obscurantism. It is, therefore, clear that the strong popularization of science agenda which characterized the new Republic had its roots at least in the final years of the Monarchy. In the case of Greece, the local scientific community engaged in science popularisation from the 1870s, mostly through the publication of articles in general periodicals and through public lectures. At the turn of the century, they started contributing to more specialised journals and appeared on the daily press mainly when they were interviewed by journalists. With the exception of engineers, who were often involved in debates concerning public works, we rarely find cases of local scientists contributing systematically to the daily Greek press.

The participation of the public, specifically with letters to the Lisbon newspaper, has no parallel in Greece. In the Athenian newspapers, journalists were the ones who reported the reaction of the public to the news, and transcribed dialogues between themselves and the public so as to create a sense of immediacy. However, it is impossible to know whether such descriptions were invented so as to serve the journalists' purposes, or whether they actually took place. This difference is quite unexpected having in mind the greater illiteracy of the Portuguese population. But on a second reading, one should recall that despite oral and multiple readings in various public places, these contributing readers were the local elite of each city, town or village. A newspaper such as *Diário de Notícias* counted on the reports of informants from various localities all over the country, who were often their primary school teachers, lawyers or solicitors, doctors or priests.

Coming back to the questions posed at the beginning of this paper, we conclude that despite and at the same time because of differences in contents and approaches, newspapers in Greece and Portugal are privileged sources for the examination of the cultural meanings of science in different local settings. The main political, social and cultural features of the first decade of the 20th century in Greece and the republican movement in Portugal, with its emphasis on positivism and scientism shaped the public discourse about science in characteristic ways. How, in turn, discussions about science in the public sphere, and specifically in the daily press, transformed the ideological, social and cultural formations of each historical period is harder to ascertain and are still to be clarified.

References

Embros, *Skrip* (1909, 1910).

Diário de Notícias, *Diário dos Açores* (1909, 1910).

MERGOUPI-SAVAIDOU, Eirini; PAPANELOPOULOU, Faidra; TZOKAS, Spyros (2009). 'The public image(s) of science and technology in the Greek daily press, 1908-1910', *Centaurus*, 51/2: 116-42.

MERGOUPI-SAVAIDOU, Eirini, PAPANELOPOULOU, Faidra; TZOKAS, Spyros (2010) 'Science and Technology in the Greek Newspapers, 1900-1910. Historiographical Reflections and the Role of Journalists for the Public Images of Science and Technology', *Science and Education* (First Online, 7 September, DOI 10.1007/s11191-010-9292-5).

PAPANELOPOULOU, Faidra & KJAEGAARD, Peter (2009). 'Science in the Press: The public images of science and technology in fin-de-siècle Denmark, Greece and Spain', *Centaurus*, 51/2: 89-96.

SCHIRRMACHER, Arne (2011) 'Popular Science Between News and Education. A European Perspective', *Science and Education* (First Online, 28 February, DOI 10.1007/s11191-011-9347-2).

SIMÕES, Ana; CARNEIRO, Ana; DIOGO, Maria Paula (2010) 'Riding the wave to reach the masses: natural events in early twentieth century Portuguese daily press', *Science and Education* (First Online, 12 September, DOI 10.1007/s 11191-010-9299-J).

COLD WAR SCIENCE AND TECHNOLOGY IN THE ARCTIC

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 859.

SECRETIVE GEOLOGIES: DANISH AND AMERICAN AGENDAS FOR THE GEOLOGICAL INVESTIGATION OF GREENLAND, 1946-1960

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Abstract

Between 1946 and 1958 a peculiar division of labour existed in the geological investigation of Greenland. During this period, the ice-free lands of Greenland was generally explored in four largely independent institutional settings –one of them American and three of them Danish– each investigating their own region of the island according to their own agenda, and each having their own reasons for maintaining varying degrees of isolation from the others. Preliminary in nature, this paper attempts to outline some Danish and American institutional settings for geological investigations in Greenland, as well as some of the scientific and political agendas that they followed.

A geology of Greenland in four parts (+ one)

By the end of WW II the Arctic had achieved superior strategic importance to the nations on the Northern Hemisphere. Consequently, the Danish colony of Greenland became the scene of extensive geological investigations conducted under the auspices of the US military, as geologists of the Military Geology Branch of the US Geological Survey investigated large portions of the ice-free areas of northern Greenland to identify possible sites for air bases, natural landing strips and other military activities (Davies 1972; DeGoes and Neal 1998; Krinsley 1994, 1998; Weidick 1999).

In contrast, Danish field activities were almost entirely concentrated south of 78°N. On the east coast, the Danish geologist Lauge Koch, resumed the large-scale investigations conducted by him in the 1930s, directed on behalf of the Danish Greenland Administration with almost no participation from Danish geologists. Paid by fees from the Greenland Administration negotiated year by year supplemented with contributions from private investors, Koch relied heavily on his ability to muster sufficient popular and political interest in his activities to secure additional state support and private funds for his activities. Thus, economic geology and practical mineralogical investigations became important elements in Koch's investigations (Ries 2003, 2007).

The Danish Greenland Geological Survey (GGU), established in 1947 for that exact purpose, investigated the west coast. As a regular state institution financed routinely via the state budget closely aligned with the Dept. of Geology at Copenhagen University, GGU focused on general geological surveying and mapping, while serving as a training ground for a new generation of Danish geologists emanating from Copenhagen University in the decades following WW II (Ellitsgaard-Rasmussen 1996).

By comparison, Danish involvement in the geological investigation of Northern Greenland was limited. From 1954 onwards the Danish Liaison Officer at Thule Airbase was assisted by a Danish Scientific Advisor recruited on a yearly basis –often a young geologist– to assist in the supervision of US scientific activities in the region. However, given the rate of exchange of persons in this position, and the intensity, extent and variety of US activities within a range of scientific disciplines, this task did not leave much time for scientific investigations.

The most constant Danish individual presence in Northern Greenland in this period was the archaeologist Count Eigil Knuth, who stayed and conducted a series of smaller Danish expeditions in and around Peary Land between 1947 and 1996. In the period dealt with in this paper, three geology students of Copenhagen University each carried out one season of geological investigations on Knuth's Danish Peary Land Expedition by 1947-50. In 1952 and 1953 small parties –two or three men– of Swiss and English geologists from Koch's East Greenland expeditions carried out geological work with summer parties in Peary Land. But apart from these minor exceptions, Danish activities were concentrated on the east and west coasts of Greenland, leaving the geological exploration of North Greenland to the Americans.

Greenland Geology between Danish Sovereignty and US Military Interests

Until the early 1960s, when missiles took over the role as the primary strategic deterrent for Soviet aggression, the ability to apply nuclear weapons between continents was determined by the availability flight routes between the Western Hemisphere and the Soviet Union. In 1946, a report from the US Joint War Committee gave the following assessment of the North Greenland coast as a possible steppingstone for transatlantic aircraft activity:

> *"Within the next ten years the strategic importance will increase progressively. When certain technical difficulties of operation from Arctic bases can be overcome and the problem of logistic support be solved, the northern part of Greenland could provide a base area for the projection of strategic air operations. However, a more positive determination of the extent to which this area could be utilized will be obtained when the data collected by recent expeditions has been analyzed."* (DUPI 1997)

During WW II, the USA had operated a small weather station at Thule, Greenland's northwest coast. In 1946, the army built a landing strip at Thule as an operational base for photographic mapping missions, and from 1946 onwards the US Air Force carried out systematic photogrammetric mapping over all of North Greenland to investigate regions and locations fit for additional weather stations, military bases, auxiliary airstrips and trafficability (Laursen, 1972, p. 150-151).

As US air operations increased in North Greenland along with demands for more complete weather information, it became apparent that there was an urgent need for a weather station and an emergency airstrip on the northeast Greenland coast. In 1948 and 1949 the US Geological Survey in cooperation with the Arctic Section of the US Weather Bureau, began air surveys of the north-easternmost part of Greenland in search of a site for a weather station in the region. When, at a meeting in Copenhagen in June 1950, the USA formally suggested the establishment of a joint Danish-American weather station, it caused some concern among Danish government officials. The then Danish Head of the Greenland Department of the Danish Ministry of State, Eske Brun, in 1957 summarized the negotiations and their outcome to Danish Minister of Greenland Johannes Kjærbøll: *"Station Nord was originally established because the Americans wanted a weather-station in that particular region, and we wanted to keep them out of there. We have succeeded in keeping the station entirely on Danish hands …"* (DUPI 1997, 200).

On April 27th 1951, an agreement was signed between the USA and Denmark concerning the common defence of Greenland, allowing the USA to conduct military and scientific activities inside their base areas without Danish interference, while demanding all planned activities outside the bases to be presented to and approved by the Danish state. The agreement also gave the USA the use of a greatly expanded area around Thule, where major construction works from 1951 through 1953 resulted in a 3 km long asphalt-surfaced runway with operational apron, hangars, taxiways and considerable supporting base facilities.

Preparations for the establishment of Station Nord commenced in the autumn of 1951 when, even before the exact location was decided, the Americans promised to provide technical assistance and transport of necessary personnel and material from Thule. The Station Nord site was selected in April 1952, construction began in the summer of 1953, and the 2.5 km gravel runway at Nord became operational in 1954. The station however formally remained under Danish authority, and US requests for Danish permission to establish an 'outlying base' under US control on the East Coast opposite the Thule Base recurred several times. The permission was never given, and in 1953 the USA formally relinquished all plans to establish their own base in North East Greenland. Station Nord remained under Danish control, allowing the US Air Force to use the airfield for emergency landings only. Such landings appear to have occurred only three times: in January 1957, October 1957 and February 1958 (DUPI 1997).

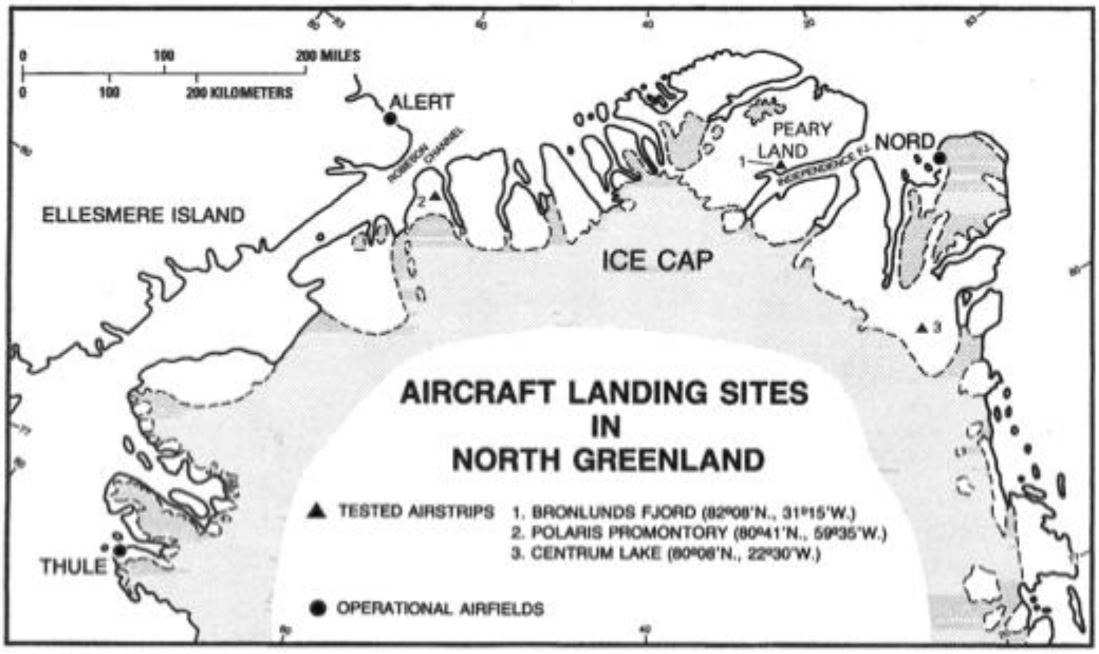

Fig. 1: Aircraft landing sites in North Greenland (Krinsley 1998).

US military geology and terrain intelligence in Northern Greenland

Although attempts to establish a second US base in North East Greenland had been formally renounced, American military activities in the region persisted. However, transformation of Greenland from Arctic wasteland into a bridgehead in American Cold War military strategy required the development of new forms of extensive and detailed scientific investigation of the terrain and environmental characteristics of the region.

It was exactly this understanding that informed the views of the Office of Naval Research under the Arctic Research Advisory Committee of the Arctic Institute of North America, as it charted out the military strategic potential of an expansive US engagement in Greenland at a meeting in the Commission on Merchant Marine and Fisheries of the House of Representatives in January 1957:

> *"Science will permit our use of Greenland as an Arctic sword and shield –a mighty bastion of deterrent power essential to the NATO concept* […] *An absolute prerequisite to our effective use of the high Arctic is harnessing of its environment. The Arctic's true military potential can only be transformed to the dynamic by means of studies specifically oriented to the problem.* […] *However, more knowledge is required to permit military man to work with the cold rather than against it, and to do so in a practical and economic manner. The Arctic is friendly only to those who comply with its implacable laws."* (Arctic Institute of North America 1958)

In general terms, the development of nuclear weapons for tactical use after WW II was followed by a concept of warfare based on small, mobile combat units equipped with helicopters, aircraft with short take-off and landing capability, and low ground-pressure vehicles, which could move across country to supply rapidly shifting forces. This in turn affected the kinds of geographical knowledge required for the strategic planning of military operations.

Analysis of landforms as they offered shelter from nuclear blast, safe routes for helicopters, tanks and supply vehicles, sites for rapid airfield construction and paratrooper landings and the suitability of soil for hasty excavation of shelters became of prime importance for the conduction of military operations (Whitmore 1960).

Guided by the military need for terrain analysis for strategic and engineering purposes then, the US scientific agenda for geological investigations of North Greenland focused on studies of short-term dynamic processes of geological disintegration, erosion, transportation and deposition of rock material with a marked interest in surface geology. Shore processes were studied in order to appreciate the effect of frost and thaw processes and sea ice shove, and the stability or lack of stability of beach shapes, to assess the possibility of the construction of docks. Likewise, the formation and preservation of permafrost and its effects on various types of terrain were studied relative to the establishment of foundations, water supply, waste disposal and transportation.

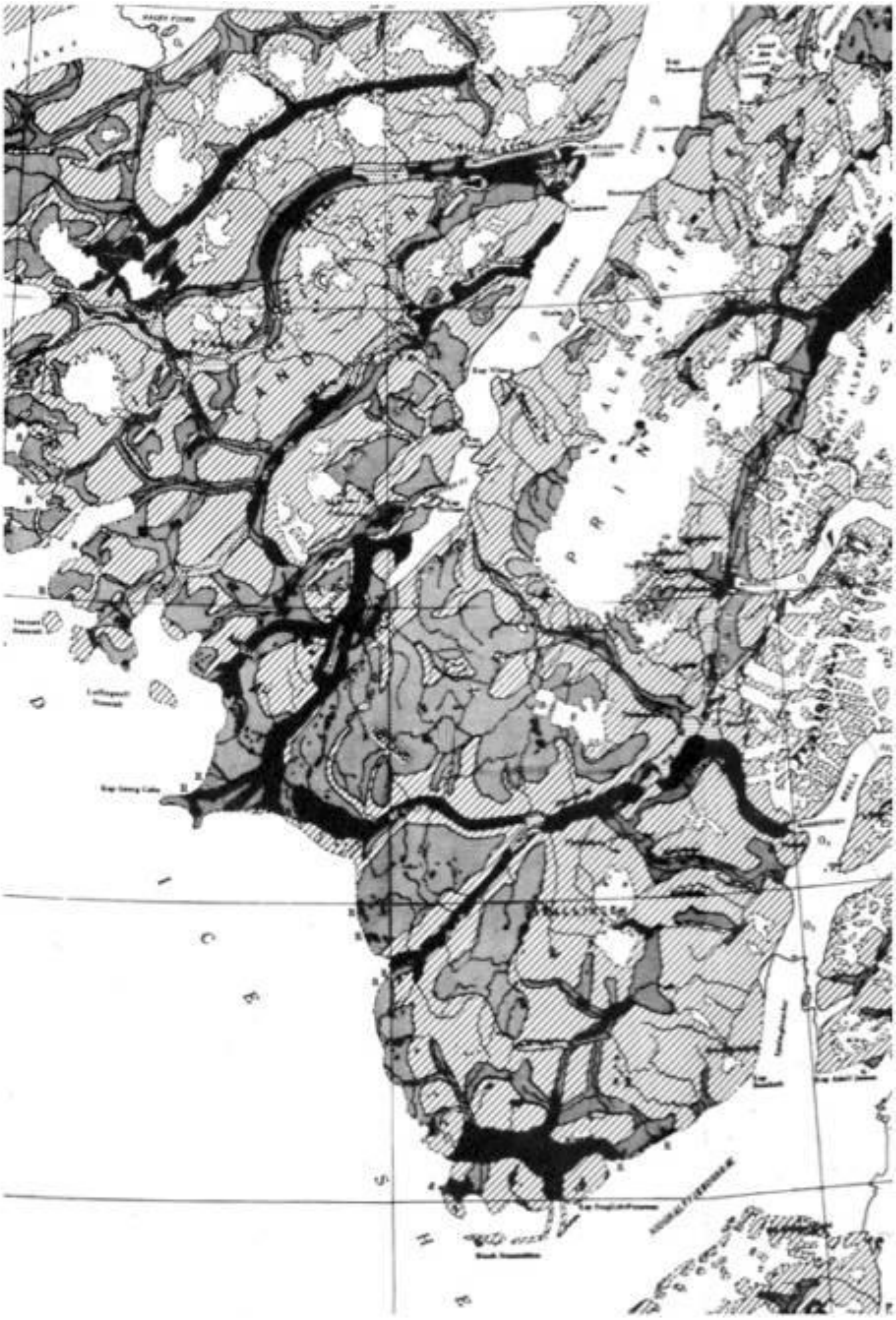

Fig. 2: Terrain Map of North Greenland with reference to the operation of air-cushion vehicles (excerpt) (Davies 1972).

These were the concerns that shaped the scientific tasks of the personnel of the Military Geology Branch of the US Geological Survey, which partook in a series of investigations during the 1950s and early 1960s under contract with the US Air Force and US Army, in order to find locations fit for establishing auxiliary landing sites and other logistic centres along the North Greenland coast between Thule and Station Nord. Candidate sites were selected by analysis of aerial photography, with subsequent detailed on-site studies. Where possible, those sites with acceptable parameters were subjected to test landings by heavy C124 and C130 transport aircrafts (Krinsley 1998).

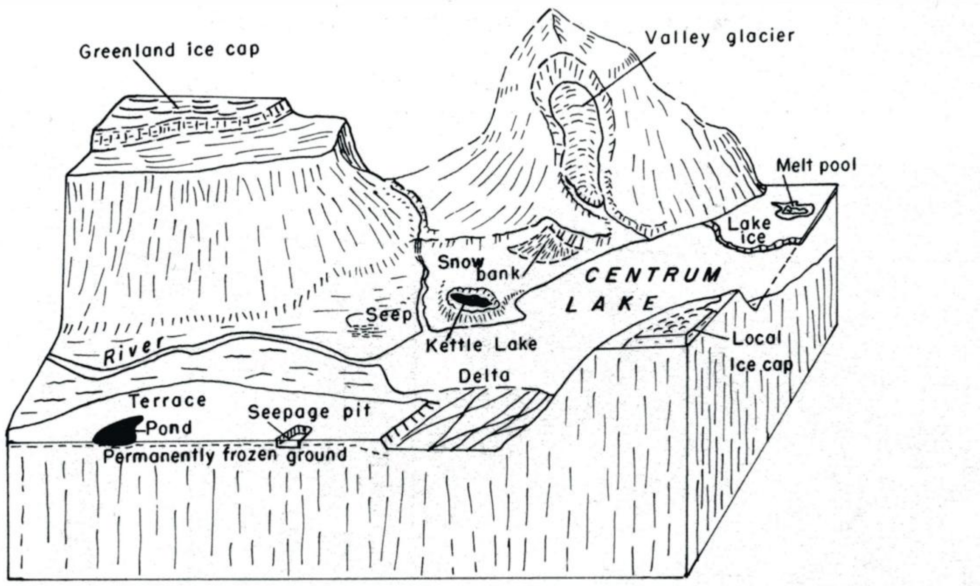

Figure 55. Possible sources of water at Centrum Sø.

Fig. 3: Possible sources of water at Centrum Sø (Needleman 1962).

Similarly, terrain analyses were made as a basis for site selection for transportation routes, facility sites, foundation conditions and camouflage possibilities. This would involve the mapping of glacial features, terraces, flood plains, beaches, including raised beaches, and other land forms. Engineering geology studies explored for the use not only of surface materials, but also of bedrocks and caves as underground facilities, of particular interest militarily, held special possibilities for protection both from the enemy and the weather, for economic operation and maintenance, and for good foundations (Davies 1959; Davies, Krinsley et al. 1963; Davies 1972).

Danish Greenland geology 1946 -1960: sovereignty claims and institutional conflict

During WW II the US had experienced a techno-scientific revolution, making vast amounts of technological and financial resources and manpower available to the development of the US military 'exploratory machine'. Denmark on the other hand, having tolled under German occupation, had been economically and technologically weakened, leaving a comparatively diminutive geo-scientific community frustrated by lack of scientific prospects and opportunity for action.

Through the first four decades of the 20th century, Danish sovereignty of Greenland had provided a great opportunity for Denmark to play a unique and internationally significant role in Arctic science and exploration. Not surprisingly, the end of WW II caused a release of pent-up energy among scientists and politicians to reclaim the last remains of Denmark's once great colonial empire, as an opportunity for international agency, rekindling pre-war hopes of significant finds of fossil fuels and mineral resources to support the ailing national budget.

Yet at the same time, the Danish geo-scientific community was tormented by internal competition and conflict. Before WW II, Lauge Koch –conducting large-scale expeditions in East Greenland on behalf of the Danish state– had effectively monopolized the geological investigation of the island, relying almost entirely on foreign geologists for his investigations. Professionally and economically stunted and bereft of options to partake in his prestigious and financially lucrative investigations, Koch's Danish colleagues accused him of improper scientific conduct, to which Koch responded with a lawsuit for slander. Bitter and mean, this conflict led to seriously strained relations between Koch and not only the Danish geologists, but to a certain extent also the entire Danish geo-scientific community (Ries 2002; Ries 2003; Ries 2007).

Between 1946 and 1958, when Koch retired, Denmark conducted two independent and competing state-subsidised geological surveys –GGU to the West and Koch to the East– each operating according to their own strategy and with very limited contact and cooperation. Danish presence in North Greenland converged mainly around a chain of smaller semi-private expeditions led by the archaeologist count Eigil Knuth, while, the Danish Geodetic Institute and its ambitious director, mathematician Niels Erik Nørlund, eagerly pursued topographic mapping activities, air surveys and gravimetric measurements across the island, financed partly by the private Danish mining company 'Kryolit-selskabet'.

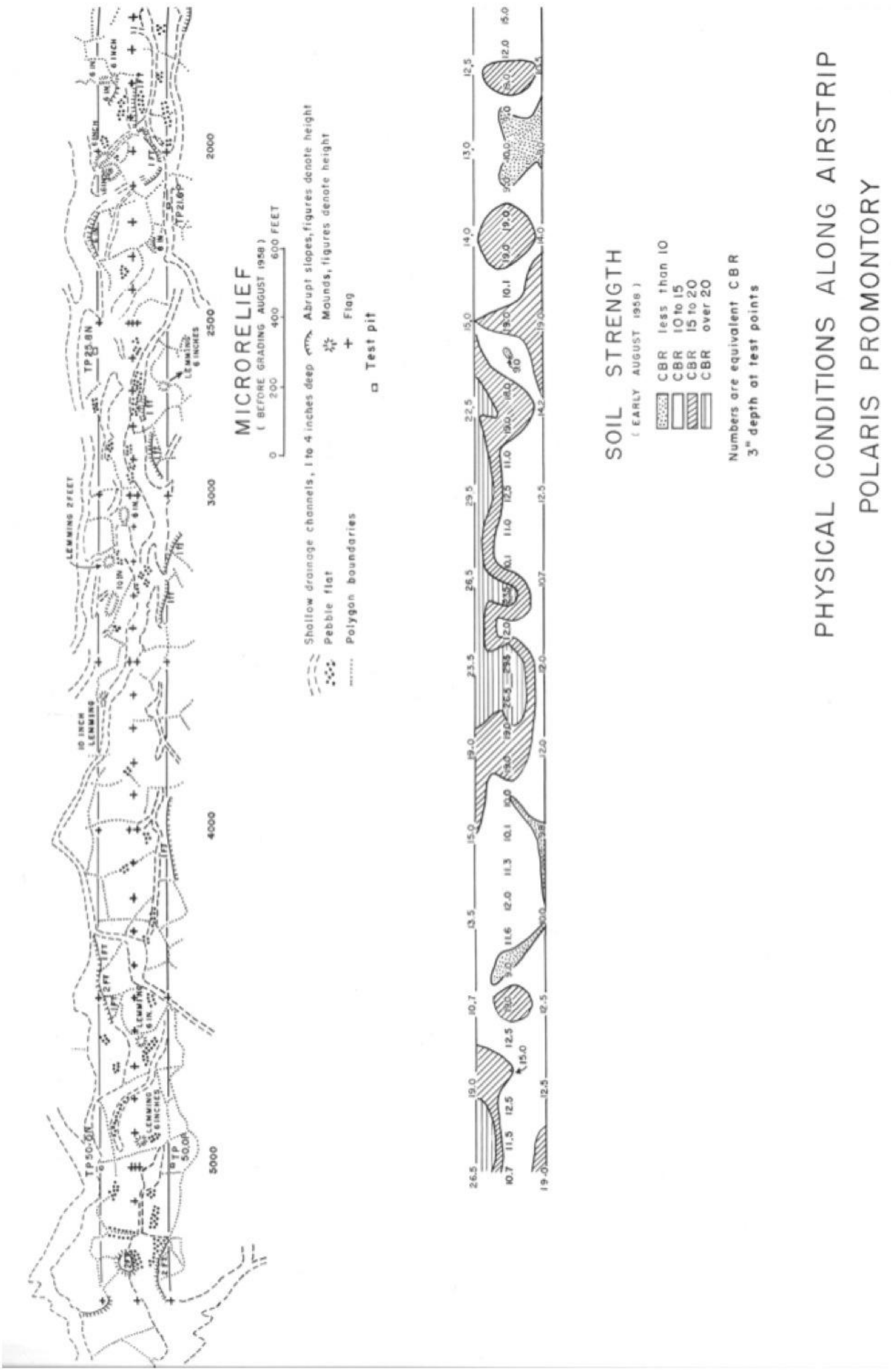

Fig. 4: Physical Conditions along airstrip Polaris promontory (Davies 1959).

In June 1945 Lauge Koch recapitulated the situation to his long-time friend Morten Porsild. Although still squarely rooted in a position as geological consultant for Director Knud Oldendow of the Greenland Administration, and with Oldendows word, that his ambition to continue internationally staffed explorations in East Greenland, Koch, was not optimistic about the future prospects of a serious and coordinated Danish geological exploration effort. In Koch's opinion, Knuth was merely an annoying disturbance; an amateur seeking only to conduct "*individual scattered investigations, mountaineering and artistic expeditions.*" His faith in the Danish geologists was not much stronger: " […] *it will be my role, in fact it is already my role to say to the director: 'The Danish geologists are not capable of the job.' "*(Koch 1945)

In Koch's own view, his 'most dangerous opponent' was Niels Erik Nørlund, mathematician and ambitious director of the Danish Geodetic Institute, who eagerly pursued topographic mapping activities, air surveys and gravimetric measurements across the island, financed partly by the private Danish mining company 'Kryolit-selskabet':

> *"Nørlund knows, that the only person capable of exposing his plans for what they really are – namely under the pretence of practical geological investigations to raise heaps of money for level measurements and a gravimetric network, which undoubtedly is of high scientific value, and will make Nørlund famous in his own circles– is me, who is honest and ruthless enough to point out, that even if Nørlund commands a large and capable crew, this has next to nothing to do with practical geological investigations."* (Koch 1945)

Practical geological investigations played an important role in Koch's own plan for a re-continuation of his geological investigations on the East Greenland Coast. Already during the 1930s Koch had located isolated small low-value deposits of coal, oil and gold on his expeditions sufficient for local use, and Koch now suggested continuing and expanding these practical investigations to include radioactive materials such as uranium and thorium (Grønland 1947). Although Koch disregarded the gravimetric measurements conducted by the Geodetic Institute marginal to practical geology, he agreed that accurate topographical maps were a precondition for any modern geological investigation. But he saw no need to wait for the Geodetic Institute to move into East Greenland, since already before the war, large enough parts of East Greenland had been topographically mapped, that it was possible to resume geological investigations in the region right away.

Arne-Noe Nygaard, once student of Lauge Koch now professor of geology at Copenhagen University, regarded his former mentor as 'a scoundrel of calibre' who should best be kept out of influence:

> *"If Koch gains footing for one year, we will have a five-year-plan on our hands […] The geologists know that they can handle him before he is established. Established, with a five-year-plan and trusted with a national task, he will be untouchable, and we will pay the price through a poisoning of our profession inwardly as well as outwardly. Geologists aren't angels, and our profession has been weakened for a number of years. We are now a small group, who do what we can and work very hard to catch up; we have a new batch of students underway and are getting to grips. Give us the right conditions and peace to work, and we will make it."* (Noe-Nygaard 1946?)

Still deeply hostile towards Koch, the Danish geologists regrouped in 1945 to form the Greenland Geological Survey (GGU) as a state institution to carry out geological investigations in West Greenland without interference from Koch. Closely aligned with the Dept. of Geology at Copenhagen University, GGU focused on general geological surveying and mapping, while serving as a training ground for a new generation of Danish geologists (Ellitsgaard-Rasmussen 1996). While GGU supported the efforts of the Geodetic Institute to provide a new and much needed topographical map of West Greenland, they were, as Noe-Nygaard explained to Oldendow, not in favour of what they feared would be premature practical investigations:

> *"Seen from the outside, it might be tempting to aim directly for what seems to be 'the raisins in the cake'. Should these raisins however turn out to be 'non-existent', we are left empty-handed* […] *in that we will still know nothing of the cake itself* […] *The conclusion must be, that a systematic geological survey should be commenced in a region, where the geodesists have completed their topographical map, and in which we from a geological point of view would expect a priori, that principal questions can be answered within a reasonable period of time."* (Noe-Nygaard 1945)

After WWII then, the Danish Greenland Administration was tasked with balancing an entanglement of different interests in a landscape of eager and ambitious actors whose mutual relations ranged from nervous to hostile, faced with the additional challenges of a strained national economy and rapid US expansion in Greenland. This was why on January 7 1947, the Danish Government decided to establish an interdepartmental board to coordinate Danish tasks in Greenland *"and especially such tasks, which during the occupation had been under American military administration*", including military and marine operations, mapping, meteorology, radio service and expedition logistics, aiming for *"increased effectiveness in addition to reasonable savings."* (Grønland 1947)

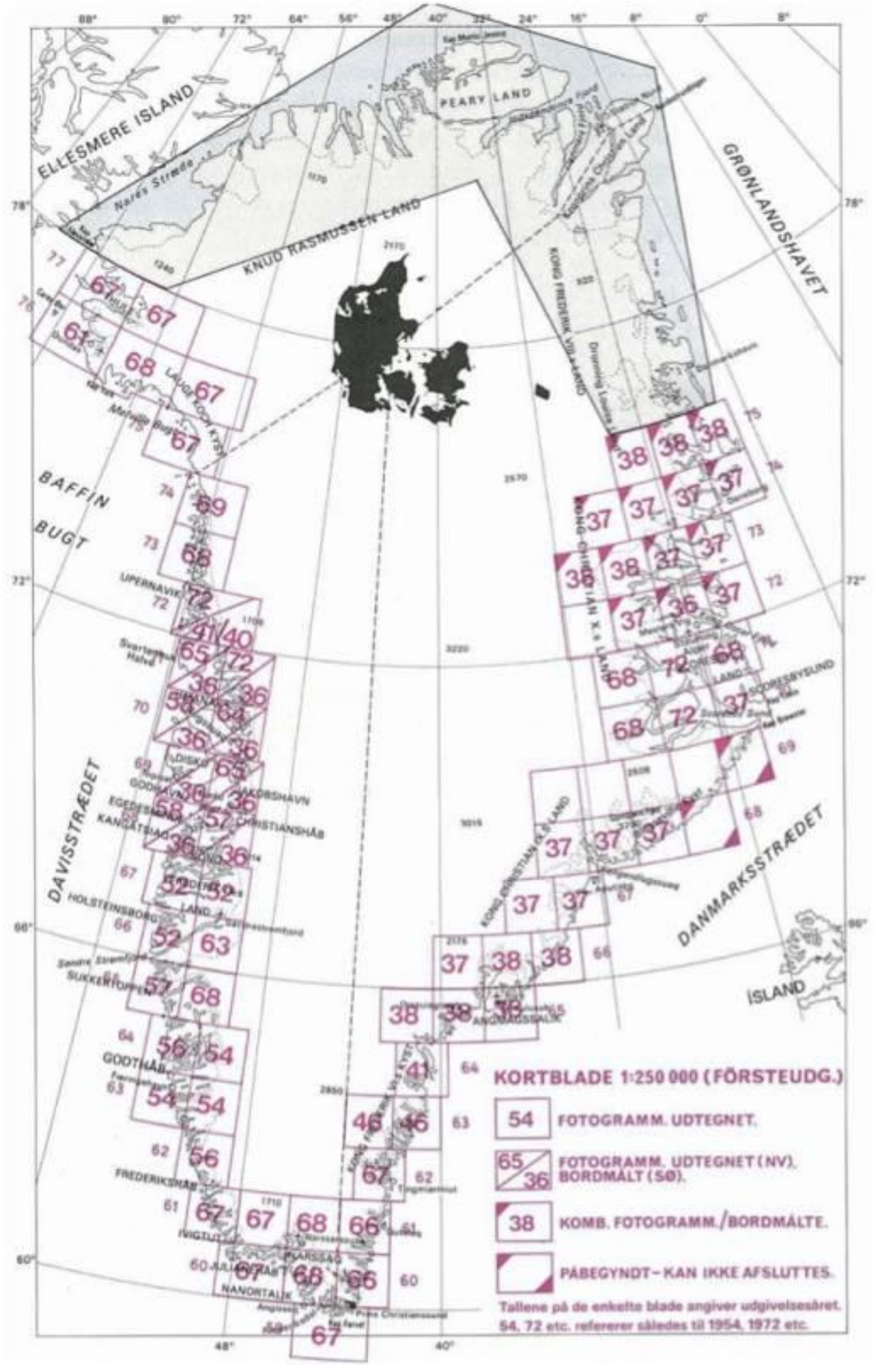

Fig. 5: Map sheets published by the Danish Geodetic Institute by 1978.
Numbers on sheets indicate year of publication (Sørensen 1978).

The board was disestablished in 1949, when Oldendow retired. In 1950, the Greenland Administration was transformed into a department under the State Ministry. In 1951, after a trial period of five years, GGU was made a permanent institution under the Greenland Administration. After Koch's retirement in 1958, GGU continued to expand its activities in close alignment with Copenhagen University, but not until 1965 did GGU begin systematic investigations in Northern Greenland, followed in 1967 by the first entry into Koch's East Greenland (Henriksen 2002). Throughout this period, Geodetic Institute continued to expand its mapping activities south of 78°N, while Knuth remained an active presence in Northern Greenland –Danish and therefore politically important. Logistically, scientifically and technologically Denmark remained dwarfed out by the Americans.

Fig. 6: Operation Groundhog 1960, left to right: William E. Davies, USGS Military Geology Unit; Count Eigil Knuth, archaeologist, Denmark; Stanley M. Needleman, project leader, geophysicist, Air Force Cambridge Research Center (Needleman 1962).

Final words

So far, the DK/US entanglements over Greenland at the geo-political level seem to have been matched by limited common interests in Greenland geology from a scientific point of view. While US geologists focused on short-term dynamic geological processes for military engineering purposes, Denmark focused on systematic structural investigations for scientific and/or economic purposes.

However, this brief paper has only scratched the surface of the history of the Danish/American geological exploration of Greenland between 1947 and 1960. Much work lies ahead to grasp not only the specific ways in which geo-political and military interests shaped American geological scientific practices in Greenland, but also how Danish Greenland geology was affected by institutional hostility and competition under the challenge of marked US presence in a post-colonial setting wrought with global military and geopolitical tensions.

References

Arctic Institute of North America, O. o. N. R., Arctic Research Advisory Committee (1958). H.R. 9196, H.R. 9978 and H.R. 10122, To authorize the construction of a Nuclear-powered icebreaking vessel for operation by the United States Coast Guard, and for other puposes, January 22, 23 and 24, 1958. *Committee on Merchant Marine and Fisheries House of Representatives*. Washington, United States Government Printing Office: 172.

DAVIES, W. E. (1959). Report on Operation Groundhog (1958) North Greenland. *Investigation of the Ice-free Sites for Aircraft Landings, Polaris Promontory, North Greenland*. Needleman and S. M., Air Force Cambridge Research Center**:** 45.

DAVIES, W. E. (1972). Landscape of Northern Greenland. Hanover, New Hampshire, Cold Regions Research and Engineering Laboratory. *Special Report 164.*

DAVIES, W. E., D. B. KRINSLEY, *et al*. (1963). *Geology of the North Star Bugt area, Northwest Greenland*. København.

DeGoes, L. and J. T. Neal (1998). Selected military geology programs in the Arctic, 1950-1970. In: *Military Geology in War and Peace*. J. R. J. a. G. Underwood, P.L. Boulder, Colorado, Geological Society of America Reviews in Engineering Geology. XIII: 205-210.

DUPI (1997). *Grønland under den Kolde Krig: Dansk og amerikansk sikkerhedspolitik 1945-68*. København, Dansk Udenrigspolitisk Institut.

Ellitsgaard-Rasmussen, K. (1996). En stjerne fødes. Beretning om GGU's tilblivelse., Danmarks og Grønlands Geologisk Undersøgelse, Miljø- og Energiministeriet.

Grønland, U. t. C. a. d. f. o. p. (1947). Udvalget til Coordination af de forskellige opgaver paa Grønland, 3. Møde, 28/2. *Niels Erik Nørlunds Arkiv*. København, Rigsarkivet. 63.

Henriksen, N. (2002). 'Geologisk kortlægning i Grønland - forudsætninger, metoder og resultater.' *Geologisk Tidsskrift* 2002/1: 48.

Koch, L. (1945). Koch to Porsild, 18/6-45. *Archive of the Editorial Board of Meddelelser om Grønland* Danish National Archive. Parcel 2.

Krinsley, D. B. (1998). Location of sites for airstrips in North Greenland. In: *Military Geology in War and Peace*. J. R. J. a. G. Underwood, P.L. Boulder, Colorado, Geological Society of America Reviwes in Engineering Geology. XIII: 189-204.

Needleman, S. M. (1962). *Arctic earth science investigations, Centrum Sø, Northeast Greenland, 1960*. Bedford, Massachusetts Geophysics Research Directorate, Air Force Cambridge Research Laboratories, Office of Aerospace Research, United States Air Force.

Noe-Nygaard, A. (1945). Noe-Nygaard to Oldendow, 31/7. *Arne Noe-Nygaards Arkiv*. København, Geologisk Museums Arkiv. GGU-udvalget, 6/2-1940 – 14/12-1955.

Noe-Nygaard, A. (1946?). Note to himself. *Arne Noe-Nygaards Archive*. København, Geologisk Museum. GGU's fremtidige ordning.

Ries (2003). *Retten, magten og æren*. København, Lindhardt og Ringhof.

Ries, C. J. (2002). Lauge Koch and the Mapping of North East Greenland: Tradition and Modernity in Danish Arctic Research 1920-1940. In: *Narrating the Arctic: A Cultural History of Nordic Scientific Practices*. M. Bravo and S. Sörlin, Science History Publications**:** 199-231.

Ries, C. J. (2007). "Inventing the four-legged fish: palaeontology, politics and popularization of the Devonian tetrapod *Ichthyostega* 1931-1955." *Ideas in History* 2: 41.

Sørensen, E. A. e. (1978). *Geodætisk Institut 1928-1978*, Geodætisk Institut.

Whitmore, F. C. (1960). "Terrain Intelligence and Current Military Concepts." *American Journal of Science* Bradley Volume, Vol. 258-A: 12.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 869.

CONFIDENTIALITY VS. PUBLICITY: EMERGING TENSIONS IN SCIENCE AND TECHNOLOGY DURING THE COLD WAR

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Abstract

One important tension involved in the military patronage of scientific research is that of confidentiality vs. publicity. While most military operations are based on controlling flows of information, it is commonly agreed that science needs open publishing, unrestrained communication among peers across national borders, academic freedom, and public understanding of science to flourish. With the spread of extensive military-science collaborations during the Cold War, the balancing of military needs and scientific norms became critical, not the least when it came to public understanding of science and science policy issues. This paper aims to explore the tension between confidentiality vs. publicity as a tool of Cold War Science historiography. Looking at scientific activities during the International Geophysical Year of 1957-58 (IGY), the paper provides a few examples of how military bodies and scientific organizations in practice managed the tension.

Classified vs. open science

By the end of World War II, two opposed conceptions of science had emerged: classified vs. open science. On the one hand, science had proven highly efficient as a tool of military technology. The Manhattan Project, led by the USA with participation from Britain and Canada, which led to the development of the first atomic bomb, clearly demonstrated that focused and organized research could result in technological achievements of unprecedented military-strategic importance. The Project began as small research program and eventually employed more than 130,000 people doing research and production at more than 30 sites. Also, the Manhattan Project was charged with intelligence gathering on the German nuclear energy program, seizing materials, personnel, and other resources at any given opportunity. Placing tight restrictions on the flow of scientific information, dictating remote site locations, and required subterfuge in obtaining labor and supplies, the Project was a highly secretive endeavor, which served as a constant irritant to the scientists involved (Atomic Archive, 2011; Kelly, 2007; Stoff, Fanton, & Williams, 1991).

Whereas the Manhattan Project established the military value of classified science, many scientists and sociologists of science at the time firmly believed in and frequently endorsed the ideal of open science. For example, in 1942, sociologist of science Robert K. Merton, provoked by what he and many others saw as the exploitation of science by Nazi Germany and other totalitarian regimes, published an article on the normative structure of science (Merton, 1973 [1942]). Merton's norms included:

- *Communalism* (Merton used the term *Communism*): Scientific results are the common property of the entire research community.
- *Universalism*: All scientists regardless of race, nationality, culture, and gender can contribute to science.
- *Disinterestedness*: Scientists should not present their findings entangled with their personal beliefs or activism for a cause. In other words, scientists should maintain an arms-length attitude to their own findings.
- *Organized skepticism*: All scientific claims must be exposed to critical scrutiny before being accepted.

Merton's norms prescribed an open and disinterested attitude to scientific claims and findings on behalf of all scientists. At the same time, Merton clearly envisaged a link between the micro-society of scientists and a properly democratic macro-society. The open society, to use Karl Popper's term, was fully compatible with the ideal of open science – indeed, the two feed on each other (Kitcher, 2001; Popper, 1945). The close link between democratic societies and science, however, also meant that science had to be open to influences from society. In Vannevar Bush's 1945 report to the President, *Science—the Endless Frontier*, which proved highly influential with respect to US science policy, the freedom of science was tightly coupled to the progress of society. Because of its huge implications for health, public welfare, industry, and national security, science was a proper concern of Government, Bush argued (Bush, 1945). The openness of science in a democratic society based on social welfare and economic growth not only pertained to the communication and sharing of results, but also to political and military planning of scientific research.

Science and public policy

Vannevar Bush recommended that a single new agency should be established to provide Government support of science, or more precisely, in the words of William A. Blanpied (1999), "to support the self-directed basic research of university scientists." The agency envisaged by Bush was not realized until 1950. Instead, a Presidential Scientific Research Board was charged with reviewing current and proposed activities both within and outside the Federal Government. Its report, *Science and Public Policy*, came out in 1947, recognizing the need for finding a "workable compromise" between scientists' call for open science and the military's and the Government's need to keep some scientific results secret (Steelman, 1947, p. 35).

In particular, the Steelman Report, named after the chairman of the committee, made a distinction between the need for freedom of scientific inquiry and the need for national security. Freedom was not part of Merton's normative structure for science, see above. But freedom was highly stressed by Vannevar Bush in his *Frontier* report:

> Many of the lessons learned in the war-time application of science under Government can be profitably applied in peace. The Government is peculiarly fitted to perform certain functions, such as the coordination and support of broad programs on problems of great national importance. But we must proceed with caution in carrying over the methods which work in wartime to the very different conditions of peace. We must remove the rigid controls which we have had to impose, and recover freedom of inquiry and that healthy competitive scientific spirit so necessary for expansion of the frontiers of scientific knowledge. (Bush, 1945, Chapter 1.4)

The Steelman Report fully acknowledged this view on freedom, putting it down as "the views of scientists" (Steelman, 1947). The opposing view was equally recognized, namely that freedom for science requires democratic institutions, which cannot survive or thrive without strong national security measures. Modern warfare, it was argued, is total war, involving all of the resources of the nation. Scientific knowledge, bearing upon people, education, industry, agriculture, military, etc., therefore, is vital to national security and must be considered secret and kept under strict security regulations (p. 35).

However, the report further argued, search for an acceptable middle way between the two extreme viewpoints for security and freedom of science was required. Some leading military officers, Government officials, and politicians recognized the importance of scientific freedom; among the scientists, the need for some degree of security was also acknowledged. Besides the personal awareness of the other parts needs and requirements, some of the following measures were recommended in order to guarantee secrecy on scientific work (p. 36):

- *Compartmentalization*: No individual should know more about the total project than is absolutely necessary for him to work effectively.
- *Classification*: Indicating the degree of precaution to be used in safeguarding factual knowledge against unauthorized disclosure.
- *Loyalty checks*: Ensuring that every scientific worker and every other employee engaged in scientific research is completely loyal to the US as well as thoroughly discreet.

The tension between secrecy and freedom, the Steelman report maintained, had to be solved by means of a workable compromise. Strict military security in the narrow sense was not seen as consistent with the full flowering of free scientific inquiry, which again was one of the broader requirements of national security. The US, being in a unique situation because of its advanced technology, had nothing to fear from the free circulation of scientific knowledge, the Steelman Report concluded:

> On the contrary, as a people we have a real national interest in such circulation, in the certainty that we can apply basic discoveries with the same ingenuity and technical skill and speed we have displayed in the past. **Security regulations, therefore, should be applied only when strictly necessary and then limited to specific instruments, machines or processes. They should not attempt to cover basic principles or fundamental knowledge.** (Bold in original: Steelman, 1947, p. 37)

Science and foreign relations

The difference between classified and open science was more than a tension for post-war science policy-makers. It also served as a constructive way of dealing with, or rather including science into US foreign policy. Classified science was helpful to the US military in terms of knowledge production, technical innovation, and scientific intelligence, aimed at increasing the US strategic advantage. Open science, for its part, played an important role in the ideological struggle of the Cold War. As we shall see later, promoting open science in public became a way for scientists and science policy-makers in which to demarcate Western science from Communist science, thus making the link between open science and open society useful as a weapon in the clash between ideologies.

During the Cold War, the USA and the Soviet Union both attempted to build empires that strived for ideological hegemony by a number of different means such as diplomacy, psychological or political warfare, European reconstruction, intelligence, nuclear deterrence, etc. This was recognized by contemporaries, cf. the Steelman report cited above, and this recognition today forms part of the so-called "new historiography of the Cold War": According to Gaddis (1997), the Cold War resulted from a diversification of power, rather than a balancing of (military) power. Whereas, previously, historians have tended to see the power struggle of the Cold War as a bipolar battle between nuclear superpowers, we now recognize that, during the Cold War, the exercise of power also took place on ideological, moral, economic, technological, scientific, and cultural grounds. Communication and cooperation were "weapons" of the Cold War just as conflict and coercion. Ultimately, the Cold War did not end because one of the two superpowers acquired military superiority –in fact, the nuclear arms race seemed to have prolonged the Cold War rather than end it. What did end the Cold War was the superiority of the "soft power" of the USA: liberal-democratic and capitalist values, mass consumption culture, and the proliferation of private companies and public institutions organized in accordance with American ideals.

By the end of World War II, some US scientists such as Vannevar Bush clearly saw the need to bring closer together science and State affairs. Others, such as those engaged in the International Science Policy Survey Group (1950), led by Lloyd V. Berkner, also considered scientific cooperation, communication, and intelligence as pivotal elements of science in relation to government and foreign relations. The basic premise was that, in order to make American foreign policies effective, it was deemed necessary –the report cited Secretary of State Dean Acheson on this point, to make such policies articulate through all the institutions of our national life, including science being "an important element of American culture" (International Science Policy Survey Group, 1950, p. 9).

The American government, the International Science Policy Survey Group (1950) argued, would have to take an active stance in promoting scientific advances, in particular "research that can be beneficial to ourselves and our Allies" (p. 4), collaborations between scientists in the USA and abroad, and the free flow of scientific information all around the world. Advancing scientific collaboration and communication potentially would have three important consequences: 1) it would provide scientists (and intelligence officers) with cutting-edge knowledge about the progress of science and the state of scientific institutions in many countries (also countries on the other side of the Iron Curtain, it was hoped); 2) it would help place topics deemed fundamental to American science, technology, industry, and policy on the research agenda of other countries' research programs; and 3) it would enable the spread of democratic and liberal values, which were thought to be inherent to science and to the American way of life, around the globe (International Science Policy Survey Group, 1950).

Trained as radio engineer and ionospheric physicist, Berkner spent a significant part of his professional life mediating between science and foreign policy in the USA during the first part of the Cold War (Needell, 2000). His second State Department assignment, after the *Foreign Relations* report, was facilitating Project TROY, a top secret study group of twenty-one scientists, social scientists, and historians, most of whom were academics. Project TROY engaged brilliant minds in thinking about the problem of "getting the truth behind the Iron Curtain", i.e. the truth about ideologies and forms of government on both side of the Iron Curtain. As explored by scholars such as Kenneth Osgood (2006), psychological or political warfare formed an important part of the American response to the Cold War. The Project TROY report in particular analyzed the problem of getting information into enemy territory (Needell, 1993). Converting the minds and hearts of the population, it was believed, would enable a "rollback" of Soviet influence in the Eastern Block. Another element of psychological warfare envisaged by the Truman and the Eisenhower Administration was presenting US policies to an international audience in order to convince them of the peaceful and collaborative attitude of the Americans. One example is provided by the Atoms for Peace Campaign enacted by President Eisenhower (Osgood, 2006, pp. 153-180). Importantly, the campaign involved many strategic communication activities, all of which aimed to show the American initiative as a way in which to promote scientific advance, technological progress, and material welfare all around the globe (see Figure 1).

Fig. 1: The Atoms for Peace postage stamp, designed by the US Postal Service in order to promote Eisenhower's campaign.

Balancing secrecy and openness: the making of the International Geophysical Year 1957-58

The Cold War provided leading US scientists with many occasions to get together and discuss collaborative efforts and funding opportunities (Kevles, 1990). One example, which evolved into an international scientific endeavor, was the International Geophysical Year 1957-58 (IGY), described in numerous publications (for an overview of the historiography of the IGY, see: Launius, 2010). Too much of this writing has tended to overlook the importance of the Cold War setting of the IGY, highlighting, with a slant of sensationalism, the scientific planning, collaborations, and contributions. However, as Fae Korsmo (2009) emphasizes, "basic science and military objectives both were motivations" (p. 24).

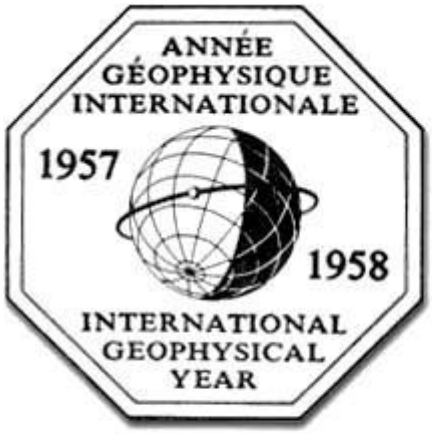

Fig. 2: The original IGY logo depicts planet Earth with no geographical or political features. The one circulating celestial body symbolizes the US and Soviet programs to launch artificial satellites. With its atomic connotations (compare Figure 1), we might in hindsight see the encircled globe as being entirely enclosed in a titanic nuclear struggle.

The tension between classified and open science is directly reflected in the ways in which the story of the origin and context of the IGY is depicted (see Figure 2). By far most accounts see the IGY as a prime example of open and free scientific inquiry. Such narratives usually begin with a dinner conversation that took place in the home of James Van Allen, then leader of a high altitude research group at Johns Hopkins University, on the evening of April 5, 1950. The dinner party included Sydney Chapman, British geophysicist, Lloyd V. Berkner, and three other geophysicists: J. Wallace Joyce, Fred Singer, and Ernest H. Vestine. According to Van Allen:

> The dinner conversation ranged widely over geophysics and especially geomagnetism and ionospheric physics. Following dinner, as we were all sipping brandy in the living room, Berkner

> turned to Chapman and said, "Sydney, don't you think that it's about time for another international polar year?" Chapman immediately embraced the suggestion, remarking that he had been thinking along the lines himself. (Van Allen, 1983)

The open narrative of the IGY then proceeds with Berkner and Chapman, three months later, presenting their ideas at the meeting of the Mixed Commission on the Ionosphere in Brussels. The Commission was one of several of such bodies, designed to bring together men from various international scientific unions with a common area of interest. The participants recommended the proposal to the scientific bodies represented in its membership and to the International Council of Scientific Unions (ICSU). ICSU, a nongovernmental body supported by UNESCO, embraced the idea, securing its internationalist, scientific aspirations. In the fall of 1951, the ICSU Executive Board met in Washington and decided to create a special committee to work on plans for what was still being referred to as the Third International Polar Year. Later, by recommendation of Chapman, it was decided to change the name to International Geophysical Year. The scientists decided that the IGY should run for eighteen months, from July 1, 1957, through December 31, 1958, a period which would coincide with an expected peak of sunspot activity as well as several eclipses. Many nations formed IGY scientific committees to organize national efforts –sixty-seven national scientific teams were involved. In the US, the National Academy of Sciences persuaded the Government that it should underwrite a major US effort. Making explicit the scientific, international driving forces of this version of the story, one of the popularizers of the IGY, Walter Sullivan, in *Assault on the Unknown*, describes the IGY as "a scientific club. To gain admittance –that is, to be included in the IGY program– a scientific project had to be concerned with 'specific planetary problems of the earth'" (Sullivan, 1962, p. 30).

The view of IGY as a purely scientific effort may be contrasted with the historical narrative that places the IGY in a Cold War setting. Allan A. Needell (2000), historian of science and curator of space history at the Smithsonian, notes that it probably was no coincidence that Berkner's suggestion at the Van Allen appeared right after the publication of the *Science and Foreign Relations* report (see above). Even though the Polar Year proposal, at least at first, was pursued quite independently of specific national security projects at the time, Berkner and others, who played a double role ad scientist and Government adviser, made every effort to ensure a seamless blending of science and national security. A case in point is the 1950 recommendation, which emerged from a panel under the US Research and Development Board, to establish ionospheric observing stations along the 75° west meridian linking a north-south chain of stations with existing US-Canadian installations. Within a year, the proposal was adopted in the concept of the Third International Polar Year, and the three meridians chosen in 1952 for extensive ionospheric observation did include the 75° west. Arguing that it was not simply the case that the choice of meridians was good for the military, but bad for science, Needell (2000) explicitly warns against making too much of this circumstance (p. 302). Still, it was the case that scientific advisers such as Berkner actively mediated between national security and scientific interests. With respect to Berkner's active role in organizing the IGY (he served as one of the core members of ICSU's Comité Spécial de l'Année Géophysique Internationale), Needell (2000) remarks:

> Berkner continued to welcome and refine the complex roles of scientific advocate and broker between the realms of science and national security. […] secrecy, and contradictions between norms and values current in the different realms, seemed ultimately to be manageable as long as they remained effectively compartmentalized and their overlap remained unpublicized and under the management of a few discreet, well-placed, and experienced individuals. Juggling scientific values and national requirements was Berkner's special talent and increasingly his most powerful calling. (p. 307)

Another historian of science who has emphasized the carefully managed relationship between science and national security in the making of the IGY is Rip Bulkeley (Bulkeley, 2000, 2010). Bulkeley (2010) notes that, although scientific interests have been strongly promoted by IGY founders as the only incentive to commence on the huge international collaboration and competition, other factors too were decisive. The Cold War, of course, was the overarching political framework in which the IGY made equally sense to scientists and politicians. The scientific networks mobilized during World War II to forecast weather, trace ocean currents, detect ionospheric dynamics, etc., were not only extended, but also improved in the course of the Cold War (Doel, 2003). Along with a number of current domestic affairs, scientists and policy-makers had every reason to use the impetus to the geophysical sciences to serve scientific ideas as well as US foreign policy.

A short conclusion

The IGY serves as a prime example of the tension between confidentiality and publicity that emerged during the Cold War. For practical as well as ideological reasons, scientists increasingly sought military and political backing. They carefully had to manage their projects such as the IGY balancing the open norms of science and the closed world of the military/foreign relations. The IGY serves as a (successful) reminder that the tension not only was problematic, but in some cases proved too highly fruitful. Confidentiality and publicity provided scientists with two strong ideological, political and scientists resources during the Cold War.

References

Atomic Archive. (2011). The Manhattan Project: Making the Atomic Bomb. Retrieved 6 March, 2011, from http://www.atomicarchive.com/History/mp/index.shtml

BLANPIED, W. A. (1999). Science and Public Policy: The Steelman Report and the Politics of Post-World War II Science Policy. Retrieved 7 March, 2011

BULKELEY, R. (2000). The Sputniks and the IGY. In R. D. LAUNIUS, J. I. LOSDGON & R. W. SMITH (Eds.), *Reconsidering Sputnik: Forty years since the Soviet Satellite* (pp. 125-160). London: Routledge.

BULKELEY, R. (2010). Origins of the International Geophysical Year. In S. BARR & C. LÜDECKE (Eds.), *From Pole to Pole: The History of the International Polar Years* (pp. 235-238). Berlin: Springer.

BUSH, V. (1945). Science, The endless frontier: A report to the President. Retrieved 7 March, 2011, from http://www.nsf.gov/od/lpa/nsf50/vbush1945.htm

DOEL, R. E. (2003). Constituting the Postwar Earth Sciences: The Military's Influence on the Environmental Sciences in the USA after 1945. *Social Studies of Science*, 33(5), 635-666.

GADDIS, J. L. (1997). *We now know: rethinking Cold War history*. Oxford: Clarendon Press.

International Science Policy Survey Group. (1950). *Science and foreign relations: international flow of scientific and technological information*. Washington: Division of Publications, Office of Foreign Affairs.

KELLY, C. C. (2007). *The Manhattan project: the birth of the atomic bomb in the words of its creators, eyewitnesses, and historians*. New York: Black Dog & Leventhal Publishers : Distributed by Workman Pub.

KEVLES, D. (1990). Cold-War and Hot Physics - Science, Security, and the American State, 1945-56. *Historical Studies in the Physical and Biological Sciences*, 20(2), 239-264.

KITCHER, P. (2001). *Science, truth, and democracy*. Oxford ; New York: Oxford University Press.

KORSMO, F. L. (2009). The Policy Process and the International Geophysical Year, 1957-1958. In I. KRUPNIK, M. A. LANG & S. E. MILLER (Eds.), *Smithsonian at the Poles: Contributions to International Polar Year Science* (pp. 23-34). Washington: Smithsonian Institution Scholarly Press.

LAUNIUS, R. D. (2010). Toward the Poles: A Historiography of Scientific Exploration during the International Polar Years and the International Geophysical Years. In R. D. LAUNIUS, J. R. FLEMING & D. H. DEVORKIN (Eds.), *Globalizing Polar Science: Reconsidering the International Polar and Geophysical Years* (pp. 47-81). New York: Palgrave Macmillan.

MERTON, R. K. (1973 [1942]). The Normative Structure of Science. In R. K. MERTON (Ed.), *The Sociology of Science: Theoretical and Empirical Investigations* (pp. 267-278). Chicago: University of Chicago Press.

NEEDELL, A. A. (1993). "Truth Is Our Weapon": Project TROY, Political Warfare, and Government-Academic Relations in the National-Security State. *Diplomatic History*, 17(3), 399-420.

NEEDELL, A. A. (2000). *Science, Cold War, and the American State: Lloyd V. Berkner and the Balance of Professional Ideals*. London: Harwood Academic Press.

OSGOOD, K. (2006). *Total Cold War : Eisenhower's Secret Propaganda Battle at Home and Abroad*. Lawrence, Kan.: University Press of Kansas.

POPPER, K. R. (1945). *The open society and its enemies*. London,: G. Routledge & sons.

STEELMAN, J. R. (1947). *Administration for research. Science and Public Policy, volume 3*. Washington DC: The President's Scientific Research Board.

STOFF, M. B., FANTON, J. F., & WILLIAMS, R. H. (1991). *The Manhattan Project : a documentary introduction to the Atomic Age*. Philadelphia: Temple University Press.

SULLIVAN, W. (1962). *Assault on the unknown*. [London]: Hodder and Stoughton.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 875.

POLAR RESEARCH AND THE ENDINGS OF THE COLD WAR

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Abstract

Throughout the Cold War, the scientific efforts would serve many political interests. It is not difficult to find examples where science was attached to a political strategy –both in the East and in the West– which focused on power-oriented manoeuvring. This also holds to be true for the activities of the Norwegian Polar Institute (NPI). For decades it seemed to be some kind of a leitmotiv for the NPI not to let any of the Great Powers, and especially not the Soviet Union, get such advantages at Svalbard or in any other strategically important areas, that it loosened up or threatened Norway's sovereign rights. But this is not the whole story. Another strategy was also always evident in polar research, the one that was oriented towards true international cooperation, and this strategy will be described in this presentation. On the basis of the rich source material from the NPI, we will analyse the role that NPI and polar research played towards the end of the Cold War. Polar scientists from several countries –Canada, Denmark, Norway– invited partners in the Soviet Union to a closer cooperation, and when Gorbachev took seat in the Kremlin in 1985, this also laid a new foundation for polar research throughout the whole Arctic region.

Where were the drivers behind a wider scientific cooperation between the West and the Soviet Union throughout the Cold War? Many explanations must be taken into consideration, and in this presentation I only hope to shed some light on a few.

The political development inside the Soviet Union mattered much. This is certainly true for the period after Stalin's death in 1953 –with Khrushchev's attack on Stalinism, and eventually, with the abolishment of Stalin's severe strategy of scientific screening. It made it possible for the Soviet Union to take part in the IGY, and in many international scientific bodies. As for the 1980s, Gorbachev and the end of the Cold War, I will return to that.

Important was also the general trend towards lower political tensions after "the deep Cold War years 1947-53", particularly during the détente period after the Cuban Missile Crises in 1962.

In addition, and with special relevance for this Symposium, are the many scientists –both in the West and in the East– who believed in scientific internationalism; that they were not just servants of nations, but also of the "Republic of letters" –the international scientific community–, and thereby serving humanity.[1] One of the best coverages of this is Matthew

[1] More on the concept, see Sverker Sörlin, *De lärdas republic. Om vetenskapens internationella tendenser*, Malmö 1994.

Evangelista's book, *Unarmed Forces*.[2] He shows how physicians and other scientists managed to form transnational networks which eventually gave a decisive impetus behind Gorbachev's "new thinking". These networks certainly made a difference.

While Evangelista is primarily focusing on the work that was done to prevent a "nuclear showdown", for instance by the Pugwash movement, I believe that also other scientific fields and disciplines are, to some extent, comparable to this.

International contacts forged through scientific cooperation contributed to a lasting dialogue that helped to ameliorate the antagonism of the Cold War, Evangelista states.[3] So there *was* an option for scientists to work as political bridge-builders. I believe that Evangelista would agree if we rephrase this sentence slightly, and state that also scientific *problems* – the common challenges shared both by Western and Eastern scientists– might work convergent. The case of Soviet-Norwegian cooperation in polar research can illustrate both of these trends.

* * *

Norway was the only European NATO member bordering the Soviet Union. This "neighbourly asymmetry" was no doubt the most important foreign policy question throughout the cold war, and thus it can be argued that Norway was in fact *forced* into some sort of cooperation with its powerful neighbour. To some extent this can serve as an explanation, but as an overall explanation, it falls short of evidence.

The scientific field where Norway and the Soviet Union established the closest and most lasting cooperation, was in marine research. The cooperation started up in 1956, as a result of a joint political and scientific initiative from the *Norwegian* side. The Norwegian Prime Minister, Einar Gerhardsen, saw the thaw under Premier Nikita Khrushchev as an opportunity to move into a political dialogue with the Soviet Union, and suggested that marine research might be one of the suitable channels. The Institute for Marine Research was also interested in this, and leading scientists there saw the newly established contacts as an opportunity of getting access to Russian data sets and to improve their models. Although the continued contact was formally taking place under international or regional agreements (such as ICES and NEAFC)[4], the Soviet-Norwegian cooperation proved so successful that fishery cooperation was formalized by means of two *bilateral* agreements in 1975 and 1976. In effect, both the Norwegian and the Soviet management of the fish stocks in the Barents Sea from 1976 were carried out on the foundations of a joint scientific effort.

Marine research can be described as the Soviet-Norwegian success story, but what about other disciplines in the natural sciences? This brings me to the Norwegian Polar Institute, and this institution will be at the centre of the rest of my presentation.

The Polar Institute was Norway's principal research institute in the Polar Regions, and was also given the demanding task of surveying and mapping Norwegian Polar territories, both north and south. In addition it functioned as a consultative organ for many ministries, especially the Ministry of Industry (which was responsible for the Polar Institute), the Ministry of Justice, the Ministry of Foreign Affairs, and (later on) the Ministry of the Environment, which took over the responsibility for the Polar Institute in 1979. As the Director, Tore Gjelsvik, put it in 1973: "One can therefore not see the Institute only as a research institution, since its structure and multi-disciplinary activity make it a useful instrument for the authorities in the forming of Norwegian polar policy". "It feels a responsibility", Gjelsvik stated, "for guarding our national interests in a border area, and has been able to warn and help the authorities in connection with crisis types of situations both in the north and in the south".[5]

The IGY provided, for the NPI, a good example on how "Norwegian interests" could be best served. An effective international cooperation in the Antarctic helped temper the antagonism between East and West, which was of course positive for a small state such as Norway, but indeed also for the international community as a whole.

Norway's primary goal was to establish multilateral agreements also on Arctic research, and to strengthen already existing international organisations. The Director of NPI, Tore Gjelsvik, who was Norway's national delegate to the SCAR meetings and the president of SCAR 1974-78, tested out various possibilities. One idea was to expand the scope of SCAR

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[2] Matthew Evangelista, *Unarmed Forces. The Transnational Movement to End the Cold War*, Ithica and London 1999.

[3] One might, although very simplified, operate with two possible strategies which can serve as analytical tools. One would be leaning towards dialogue, internationalism and, perhaps, transformation of the international system (where "open science" and open societies would prevent or force back totalitarian tendencies); the other would be leaning towards scientific screening and block-building towards the totalitarian competitor, which would require "closed science".

[4] ICES: International Council for the Exploration of the Sea; NEAFC: North East Atlantic Fisheries Comission.

[5] Susan Barr, *Norway – A Consistent Polar Nation? Analysis of an Image seen through the History of the Norwegian Polar Institute*, Oslo 2003, p. 376.

so that it also included Arctic research. Before a SCAR meeting in Oslo in 1970, the idea was presented to the member states, but with limited success. Especially the southern member states resented it.[6]

But Gjelsvik didn't give up. In 1972 he invited what he considered the three most "representative" leaders of polar research in the US, Canada and the Soviet Union to Oslo. The theme was once again prospects for a future multinational scientific cooperation in the Arctic.[7] But the soviet representative, Alexey Threshnikov, leader of the Arctic and Antarctic Research Institute (AARI) 1960–81, said quite openly that although soviet scientific institutions were certainly interested, it was politically impossible. Behind this Soviet political imperative stood the State Committee for Science and Technology (GKNT), Gjelsvik believed; they were real decision-makers in matters like these. The State Committee's political strategy was instead to establish cooperation on Arctic research on a bilateral basis.

What now, then? Norwegian authorities chose to try and establish a bilateral agreement, as the State Committee had suggested –both out of political and scientific considerations. It was obvious, Gjelsvik thought, that both Norwegian and Soviet scientists would benefit from a closer cooperation in for instance geology, oceanography, sea ice research, or biology. A Foreign Ministry memorandum emphasized the following:

> From a political perspective a developed Norwegian/Soviet co-operation in the implementation of concrete research projects must in principle be perceived as positive – provided such projects are genuinely academically/scientifically based.[8]

In fact, many valuable contacts were already established. As early as 1968, for instance, NPI's meteorologist and sea ice researcher Torgny Vinje was invited on a truly pioneering visit to one of the Soviet floating research stations –*North Pole 18*. Vinje, who by the way was at the time also affiliated with a NATO research project on "Military Operations under Arctic Conditions", had a strong scientific motivation for broadened contacts –it was crucial as a means of reaching a comprehensive understanding of how the Barents Sea functioned as a kind of "intermediary" between the Arctic Ocean and surrounding seas.

In 1974, delegations met, they had lengthy discussions, but in the end, no agreement was reached. The reason was solely political: The Soviet side insisted that cooperation should relate only to Svalbard. That was of not acceptable for Norway –both out of scientific and political considerations.

Now ten years passed with no significant changes, but in June 1984 there was a new sign of progress. The Director of Foreign Relations of the powerful State Committee for Science and Technology, Nikolaj Borisov, visited the Polar Institute. He came with an outstretched hand concerning a Polar research agreement that would also provide access to parts of the Soviet Polar Regions.[9] Thus the "Svalbard threshold" had been crossed and it was possible to go further. The Norwegian side proposed starting up with "research topics affecting both countries", such as biological research, oceanography and upper atmosphere research (cosmic physics).[10] On these terms, an agreement was finally reached in 1987.

Seen from a political point of view, the agreement was remarkable. It established, for the first time, a "geographical balance" between Norway and the Soviet Union, making it possible for Norwegians to do research in Soviet waters.

However, there is solid documentation that the geographical working area covered by the agreement was as much the result of *scientific* rationale. Several of the scientific programmes were for instance characterized by the new ecological systems theory that developed in the course of the 1980s, especially the programmes directed towards environmental investigations and monitoring. The agreement that was signed between the Polar Institute and the Murmansk Marine Biological Institute includes the statement that "the Parties […] recognize the fact that Svalbard, the Barents Sea and the Western Soviet Arctic probably form an ecological entity".[11]

Other programmes as well, such as the four-year Soviet-Norwegian Oceanographic Programme (SNOP), led by the above-mentioned Torgny Vinje, were affected by this theme. The main aim of the geophysical programme, which was

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[6] The Regional State Archives in Tromsø (SATO), NPI, box 261, file "1974, X 3.1, Samarbeid med Sovjetunionen", "Vitenskapelig samarbeid i Arktis", 28. November 1974.

[7] Same place. The representatives were J. O. Fletcher (Rand Corporation), E. F. Roots and A. F. Treshnikov.

[8] NP, bortsettingsarkivet (reserved archive), box 338, file "x-062.6 Sovjetunionen (1980–31.12.1986), Bind 1", Notat: Forskningssamarbeid med Sovjetunionen i Svalbardområdet (Memorandum: Research co-operation with the Soviet Union in the Svalbard region), dated 16.02.84, 5. politiske kontor, UD (5th political office, Norwegian Foreign Ministry).

[9] NP, bortsettingsarkivet (reserved archive), box 338, file "x-062.6 Sovjetunionen (1980–31.12.1986), Bind 1", Norwegian-Soviet Polar research co-operation. Report from a trip to Moscow and Leningrad 21-28.3.1985, dated 2 April 1985, signed Odd Rogne.

[10] *Idem*.

[11] NP, bortsettingsarkivet (reserved archive), box 338, file "x-062.6 Sovjetunionen (1.1.1987-92), Bind 2", "Agreement between Murmansk Marine Biological Institute and Norwegian Polar Research Institute on Scientific Cooperation in the Arctic", corrected version 18/2 1991.

established in 1988, was to study the transport of water and ice through the straits from Greenland in the west to Franz Josef Land in the east, but also to model development of the circulation in the Barents Sea.

Why were the efforts to realize the Norwegian-Soviet Polar research cooperation successful during the 1980s? As I mentioned earlier, cooperation was viewed positively from the Norwegian side, even though there were some reservations. What primarily explains how the reservations were overcome, is the changes in the Soviet approach to Arctic cooperation, but also to an increasing desire to overcome long-standing opposition from the Western side. Here, Canada and Norway in particular, had a role to play, it seems.

In 1984, Canada and the Soviet Union agreed to formalize research cooperation in the Arctic, and the Norwegian authorities were kept well informed about this process. It was in the wake of this Canadian-Soviet agreement that the Norwegian-Soviet cooperation also became a reality. At the same time, this closer East-West dialogue also contributed to promoting another idea of old, namely the notion of a permanent scientific cooperative forum linked to the Arctic. Canada and Norway collaborated in this work, which was eventually led by Norway, with the Director of the Polar Institute, Odd Rogne, as one of its assets.

When Mikhail Gorbachev became General Secretary of the Communist Party of the Soviet Union in 1985, contact had already been established with individuals at a high level in the decision-making hierarchy on the Soviet side, and this provided the basis for a steadily improving relationship. This was particularly the case with regard to Nikolay Borisov. When Gorbachev made his famous “Arctic Zone of Peace”-speech in Murmansk in 1987, of which research cooperation in the Arctic was one of the pillars, sources from the NPI indicates that Nikolay Borisov was responsible for this section.[12] And of course behind Borisov’s input there was a Canadian-Norwegian initiative.

On the Western side, the Murmansk speech was greeted as a clear Soviet declaration of intent to develop cooperation with the West. It was also interpreted thus by Director Odd Rogne, who –together with Fred Roots of Canada– was able to lead this process further, to the foundation of the International Arctic Science Committee (IASC) in 1990. The secretariat was based in Oslo, and Odd Rogne became its first General Secretary.

I have no sources, and have seen no analyses, that indicates that the transnational aspirations within Arctic research had the same kind of significance that the cooperation within, say, nuclear physics. But I do think that it mattered –both during the cold war, towards the end of the conflict, and as a very important means of contact in the unstable transition period which followed in the beginning of the 1990s.

[12] NP, bortsettingsarkivet (reserved archive), file “x-066 International Arctic Science Committee (IASC) (1986-1990), Bind 1”, “Rapport fra flere møter i Moskva” (Report of several meetings in Moscow), dated 19 July 1988, signed Odd Rogne.

ATOMIC ENERGY
IN THE PUBLIC SPHERE

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THE DEVELOPMENT OF A PUBLIC IDEA OF ATOMIC ENERGY IN THE FRANCOISM (1945-1964): THE ROLE OF THE OFFICIAL NEWSREEL NO-DO.

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Abstract

The NO-DO, the official newsreel of Francoism, was for twenty five years a monopoly of information in the mass media. One of the most unexplored and yet unravelled aspects relate to the central role that NO-DO had in the broadcasting of scientific news and technical advances. The scientific point of view offered by NO-DO definitely contributed to elaborate a common view in the Spanish public for that period of time.

Particularly relevant to study in deep is the public image spread from Franco's regime regarding atomic energy, an official vision that underwent strategic changes according to Franco's domestic and foreign policies. Over the years, the official vision of "the atomic affair" moved from a terribly dangerous weapon towards the hopes for health and the promise of an unlimited energy resource. The NO-DO launched through nearly two hundreds news during twenty years, and became the media in charge of broadcasting to Spaniards both the fear and the hopes.

Moreover, Francoism, aware of the power of atomic energy, employed it as an instrument representing to the Spaniards the fragile equilibrium in the world of that period, as well as a matter of anti-communist propaganda. The Spanish atomic endeavour was one of the outstanding manifestations of the Spanish-nationalism spirit and importantly, a rationale to justify the political regime to the citizens.

The aim of this presentation is to state some of the style features used in NO-DO to develop a particular public perception of "the atomic affair" close to the regime's one. Indeed we will present data supporting the hypothesis that NO-DO contributed to the efficient spreading the official ideology to the public going to the movies.

Introduction to the newsreels

The role of the newsreel as a source for the study of history and, in particular, the history of science in the 20th century has been re-evaluated in recent years. However, there have been few studies to date devoted to studying the science and technology content included in those audiovisual presentations.

The case of the Spanish newsreel is special in a global context. The official newsreel created by the Franco regime in 1943, the NO-DO, had some characteristics that distinguished it significantly from other newsreels. NO-DO was created with a monopolistic nature, bringing together all of the audiovisual information filmed and screened in Spain. Moreover, its projection was mandatory at all sessions in all Spanish cinemas before the movies in the main programme. This meant the only filmed news the Spanish people saw for 20 years was that screened by NO-DO. Unlike other countries, Spanish TV did not have an extensive presence in Spanish homes until well into the 1960s. This is why the time frame of this presentation only extends to 1964, the plausible date of the end of the newsreel monopoly.

National and international

The science and technology content gathered by NO-DO was abundant and constant throughout the period 1943-1964. Our research has recovered over 2,000 news items relating to this topic, covering very different content. However, three themes had a specific role in NO-DO newsreels: aeronautics, aerospace technology (the conquest of space) and technology related to atomic energy. These issues are distinguished from others because they were **permanently** included in the newsreel schedules over this period. This makes NO-DO an invaluable long-distance narrator of the advances made in these three areas of science and technology. In this way, NO-DO built a sequentially filmed history of science in the second half of the twentieth century. This is the primary value of the newsreel.

We will focus our attention on technologies related to atomic energy.

Description of the news found in the file

From 1945 to 1964, 145 news stories related to atomic energy have been found in the NO-DO Historical Archive.[1] The geographical distribution of these news items is shown in the following graph, in which it can be seen that the United States provided most of the news (61%). Only 19 news items (12.75%) are of Spanish origin.

New discoveries in physics, new applications of atomic energy, the development of the cold war and political interests on their use determined the contents in the newsreels. These contents can be classified into:

- atom bomb explosions.
- nuclear armament.
- application of isotopes in medicine.
- the use of atomic energy in transport with special emphasis on nuclear submarines .
- popular science through exhibitions about nuclear energy .
- safety in the treatment of radioactive elements and waste.
- nuclear power plants for the production of electrical energy.
- passive defence and war.
- basic scientific research.
- political issues.

This presentation will focus only on the atomic explosions and their representation in the news. We have chosen three paradigmatic events: the bombing of Japan, the Bikini Atoll test and the news of the first Soviet atomic test.

[1] From now on we'll use the convention for references to the news: "NO-DO number of issue- Edition of issue (A,B or C), TITLE OF THE SECTION WHERE THE NEWS WAS INCLUDED, *Title of the news*, Date of the issue opening."
For example "NO-DO 143-B, UNITED STATES, *Research on the developing of the atomic bomb. The New Mexico test experiences,* 09/24/1945", makes reference to the news opened on September 24th, 1945 in the issue numbered 143, edition B down the section "United States" and under the title "Research on the developing of the atomic bomb. The New Mexico test experiences".

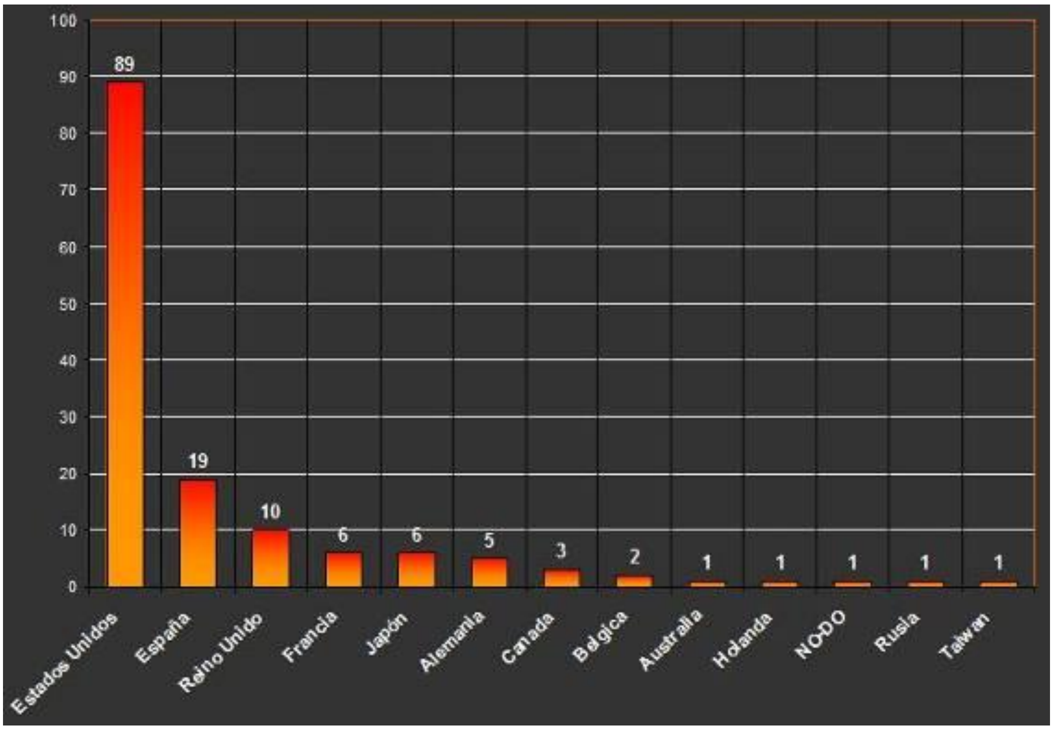

Fig. 1: News about atomic energy by country (1945-1964).

Atomic explosions

It is very important to point out that although the news items covered were put together and distributed by NO-DO, the Spanish news agency was a mere recipient of this material. It had no control over the material received, which was administered by the USA tailored to its own needs and objectives. The secretive and sensitive nature of the material concerning atomic explosions, along with the NO-DO editorial team's inexperience in these specialised subjects must be taken into account if we are to properly understand the role of NO-DO.

The identification of atomic energy with destruction and nuclear bombs was the common thread of the information included in the newsreels until the early years of the 1950s. These explosions were the events that most contributed to the forming of "*atomic*" in the public's mind. The representation of nuclear energy in the newsreels later evolved to show the non-military applications of atomic energy, although the detonation of atomic bombs never totally disappeared from NO-DO programs.

The news

The destruction of the Japanese cities of Hiroshima and Nagasaki were the first news items that made reference to an atomic explosion. The series of reports on the Japanese devastation and the initial atomic experience screened in Spain was divided into three stories in 1945, with a very different treatment of the news.

I. The atom ends a war.

The first of this news[2] summarized the military conflict between the Allies and Japan and the final surrender. Very brief images of the atomic explosions over Hiroshima and Nagasaki were inserted into a montage of images from the Pacific War. The text made no mention of the atomic bomb, or the number of deaths or damage caused.

II. Atomic energy. A matter of Science.

The second piece of news that the Spanish saw about atomic energy dealt exclusively with the research prior to the detonation of the bombs in Japan.[3] The creation of the first atomic bomb was represented as the product of a long and

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[2] NO-DO 142-A, VICTORY OVER JAPAN, *[…] The atomic bomb over Japan […]*, 09/17/1945.

[3] NO-DO 143-B, UNITED STATES, *Research on the developing of the atomic bomb. The New Mexico test experience*, 09/24/1945.

difficult scientific project. The report had a clear didactic purpose and explained the physical origin of this energy using simple animations. It also offered a brief summary of different moments during the research, the laboratories and the leading scientific figures.

The atomic bomb was presented to the audience for the first time as "*the most wonderful creation of science*."[4].

Its power was exaggerated, as was customary for NO-DO, declaring that: "*it is the energy generated by the heat of the sun and moves the Solar System*".

This news is very important historically, as it showed images of the Trinity detonation at Alamogordo on the night of July 16, 1945 for the first time.

The explosions at Hiroshima and Nagasaki were never once mentioned.

Therefore, this film document should be considered as a purely scientific news item.

Surprisingly, even at this time the possibility of using the 'atom' as a source of energy was already being mooted.

> "This heralds a new era in the use of energy that will cause a genuine revolution, NOT ONLY in terms of destruction, but also in many affirmative and constructive aspects, as well as the use of a new and gigantic driving force".

III. At last the bomb is deadly.

The third item of news from 1945 was the one that dealt with the bombings of Japan in detail. It's a very long report of nearly 3 minutes, published under the title "Japan ravaged".[5]

Four elements are highlighted in how the news is dealt with:

(I) The dead and wounded do not appear in the images. Only material destruction is shown, in which passing survivors are seen.

(II) There is no data on injuries or on the type of injuries or the future consequences radioactivity would have. Nor is there any data on the radius of the explosion, the level of destruction or its explosive power.

(III) It compares the destruction of Hiroshima and Nagasaki with the destruction of other cities as a result of conventional incendiary bombs. Through the images it is not possible to distinguish the different effects between the two types of technologies. When comparing images from Kura or Tokyo with Hiroshima, there is no way of distinguishing them.

(IV) The report states that "*these figures prove that Japan had been totally defeated before the atomic bombs were dropped*", thus implying that the atom bombs detonations on Japan were superfluous to their surrender.

It can therefore be said that the news defined the capacity for destruction but not the deadliness of atomic weapons. There is no blame or responsibility and science has completely disappeared from the discourse. The specific terminology is still to be established (kiloton or radioactivity are not even mentioned) and the consequences of the detonations are summarized in one word: "devastation".

IV. The weapon becomes a spectacle.

The fourth story is from 1946, in which the news covered the fifth atomic explosion detonated at Bikini Atoll (Marshall Islands, Pacific) in July 1946. The blast was covered by two NO-DO news reports: the first focusing on the preparations for the explosion[6] and the second on the blast itself.[7]

The first of these began with feedback from the Trinity explosion, from which the audience was transported to the preparations for the anticipated explosion at Bikini Atoll.

The three nuclear explosions prior to Bikini were a military and scientific secret (Trinity, Alamogordo, 1945) and two military operations (Little Boy and Fat Man, Japan, 1945) which were consequently narrated retrospectively. In contrast, the

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[4] The intention of the newsreel becomes clear on examining the text accompanying the story in the Film Archive and the final sentence of the news item. In one it is called "the most extraordinary creation in science" and finally changed to the "most wonderful creation of science." According to the RAE: super, da. (Del lat. Wonderful) .1. adj. astonishing, amazing, stunning special, ria. (Del lat. Extraordinarius) .1. adj. Out of order or natural or common law.

[5] NO-DO 151-B, JAPAN RAVAGED, *Hiroshima and Nagasaky after the atomic bombs blast. Other efects of the air bombing over Japanese territory. Osaka, Kura and Tokyo*,11/19/1945.

[6] NO-DO 180-B, ATOMIC ENERGY, *Setting up the Marshall Islands experiment. War stuff and ships will be tested by the blast*, 06/17/1946.

[7] NO-DO 191-B, THE ATOMIC BOMB, *Sensational report at Bikini. The moment of the blast of and its results*, 09/02/1946.

explosion at Marshall Islands, although an event with very precise scientific and military objectives, was carefully planned to be broadcasted throughout the world. This, therefore, represents a turning point in the public handling of nuclear explosions.

The event, such as it was depicted by NO-DO, was a staging of the United States' atomic power in the purest of cinematographic styles. The world would attend a unique scientific experience, the first underwater nuclear explosion, with front row seats for what was probably the first public broadcast of a scientific experiment. Far from being a military, political and scientific secret, inherent in previous explosions, the entire world was invited to this one. In an elaborate propaganda manoeuvre, the US saw to it that the audience received the best, the most compelling, the most aesthetic and the most stimulating images available about the power of the atom.

However, this public experience had to be innocuous. It was a scenario free of collateral damage in which the immense power of atomic explosions could be contemplated unequivocally. No one would be shown in a bad light; there would be no wounded or dead, and no destroyed cities. Only the nature of an unknown Atoll hidden somewhere in the Pacific would be conveniently evicted by the power of the atom.

The showing of the preparations was an essential condition in the US programme. Hence, the first news report of the explosion was as important as the second report. It was shown how the population was evacuated, how king Judah of the Bikini inhabitants was informed about the bloodless explosion, and how the transfer of residents, in agreement with the decision of their king, was carried out. It was a premise that they knew that nothing but nature would be destroyed.

Yet, at the same time, it was essential that this manifestation of power was understood in all its magnitude. The numerous large vessels the US navy had in the blast area were used as a scale so that the images would let the power of the detonation be understood to everyone. It was a politically and militarily clean and safe experiment, but ambitious in terms of propaganda.

And it was a success. The images of the explosion are still used today as an expression of the power locked inside the atom. Those images became icons of the twentieth century. More so than those of the devastation of Japan.

It was an almost perfect propaganda manoeuvre. The dead forgotten, the enormity of the Bikini explosion remembered, a once anonymous place on earth eternally famous since the summer of 1946.

V. And the Soviets have the bomb.

After the Bikini Atoll explosion, no more news about atomic explosions was screened by NO-DO. In late 1949 a major event would change the strategic panorama of the world: the announcement of the first atomic explosion by the Soviets.

The coverage given to the global recognition of the first atomic detonation by the USSR was narrated in NO-DO with a brief news item put together with an evidently propagandist slant.[8]

Without the use of any images of the Soviet detonation, the front pages of newspapers served to create alarm among the population. Shots of the Soviet representative, Andrei Vichinsky, entering the UN completed the story. Despite the fact that this event marked the de facto beginning of the Cold War, nothing was explained to the Spanish people except: *"A scowling [Andrei Vichinski] gives an example of Soviet manners."*

The brevity of the information about the Soviet bomb is in our opinion a deliberate omission, as the newspapers filled pages with it. The report was supplemented, without interruption, with interventions at the UN by representatives of the US, Canada and the UK, urging the need for arms control and the control of nuclear material by the United Nations. Quite a show of political irony, since, until that moment, only these three countries, with different technical levels, had a say in the UN Security Council over the control of atomic energy, over which they kept a strict and absolute monopoly.

Whereas, the text of the newsreel clearly set out the future of the world: *"…doesn't wonder what happens now", a* sentence omitted by NO-DO in the Spanish version.

Finally, the security measures taken regarding the control of atomic secrets is maximised and the newsreel shows extreme dramatic footage of the various measures taken by the US to ensure complete control over their facilities. It is rather paradoxical that when the Soviets possessed the necessary technology to manufacture atomic weapons, the United States redoubled their security measures. Would it not have been wiser to have done so beforehand?

At least one mystery was cleared up: the anonymous enemy referred to in previous newsreels at last had a name: The Soviet Union.

VI. What's next?

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[8] NO-DO 354-B, REFLECTIONS FROM THE WORLD, *The United Nations receives the news of the Soviets' first atomic blast. A report about this important international moment*, 10/17/1949.

The future of nuclear testing was glorious. It continued until it reached 205 detonations in 1963, when France joined in.

NO-DO made echo of many of these by showing a wide range of experiments and a variety of events related to nuclear explosions: explosions underground, at ground level, in military manoeuvres with models of houses and people to determine their effect, experiments with the US and British hydrogen bomb, atomic artillery, guided missiles with nuclear warheads…

Far from keeping things secret, the newsreel strove to publicize each and every one of the achievements in nuclear weapon material produced.

All this with a constantly repeated assertion: nuclear power is the guarantor of armed peace against an unidentified enemy. No detail or facts about enemy activity were ever offered by the newsreel. Their power and eagerness to launch a destructive mechanism, which curiously was not created by them, but by the nations converted into the guardians of world peace, is taken for granted.

References

CEBRIÁN HERREROS, Mariano (1994) *Cine documental e informativo de empresa: 50 años de producción de F. López Heptener en Iberduero*, Madrid, Síntesis D.L.

HERNÁNDEZ ROBLEDO, Miguel Ángel (2003) *Estado e información el NO-DO al servicio del Estado Unitario (1943-1945)*, Salamanca, Publicaciones Universidad Pontificia de Salamanca.

MONTERO DÍAZ, Julio y CABEZA, José (eds.),(2005) *Por el precio de una entrada*, Madrid, Ediciones Rialp S.A.

MONTERO DÍAZ, Julio & RODRÍGUEZ MATEOS, Araceli (dirs.),(2005) *El cine cambia la historia*, Madrid, Ediciones Rialp S.A.

ORDOÑEZ, Javier & RAMÍREZ, Felipe E. (2008) "Los públicos de la ciencia española: un estudio del NO-DO" en ROMERO, Ana y SANTESMASES, Mª Jesús (coords.), *Cien años de política científica en España*, Bilbao, Fundación BBVA, pp. 257-292.

PAZ REBOLLO, Mª Antonia & MONTERO DÍAZ, Julio (coords.),(2002) *Historia y cine. Realidad ficción y propaganda*, Madrid, Editorial Complutense.

PAZ REBOLLO, Mª Antonia & SÁNCHEZ Inmaculada (1999) "La historia filmada: los noticiarios cinematográficos como fuente histórica. Una propuesta metodológica", *Film Historia*, Vol. X, nº1, pp. 17- 33.

PRESAS I PUIG, Albert (2005) "Science on the periphery. The Spanish reception of nuclear energy: an attemp at modernity?", *Minerva* 43, pp. 197-218, Springer.

RAMÍREZ, Felipe E. (2006) "Ciencia, tecnología y propaganda. El NO-DO un instrumento de popularización de la ciencia al servicio del estado (1943-1957)" en ORDOÑEZ, Javier (dir.) & RAMÍREZ, Felipe E. (coord.), *El pensamiento científico en la sociedad actual*, Madrid, Ministerio de Educación y Ciencia, pp. 77-108.

RODRIGUEZ MATEOS, ARACELI (2008) *Un franquismo de cine. La imagen política del Régimen en el noticiario No-Do (1943-1959)*, Madrid, Ediciones Rialp S.A.

SÁNCHEZ-BIOSCA, Vicente & TRANCHE R., Rafael (1993) "NO-DO: El tiempo y la memoria", *Cuadernos de la Filmoteca*, Madrid, Filmoteca Española, Instituto de Cinematografía y de las Artes Visuales.

SÁNCHEZ-BIOSCA, Vicente & TRANCHE R., Rafael (2002) *NO-DO El Tiempo y la Memoria*, Madrid, Cátedra/ Filmoteca Española.

TRANCHE R., Rafael & SÁNCHEZ-BIOSCA, Vicente (1997) "Los años 50 en NO-DO, de la autarquía al desarrollismo" en YRAOLA, Aitor (comp.), *Historia contemporánea de España y Cine*, Madrid, Ediciones de la Universidad Autónoma de Madrid, pp. 115-124.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 886.

PUBLIC OPINION STRIKES BACK: THE ITALIAN REFERENDUM ON NUCLEAR ENERGY

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Abstract

In its very beginning, nuclear power in Italy was well accepted by a bipartisan group of supporters among the political parties. Working on the historical archives of ENEL, the research reconstructs the shifting in the public opinion toward an antinuclear attitude, a shifting that brings the Italian state to reject all the nuclear plants on its territory. This happened through, the only case in the world, a popular referendum.
The paper investigates on the historical context for such public opinion stance, moving from the Italian Communist Party congress, in which an anti-nuclear motion was presented and got almost 50%. Two weeks after the congress, the Chernobyl accident happened. It raised a deep impression and a wide worry for the behaviour of the "Chernobyl cloud", and the public debate and polemic revived. Local and national demonstrations (Rome, 10 May) proliferated. The signature gathering for a national referendum began in July. In October, after a huge demonstration at Montalto di Castro, the Craxi government decided the stop to the yard, and called for a big Conference on Energy, which was held in February 1987 without any important result. The execution of the referendum, on 8-9 November 1987 stems from this background. On one side, the growing proposal of an Italian nuclear complex, and on the other side the bipartisan public opinion and political representatives opposing to the power plant. It is well known that in the referendum almost 80% of the votes were against nuclear. The legitimate question is: did this result univocally manifest the will of shutting down every nuclear power activity?

At the very beginning of the nuclear power era, Italy undertook a rapid development of nuclear programs, with the construction after the end of the Fifties of three nuclear power plants, entering into operation as early as 1964-65. By doing so, Italy was among the top nuclear countries in the world. Actually such a quick start did not reflect a coherent program that the Italian government was supposed to endorse.

It must be recalled that Italian scientific progress in the post-war period was damaged by both fascism and war destruction. Nevertheless, Italian progress in nuclear physics (the seminal experiment by Conversi-Pancini-Piccioni), after the fall of fascism and the end of the war, involved a development, by Edoardo Amaldi, of the fundamental work for reconstructing Italian physics through collaboration with the US research centres.

As early as 1946, a group of young physicists led a group of private industries to establish in Milan a research centre named *Centro Informazioni Studi Esperienze* (CISE, Centre Information, Studies, Experiences), with the aim of developing applied nuclear research and designing an Italian power reactor. The participation of Italy in NATO in 1949 opened the way to collaboration with the United States in this field.

In spite of the indifference of the Italian political community towards nuclear energy, in 1952 a State committee was established within the National Research Council (CNR), the *Comitato Nazionale per le Ricerche Nucleari* (CNRN, National Committee for Nuclear Researches), with the aim of promoting research, collaboration and applications of nuclear energy. However, the Committee was created without a specific law, it had no juridical state, and could not administer funding. Private industry strongly opposed a nuclear regulation, which arrived on the scene only in 1962. Actually, in 1952 CISE (who had initially opposed the birth of CNRN) proposed to CNRN's President, Francesco Giordani, a project for the construction of an Italian 1,000 kW natural uranium, heavy water, reactor, but this was not given serious consideration. Only later did Giordani advance the necessity for a higher power reactor, and CISE began the work for the project CiReNe (*CISE Reattore a Nebbia*, CISE "fog type" reactor). Moreover, Giordani initially opposed CISE's requirement for financing as it was a private institution, and when this aspect was resolved the funds were in any case insufficient.

In 1955, in the context of the Atoms for Peace campaign, the CNRN sent a commission to the US, which contracted the purchase of a 1,000 kW, CP-5 type reactor fuelled with 20% enriched uranium, which caused a conflict with the President of the private electrical society, Edison.

In subsequent years nuclear energy entered the Italian political debate, and raised growing interests in the energy industry. These developments were biased by the usual clash between private and public interests and programs. The events concerning CNRN revealed great relevance for the consequences that were to follow in later years (during the 1960s). In 1956 Giordani resigned, while Felice Ippolito assumed the role of General Chairman.

While the Euratom and the IAEA were established, in 1957, the debate on nuclear power also continued to grow in Italy. In the same year a bilateral agreement with the United States was signed, but its take-off was slowed by the lack of nuclear legislation in Italy, merging into the debate about the nationalisation of electricity.

In this period, the CP-5 reactor was purchased by CNRN: it entered into use in 1959, with a power of 5,000 kW and the name Ispra-1, at the newly established centre in Ispra. The centre of Ispra represented the Italian commitment to European efforts in developing nuclear energy on a supranational level. But this is another story.

Between 1956 and 1958 the contracts were signed for the purchase of three power nuclear plants: they were in fact three completely different reactors, but they led Italy in 1964 to the upper levels of electronuclear production in the world (the third one, following the US and the UK) In 1957 there were not yet any civil nuclear plants operating in the world (apart from Calder Hall in the UK), and the choice between different models was objectively very complex: it would be enough to recall that the choice of an enriched uranium reactor would have determined a strict dependence on the process of enriching-uranium.

In 1957 a transaction of the World Bank, BIRS, together with the Banca d'Italia, led to the establishment of the project ENSI (*Energia Nucleare Sud Italia*), which led to the purchase of a US reactor. The final choice (tutored by American, French, and British experts) was a 150 MW, BWR light water reactor from General Electric, which was built at Garigliano, Caserta, near Naples. It entered into operation in January 1964, but it was closed permanently following an accident fourteen years later.

In the meantime, as previously pointed out, at the end of 1955 the President of the private electrical utility Edison, Giorgio Valerio, had visited the US, looking for a contract for the purchase of a nuclear plant. This contract was delayed and, finally guaranteed by the American Export-Import Bank, and was signed only in 1957 with Westinghouse, for a 242 MW reactor. It entered into operation in October 1964 at Trino Vercellese, near Turin, Pedemont.

The third project was realized by public utility *Ente Nazionale Idrocarburi*, (ENI, National State-Company Hydro carbides), whose President, Enrico Mattei, had in 1956, created a specific entity (*AGIP-Nucleare*). Mattei's choice is important since, on the same line of gaining autonomy from the US enrichment industry, he chose a British "Magnox" reactor, fuelled with natural uranium and gas refrigerated. The plant was built in Latium near Latina; its total cost was 49 billion Italian Lire and in fact it was the first one to enter into operation, in May 1963.

Still lacking a comprehensive Nuclear Law, Italy passed a preliminary act regulating only some aspects of this matter in 1960. This Act was substantially limited to the attribution of a juridical status to CNRN, changing its name into CNEN (*Comitato Nazionale per l'Energia Nucleare*) and attributing it new tasks and responsibilities, financed with 80 billion Lire for the first five-year plan. CNEN had the responsibility for control and technical surveillance over all the nuclear plants, both in the construction and management stages, and of the implementation of tests. Ippolito was appointed Secretary General of CNEN, while its President was formally the Ministry of Industry.

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The first centre-left government nationalized electric power, seizing its production from private companies: it was a turning point in the history of Italian energy production. It was instituted as *Ente Nazionale Energia Elettrica* (ENEL, National State-Company for Electric Power), a state owned utility for electric power. The parliament eventually approved the Nuclear Law in December 1962, which established that the production of nuclear energy is a prerogative of the State, or of societies with prevailing state participation. The second five-year plan had already been approved in October 1962, providing for the installation until 1970 of 1,000-1,500 MW with 2-4 nuclear plants.

Some months later, Ippolito was put on trial for administrative crimes. The affair split public opinion: many scientists (Edoardo Amaldi among them) defended Ippolito, while Giuseppe Saragat and his social-democratic party were accusative towards the maybe over-assertive CNEN leader. Ippolito was eventually condemned.

CNEN, in the eight years following Ippolito's trial, lost its *raison d'être*. ENEL took away all its prerogatives, and with the nationalization of electric energy it inherited from the private companies the three nuclear plants along with their technical staff. Even inside ENEL, besides in the country itself, there was an influential "oil party": but there was also a strong "nuclear party", reinforced by the Communist Party (PCI) and the Unions.

During the Seventies and the Eighties a series of National Energy Programs was proposed by different Governments, providing new programs for the construction of a large number of nuclear plants, while strong popular opposition and protests were growing in the country against these programs. Even a 30% participation by ENEL in the French fast-breeding reactor program started, with the national project of building a pilot reactor for testing the fuel bars (PEC, *Prova Elementi di Combustibile*), which had remained unfinished not only when the Italian nuclear programs were terminated, but would have been in great delay even with respect to the progress of the French program.

On the other hand, in any case, the second five-year plan had become outdated, since the models for nuclear plants were still in a process of being defined. The PCI direction engaged Ippolito's position, but it had no coherent program toward nuclear issues, as was the case with the majority of the political class. So the Italian Left were supporters of nuclear energy, and public opinion was generally well oriented toward a "Nuclear Italy".

When the idea of new nuclear plants returned, around the end of the decade, ENEL called for tenders, getting six offers, from American, British, and other European utilities. Finally an 840 MW BWR reactor form General Electric was chosen, which in fact was a somewhat hybrid model, a transitional model between small and big power plants. The construction of a new plant began at Caorso (Northern Italy) in 1970, led by the consortium ENEL-Ansaldo Nucleare-GETSCO: the works should have been finished by 1975, but suffered delays and increased costs; tests began in 1978, and the connection to the electrical grid was achieved in 1981.

During the Seventies and the Eighties a sequence of National Energy Programs were proposed by the different Governments, providing new programs for the construction of large numbers of nuclear plants, while strong popular oppositions and protests were growing in the country against these programs.

Although a law for the rearrangement of CNEN had been approved in 1971, its new role was delayed, so that it was ENEL that commissioned the Caorso plant, and in 1973-74 proposed four nuclear plants to be located in Central Italy, two in Latium (it would subsequently have been Montalto di Castro, see later) and two in Molise.

In 1975 CNEN submitted, and *Comitato Interministeriale per la Programmazione Economica* (CIPE, Interministry Committee for Economic Program) approved, a "*Piano Energetico Nazionale*" (PEN, National Energetic Plan), foreseeing different future scenarios of energy demand in Italy, and in respect of electric energy the installation in the period 1983-85 of a nuclear power of 13,000-19,000 MW, and further plants providing for a total nuclear power in 1990 of between 46,100 and 62,100 MW.

Possible locations had been preliminarily studied by ENEL, and were cemented by swiftly approved laws. Actually, these laws were based on the recently published American "Reactor Safety Study" (known as the "Rasmussen Report"), which suggested a safety zone around nuclear plants of a 16 km radius, successively confirmed by the Nuclear Regulatory Commission in 1978. In any case, the previsions of the PEN were recognized as being exaggerated.

In the meantime, ENEL got a 25% participation in the French gas diffusion enrichment plant Eurodif. Moreover, in 1976 a dubious project was elaborated of a second enrichment plant to be built in Italy, Coredif, fuelled by four nuclear plants of 1,000 MW each. (Four times the total installed nuclear power, including the not yet operative Caorso plant). Considering that France, for its military nuclear program, had a great need of plutonium and highly enriched uranium, nothing similar could be said to be necessary for Italy.

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Also in 1976, a study of the environmental impact was presented in respect to the location of the aforementioned 2,000 MW nuclear plant at Montalto di Castro, which would lead to the authorization for its construction in 1979. In 1977 Donat Cattin, Ministry of Industry in the 3rd Andreotti cabinet, issued an ultimatum to the Regions in order to indicate potential sites for the construction of 20 nuclear plants. So the State gave the word to local public opinion, located close to possible nuclear sites, and this permitted a shifting of attitude in the public opinion as a whole.

Actually, in the meantime strong protests were growing, from the local populations, committees, environmentalist associations, some minority political forces, and even local administrations. If not the majority of the whole population living near the nuclear sites, but in any case a very visible part of that population took part in big demonstrations at Montalto di Castro, Viadana, Suzzara, San Benedetto Po (in Lombardy, when the location of nuclear plants was proposed there); the associations WWF and "Italia Nostra" also produced documents and organized meetings. The Lombardy Region appointed a Commission of study on nuclear plants, and requested advice from the *Istituto Superiore di Sanità*. A bipartisan front arose, based at the local level of the populations living near some plant sites.

However, the majority of the political forces and Unions were strongly in favour of nuclear energy, including the majority of the Communist Party and the left-wing Union CGIL. In any case, in response to these movements the political debate grew: the Commission of Industry of the Parliament held a fact-finding inquiry, and there was a Parliamentary debate.

However, a second PEN was approved by CIPE in December 1977, providing for the immediate construction of 12-13 nuclear plants, leaving the remaining 8 for after 1985. In response to this, the popular protests and demonstrations continued to grow. Even more so when Romano Prodi, Industry Minister in the 4th Andreotti cabinet, authorized on 19th February 1979 the construction of the plant in Montalto di Castro: just before the Three Mile Island accident, on 28th March 1979. During the same days the movie *The China Syndrome*, with Jane Fonda, came out. In the meanwhile, in August 1978, the Garigliano plant had been shut down after several accidents.

In the USA, following the Three Mile Island accident, two commissions were appointed (headed respectively by Keenly and Regain), which invited nuclear utilities to radically change their safety regulations, and proposed to authorize nuclear plants far from residential areas, to provide emergency plans approved by a Federal agency for safety, and to provide for the evacuation of population in case of accidents to a radius of 30-40 km.

In Italy, on the institutional side, in June 1979 the results of a fact-finding special ecological commission from the Senate gathered a majority of favourable opinions, with the exception of the ecological associations WWF and "Italia Nostra". In December the new Minister of Industry, Antonio Bisaglia, appointed a Committee on nuclear safety, which approved a document with the relevant opposition, and a minority report, from the three environmentalist representatives, denouncing the deficiency of the Italian safety rules with respect to the international ones. The PEN was successively revised in 1980 and 1981, providing for the construction of nuclear plants of at least 6,000 MW (indicating potential sites in the Regions of Piedmont, Lombardy, Veneto, Tuscany, Campania, Puglia, and Sicily), with a *Nuclear Unified Plan* (PUN) based on the PWR Westinghouse reactor (note the contradiction with the previous choice of the BWR Caorso plant from General Electric). Also note that in these same years Italy had to reduce from 25 to 16,5% its participation in the Eurodif enrichment plant, and was obliged to undersell a part of the enriched uranium it had already acquired, following a down-sizing of its nuclear ambitions.

In the meantime, in 1982 CNEN acquired the new name ENEA (*Ente Nazionale per l'Energia Nucleare e le Energie Alternative*), with a few changes, but with a new research section on renewable energies: a preferred choice, since the new 1985 PEN confirmed 12,000 MW of nuclear energy.

In the years 1981-1983 the opposition against nuclear energy grew further. Several municipalities expressed their opposition. A law in 1983 provided for economic incentives to those municipalities which had accepted nuclear and thermoelectric plants on their territory (besides nuclear power, also coal fuelled plants were pushed by the various PENs).

ENEA expressed its positive opinion for the suitability of the sites of Viadana and San Benedetto Po, and ENEL begun the geological tests. Anti-nuclear demonstrations, violent clashes with the police, and arrests followed. Two municipal popular referendums were held in Viadana (1984) and in San Benedetto Po (1985) and opposition to the nuclear plants won out in both cases. In 1985 there was a big demonstration in Rome.

It must be recalled that the anti-nuclear movement was reinforced by the "Euromissiles" crisis (the "Atomic Clock" of the Bulletin of the Atomic Scientists was put at barely 3 minutes from Midnight. The debate on a "nuclear winter" grew, and the movie *The Day After* made a strong impression on the public), along with opposition to the deployment of Cruise missiles in Comiso, Sicily.

* * *

And so we arrive at the penultimate act, only 36 days before the Chernobyl accident. On 20 March 1986 the CIPE approved the 4th PEN, providing only for the construction of the 2,000 MW plant at Montalto di Castro, plus 2,000 MW more at Trino Vercellese, in Piedmont (never begun), and the localization until 1986 of two more 2,000 MW plants each, respectively in Lombardy and Puglia; in addition it provided the acquisition of 400 MW from the 1.2000 MW fast reactor *Superphénix* under construction in France, an ill-fated project.

In April 1986 the PCI held its XVII congress, in which an anti-nuclear motion was presented and attracted many votes. Two weeks later, on 26 April 1986 the Chernobyl accident happened. It made a deep impression amidst great concerns for the behaviour of the "Chernobyl cloud", and the public debate and polemic was thus revived. Local and national demonstrations (Rome, 10 May) proliferated. In July the signatures gathering for a national referendum began. In October, after a huge demonstration at Montalto di Castro, Bettino Craxi's (Italian Socialist Party) cabinet decided to halt the process, and called for a major Conference on Energy, which was held in February 1987, without any significant result.

The execution of the referendum, on 8-9 November 1987, was the prologue to the termination of Italian nuclear power. It is well known that in the referendum almost 80% of the votes were against a nuclear program. The legitimate question is: did this result univocally impose the closure of every activity related to nuclear power? The popular will in this sense was quite evident (though the Chernobyl accident undoubtedly played a role). The problem is that Italian law formally allows only for "abolitionist" referendums, concerning specific existing laws or regulations. So that the referendum abrogated: (1) the prerogative of CIPE to decide on the location of nuclear plants, when the interested municipalities were not able to decide; (2) the compensation available for municipalities which hosted nuclear or coal plants; and (3) the possibility for ENEL to participate in international nuclear programs (in this case, *Superphénix*).

At this point the last act intervened. In the aftermath of the referendum the Government of Giovanni Goria (the first Christian-Democratic) suspended the project of the Trino plant, cancelled the Latina plant, and started verifications on the safety of the Caorso plant and the feasibility of the Montalto di Castro plant under construction (for the non-nuclear parts).

In subsequent years all the Italian nuclear plants were closed (they still await decommissioning, and they will no doubt wait a very long time, whilst the majority of the fuel elements are still in the deactivation pools, often under precarious conditions). Furthermore, almost every activity in the field of nuclear energy has been terminated, specialists and agencies having been transferred to other fields.

GENDER STANDARDS IN DRUGS HISTORY: CROSSING BOUNDARIES

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HORMONAL CONTRACEPTION, GENDER AND SOCIETY IN SPAIN (1966-1979)

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Abstract

Social studies focusing on women's health, women's history and/or medicine for women are not always elaborated using gender as a category of analysis. In the last twenty years in Spain, many scholars have made important contributions on contraception focusing on demographic, political or ethical aspects, but few of them have discussed their results from a gender perspective. These studies usually do not include woman's medical, scientific, social and religious definitions; the role of women as doctors, health administrators, scientific researchers, and/or health activists; the contribution of women's and social movements to the improvement of women's life conditions; the relations between sanitary and women's movements, the construction of collective gender identities, the experience of real women in accessing and using oral contraceptives and so on.
In Spain, during the last years of the Franco dictatorship and the democratic transition (1970-1982) there was a widespread movement towards the legalisation of contraception, prohibited since 1941. In this paper we want to look at the discourses surrounding the legalization of the pill as represented in the press, and at the role played by the feminist movement and the medical professionals in this debate. For this purpose we analyse, on the one hand, the information on the hormonal contraceptives that was published in the Spanish general press, feminine magazines and different kinds of feminists' publications during the 1970s, as well as in the main gynaecology handbooks used in Spanish faculties of medicine during this period. On the other hand, we will use oral interviews with health professionals and feminist activists.

Introduction: intersectional approach in the historiography of oral contraceptives

Numerous works on the history of oral contraceptives have been published during the last two decades. These studies include a spectrum of topics ranging from, among others, the first contraceptive pill, contraceptive implants, to injections and emergency contraceptives. Many of these works apply intersectionality as a tool for in-depth analysis of the relationship between categories such as gender, class and ethnicity in the processes of the design, production and commercialization of contraceptive drugs. Gender as a category of analysis has also been adopted by some contemporary historians and sociologists interested in psychotropic drugs (Metzl 2003; Tone 2007; Romo Avilés, Gil García 2006).

An intersectional perspective is considered to be one of the most important theoretic contributions of women's studies (McCall 2005, 1771). For the social history of drugs, intersectionality has become a useful concept to analyze with more precision and complexity the process of circulation of drugs in its social and scientific context. Intersectionality also helps situate the analysis in the cultural, economic and ideological diversity of women who produce, publicize, consume, or reject drugs, thus, providing the medication with a new symbolic content which goes beyond its pharmacological properties.

An intersectional perspective makes it possible to study, following the proposal made at the end of the 1980s by the historian Linda Gordon, how different collectives and people can simultaneously be agents and victims, and discriminate and be discriminated against (Gordon 1988). Intersectionality also makes it possible to analyze the role of pharmaceutical technologies within the context of gender, race, ethnicity and class hierarchies. Lara Marks and Elisabeth S. Watkins, the first scholars who published on the history of the contraceptive pill from a gender perspective (Marks, 2001b; Watkins 1998), have been followed during the last two decades by authors who incorporate categories of race, ethnicity and class into their studies of the processes of producing and testing drugs. These works focus on different responses to contraceptive and reproductive technologies among women of different races, ethnicities and classes, and the possible uses of these technologies by state institutions and agencies providing family planning, as well as the benefits and social problems these practices generate (Grant 1993; Ross 1998; Roberts 1997; Rodrique 1998; Nelson 2003; Solinger 2005; Schoen 2005). They also study issues such as abortion, sterilization, oral contraception and single parenthood from the particular perspective of African American women and analyze the practices of resistance used against the institutional attempts to impose certain contraceptive methods on women of colour, such as sterilization or hormonal implant. These works are limited to US or British contexts, where there has been a more articulated academic interest in the ways in which categories of gender, race and class have closely interacted during the past two centuries. Other authors focus on Puerto Rican women and discuss, among other topics, their participation in the late 1950s in large scale clinical trials of Enovid, the first oral contraceptive commercialized in the US (Briggs 2002; López 2008).

Using gender, race, ethnicity and class as categories of analysis does not necessarily guarantee avoiding victimization. Some recently published works, especially those by Roberts (1997), Briggs (2002) or López (2008) focus more on women as subjects of racist and sexist discrimination rather than on examples of their agency. Furthermore, these authors do not give sufficient space to the practices of using different contraceptive technologies as possible strategies of resistance and empowerment.

The pill in the Spanish press: aims and methodology

In 1960, when the first contraceptive pill was commercialised in the United States, all contraceptive methods were prohibited in Spain. From January 1941, six months after the victory of Franco's army over the republican state, until 1978 there existed a law for "the protection of natality, against abortion and contraceptionist propaganda" (Federació Local de Sindicats de Barcelona 2008), which prohibited the sale and publicity of any contraceptive methods.

Nevertheless, the pill started to circulate in Spain in 1964. A year later, a new law would broaden the variety of drugs that could be used by doctors, including hormonal drugs. Prescribing the pill became possible but was limited to cases of gynaecological disorders (Ministerio de Gobernación 1965; Jones 1977). The commercialisation of the contraceptive pill and the publication of the encyclical *Humanae Vitae* in 1968 generated ten years of great interest and lively debate on contraception that culminated with the legalization of contraceptives in late 1978 in large part due to a successful struggle from the feminist movement.

In Spain, as far as we know, intersectionality has never been applied to the study of the history of the contraceptive pill. Specific academic production on the history of hormonal contraceptives is limited to an unpublished thesis that focuses mainly on ethical aspects and lacks any gender perspective (Sánchez Carazo 1998). Moreover, in Spain during the 1960s and 1970s, an intersectional analysis was confined to categories of gender and class, considering that ethnic diversity was practically limited to gypsy collectives. Including ethnicity is beyond the scope of this paper, although we do not exclude it from our further studies.

In this paper we look at the debates surrounding the circulation and legalization of the pill in the 1960s and 1970s in Spain, a period that comprises the last fifteen years of Francoism and the beginnings of Spanish democracy, by using gender as a category of analysis. We pay attention to the actors and discourses that appeared in the daily press, medical press and feminist press. We review two of the oldest Spanish daily newspapers: *La Vanguardia,* a liberal newspaper published in Barcelona, and *ABC,* a conservative newspaper published in Madrid. We also review six years of *Tribuna Médica* (Madrid, years 1964-1969), the most prominent general Spanish medical journal during the 1960s and the 1970s, and *Vindicación Feminista* (Barcelona), the first feminist magazine to be published in Spain from 1976 to 1979.

The pill in the daily press

The dominant participants in the debate on hormonal contraception in both newspapers are foreign and Spanish physicians. Among the Spanish experts, the two most prominent figures seemed to be Professor Botella Llusía and Dr. Ángel Sopeña Ibañez. Each of them exhibited a different position; whereas Professor Botella Llusía was quite conservative, Dr. Ángel Sopeña Ibañez seemed to be more open and favourable to contraception. Botella Llusiá (1912-2002), cited in the late 1960s and 1970s, was a professor of gynaecology and obstetrics at the Complutense University of Madrid. He was author of numerous publications on gynaecology, sterility, gynaecologic endocrinology and oral contraceptives, and founded the most important school of gynaecology in Francoist Spain (Díaz Rubio 2003, 28-29). Ángel Sopeña Ibáñez (1913-1991) was consulted more frequently in the second half of the 1970s, when he was a professor of gynaecology at Complutense University in Madrid. He was a member of the Spanish Communist Party, and collaborated with the feminist family planning movement in the Spanish capital (Ortiz and Ignaciuk 2010). The debate on the pill was not limited only to gynaecologists and obstetricians, but other medical specialists, including dermatologists and psychiatrists, also participated.

Another category of experts whose opinions were frequently presented was that of Catholic priests, who appeared in all the important articles or multi-expert debates on the pill in which they stated their opposition to the pill as something unnatural and unacceptable within a Catholic marriage. This opinion was well represented in both newspapers. However, some priests exhibited a more moderate point of view, and even questioned the legitimacy of the church to participate in the debate on contraception. Still other priests defended sexuality as one of the pillars of a Catholic marriage, and criticized the rhythm (or Ogino) method as psychologically destructive (Ortiz and Ignaciuk, in press).

The most striking fact in the debate on the pill in the reviewed press is the nearly complete absence of voices from women and feminists. The few women who did talk about the pill were conservative or sceptical for religious and medical reasons. In 1975 –the year of Francos's death– *ABC* offered a breakthrough, as female journalists started to write about controversial issues related to women, such as single motherhood, or later on, family planning and female sexuality. In *La Vanguardia*, the feminist movement's fight for legalization of contraceptives received some, but rather unfavourable, attention in the late 1970s (Castillo García 2010).

The vast majority of the articles and news items published in both daily newspapers were dedicated to the side-effects of the pill (Ortiz and Ignaciuk, in press). This is similar to the media coverage in Britain and the United States in the late 1960s and 1970s, when the initial emphasis on the socially liberating effects of the pill was replaced by a more careful examination of the risks related to its consumption (Junod 2007). As historians of the contraceptive pill indicate, the potential health hazards related to the pill had to be carefully evaluated from its earliest stage of commercialization, considering that the pill was being used by healthy women to prevent pregnancy (Junod and Marks 2002, 120). The scientific debate on the side effects of the pill was long and confusing, along with its representation in the media, which often published contradictory information about its safety.

The first known side effects of the pill such as nausea, breast tenderness, weight gain and breakthrough bleeding were reported during the first large-scale clinical trials performed on women in Puerto Rico, Haiti, Mexico, Hong Kong, Australia, Ceylon or Japan (Marks 2001b, 96-106). These side effects, considered similar to those experienced during early pregnancy and/or menstruation, were underestimated by researchers and medical professionals as minor and temporary (Watkins 1998, 77), and also were often represented in a similar manner on various occasions in *ABC*. Lara Marks (2001a, 219) and Elizabeth Watkins (1998, 79) pointed out that the evaluation of these side effects was generally dependent on the concept of pregnancy, either as a natural or as a potentially hazardous condition for women.

The side effect of the pill that received the most coverage in the reviewed newspapers was thrombosis. The news reporting was confusing and contradictory, and there was a constant interchange of articles that either underlined the risks or reassured the public of the pill's safety. In the 1970s, the concern about the potential relationship between the pill and cancer began to appear. These articles expressed opinions both for and against the carcinogenity of the pill, and cited both national and international experts to support their positions.

Another important aspect of the medical debate on side effects of the pill, as represented in the daily press, was the repeated plea that women should take the pill under medical surveillance. Medical experts often quoted in *ABC* referred to the need for medical control concerning the use of oral contraceptives. This opinion seemed to reflect a need to maintain the privileged position of doctors in relation to their female patients. This hierarchal relation was being challenged at that very moment by feminist activists and feminist doctors working in the first family planning centres established in Madrid and Barcelona (Ortiz and Ignaciuk 2010).

The pill in the medical press

The scarce amount of information published in *Tribuna Médica*, the most widespread Spanish professional medical journal in the late sixties, seems to indicate, first, that the pill was not a great concern for medical professionals and, second, that the main opinions being expressed about the pill were the same voices as those in the general press: medical doctors and priests. Despite the fact that doctors could legally prescribe hormonal drugs, including the pill –even if not explicitly for its contraceptive properties–, since 1965, very little attention was paid to oral contraception in *Tribuna Médica*. The editors of the magazine were generally indifferent to the pill, and when discussed, it was in the broader context of population concerns or according to the official position of the Catholic Church.

During the years of 1964 to 1969 only two references to the pill were published. In May 1967, within the context of a report about a conference organized by the Association of Medical Writers [*Asociación de Escritores Médicos*] on social, legal and ethical aspects of population growth. The authors (a sociologist, a lawyer, and two priests) discussed overpopulation in terms of "responsible parenthood" as defined by Paul VI, or reproduction as "the most basic natural function" of the human kind. None of them mentioned oral contraceptives as a possible solution to the "population problem." This is very similar to the medical discourse published in *La Vanguardia* during the early 1970s in which the pill was often depicted as a threat to the survival of the human kind (Castillo 2010, 92).

The medical version of the topic was given by Professor Botella Llusiá, which displayed a more technical approach than did his contributions to the general press. He described the pharmacological action of the oral contraceptives available in the Spanish market at the time, and established a clear division between sequential, mixed and progesterone-only contraceptives. For moral reasons the author considered the use of the progesterone-only pills unacceptable since they stopped the implementation of the blastocyst in the uterine wall, which he considered to be the same as inducing an abortion (Adroer 1967).

A more explicit mention of the pill appeared in a letter to the editors of *Tribuna Médica* by a paediatrician (Javier Oroz 1967), where he discussed the co-existence of doctors for and against oral contraceptives in his medical speciality.

The pill in the feminist press

As stated above, women's voices concerning the use of the contraceptive pill were practically absent from the daily and medical press that we analyzed until the late 1970s. Only then did articles on the pill authored by women start to appear in *ABC* and *La Vanguardia* following the wave of liberalization and the growing impact of the feminist movement. Nevertheless, the dominant role of the male medical expert remained undisputable. However, female journalists and activists used alternative media spaces to discuss their opinions and preoccupations related to family planning, such as the newly created liberal daily *El País*, some magazines published during the period of democratic transition (*Triunfo*, *Cambio 16*) and the first feminist magazine, *Vindicación Feminista*. Founded in 1976 by Lidia Falcón (born 1937), a feminist philosopher and lawyer. She became one of the key figures of radical feminism in Spain during the transition to democracy (Larumbe 2009, 23). The magazine's ambition was to construct a plural and autonomous space for the Spanish women's movement. *Vindicación Feminista* was published in Barcelona from July 1976 to December 1979, and all the articles that appeared in the magazine were authored by feminist journalists.

The pill did not appear in *Vindicación Feminista* as an issue of concern on its own, though the pill was discussed within the broader debate on the legalization of contraceptives and abortion. The eleven articles that mentioned the pill also spoke about abortion, contraception and family planning. In contrast, there were eight articles that focused solely on the issue of abortion in Spain. When discussed, oral contraceptives were presented as the most widely used contraceptive method by middle class, educated women, who were able to get the pill from private gynaecologists as a therapeutic drug, not as a contraceptive. This semi-legal way of obtaining the pill was criticised since it was out of reach for women from marginalised neighbourhoods and regions, who did not have easy access to doctors (Begoña 1977). While accessibility to the pill was considered a class privilege, it also appeared to be the only available method in Spain used by women who should have been discouraged to do so for health reasons. It is worth noting that general newspapers like *ABC* or *La Vanguardia* expressed the opinions of priests and male doctors, while alternatively *Vindicación Feminista,* being a feminist magazine, cited mostly women's opinions on the pill, both as consumers and as feminist activists. These women often questioned and problematized the role of doctors and the hierarchy of the Catholic Church in their debates on oral contraceptives. They discussed issues such as doctors' reluctance to prescribe contraceptives, the internal disagreements within the Church following the publication on the encyclical *Humanae Vitae* (Larrauru 1977, 3), and the efforts of certain priests to prevent women from using the pill (Begoña 1977).

Vindicación Feminista gave much importance to the negative discourses and attitudes towards the pill. Side-effects were strongly emphasised (Encuesta 1979), sometimes with the support of the authority of doctors, such as José Badía Serra, from the Department of Gynaecology and Obstetrics of Sant Pau Hospital in Barcelona (Falcón 1978). The consumption of oral contraceptives was considered as a form of excessive medicalization of the female body (Taboada 1978). Only one author considered the bias and negative representation of the pill in the media (Belaguer 1977).

Conclusions

The presence of numerous publications concerning the contraceptive pill in the Spanish daily press during the time that it was illegal proves beyond a doubt the existence of a public debate on oral contraception in this country as early as the late 1960s. During the initial period of democratic transition there was a great increase in the number of publications that reflected the political climate of the time. The frequent references to British, American, and, to a lesser extent, French and Italian studies and practices suggest the presence of a vivid interest in the international debate on the pill, and especially its safety, which constituted the main argument in the debate represented in *ABC* and *La Vanguardia*. The debate on oral contraceptives in the reviewed daily press was dominated by the concern of side effects with doctors being the primary experts. The religious and moral discourses were also present, especially in relation to the encyclical *Humanae Vitae*, but they were neither dominant nor homogenous. In *Tribuna Medica*, there was scarce coverage on oral contraceptives in the late 1960s, and articles that were published did not differ from what appeared in the contemporary daily press. In contrast, the feminist press focused on issues that were not reflected in daily or medical press, such as access to other possibilities for women from different socio-economic backgrounds, or the pill being the only contraceptive choice available. By the late 1970s *Vindicación Feminista* openly criticized both the pill and the medicalization of contraception on the female body.

References

ADROER, Xavier (1967) Conferencia de la asociación de escritores médicos. *Tribuna Médica* 15/09/1967, (176): 11.

BALAGUER, Soledad (1977) Contracepción a la española: Todas somos delincuentes (reportaje). *Vindicación Feminista* 01/01/1977, (7): 41-43.

Begoña (1977) Curas, maridos y anticonceptivos (letter to *Vindicación Feminista*). *Vindicación Feminista* 01/06/1977, (11): 66.

BRIGGS, Laura (2002) Demon mothers in the social laboratory. Development, overpopulation and "the pill", 1940-1960. In: *Reproducing empire. Race, sex, science and U. S. imperialism in Puerto Rico*. Berkley: University of California Press, 109-141.

CASTILLO GARCÍA, Elisabeth (2010) [Unpublished MA dissertation] *El debate sobre la anticoncepción en la prensa de la transición española. El caso de la píldora en La Vanguardia*. Granada: Universidad de Granada.

DE SEMIR, Vladimir; REVUELTA, Gemma (2002) Ciencia y medicina en La Vanguardia y The New York Times. Un capítulo del periodismo científico. *Quark: Ciencia, Medicina, Comunicación y Cultura* 26, http://www.prbb.org/quark/26/026068.htm (accessed 15 February 2011).

DÍAZ RUBIO, Manuel (2003) 100 médicos españoles del siglo XX. 2nd ed. Madrid: You & Us.

Encuesta sobre la conducta sexual femenina (1979). *Vindicación Feminista* 28/07/1979, (28): 16-26.

FALCÓN, Lidia (1978) Bilbao, Zaragoza, Valencia. Las condenas españolas. *Vindicación Feminista* 01/01/1978, (Especial Aborto): 23-25.

Federació Local de Sindicats de Barcelona (2008). *Evolució de la Legislació Espanyola sobre L'avortament* http://www.cgt.org.es/IMG/pdf/evolucio_legislacio_sobre_avortament.pdf (accessed 4 March 2011).

GORDON, Linda (1988) *Heroes on their own lives: The politics and history of family violence*. New York: Viking.

GRANT, Nicole J. (1993) *The selling of contraception: The Dalcon Shield case, sexuality, and women's autonomy*. Columbus: Ohio State University Press.

JONES, Maggie (1977) Will the pill come in with the vote? *The Guardian* 24/12/1977.

JUNOD, Suzanne White (2007) Women over 35 who smoke. A case study in risk management and risk communications, 1960-1989. In: *Medicating modern America: Prescription drugs in history*, eds. Andrea TONE, Elizabeth Siegel WATKINS. New York: New York University Press, 97-130.

JUNOD, Suzanne White; MARKS, Lara (2002) Women's trials: The approval of the first oral contraceptive pill in the United States and Great Britain. *Journal of the History of Medicine*, 57: 117-160.

LARRABURU, Carmen S (1977) Cuarenta años de oscurantismo ginecológico. *Vindicación Feminista* 01/10/1977, (16): 30-31.

LARUMBE, María Ángeles (2009) Vindicación Feminista: Un ideal compartido. In: *Vindicación Feminista: Una voz colectiva, una historia propia. Antología facsímil de textos (1976-1979)*. Zaragoza: Universidad de Zaragoza, 17-43.

LÓPEZ, Iris (2008) The birth control movement in Puerto Rico. In: *Matters of choice: Sterilization and Puerto Rican women's reproductive rights*. New Brunswick: Rutgers University Press, 3-19.

MARKS, Lara (2001a) "Andromeda freed from her chains": Attitudes towards women and the oral contraceptive pill, 1950-1970. *Clio Medicine*, 61: 217-244.

MARKS, Lara (2001b) *Sexual chemistry. A history of the contraceptive pill*. New Haven; London: Yale University Press.

MCCALL, Leslie (2005) The complexity of intersectionality. *SIGNS: Journal of Women in Culture and Society*, 30 (3): 1771-1800.

METZL, Jonathan M. (2003) *Prozac on the couch: Prescribing gender in the era of wonder drugs*. Durham: Duke University Press.

Ministerio de Gobernación (1965) Orden de 14 de agosto 1965 por la que se dictan normas para la dispensación de medicamentos, *BOE* 235/1965.

NELSON, Jennifer. Women of color and the reproductive rights movement. New York: New York University Press, 2003.

OROZ, Javier. Los pediatras españoles y la píldora. Tribuna Médica 10/11/1967, (184): 2.

ORTIZ GÓMEZ, Teresa; IGNACIUK, Agata (2010) The family planning movement in Spain during the democratic transition. Paper presented at *Health Activis Symposium*, Yale University, 10/2010.

ORTIZ GÓMEZ, Teresa; IGNACIUK, Agata (In press) *"Pregnancy and labour cause more deaths than oral contraceptives": the debate on the pill in the Spanish press in the 1970s.*

ROMO AVILÉS, Nuria; GIL GARCÍA, Eugenia (2006) Género y uso de drogas. De la ilegalidad a la legalidad para enfrentar el malestar. *Trastornos Adictivos*, 8(4):243-50.

ROBERTS, Dorothy (1997) From Norplant to the contraceptive vaccine. The new frontier of population control. In: *Killing the black body: Race, reproduction, and the meaning of liberty*. 104-149. New York: Pantheon Books.

RODRIQUE, Jessie M. (1999) The black community and the birth control movement. In: *Women and health in America: Historical readings*, ed. Judith Walzer LEAVITT. Madison: University of Wisconsin Press, 293-305.

ROSS, Loretta J. (1998) African-American women and abortion. In: *Abortion wars. A half century of struggle*, ed. Rickie SOLINGER. Berkley: University of California Press, 161-207.

SANCHÉZ CARAZO, Carmen (1998) [Unpublished PhD dissertation] *Introducción de los anovulatorios orales en España: Aspectos morales, sociales y médicos*. Madrid: Universidad Complutense.

SCHOEN, Johanna (2005) *Choice & coercion: Birth control, sterilization, and abortion in public health and welfare*. Hill: University of North Carolina Press.

SOLINGER, Rickie (2005) The human rights era. The rise of choice, the contours of backlash, 1960-1980. In: *Pregnancy and power: A short history of reproductive politics in America*. New York: New York University Press, 162-208.

TABOADA, Leonor (1978) Cómo derribar la medicina masculina. El self-help o la descolonización de nuestro cuerpo. *Vindicación Feminista* 01/02/1978, (20): 38-40.

TONE, Andrea; WATKINS, Elizabeth Siegel (2007) *Medicating modern America: Prescription drugs in history*. New York; London: New York University Press.

VAN DIJK, Teun A. (1997) *Racismo y análisis crítico de los medios*. Barcelona: Paidós.

WATKINS, Elizabeth Siegel (1998) *On the pill: A social history of oral contraceptives, 1950-1970*. Baltimore: Johns Hopkins University Press.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 898.

(UN-)SAFE DOSE LEVELS. SCIENTIFIC-FEMINIST COALITIONS AND CONTRADICTIONS IN WEST GERMANY IN THE 1950S AND 60S

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Abstract

This contribution deals with the different and varying agendas concerning policies of zero tolerance for chemicals invading the gendered human body in post-war West Germany. The main focus thereby is on scientific-feminist coalitions and contradictions in regard to two very different chemical substances –estrogens and butter yellow.

Estrogens

In the 1930s the just-standardized estrogens combined the function of physiologically active agents in a chemically regulated mostly female body with industrial produced and clinical tested measures of intervention into this body, like the mere biopolitical treatment of sterility and the mere clinical treatment of menstrual or climacteric complaints. In both regards they were defined as highly effective molecular substances, thereby creating a new ontology of the female body as well as new commodity and tool for the optimization of female functions. Its existence could be proved through the experimental production of deficiency and its suspension: the removal of the ovaries; the recovery of specific functions through organo- and hormonal therapy.[1]

The increasing demand for sex hormones was satisfied by the leading pharmaceutical companies. While testosterone and progesterone could be synthesized in the 1930s, the biochemists did not succeed in the synthesis of estrogens (ethinylestradiol) until the late 1940s. The only alternative to estrogens isolated from pregnant mare urine, which the Berlin *Schering AG* distributed under the name of *Progynon*, was the stilben derivate diethylstilbesterol (DES), which in 1938 Charles Dodds produced as an estrogen-active compound.[2] In the course of the 1930s a new (hormonal) physiology of

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[1] On the history of estrogens see amongst others: Ratmoko, 2010; drowning; Sengoopta, 2006; and Oudshoorn, 1994.

[2] Dodds, Goldberg, Lawson, Robinson, 1938. Gaudillière, 2008, p. 117 and Gaudillière, 2004, pp. 220-221.

the improvable gendered body was set into clinical and medical practice.[3] But a dark cloud overshadowed this success story of industrial-clinical-scientific cooperation. Biologically active substances like hormones and vitamins were not only acclaimed as a cure for all, but exactly because of their effectiveness to induce growth, they were also suspected to be toxic or even cancerogenous. While this concerned nearly all hormones and vitamins, estrogens addressed a particular threat.

In 1915, the Japanese pathologists Katsusaburo Yamagiwa and Koichi Ichikawa painted rabbit ears with coal tar thereby inducing skin cancer. A working group around Ernest L. Kennaway and James W. Cook blamed the pure chemical compound benzpyrene (present in coal tar) to cause the cancerogenous effect. Therefore, in the 1920s aromatic hydrocarbons were regarded as extremely carcinogenic substances. One of these aromatic hydrocarbons, methylcholanthrene, was related to steroids. It was even possible, as the leading chemists Heinrich Otto Wieland and Adolf Windaus independently of each other showed, to convert cholesterol and bile acid into methylcholanthrene. Therefore wasn't it possible, that under certain circumstances steroids also could turn into methylcholanthrene? Because estrogens were characterized by a partially aromatized hydrocarbon framework, these useful biopolitical agents were suddenly in the critical and not therapeutical focus of cancer research.[4]

Adolf Butenandt, Germany's leading biochemist of the 1930s, director of the *Kaiser-Wilhelm-Institute for Biochemistry* in Berlin-Dahlem, who, in close cooperation with the *Schering AG* had isolated estrone, was indeed shocked, that this thesis seemed to gain facticity in some scientific writings. There was an achievement at stake which was of high importance for Butenandt's own career, for everyday gynaecological practice, for the biopolitical interests of the national socialist state, and finally and most of all for Schering. In the summer of 1937 Butenandt, financed by the *German Research Foundation* and in cooperation with the gynaecologist Carl Kaufmann from the *Charité* in Berlin, organized a working group to prove this suspicion.

The story of this research project which combined the interests of laboratory science, clinic and industry has been written at length by the French science historian Jean-Paul Gaudillière.[5] The expectations were surely to give estrogens the benefit of the doubt, but Butenandt was a respectable scientist, who never would have faked his findings. What Kaufmann and Butenandt did was to concentrate their research on the question of genetic precondition. Kaufmann, who in the 1930s had tested *Schering*'s hormone products for optimal dosages and broader indications (he was an expert on the treatment of amenorrhoea, the lack of menstrual periods), administered three thousand mice with estrogens. He concluded that even a continuous administration wouldn't increase the rate of tumours. Butenandt again referred back to the experimental work done by Antoine Lacassagne, who had injected follicle hormones to male mice, thereby inducing breast cancer. Sex steroids, Butenandt stated, affected genetic preconditions which existed only in certain mice breeds. Estrogens were only the causer, but not the cause of cancer; the genetic precondition was the necessary condition for the hormonal effect. When Butenandt presented the results of the working group in June 1940 he summoned that estrogens just like DES could indeed induce breast cancer with genetically preconditioned mice, but that this had nothing to do with its chemical structure. Schering wasn't very pleased with this statement, because it gave DES, which the competitor *IG Farben* produced under the brand name of *Cyren*, the same status as *Progynon*. Anyway, soon after this they settled an agreement: that *IG Farben* would stop to compare its cheaper product to *Progynon*, while *Schering* would be quiet about the toxicity of *Cyren*. But what remained as a fact was the experimental testimony, that estrogens themselves were not a cancerogenous substance.[6]

The actors involved were merely scientists, clinicians and the pharmaceutical industry. But where were the persons concerned, where were the women? Now this took place in Nazi Germany, so there were limits to dissent. But as I will show next, this wasn't true for another debate on other cancerogenous substances. I will propose two assumptions to explain the silence of women and women's organizations: 1. the reproductive and biopolitical issue was part of the feminist discourse in the first decades of the 20th century. There was simply no interest to criticize hormonal therapy; 2. medical topics were negotiated between the mostly male experts. In the first half of the 20th century a very hierarchical relationship existed between doctors and patients, which left no space for any objection by the side of the patient.[7]

Butter yellow

In the early 1930s the use of food additives like bleaching agents, colorants and preservatives were related to a statistically presumed rise in cancer. A critique of modern food production was common in all western nations since the last third of the 19th century. But while this critique was focussed on food fraud, in Germany the highly influential interplay of diet reform (as part of the life reform movement) and new dietetics emphasized the need for a healthy diet based on natural and

[3] Stoff, 2004.

[4] Butenandt, 1940, p 348. Deichmann, 2001, p. 344.

[5] Gaudillière, 2006, 2004.

[6] Gaudillière, 2008, p. 117.

[7] Grossmann, 1995.

pure food. In the 1930s diet-reform advocates like Werner Kollath, the German guru of wholefood nutrition, denounced industrially produced food as denaturalized and dangerous for the health of the people. In 1931 Curt Lenzner published a book entitled *Gift in der Nahrung* (*Poisoned food*). One year later Erwin Liek, the notorious enemy of the social and health security system of the Weimar Republic, finally proclaimed a connection between civilization and cancer, actualized in chemicalized and technicalized food.[8] This strong position gained even more strength with the empowerment of the National Socialists, its narrative of a pure body threatened by foreign matter fitted well into Nazi ideology. In the late 1930s and early 1940s there were inner contradictions and an open dispute between diet reformers and those nutrition experts who had to fulfil the need of belligerent Nazi Germany to secure the nutritional situation with the help of canned or frozen food in particular and food technology in general.[9]

Around 1940 the case of butter yellow, an azo compound used to give butter an attractive yellow colour, caused international stir. Between 1932 and 1937, the Japanese pathologist Riojun Kinosita proved that several azo dyes, most prominently and widely used butter yellow, were cancerogenous. The German pharmacologist Hermann Druckrey confirmed these results.[10] Therefore, in June 1939 the *International Congress for Cancer Research* recommended the ban of butter yellow for food colouring. Only a few months later Hans Reiter, the president of the *German Reich Health Office*, suggested a new *German colour law* (*Neues Deutsches Farbstoffgesetz*). Robert Proctor in his book on the history of cancer research in Nazi Germany outlined the complicated situation Reiter was in, because a sudden removal of colours in the middle of the war might have been interpreted as the application of inferior foodstuffs. But on the other hand there were already rumours that coloured food might be dangerous for health. And at this point, the women's organizations of Nazi Germany interfered, applied pressure to Reiter and finally succeeded. As Proctor tells this story, in 1941 a member of Göttingen's so called *NS-Frauenwerk* asked her superiors why "cancer-causing" substances were still allowed in butter and margarine. The regional women's leader informed Reiter that (quote) while women were certainly willing to sacrifice for the war, accepting the presence of cancer-causing agents in food was something else. And indeed Reiter, who appreciated the housewife's organization as allies in his efforts for wartime food security, rather successfully negotiated with the different groups producing and marketing coal tar dyes to reduce the use of dyes. Finally, even the almighty *IG Farben* stopped the production of butter yellow.[11]

To sum this story up, it was a coalition of scientists, politicians and women's organizations who succeeded in the prohibition of butter yellow. Women organizations, as has been shown in several studies, were deeply involved in Nazi Germany's health and nutrition policies. Housewives thereby were explicitly defined as consumers and guardians of nature. They were far from being marginalized and were able to determine health political decisions. They were worried by poisoned food but not by cancerogenous hormones.

Estrogens and butter yellow

In the 1950s and 60s these two stories of silent and worried, of apolitical and political women convened. It was indeed Butenandt, the defender of estrogens, who in 1948 frightened the public when he proclaimed that cancerogenous butter yellow was still in use.[12] Even if this accusation was immediately denied by nutrition experts and representatives of the pharmaceutical industry a debate was opened, which shaped the policies on food additives in Germany throughout the 1950s. It was the well respected physician Karl-Heinrich Bauer who introduced a new oncologist theory, neglecting the role of genetics for cancer while emphasizing the significance of exogenous agents like rays or chemical compounds. Bauer based this assumption on the case of azo dyes, but he also recapitulated the thesis of a strong connection of civilization and an assumed rise in cancer.[13] These mere speculations gained scientific facticity through the collaboration of Druckrey with the mathematician Karl Küpfmüller in 1948: Druckrey conducted animal tests with butter yellow. These experiments showed that to produce tumours, a certain total dose is necessary, regardless of how it is distributed between 35 and 365 days. The latency period, Druckrey stated, is inversely related to the daily dose. If experiments are extended over the life span of the animals, a smaller dose is necessary to produce an effect: with increasing age, there is increasing disposition to develop tumours. And as Druckrey concluded: "The carcinogenic effect of butter yellow was thus, even at the smallest doses, irreversible from the beginning of the experiment during the entire life span of the animals, and is additive with further exposure without any modification, until, after a critical total dose has been exceed, the tumours develop". Because of the

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[8] Kollath, 1940; Liek, 1932; and Lenzner, 1931. Fritzen, 2006; Melzer, 2003; and Merta, 2003.

[9] Stoff 2011 and Sperling, 2010.

[10] Kinosita, 1940, pp. 287-292) and Brock, Druckrey, Hamperl, 1940.

[11] Proctor, 1999, pp. 165-170.

[12] Hartmann, 1949, pp. 247-248.

[13] Bauer, 1950.

latency period –the dose-effect and dose-time relation– it was nearly impossible to decide whether a certain substance is cancerogenous or not. From then on it was chemical substances in everyday use that showed the most risk.[14]

Bauer, Druckrey and also Butenandt were comrades in arm in the ongoing war against cancer. The field for this battle was the senate commissions for preservatives, foreign matter and colorants established by the *German Research Foundation*, in which representatives of the pharmaceutical and food stuff producing industry and politicians under the guidance of scientists negotiated the use of food additives.[15] While the commissions worked behind closed doors, a public discourse about the "toxic condition" of modern life and the negative role of the pharmaceutical and chemical industry gained strength. The catchphrase of a "toxische Gesamsituation" ("toxic total situation"), coined by Fritz Eichholtz, director of the Pharmacological Institute at the University of Heidelberg, thereby instructed a far-reaching debate on the boundaries of risk assessment and the dangers of chemical substances.[16]

But already in the early 1950s there was an open critique that consumers were not represented by the commissions. At this time there were indeed no consumers' associations existing. But housewife's and women's organizations resumed their battle against poisoned food. In February 1950 the *Deutsche Frauenring* (*German Women's Association*) demanded measures against the colouring of food. Other catholic or protestant women's and housewife's organizations wrote letters to the ministry of health and of the interior to prohibit food colouring. The Information service for women's question, uniting eighty women's organizations and groups, applied to all relevant political representatives to claim the passage of a new food law based on a white list of experimentally proven substances.[17] On February 24th, 1956 the members of parliament Hedwig Jochmus (CDU), Käte Strobel (SPD), Marie-Elisabeth Lüders (FDP) and 43 other female delegates of the German Bundestag presented an application, that the Bundestag should request the Federal Government to produce a draft for a new food law until May 1st, 1956. This proceeding was well prepared by Jochmus, Strobel and Werner Gabel, undersecretary in the ministry of the interior. While there was a lot of laughter by the male members of parliament about this issue, the „Einheitsfront der weiblichen Abgeordneten" ("unity front of female delegates") found strong response by the public.[18] The women's organizations succeeded in releasing a new and much stricter food law. But the consumer's organizations which were established right in the context of this debate were soon to be masculinized. As well as the food law, which based on Druckrey's dose-time-effect-law was successively abolished by specific regulations, which again based on the much more industrial friendly concept of an acceptable daily intake.

To finally unite the threads of this story I will return to estrogens, hormonal therapy and cancer. In the mid-1960s a debate on the use of estrogens, provoked by the translation of the bestseller *Feminine forever*, written by the gynaecologist Robert A. Wilson, raised tempers.[19] The question was if menopause is a natural condition to which women should adopt or if it is a deficiency symptom which could be taken care of by gynaecologists. "Wilson wants to abolish menopause with estrogens", read one of the many headlines of the German press. Menopause, lectured the gynaecologist Josef Zander interrogated by Germany's leading news magazine *Der Spiegel*, is a deficiency disease, a lack of estrogens. The only open question for Zander was if acute or prophylactic measures should be taken.[20]

But Wilson's book also caused a heated debate on the dangers of hormonal therapy. *Der Spiegel* even referred to a „Hormon-Krieg" ("hormone-war"). Many physicians interposed that such a use of estrogens besides heavy side-effects could induce uterine and breast cancer. There were not many gynaecologists who reminded their colleagues that sixty years ago estrogens caused tumours in mice, while Butenandt's verdict about the necessary genetic precondition for breast cancer was well and alive in the 1960s. No wonder, Zander was one of Butenandt's disciples.[21] This contrasted sharply to the debate which was going on in the UK at this time and was mostly met with opposition by German physicians. In 1950 Alexander Lipschütz, a veteran of hormone research, published a monograph under the title *Sexual Hormones and Tumors*. He emphasized the connection of those organs, which were controlled by hormones (breast, uterus, prostate), and which could be governed by hormonal therapy, to a vulnerability for tumours. And again, while in the USA hormonal therapy for natal complications and risk pregnancies was common practice, Eric Stephen Horning and Hadley Kirkman from the *Stanford University School of Medicine* were able to induce renal tumour with hamsters inducing estrogens under certain experimental conditions.[22]

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[14] Druckrey, Küpfmüller, 1948. Wunderlich, 2005.

[15] Stoff, 2009.

[16] Eichholtz, 1956.

[17] Deutscher Frauenring, Ausschuß für Volks- und Heimwirtschaft an Bundesminister des Innern, Abtlg. Gesundheit (14.2.1950), *BA Koblenz*, B 116/419.

[18] 2. Deutscher Bundestag. 149. Sitzung. Bonn, Freitag, den 8. Juni 1956, *BA Koblenz*, B 142/1528, 2 von 3, p. 7901.

[19] Wilson, 1966. Houck, 2003.

[20] Müller, Petermann, 1966.

[21] Dallenbach-Hellweg, Dallenbach, 1971.

[22] Kirkman, 1957, p. 757; Horning, 1951; and Lipschütz, 1950.

The story of the contraceptive pill has been written in great detail.[23] In the early 1960s hormonal contraception was discussed in regard to the risk of thrombosis. But in 1964 the Hamburg gynaecologist Oskar Guhr stated that the pill might induce uterine cancer. His statement was based on only eighty cases, while Gregory Pincus examined a thousand women, which took the pill and were absolutely healthy. Again the journalist Thomas von Randow reminded the readers of the weekly magazine *Die Zeit* of the sad case of *Contergan* (*Thalidomide*) just some years ago: "Even the fact that the pill is taken by millions of women cannot reassure us until Guhr's outrageous suspicion is not refuted".[24] In the following years there was an ongoing dispute. Every clinical study or animal experiment which seemed to prove the cancerogenity of estrogens and DES found its counter-argument and vice versa. While the dosage of estrogens in the pill was gradually low-levelled –to pass the ADI– there still remained a suspicion of the chemical structure of estrogens themselves (reminiscence to Druckrey's dose-time-effect-law). While this provoked a debate on the methods of steroid toxicology, the interpretation of animal tests, the problem of latency, the necessity for long time studies and finally the unsolved difficulties of statistics, many women already reacted as consumers: they abandoned.

The story of the pill is so complicated because there are, in dramatic difference to the 1940s, so many actors: self proclaimed progressive scientists as Carl Djerassi; neo-malthusianists and population politicians; conservative, pro natalistic and biopolitical physicians; a new media hunting for headlines, riding on the sex wave while fanning breast cancer fear; the famous papal encyclical; and last but not least an itself not homogenous women's movement. While for centuries the women's movement was associated with sexual reform and emancipation, in the late 1960s it, as historians like Lara Marks and Barbara Duden emphasize, turned into a consumer's movement, which in Germany followed the example of the 1950s fight for a new food law.[25] Indeed the social and political situation had changed over the 1950s. In 1970, female members of parliament, Hedda Heuser (FDP) and again Käte Strobel (SPD), organized a non-party inquiry regarding the dangers of the contraceptive pill. Feminists, thereby overcoming the former, hierarchical separation of experts/doctors and patients, used the expertises of scientists but also referred to the experiences of women. As part of a consumer's movement the feminist critique of the pill was raised against elitist negotiations of interests and risks, which neglected what was simply going on in women's bodies. The controversy about the pill, as Barbara Duden summarizes, accelerated the transformation of women from patients to self-determined consumers.[26] But thereby they not only lost essentials of the feminist agenda but also inherited the life reform discourse which had been so fundamental in the debate on butter yellow.

References

BAUER, Karl-Heinrich (1950) 'Über Chemie und Krebs – dargestellt am Anilinkrebs', *Langenbecks Archiv für Klinische Chirurgie*, 264, pp. 21-44.

BROCK, Norbert (1940) Hermann Druckrey, Herwig Hamperl, 'Die Erzeugung von Leberkrebs durch den Farbstoff 4- Dimethylamino-azobenzol', *Zeitschrift für Krebsforschung*, 50, pp. 431-456.

BUTENANDT, Adolf (1940)'Neuere Beiträge der biologischen Chemie zum Krebsproblem', *Angewandte Chemie* 53, pp. 345-352.

DALLENBACH-HELLWEG, G.; DALLENBACH, F.D. (1971) 'Besteht ein morphologisch faßbarer Zusammenhang zwischen Oestrogen und Carcinogenese?', *Archiv für Gynäkologie*, 211, pp. 198-200.

DEICHMANN, Ute (2001) *Flüchten, Mitmachen, Vergessen. Chemiker und Biochemiker in der NS-Zeit* (Weinheim: Wiley-VCH).

DODDS, E. C.;GOLDBERG, L.; LAWSON, W.; ROBINSON, W.R. (1938) 'Estrogenic Activity of Certain Synthetic Compounds', *Nature*, 141, pp. 247-248.

DRUCKREY, Hermann; KÜPFMÜLLER, Karl (1948) 'Quantitative Analyse der Krebsentstehung', *Zeitschrift für Naturforschung*, 3b, pp. 254-266.

DUDEN, Barbara (2008) 'Frauen-,Körper: Erfahrung und Diskurs (1970-2004)', In: Ruth BECKER, Beate KORTENDIEK (eds.), *Handbuch Frauen- und Geschlechterforschung Theorie, Methoden, Empirie. 2., erweiterte und aktualisierte Auflage* (Wiesbaden: VS-Verlag), pp. 593-607.

[23] Marks, 2001.

[24] Randow, 1964.

[25] Duden, 2008; and Marks, 2001.

[26] Duden, 2008.

EICHHOLTZ, Fritz (1956) *Die toxische Gesamtsituation auf dem Gebiet der menschlichen Ernährung. Umrisse einer unbekannten Wissenschaft* (Berlin, Göttingen, Heidelberg: Springer).

FAUSTO-STERLING, Anne (2000) *Sexing the Body. Gender Politics and the Construction of Sexuality* (New York: Basic Books).

FRITZEN, Florentine (2006) *Gesünder leben. Die Lebensreformbewegung im 20. Jahrhundert* (Stuttgart: Steiner).

GAUDILLIÈRE, Jean-Paul (2008) 'Professional or Industrial Order? Patents, Biological Drugs, and Pharmaceutical Capitalism in Early Twentieth Century Germany', *History and Technology*, 24, pp. 107-133.

GAUDILLIÈRE, Jean-Paul (2006) 'Hormones at Risk. Cancer and the Medical Uses of Industrially-Produced Sex Steroids in Germany, 1930-1960', In: Thomas SCHLICH, Ulrich TRÖHLER (eds.), *The Risks of Medical Innovation. Risk Perception and Assessment in Historical Context* (London/New York: Routledge), pp. 148-169.

GAUDILLIÈRE, Jean-Paul (2004) Biochemie und Industrie. Der ‚Arbeitskreis Butenandt-Schering' während der Zeit des Nationalsozialismus', In: Wolfgang SCHIEDER, Achim TRUNK (eds.), *Adolf Butenandt und die Kaiser-Wilhelm-Gesellschaft. Wissennschaft, Industrie und Politik im Dritten Reich* (Göttingen: Wallstein), pp. 198-246.

GROSSMANN, Atina (1995) *Reforming Sex. The German Movement for Birth Control and Abortion Reform, 1920-1950* (New York, Oxford: Oxford University Press).

HARTMANN, Fritz (1949) '55. Tagung der Deutschen Gesellschaft für innere Medizin', *Die Naturwissenschaften*, 36, pp. 245-249.

HORNING, E.S. (1951) 'Hormones and Carcinogenesis', *British Medical Journal*, 2, pp. 834-835.

HOUCK, Judith A. (2003) 'What Do These Women Want?: Feminist Responses to *Feminine Forever,* 1963-1980', *Bulletin of the History of Medicine*, 77, pp. 103-132.

KINOSITA, Riojun (1940) 'Studies on the cancerogenic azo and related compounds', *Yale Journal of Biology and Medicine*, 12, pp. 287-300.

KIRKMAN, Hadley (1957) 'Steroid Tumorigenesis', *Cancer*, 10, pp. 757-764.

KOLLATH, Werner (1942) *Die Ordnung unserer Nahrung. Grundlagen einer dauerhaften Ernährungslehre* (Stuttgart: Hippokrates).

LENZNER, Curt (1931) *Gift in der Nahrung* (Leipzig: Verlag der Dykschen Buchhandlung).

LIEK, Erwin (1932), *Krebsverbreitung, Krebsbekämpfung, Krebsverhütung*. (München: Lehmanns).

LIPSCHÜTZ, Alexander (1950) *Steroid Hormones and Tumors. Tumirogenic and antitumirogenic actions of steroid hormones and the steroid homeostasis: experimental aspects* (Baltimore: Williams & Wilkins).

MARKS, Lara V. (2001) *Sexual Chemistry. A History of the Contraceptive Pill* (New Haven, London: Yale University Press).

MELZER, Jörg M. (2003) *Vollwerternährung. Diätetik, Naturheilkunde, Nationalsozialismus, sozialer Anspruch* (Stuttgart: Steiner).

MERTA, Sabine (2003) *Wege und Irrwege zum modernen Schlankheitskult. Diätkost und Körperkultur als Suche nach neuen Lebensstilformen 1880-1930* (Stuttgart: Steiner).

MÜLLER, Rolf S. (1966) 'Jürgen Petermann, ‚Östrogen für alle Frauen?', *Der Spiegel*, 20: ?, pp. 140-149.

OUDSHOORN, Nelly (1994) *Beyond the Natural Body. An Archaeology of Sex Hormones* (London, New York: Routledge).

PROCTOR, Robert N. (1999) *The Nazi War on Cancer* (Princeton: Princeton University Press).

RANDOW, Thomas von (1964) 'Statt Baby Angst vor Krebs? Fatales Sensationsgeschrei um einen umstrittenen Befund', *Die Zeit*, 19:44 (30.10.1964) http://www.zeit.de/1964/44/Statt-Baby-Angst-vor-Krebs

RATMOKO, Christina (2010) *Damit die Chemie stimmt. Die Anfänge der industriellen Herstellung von weiblichen und männlichen Sexualhormonen 1914–1938* (Zürich: Chronos).

ROBERTS, Celia (2007) *Messengers of Sex: hormones, biomedicine and feminism*. (Cambridge: Cambridge University Press).

SENGOOPTA, Chandak (2006) *The Most Secret Quintessence of Life. Sex, Glands, and Hormones, 1850-1950* (Chicago, London: Chicago University Press).

SPERLING, Frank (2010) *'Kampf dem Verderb mit allen Mitteln? Der Umgang mit ernährungsbezogenen Gesundheitsrisiken im Dritten Reich am Beispiel der chemischen Lebensmittelkonservierung*. (Unpublished doctoral dissertation, Braunschweig).

STOFF, Heiko (forthcoming) *Wirkstoffe. Regulatoren des Leistungsgetriebes: Eine Geschichte der Institutionalisierung, Standardisierung, Aktivierung und Prekarisierung der Hormone, Vitamine und Enzyme, 1920-1970* (Stuttgart: Steiner).

STOFF, Heiko (2009) 'Hexa-Sabbat. Fremdstoffe und Vitalstoffe, Experten und der kritische Verbraucher in der BRD der 1950er und 1960er Jahre', *N.T.M*. 17, pp. 55-83.

STOFF, Heiko (2004) 'Janine. Tagebuch einer Verjüngten. Weibliche Konsumkörper zu Beginn des 20. Jahrhunderts', In: Claudia BRUNS, Tilmann WALTER (eds.), *Von Lust und Schmerz*. (Köln, Weimar: Böhlau), pp. 217-238.

STOFF, Heiko (2004), *Ewige Jugend. Konzepte der Verjüngung vom späten 19. Jahrhundert bis ins Dritte Reich* (Köln, Weimar: Böhlau).

WILSON, Robert A. (1966) *Feminine Forever* (New York: Evans & Co.).

WUNDERLICH, Volker (2005) 'Zur Entstehungsgeschichte der Druckrey-Küpfmüller-Schriften (1948-1949): Dosis und Wirkung bei krebserzeugenden Stoffen', *Medizinhistorisches Journal*, 40, pp. 369-397.

SCIENTIFIC CORRESPONDENCE

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 906.

CIRCULATING KNOWLEDGE THROUGH LETTERS, CIRCULATING LETTERS THROUGH TECHNOLOGY: THE «EASTWAYS OF SCIENCE» PROJECT[1]

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Abstract

Correspondences are one of the most important sources for a science historian: they shed light upon the origins of new scientific theories; they are a privileged point of view to understand the debates of an age of censorship; last but not least correspondences are necessary instruments to understand that extraordinary phenomenon that was the ideal super-national academy of the 17th century European "scientists".
These are some of the reasons why the letter exchanges kept by the «heroes» of modern science still offer material for monumental edition plans. Anyway, if we consider the scientific correspondences as the main source for mapping boundaries, extension, density, population and roadways of the actual "Republic of sciences", the editions on paper –even if philologically peerless– are widely insufficient to manage the huge mass of information inferred from letters. Eastways of Science *is one of the many projects that nowadays exploit the web-technologies to edit, manage and study such documents. It is a research project co-financed by the European Commission, hosted by the University of Rome* La Sapienza *and technologically implemented by the* Rinascimento Digitale *Foundation-Institute and the Museum for the History of Science of Florence. It aims at creating an electronic database that would work as a common repository of letters of scientific-historical interest; a register of the men, women, lecturers, practitioners, patrons, institutions, things etc. involved in such a "Republic" (especially as to Middle-East Europe) in the second half of the 17th Century; a «dynamic» database able to reconstruct the networks of scientific relations that crossed early modern Europe –that is the scientific information flows– making them visible on a geographic map. It focuses on the Academy of the Cimento correspondence, and on its intersections with the epistolaries by Johannes Hevelius and Giovanni Alfonso Borelli.*
This paper aims at explaining and discussing methodologies, ends and temporary results of the project.

[1] §§ 1-7 are by Federica Favino; §§ 8-11 by Andrea Scotti; § 12 is common.

1. Scientific correspondence is an important primary source for early modern European history of science. It throws light upon the origins of scientific theories and their experimental background not easy to detect only by published proceedings. It is a privileged point of view to understand the genesis and development of debates in an age marked by censorship; it is the instrument through which the learned community was construed on the basis of the humanistic ideal of exchange as well as the space where scientific knowledge was produced (think about mathematical challenges or experiment's reports comparison).

But letters do not serve only intellectual history. As to the social history of science, they are often the only means to detect information on those hybrid and apparently "minor" characters that contributed meaningfully to make scientific knowledge, to promote and to spread it, like *amateurs*, public lecturers, draftsmen, craftsmen, instrument makers, trade-men and merchants; as well as on the "minor" institutional settings of early modern science, like universities and colleges, academies and conversations, publishers, printers and booksellers.

As to the «material culture» of scientific enterprise, letters used to work as a vehicle of scientific information (theories, observations, experiment reports, experimental protocols), as well as of objects (letters, books, drawings, instruments, seeds, plant specimens, exemplars to be collected).

In a word, scientific letters are at the same time vectors that fix the boundaries of early modern space of sciences; repositories of a huge mass of information on people, places and objects that populated that space; signs that draw the street-networks scientific information and objects flowed through. A huge mass of data that only informatics technologies are able to detect, to manage and to give back so as to answer the scientific questions made by a discipline, like the history of science, that is getting more and more complex.[2]

2. *Eastways of Science* –acronym for *At the fringes of European "Republic of science": the east-west routes of scientific communication in the age of experiment (1650-1680 ca.)*– is a project co-financed by the European Commission of the European Union under the 7th Framework Programme PEOPLE - Marie Curie Actions[3] and by the Department of History, Cultures and Religions of the *Sapienza*, University of Rome[4], in collaboration with the *Museo Galileo* in Florence, formerly *Istituto e Museo di Storia della Scienza*[5]. The project is hosted by the *Sapienza*'s History Department and is developed by me and is directed by Renata Ago, full professor of Early Modern History[6], with the technological support of Andrea Scotti for the *Fondazione Rinascimento Digitale - New Technologies for Cultural Heritage*, Florence.[7]

3. *EoS* aims at carrying out the prototype of a common web infrastructure that takes into account evidence gathered from scientific correspondences so as to define and highlight the contents the networks and the ways of transmission of scientific information in the north-east periphery of early modern Europe.

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[2] Cf. 'Les correspondances: leur importance pour l'historien des sciences et de la philosophie. Problèmes de leur édition', *Revue de Synthèse*, 81-82 (1976), especially the essays by R. Taton (p. 7-22) and P. Dibon (p. 31-50); H. J. M. Nellen, 'La correspondance savante au XVIIe siècle', *XVIIe siècle*, 178 (1993); A. Johns, 'The ideal of scientific collaboration: the «men of science» and the diffusion on knowledge', in H. Bots, F. Waquet eds., *Commercium litterarium: la communication dans la république des lettres, 1600-1750*, Amsterdam 1994, p. 3-22; R. Chartier ed., *La correspondance: les usage de la lettre au XIX siècle*, Paris 1991; D. S. Lux, H. J. Cook, 'Closed circles or open networks? Communicating at a distance during the scientific revolution', *History of Science*, 36 (1998), p. 179-211; C. Berkvens-Stevelinck, H. Bots, J. Häseler eds., *Les grands intermediaires culturels de la république des lettres: études de réseaux de correspondances du 16. au 18. siècles*, Paris 2005; L. Giard, A. Romano, 'L'usage jésuite de la correspondance. Sa mise en pratique par le mathématicien Christoph Clavius (1570-1611)', in A. Romano ed., *Rome et la science moderne. Entre Renaissance et Lumières,* Rome 2008, p. 65-119. A list of the electronic editions of scientific correspondence on the web is already not easy to draft. Besides the projects showed in the *Scientific Correspondence* panel of the 4th ICESHS Symposium, see, at least, *Cultures of knowledge. An Intellectual Geography of the Seventeenth-Century Republic of Letters*, developed by the University of Oxford (http://www.history.ox.ac.uk/cofk/) and the web infrastructure *Mapping the Republic of Letters Exploring correspondence and Intellectual Community in the Early Modern Period (1500-1800)*, supported by Stanford Humanities Center (https://republicofletters.stanford.edu).

[3] http://cordis.europa.eu/fp7/mariecurieactions/home_en.html

[4] http://www.dipscr.uniroma1.it/

[5] http://www.museogalileo.it/

[6] http://w3.uniroma1.it/dsmc/index.php?q=node/38

[7] http://www.rinascimento-digitale.it/fondazione.phtml

More in detail, it aims at:

1. 1 map the network of scientific networks
2. 2 pick-up from letters “quanta” of scientific information
3. 3 follow the information flows on the network

4. To achieve these aims means in practice:

4.1. Populate the existing web infrastructure by means of:

- building a repository of letters
- defining the letters’ extrinsic information (sender, addressee, place origin, place of destination, date);
- defining the letters’ internal information (people, books, instruments, manuscripts, maps, related letters, observations etc.);

4.2. Establish the relations among all found sources in order to be able to navigate across. For instance: expanding search for *ad hoc* combined items could give answers to queries like: who was in 1680 in possession of Hevelius’ *Selenographia* and where (book - date - place)? Which way did Eustachio Divini’s telescope pass from Rome to Warsaw in 1661 (object - dates in between - places)?

4.3. Project these relations on a geo-historical map. This step is still undeveloped.

5. As to time, *EoS* looks at the second half of the 17th century; the age when, in England but on the continent too, «the accumulation of individual matter of facts, socially checked and shared, became the only undoubted foundation of a reformed philosophical knowledge of nature, and scientific correspondences became a selective mean to let the experimental report and the observation’s result pass from the individual domain to the public sphere»[8].

6. As to space, it takes into account the geographical area included in the triangle Rome - Florence - Paris - Danzic, with special respect of the Mitteleuropa areas, that is Poland and the Habsburg Empire’s «periphery» (the yellow area in Figure 1).

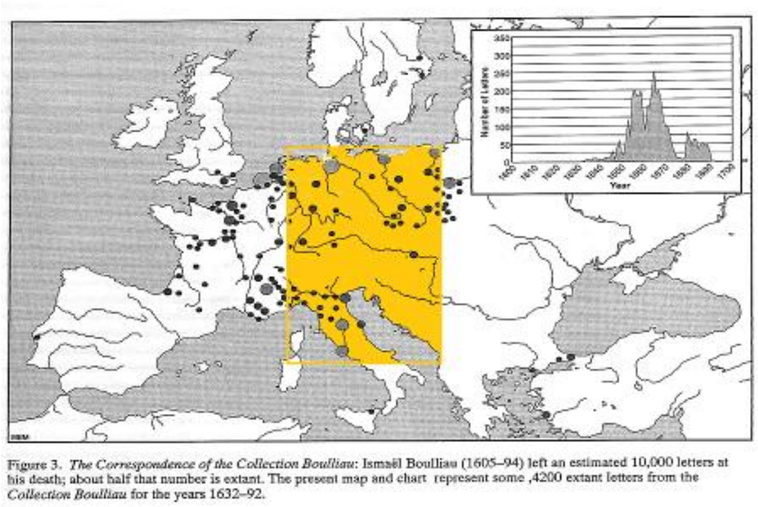

Fig. 1: Source: R. A. Hatch, ‘Between Erudition and Science: the Archive and Correspondence Network of Ismaël Boilliau’, in M. Hunter ed., *Archives if the Scientific Revolution*, Woolbridge 1998, p. 54.

[8] S. Shapin, *The scientific revolution*, Chicago 1996. Cf. also S. Shapin, S. Schaffer, *Leviathan and the air-pump: Hobbes, Boyle and the experimental life*, Princeton : Princeton University Press, 1985; ID. *A social history of truth: civility and science in seventeenth-century England*, Chicago ; London, 1994.

The choice of this geographical area wants to challenge one *topos* of the historiography on the "Republic of letters". While its citizens postulated the ideal of a men-of-letters' world-wide community without frontiers, its actual extent was much less wide than this, limited as it was only to some regions of the Western world. The European *savants* themselves were aware of this discrepancy. In the note to the reader that opened the first issue of the *Journal des savants* (1665), we read: «this Journal, since it aims to advertise what new is going on within the «République des Lettres», will consist of a faithful catalogue of the main books published in *Europe*» (our italics). The «République des Lettres» did not overlap even the whole Europe: listing the countries that provided the books to review, the *Memoires de Trévoux* drew a map that covered only the Continent's Western side. That same year, 1701, the botanist Joseph Pitton de Tournefort (1656-1708) besought the *Royal Society* of London secretary for epistolary contacts with the world of the learned men. Working in the east-Mediterranean countries –he wrote– he was eager for news from the «République des Lettres».

According to two of the main experts of this subject (Hans Bots and François Waquet): «the scanty number of men-of-letters in Transylvania, Moldavia and Valacchia bears witness to the low intellectual density of those countries and to the rarity of their contacts with the West»[9]. Bearing in mind this authoritative judgement, the research we propose here begins with some specific questions: is the scanty density of men-of-letters and learned men accounted for in the 17th century Eastern and Central Europe [see Figures 2 and 3] a matter of fact, or rather it derives from a lack of sources or from inaccurate census rules? And, again: the flow of learned relation between Western and Eastern Europe was indeed a «limited and one-way» flow or rather it was a thick bilateral exchange, as does the map in the picture, drawn by Robert Hatch on the basis of Ismaël Boilliau's correspondence, suggest? [see Figure 1].

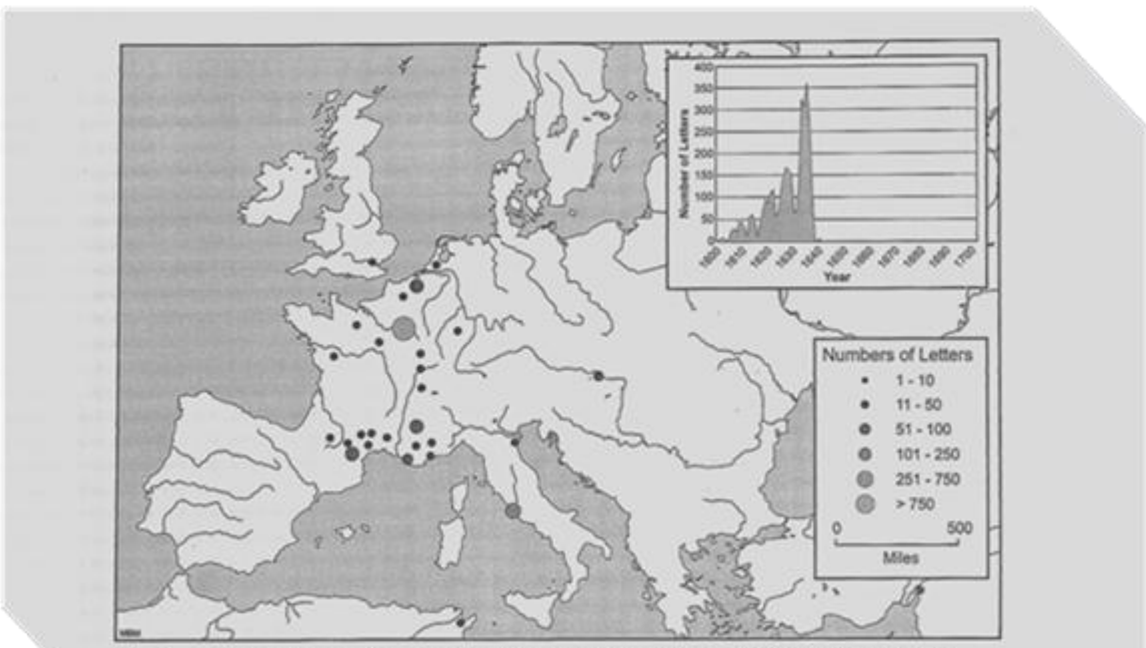

Figure 1. *The Published Correspondence of Peiresc*: Nicolas-Claude Fabri de Peiresc (1580–1637) left an estimated 10,000–14,000 letters at his death; perhaps half that number is extant. The present map and chart represent some 3,200 published letters for the years 1598–1637.

Fig. 2: Source: R. A. Hatch, 'Between Erudition and Science: the Archive and Correspondence Network of Ismaël Boilliau', in M. Hunter ed., *Archives if the Scientific Revolution*, Woolbridge 1998, p. 52.

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9 H. Bots, F. Waquet, *La République des Lettres*, Paris 1997 [it. ed. Bologna 2005, p. 97, my English]. On self-representation of the learned community: L. Daston, 'The ideal and reality of the Republic of Letters in the Enlightenment', *Science in Context,* 4, (1991), p. 367–386. On the recent historiographical revival of the subject: G. Pancaldi, 'The Republic of Letters in transition: William Thomson and natural philosophy ca. 1850, in The Global and the Local: The History of Science and the Cultural Integration of Europe'. *Proceedings of the 2nd ICESHS (Cracow, Poland, September 6–9, 2006)*, Ed. by M. Kokowski, p. 49ff. [*http://www.2iceshs.cyfronet.pl/2ICESHS_Proceedings/Chapter_2/Plen_Lec_Pancaldi.pdf*]. See also: R. Hatch, 'The republic of letters : Boulliau, Leopoldo and the Accademia del Cimento', in M. Beretta, A. Clericuzio, L. Principe eds., *Accademia del Cimento and its European context*, Science history publications 2009, p. 165-180.

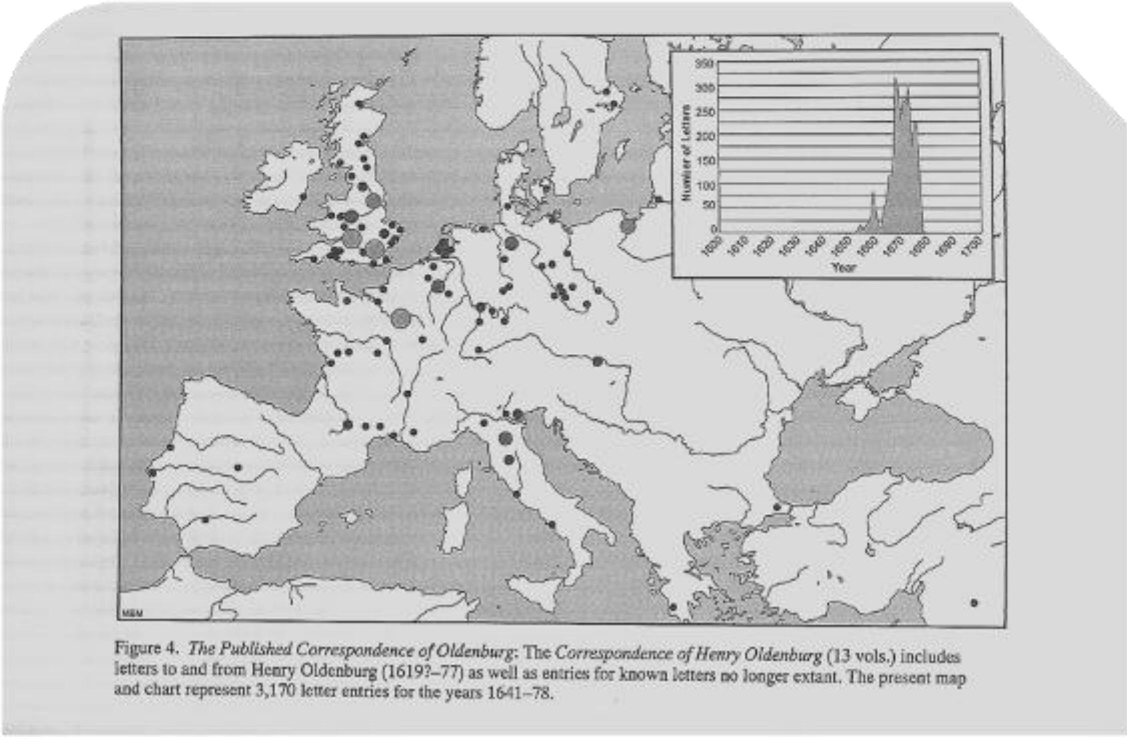

Figure 4. *The Published Correspondence of Oldenburg*: The *Correspondence of Henry Oldenburg* (13 vols.) includes letters to and from Henry Oldenburg (1619?–77) as well as entries for known letters no longer extant. The present map and chart represent 3,170 letter entries for the years 1641–78.

Fig. 3: Source: R. A. Hatch, *Between Erudition and Science*, cit., p. 56.

The *EoS* project means to contribute an answer to these questions by exploiting the web tools: the research aims at reconstructing the networks of the scientific relations which in the second half of the 17th century linked "scientists" and *amateurs* who lived and worked in the western European countries of high intellectual density –that is, France, England, the Netherlands and Northern Italy– and those who lived especially in Poland, Bohemia, Moravia and Hungary.

7. Sources. More in detail, *EoS* project focuses on the space cartographed by letters from and to prince Leopoldo de' Medici and the members of the Academy of the Cimento in Florence (1657 and 1667),[10] crossed with the scientific epistolary produced by Jonannes Hevelius, Polish astronomer, brewer and mayor, who lived and worked in Danzig from 1634 until the end (1687).[11]

These sources have been selected *pour cause* in order to realize our tentative project; they fulfil different criteria, which are scientific, juridical, logistical.

A. They are scientifically promising: by crossing the trajectories even of a sample of letters we can draw the threads of a network that, on the one hand, overlaps the white geographical area I spoke before; on the other, they cross other correspondences networks –like Huygens'[12], Boilliau's[13] or Oldenburg's[14]– already edited or under edition. In a word, they promise to give birth to a wide and thick "network of networks";

[10] M. Beretta, A. Clericuzio, L. Principe eds., Accademia del *Cimento and its European context*, Science history publications 2009, p. 165-180 (with former bibliography).

[11] J. H. Westphal, Leben, *Studien und Schriften des Astronomen Johann Hevelius*, Königsberg 1820; K. Targosz, *Jan Heweliusz, uczony – artysta*, Wrocław 1979; R. Przemysław, *Heweliusz*, Warszawa 1989; R. Głe bocki, A. Zbierski eds., *On the 300th anniversary of the death of Johannes Hevelius. Proceedings of the International Conference, Gdańsk, 14 - 16 Sept. 1987*, Wrocław, 1992; A. H. Cook, 'Johann ed Elizabeth Hevelius, astronomi di Danzica', in R. Simili ed., *Scienza a due voci*, Firenze 2006, p. 1-11.

[12] *Oeuvres complètes de Christiaan Huygens*, Publiées par la Société hollandaise des sciences, La Haye 1888-1950, voll. 1-10. Huygens' correspondence on the web is available in the Catal*ogus Epistolarum Neerlandicarum* (*http://picarta.pica.nl*).

[13] R. A. Hatch, *The collection Boulliau (BN, FF. 13019-13059): an inventory*, Philadelphia 1982. An electronic edition of Boilliau's correspondence is under construction thanks to R. A. Hatch (*http://www.clas.ufl.edu/users/ufhatch/pages/11-ResearchProjects/boulliau/index.htm*).

[14] *The correspondence of Henry Oldenburg*, edited and translated by A. Rupert Hall & Marie Boas Hall, 13 voll., Madison - London 1965-1986.

B. In the last decade, the manuscripts of the Galilean collection, and among them Leopoldo's letters, have been the object of a long-lasting and careful work of description, cataloguing, digitalization and transcription. The copyright of this huge data-base is shared by the National Library in Florence[15] and by the *Museo Galileo*; and the *Museo Galileo* choose to share it with *EoS* project.

C. On the contrary, the 15 volumes of letters written and received by Hevelius in his long career lie still manuscript and un-catalogued in the Library of the Paris Observatory. A brief inventory of this collection can be found only in the Inv*entaire général et sommaire des manuscrits de la Bibliothèque de l'Observatoire de Paris* by Guillaume Bigourdan (1895), a bibliographical rarity now available on line on the Observatoire's home-page, among the resources managed by Alidade.[16] EoS aims at putting the Hevelius collection on its best use –as to availability and visibility– by exploiting the web resources.[17]

Fig. 4: J. Hevelius (1611-1687).

Fig. 5: Baciccio, *Portrait of Cardinal Leopoldo de' Medici* (1667), Florence, Uffizi Gallery.

8. EoS technology: Pinakes 3.0. *EoS* chose to carry out the classification and analysis of the scientific correspondence in 1600 Europe within the shared web environment *Pinakes 3.0*. The *Pinakes* project is the result of a long term activity within the research framework of the *Museo Galileo* in Florence, focused to facilitate the digital production of knowledge on the web within the domain of Science History. Currently, *Pinakes* is one of the main research projects of the

[15] See: http://www.bncf.firenze.sbn.it/pagina.php?id=43

[16] http://alidade.obspm.fr/sdx/alidade/toc.xsp?id=FROBSPM-ALIDADE-MANU00002_G591&qid=sdx_q0&fmt=alidade-manuscrits&idtoc=FROBSPM-ALIDADE-MANU00002-pleadetoc&base=fa&n=1&ss=true&as=true&ai=archives|

[17] Another project focusing on Hevelius' correspondence started at the same time as EoS. It is supported by the CRIT- Cultures, Régulations, Institutions et Territoires of the University of Versailles – Saint Quintin en Yvelines, and is directed by Chantal Grell. Unlike EoS, the CRIT project aimed at editing Hevelius' epistolary on paper.

Fondazione Rinascimento Digitale. The main scope of this project is to provide an open and free downloadable application customizable for the different disciplinary tasks of humanities research.[18]

9. Why Pinakes3? Why choose *Pinakes* as the environment for this project? First of all, because everything produced in *Pinakes* can be exported to any other application. In fact, *Pinakes* stores data in the suggested standard RDF/OWL which allows any other application to see and re-use all information. Secondly, because every knowledge domain can shape its own data structure as requested in *Pinakes*. For instance, what kind of classification can be carried out in the case of the drawing of an instrument quoted in a letter, that eventually could be located in a particular museum, that has been used for particular experiments in many different places of Europe and which other sources also refer to? Here you have a string of different references: a letter, that is a manuscript; an instrument, which is an artefact; an experiment, that is an action; some other references, that could be book engravings or else. Each of these objects has a classification tradition. *Pinakes* allows you to use all the concepts of all these traditions in the same place in order to satisfy each discipline's requirements. At the same time, you will be able to establish relations among all the included objects in an explicit way, so much so that from a highly structured environment you will be able to reconstruct the complex narrative embedded in historical primary sources.

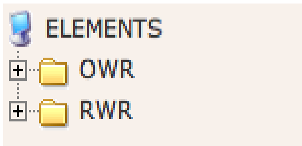

Fig. 6: 1- Elements: The root: abstract class without domain; 2- OWR: Object world reification: the four foundational classes; 3- RWR: Relation world reification: all possible service classes.

10. Pinakes "root" (that is its "upper ontology") *Pinakes* project aims at offering a common methodology of structuring data within the humanities. This means that the long term target is to reach, at least in its foundational concepts, the sharing of a "root" that enables any kind of possible specialization. The foundational schema, i.e. the common root and service classes of *Pinakes*, is structured as follows:

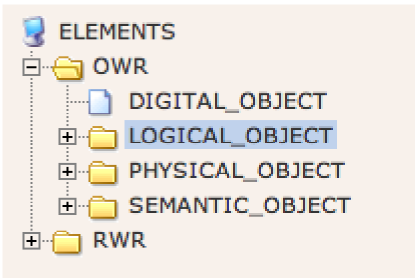

Fig. 7: The four main super classes.

1. The four main super classes with basic attributes:

   1.1. LOGICAL OBJECT: Is a collection of *semantic objects* and is used to describe objects of the reality of which it is possible to have experience/knowledge but that has no physical extension. For example: if you want to represent the events of the life of a scientist you will have a time line among which the facts will be represented into a description. The same thing would be needed if you want to reconstruct the history of a concept such, for example, that of motion or matter. Obviously, each *logical object* can

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[18] http://pinakes.imss.fi.it/index.php/Main_Page; A. Scotti, 'Nuove metodologie e tecnologie nella storia documentaria della scienza : l'esempio di Pinakes', in M. T. Monti ed., *Antonio Vallisneri: l'edizione del testo scientifico d'età moderna. Proceedings, Scandiano, 12-13 ottobre 2001* - archiviazione, lo studio e l'interrogazione di documenti digitali di cultura', in I. dal Prete, D. Generali, M. T. Monti eds., *Le reti in rete* [electronic resource]: *per l'inventario e l'edizione dell'Archivio Vallisneri*, Firenze 2011, p. 31-52.

have relation with a *physical object*, but also the latter can be knowable only due to a *semantic object*, i.e. there is no possible knowledge without a whatsoever given denotation.

1.2. PHYSICAL OBJECT: is any object of realty that has an extension whose attributes can vary due to the different needs of the single disciplines/projects. The attributes of this object are therefore the basic ones of any possible physical object: matter, dimensions, property. The latter is also a foundational attribute because no existing object been placed somewhere has a juridical status of property: a landscape belongs to a state in the same way that an artifact belongs to a museum or private individual.

1.3. SEMANTIC OBJECT: is the denotation of any possible physical or logical objects. The attributes of such object also vary by means of the knowledge domain. For example a book has a physical description in measure, number of pages etc. but is knowable only due to its title (our semantic object). An archivist will describe such object in a way that can be very detailed whereas an historian would not need all those details but simply a reference record. These variations in number and type of attributes will not interfere with the fact that the title of the archivist and that of the historian belong to the same class of semantic object, so both would be found by the browsing.

1.4. DIGITAL OBJECT: this is a special class that has been set here (among the foundational classes) for simplification reasons. In fact whereas all other classes are abstract and can be specialized, this one in not abstract (meaning that it can have instances) and cannot be specialized. This class describes the main required attributes of any born digital object, for example its weight (KB, MB, GB etc.), its extension (mime type), its URL, etc. And is always a range of any possible *semantic object*. In this way all digital collection can be grouped by means of their description but also by means of their attributes and can be browsed as such independently of their denotation, for example by a pattern recognition engine.

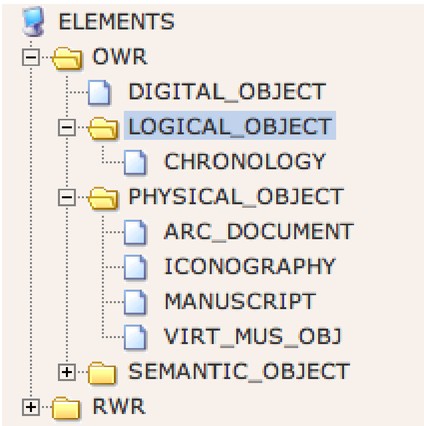

Fig. 8: This is how the root of *Pinakes* foundational ontology has been specialized in order to classify all the possible objects to be found in the scientific correspondence of *EoS* network.

2. The four main service classes:

2.1. ACTOR: this is a super-class that represents all possible typologies of actors (human, animal, fictive, physical etc) and can be specialized.

2.2. ACTION: this is a super-class that represents all possible typologies of actions (voluntary, involuntary, processes, flux etc.).

2.3. TIME: any portion or point of any given time line with all its possible representations.

2.4. SPACE: any knowable area or extension, with all possible declinations from measurements to toponyms.

2.5. SIMPLE TYPES: this class groups all the so-called primitives, i.e. all those types required to make machine readable information such as: String, Integer, Float, etc. This class cannot be specialized because it already includes all required type for any machine usable at the time being.

11. Pinakes 3.0: short functional description. The *Pinakes 3.0* application is distributed under the regulation of the so-called General Public License and undergoes the rules of the copy-left. This means that anyone interested in using it as it is will be authorized to do so, and that anyone interested in participating to its further development will be authorized to do so by taking part in the *Pinakes 3.0* developer and user community.

We are going to show here three of main functional in which this application is divided:

1. The *management area* where, on the basis of the existing methods in the application (an upper-foundational ontology), the user can define using its own taxonomy (a set of concepts depending on the discipline) –in the case presented here the History of Science– by means of the correspondence which implies:
   - how detailed the description of the objects should be;
   - what type of relations objects should have with other objects;
   - what kind of indices would be used and where or how to create them;
   - what should be published on the web and what should not;
   - which data can be shared and which not;
   - who does what and at which level of authorization.
2. The *classification area* where the user can:
   - input data using its own definitions and, in case it is needed, re-edit them according to a given method;
   - refine the definition of information and knowledge domain according to new needs found along the research activity;
   - browse and use data belonging to other projects that may be relevant for his/her on own research;
   - use and/or offer other projects relations and indices definition if needed;
   - get access to the digital repository of all projects and use, under the Intellectual Property constrain, all digital objects (images, moving pictures, music, etc.);
   - define a data navigation for the purpose of editing them;
   - get access through the web to other databases in order to capture data and import them in a given project (this according to the rules of Intellectual Property);
   - access external (out of the *Pinakes* project) digital repositories to capture new resources or refer to them through the automatic creation of a web address (this according to the rules of the Intellectual Property and copyright);
3. The *navigation area* where the user can:
   - get access to all data produced controlling the publication of his own data and, if needed, go back to the input to edit them.

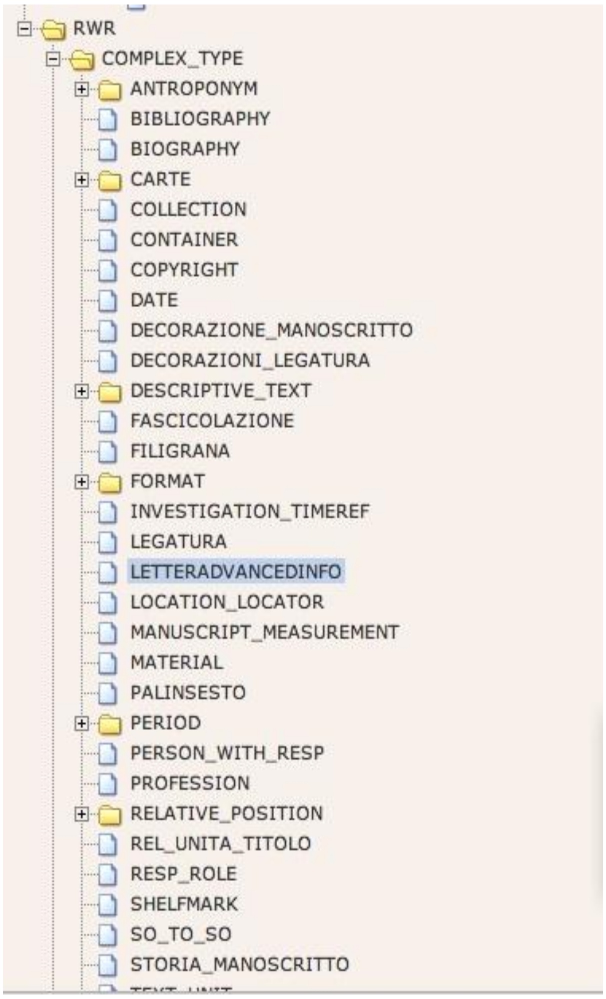

Fig. 9: The specialization of the Relation World Reification class, i.e. the so called service classes that allow describing all what the project requires. Here only a part of the class tree is visible.

12. The structure of EoS data in Pinakes 3.0: some input samples

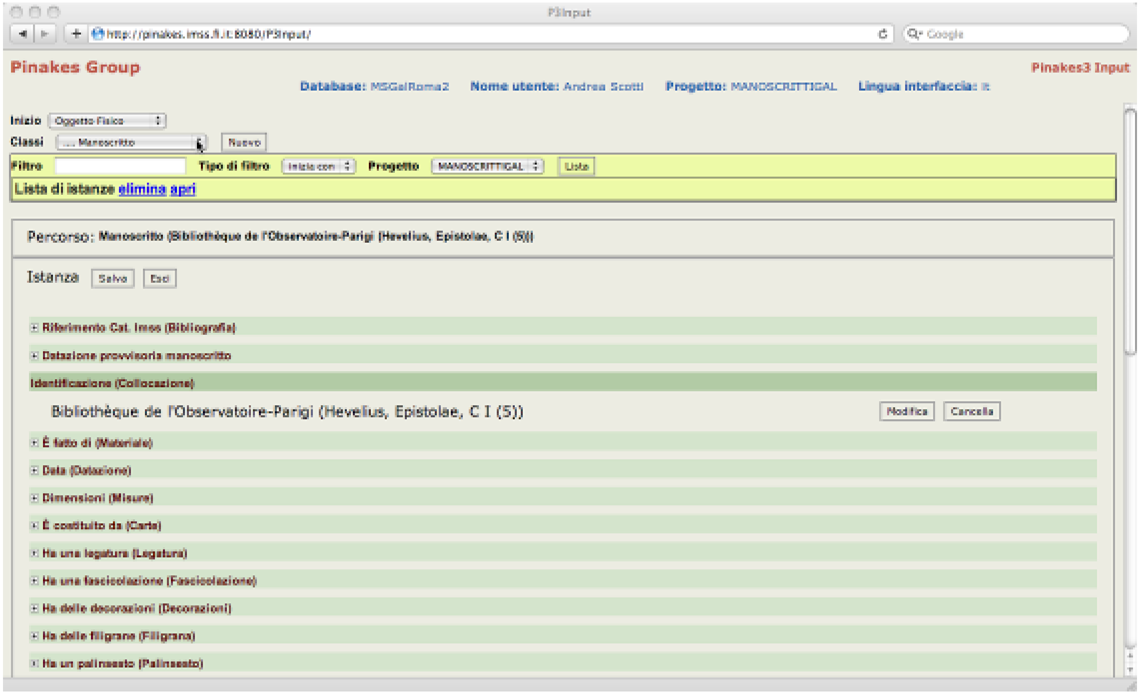

Fig. 10: Input data form for manuscripts description.

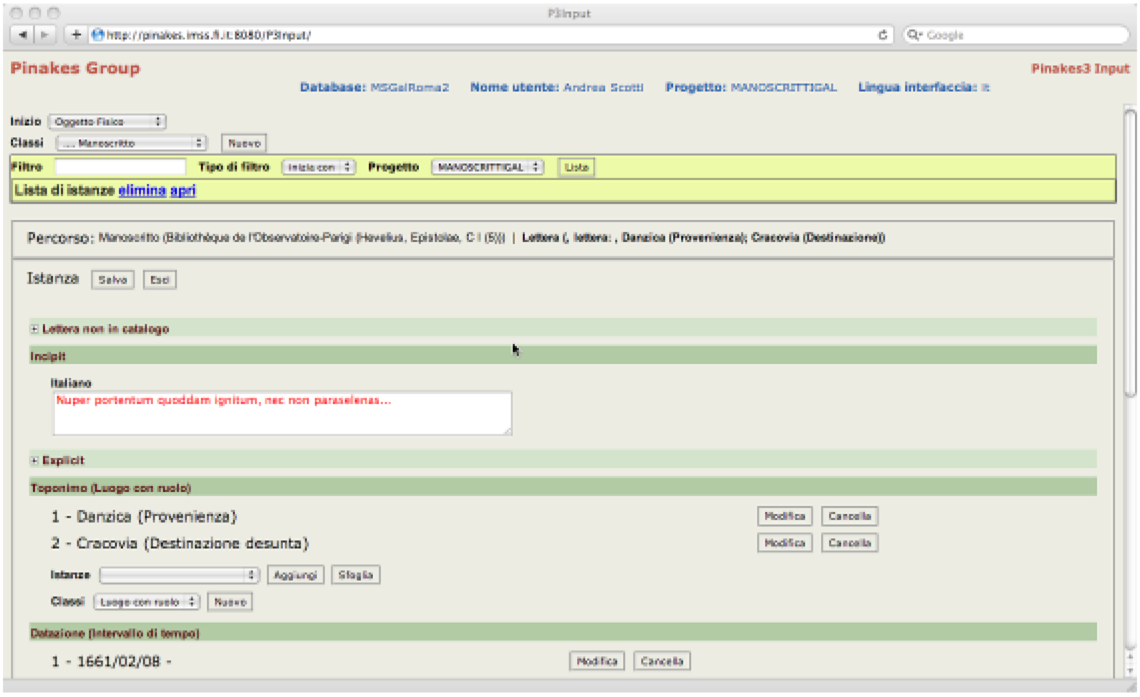

Fig. 11: Input data form for letter's extrinsic information (sender, addressee, place of origin, place of destination, date).

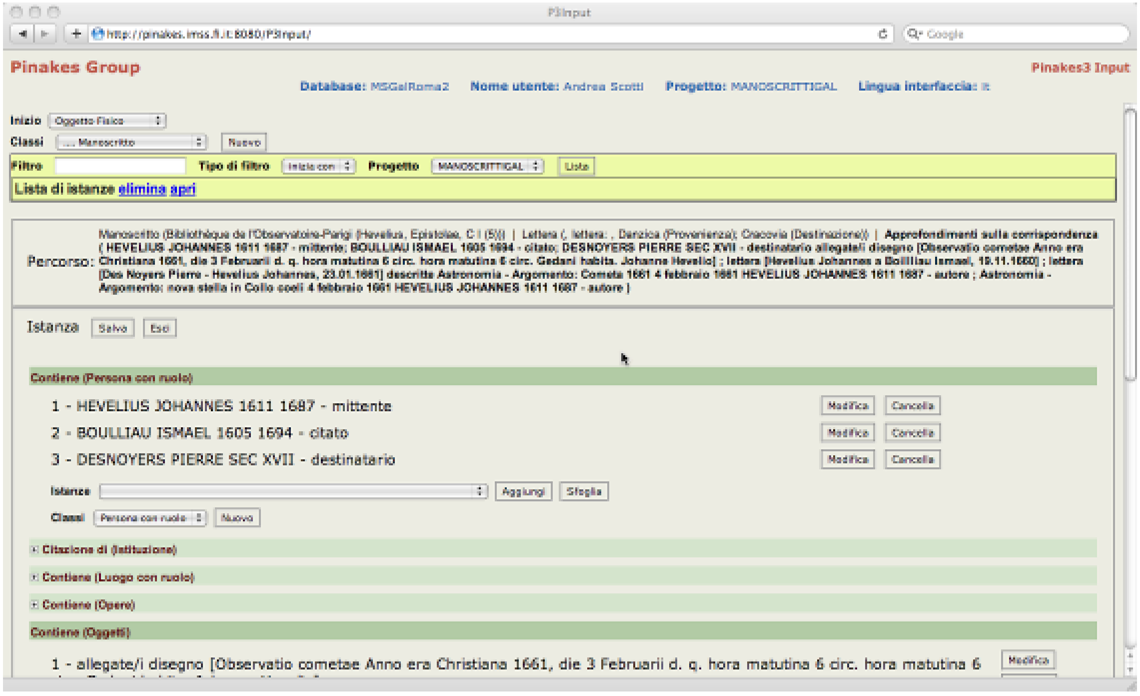

Fig. 12: Input data form for letter's internal information (people, books, instruments, manuscripts, maps, related letters, observations etc.) [top].

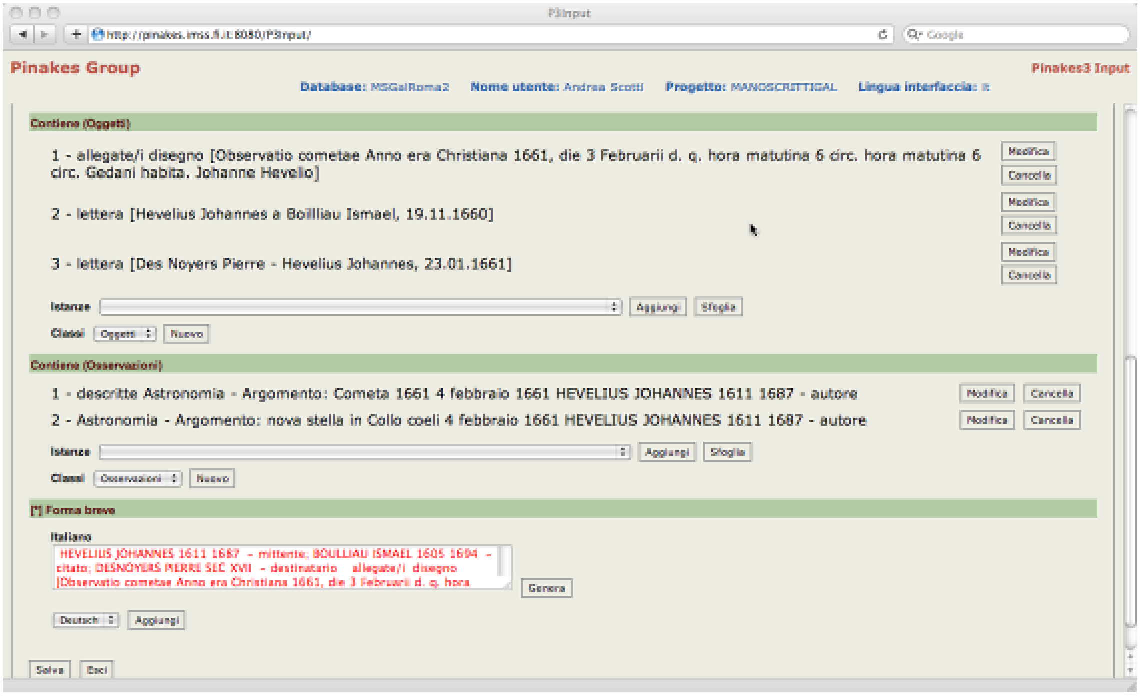

Fig. 13: Input data form for letter's internal information (people, books, instruments, manuscripts, maps, related letters, observations etc.) [bottom].

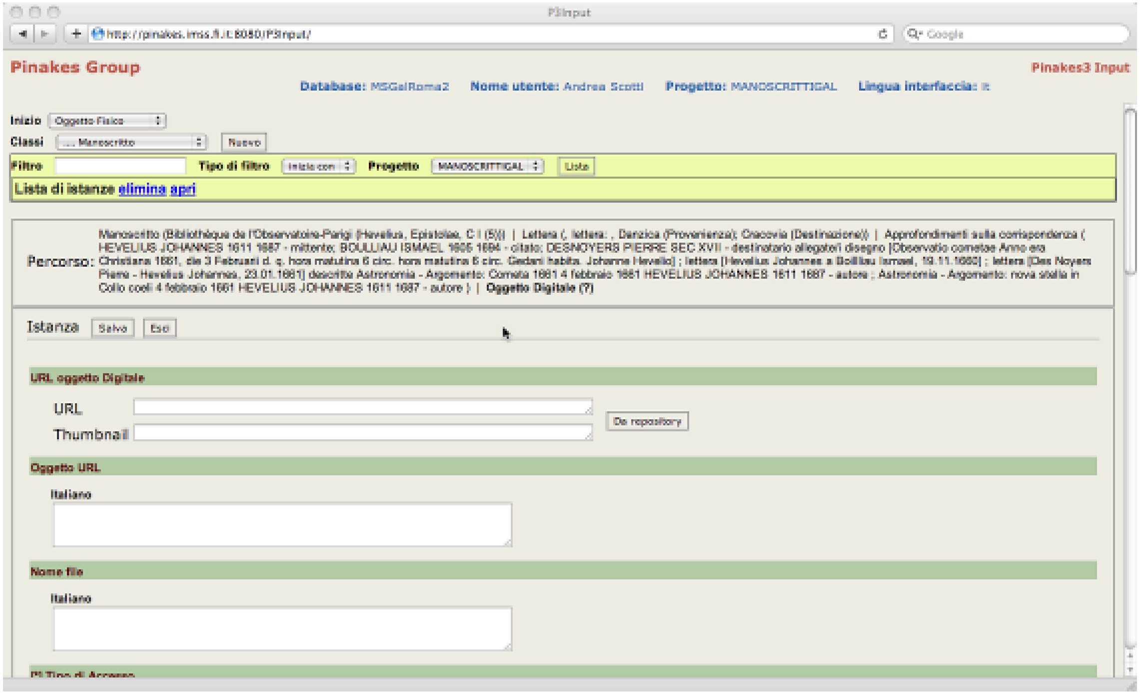

Fig. 14: Input data form for letter's digital copy (1).

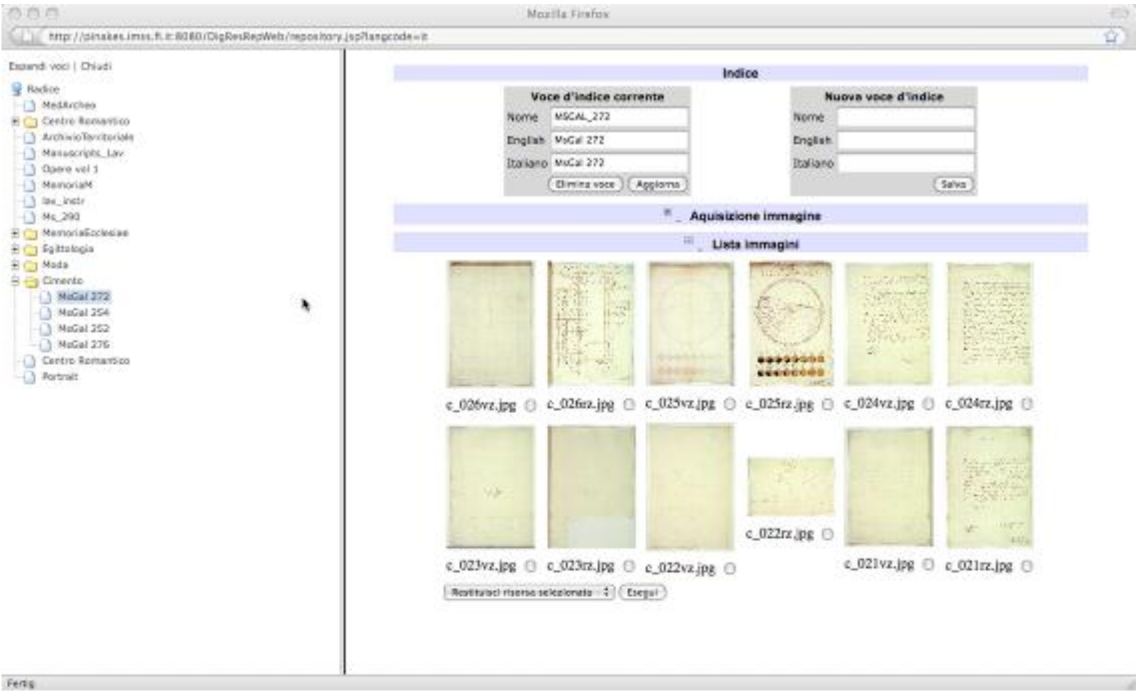

Fig. 15: *Pinakes* repository of the digital copies of the Galilean Fund, at the National Library of Florence.

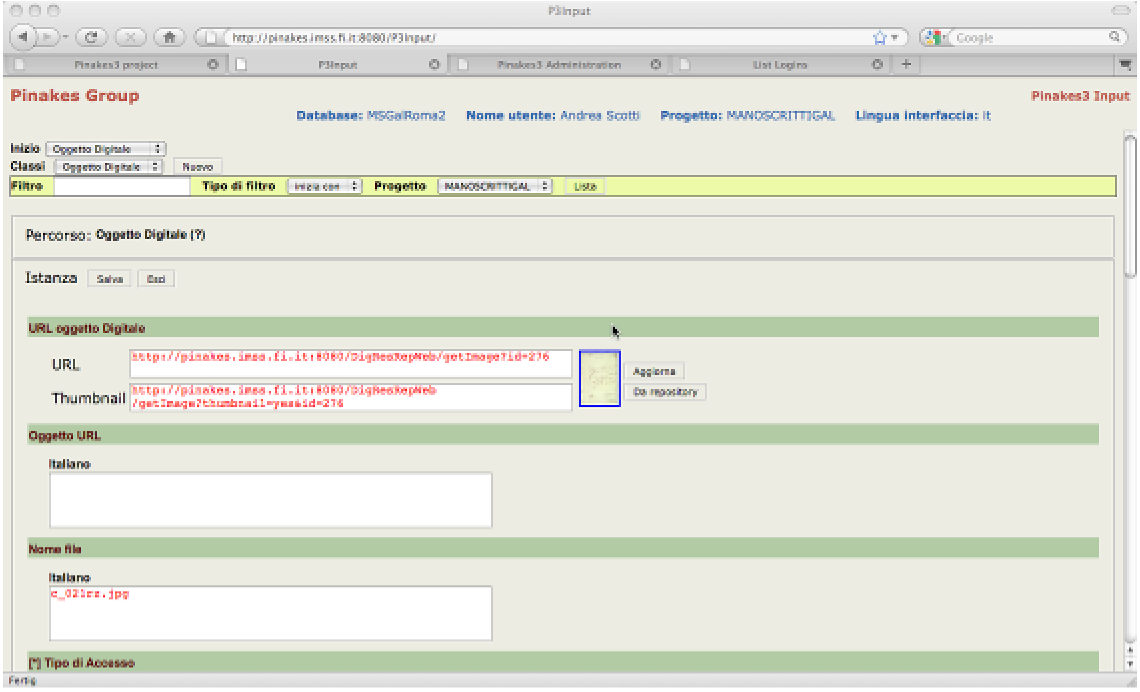

Fig. 16: Input data form for letter's digital copy [2]. In this case, it has been picked-up from internal repository.

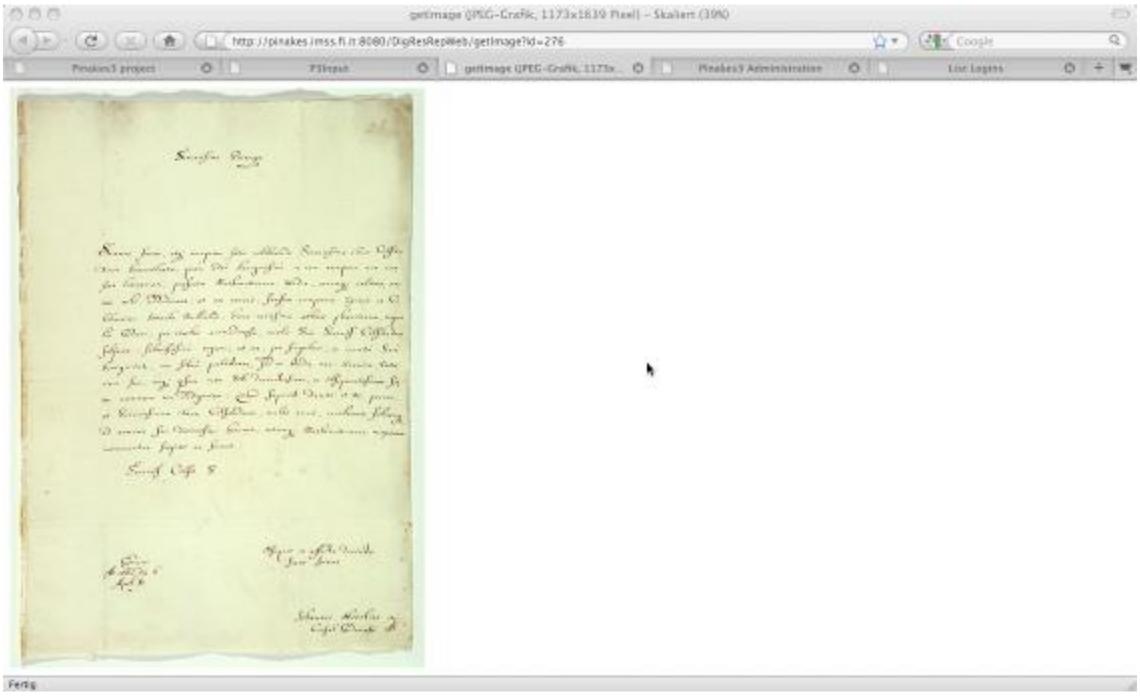

Fig. 17: Digital copy of a catalogued letter.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 920.

NEGOTIATING MEDICAL AUTHORITY IN EARLY MODERN PHYSICIANS' EPISTOLARY NETWORKS

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Abstract

In 16th- and 17th-century medicine letter writing played a crucial role. Numerous physicians participated in extensive epistolary networks. Their letters covered a wide range of topics, from new theories and empirical findings to political and confessional controversy, from the challenges and rewards of medical practice to domestic affairs. They were much more than a means of communication, however. In the absence of scientific periodicals, epistolary networks constituted the principal public sphere in which the success or failure of new books and ideas was decided upon and in which some authors rose to the position of an authority while others remained controversial or were marginalized. In their letters, early modern physicians could exchange their opinions and criticism of individual authors and books much more quickly, widely and openly than in books. This paper will draw on a sample of more than a thousand manuscript letters written, for the most part, by physicians –some famous, some hardly known– in the German-speaking areas between 1530 and 1650. The letters are taken from a database which is at the core of a 15-year project on "Early modern physicians' letters" started in early 2009 at the Institute for the History of Medicine in Würzburg under the auspices of the Bayerische Akademie der Wissenschaften *which will ultimately provide access to many thousands of early modern physicians' letters via OPAC. As the paper will show, a thorough analysis of such letters offers unique insights into processes involved in the creation of new medical authorities and into the relative importance of criteria such as originality, learning and style, association with established authorities or intra-professional and moral conduct which prompted the appreciation or rejection of an author and his work.*

In recent work on the history of science and medicine, the processes and strategies involved in the construction, confirmation and rejection of scientific authority have attracted considerable interest. Examining the rhetoric of scientific texts and illustrations, the strategic function of footnotes, prefaces, dedicatory epistles and similar devices has become a very fruitful and increasingly sophisticated field of historical inquiry. And last but not least the impact of communicative practices on scientific authority has undergone closer scrutiny. Successful "networking", to use a modern term, has been identified as a crucial strategy for the acquisition of authoritative status.[1]

Especially in the 16th and 17th centuries, in the virtual absence of scientific periodicals, epistolary networks constituted a major forum in which the success or failure of new books and ideas could be decided upon and in which some authors rose to the position of an authority while others remained controversial or were marginalized. Letter writers could also exchange their opinions and criticism of individual authors and their works much more quickly, widely and openly than in books thus providing the historian with a more immediate access to the actual processes and criteria involved in the attribution and denial of authority.

For historians of early modern medicine, correspondences offer a particularly interesting and revealing source in this context. Firstly, medical authority was at the time in certain respects still very precarious. Frequently it rested to a considerable degree upon the physician's fame as a medical practitioner. But diagnostic and therapeutic skills were much more difficult to validate and much more open to debate than, say, empirical observations. In retrospect, there is little reason to believe, in fact, that the most famous physicians of the time had much better therapeutic outcomes than the most humble barber-surgeons. Nevertheless, some physicians achieved international renown as highly skillful practitioners and their advice was sought eagerly be kings and princes all over Europe while others fell into disrepute. Medical authority in medicine, this implies, was to a very high degree the result of successful professional self-fashioning.[2]

Early modern physicians' correspondences: a new data-base

In what follows, I will present the first results of a survey of about 3,000 physicians' letters written between 1500 and 1650. These letters come mostly from a data base of the correspondence of early modern physicians in the German-speaking areas of early modern Europe, which was started in Würzburg in 2009 under the auspices of the *Bayerische Akademie der Wissenschaften*.[3] The aim of this project is to identify and make accessible 10,000s of letters written by and to doctors in the German-speaking areas in that period. In many cases, the database offers detailed freely searchable summaries of the letters' contents as well as the names, works and places which are mentioned. By the end of 2011, the database is to go online. Scholars from all over the world will then have access via internet and free of charge to the data and the summaries of the contents of an increasing number of letters. They will be able to search for specific names or subjects, such as "Paracelsus", "town physician", "*aurum potabile*" or "eucharist" and in some cases they will also be able to download a digital image of the original letter in question.

Letter writing

So what can we learn from early modern physicians' letters about the ways in which medical authority was established, strengthened, questioned or rejected at the time? First of all, a major feature of early modern epistolary networks –as of most networks– were processes and strategies of inclusion and exclusion. Becoming part of an epistolary network –moving in the "right circles"– already constituted an important step towards the acquisition of authority –and it could pose a serious challenge. For authors who had already acquired fame thanks to their printed works or to their position at a renowned university or court it was fairly easy. Here the major question was whether mutual interests justified the effort (and expense) involved in maintaining a correspondence. But often a young and/or unknown physician turned to an older, more famous and experienced physician –and here the success was much less certain. The early modern "medical republic of letters" as it was occasionally called at the time, was far from open and egalitarian. Rather, the processes and strategies resembled in many ways those familiar from systems of clientelism. Just as in the case of personal visits, the "lower-ranking"

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1 To cite just a few "classics": Steven Shapin/Simon Schaffer, *Leviathan and the air pump. Hobbes, Boyle, and the experimental life*. Princeton 1985; Bruno Latour, *Science in action. How to follow scientists and engineers through society*. Cambridge, MA 1987; Steven Shapin, *A social history of truth. Civility and science in seventeenth century England*. Chicago/London 1995; good overview in Mario Biagioli (ed.): *The science studies reader*. New York/London 1999.

2 On the situation on the highly competitive early modern medical market see e. g. Roy Porter/Dorothy Porter, *In sickness and in health. The British experience, 1650-1850*. New York 1989; Robert Jütte, *Ärzte, Heiler und Patienten. Medizinischer Alltag in der frühen Neuzeit*. München/Zürich 1991; Gianna Pomata, *La promessa di guarigione. Malati e curatori in antico regime. Bologna XVI-XVIII secolo*. Bari 1994; Laurence W. B. Brockliss/Colin Jones, *The medical world of early modern France*. Oxford 1997; Michael Stolberg, Medizinische Deutungsmacht und die Grenzen ärztlicher Autorität in der Frühen Neuzeit, in: Richard van Dülmen/Sina Rauschenbach (eds.), *Macht des Wissens. Entstehung der modernen Wissensgesellschaft 1500–1820*. Köln/Weimar 2004, pp. 113-130; idem, Formen und Strategien der Autorisierung in der frühneuzeitlichen Medizin, in Wulf Oesterreicher/Gerhard Regn/Winfried Schulze (eds.), *Autorität der Form – Autorisierungen – institutionelle Autorität*. Münster 2003, pp. 124-152.

3 URL: *http://www.medizingeschichte.uni-wuerzburg.de/akademie/index.html*. The team is headed by the author and currently consists of Dr. Ulrich Schlegelmilch, Dr. Tilman Walter, Simone Herde M.A., Anne Rappert-Sälzer and Svenja Wenning M.A.

physician usually needed the recommendation of an established physician who was already part of the network. Even then letters of introduction would praise the prospective correspondent lavishly, mention if possible some other renowned personalities he was already acquainted with and offer his services –for example in procuring books or plant specimens. Sometimes a young or unknown physician also simply wrote to a famous physician, without anyone to recommend him. Surviving letters, in which the writer mentioned or complained about not having received an answer to his first attempt or in which a physician regretted in retrospect that he had not accepted and opened a letter which had been sent to him suggest that indeed many if not most letters which lacked personal recommendation remained unanswered and ended up in the chimney fire.

For those who made it, becoming part of an epistolary exchange with more or less famous personalities, could entail a considerable boost to their authority. Letter-writing was much less a private affair than today. Letters were shown around. Sometimes –indeed this was very common practice– correspondents would also ask to greet friends or acquaintances, which allowed the lower-ranking physician to underline that he had personal contact with a well-known authority. Lower ranking physicians could also use epistolary contacts to ask a well-established authority for medical advice or even obtain a council for individual patients from him, which would raise their standing among their patients. Their local physician, the patients were made to understand, personally knew one of the famous personal physicians of this or that duke or prince – who, with all the money he had, undoubtedly accepted only the best medical care he could find.

Sometimes physicians might even publish the letters they exchanged with famous physicians and thus further boost their own standing. The German surgeon Wilhelm Fabricius of Hilden is a good striking example. Around 1600, at a time when surgeons still very much battled to get academic respect, he managed to put himself at the center of a growing epistolary network of academic physicians. His fame rested above all on the publication of his correspondence combined with his own observations and those which his correspondents had communicated to him. And the fact that Fabricius promised to publish their letters as well may well have enticed others to enter into an epistolary exchange, too, which would make them appear as part of a circle of highly renowned physicians.[4]

If necessary, letter writing could also serve as a welcome tool to defend oneself against attacks from others, to repair one's own public image or that of friends. Thus Carlo Avanzi, in 1638 complained to Michael Besler that Johannes Vesling had become the head of the botanical garden in Padua without knowing much about plants. He had outmaneuvered Avanzi –at least this is what Avanzi claimed– by spreading calumnies. But Avanzi had taken revenge: he had made a fool of Vesling asking him to identify some very common kitchen plants like parsley and mustard in front of 200 invited students. According to Avanzi's account, Vesling had, of course, miserably failed, and by telling this story in a letters to his German correspondent Besler, Avanzi made sure the news would spread also in Vesling's home country.[5]

Of course, epistolary networks were not just about medical authority. They could also serve other purposes –though many of these other purposes could at least indirectly also contribute to a physician's authority. Letters could offer intellectual stimulus and exchange especially for those who worked in rather isolated situations. They could give access to the latest political and local news. They were an excellent and widely used means to spread the word that the author was just about to complete a new book which would soon be on the market. They provided a welcome opportunity to test colleague's responses to new ideas or findings. And in numerous letters the correspondent was also simply asked to buy and send certain books, or correspondents mutually exchanged seeds, plants, minerals or other specimens related to natural history, as part of a highly elaborate epistolary gift culture. Sometimes a correspondent might also quite straightforwardly seek support in his effort to secure a new position or to attract new patients. In this sense young Prospero Alpino, for example, asked Joachim Camerarius to help him open the doors to the German community in Venice and Padova.[6]

Just being part of the circulation of letters and knowledge as such, it seems, was perceived as being of considerable value. This is well illustrated by the numerous letters which, at a closer look, lack any real content. This can be rather frustrating for the historian who seeks to analyze them but it is a very revealing feature. These letters served simply to confirm that the epistolary "friendship" still existed. Quite commonly correspondents even expressedly stated that they actually had nothing to say, nothing to write about, or that they had had a hard time finding the topic which they now proposed to discuss. Sometimes correspondences were also marked by long intervals of silence –and yet at some point one of the two resumed the exchange, without any obvious reason and with little to say.

Even some of the most famous physicians of the period attributed considerable importance to maintaining epistolary networks far beyond immediate, pragmatic, utilitarian considerations. Some, like Theodor Zwinger in Basel must have spent a substantial part of their time answering the thousands of letters addressed to them which have come down to us. In spite of

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[4] Wilhelm Fabricius, *Observationum & curationum cheirurgicarum centuriae*, Geneva 1611-1614.

[5] Universitätsbibliothek Erlangen, Handschriftenabteilung, Trewsammlung, letter from Carlo Avanzi to Michael Besler, Padova, July 15, 1638.

[6] Universitätsbibliothek Erlangen, Handschriftenabteilung, Trewsammlung, letter from Prosper Alpino to Joachim Camerarius jun., Venice, September 20, 1591.

the considerable time needed for maintaining an epistolary network and of the expenses for forwarding the letters, examples of physicians who were, on the contrary, reluctant to enter into epistolary exchange are rare. Felix Platter, Zwinger's colleague in Basel is one of them. He was undoubtedly one of the most renowned physicians of his time and was frequently contacted by others. But his surviving letters tend to be extremely short, and those addressed to him suggest that he often preferred not to answer at all. Why he acted in this manner we do not know for sure. He was a busy man –but so were many others. Probably he could afford to do without an epistolary network better than most of his colleagues thanks to his outstanding and financially rewarding position as a practitioner in Basel: he counted most of the patricians and aristocrats among his clients. But as a look at his surviving letters suggests he may also not have felt quite at ease with the demands of elegant Latin letter writing.

Medical authority

For many early modern physicians maintaining a good epistolary network was a privileged means of acquiring and maintaining authority within the learned medical world. But what can we learn from their letters about the construction, questioning and refutation of early modern physicians' authority? On what grounds did early modern physicians accept certain colleagues as authorities and rejected others? Much more research is needed in this area and it should include prosopographical studies which would look at how physicians with different training and different strategies of self-fashioning fared. A closer analysis of their letters does already bring to light, however, some major criteria which guided their judgment.

A first feature is very familiar to historians of early modern society but it is nevertheless worth underlining: authority and fame in the early modern republic of letters were not only attributed based on scientific or literary achievements. Virtue was an indispensable prerequisite as well. When correspondents mutually praised each other or when they wrote about other physicians' good moral qualities, like "humanitas" and "pietas" were often presented as crucial. Without them physicians could not expect to be accepted as an authority. Vice versa, criticizing the moral qualities of an opponent –the wide-spread slandering of Paracelsus is a good example– was a very promising strategy when it came to undermining an opponent's medical and philosophical standing.

A second attribute which had much greater importance than today was eloquence and style. Thus Leonhard Bausch praised Georg Agricola –not so much for his mineralogical findings but for his elegant Latin style.[7] Elegant style also had to be demonstrated in the epistolary exchange itself. For good reasons, fathers of future physicians asked their sons to correspond in Latin with them. A simple Latin mistake could have disastrous consequences and the word of such linguistic incompetence might spread quickly. Thus Joachim Camerarius in a letter to Sigismund Schnitzer reported the story of a Spanish scholar in Nuremberg who had translated various texts into Latin and was admired for his linguistic skills –until one day great shame fell upon him: he had quoted Priscian wrongly with the expression: "Hispanus est rex omnium regorum" instead of "regum" –a blatant blunder of which Camerarius and Schnitzer who later published his letter helped spread the news.[8]

The high esteem for an elegant style went hand in hand with great appreciation for learning in general, a learning which letter writers often also sought to demonstrate in their letters, too, scattering them with quotes from the major works of ancient Roman authors or indeed short phrases from Greek works in Greek script.

Of course, scientific authority in a more narrow sense also played a major role. Early modern physicians had heated debates on medical theories and, especially in their publications, sometimes attacked each other viciously for disregarding – or, vice versa, for adhering slavishly– the views of Galen, Hippocrates or other ancient authorities.[9] His adherence to a specific school of thought as such provided only a limited basis for questioning a physician's authority, however, except if it could be linked to reproaches of a lack of piety, virtue or learning as in the case of orthodox criticism of the Paracelsians.

In debates on medical matters another argument was much more lethal: deviation from factual, empirical truth. The early modern period saw a pronounced shift from a focus on the truthful representation of authoritative texts to the truthful representation of nature, based on careful and well-informed observation. This shift can be perceived already in the 16th century in epistolary exchanges about matters of natural history, an area in which many physicians took great interest. Leonhard Fuchs for example could hardly have expressed himself in more devastating terms about Pietro Mattioli. Once his own book was out, Fuchs declared, everybody would see that he had corrected Mattioli, this "arrogant", "inflated" Italian, in hundreds of instances.

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[7] Letter from Leonhard Bausch to Sigismund Schnitzer, Schweinfurt, November 10, 1607, in: Johannes Hornung (ed.), *Cista medica,* Nürnberg 1626, p. 370.

[8] Letter from Joachim Camerarius jun. to Sigismund Schnitzer, Nürnberg, January 29, 1603, in: *ibid*., p. 276*f*.

[9] Cf. for example the vitriolic disputations by Johann Freitag, *De paradoxis et erroribus novae sectae Sennertiano-Paracelsicae*, Groningen 1635.

The esteem for factual truth was closely linked to a growing appreciation of empirical findings, which was the driving force behind the dramatic rise of another criterion: originality.[10] It came to the fore above all in the guise of the accusation of plagiarism. He hoped that the author was not selling other people's work as his own, Georg Agricola even said about Andreas Vesal's famous *De humani corporis fabrica libri septem* before he had seen it.[11] Fuchs, in turn, was adamant that Conrad Gesner was making a too liberal use of the work of others, that he had indeed sometimes integrated entire works by others, like Rondelet's work on fish, into his own, and intended to do the same with Fuchs' work.[12] This accusation reflects the growing importance attributed to original work, to original, empirical findings in early modern science and medicine but it was also heavily fraught with morals: plagiarism was incompatible with the image of a virtuous physician whose ultimate aim was the good of mankind and not the selfish promotion of his interests.

In many respects then, medical correspondences were a major site for the negotiation of early modern medical authority. Their analysis offers important insights into the strategies and processes involved and can thus ultimately also contribute to our understanding of the reasons why some authors, theories, findings and books were highly successful and influential while others remained marginal or were quickly forgotten.

[10] See also Tilman Walter, New light on antiparacelsianism (c. 1570–1610). The medical republic of letters and the idea of progress in science, in: *Sixteenth Century Journal* 42 (2011) (forthcoming).

[11] Niedersächsische Staats- und Universitätsbibliothek, Handschriftenabteilung, 2 Cod. ms. philos. 89 1.1.1544 an Wolfgang Meurer

[12] Universitätsbibliothek Erlangen, Handschriftenabteilung, Trewsammlung, letter from Leonhard Fuchs to Joachim Camerarius jun., Tübingen November 24, 1565; Fuchs was clearly referring to Guillaume Rondelet, *Libri de piscibus marinis in quibus verae piscium effigies expressae sunt*, Lyon 1554.

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VERBA VOLANT SCRIPTA MANENT: THE IMPORTANCE OF CORRESPONDENCE IN EARLY MODERN MEDICAL DIAGNOSIS

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Abstract

The aim of my talk is to underline the importance of correspondence between physicians in Early Modern medical diagnosis. The paper studied the correspondence between Romolo Spezioli, personal physician of Queen Christina in Rome and the famous Italian scientist Marcello Malpighi. The research examines the serial of letters that is kept in Archiginnasio Library and in University Library in Bologna. The collection of letters, unpublished and unstudied, offers a unique example of medical correspondence full of details about the description of diseases and therapy in the 17th century and it represents an important source of reconstruction of the epistemological approach to catarrhal diseases in Early Modern medicine.

During the 17th century, a doctor's reputation was determined by many factors, the culmination of which was an improvement in the patient's health and, if possible, a complete recovery. Personal medical visits, the easiest to carry out in terms of clinical procedure, certainly contributed to making a doctor's therapeutic successes known, but it was difficult to go beyond such limits of a typical 'medical practice'. Once having achieved a good, local reputation, the physician devoted himself to long-distance medical consultations to get around this limitation. These took the form of epistolary communications, which placed the hopes for a patient's recovery –sometimes someone whom he had never met in person– in a written opinion sent by post known as a *consilium*.

In order to define the *consilia* –the dense and interconnecting documentation of advice supplied, requested, and received by physicians– I have divided my presentation into two parts. The first offers a brief survey of the *consilium* in a very technical sense as a specific literary genre of practical medicine. In the second part, I would like to point out some levels and forms in which the consulting practice and circulation of opinions, which defined a physician's skill and the extent of his influence, were expressed by analyzing consultations signed by Marcello Malpighi, the Italian scientist (1628-1694), and by Romolo Spezioli (1642-1723), the personal physician of Queen Christina of Sweden during her stay in Rome (1654-1689) and court physician to Pope Alexander VIII[1]. The first relates to the ill-health of Cardinal Girolamo Gastaldi's sister (1616-

[1] Cf. F. Zurlini, *Romolo Spezioli (Fermo 1642- Roma 1723). Un medico fermano nel XVII secolo a Roma*, Roma 2000; Ead., Fabiola Zurlini. Cultura scientifica, formazione e professione medica tra la Marca e Roma nel Seicento. Il caso di Romolo Spezioli, Macerata 2009.

1685)[2] and the second to the illness that led to the death of the powerful Venetian cardinal Pietro Basadonna (1617-1684)[3]. An examination of these documents, now stored in Bologna at the Archiginnasio and the University Libraries, will show how these documents are not merely a collection of data and symptoms, but rather present a microcosmic overview of the different epistemological levels of seventeenth-century medicine, with special reference to etiological research and the treatment of catarrhal illnesses.

What is a *consilium*? Critics have suggested several definitions, all subject to revision by whoever does not find them completely satisfactory in respect to the texts encountered in their own research, or finds the definitions so broad that lose any historiographical usefulness. Perhaps, it was this kind of uncertainty that led Jole Agrimi and Chiara Crisciani, editors of Les *Consilia médicaux*[4], one of the most comprehensive studies available on the subject, to provide a definition that takes into account the possible subgroups, specific cases and their evolution. The authors therefore have suggested an ideal model that, while not always reflected *per se* in the actual texts, is certainly useful to provide a preliminary framework. A *consilium* is therefore a document in which a physician, following the rules of a codified writing discipline, formulates regarding a current, individual case of a pathology in which the illness is defined and an appropriate treatment is prescribed[5]. A form of epistolary communication, these documents are, therefore, an indispensable tool for translating a by and large literary document into a professional one. Thus, a vast sea of well-established literature was created, which served as the empirical foil to speculative treatises in institutional manuals and the theoretical foundations of the various medical systems.

Alongside the vast river of published *consilia*, there is a parallel current of unpublished works –often for centuries and according to the criterion of ills (*a capite usque ad calcem*)– that ran flowed in the initial period of their evolution. This does not mean that these forms of scientific communication may not also have circulated in various handwritten copies among a circle of insiders and the patient's relatives and created some debate on the issues raised[6].

This is the case, for example, with the illness that struck the sister of Cardinal Girolamo Gastaldi. Her state of health so disturbed the soul of that illustrious prelate that he was driven to write a letter dated 6 July 1684 to the famous Marcello Malpighi, asking for a *consilium*, and including a consultation by another, less well-known physician, Romolo Spezioli[7]. This latter set of papers, enclosed with the epistle, are now lost. Nevertheless, research conducted at the Bologna's Archiginnasio Public Library has brought to light an unsigned *exemplum* (report)[8] that summarizes the patient's physical symptomatology together with the medical treatment prescribed by Spezioli[9]. In this way, we learn that the noble woman, frail of body but of cheerful temperament, is plagued by various kinds of maladies, such as erysipelas, (intermittent) ephemeral fevers as well as a shortness of breath and continual coughing that degenerated into pleurisy. Spezioli's direct knowledge of the disease through personal observation of sputum –sticky, white or sometimes a very dark color– led to clearing up the painful disease in which certain afferent bodily fluids, corrupted by the poor coction of humors, gave life to a rotting waste, concentrated particularly in a series of catarrhs in the upper respiratory system. To purge the rotting bedsores and allow the body to regain the lost crasis, the doctor prepared decoctions made from goat's milk serum, borage and Prunella salt[10] and medicines to counter the atrabilious components of the humors, restored the proper degree of fermentation as well as bled the salvatella[11].

The clinical picture and therapeutic treatments just highlighted were confirmed in Malpighi's consultation of 9 August 1684 when, in answer to the prayers of Cardinal Gastaldi, the physician from Crevalcore reassured him of the suitability of the assistance being given to the sick woman[12]. The *consilium* is divided into three sections. The first gives an account of the resulting unhealthy situation; the second and third prescribe medications and therapeutic interventions to be adopted. These include a way of life based on the proper use of the six *res non naturales*, the only area in which the patient can make a behavioral practice for which the *consilium* provides precise guidance. It must be emphasized that the consultation's structure by topic where indications that may lead to presuming a personal visit are few and indications of the 'distance' are

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[2] Cf. N. Marsili, "Girolamo Gastaldi" in *Dizionario Biografico degli Italiani*, Roma 1965, Vol. LII, 532- 533.

[3] Cf. G. Benzoni, "Pietro Basadonna" in *Dizionario Biografico degli Italiani*, *op. cit.*, Vol. III, 51- 53.

[4] Cf. J. Agrimi et C. Crisciani, *Les consilia médicaux*, Turnhout 1994.

[5] Cf. J. Agrimi et C, Crisciani, *Les Consilia*…, *op. cit.*, 27- 38.

[6] Cf. A. Cermisone, *Consilia*, Venetis 1498. On the rationality of the diagnostic procedure proposed by Cermisone see M. Savonarola, *Practica Maior*, Venetiis 1559, f. 7rb.

[7] University Library of Bologna (ULB), MS (LS) 2085, Vol. X, f. 114*r*, 'Girolamo Gastaldi To Malpighi, Bologna, 6 July 1684'.

[8] Cf. J. Agrimi et C. Crisciani, *Les Consilia*…, *op. cit.*, 19- 27.

[9] Archiginnasio Library of Bologna (ALB), Coll. Aut. XLI, 11. 181, ff. 18*r*- 19*v*.

[10] Sort of artificial salt prepared from potassium nitrate and flowers of sulphur. Cf. G. B. Capello, *Lessico farmaceutico*, Venezia 1754, 128.

[11] The vein between the ring and little fingers of the right hand.

[12] ALB, Coll. Aut. XLI, 11, 181, ff. 75*r*- 76*v*, 'Malpighi to Girolamo Gastaldi, Ronchi di Corticella, 9 August 1684'.

greater, making the document a typical exemplar of a *consilium in absentia*[13]. Among those indications that stand out are the need for a theoretical and doctrinal completeness in the first section with its precise list of symptoms related to the pituitary, including persistent night cough, and the critical scrutiny of those symptoms with a view to understanding the etiology of the disease, getting over the bad coction of the chyle and the consequent stagnation and fermentation of corrupt sera in the upper viscera. This is not a simple display of erudition, but a manifestation of a diagnostic method, where the consulting magister works, at a distance, with his books and coordinates with his teaching the discreet work of attending physicians, seen as intermediaries between the literary authors and the patient's body.

As can be developed doctrinally and related also or above all to skill, Malpighi's consultation was however a public document that circulated both within the Gastaldi family circle and among physicians in Bologna and Rome. Attention is drawn to the "competition" between the expert consulted and the consulting physician as the letter that the physician and mathematician Domenico Guglielmi sent to Malpighi[14], once again at the request of Cardinal Gastaldi, to take the case of Cardinal Pietro Basadonna, one of Spezioli's patients and who was afflicted with a severe urological complaint. The long-awaited consultation never took place as the illustrious prelate died that same day, 6 October 1684. Nevertheless, research in the archives of the University Library in Bologna has permitted tracing the *consilia*, drawn up by Spezioli on the Basadonna case, probably summed up –as suggested by Guglielmini's letter– in the message forwarded to Malpighi[15].

In relation to the singularity of the case, the structure of the *consilium* became established and had an epistemologically important effect on the diagnosis of pituitary diseases with the wording in the first section of the paper, of the *casus in terminis*: the elderly Cardinal's symptoms were thus subsumed in a abundance of corrupt humors in the urogenital region whose withered putrescence was the cause of both urination disorders suffered by the elderly cardinal and the suppurating tubercle that, as a result of the decubitus of the catarrhal waste, was found between the neck of the bladder and the base of the kidney. According to this skeletonizing process, the patient's data and symptoms (*signa narrata*) became conclusions that corresponded to texts often cited in detail. The authors that appear in all sections of the *consilium* make the latter an alternative to the printed page, as a means to teach and train, showing not only the sophisticated medical training of its author, but also a precise framework within which to resolve the epistemological search for the underlying causes of disease. The reading of classic and modern primary sources has been joined with a direct observation of the disease, thereby resolving the pathological etiology of catarrh in a particular canon of healing. The diagnostic outline thus inferred led Spezioli, with an amazing intuition for the state of the art at that time, to attribute the clinical picture to a fault of the blood, pathogenically unrelated to non-specific symptoms of dysuria, vesical tenesmus, and pyuria. The explanation of the disease's etiology followed the treatment guidelines, evacuated and purgative in nature, designed to correct the corrupt matter in the blood to allow restoring the correct crasis of the bodily fluids.

In conclusion, the literary genre of the *consilium* is better than other documents the path of medicine as a practical science, as a discipline consisting of a *pars theorica* and a *pars practica* where one is conceived in terms of the other and vice versa. The texts analyzed here are excellent examples of this policy: the *consilia* of Marcello Malpighi and Romolo Spezioli, with their references to a wide range of therapeutic interventions such as phlegmatic diseases, are important documents to understand how the medicine of the time wished to go beyond the simplistic definition of *ars mechanica* that was sometimes attributed to it. Spezioli's texts, in particular, are very eloquent in this regard because, rather than presenting the reader with a descriptive and bibliographical survey of the case under study, they offer a bibliographical diagnostic description, which supports and corresponds to a bibliographic therapeutic record in which it summarizes a changed knowledge of medicine, aimed at the universal tense but always subject to experimental revision and control.

[13] Cf. R. French, *Gentile da Foligno and the via medico rum, in The light of Nature*, edited by J. D. North and J. J. Ruche, Dordrecht 1985, 21- 34.

[14] ULB, MS, 2085, IV, f. 184*r*, 'Giovanni Domenico Guglielmini to Malpighi, Bologna, 6 October 1684'.

[15] 15 ULB, MS 2085,. IV, ff. 202*r*- 203*v*.

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THE ZOOLOGICAL COLLECTIONS OF THE *MUSEU DE LISBOA* AND THE NETWORKS OF SCIENTIFIC CORRESPONDENCE AND EXCHANGE (1858-1898)

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Abstract

In 1858, the royal zoological collections hosted in Lisbon became part of the Polytechnic School (1837-1911) to be of assistance to the classes in Zoology. The previous «Museu de Lisboa» at the Royal Academy of Sciences was now to be organized following proper scientific and up-to-date knowledge. José Vicente Barbosa du Bocage (1823-1907), as the head professor in Zoology, became in 1859 the Director and first organizer of the "Zoological Section of the Museum of Lisbon". Through his work and direction the royal collections became a museum designed as part of the exchange network of specimens and the scientific knowledge within.

To discover, collect and send home to Europe natural objects and images of the new world was a comprehensive task held by many naturalists whether with an academic, military or religious background. Nevertheless, the understanding of the knowledge held in all the thousands of specimens being brought to European collections and exchanged between European societies, academies and universities was now being completed inside the collections storage rooms.

In this paper we analyse the correspondence of Barbosa du Bocage to his foreign peers aiming to contribute for a clearer picture of the importance of the network established between private collections, universities and museums in the construction of new knowledge in the study of nature at the second half of the nineteenth century. We argue that some influential authors were organizing knowledge about nature from inside the museum's walls and that the way the trade of specimens inside Europe was made is of major importance for the production of knowledge. Analysing the relationships established between these authors (their institutions and nations) and other professors, collectors, patrons, diplomats, naturalists and taxidermists may facilitate the study of scientific knowledge production.

With this paper I aim to present the available data from an on-going study of the vast correspondence of the director of the zoological section from the *Museu de Lisboa*[1], José Vicente Barbosa du Bocage (1823–1907), with his international peers attempting at a possible outline of his personal and institutional networks of scientific correspondence from the year 1858 and the following four decades. I will begin with an introduction of the overall setting, then briefly go through Bocage's biography, and finally address three of the most occurring typologies of correspondents identified so far.

Biographical draft of the Zoological Collections from the Museu de Lisboa

After the big earthquake in 1755 the new Royal collections were organized in the Royal Cabinet of Ajuda (Palace of Ajuda, Lisboa) and grew as ship loads came from abroad, from several places in the globe, arriving at the Lisbon port. Furthermore, the Marquis de Pombal refurbished the Palace of Ajuda with a Botanical Garden, a Chemical Laboratory and museums to where he called on Domenico Vandelli (~1735–1816) to assure its scientific organization.

In 1808, during the French Invasion, the still incipient arrangement in the Ajuda Museum received an imminent Zoologist's visit from the Parisian Jardin des Plantes: Geoffroy Saint-Hilaire (1772–1844) was the naturalist selected to accompany the French troops and 'collect' from the natural history collections of Lisbon. He was accompanied by Vandelli himself, whm later stated he had tried to «make the best deal possible» with Geoffroy –who had in his agenda the commitment to complete the French natural collections of the Americas, especially Brazil.

The first decades of the Portuguese nineteenth century were all but quiet[2], and even with great directors as Vandelli and Avelar Brotero (1744–1828), and with the collections of Alexandre Rodrigues Ferreira (1756–1815), the natural history collections didn't develop. When these collections arrive at the Polytechnic School, they are welcomed by Barbosa du Bocage, who inherently with his professor post becomes the responsible «Director» of the Zoological Section of this Museum. In fact Barbosa du Bocage, as the main Zoology Professor, was the official receiver[3] of the very large set of collections held at the Academy of Sciences, collections that had previously come from the Royal Cabinet of Ajuda, Lisboa. He becomes thus associated with the growth registered from then on in the zoological taxonomical studies in Lisbon. In fact, the zoological section of the museum later came to take his name –and became to be known as "Museu Bocage"[4].

José Vicente Barbosa du Bocage (1823-1907)

Barbosa du Bocage was born in Funchal (Madeira) into a family that was –due to the political instabilities in Portugal at the time– moving between Madeira and Brazil. He graduates from Coimbra in 1846 with a Baccalaureate in Medicine that will later take him to Lisbon to become an attendant at the Hospital de São José. In 1849 he becomes assistant lecturer in Zoology at the Polytechnic School –becoming a part of this, by now consolidated, educational project. In the following years, he marries Teresa Roma (1851) and begins an extraordinary career as a zoologist (publishing in total around two hundred papers on taxonomy) being part of the Lisbon's intellectual sphere (member of the Royal Science Academy, and a founding member of the Geographical Society in Lisbon, where he was President from 1877 to 1883) and the politician sphere as well, particularly after his retirement as Professor; who was involved in trying to contribute with new and pragmatic ideas for the country[5].

He becomes the Chair for Compared Anatomy and Zoology in 1851 and amplifies his interest in zoological studies. While he is part of the Polytechnic School board of professors, in 1857 he publishes his first important work on Portuguese zoology and taxonomy, describing the by now extinct Portuguese Ibex (Cabra do Gerez) making use of the Royal Collections from the Prince's Cabinet. His later works with the Museu de Lisboa's Collections *Ornithology of Angola*, or *Herpetology of Angola and Congo* –just to name a few– were international references in the field.

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[1] The *Museu Nacional de Lisboa* was established at the Polytechnic School of Lisbon by decree in 1858 (Dec-Lei 09.March.1858) holding two different sections: Mineralogy and Zoology, its main purposes to manage the collections from the Academy of Sciences of Lisbon, and to use them as didactical support.

[2] Several civil wars disrupted the Portuguese society throughout the first half of the nineteenth century.

[3] There was a formal session held at the Academy of Sciences in 1858, where Bocage was the official representative of the Polytechnic School.

[4] In 1905, the Polytechnic School Board rendered homage to Barbosa du Bocage and decreed the Zoological section of the school's museum as «Museu José Vicente Barbosa du Bocage».

[5] He was in 1866 called to the Education Council, and became in 1881 a member of the Câmara dos Pares do Reino (with legislative powers) and then in 1883 to the Fontes Pereira de Melo administration to be Naval Affairs and Overseas Minister; and again in 1890, he was part of the government of João Crisóstomo taking the Foreign Affairs when he was in charge with the difficult negotiations of the British Ultimatum (1890).

In 1875 he holds the office of Secretary at the Science Academy in Lisbon and by 1886 he was a corresponding member for the Zoological Society of London, the Natural History Society of Strasbourg and the Royal Imperial Zoological and Botanical Society of Vienna. Unfortunately, this Museum section suffered from a major fire in 1978, leaving the collections with almost nothing from the past specimens or memories. Fortunately, many of Bocage's papers, folder, manuscripts and correspondence still exist today.

From 1858 on, Bocage dedicated himself to the promotion of a large network of contacts, asking yearly budgets for new purchases, and soliciting more and more specimens from other museums and collectors. These would seem strange requests –specially knowing that the school's building storage rooms were still under construction– if we didn't take into account that the value of zoological collections lays fundamentally on its ability to compare with large reference collections. The Lisbon collections were still not properly identified or catalogued and Bocage felt the urgency of gathering more "knowledge" than the available on the specimens he had to study.

The "Sedentary Naturalist"

> *If the sedentary naturalist does not see nature in action, he can yet survey all the products spread before him. He can compare them with each other as often as is necessary to reach reliable conclusions. He chooses and defines his own problems; he can examine them at his leisure. He can bring together the relevant facts from anywhere he needs to. The traveller can only travel one road; it is only really in one's cabinet that one can roam freely throughout the universe.*[6]

This quote underlines the role of such *sedentary naturalists*, as naturalists do not produce new knowledge all by themselves –they need expeditions, comparative collections and networks. A *sedentary naturalist* is, in this context, a particular scientific persona that represents a node of a larger and essential network of contacts, aggregating places, materials, texts and books, and all the available information.

So, immediately after hosting the collections into the recent rooms of the department, Bocage begins the immense task of organizing them systematically –so that they could be used for both didactical and scientific functions. It is from this moment on that Bocage will insist with the School's Council to be excused from the teaching tasks, in order to focus on the research with the collections. He soon realizes that, in order to become an appropriate systematic zoology museum, this would need large increases of specimens not yet available in the collections.

In 1859 he asks for, plans and carries through a major digression to acquire specimens and establish connections with other European museums, what we today would call an exchange network. Zoological specimens, but also taxonomic information and scientific books and periodicals, travelled between nodes that could be scientific personalities or institutions. Bocage leaves on a European Tour during the School's summer break to visit not only Paris, but Madrid, Brussels, Strasbourg, Leyden, London and Frankfurt –and he makes contacts not only with museums, and museum staff, but also with taxidermy tradesman, field naturalists, specimen merchants, etc. With his tight budget he buys specimens, books, taxidermic supplies as well as didactical cutting edge material for the classes back in Lisbon.

Always having in mind the many systematic gaps that 'his' museum had missing, he chose as main strategy to try and invest in the comprehensive completion of the animal groups (Classes) of which the museum already had some specimens. For him, as stated in his reports, it is better to have a 'whole' group than groups with 'holes' on them.

The process of augmentation of the collections made possible by Bocage had more than one approach. The expeditions to the far corners of Africa, South America or India didn't stop in the meanwhile. And in an effort to better the results of these expeditions Bocage published in 1862 his *Instrucções praticas sobre o modo de colligir, preparar e remetter productos zoológicos para o Museu de Lisboa* (Instructions on how to collect and send specimens to the Museum in Lisbon). Also in 1863, the Royal collections of the king D. Pedro V are donated and transferred to the museum. All this was maybe sufficient for a study collection to serve the classrooms at the Polytechnic School, but it seems clear that the collections were from the beginning being considered by Bocage as serving much more than a didactical purpose. In order to be able to develop scientific work in zoology, quantity of specimens and suitable reference collections are of major importance.

Networks of Correspondence

Following, I try to provide a brief analysis of the network of scientific correspondence, exchange and trade of zoological specimens as part of a designed strategy of augmentation of the collections. Barbosa du Bocage was related with his peers in museums and academies from all over Europe and beyond: so far this study has already documented twenty-

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[6] Georges Cuvier as quoted in Dorinda Outram "New Spaces in Natural History", in Jardine, Nick et al *Cultures of Natural History* (Cambridge: Cambridge Press, 1996) pp.260-266.

eight correspondents, with epistolary relations ranging from 1858 until 1900, mainly written in French but also in English and German, with correspondents from seventeen cities from fourteen different countries, with no registered complaints or misunderstandings of word meanings. There are even specific phrases that are left in the original language independent of the exchange language used.

The content of the letters is varied although, as expected, mainly about taxonomical considerations or problems, or on the transaction details of specimens, ranging from commercial dealings to letters with a more intimate and personal tone.

It may also be interesting to make a note of the role of mediator that Bocage occupied between other foreign zoologists and the Portuguese crown –he was many times asked to forward letters, specimens or publications for appreciation by the king himself. Bocage is certainly a preeminent figure with a high status and sufficiently close to the crown; surely King Dom Pedro, as later D. Luís and D. Carlos, had an interesting reputation amid naturalists, but it is still remarkable that Bocage represented such an important connection.

Some letters indicate close and almost intimate relationships between scholars and Bocage himself: there are some remarks on his wife's health and compliments on his son's accomplishments with his scientific instruction.

From the letters this study processed so far, becomes apparent that several authors play significant roles inside the networking of this period in Europe, regarding knowledge on zoological specimens. From the epistolary relationships identified, an attempt was made to categorize them into their own taxonomy. I will consider for this paper as *network nodes* – correspondents that provided clusters of more than three letters from the same source that may allow a picture of a specific correspondence relationship. The study so far comprehends three different clusters of correspondence: trade and commercial affairs; trades between museum naturalists; and between diplomatic contacts. Other cluster that may be identified and characterized in the future is, for example, the periodical publications exchange between journal editors and naturalists.

Commercial exchange (specimens for money)

Commercial liaisons are more formal, as would be expected, but significantly cordial in allowing for mishaps in the long journey transactions to happen, and offering to make replacements of broken glass containers, for example. Many different commercial arrangements may be made depending on the quantity requested –more specimens bought equals less money spent in each, which may suggest an interesting study to be made on the monetary value attributed to each *taxon*.

There seems to be a great investment in the seriousness of the business –it is after all dealing with scientific, objective knowledge and thus, traders resolve their legitimacy and authority regarding classification, with the collaboration of 'uninterested' savants from universities or academies that help build the advancement of science, even if it is for commercial purposes. This seems to be a very interesting modus operandi since there is an objective trade value attached to each specimen but simultaneously a sort of detachment associated with the scientific knowledge principles and values. The expected jargon of 'the best offer' and 'buy now!' is present even if under a coating of formal and polite writing, as in the following example:

> Je vous envoie ci joint la liste de deux belles collections de Reptiles dont je me suis rendu acquéreur. Il y a des pièces bien rares principalment parmi ceux d'Australie. C'est le premier choix que je vous offre. De certaines espèces, je n'ai qu'un seule exemplaire. [...] J'ai aussi [...] espèces [...] tous nommés par le Savant Icthiologue Professeur Bleccker.
>
> A. Boucard (12.VIII.1873)[7]

In the excerpt presented[8], the same letter legitimates the goods through the recognized authority of Professor Bleccker –as a sign of authenticity and care for the proper serious specimens' identification. Adolphe Boucard (1839–1905) was himself a known field naturalist, but for his commercial transactions chose to have another name associated with his own authority as a collector, to reinforce his authority also as a trader.

Some phrasing is also used to apparently detach the discourse from the pure commercial interest, like the expression *«je me suis rendu acquéreur»* as if the purchase of such items were a sort of a random, non intentional event. The formality of the letter also seems to relate more with gentlemen prose than with straightforward commercial transactions of material goods. These marks may be explored further as a different type of transaction appears, one that falls in between commerce and intellectual exchange blurring boundaries between both.

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[7] Manuscript correspondence from the historical archives of the Museu Bocage – Museu Nacional de História Natural da Universidade de Lisboa [AHMB CE.B.27].

[8] The available correspondence with Adolphe Boucard (from both Paris and London) continued from 1873 to 1875, counting six epistles exchanged between Bocage and Boucard.

Museum Collections (specimens for specimens)

Regarding another sphere of transactions, it is a distinguished feature for natural history museums to act as repositories of doubles, to be used in direct transaction with each other. Even if the museum is a shared arena between several specialists, curators still tend to work with collections in a very personal way, since *new species* discoveries would still occur; authorship was still an important marker for distinction. The networks would also serve the purpose to render "hot news" from one place to another, since it is normal for this kind of epistles to be sent along with specimen samples or doubles, as material proof but also material representation of a specific issue or information[9]:

> Vous me permettez de vous envoyer en même temps que cette lettre quelques épreuves de coquilles fossiles du miocène supérieur de Steinheim Aalbuch (Württenberg), du genre Planorbis ou Carinipex (autrefois Valvata), qui échauffent au moment tous les paleontologistes et conchyologistes allemands Darwinians ou Non-Darwinians.
>
> O. Boettger (14.X.1877)[10]

This community of savants was so much engaged with the progress of scientific knowledge that was indeed advancing very rapidly by then, that book corrections were taken as part of a *shared knowledge*.

> Je vous remercie pour vos observations au sujet des ***Bucorvus*** et quand je fais quelques fautes, je veux bien qu'on me corrige.
> Vous avez raison, et je suis en erreur [...]. Nous cherchons que la verité, et dans ce but il faut nous corriger les uns les autres. Maintenant, pour les autres figures nous ne sommes pas encore d'accord. Pour savoir d'ou vient la fleuve il faut monter à la source. ...
>
> D. G. Elliot (01.XI.1877)

A very interesting feature of these letters is the attitude towards scientific knowledge sharing and the common understanding that taxonomic knowledge could only increase via and intense comparison and critical observation and reflection, circulating in a large network of experts.

Correspondence with Daniel Giraud Elliot (1835–1915)[11] was written while Elliot was staying in Paris in 1877 and this cluster mentions corrections made to his article by Bocage, by his own initiative, and a remarkable inquiry shared by both Elliot, Bocage and another zoologist in Germany on the correct description of one of the Ground-hornbills (Bucorvinae). There was an exchange of specimen descriptions spurred by the noticeable differences noted between German specimens in a catalogue description and Bocage's own description of his own specimen in Lisbon. Elliot also mentions John G. Keulemans (1842–1912), the skilful Dutch artist whose depictions he relied on to further inquire differences between the three birds and whether the Lisbon specimen was indeed unidentified, and therefore *new* to science. Museum catalogue descriptions were not comprehensive enough when it came to specific traits, and not all specimens had individual depictions in engravings or drawings. In this case, direct comparison between specimens was only attained through an intense epistolary exchange between the various nodes.

Diplomat liaisons and "donations" (specimens for honours)

In an epistolary transaction between Bocage and Barnet Lyon[12], other commodity values that scientific natural history specimens may have become evident. Albeit Barnet Lyon is, at the time, the Netherlands consul in Belgium, he writes to Bocage in order to ask for assistance to his brother –the Portuguese crown consul in Suriname, in getting the right contacts in Lisbon for a shipment of natural objects destined to the Museum of Lisbon. Barnet Lyon wanted to know to which of Bocage's *contacts* he should address the cargo to, and whether to send it via Amsterdam or London. In addition, he proposes another shipment: a collection of 150 models of exotic fruits he possesses, and that he wishes to *gift* to the Portuguese crown.

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[9] Correspondence with museum curator Oskar Boettger (1844-1910) from the Senckenberg Museum Frankfurt, Germany lasted, at least, from 1877 through to 1893, and was written in French.

[10] Manuscript correspondence from the historical archives of the Museu Bocage – Museu Nacional de História Natural da Universidade de Lisboa [AHMB CE.B.20].

[11] American naturalist, founder of the American Museum of Natural History, New York and curator of zoology at the Field Museum, Chicago.

[12] This specific episode of correspondence with George Henry Barnet Lyon (1848–1918) went on mainly during 1868 [AHMB CE.L.18-21].

> si cette collection, dont une pareille, a été achetée par le Musée Britannique, loin de l'Exposition Universelle en 1862, pourrait être agréable au Gouvernement de Sa Majesté, je serais aussi très flotté et heureux, de pouvoir l'offrir.
>
> Barnet Lyon (19.[?].1867)

That a certain *gift economy* should be a part of the diplomatic networking is to be expected, and of course museums depend on their overseas national officials for a big part of their travels and transactions, even so, that the diplomatic liaison was to be so closely intertwined with the 'scientific' network amongst European nations is an interesting feature that needs more study and reflection. In this first letter is noted that the Portuguese consul in Suriname doesn't have a direct contact with the capital, Lisbon. And also that it is via the natural objects exchange that this connection will, in fact, be established. In the following letters, both from Barnet Lyon and his brother, Bocage carries on the exchange of several deliveries from Suriname via Barnet Lyon (in Brussels) until at least April 1869, when he receives a thank-you note from Sally Lyon, still in Paramaribo, Suriname. Sally Lyon had just received notice of his appointment for the Honorific Order of the Conception of Villa Viçosa, of which he was made an official "Cavaleiro". This may be an example of how these donated "gifts", be they specimen data or supplies, are transferred with the consent of the donor not for material or tangible profit but for social prestige, as several honours are being exchanged alongside the scientific materials. In this particular case, in the end of the day, honorific titles were «traded» off for specimen collections.

Exchange of Nature, Nature of Exchange – Closing considerations on the nature of a zoological specimen

Possession of 'nature' is obtained from different procedures that engage different kinds of behaviours. There appears a specific 'gift economy', as defined by Marcel Mauss, a particular circulation of goods in societies. Even modern scientific production is known to entail some of the characteristics of this kind of shared community, when scientists publish papers and quote one another's produced knowledge in roughly the same 'detached' offering way. In the correspondence analysed there were several markings of this kind of shared knowledge. Small pieces of knowledge were traded along the network in order to fill in gaps of a larger puzzle: the systematic inventory of nature, controlled, stabilized and sanitized inside the museum walls. Scientific knowledge is exchanged openly over national borders and languages, in a tense relation between the ideal knowledge and control over nature, individual scientists and their localized institutions.

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THE CORRESPONDENCES BETWEEN THE MATHEMATICIANS BRIOSCHI, CREMONA, BETTI AND GENOCCHI DURING ITALIAN UNIFICATION.

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Abstract

In the 50s of the nineteenth century, there was the necessity to organize new structures in the borning Italian State: the Unification of Italy marked a turning point not only in the political life of the Country, but also in the organization and content of university education and scientific research. A group of mathematicians helped to build the foundation for the renewal and development of Italian mathematics that took place in the following decades. This group included, among others, Francesco Brioschi (1824-1897), Enrico Betti (1823-1892), Angelo Genocchi (1817-1889), and Luigi Cremona (1830-1903).
I want to present here the epistolary correspondence between Brioschi and Cremona and Betti and Genocchi,[1] *covering the period from the mid to late 19th century, provide greater insight into scientific developments and political-institutional arrangements that took place at the time of the formation of the new unified Italian state.*

Mathematicians of the first magnitude, who came from different States existing in Italy in the 19th century, gave a great impulse to the formation of the Italian State and devoted themselves to give it new cultural structures. The letters between two Lombard scholars, Francesco Brioschi and Luigi Cremona (these letters were written from 1855 to 1893) and between Enrico Betti, Tuscan, and Angelo Genocchi, from Piacenza (their correspondence goes from 1862 to 1886), refer to a period in which different States of the Italian peninsula were transforming or failing and to the following unification of the Italian Kingdom.

From a chronological point of view, these letters cover almost all the second part of the 19th century, while their authors are the most important mathematicians of Risorgimento, due to their scientific capacities and their both direct and indirect political action (it is important to mention also Battaglini, Sannia, Casorati, Beltrami).

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[1] [PALLADINO *et al.* 2009].

All these scholars were well-known and of great value, but Brioschi had the most complex personality. His letters, addressed to Cremona, ninety-two, are stored in "Istituto Mazziniano - Museo del Risorgimento" in Genova –*Legato Itala Cremona in Cozzolino*– while those written by Cremona are just six and they are in "Biblioteca Centrale della Facoltà di Ingegneria of Polytechnic" in Milan. Even if Brioschi was a virtuoso in computation in topics he chose[2], he dedicated his studies to engineering with a great didactic, institutional (thanks to unitary feeling and his institutional role as General Secretary of the Ministry of Public Education, in 1862, he organized the "Istituto Tecnico Superiore" –now *Polytechnic*, opened in November 1863– in Milan, serving as director from 1863 until his death) and entrepreneurial commitments. In his letter of 10th July 1865 to Cremona, Brioschi describes his choices: "Unfortunately, my activities at the school don't give me the possibility to dedicate more time to mathematics [...]. Moreover, I have a lot of courses because there is a lack of teaching staff; from November to May I have taught nine hours every week on theory-building and hydraulics. This explains why I work so hard but I can't publish any work: even if I reach a result, I have no time to present it".[3]

Brioschi held important institutional roles: General Secretary of the Ministry of Public Education, member of the Executive Council of the Ministry of Public Education, when Florence first and Rome then, were capital of Italy, president of the *Società Italiana delle Scienze*, known as *Dei XL*, president of the *Accademia Nazionale dei Lincei*, *Consigliere di Lungotenenza*[4] and, as senator, he participated in the commission elected by the Minister of Finance to talk about the tax on flour.[5] He also was director of *Polytechnic*, journal founded by Cattaneo in 1839 and director of the *Annali di matematica pura e applicata*.

As a matter of fact, the new Italian Kingdom needed to be built up and reorganized and Brioschi, able to examine the European situation because of his open-mindedness and with a good education (his tour with Betti and Casorati round German universities and Paris in 1858 represents an evidence[6]), strong-willed, with spirit of enterprise and never exhausted, wanted to be always present (in his opinion, "six o seven days of the month were wasted travelling"[7] to go from Milan to Florence first and to Rome then, were good for his health, even if "this changed a lot his habits as scholar"[8]).

A man divided by the love for scientific research, which he at times practised, regretting the continuous interruptions and rejoicing at his renewals, as it is possible to understand in his letters to Genocchi, and his attraction for action, inspired by his strong self-esteem: when he accepted to be *Consigliere di Lungotenenza*, Brioschi explained his choice:

> "If I hadn't been in Rome those last days and I hadn't seen what there was to do, I probably wouldn't have accepted; I convinced myself that it was necessary to deploy great activities and a wary energy, all qualities I can rely on. For this reason I will send a dispatch of acceptance right now".[9]

But Brioschi's masterpiece was the creation of the *Istituto Tecnico Superiore* (he took as model German polytechnics) and he was able to keep it alive and, with great care, grow it thanks also to a little expedient to "catch" new students: instead of forcing students to pass the preparatory two years in Mathematics at the university, where they were tempted to stay keeping on their studies, they were sent to the polytechnic of Zurich or Lausanne (where courses started a month before) and then, because of "good reasons, difficulties of languages, health…",[10] they were directly transferred to three-year periods, superficially interpreting Italian law. In Milan, Brioschi also organised a "semi-official" course of one year and students were directly transferred to three-year periods.

The correspondence Brioschi-Cremona doesn't show the leading role of Cremona (in 1898, for a short period he was Minister of Public Education and then he was appointed a senator. He had a great influence on post-unitary Italian educational system) and this also because Brioschi kept few works of his pupil first and colleague later. As a matter of fact, during his years at the University of Pavia, Brioschi was Cremona's professor and patron: thanks to Brioschi, he gained

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2 See [Carbone *et al.* 2006].

3 "Pur troppo le occupazioni della mia scuola mi tolgono la possibilità di lavorare come desidererei negli studj di matematica pura [...]. Oltre che il personale dell'Istituto essendo un po' ristretto devo fare molte lezioni; dal Novembre al Maggio feci nove ore di lezione per settimana sulle costruzioni e sull'idraulica. Ciò vi spieghi come lavorando come forse non mai in vita mia, pure non arrivo a pubblicare, giacché se giungo anche a qualche risultato non trovo il tempo per renderlo presentabile al pubblico"; *Part I*, [Palladino *et al.* 2009].

4 Letter of the 6th October 1870, *Part II*, [Palladino *et al.* 2009].

5 Letter 42, *Part I*, [Palladino *et al.* 2009].

6 See letter 8, *Part I*, [Palladino *et al.* 2009].

7 "Sei o sette giorni al mese sciupati in viaggio"; letter of the 23rd November 1870 to Cremona; *Part I*, [Palladino *et al.* 2009].

8 "Assai le abitudini studiose"; letter of the 23rd November 1870, *Part I*, [Palladino *et al.* 2009].

9 "Se non fossi stato a Roma gli scorsi giorni e non avessi dovuto toccare con mano quanto c'è da fare, probabilmente non avrei accettato; ma ho potuto convincermi che è necessario spiegare una grande attività, ed una prudente energia, qualità sulle quali mi pare di poter contare. Perciò manderò or ora un dispaccio di accettazione"; letter 58, *Part I*, [Palladino *et al.* 2009].

10 "Buone ragioni, difficoltà di lingua, salute, ecc."; letter 59, *Part I*, [Palladino *et al.* 2009].

permission to teach at the Ginnasio in his city and at Liceo Beccaria in Milan –before the Italian Union– and then, "well-disposed to talk to the Minister about your aptitude for teaching and the importance of your works"[11], Brioschi obtained that Cremona was appointed as professor at the University of Bologna.

Cremona did his best to unify Italy: the first sign is the journey around south of Italy, in 1863; it was not only an ideal continuation of his relationship with Neapolitan volunteers during the defence of Venice against the Austrian, but it seemed also a recognition to know the problems and act better.

At the end of the battles of Solferino and San Martino, in an atmosphere of militant euphoria, Cremona was risen up by Brioschi to a higher level then a pupil, as he really did at the *Instituto Tecnico Superiore* and in the *Annali*.[12]

From Genocchi's letters (in "Archivio Betti", stored at the library of Scuola-Normale in Pisa; Betti's letters are at the Passerini–Landi library in Piacenza) the importance of his personality emerges. He was not a simple spectator destined to applaud others:

> "I thank you for your dedications: I always read them. But I am old and I can't see always the same words. Even if I desire to do a lot of things, my strength fails me; and I can just applaud people braver and more capable than me".[13]

Genocchi is fifty, he is the oldest of the group, but he was the only one to dedicate his entire day to research (he confessed to Betti: "I prefer my quiet more than the interference of a chair"[14]); he had a lot of interests but the most important was mathematical analysis and theory of numbers. He also was a great scientific interlocutor: he read, asked for explanations, reflected and honestly commented works that his correspondents sent him; it seems that they often used him as a "tester" to verify the quality of their works. He was a sort of "scientific uncle" also ready to "spank" Giulio Ascoli.[15]

As every letter in which Genocchi is the correspondent, also these addressed to Betti had a high scientific density. They talked, for example, about problems of didactics of mathematics ("to force to explain Euclide"[16] in Italian schools) but also other important questions: there is a long discussion on pseudo-spherical surfaces, about which the sceptical Genocchi said that, to continue discussing it is essential to "demonstrate that these surfaces exist"[17]; in letter 27[18] there is another important discussion with Betti (in the sixties, he was dedicated to research about the theory of elasticity[19]) where Genocchi showed his uncritical acceptance, as it was usual at that time, of "postulate" according which "all functions have a derivative".

Genocchi was austere and severe (he disagreed, for example, with the way that Universities, the *Consiglio Superiore* of Public Education and the Ministry overused *art. 69* of Casati Law, that is to say the direct call-up, "for good reputation", of professors[20]) but he was partial for professors of the University of Turin: he often conducted their defence and he was always ready to give them his recommendation, both for less good and for the best.[21] When a good fight entered in his heart, Genocchi always dedicated to it the right consideration, as it happened for the admission of Sonja Kovalevski at the Academy of Sciences in Stockholm: in Italy he became active to support her admission.[22]

During the correspondence with his Turinese interlocutor (his letters are fifty-one while Betti's answers, always short, are twenty-four), the Tuscan scientist, who, during the formation of the Italian Kingdom, often went to Turin as deputy of Pistoia and could meet Genocchi, was involved in that issue because he was member of the *Consiglio Superiore* of the Ministry of Public Education and for his studies in physical mathematics. The letters 12, 18, 26, 31, 38, 59[23] from Betti to

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[11] "Dispostissimo a far conoscere al Ministro la vostra attitudine all'insegnamento e l'importanza dei vostri lavori"; lettera 15, *Part I*, [PALLADINO *et al.* 2009].

[12] See letter of the 26th June 1859, *Part I*, [PALLADINO *et al.* 2009].

[13] "Vi ringrazio delle vostre importanti Memorie che mi favorite e che io leggo sempre con interesse e con mio profitto. Ma io sono vecchio e ormai non posso uscire da cose trite e ritrite, benché non mi manchi la voglia di far meglio: le forze mi mancano; e solo mi è dato di applaudire ai più arditi e valenti di me"; letter 16, *Part II*, [PALLADINO *et al.* 2009].

[14] "Ho sempre preferita e preferisco la mia quiete alla vanità di avere un'ingerenza qualunque nel conferimento delle cattedre"; lettera 42, *Part II*, [PALLADINO *et al.* 2009].

[15] Letter 58, *Part II* of [PALLADINO *et al.* 2009].

[16] "Sull'obbligo imposto di spiegar Euclide"; letters 17, 18, *Part II*, [PALLADINO *et al.* 2009].

[17] "Dimostrare che tali superficie esistano"; letter 44, *Part II*, [PALLADINO *et al.* 2009].

[18] *Part II*, [PALLADINO *et al.* 2009].

[19] See [CAPECCHI *et al.* 2006].

[20] Letter 52, *Part II,* [PALLADINO *et al.* 2009].

[21] See the letter 49, *Part II*, [PALLADINO *et al.* 2009].

[22] Letters 75, 76, 77, *Part II*, [PALLADINO *et al.* 2009].

[23] In *Part II*, [PALLADINO *et al.* 2009].

Genocchi are very important: those letters present some consideration about non-Euclidean geometry and Helmholtz's physiological optics.

The documents here collected until now represent the most important cards of a patchwork setting up the time when a country, Italy, that, even if varied, was joined together after so many centuries under the same entity of State and it tried to build a stronger political, civil and scientific future: that historical period required the presence of great national states and the Italian ambition was that of conquering an outstanding place in the European nations "concert" or, at least, the honor of a first line place in the "haute culture" under the great "Italians" flag that until the 17th century had lived up to the whole peninsula. This is the direction where Genocchi, for example seems to look at when corresponding with Betti; he praised the scientific activity carried out by the latter:

> "You also work a lot and with your and Italy's honor. I cannot play any other role than the spectator and I am delighted in clapping the talented actors".[24]

The epistolary exchanges published in this volume, fitting together one in another, realize images evoking a heroic period lived by the Italian people or, at least, from a coat, much as thin, of it. These are images representing rush actions, gone along with a considerable sense of dignity. We can also compare the publishing of these epistolary exchanges, still, to the act which gives voice to some poets; they take part in a choir which can bank on a large number of qualified voices.

The new structures and the new rules building up in this time, will impress on a territory wider and wider since, at first, the Kingdom of Sardinia with the united Lombardy (a sort of "Regno dell'Italia Superiore" as Brioschi writes[25]), and they will find themselves being broadened, a month later or so, to the Italian Kingdom with Turin as its capital at first, from 1861, and then Florence from 1865 to 1871, to pass afterwards to Rome. An important example, from this point of view, is given by the passing of the *Casati Law*. It concerned the whole public education, of every order and rank, and it approved a part of the law project presented, in 1854, from Cibrario, Minister of the Public Education of the Kingdom of Sardinia. The *Casati Law*, thought for the "Superior Italian Kingdom", will be expanded gradually, with some modifications and integrations, to the whole Italian Kingdom: it was elaborated according to a politic view and it assigned to the highest level of education a constitutional organism function which had to ensure the birth and growth of a national science, the formation of the class heading the country not submitted to the clergy influence, the validity of the educational qualifications, the access to the freelances, the grounding of new grammar school professors and so on. Consequently to the listed reasons, the *Casati Law* calculated to give to the State the entire control and the financial burden of the superior Education, cutting out any private or clerical participation.[26]

A progressive, considerable documentation is collected supporting the statement according to which, once the Italian Unity reached, it produced a renewed involvement for research, an increase in the chances offered to the experts. In particular, the mathematics enthusiasts –that had previously already found the way, on the occasion of the invitations to the yearly *conferences* of the *Italian scientists* which went over from 1839 to 1847,[27] of giving themselves some "solemn occasions" of meeting and, subsequently, they continued actively to look for each other– could take more advantages of the new politic affair. With a remarkable enterprise, they made a good use of the motivating forces that came from the natural intensifying of their relations. Having an edge, in living this occasion of growth, from the fact that their need of scientific equipment and teaching aids were not, as the usual procedure of that time, very strong and so to compromise, in the absence, their capacity of realizing their potentials.

The starting limits outlined a picture of backwardness –less evident in the Grand Duchy of Tuscany, though of little size entity (but where they were well decided, after the risings of 1848, to close the two Universities existing at that time, Pisa and Siena, which were later re-opened by the provisional Government of Tuscany in 1858-59)– in some districts in Emilia and Romagna and in Piedmont, that could count, looking to the grass roots, on a quite strict and common ethical behaviour but, especially, it could have a controlled State deployment; and where also the capital before the Italian unification, Turin, was enriched in some measure by the people who came from the different states of the peninsula, taking refuge there. It is known that during the so-called "preparation decade" for the Unity, many exiles, leading figures of the future Italian governing class, were received in Turin University (but not always with full acceptance by their colleagues). However, the state apparatus of the Piedmont monarchy, despite the presence of a Parliament (the Kingdom of Sardinia was the only Italian state having a Parliament), got to wield forms of control, over its own governed people, even too extreme; more or less in the way of a Bourbon government, as it seems to be from Genocchi's uneasiness confided to Brioschi.[28]

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24 "Voi pure lavorate molto e con onore vostro e dell'Italia. Io non potendo oramai sostener altra parte che di spettatore godo di batter le mani ai valenti attori"; letter of the 7th June 1868, *Part II*, [PALLADINO *et al.* 2009].

25 "Italy's Superior Kingdom"; letter of the 22th July 1859, *Part I*, [PALLADINO *et al.* 2009].

26 See [MORETTI *et al.* 2001], [POLENGHI 1993], [PORCIANI 1994], [PORCIANI 2001].

27 The places of the *meetings* were: Pisa, Turin, Florence, Padua, Lucca, Milan, Naples, Genoa, Venice. See [PANCALDI 1989], [BARTOCCINI *et al.* 1952].

28 Letter of the 29 August 1858, in [CARBONE *et al.* 2006], p. 381.

The south part of the peninsula, the south which Cremona knew personally and a little bit better both for thrusting to the far provinces[29] and for staying in Naples for many,long times –it was in this city that his daughter Itala lived as wife of the Neapolitan doctor Vincenzo Cozzolino–, was a definitely a different world.

It already was, anyway, a cause of wonder if the Kingdom of Naples along with Sicily (the largest and most populated of the reigns before unification, and the second for richness production after the Lombardo-Veneto), preserved itself, in broad cultural, civil and religious terms, clung to the European continent. It was a piece of Italy that was felt as far away. But, even if in its irregular way of life in the crowded capital, which naturally represented its core, the Kingdom of Naples could be proud, if nothing else in the Italian area, of some supremacy[30] and could count upon the presence, often very active in the capital and somewhere else, of all those institutions, including the cultural expressions, which connoted a country as civil while, provided with a large hard-working youth, and studious,[31] mainly centered in the capital itself, it had its intellectuals' view addressed to the central and northern –more advanced– part of Europe with which they actively interacted.

References

BARTOCCINI, VERDINI (1952) *Sui Congressi degli Scienziati*, Roma, Edizioni dell'Ateneo.

BRIGAGLIA, DI SIENO (2009) *L'opera politica di Luigi Cremona attraverso la sua corrispondenza, in La Matematica nella Società e nella Cultura. Rivista dell'Unione Matematica Italiana*, Bologna, Serie I, vol. II.

BRUNATI, FRANCHETTI, PAPAGNA, P. POZZI (2000) *Francesco Brioschi e il suo tempo (1824-1897)*, vol. II, *Inventari*, Milano, F. Angeli.

CAPECCHI, RUTA, TAZZIOLI (2006) *Enrico Betti: Teoria della Elasticità*, Benevento, Hevelius Edizioni.

CARBONE, GATTO, PALLADINO (2001) *L'epistolario Cremona-Genocchi (1860-1886)*, Firenze, Olschki.

CARBONE, GATTO, PALLADINO (2002) Una comunità e un caso di frontiera. L'epistolario Cremona-Cesàro, *Memorie dell'Accademia di Scienze Fisiche e Matematiche*, Napoli, Liguori.

CARBONE, MERCURIO, PALLADINO, PALLADINO (2006) La corrispondenza epistolare Brioschi-Genocchi, *Rendiconto dell'Accademia delle Scienze Fisiche e Matematiche di Napoli*, s. IV, vol. LXXIII.

CERRONI, FENAROLI (2007) *Il Carteggio Cremona-Tardy*, Milano, Mimesis.

GASCA (1992) *La corrispondenza di Luigi Cremona (1830-1903)*, v.I, quaderno n.24, Università di Roma La Sapienza, Roma.

LACAITA, SILVESTRI (2000) *Francesco Brioschi e il suo tempo*, vol.I, *Saggi*, Milano, Angeli.

LACAITA (2003) *Francesco Brioschi e il suo tempo*, vol.III, *Scritti e discorsi* Milano, Angeli.

MENGHINI *et al.* (1994) *La corrispondenza di Luigi Cremona*, quaderno n. 3, vol. II, Università di Roma La Sapienza, Roma.

MENGHINI *et al.* (1996) *La corrispondenza di Luigi Cremona (1830-1903)*, vol. III,"Per l'archivio dei matematici italiani" *Quaderni P.RI.ST.EM*. n.9.

MERCURIO, PALLADINO (2007) Intorno alla risoluzione delle equazioni algebriche di quinto grado per funzioni ellittiche in Betti e Brioschi, *Accademia Nazionale di Scienze Lettere e Arti di Modena – Memorie Scientifiche, Giuridiche, Letterarie*, s.VIII, vol.X.

MICHELACCI (2005) Lettere di Charles Hermite a Angelo Genocchi, *Bollettino di Storia delle Scienze Matematiche*, XXV.

MORETTI, PORCIANI (2001 [1998]) *Il volto ambiguo di Minerva. Le origini del sistema universitario italiano*, in *Ricerca e istituzioni scientifiche in Italia*, Bari, Laterza.

NURZIA *et al.* (1999) *Per l'Archivio della Corrispondenza dei Matematici Italiani. La corrispondenza di Luigi Cremona*, vol.IV, *Quaderni P.RI.ST.EM*. – Università "L. Bocconi", Milano, n.11, Palermo.

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[29] See also the letter from Cremona to Genocchi of the 18th January 1863, edited in [CARBONE et al. 2001], pp. 73-74.

[30] See [PALLADINO *et al*. 2009], pp. XIII-XIV.

[31] See [PALLADINO 1999 b], pp. 236-237.

PALLADINO (1999a) *Il Fondo di modelli e strumenti matematici antichi dell'Università di Padova e l'iniziativa di Giuseppe Veronese per un Laboratorio Nazionale Italiano*, Padua, Università degli Studi-Dipartimento di Matematica pura ed applicata.

PALLADINO (1999b) *Metodi matematici e ordine politico*, Napoli, Jovene.

PALLADINO, PALLADINO (2006) *Dalla "moderna geometria" alla "nuova geometria" italiana*, Firenze, Olschki.

PALLADINO, MERCURIO, PALLADINO (2009) *Per la costruzione dell'Unità d'Italia. Le corrispondenze epistolari Brioschi-Cremona e Betti-Genocchi*, Firenze, Olschki.

PANCALDI (1989) *I congressi degli scienziati italiani nell'età del positivismo*, Bologna, CLUEB.

POLENGHI (1993) *La politica universitaria italiana nell'età della Destra storica*, Brescia, La Scuola.

PORCIANI (1994) *L'Università dell'Italia unita, in Università e professioni giuridiche in Europa nell'età liberale*, Napoli, Jovene.

PORCIANI (2001) *Università e scienza nazionale*, Napoli, Jovene.

BOTTAZZINI (1994) *Va' pensiero. Immagini della Matematica nell'Italia dell'Ottocento*, Bologna, Il Mulino.

BOTTAZZINI (1998) Francesco Brioschi e la cultura scientifica nell'Italia post-unitaria, *La matematica nella Società e nella Cultura – Bollettino U.M.I.*, (VIII) 1-A.

SCIENCE, ASTRONOMY AND INSTRUMENTS FROM THE MIDDLE AGES TO THE 17TH CENTURY

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THE RECEPTION IN THE WESTERN AREA OF A SAME METHOD FOR TWO MEDIEVAL ASTROLOGICAL PRACTICES

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Abstract

In Islamic medieval astrology, there were three basic practices used to cast a horoscope: 'the determination of the celestial houses', 'the prorogation' and 'the projection of rays'.
A horoscope is a representation of the celestial sphere in a specific moment and for a given latitude. The equator and the ecliptic are maximum circles of the celestial sphere; the ascendant of the horoscope depends on the local horizon. One or several of these circles intervened in the computation of the above mentioned practices. Likely, this fact helped to the establishment of a link between them, even though they had different aims and an independent development with their own methods.
My study is devoted to the connection between the prorogation and the projection of rays and to the dissemination, in the Islamic West, of a common method for these techniques: The Single Hour Line Method. To date, what we knew about it is that it was the standard calculation for the projection of rays since the 9th century.
However, in the Islamic East in the second half of the 10th century, The Single Hour Line Method was also used for the prorogation in a book dedicated to the prince of Aleppo Sayf al-Dawla (945-967) The Introduction to the Craft of Astrology *by al-Qabīṣī. In the Mediterranean Western Area, the popularity of al-Qabīṣī's book was such, that its Latin translation was still being printed in 1521.*
The Single Hour Line Method for the prorogation was followed as well by a Moroccan astrologer of the 15th century, al-Baqqār: in his text, the use of several technical terms denotes that it was written by a Western astrologer. The present work is based in al-Baqqār's contribution.

Introduction

This article deals with an astrological procedure documented in the Islamic East, The Single Hour Line Method for the prorogation, and its reception in the Maghrib in Abū 'Abd Allāh al-Baqqār's work (fl. Fez, *ca*. 1411-1418) (Díaz-Fajardo, 2001; Guesmi, 2005) *Kitāb al-adwār fī tasyīr al-anwār* (*Book on cycles for the prorogation of the celestial elements*) where this method incorporated principles of the Andalusian astronomical tradition.

In this paper I shall explain: (a) how prorogation and projection of rays are related by means of The Single Hour Line Method, a common method for both astrological procedures; (b) the sources in which al-Baqqār is based and (c) the transformation of this common method in the Islamic West.

The Single Hour Line Method

In the projection of rays and the prorogation, The Single Hour Line Method distinguishes the computation according to the position of the indicator on the meridian, on the horizon or between the meridian and the horizon. For briefness, in this paper I only consider the case of an indicator placed on the meridian.

1. Relation between prorogation and projection of rays

The prorogation and the projection of rays were indispensable appliances for interpreting a horoscope, and they are based in mathematical computations that follow astronomical rules. There is a basic astrological concept in both techniques, the celestial element which is placed in the circle of the ecliptic, may be moved on the same circle of the ecliptic or on the circle of the equator. This motion has the purpose of reaching another imaginary position in which the astrologer establishes the celestial influences.

1.1. The Single Hour Line Method for the projection of rays

The projection of rays states that a celestial element (a planet generally) sends its nature or influence to a second element. The planetary influence can be beneficial or malefic and brings to the second element the change of its original condition.

The act of transmitting the nature is known in Arabic texts as projection of rays and it takes place when the second celestial element is placed within the domain of the planet, namely, when between the planet and the second element there is a distance of sixty, ninety, one hundred and twenty, or one hundred and eighty degrees.

The most common calculation for the projection of rays was The Single Hour Line Method (Kennedy and Krikorian-Preisler, 1983: 372-375; Hogendijk, 1998; Hogendijk, 2005: 91-92; Casulleras, 2007: 38-41; Casulleras, 2010: 88-92), which owes its denomination to the scholar Jan P. Hogendijk. This method remained in use in the East until the first half of the 15^{th} century and in the West until the 14^{th} century.

When a planet is placed on the meridian, the simplest procedure is employed, and its computation is as follows: the right ascension (α_0) of the longitude (λ) of the planet (p) is added to the number of degrees of the ray (R) (sixty, ninety, one hundred and twenty, or one hundred and eighty degrees) = $\alpha_0(\lambda p) + R$.

The projection of the ray equals to the former amount converted into ecliptical degrees = α_0^{-1} [$\alpha_0(\lambda p) + R$].

1.2. The Single Hour Line Method for the prorogation

In the prorogation technique, the first indicator, a significant celestial element for the subject of the horoscope like his ascendant, may be moved or prorogated on the circle of the equator in order to find a second indicator that, due to its malefic nature, has the ability of stopping the motion of the first element. The two indicators delimit an arc on the equator that, in medieval Arabic texts, was called 'prorogation arc' which is the distance between both indicators in equatorial degrees. The equatorial distance agrees with time according to a system of equivalences that matches equatorial degrees and years, months or days. In this way, the astrologer could determinate the date of any event.

The prorogation was also used in a second way consisting in the computation of the ecliptical degree or terminal point at which the motion of the celestial element finishes for a specific year.

The computation for the latter purpose works as follows:

When the indicator is placed on the meridian, the right ascension (α_0) of the longitude (λ) of the indicator (d) is added to the number of total solar years (Y) elapsed since an event, for instance, the elapsed years since a birth until the date in which the horoscope is cast or until the date in which the prorogation is calculated = $\alpha_0(\lambda d) + Y$.

The terminal point of the prorogation is equal to the former sum converted into ecliptical degrees = α_0^{-1} [$\alpha_0(\lambda d) + Y$].

As we may see this procedure for the prorogation and the procedure of projection of rays explained in section 1.1 above is the same method: it is the standard calculation for the projection of rays called The Single Hour Line Method, but applied to the prorogation technique. The total solar years (Y) elapsed since a specific event must be used in the computation for obtaining the prorogation instead of the number of degrees of the ray (R) used in the projection of rays:

Terminal point of the prorogation = α_0^{-1} [$\alpha_0(\lambda d) + Y$]

Projection of ray = α_0^{-1} [$\alpha_0(\lambda p) + R$]

The use of this method in the prorogation technique and its relation with The Single Hour Line Method for the projection of rays had not been identified, until now.

1.2.1. Sources

The Single Hour Line Method for the prorogation was followed by the Eastern astrologers al-Qabīṣ ī (fl. Mosul, 10th century) (Burnett, Yamamoto and Yano, 2004: 124-126), Kūshyār ibn Labbān (Gīlān, *ca*. 971-1029) (Yano, 1997: 232-235) and al-Bīrūnī (Khwārizm, 973-*ca*. 1048) (*Kitāb al-qānūn*, III: 1393-1395; Hogendijk, 1998) and by the Western astrologers Ibn al-Raqqām (manuscript (ms) 2461, National Library, Rabat, p. 222) and al-Baqqār in his *Kitāb al-adwār* (ms 916, El Escorial Library, Madrid, ff. 248v-249r; ms 5372, al-Ḥasaniyya Library, Rabat, pp. 23-24; ms 826, al-Ḥasaniyya Library, Rabat, ff. 97v-98r).

According to the extant sources, The Single Hour Line Method for the projection of rays is found in the 9th century in the writing of Abū Ma'shar (Balkh, *ca*. 787-*ca*. 886) (Lemay, 1995: vol. III, 549-550) and the same method for the prorogation is documented long after, in the 10th century in the *al-Madkhal* of al-Qabīṣ ī.

Nevertheless, The Single Hour Line Method for the prorogation possibly predates the 10th century. Al-Qabīṣ ī never assured to be its author, in fact he states that he collected the very essential of the astrological knowledge. It is difficult to clarify the source that al-Qabīṣ ī used for the section of prorogation since he did not mention it. In al-Qabīṣ ī's words: 'Since we have completed the explanation of what is known of the technical terms of the astrologers…'.

In light of all the above we should question ourselves which method really came first. Al-Bīrūnī shall help us to answer this matter: according to him in the *Kitāb al-qānūn* (p. 1394), 'this procedure [he means The Single Hour Line Method] for the projection of rays came from the procedure of prorogation'.

Although al-Bīrūnī does not specify what method of prorogation he refers, we may deduce that is The Single Hour Line Method for the prorogation because of its close similarity with The Single Hour Line Method for the projection of rays (see section 1.2 above).

Therefore, as indicated in the testimony of al-Bīrūnī, The Single Hour Line Method for the prorogation came out before, and then was applied to the technique of projection of rays.

The Single Hour Line Method for the prorogation was one of the most ancient procedures for the prorogation on the equator used in Islamic astrology, and it probably appeared in the first half of the 9th century or earlier before.

The Single Hour Line Method for the prorogation in al-Baqqār's book

1. Sources

The fourth chapter of the summary of astrology *al-Madkhal ilà ṣ inā'at aḥ kām al-nujūm* of the Eastern astrologer al-Qabīṣ ī has been arranged into fifteen sections, each one of them devoted to a different astrological matter. Al-Qabīṣ ī explains in the prorogation section (see section 1.2.1 above) The Single Hour Line Method for the prorogation.

Al-Baqqār does not quote the name of al-Qabīṣ ī, but he might use *al-Madkhal* as his source, because the passage, in which The Single Hour Line Method for the prorogation is described, is almost the same in both astrologers' writings and the existence of an al-Qabīṣ ī's book translation into Latin points out that *al-Madkhal* was known in al-Andalus.

2 Transformation

Al-Baqqār enters the computation of The Single Hour Line Method for the prorogation using the tropical degree of the indicator. Al-Baqqār argues that 'when the indicator is placed on the tenth or on the fourth house [it means, it is placed on the meridian], you add the number of years to the ascensions of the tropical degree of the indicator on the right sphere, and then you convert them into ecliptical degrees with a table of right ascensions. Wherever it falls on the ecliptic (on the sidereal sphere), the prorogation in that year will arrive there'.

Al-Baqqār speaks about 'the tropical degree' (*al-daraja al-ṭ abī'īya*) and about 'the sidereal sphere' (*al-falak al-dhātī*). These expressions do not appear in al-Qabīṣ ī's text. The introduction of 'tropical' (*ṭ abī'ī*) and 'sidereal' (*dhātī*) terms in al-Baqqār's book is really significant because this language is used by Western astronomers to distinguish between tropical and sidereal longitudes and it is often found in writings focusing on the trepidation theory (Samsó, 2001: 169-174; Díaz-Fajardo, 2001: 24-25; Comes, 2002: 130-131).

In medieval Arabic astrology, the expression *al-mabdā' al-ṭ abī'ī* refers to a longitude calculated from the vernal point and the expression *al-mabdā' al-dhātī*, to a longitude calculated from a star called The Head of Aries.

Andalusian astrology usually deals with sidereal positions and not tropical. Sidereal positions are computed from the position of a very specific star, which in an historical moment in the past was placed in front of the vernal point. Therefore, we might deduce that the position of the indicator which the astrologer prorogates is a sidereal position. However, the procedure of prorogation required the use of right ascensions, which are computed from a point of the equator placed at a distance of

90º from the vernal point corresponding to Capricorn 0º on the ecliptic (right ascensions in medieval tables). In this case, it is clear that the astrologer must work with tropical positions. That is why al-Baqqār mentions that the position of the indicators must be tropical when computing prorogation.

Furthermore, al-Baqqār ends the explanation of the computation with the following words: 'Subtract from the result the accession (*al-iqbāl*) motion or add to it the recession (*al-idbār*) motion. The prorogation for that year will finish at the resulting degree'. In this paragraph, there is a statement that does not appear in al-Qabīṣ ī's text: 'Subtract from the result the accession motion or add to it the recession motion'. Al-Baqqār is indicating the effects of trepidation theory on the celestial longitudes.

The theory of trepidation was an attempt to explain the constant motion of the equinoxes, known as the precession of the equinoxes. The followers of this doctrine believed that the celestial sphere made a first motion of accession (*iqbāl*) and a second motion of recession (*idbār*). During the motion of accession, a tropical longitude increase occurred as the vernal point went back, while during the motion of recession, there was a tropical longitude decrease as the vernal point went forward in the order of the zodiac signs.

* * *

The tropical longitude may contain a negative or a positive value of precession. By introducing these amounts in the formula, al-Baqqār obtained a sidereal longitude, that is, a longitude where precession is not present. Hence:

During the phase of accession:

Sidereal terminal point of the prorogation = $(\alpha_0^{-1} [\alpha_0(\lambda d) + Y])$ – value of precession

During the phase of recession:

Sidereal terminal point of the prorogation = $(\alpha_0^{-1} [\alpha_0(\lambda d) + Y])$ + value of precession

To obtain the terminal point of the prorogation, al-Baqqār achieved the mathematical operations using the tropical longitude, but for the astrological interpretation, he worked with the sidereal longitude that is measured on the circle of the zodiac signs. As al-Baqqār himself explains, 'sidereal longitudes must be used for astrological matters in which you have to verify the celestial influences' (Díaz-Fajardo, 2001: 24-25, 78-79).

First, al-Baqqār followed the Eastern tradition for prorogation computing. This tradition is showed in al-Qabīşī's text: al-Baqqār, likely following al-Qabīşī, added the ascension (α) of the longitude (λ) of the indicator (d) to the number of years (Y) ($[\alpha(\lambda d) + Y]$) and then, he searched for the inverse ascension (α^{-1}), i.e., the longitude that corresponds to the ascension of the sum.

Second, al-Baqqār adapted the method of prorogation to the Western astronomical context. This Andalusian-Maghribi tradition settles two kinds of longitudes: one tropical and another one sidereal. Al-Baqqār mentions that he worked with the tropical degree of the indicator and that the terminal point of the prorogation finishes in the sidereal sphere [the place where sidereal longitudes are measured].

Conclusions

Medieval Arabic astrologers took advantage of the available knowledge to carry out their trade: The Single Hour Line Method had a double function due to its use in the prorogation and the projection of rays. This relationship clarifies why some astrologers referred to two different practices as a single discipline with the following words, 'the science of prorogation and projection of rays (*'ilm al-tasyīr wa-maţārih al-ashi''a*)'.

Finally, the study of al-Baqqār's book illustrates the transmission of the astrological ideas in the Middle Ages. Al-Baqqār introduced a terminological feature in his text, which implies that the ruling astronomical practice in Western Islamic astronomy, the trepidation theory, was applied to an Eastern astrological procedure, The Single Hour Line Method for the prorogation.

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Bibliography

AL-BĪRŪNĪ, *Kitāb al-qānūn al-mas'ūdī*, 3 vols., Hyderabad, Osmaniya Oriental Publications Bureau, 1954-1956.

BURNETT, Ch., YAMAMOTO, K. and YANO, M. (2004) *Al-Qabīṣī (Alcabitius): The Introduction to Astrology*, London-Turin, Warburg Institute Studies and Texts.

CASULLERAS, J. (2007) "El cálculo de aspectos o la proyección de rayos en la astrología medieval árabe", *Archives Internationales d'Histoire des Sciences* 57, 25-46.

CASULLERAS, J. (2010) *La astrología de los matemáticos. La matemática aplicada a la astrología a través de la obra de Ibn Mu'āḏ de Jaén*, Barcelona, Publicacions i Edicions de la Universitat de Barcelona.

COMES, M. (2002) "Some New Maghribī Sources Dealing with Trepidation", in S.M. RAZAULLAH ANSARI (ed.) *Science and Technology in the Islamic World, Proceedings of the XXth International Congress of History of Science (Liège, 20-26 July 1997)*, vol. XXI, Brepols, De Diversis Artibus; Collection de Travaux de l'Académie Internationale d'Histoire des Sciences, 121-141.

DÍAZ-FAJARDO, M. (2001) *La teoría de la trepidación en un astrónomo marroquí del siglo XV. Estudio y edición crítica del Kitāb al-adwār fī tasyīr al-anwār (parte primera) de Abū 'Abd Allāh al-Baqqār*, Barcelona, Anuari de Filologia (Universitat de Barcelona) XXIII, B4; Instituto "Millás Vallicrosa" de Historia de la Ciencia Árabe.

GUESMI, Ch. (2005) *El* Kitāb al-amṭār wa'l-as'ār *de Abū 'Abd Allāh al-Baqqār. Edición crítica y estudio*, Barcelona, Universitat de Barcelona.

HOGENDIJK, J.P. (1998) "Progressions, Rays and Houses in Medieval Islamic Astrology: A Mathematical Classification", paper presented at the Dibner Institute Conference *New Perspectives on Science in Medieval Islam*, Cambridge-Mass., November 6-8.

HOGENDIJK, J.P. (2005) "Applied Mathematics in Eleventh Century Al-Andalus: Ibn Mu'ādh al-Jayyānī and his Computation of Astrological Houses and Aspects", *Centaurus* 47, 87-114.

KENNEDY, E.S. and KRIKORIAN-PREISLER, H. (1983) "The Astrological Doctrine of Projecting the Rays", in D.A. KING and M.H. KENNEDY (eds.) *Studies in the Islamic Exact Sciences*, Beirut, American University of Beirut, 372-384.

LEMAY, R. (1995) *Abū Ma'šar al-Balkhī [Albumasar], Kitāb al-madkhal al-kabīr ilà 'ilm ahkām al-nujūm, Liber introductorii maioris ad scientiam judiciorum astrorum, Édition critique*, Napoli, Istituto Universitario Orientale.

SAMSÓ, J. (2001) "Astronomical Observations in the Maghrib in the Fourteenth and Fifteenth Centuries", *Science in Context* 14 (1/2), 165-178.

YANO, M. (1997) *Kūšyār ibn Labbān's Introduction to Astrology. Edited and Translated by Michio Yano*, Tokyo, Studia Culturae Islamicae 62; Institute for the Study of Languages and Cultures of Asia and Africa.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 946.

THE ARTISANS OF ‘PLUS ULTRA’: PILOTS, CARTOGRAPHERS, AND COSMOGRAPHERS IN THE *CASA DE LA CONTRATACIÓN* IN SEVILLE DURING THE SIXTEENTH CENTURY[1]

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Abstract

In the context of the growing historiographical interest that is causing the world of Iberian science, especially among non-Spanish authors, during the Early Modern period after the Discovery of America, in this paper the author shows how it carried out the grand project of institutionalization of cosmography throughout the sixteenth century, from the Catholic Monarchs to Philip II. Through the inner workings of the House of Trade, the Indian Council, and the Academy of Mathematics this paper highlights how the Universal Monarchy and its conqueror slogan ‘Plus Ultra’ tried to encircle the Atlantic world first, and to dominate the New World later, by means of Cosmography, an essential science for the maintenance of the overseas Empire[2].

The aim of this paper is to demonstrate how the institutionalization of cosmography and navigation ended with the slogan Non Terrae Plus Ultra*, and led to the emergence of imperial heading* Plus Ultra *helped by the navigation of a* Mare Tenebrosum *(the Atlantic Ocean), and the delineation of the contours of a new world that began beyond the Columns of Hercules. This process was made possible by the establishment in Seville of the House of Trade in 1503 and the creation of scientific offices such as Pilot Major, master of making nautical charts or cosmographer. The ship that appears on the cover of the* Regimiento de navegación *(1606) by Andres Garcia de Céspedes across the pillars of the hero of Greek mythology highlights the Baconian premise of man’s dominion over nature, the knowledge gained through the conquest of the West Indies, and also the wishes of the Spanish Monarchy by taking advantage of the usefulness of scientific knowledge by joining the nautical experience and cosmographical theory.*

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[2] On this subject, the author of this paper has recently published a larger study. See Antonio Sánchez Martínez, “Los artífices del *Plus Ultra*: pilotos, cartógrafos y cosmógrafos en la Casa de la Contratación de Sevilla durante el siglo XVI”, *Hispania*, LXX (236): 607-632.

1. Since 1503, Spanish nautical and cartographic activity was organized in the House of Trade (*Casa de la Contratación*) in Seville. Its members offered classes in navigation, designed nautical tools and made maps while information from the Indies was centralized and systematized[3].

In 1508 was established the post of Pilot Major, devoted to draw nautical charts. The House created in this way its department of cosmography, with a school of navigation dedicated to training pilots and making maps. Pilot Major was the first scientific position of the House, and he should examine the navigators of the Indies, approve their instruments, draw and correct the charts and keep them secret. One of the most important tasks of the Pilot Major was the production of a model chart –called *Padrón Real*– that would reflect, with updated information, all exposed coastlines.

The establishment of the first pilot of the House has been considered the origin of the first technical school in Europe, where the development and application of scientific knowledge was led to improvements in ocean navigation. At the age of exploration, if the map had the virtue of opening the windows of the empire, navigation was the real eye of the kingdom.

2. The post of Pilot Major was assigned during the sixteenth century by Royal Decree, although the criteria for election depended on the appointment of the king or a sort of competitive examination. This latter procedure depended of an expert tribunal in cosmography, whose election was obtained first, a proposal of three persons to the Indian Council, where the fortunate was finally appointed. The political and scientific direction taken by the post of Pilot Major not only favoured the specialization of the sailors, but also saw increased the social status of a group of workers of the ocean who had not previously enjoyed any prestige.

What people could aspire to the position of Pilot Major and what were the conditions to qualify for it? The requirements were similar to those required to become a pilot. Often, candidates were experienced sailors in the art of navigation, but experience was not everything. Its expertise in maritime matters had calmed its experimental character in favour of the resolution of technical problems. These wise pilots should have the ability to unite theory and practice to discipline also the experience of young navigators. In addition, men were admitted for examination who were more than twenty-four, people who would not reveal the secrets of their trade, navigators with a minimum of six years experience in Indian and Atlantic navigation, and people of good habits and honest, Christian, and not foreigners, even when married to Spanish women. People previously excluded could not be admitted. Sometimes all these conditions were altered depending on the suitability and skill of the candidate. While Vespucci and some of his successors were foreigners, the court emphasized the importance of providing exclusively Spanish pilots for the Indian venture. The problem of treason was always present. The maps used for Atlantic navigation should not fall into foreign hands. The lack of training and perhaps the importation of new methods led the Spanish monarchs to offer these jobs to foreigners.

In 1519 was created the position of 'Master of making nautical charts'. Masters and cosmographers should make charts for the crossings to West Indies, after review and consideration of Pilot Major. Such maps could only be made by officers who had granted license. These permissions were the source of many of the disputes arising between 'officials' of the House. All they were trying to find an answer to the next question: who was prepared to make good charts? What was the interest of men who without possessing a license to make charts invaded the field of other authorized cartographers? What were the arguments dictating who should get permission to build charts and become a renowned cartographer, and who gave this credential?

In 1523 the mapmaker Diego Ribero was appointed cosmographer of the House. Thereafter, and until the creation of the Chair of Comography in 1552, the Pilot Major and the House's Cosmographer would be the two highest scientific positions to which any navigator could aspire.

In the House there were two kinds of cosmographers, some manufactured instruments and others were engaged in educational activities. Cosmography, guarded by men trained in Castilian universities and not at sea, exercised its power as the theoretical arm of navigation. The House's cosmographers with theoretical training had the ability to turn, which apparently were practical problems affecting only the pilots, in a case that should be resolved by universal laws. Only those who had learned in the classroom the Ptolemaic system of representation or the Euclidean geometry would be able to solve such problems.

Cosmography gained more reputation at the expense of pilots. Only the Pilot Mayor, an expert navigator, kept as much reputation as the House's cosmographers to control the experience of pilots. By the mid-sixteenth century, the House's

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[3] See Antonio Sánchez Martínez, "Cosmographers, Cartographers, and Navigators in Sixteenth Century Spain". In *Proceedings of the 3rd International Conference of the European Society for the History of Science*, Vienna, The Austrian Academy of Sciences, 2009.

cosmographers with scientific training were graduates of the Faculties of Arts. These universities offered an education dedicated to geometry, mathematics and the study of the cosmos. Once they were part of the crown, their work consisted in solving different problems of navigation. The cosmography of the House became the servant of navigation. In other words, navigation without cosmography was blind and cosmography without navigation useless.

3. What aspects did cosmographers prioritize in the arduous task of improving navigation? House's cosmographers tried to optimize and reform the methods of celestial navigation traditionally used by pilots, even knowing the difficulties that it would imply against the conservatism of a routine office-based practice. Many of these cosmographers were the authors of famous regiments that guided the pilots at sea.

This wave of cosmographers between the thirties and the seventies witnessed the interest of the crown not only to dominate the Atlantic and control the New World, but also to carry out this control in safety conditions. In these decades new positions were created related to cosmography. In 1563 Alonso de Santa Cruz would be the first Cosmographer Major of the House, a new post that was the precedent of Cosmographer Major of Indies.

The House's cosmographers would make maps and instruments, and also would reinforce navigation and cosmography with theoretical knowledge. During these four decades the Spanish monarchy attempted to combine practical experience with scientific data of the theory. In the inner workings of the House this task would not be easy. The results would only be reflected in the cartographic representations of the new possessions.

4. In 1552, given the organizational needs with the massive influx of geographic information to Seville and as a result of errors in nautical charts, the Chair of Cosmography was created. This chair allowed the Pilot Major to leave his teaching assignments. Jerónimo de Chaves would be the first professor of cosmography. In general, this professor was always a reputable cosmographer with a long career in nautical and cosmographical issues.

The precepts of the Ordinances of 1552 had a strong scientific aspect. The office of Pilot Major would share authority with other positions and was not the only person responsible for approving charts and instruments made in the House. Henceforth, any cartographer and instrument maker should submit its production to the Pilot Major and the House's cosmographers to be accepted. The scientific work of the House would fall into the hands of three offices: the Pilot Mayor, the Professor of Cosmography and the Cosmographer responsible for making nautical charts and nautical instruments.

5. Since its establishment, the office of Pilot Major was destined to the production and updating of a standard nautical chart, the *Padrón Real*, but also to the transmission of knowledge for future navigators. Education and training of pilots was one of the primary tasks for which this position was created. Men who wanted to be navigators should know the basic principles of celestial navigation, how to handle the quadrant and the astrolabe and, of course, reading a nautical chart correctly. The House combined from 1508 the urgency of the crown for having qualified pilots with the necessary training to discover new riches and transport them safely to Seville. At the beginning of the century, the House was the only school of nautical education in Europe, except for the one Henry the Navigator inaugurated in Sagres in the fifteenth century. Under the protection of the court, the school of Seville represented one of the first scientific institutions dedicated to education by the State in the early modern world. The House was also a school of geographical and cosmographical studies that formed the scientific basis of the expeditions to the New World. The court, the Indian Council, the Pilot Mayors and cosmographers joined forces for pilots to acquire practical and theoretical knowledge.

The House was the most appropriate place for the training of seamen. To become a pilot it was only necessary to have interest in learning the trade. However, for safety and convenience reasons, discriminatory measures began to be introduced from the twenties, especially against foreigners, in order to retain the exclusive control of Indian trade. In 1527 the king ordered Cabot to not examine foreigners and, among other measures, candidates should take a nautical chart on which their position, the courses, and most important ports and lands were shown. The measures against foreigners were not always implemented and they were altered according to the interest of the crown. The applicants from other kingdoms should be married to a Spanish woman and domiciled in Castile.

6. The world of navigation was during the Age of Exploration a very influential space and an inexhaustible source of possibilities. Any activity related to the Atlantic, even the prosperity of the empire, depended on the pilots and adjacent sciences. The navigators were no strangers to the new opportunities offered by this old job. Pilots and cosmographers of the House fed Europe with first-hand information about America. They were the artisans of a new world. Only they, theoretical and practical men, had the ability to adapt ancient navigation techniques from the Mediterranean and old cartographic models to a new geographic reality. Their pragmatism to the problems of measurement, calculation and representation of spaces made them builders of a new scientific, technological and conceptual framework. The compass, measurement and representation of the conquered territories were as important as the occupation itself, for the definition of territorial sovereignty was behind the imperial slogan 'Plus Ultra'.

The activity of these new artisans of the world was not independent of the administrative needs of an expanding empire. The creation of their positions and the development of their disciplines assembled both bureaucrats and intellectuals for the same cause: the dominion of the New World. Navigation, cartography and cosmography were subjects of power, bureaucratic and institutionalized, sciences that founded the Universal Monarchy.

Spanish imperial science related to geographical discoveries was dominated by a utilitarian conception of knowledge. Undoubtedly, adequate knowledge was useful knowledge, but not only. In some cases, this science was also concerned with revealing certain secrets of nature that directly affected its development. Some cosmographer who worked for the crown developed an unprecedented epistemological approach in the search of a causal analysis of observed phenomena. The problem of magnetic declination, for example, was one of those secrets of nature. But unlike the new natural philosophy of the seventeenth century, the study of occult forces during the previous decades was led by often discredited artisans, for which the experiment was not the end product of their work, but the starting point. It was necessary to experience the effects of magnetic declination before analyzing its causes. The experience was the essential support to be able to carry out useful science in sixteenth-century Spain, an experience that should be disciplined, educated and considered by the theory. The virtues of experience and theory built a more measurable and visible world, in short, a more vulnerable world.

Bibliographical references and archives

Archivo General de Indias (Seville)

- Consulados
- Contratación
- Indiferente
- Patronato

ACOSTA RODRÍGUEZ, Antonio, GONZÁLEZ RODRÍGUEZ, Adolfo & VILA VILAR, Enriqueta (Coords.) (2003) *La Casa de la Contratación y la navegación entre España y las Indias. XXV Congreso 500 Años de la Casa de la Contratación en Sevilla*, Sevilla, Universidad de Sevilla.

BARRERA-OSORIO, Antonio (2006) *Experiencing Nature: The Spanish American Empire and the Early Scientific Revolution*, Austin, University of Texas Press.

CERVERA PERY, José (1997) *La Casa de Contratación y el Consejo de Indias (las razones de un superministerio)*, Madrid, Ministerio de Defensa.

COLOMAR, María Antonia (2000) "La Casa de la Contratación de Sevilla y las ciencias náuticas, el comercio y los descubrimientos geográficos", In: VV. AA., *Carlos V, la náutica y la navegación*, Madrid, Lunwerg, pp. 167-192.

DANVILA Y COLLADO, Manuel (1892) *Significación que tuvieron en el gobierno de América la Casa de la Contratación de Sevilla y el Consejo Supremo de Indias*, Madrid, Establecimiento Tipográfico ‹‹Sucesores de Rivadenyra››.

ESTEBAN PIÑEIRO, Mariano & VICENTE MAROTO, María Isabel (2002) "La Casa de la Contratación y la Academia Real Matemática", In: José María LÓPEZ PIÑERO (Dir.), *Historia de la ciencia y de la técnica en la Corona de Castilla III: Siglos XVI y XVII*, Valladolid, Junta de Castilla y León, pp. 35-51.

ESTEBAN PIÑEIRO, Mariano & VICENTE MAROTO, María Isabel (1991) *Aspectos de la ciencia aplicada en la España del Siglo de Oro*, Salamanca, Junta de Castilla y León.

ESTEBAN PIÑEIRO, Mariano (1991) "Cosmografía y matemáticas en la España de 1530 a 1630", *Hispania* 51, 177, pp. 329-337.

ESTEBAN PIÑEIRO, Mariano (1993) "Los oficios matemáticos en la España del siglo XVI", In: NAVARRO, Víctor, SALAVERT, Vicent L., CORELL, Mavi, MORENO, Esther & ROSSELLÓ, Victòria (Coords.), *II Trobades d'Història de la Ciencia i de la Técnica*, Barcelona, SCHCYT, pp. 237-251.

ESTEBAN PIÑEIRO, Mariano (1995) "Matemáticas, astrología y navegación en la Castilla del siglo XVI", In: GARCÍA SIMÓN, A. (ed.), *Historia de una cultura: la singularidad de Castilla*, vol. 2, Valladolid, Junta de Castilla y León, pp. 691-739.

ESTEBAN PIÑEIRO, Mariano (1995) "Los cosmógrafos al servicio de Felipe II. Formación científica y actividad técnica", *Mare Liberum* 10 , pp. 525-539.

GIBERT Y SÁNCHEZ DE LA VEGA, María Dolores (1935) *La Casa de la Contratación de Sevilla: una institución de los Reyes Católicos*, Madrid, Gráfica Universal.

GIL-BERMEJO GARCÍA, Juana (1973) "La Casa de Contratación de Sevilla: algunos aspectos de su historia", *Anuario de Estudios Americanos*, 30, pp. 679-761.

LADERO QUESADA, Miguel Ángel (2002) *El primer oro de América: los comienzos de la Casa de la Contratación de las Yndias (1503-1511)*, Madrid, Real Academia de la Historia.

LAMB, Ursula (1969) "Science by Litigation: A Cosmographic Feud", *Terrae Incognitae*, 1, pp. 40-57.

LATORRE, Germán (1915) *La enseñanza de la geografía en la Casa de Contratación*, Madrid.

LYRA, Francisco de (1647) *Ordenanzas Reales para la Casa de la Contratación de Sevilla y para otras cosas de Indias, y de la navegación y contratación de ellas*, Sevilla.

MARTÍN-MERÁS, Maria Luisa, "Las enseñanzas náuticas en la Casa de la Contratación de Sevilla", In: ACOSTA, A., GONZÁLEZ, A. & VILA, E. (Coords.), *La Casa de la Contratación*, pp. 667-693.

NAVARRO GARCÍA, Luis (1967) "Pilotos, maestres y señores de naos en la Carrera de las Indias", *Archivo Hispalense* 46-47, 141-146, pp. 241-295.

NAVARRO GARCÍA, Luis (1969) "La gente de mar en Sevilla en el siglo XVI", *Revista de Historia de América*, 67-68, pp. 1-64.

PÉREZ-MALLAÍNA, Pablo E. (1998) *Spain's Men of the Sea: Daily Life on the Indies Fleets in the Sixteenth Century*, Baltimore, The Johns Hopkins University Press.

PIERNAS HURTADO, José Manuel (1907) *La Casa de la Contratación de las Indias*, Madrid, Librería de Don Victoriano Suárez.

PORTUONDO, María, M. (2009) *Secret Science: Spanish Cosmography and the New World*, Chicago, The University of Chicago Press.

PULIDO RUBIO, José (1923) *El Piloto Mayor de la Casa de la Contratación de Sevilla: Pilotos Mayores del siglo XVI (datos biográficos)*, Sevilla, Centro Oficial de Estudios Americanistas de Sevilla.

SÁNCHEZ MARTÍNEZ, Antonio (2010) "La voz de los artesanos en el Renacimiento científico: cosmógrafos y cartógrafos en el preludio de la 'nueva filosofía natural", *Arbor* 186, 743, pp. 449-460.

SÁNCHEZ MARTÍNEZ, Antonio (2010) "La institucionalización de la cosmografía americana: la Casa de la Contratación de Sevilla, el Real y Supremo Consejo de Indias y la Academia de Matemáticas de Felipe II", *Revista de Indias*, LXX (250), 715-748.

SÁNCHEZ MARTÍNEZ, Antonio (2010) "Los métodos pedagógicos de la Corona para disciplinar la experiencia de los navegantes en el siglo XVI", *Anuario de Estudios Americanos* 67, 1, pp. 133-156.

SANDMAN, Alison D. & ASH, Eric H. (2004) "Trading Expertise: Sebastian Cabot between Spain and England", *Renaissance Quarterly* 57, 3, pp. 813-843.

SANDMAN, Alison D. (1999) "Educating Pilots: Licensing, Exams, Cosmography Classes, and the Universidad de Mareantes in 16th Century Spain", In: GUERREIRO, Inácio & CONTENTE DOMINGUES, Francisco (eds.), *Fernando Oliveira and his Era Humanism and the Art of Navigation in Renaissance Europe (1450-1650). Proceeding of the IX International Reunion for the History of Nautical Science and Hydrography*, Cascais, Patrimonia, pp. 99-109.

SANDMAN, Alison D. (2001) *Cosmographers vs. Pilots: Navigation, Cosmography, and the State in Early Modern Spain*, Doctoral thesis, Madison, University of Wisconsin.

SANDMAN, Alison D. (2004) "An Apologia for the Pilots' Charts: Politics, Projections and Pilots' Reports in Early Modern Spain", *Imago Mundi* 56, 1, pp. 7-22.

SCHÄFER, Ernst (2003) *El Consejo Real y Supremo de las Indias* (2 vols.), Madrid, Junta de Castilla y León y Marcial Pons, p. 352.

SERRERA, Ramón María, "La Casa de la Contratación de Sevilla (1503-1717)", en: VV. AA., *España y América*, pp. 47-64.

STEVENSON, Edward Luther (1927) "The Geographical Activities of the Casa de la Contratación", *Annals of the Association of American Geographers* 17, 2, pp. 39-59.

VEITIA Y LINAGE, José de (1672) *Norte de la Contratación de las Indias Occidentales*, Sevilla.

-VV. AA. (2003) *España y América. Un océano de negocios. Quinto centenario de la Casa de la Contratación, 1503-2003*, Sociedad Estatal de Conmemoraciones Culturales, Ediciones El Viso.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 952.

FROM DENMARK TO ITALY: FRANCESCO PATRIZI'S RECEPTION AND CRITICISM OF TYCHO BRAHE'S GEO-HELIOCENTRIC PLANETARY SYSTEM

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Abstract

In 1588 Tycho Brahe published a book on the comet of 1577 De mundi aetherei recentioribus phaenomenis*, which contained the first published account of his new geo-heliocentric planetary system, with all the planets revolving around the Sun and the Sun in turn revolving around the stationary Earth. In the Tychonic system the orbits of Mars and the Sun intersected, which was made possible by replacing the traditional solid, real spheres with the fluid heavens. Three years later in Ferrara (1591) the first note on the Tychonic system appeared in a published book. Francesco Patrizi briefly described it in his* Nova de universis philosophia *at the end of his overview of the history of astronomical systems. In my paper I will investigate Patrizi's criticism of astronomy (according to him* all *astronomers, including Tycho Brahe, believe that the stars are fixed in the heavens* sicut nodi in tabula*), and how it happened that Patrizi completely overlooked one of the crucial achievements of Tycho Brahe's book (fluidity of heavens).*

In 1588 Tycho Brahe published a book on the comet of 1577 entitled *De mundi aetherei recentioribus phaenomenis*, which contained the first published account of his new geo-heliocentric planetary system, with all the planets revolving around the Sun and the Sun (as well as the Moon) in turn revolving around the stationary Earth. In his system the paths of Mars and the Sun intersected, which was made possible by replacing the traditional solid, real spheres or orbs with fluid heavens, in which the orbs are nothing more than geometrical boundaries.[1]

[1] On the elimination of solid celestial orbs or spheres from astronomy, see M. A. Granada, *Sfere solide e cielo fluido: Momenti del dibattito cosmologico nella secunda metà del Cinquecento* (Milano: Guerini e associati, 2002), and *El debate cosmológico en 1588: Bruno, Brahe, Rothmann, Ursus, Röslin* (Naples: Bibliopolis, 1996); M. P. Lerner, *Le monde des sphères*, vol. II (Paris: Les Belles Lettres, 2008²).

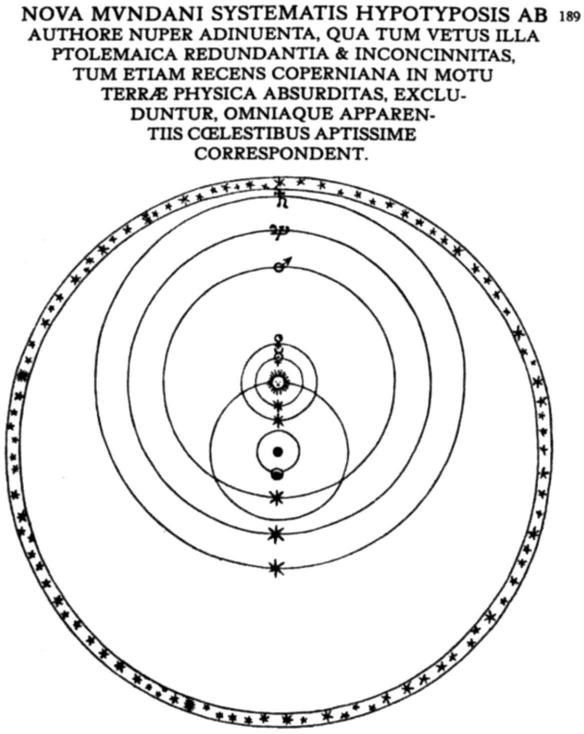

Fig. 1: Brahe's geo-heliocentric system as depicted in his *De mundi aetherei recentioribus phaenomenis* (Uraniborg, 1588), p. 158.

A few copies of the printed book were sent to some of the leading astronomers throughout Europe, some were offered for sale at the Frankfurt book fair, but most copies were stored unbound.

Three years later, in 1591, the first note on the Tychonic system appeared in a published book. Francesco Patrizi, an idiosyncratic Neoplatonic philosopher, briefly described it in the fourth part of his *Nova de universis philosophia*, published in Ferrara. Here's what he says:

> *If I am not mistaken, there is still alive a Dane by the name Tycho Brahe* (Tychon Brae)*, who on the basis of his own observations of the universe has established a hypotyposin different from Copernicus's. He places the eighth and three superior* [orbs] *in the same order [as Copernicus], but eccentric. But he says that the orb of Mars intersects the orb of the Sun in two places. Above the Sun he places Mercury and next Venus in two small circles, one bigger and the other smaller, which both revolve around the body of the Sun and the orb of the Sun, and both intersect the orb of the Sun in two places. In the centre of the universe he places the Earth, around which is the orb of the Moon, which is enclosed by the orb of the Sun, but which the orbs of Mercury and Venus do not enclose.*[2]

Before I give a fuller account of Patrizi's views on astronomy in general and on Tycho Brahe's in particular, a few words about his scholarly career and work are in order.[3]

Patrizi was born on the island of Cherso off the Dalmatian coast in 1529. In 1547, four years after the publication of Copernicus's *De revolutionibus orbium coelestium* and around 50 years after Copernicus studied there, he enrolled as a student of medicine at the University of Padua. He soon discovered the subject was not for him and switched to studies in philosophy and mathematics. Despite the predominantly Aristotelian, even Averroist orientation of the Padua *Studio*, he soon discovered Platonic and Neoplatonic philosophy, and became interested in the mystical, Chaldaic, Arab, and Hebrew

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[2] Nova de universis philosophia, IV, 12 (91r). I will subsequently refer to *Nova de universis philosophia* as NUP.

[3] For a fuller account of Patrizi's life, works, and career, see for example the online entry by Fred Purnell in the *Stanford Encyclopedia of Philosophy* (http://plato.stanford.edu/entries/patrizi/). There is a nice outline of Patrizi's cosmology by M. A. Granada, "New Visions of the Cosmos", in J. Hankins, ed., *The Cambridge Companion to Renaissance Philosophy* (Cambridge, Mass.: Cambridge University Press, 2007), pp. 275–278.

traditions and in Hermetic writings. He studied the works of Plato and Aristotle and of the most interesting Renaissance Neoplatonic thinkers, such as Marsilio Ficino, particularly his doctrine of the renewal of *prisca theologia*, a philosophical project of renewal of Christianity.

The first of his two major philosophical works appeared in its entirety in 1581 in Basel.[4] *Discussionum peripateticarum libri decem*, as the book was called, was a fierce attack on Aristotle and peripatetic philosophy. It can be characterized as the negative part of his project. Ten years later, in 1591 he provided his positive part, his most significant philosophical work being *Nova de universis philosophia*.[5] The book is divided into four parts: *Panaugia*, *Panarchia*, *Pampsychia*, and *Pancosmia*. The fourth book, *Pancosmia*, is subtitled "Principles and Constitution of the Corporeal World," and is therefore extremely interesting from the perspective of the history of science.

Patrizi's criticism of astronomy: astronomers believe that the stars are fixed in the heavens *sicut nodi in tabula*

Patrizi mentions Tycho Brahe's geo-heliocentric system in Book 12, "On the Number of Heavens", of *Pancosmia*, wherein he presents some kind of history of astronomy from Thales to Tycho Brahe, written from the perspective of the number of celestial spheres astronomers used to explain celestial phenomena. Patrizi's overview begins with Thales and the Pythagoreans, continues with Ptolemy and some Arabic astronomers and philosophers (Thabit ibn Qurrah, Averroes, and al-Battani), and finally comes to the "modern era".

"In our time three new astronomical theories have arisen, very different from those of the ancients and very different from one another," he says. First, there is Copernicus, who revived the teachings of Aristarchus of Samos. He "overturned the whole astronomy of the ancients and the whole order of the universe". The second theory is that of two men from Verona, Giovanni Battista della Torre and Girolamo Fracastoro, who brought back in memory and use the old homocentric spheres, increasing their number to 77.[6] And finally there is the passage, quoted above, which describes Brahe's system.

After this short overview of the history of astronomy, Patrizi continues with the following argument, which is very representative of his overall mode of reasoning in *Nova de universis philosophia* –and in his other works.

It is necessary that everything that was mentioned is either true or false or that some part is true and some part is false.

It is impossible that everything is true since all those astronomical theories and systems are completely mutually incompatible. It is therefore more probable that all of them are false. Why? Patrizi replies: because they are all monstrous. Why? The astronomers claim that heaven is a simple body. How can it happen that there is so much diversity in this simple body?

But if some theories are true and some theories are false, one has to determine which theory or theories are true. Now, if one looks at the tables and ephemerides of ancient astronomers, he reasons, one realizes that they do not adequately respond to recent observations; they are out of date. This means that ancient astronomy is false. On the other hand, modern astronomers make their calculations on the basis of Copernicus's hypotheses and claim that they have furnished true tables. But according to the "new observer of the stars", Tycho Brahe, even those are false and defective and he himself has set out to improve them.[7]

Patrizi is obviously perplexed faced with all these different, mutually exclusive astronomical theories and the ever increasing number of celestial spheres that astronomers used and still use in order to "save appearances". His conclusion, which he does not explicitly write down but which is implicit in his reasoning, is absolutely clear: *all*, I repeat, *all* astronomical systems are in error, they are all *deliria* (or *deliramenta*) and *monstra*, and the most monstrous of them all are those of Copernicus and Tycho.

Why is that so? What is the ultimate reason that lies behind astronomy's "innumerable delusions", "monstrosities", and "chimeras", and which in the end produced "a whole new chaos"?

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[4] Volume I of *Discussiones* was first published in Venice in 1571.

[5] See M. Muccillo, "La dissoluzione del paradigma aristotelico", in C. Vasoli, ed., *Le filosofie del Rinascimento* (Milan: Mondadori, 2002), pp. 506–533.

[6] See NUP, IV, 12 (91r). It seems to me that Patrizi had read Fracastoro's dedication to Paul III from his *Homocentrica* (Venice, 1538), where he mentions Ioannes Baptista Turrius as somebody who revived homocentric astronomy. Patrizi does not mention Givanni Battista Amico, who made the same attempt two years earlier in *De motibus corporum coelestium iuxta principia peripatetica sine eccentricis et epicyclis* (Venice, 1536).

[7] See NUP, IV, 12 (91v).

First: the general tendency to "save appearances". Second: the specific assumption, which was, and according to Patrizi still is, accepted by *all* astronomers, including Tycho Brahe, "that the stars are fixed in the heavens like knots or nails in a plank", that is to say, that the stars are fixed in solid, real heavenly orbs.

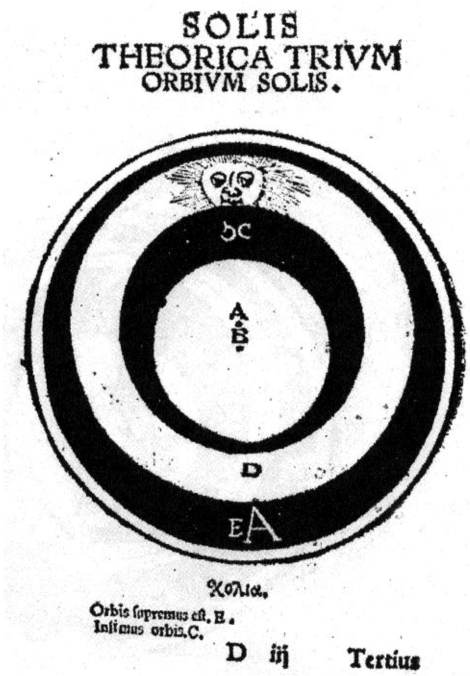

Fig. 2: Model of real, solid orbs for the Sun (*theorica solis*) from Peurbach's Theoricae novae planetarum, first published in 1472. Between 1472 and 1653, more than fifty editions of Theoricae novae appeared, prompting a massive number of commentaries to be produced.

This assumption, and this assumption alone, forced astronomers –and it is important to note that Patrizi claims that this assumption was the invention of astronomers–[8] to adopt various flawed theories:

> *All those* [astronomical theories and systems]*, these two* [Copernicus's and Brahe's]*, and the old ones derive from that one position: that the stars are fixed in the heavens like knots or nails in a plank. Because of this fixation they first invented eight spheres and since those were not sufficient to save appearances they added innumerable deliraments and monstrosities, which in the end produced a complete perturbation of the whole world, like a whole new chaos.*[9]

In sum: Patrizi is convinced that *all* astronomy suffers from an erroneous belief in the fixation of the stars in the heavens, that this illness brought about a chaos and monstrosity in the universe, and that the most monstrous universes were those conceived by Copernicus and Tycho Brahe, who were both firm believers in the doctrine of the fixation of the stars in the heavens.

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[8] See NUP, IV, 12 (90v).

[9] NUP, IV, 12 (91v). Patrizi's claim that *all* astronomers believed that stars are attached in solid orbs is in my view additional proof that before the late 1570s and especially 1580s, this was a widespread, common astronomical assumption. See also P. Barker, "The Reality of Peurbach's Orbs", in P. J. Boner, ed., *Change and Continuity in Early Modern Cosmology* (New York: Springer, 2011), pp. 7–32, who, after examining the published commentaries on Peurbach's *Theoricae novae*, and commentators on Sacrobosco's *Sphaera*, came to the conclusion that, with the exception of Prieras, all these commentators follow Peurbach in treating the celestial orbs as real and that there may be historical reasons for Prierias' reservations.

How did Patrizi completely overlook one of the crucial achievements of Tycho Brahe's book?

Since there is a very clear statement about the elimination of solid spheres and their replacement by fluid heavens in Chapter 8 of Brahe's book,[10] one wonders how it happened that Patrizi overlooked it. Did he read Brahe's *De mundi aetherei* at all?

In their excellent edition of Kepler's *Apologia pro Tychone contra Ursum* N. Jardine and A. Segonds suggest that Patrizi's note on Brahe's system "depended on a lecture or at least on a direct consultation of Chapter 8 of *De mundi aetherei*". They underline his use of the term *hypotiposis* in describing Brahe's system, which is the same word as appears on the page with the figure of the system,[11] and they conjecture that Patrizi's note is nothing other than a "pure and simple description of Brahe's figure" and that he simply did not take into account the details which Brahe gave on the next page about the intersections of the spheres.[12]

In my opinion, it would be extremely strange for Patrizi to have access to Brahe's book, to take into account the strange figure with intersecting orbs, but to dismiss Brahe's explanations of this strange feature of his system on the next page. I closely considered this puzzling situation and from what I have been able to discover so far, I can say that N. Jardine and A. Segonds are partly wrong and partly right. Patrizi did not have direct access to the book and its content, but nevertheless described Brahe's system on the basis of the figure therein. How was that possible?

When Brahe invented a new system of the world he was eager to win general approval and sent the book to his usual correspondents. The task of distributing the book was partly bestowed upon one of his collaborators at Uraniborg, Gellius Sascerides.[13] In 1588 Sascerides departed for Padua via Germany and Switzerland. In 1590, while attending Frankfurt's annual book fair, he received a copy of *De mundi aetherei* with instructions from Brahe to send it forward to the famous Italian astronomer Giovanni Antonio Magini, teacher of mathematics at the University of Bologna. Sascerides went back to Padua and duly sent the book to Magini.[14]

After the publication of Patrizi's *Nova de universis philosophia* in 1591 Sascerides somehow found out about Patrizi's account of Brahe's system. We do not know how that happened, if he bought the book, read it in some library, or just heard about it.

What we do know is that on 12 March 1592 Gellius wrote to Patrizi. I have so far been unable to determine whether his letter to Patrizi is extant –it seems that it is not– and what its exact content was. I also do not know whether he wrote to him on his own initiative or was asked to do so by Tycho Brahe. It seems to me more probable that Brahe was behind it. But from Patrizi's reply to Sascerides, written in Ferrara on 25 March 1592, which is extant[15] –a reply that Alain Segonds and Nicholas Jardine overlooked–, one learns that Sascerides (or more probably Tycho Brahe himself) was very upset because of Patrizi's misrepresentation of Brahe's astronomy. From that letter it is also clear that Gellius wrote to Patrizi:

> *that in Book 12 of Pancosmia he said something about his* [i. e. Brahe's] *astronomy, which cannot be read at all in his books, and that there are written down exactly opposite opinions about the heavenly orbs and the hardness of the heavens.*[16]

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[10] See *De mundi aetherei recentioribus phaenomenis*, p. 159: "Per cometarum motus prius ostensum et liquidum comprobatum fuerit, ipsam Coeli machinam non esse durum et imperuium corpus variis orbibus realibus confertum, vt hactenus a plerisque creditum est, sed liquidissimum et simplicissimum, circuitibussque Planetarum liberis, et absque ullarum sphaerarum opera aut circumvectione, iuxta diuinitus inditam Scientiam administrates, vbique patere, nihilque prorsus obstaculi suggerere."

[11] See the text above the figure of Brahe's system from *De mundi aetherei*.

[12] See *La guerre des astronomes: la querelle au sujet de l'origine du système géo-héliocentrique à la fin du XVIe siècle* (Paris: Les Belles Lettres, 2008), vol. II/2, p. 433.

[13] On Gellius Sascerides, see J. R. Christianson, *On Tycho's Island: Tycho Brahe: Science, and Culture in the Sixteenth Century* (Cambridge: Cambridge University Press, 2003), pp. 351–352. On his voyage to Italy, see W. Nordlind, "Tycho Brahe et ses rapports avec l'Italie," *Scientia* 69 (1955), pp. 47–61, and I. Pantin, "New Philosophy and Old Prejudices: Aspects of the Reception of Copernicanism in a Divided Europe", *Studies in History and Philosophy of Science* 30 (2/1999), pp. 237–262.

[14] See Sascerides's letter to Magini, 15 January 1590, in A. Favaro, *Carteggio inedito di Ticone Brahe, Giovanni Keplero e di altri celebri astronomi e matematici con Giovanni Antonio Magini tratto dall'Archivio Malvezzi de'medici in Bologna* (Bologna: Zanichelli, 1886), p. 13. Brahe thanked Gellius Sascerides in October 1590. See T. Brahe, *Opera omnia*, ed. by J. L. E. Dreyer and E. Nystrøm (Copenhagen: Gyldendal, 1913–1918), VII, p. 278.

[15] Patrizi's letter to Sascerides was published in a critical edition of his letters and unpublished works, prepared by D. A. Barbagli, in Francesco Patrizi da Cherso, *Lettere ed opusculi inediti* (Florence: Instituto nazionale di studi sul rinascimento, 1975), pp. 79–82. See also P. Rossi, "Sfere celesti e branchi di gru", in P. Rossi, *Il tempo dei maghi: Rinascimento e modernità* (Milano: Raffaello Cortina, 2006), pp. 185–225.

[16] Patrizi's letter to Sascerides, 25 March 1592, in Francesco Patrizi da Cherso, *Lettere ed opusculi inediti*, p. 80.

Secondly, Sascerides excerpted these thoughts from Brahe's writings and sent them to Patrizi.

In the rest of his letter to Sascerides, Patrizi explains how he learned about Brahe's astronomy. When he heard about the novelties of Tycho's astronomy he tried to get hold of them. He tried to purchase the book but was not successful. Then he heard from Giovanni Francesco Pinelli that Tycho had sent one copy to Magini.[17] Patrizi wrote to Magini, asking him for a copy or at least for a description of the new system. Magini sent Patrizi some of Sascerides's letters to Magini and the "*hypotiposin* of the universe outlined in orbs". From these he deduced what is written in Book 12 of *Pancosmia* about Tycho's new astronomy. In the letter Patrizi then promises Sascerides that he will compare his description of Brahe's system with the papers he has now received. If he realizes that he was mistaken, he will recant and restore Brahe's praise. But at the moment he can do nothing. He has sent a part of his books and papers to Rome, where he has been summoned by the Pope. And he wants Tycho to be informed of all this.

It is obvious from this letter that Patrizi had not read nor seen Brahe's book, and that he had drawn on Sascerides' letters to Magini that had been forwarded to him. These letters should, according to what Sascerides wrote to Patrizi, contain Sascerides' description of Brahe's new astronomy from Sascerides' letters to Magini and Magini's copy of the figure of the new *hypotiposin*.

Sascerides' letters to Magini (together with Brahe's letters to Magini and Magini's to Brahe) are extant and were published by A. Favaro, in *Carteggio inedito di Ticone Brahe, Giovanni Keplero e di altri celebri astronomi e matematici con Giovanni Antonio Magini,*[18] but we do not know exactly which letters Magini sent to Patrizi. What I have been able to learn from this correspondence thus far is the following. There is relatively little talk of Brahe's system *per se*. In one of Gellius' letters to Magini the system is called *dyatiposin* and not *hypotiposin*. Brahe informs Magini that he has been plagiarized by Ursus. Magini praises his system, but says he would prefer if the orbs of the Sun and Mars did not intersect.

This laudatory comment was enough for Tycho. In November 1591 he wrote to John Craig about his relationship with Magini in order to prove that he had followers even outside Germany:

> *I will add some extracts from the letters of this most illustrious and excellent mathematician, Giovanni Magini, so that you might see that the excellent minds in Italy, as* [in Germany]*, have agreed with our theses and entirely approved them.*[19]

As we have seen, exactly the opposite was the case with the other excellent mind in Italy, philosopher Francesco Patrizi, who had a very low opinion of Brahe's system and of all of astronomy, for that matter. But Patrizi soon had some other problems to think of. His *Nova de universis philosophia* upset not only astronomers, but stirred quite a controversy with critics from theological and philosophical quarters as well. Despite the fact that he started lecturing in Rome in May, 1592, his *Nova de universis philosophia* came under investigation and was finally put on the Index of Prohibited Books "until corrected" in 1596. It comes as no surprise that Patrizi, despite his promise, never revised his views on Brahe's astronomy. He had bigger concerns. He spent the last years of his life teaching Platonism, passing away in 1597 while still engaged in defending his controversial work.

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[17] I am a little bit puzzled by M. Bucciantini's claim, in *Galileo e Keplero: Filosofia, cosmologia e teologia nell'Età della Controriforma* (Torino: Einaudi, 2007), p. 38, and n. 69, that Pinelli himself received a copy of *De mundi aetherei* from Brahe. See also Brahe's letter to Johannes Camerarius, 21 October 1590 (TBOO, VII, p. 276). The book is listed in Pinelli's library as "Thiconis Braeae phenomenae, Uranibirgi 1588". While it is very probable that Patrizi asked Pinneli about *De mundi aetherei* before fall/winter 1590, when it should have arrived at Pinelli's library, and who at the time of his inquiry was therefore redirected to Magini, who received it sometime in the first months of 1590, it is not clear – at least at first sight – why Brahe, who obviously had sent two copies to Italy, in a much later letter to Kepler, 19 December 1599 (TBOO, VIII, pp. 206–207), referred only to the copy sent to Magini: "MAGINUM Italum nunquam existemarim tale quid affectasse. Quod si fecit literis propria manu ad me datis, quibus se illam Hypothesin ex libro nostro secundo de Aethereis Phaenomenis primum perspexisse, sibique apprimere placere, modo Martis orbis Solarem transire posset (putavit enim ex vulgata sententia reales esse in coelo orbes) fassus est, contraria admisit. A MAGINO vero, si qui alij huius speculationis conscij sunt, hac occasione habuerunt, inter quos etiam est Francisus Patricius, qui eius mentionem in erudito alias de nova Philosophia Volumine facit, is quidem meam esse expresse asserit, sed eam ita pervertit, et per ignorantiam male accomodat, et contra mea ipsius postulata et assumpta eam coinquinat et insectatur, ut plurima isthic ex ipsius relatione apposita pro meis non agnoscam, quod etiam publice protestabor." N. Jardine and A. Segonds, *La guerre des astronomes*, vol. II/2, p. 433, interpret this passage in the sense that Brahe at that time knew only of one copy of his book in Italy, the one he had sent to Magini. Since Brahe sent one copy also to Pinelli, it seems to me that it is more likely that Brahe's stress here is not on the one and only existing copy of his book in Italy, but on his conviction that Magini, who still believed in the reality of celestial orbs, was the ultimate source of Patrizi's misinterpretation of Brahe's astronomy.

[18] Cf. n. 15 above.

[19] TBOO, VII, p. 315.

This is not, of course, the end of the story. As it is well known, Brahe decided –almost a decade after the publication of Patrizi's *Nova de universis philosophia*– to strike back or, to be more precise, he delegated the job of replying to Patrizi to Johannes Kepler, who did so in his manuscript *Apologia Tychonis contra Ursum*. I will analyse this part of the story, which from the perspective of historical epistemology is perhaps even more intriguing than the part analysed so far, in a forthcoming article. My contention is that in order to achieve a historically and epistemologically adequate understanding and assessment of the situation, we should take a look at it from two perspectives, astronomical or, even better said, astronomical-philosophical (represented by Kepler) and philosophical (represented by Patrizi).

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THE BATTLE OF THE ASTRONOMERS. JOHANN ADAM SCHALL VON BELL AND FERDINAND VERBIEST AT THE COURT OF THE CELESTIAL EMPERORS

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Abstract

My talk will focus on the two most outstanding figures among the Jesuit missionaries in 17th-century China: Johann Adam Schall von Bell (1591-1666) and Ferdinand Verbiest (1623-1688). In 1619 von Bell reached Macao, bringing some telescopes along and translating European mathematical, astronomical, and optical works from Latin to Chinese.
Von Bell aimed at introducing the telescope into Chinese astronomy, traditionally based on naked-eye observation and calculation. In 1634 he started a correspondence with the last Ming Emperor, Chongzhen (1611-1644), describing the many useful applications of the telescope.
With the advent of the Qing dynasty, von Bell became counselor of the new Emperor, Shunzhi (1638-1661), as well as Head of the Mathematical Board and Director of the Imperial Observatory. Verbiest was called in 1660 to assist von Bell in his project of reforming the Chinese traditional calendar.
The political situation changed dramatically in 1661, with the Empire ruled by four regents who were hostile to the Jesuits, as much of the Chinese mathematicians at the Observatory. What followed was not just a conspiracy against the Jesuits, but a sort of "Kulturkampf" raised by the most conservative side of the Manchu regime, which regarded the Europeans and their increasing authority as a threat for the Empire.
I want to discuss the famous contest between the Chinese astronomer Yang Guangxian (1597-1669) and the Jesuit mathematicians, which lasted from 1664 to 1669. Behind the scenes of a public competition to compare the merits of European and Chinese astronomy, a dangerous game was played at court, which involved science, technology, philosophy, power, credit, patronage, personal rivalry, and luck.
The contest was finally won by Verbiest, who succeeded to von Bell as Head of the Mathematical Board and became a close friend of the new Emperor, Kangxi (1654-1722), teaching him geometry, philosophy, and music. He translated Euclid into Chinese and in 1673 he rebuilt the Observatory according to the European standards.

The very first generation of Jesuit missionaries who went to China after Matteo Ricci's (1552-1610) death was made by scientists belonging to Christoph Clavius' (1538-1612) entourage at the Collegium Romanum. Ricci himself was one of Clavius' pupils, who were regarded at that time as the most brilliant mathematicians and astronomers of the Company. One must mention people like father Johann Terentius Schreck (1576-1630), who had attended Galileo's lectures in Padua between 1603 and 1604 and was elected *academicus lyncaeus* together with his former master in 1611, before joining the Company. Schreck and his confrere Johann Adam Schall von Bell (1591-1666) were both in Rome when Galileo, on May 18, 1611, was invited by Clavius to discuss his telescopic observations.

Von Bell was the main prosecutor of Ricci's missionary action in China. Born as an individual adaptation strategy, the "enculturation" practice became the Jesuit manifesto in the Far East. Unlike the Franciscans, who had formed some small, isolated communities in the poorest and most peripheral regions of the Empire, the Jesuits soon realized that speaking the language of the exact sciences was the only way to overcome the local prejudices against the Europeans and their culture. If they wanted to be accepted by the highest ranks of the Chinese society, they had to be recognized as part of the intellectual and political elite of the Empire. Already in his later years, when he finally managed to enter Beijing and to establish the first direct contacts with the Imperial Court, Ricci made official requests to his superiors about the necessity of supporting his mission with the best intellectual resources available at the mathematical school of the Collegium Romanum.

Between April and July, 1619, von Bell, Schreck, and Nicolas Trigault (1577-1628) reached Macau with over 20 newly recruited Jesuit missionaries, bringing some Galilean telescopes along, as well as many other scientific instruments. Probably the "Dutch glasses" had already been introduced in China by Italian, Portuguese, and Dutch merchants since 1608-1610, but the use of the telescope as an instrument is clearly attested only after von Bell's arrival. While in the West the Copernican debate was turning into a broader cultural, religious, and even political struggle, von Bell and Schreck were translating European mathematical, astronomical, and optical works previously approved by the Company from Latin to Chinese, as well as several textbooks on mathematics, engineering, medicine, and astronomy from Chinese to Latin. In 1626 von Bell wrote a treatise both in Latin and Chinese on dioptrics and the principles of telescope-making. His Biography of *Western Astronomers* (1640) was the first Chinese text written by a Jesuit that explicitly mentioned Galileo and his scientific achievements, providing a Mandarin transliteration of his name.

Since 1629 von Bell and his closest collaborators, like the Belgian father Jean-Nicolas Smogolenski (1610-1656), became court astronomers. They aimed at introducing both the Galilean and the Keplerian telescope into the practice of Chinese astronomy, traditionally based on naked-eye observations (helped by dioptres, alidades, and grids) and on measuring or calculation devices which were quite similar to the Western ones, like sundials, quadrants, sextants, armillary spheres, and celestial globes. Smogolenski is also known for having introduced the Western methods of predicting solar and lunar eclipses on the basis of Copernican ephemeredes, provided that they were regarded just as hypothetical and useful mathematical tools, without any commitment about their cosmological value.

However, the Chinese astronomers made a very limited use of the telescope until the 1680s, only to observe phenomena which were otherwise impossible to detect, like those described in the *Sidereus Nuncius* (1610). Furthermore, according to them, objects like sunspots had already been observed in China some centuries before, looking at the solar disc at dawn and sunset, as well as the stellar nature of the Milky Way had already been supposed. On the contrary, the first military use of the telescope is attested in 1635, during one of the by then frequent wars between the Imperial Army and the anti-Ming coalitions, while its presence on war and cargo ships is very rare.

It is very hard to establish when telescopes started being produced directly in China, without assembling *in loco* their components once they had been imported from Europe. One must remind that there was a very long tradition of lens-making in the country, dating back to the 13th century at least. Most of the Chinese magnifying lenses were made from rock crystal, which was very abundant in the mines of the South. As in the case of their Western medieval and early modern counterparts, these lenses were usually convex, because concave lenses were much more difficult to cut and to polish with high precision.

The Chinese craftsmen who produced such lenses were not opticians in a proper sense, but rather highly specialized glass-makers, gem-cutters, and jewellers. What is sure is that starting from the second half of the 17th century, under the new Qing dynasty, some of them began to work directly for the Jesuit astronomers and for the Imperial Court, producing the first exemplars of Chinese telescopes with bamboo tubes, rock crystal lenses, and decorated paper. We know from the registers of the European trade companies of the time that they belonged to a small community of excellent artisans. Being instructed by the Jesuits in the most recent developments of optics and instrument-making, they were able to read Latin and to speak Spanish, Dutch, and French, since they were agents of the Western companies.

In 1634 von Bell started an official correspondence with the last Ming Emperor, Chongzhen (1611-1644), describing the useful applications of the telescope in astronomy, navigation, military surveying, and geodesy, as well as the importance of the scientific novelties introduced by the Jesuits for the improvement of Chinese technology and the advantages offered by many other recent instruments, like the microscope, the analytical compass, the nocturnal watch, and the first models of mechanical clocks. Von Bell's attitude towards the Emperor was not so different from Galileo's one towards the Venetian Doge or the Grand Duke of Tuscany. Both in the Celestial Empire and in early modern Europe scholars could only be courtiers and the liberal arts largely depended on a very unstable patronage system, based on personal relationships and political contingencies. Moreover, even if some Jesuits (not all the missionaries) enjoyed the same privileges of other Han dignitaries in the Forbidden City, they could not share with them the same social status and prestige, simply because they were foreigners: tolerated, appreciated, but not Chinese.

With the advent of the Qing dynasty in 1644, the political scenario seemed to be more favourable for the Jesuits, at the very beginning, despite the fact that some of them had served Chongzhen as court astronomers. The new Manchu rulers must consolidate their power over the Empire, after many decades of revolts and conspiracies against the previous Ming domination. At the same time, they had to confront the increasing colonial pressure of England, France, and the Netherlands, while the military and political influence of Spain-Portugal was rapidly declining, especially after the end of the Thirty Years War. Being more and more isolated from this point of view, the Jesuit missionaries soon became part of a long-term political design of the Manchu elite. From the one hand, the Western "enculturers" must be put under the direct control of the Court, given the useful scientific-technological innovations they might bring to the Empire. On the other hand, their missionary action must be somehow neutralized through assimilation and political involvement in the State affaires to preserve cultural identity and traditions of the Chinese society.

Von Bell became the trusted counsellor of the new Emperor, Shunzhi (1638-1661), who created him mandarin, Head of the Mathematical Board, and Director of the Beijing Ancient Observatory, being charged of its restoration after a long period of decay. His position enabled him to procure special permission for the Jesuits to build their churches and to preach throughout the Empire. The Flemish Jesuit mathematician Ferdinand Verbiest (1623-1688), arrived in Macau in 1659, was called in 1660 to assist von Bell, mainly in his fundamental project of reforming the Chinese traditional calendar to make it compatible with the Gregorian calendar and with the most recent European acquisition in astronomy.

The Chinese traditional calendar had a very long history, dating back to the second millennium BC, with many reforms occurred in the meantime. It was a lunisolar calendar, made of 12 regular months which were alternatively 29 and 30 days long, originally with an additional day added from time to time, to catch up with drifts between the calendar and the actual moon cycle. Intercalary months were arbitrarily added at the end of the year. Between the 5th and the 2nd century BC progress in astronomy allowed the creation of calculated calendars, where intercalary days and months were set according to precise rules. Apart from minor modifications, the Chinese calendar remained the same until the 17th century: the solar year was made of 365 ¼ days, as in the Julian calendar of Rome, along with a 19-year (235-month) Rule Cycle, known in the West as the Metonic cycle. The year began on the new moon preceding the winter solstice, and intercalary months were added at the end of the year.

In 1645 von Bell introduced the calculation of the true motion of the sun and the moon with sinusoids, according to the common procedures of European astronomy. The main problem was the discontinuity in the annual motion of the sun across the different signs of the Zodiac that resulted from such a non-Gregorian system. The reform of the calendar was not just a question of better or worse instruments, more or less advanced calculations, and discussions among experts with different opinions. It was a very crucial point, since the Confucian philosophy saw an intimate connection between the astronomical order, the natural order, and the socio-political order, with the Emperor as the unifying principle and the divinized mediator among all the three ontological and cosmological levels, as the highest terrestrial being and the lowest celestial one.

Astrology had a fundamental role in Chinese culture, since any change in the heavens was supposed to have immediate consequences on the earth, and vice versa. Astral configurations had direct influence on psycho-physical constitutions and moral dispositions, as well as on health and stability of the collective body, while vicious inclinations might affect the pre-established harmony both of nature and society. The main task of a court astronomer-astrologist was precisely to provide a correct interpretation of this mutual interaction among different levels of reality. Furthermore, there was a small community of Uighuri mathematicians in the Forbidden City since the age of the Mongolian domination. They had imported Arabic astronomy from Persia, having a sort of monopoly on astrological responses. It was not easy to compete with their undisputed authority.

Unfortunately for the Jesuit astronomers, the political situation changed dramatically in 1661, after Shunzhi's sudden death. His son Kangxi (1654-1722) was only 7 and the government was placed in the hands of four regents. Unlike Shunzhi, the regents were hostile to the Jesuits, as most of the Han, Manchu, and Uighuri mathematicians who worked at the Observatory and felt deprived of their previous prominent position as court astronomers, with the great influence that such a role had on the Emperor. This turned quickly into an open persecution campaign against the Jesuit missions and the minority of Christian Chinese. What followed was a complicated and novel-like affaire, much more than a court conspiracy against the Jesuits and their supporters: it was a sort of "Kulturkampf" raised by the most conservative side of the Manchu regime, which regarded the Europeans and their increasing authority as a threat for the stability and the cultural identity of the Empire.

In 1659 the Chinese astronomer Yang Guangxian (1597-1669), a former assistant guard commander during the late Ming period, wrote two anti-Jesuit pamphlets: *On Collecting Errors*, a criticism of the Western calendar, and *On Exposing Heterodoxy*, a violent attack on Christianity. In 1660 he sent to the Imperial Board of Rites *A Call to Rectify the Country*, which contained his first direct attack on Adam Schall von Bell, claiming that he wanted to westernize the Chinese calendar to introduce Christian liturgy. It was rejected. More writings followed over the next few years. These were collected in 1665 under the title *I Cannot Do Otherwise*. The most important of these articles appeared in September 1664: *A Complaint Requesting Punishment for the Evil Religion*. This time it was accepted by the Board of Rites. In it, Yang claimed that von Bell was responsible for the death of the young Empress Xian Rui in 1660 by choosing an inauspicious day for the burial of her son in 1658. Since Shunzhi had become depressed and ill because of Xian Rui's fate, von Bell was also responsible for the Emperor's death. The allegation was extremely serious.

Yang challenged von Bell to a public competition to decide which astronomy was the best one and he won, also because the majority of the court astronomers supported him against the Jesuit. Yang took von Bell's place as Head of the Mathematical Board. Having lost the competition, von Bell and the other Jesuits were chained and thrown into a filthy prison, accused of teaching a false religion. They were bound to wooden pegs in such a way that they could neither stand nor sit and remained there for almost two months until a sentence of strangulation was imposed. A high court found the sentence too light and ordered them to be cut up into bits while still alive. Fortunately for them, on April 16, 1665, an earthquake destroyed the part of the prison chosen for the execution. An extraordinary meteor was seen in the sky, and a fire destroyed the part of the Imperial Palace where the condemnation was pronounced. This was seen as an omen and all the prisoners were released. Only five Christian Chinese were executed. However, the Jesuits still had to stand trial, and all of them but von Bell, Verbiest, and two others were exiled to Canton. Von Bell died within a year, due to the very hard conditions of his confinement. The whole "enculturation" strategy was at stake at that moment, together with the survival of the Jesuit missions in China.

In 1669, the Kangxi Emperor managed to take power by having the remaining and corrupt regent arrested. In the same year, the Emperor was informed that serious errors had been found in the calendar for 1670, which had been drawn up by Yang Guangxian. Kangxi commanded a public test to compare the merits of European and Chinese astronomy. The test was to predict three things: the length of the shadow thrown by a gnomon of a given height at noon of a certain day; the absolute and relative positions of the sun and the planets on a given date; the exact time of an anticipated lunar eclipse. Yang and Verbiest should each use their mathematical skills to determine the answers: "the heavens would be the judge".

Behind the scenes of a public competition among courtesan scholars to compare the merits of European and Chinese astronomy, a dangerous game was played within the Forbidden City, which involved knowledge, instruments, philosophical issues, patronage, personal rivalry, and luck. The contest was held at the Bureau of Astronomy in the presence of senior-ranking government ministers and officials from the Observatory. Unlike Yang, Verbiest was assisted by telescopes for observations and had access to the latest updates on Kepler's *Tabulae Rudolphinae* (1627). He succeeded in all three tests, and was immediately installed as Head of the Mathematical Board and Director of the Observatory. Yang was sentenced to the same death he had planned for his Jesuit rivals, but he was pardoned by Kangxi and exiled to his native village, due to his old age. In 1671 the exiled Jesuits were rehabilitated and authorized to return to their missions.

The 1670 calendar unnecessarily included an extra intercalary month, added to hide other errors and to bring the lunar months in line with the solar year. Verbiest suggested that the errors should be corrected removing the extra month. This was an audacious move, as the calendar had been approved by the Emperor himself. Fearing Kangxi's response, the Observatory officials begged Verbiest to withdraw his request, but he replied: «It is not within my power to make the heavens agree with your calendar. The extra month must be taken out». Much to their surprise, the Emperor finally agreed, and it was done. After this, Verbiest became a close friend of Kangxi, teaching him geometry, philosophy, and music. He was frequently invited to the Imperial Palace and to follow the Emperor in his expeditions through the country. In 1673 Verbiest was elevated to the highest grade of the mandarinate, an extraordinary privilege that no other European would enjoy after him.

Having solved the issues surrounding the calendar, Verbiest went on to compose a table of all solar and lunar eclipses for the next 2,000 years. Delighted with this, the Emperor awarded him complete charge of the Imperial Observatory, which he rebuilt in 1673 according to modern standards. The current Observatory had been completed in 1442 under the Ming dynasty. The existing equipment was obsolete, dating back to the end of the 15th century, so Verbiest consigned it to a museum close to the main building and designed six new bronze instruments, which can still be seen nowadays. They are large and highly decorated, with dragons forming the supports. Despite their weight, they are very easy to manipulate, demonstrating Verbiest's aptitude for mechanical design:

- **Altazimuth**, to measure the position of celestial bodies relative to the horizon and the zenith – the altitude azimuth.
- **Quadrant**, six feet in radius, to measure altitudes or zenith distances of celestial bodies.
- **Equatorial armilla**, six feet in diameter, to measure true solar time as well as right ascension difference and declination of celestial bodies.
- **Ecliptic armilla**, six feet in diameter, to measure the ecliptic longitude difference and latitudes of celestial bodies. This was the traditional European device, while the Chinese used only equatorial armillas.
- **Sextant**, eight feet in radius, to measure the angle of elevation of a celestial object above the horizon and to calculate the angle between two objects.
- **Celestial globe**, six feet in diameter, to map and identify celestial objects.

Verbiest undertook also many other projects, including the translation of the first six books of Euclid's *Elements* into Manchu (a full Chinese translation of Euclid had been provided in 1607 by Matteo Ricci). He created star charts for the

Emperor in order to tell the time at night. In 1674 he published his Kunyu Quantu world map, an improvement of the *Kunyu Wanguo Quantu* map drawn by Ricci between 1602 and 1604. In 1687 he completed his *Astronomia Europea*, a Latin encyclopaedic treatise on the major achievements of Western astronomy, which was of course cosmologically committed with the Tychonian system. After his death in Beijing he was succeeded as chief mathematician and court astronomer by a Belgian Jesuit, Antoine Thomas (1644-1709). His remains were buried near those of Matteo Ricci and Adam Schall von Bell on March 11, 1688. Today their grave is in the campus of the University College of Political Science. Verbiest was the only Westerner in Chinese history to ever receive the supreme honour of a posthumous name by the Emperor.

His personal success in the "calendar affaire", as well as his unsurpassed intellectual and social prestige at the court of Kangxi, was for sure a breakthrough in the history of the Jesuit missions in China, if not its prime. However, it did not imply a success of the Jesuit programme of evangelization through "enculturation". On the contrary, the temporary defeat of the traditionalists was rather a victory of the Emperor, a landmark in his politics of cooptation of the most prominent members of the Company by placing them at key positions in the establishment, also to prevent any external aggression from the Europeans in reaction to a systematic persecution against Christians.

The real political contest was inside the new dominant class of the Empire. The Manchu leaders were traditionally strong supporters of Buddhist monasteries, since their respected authority was compatible with their respectful loyalty to the Emperor. Kangxi's assimilating attitude towards the Jesuits aimed at reducing them to one of the many ethnic-religious minorities protected by the imperial government because of their unquestioned obedience to it. This would become much clearer under the reign of his successors Yongzheng (1678-1735) and Qianlong (1711-1799): the dispute about the syncretic rites and the heterodox doctrines elaborated by the Jesuits would bring their missions to an end in the 1760s, putting the remaining former missionaries and the Christian Chinese under the total control of the Emperor.

Moreover, one might say that the dignitaries of the Celestial Empire had a very similar, pragmatic approach to the scientific and technological novelties introduced by the Jesuits, despite their undutiful importance. They regarded the "new astronomy", as all the other innovations imported by the Europeans, as useful upgrades and developments of their own traditional knowledge, without any significant change in their cultural background and in their "Weltanschauung", based on millennia of sophisticated civilization. The physic-mathematical sciences and their applications were the only field where a serious dialogue between the two sides could be established. Any worry about theological "orthodoxy", the initial need of removing any reference to the Copernican debate, all the attempts to reconcile Galilean mechanics with Tychonic astronomy and Aristotelian natural philosophy were actually a matter of discussion within the Company, but of no relevance at all for Chinese scholars, who regarded Western metaphysics and theology as very naive and self-contradictory, compared with the advancement of European science.

They soon recognized the many inconsistencies of the Jesuit conciliatory attitude, coming independently to the conclusion that their astronomy and mechanics were incompatible with Scholastic philosophy. Verbiest had the occasion to experience their sharp insight when in 1683 he dedicated to Kangxi a compendium of the main achievements of both Galilean and Cartesian physics, trying to present them as a whole. Despite his high reputation at court, he was harshly criticized by many mandarins, who had never recognized his exceptional and maybe undeserved status. They attacked Verbiest precisely on the impossibility to maintain Aristotle's distinction between "natural" and "violent" motion (which was in principle coherent with Confucianism), if one must agree with Galileo's results about free fall and projectile motion. Verbiest's book was basically a manual of experimental physics, with very few references to Aristotle. Anyway the Emperor accepted the dedication, given his personal friendship with Verbiest and the many useful notions contained in the book, certainly not for its theoretical relevance.

As a matter of fact, the Jesuits were on their turn object of a much more successful strategy of "enculturation". On the one hand, the Chinese scholars had no problems to replace a traditional but less developed astronomy with a foreign but more advanced mathematical physics, even when in 1760 the French father Michel Benoist (1715-1774) finally adopted Copernicus, Kepler, and Newton as basic sources for teaching. According to them, this simply meant to add new perfection to which was already perfect, with further advantages for the sciences and the arts. Why do not use more powerful, efficient, and fruitful means, if they were available? On the other hand, the Europeans had much to learn from the Chinese in almost all the other fields of knowledge. The question of the real constitution of the Universe was subordinated to the preservation of a cosmic harmony which should reflect and be reflected by the mundane order. The idea that nature must obey to necessary and immutable laws fitted very well into the Confucian framework. The fundamental unity of metaphysics, natural philosophy, ethics, and politics had just to be reconsidered on much better (but not totally new) cosmological bases.

NATURAL HISTORY AND MEDICINE FROM THE 16TH TO THE 19TH CENTURY

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THE ILLUSTRATIONS IN GIROLAMO MERCURIALE'S *DE ARTE GYMNASTICA*: THE DEPLOYMENT OF AN ARCHITECTURAL DESIGN IN THE MAKING AND THE COMMUNICATION OF MERCURIALE'S MEDICAL DISCOURSE.

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Abstract

Due to the rich content of Girolamo Mercuriale's De arte gymnastica *(Venice, 1569), drawing from other than purely medical sources (textual as well as material), the task of designating the medical nature of the book in its full spectrum suggests at the same time the mapping of the boundaries of sixteenth century medicine as well as the book's "scientificity", as the product of a sixteenth century "rational and learned" physician. Mercuriale's diagnosis of "new diseases" and his prognosis of "future" diseases surprisingly (or not) direct the "modern" readers (medical men and others) of the book back to antiquity to find the proper method of medical treatment for all the "modern" scourges. In this framework, aspects of the ancient past are reconstructed in "medical" images and they are examined through textual and material (antiquarian objects, remaining, etc.) sources which are interpreted in "modern" terms so as to demonstrate the timeless medical value of the ancient, "traditional", "indigenous" culture. In the* De arte gymnastica *aspects of the ancient Greek and Roman culture are being medicalized by Mercuriale with the aim to form a "modern" medical art: the "art of gymnastics". In this medical discourse on health, disease and treatment Mercuriale raises issues regarding the limits and the credibility of the contemporary corpus of medical knowledge, its format and content, the focus of contemporary medical practice, as well as the nature and the effects of practices of contemporary lifestyle, having two "scientific" criteria: the notion of a healthy human body acknowledged as the actor of everyday life and culture, and the opinion that medicine can be benefited from other fields of knowledge too. How is the scientific nature of this medical book to be designated and which is its content?*

The present paper examines the use of architectural design in the making and the communication of the medical discourse impressed in the *De arte gymnastica*,[1] the work of the renowned medical humanist Girolamo Mercuriale (1530-1606). This medical book was first published in Venice by the Giunta Press in 1569, and its first edition featured a single illustration: the ground-plan of an ancient *gymnasium* (or *palaestra*). The second edition of the book, published by the same publishing house in 1573, was enriched with many more illustrations, courtesy in their majority of the painter, architect and antiquarian Pirro Ligorio (1513-1583). Among the illustrations of the second edition we find two such ground plans: one of a square and one of an oblong palaestra.

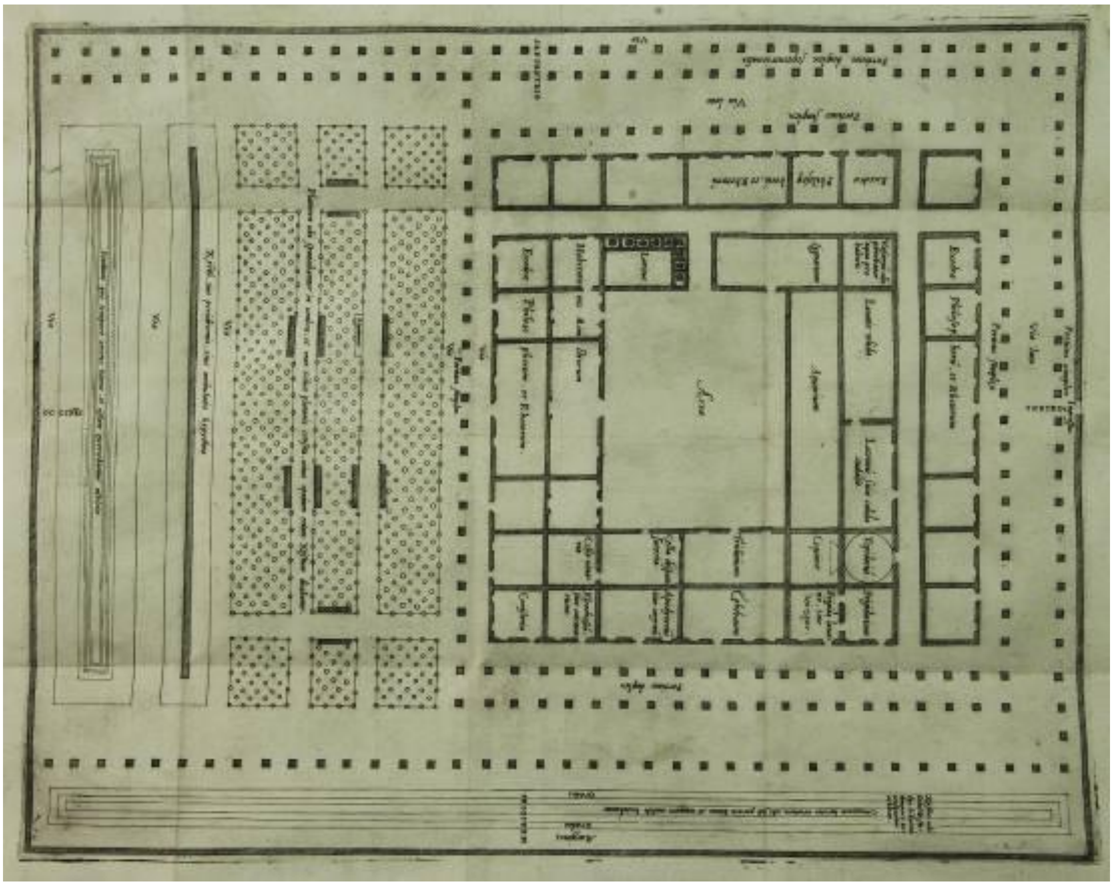

Fig. 1: The illustration of the ground plan of the ancient *gymnasium* (*palaestra*)
in Girolamo Mercuriale's *De arte gymnastica* (Venice, 1569).
Courtesy of the Biblioteca Nazionale Centrale di Roma.

The pictorial material of Mercuriale's *De arte gymnastica* has attracted much scholarly interest;[2] however scholars have focused mainly on the enriched, second edition of the book. In this way, much less has been said about the presence of this single illustration of the first edition of the *De arte gymnastica*, which is surprising especially when considering the elaboration of the particular drawing in the second edition of the book. More importantly, the use of such a *type* of drawing in an early modern medical book has not yet been problematized. In this paper I would like to offer some considerations

[1] *Artis Gymnasticae apud antiquos celeberrimae, nostris temporibus ignoratae. Libris sex, in quibus exercitationum omnium vetustarum genera, loca, modi, facultates, & quidquid denique ad corporis humani exercitationes pertinet, diligenter explicatur. Opus non modo medicis, verum etiam omnibus antiquarum rerum cognoscendarum, & valetudinis conservanda studios ad modum utile. Palestrae descriptio ex Vitruvio sub litera B. Auctore Hieronymo Mercuriali Foroliviensi Medico, & philosopho. Venetiis* (Venezia), Apud Iuntas, In officina Iuntarum, MDLXIX (1569), prima edizione.

[2] See Nancy Siraisi, "History, Antiquarianism, and Medicine: The Case of Girolamo Mercuriale", pp. 231-251, in *Journal of the History of Ideas*, Vol. 64, No. 2. (Apr., 2003); Jean-Michele Agasse, 'Girolamo Mercuriale. Humanism and Physical Culture in the Renaissance', trans. from French by Cristine Nutton, in *Girolamo Mercuriale, De arte gymanstica. The Art of Gymnastics*. Critical Edition by Concetta Pennuto, transl. in English by Vivian Nutton, (Firenze: Olschki, 2008); Frederika Jacobs, *The Living Image in Renaissance Art*, (Cambridge: Cambridge University Press, 2005); Ginette Vagenheim, 'Some newly-discovered works by Pirro Ligorio', Journal of the Warburg and Courtauld Institutes, LI, 1988.

regarding how in the context of the *De arte gymnastica* the use of an architectural design –viewed as an aspect of the circulation of architectural as well as antiquarian knowledge, interests, methods and practices, as this took place in mid-sixteenth century Rome– played a role in the making of Mercuriale's medical discourse as well as its communication to his audience, identified primarily as the Roman social (religious and lay) elite.[3]

I would suggest that Mercuriale's deployment of an architectural design in his *De arte gymnastica* presupposed and implied as well, the circulation of a series of elements; the drawing of the ground plan of an ancient palaestra in Mercuriale's book appears as the meeting point of classical authoritative texts as recovered by humanists, antiquarian interests and practices, ideas and values, as well as architectural interests and methods, as their circulation took place within a local context, that of mid-sixteenth century Rome. What I argue in this paper, is that in the context of the *De arte gymnastica* the deployment of this architectural design makes part of Mercuriale's endeavour to (re)construct and effectively communicate practical information as well as values and principles (ethical and cultural) in relation to what he defines as 'medical gymnastics', which he promotes in his book as a method of medical practice and treatment –and a legitimate part of *medicina conservativa* (or *hygiene*)– for the "new" as well as the "future diseases".[4]

Mercuriale wrote his *De arte gymnastica* during his staying in Rome, in the years 1562-1569, where he served as a practicing physician in the household of Cardinal Alessandro Farnese (1520-1589), to whom the first edition of the book was dedicated. It is in the 'Roman context' therefore, that the present paper suggests that the use of the architectural design in Mercuriale's book should be viewed.[5] There, as a member of the Cardinal's court being situated in the heart of the papal court, being a large however one amongst the numerous Roman courts (curial and lay), and, as such, making part of the broader social and cultural space of the city, Mercuriale had the opportunity to live and work among members of the local intellectual *milieu* with common interests, values and principles, as well as a common 'noble' lifestyle. Two areas where all these found a vivid expression during the sixteenth century, were the fields of antiquarianism and architecture.

Cardinal Alessandro Farnese himself, one of the greatest patrons of art and architecture at the time, had a known interest in the Greco-Roman classical culture while he owned a notable collection of antiquities.[6] Of course, by no means he was the only patron with such interests and pursuits in Rome at the time. Building and architectural (mainly secular) projects sponsored by noble individuals or families, was an area were humanist ideas and ideals as nurtured in the Roman court-life, were manifested pursuing a classical urban style and decorum for what was built and planned in Rome at the time, public and/or private.[7] These projects were greatly pre-informed on one hand by antiquarian research in the effort to achieve successfully the reconstruction, restoration of the classical Rome within the modern Rome, and on the other hand by architectural values and ideas inspired and/or drawing from (to a greater or to a lesser extend) the classical authority of Vitruvius and his work *De architectura libri decem*.

Vitruvius' *De architectura*, the only full treatise on architecture and its related arts that had survived from classical antiquity, was one of the most influential and disseminated classical texts that had a significant impact on renaissance thought.[8] After the discovery of its existence in the early fifteenth century, it was recovered by important humanist figures and was published in printed form (as well as in illustrated and translated, from Latin, editions) numerous times during the fifteenth and the sixteenth centuries.[9] It became widely diffused as a point of reference among the dedicated enthusiasts having a general or more specialized interest in antiquity and ancient architecture: those interested in the Roman ruins, those interested in designing buildings *all'antica*, those who were interested in the reconstruction and/or restoration of the ancient

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[3] The topic of this paper makes part of my ongoing PhD research, therefore the suggestions put forward are not yet made in the light of final research results.

[4] See Girolamo Mercuriale, *De arte gymnastica*, (Venice 1569), Liber I, Caput I, *De Principiis Medicinae* ('The Origins of Medicine').

[5] On the 'Roman context', which sees early modern Rome as an active centre of scientific culture, indicatively see: *Rome et la Science Moderne. Entre Renaissance et Lumières*, Études réunies par Antonella Romano, (École Française de Rome, 2008); (*Naples, Rome, Florence. Une Histoire Comparée des Milieux Intellectuels Italiens* (*XVII-XVIII Siècles*), sous la direction de Jean Boutier, Brigitte Marin et Antonella Romano (École Française de Rome, 2008); *Conflicting Duties: Science, Medicine and Religion in Rome, 1550-1750*, ed. by Maria Pia Donato and Jill Kraye, Warburg Institute Colloquia 15, (London-Turin: The Warburg Institute-Nino Aragno Editore, 2009).

[6] On Cardinal Alessandro Farnese see Clare Robertson, *'Il Gran Cardinale'. Alessandro Farnese, Patron of the Arts*, (New Haven and London: Yale University Press, 1992).

[7] See Peter Partner, *Renaissance Rome 1500-1559. A Portrait of a Society*, (California: California University Press, 1979).

[8] See Ingrid D. Rowland and Thomas Noble Howe, *Vitruvius. Ten Books on Architecture*, (Cambridge: Cambridge University Press, 1999); Ingrid D. Rowland, *The culture of the High Renaissance: ancients and moderns in sixteenth-century Rome*, (Cambridge: Cambridge University Press, 2000).

[9] Among the most important editions we find the ones by: Fra Giovanni Giocondo (an illustrated edition printed in 1511 in Venice, reprinted in 1522 in a revised edition in Florence), Cesare Cesariano (the first edition to be published translated in Italian, in 1521), Daniele Barbaro (in 1556, with illustrations by Andrea Palladio).

Roman buildings, etc., found an important tool in Vitruvius.[10] Among those enthusiasts we find Mercuriale too, who in his endeavour to provide an accurate description of the ancient Roman palaestra, turns to his erudite Roman acquaintances familiar with the work of Vitruvius as well as the city of Rome, so as to be provided with a reliable interpretation of the respective part of the *De architectura*, namely the *De palaestrarum aedificatione et xystis* ('The construction of the *palaestra* and its porticoes') from Book V, Chapter XI of Vitruvius' treatise.

In the first edition of the *De arte gymnastica*, Mercuriale notes that the description of the palaestra is based on the interpretation of the respective part of Vitruvius' work offered by Ottavio Pantagatho,[11] who was an antiquarian and one of Mercuriale's acquaintances in Rome. The drawing corresponds to the textual description provided, as it visualizes the different parts of the palaestra, which are recited in full writing on the drawing itself. Judging from the importance that Mercuriale attributed to a correct interpretation of Vitruvius as well as from the correspondence between the drawing and the textual description provided, the accuracy of the drawing rested exactly on the accuracy of the interpretation of the Vitruvian text.[12]

In fact, Mercuriale in the second edition of his book notes how he provides the reader with a meliorated interpretation of Vitruvius, benefited this time from the help of Alvise Mocenigo (son of Francesco), Gian Vicenzo Pinelli, Melchior Guilandino and Andrea Palladio, identifying the latter as the greatest expert in the field of ancient architecture.[13] In these terms, he notes how he also provides two such ground plans, because the author (Vitruvius), Mercuriale marks, says that the palaestra could be both square and oblong.[14] Given the framework and the length of this paper, I am not going to examine either the elaborated textual description, or the addition of the second ground plan, or the different placing of the illustrations in the second edition of the book;[15] rather I would like to offer a series of suggestions regarding primarily the *use* of such a *type* of drawing in the context of Mercuriale's medical discourse. In order to do this I will focus on the *synergy* between text and image and map its effect in relation to the primary audience of Mercuriale's book: the Roman social elite.

In the first edition of the *De arte gymnastica* we find the ground plan of the palaestra placed in the first, opening pages of the book, exactly before the opening of Book I. Nonetheless, as noted earlier, the drawing follows a textual description of the Vitruvian palaestra, which Mercuriale provides in Book I of the *De arte gymnastica*, Chapter VI, entitled *De gymnasiis antiquorum* ('The gymnasia of Antiquity'). On the drawing we find impressed the different parts of the palaestra recited in full writing and as indicated in the textual description provided in the chapter. In these terms, the illustration corresponds to and makes part of the particular chapter, as it is associated to the text and it visualizes, conveys information found in the text. But what is the chapter about?

The chapter begins with Mercuriale noting *that* "*after establishing that gymnastics or the art of exercise used to be practiced in specific places, it is reasonable to explain what the places themselves where and of what sort*".[16] Indeed, Mercuriale begins with asserting that gymnastics in the ancient times was practiced in specific places called gymnasia and giving the definition of the word *gymnasium* (from the Greek word *gymnazesthai*, i.e. to become naked) he tackles the scandalous issue of nakedness by noting that it was not certain that all who exercised only for the sake of health took off their clothes. Following, Mercuriale gives a brief history of the first Greek and Roman gymnasia, in which he brings up a series of explanatory and clarifying points regarding the names employed to describe gymnasia in the Roman times, as well as regarding the use of the gymnasia. In this framework, Mercuriale notes how Latin-speaking authors used other terms that described or designated the Greek gymnasium and he also makes a distinction between the original gymnasium and places introduced for other purposes and activities (e.g. for education, or other arts and festivals).

What is worth noting is how Mercuriale emphasizes on particular parts of the ancient gymnasium and aspects of its use. He notes characteristically how Plato and Aristotle were in the habit of doing philosophy in the (so-called) Athenian gymnasia: the Academy and the Lyceum respectively. As regards, he also marks how the Academy was considered "*the*

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[10] See *Andrea Palladio. The Four Books on Architecture*, transl. by Robert Tavernor and Richard Schofield, (Massachusetts: The MIT Press, 1997), pp. vii-viii.

[11] See Girolamo Mercuriale, *De arte gymnastica*, (Venice, 1569), Liber I, Caput VI, *De Gymnasiis Antiquorum* ('The Gymnasia of Antiquity'), p. 9.

[12] In fact, the original Vitruvian text was difficult to comprehend fully as it was not illustrated and it made use of technical terms in Latin and in a mixture of Greek and Latin. See *Andrea Paladio. The Four Books on Archittecture*, transl. by Robert Tavernonr and Richard Schofield, (Massachusetts: MIT Press, 1997), p. vii.

[13] '[…] *Andreas Palladius priscae totius architecturae peritissimus*' in Girolamo Mercuriale, *De arte gymnastica*, (Venice, 1573), Liber I, Caput VI, De Gymnasiis Antiquorum ('The Gymnasia of Antiquity').

[14] See Girolamo Mercuriale, *De arte gymnastica*, (Venice, 1573), Liber I, Caput VI, De Gymnasiis Antiquorum (The Gymnasia of Antiquity').

[15] These are issues that I have identified as 'problems' and I examine analytically in my PhD thesis.

[16] 'Gymnasticam, sive exercitatoriam, in certis locis fieri solitam, quoniam supra statuimus, rationi modo consentaeum, est quid loca ipsa et qualia forent planum facere', in Girolamo Mercuriale, *De arte gymnastica*, (Venice, 1569), Liber I, Caput VI, De Gymnasiis Antiquorum ('The Gymnasia of Antiquity').

noblest gymnasium on earth".[17] Calling on the authority of Vitruvius, Mercuriale marks how the gymnasia were by structure appropriate to host exercises of *both* the body and the mind, as they had a large capacity and they were great in extent. He notes characteristically that in the gymnasium "*it was possible to carry out without any hindrance numerous exercises of diverse types, of both body and mind*".[18] The chapter ends with the analytic description of the structure of the gymnasium and the use of its parts.

Judging by the content of the chapter I would suggest that Mercuriale is mostly concerned to define the palaestra as a physical space and regarding its use; initially asserting what the gymnasium was and what it was not, and in the following regarding its function (exercise of both the body *and* the mind) explaining how this was in fact served by the structure of the gymnasium. In this framework of description-definition of the palaestra, Mercuriale tries to demonstrate that the gymnasium was a perfectly legitimate and decent public place, where people (among them learned men of high authority such as Plato and Aristotle) could practice both bodily and intellectual activities. As such, in Mercuriale's discourse the ancient palaestra appears by structure to be the appropriate place for the exercise of the body (and of the mind), qualifying as well as to the status of the noble people who attended it or whish to attend it.

In these terms, I would suggest that Mercuriale's aim in this chapter is to provide an *image* of the palaestra, such, that it would correspond to the art of gymnastics promoted in his *De arte gymnastica* as the *noble* art of exercise for medical reasons (i.e. the 'medical gymnastics'),[19] suitable to the decorum of his noble audience. To put it in other words, through this discourse Mercuriale tries to dis-implicate the ancient palaestra (as the place were gymnastics was practiced) from its notorious fame owing to activities taking place there that were stigmatized as 'immoral' and 'irregular' with the decline of the Roman world, as well as in Mercuriale's time especially in the framework of post-Tridentine attitudes. In this way, he dis-implicates medical gymnastics as well, so as to promote it as a noble and gentle method of medical practice appropriated to the status and the decorum of his audience. This dis-implication could have worked in two ways in the framework of Mercuriale's professional agenda: it could either legitimize 'ambiguous' bodily activities practiced by members of the elite by attributing to them a medical value,[20] and/or promote the decorum of this medical treatment to members of the elite who were primarily interested in intellectual activities and thought less of bodily activities.

But how does the ground plan of the palaestra make part of this discourse on the noble qualities of the gymnasium and –subsequently– of Mercuriale's medical gymnastics? To answer this question it would be useful first to look into the illustration itself. The drawing is an orthographic projection, which, as a formula, constituted a chief graphic device for the sixteenth century architects.[21] It was a method to represent the interior and exterior elevations of a building, not in perspective but laid out flat in a consistent scale. In this way, every measurement and relationship of each part of the building to each other and to the whole of the building may appear on the drawing as on the building itself, only reduced by a consistent factor. As a drawing it was more abstract, but it could be translated directly from the drawing into the actual building and vice versa.[22]

This is something that had not been possible with earlier perspective drawings in which measurements were affected by the position and distance of the observer. James Ackerman has suggested that, as such, the orthographic projection was an expression of the Renaissance architects' concerns regarding the rationalization of proportion and the refinement of techniques of representation, in their endeavour (as dictated by the humanist ambitions) to describe accurately and systematically the ancient physical remains. In these terms, Ackerman suggests that this type of drawing was not merely a means to record the visible ancient findings and physical remains; rather it was a 'tool' to design a *model* of a building for others to follow.[23]

In this context, the deployment of Vitruvius is of crucial significance. Vitruvius' concern to provide architecture with a distinct structure and order, as well as his view that geometry, measure and proportion were the qualities and characteristics of the Greek and Roman architectural achievements, were considered unsurpassed paragons for Renaissance architecture;

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[17] '[…] in academia Atheniensium nobilissimo totius orbis gymnasio' in Girolamo Mercuriale, *De arte gymnastica*, (Venice, 1569), Liber I, Caput VI, De Gymnasiis Antiquorum ('The Gymnasia of Antiquity').

[18] '[…] innumerae exercitationes et corporum et animorum peragi possent…ex Vitruvii allata descriptione' in Girolamo Mercuriale, *De arte gymnastica* (Venice, 1569), Book I, Chapter VI, De gymnasiis antiquorum ('The Gymnasia of Antiquity').

[19] In contrast to the military and athletic gymnastics, the medical gymnastics were considered the only legitimate type according to Galen.

[20] Practices of the court-life mentioned by Farnese's delegates, such as wrestling, dance and hunting (see Clare Robertson, *op. cit*., pp. 75-76) are listed as 'exercises' with a medical value by Mercuriale in his *De arte gymnastica*.

[21] See James Ackerman, 'The Involvement of Artists in Renaissance Science" in John W. Shirley and F. David Hoeniger (eds.), *Science and the Arts in the Renaissance*, (London and Toronto: Associated University Press, 1985), pp. 122-124.

[22] *Ibid*.

[23] *Ibid*.

they mirrored the beauty of nature and of the human body.[24] In these terms, the sixteenth century experts and enthusiasts found in Vitruvius the means with which they could interpret the remains of ancient buildings, measuring them and then restoring their forms in drawing. In this way they could design buildings *all'antica* reviving the harmony that the ancients sought between nature, the human body and architecture.[25] With the deployment in the context of the *De arte gymnastica* of the ground plan of the palaestra following Vitruvius and the aforementioned values that his work entailed, I would suggest that Mercuriale aims to provide such a model, expressing the Renaissance attitude according to which the classical antiquity should primarily serve as a model and standard for contemporary life.

Nonetheless, the ground plan of the palaestra serves functional purposes too. The deployment of the orthographic projection enhances Mercuriale's effort to describe analytically and systematically the ancient gymnasium as a physical space, and to recite and define its parts and their use, in a way that parallels the respective interest of anatomists and anatomical illustration. It is a type of visual depiction that demonstrates Mercuriale's taxonomic concerns as well as his endeavour to define and regularize (in practical as well as in ethical terms) the place and the uses of the gymnasium in relation to what he identifies and promotes as 'medical gymnastics' in his *De arte gymnastica*.

The orthographic projection, inspired from the fields of architecture and antiquarianism as manifested in Rome at the time, and in the context of Mercuriale's *De arte gymnastica* sustains a 'visual language' of communication between Mercuriale and his audience. It is a kind of encoding, enhanced with an epistemic value owing exactly to the ability of the audience to make the necessary inferences. If the audience was unfamiliar with the conventions deployed, then the utility of the illustration was to be compromised.[26] However, Mercuriale was addressing to an audience familiar as much with Vitruvius' work and the values and principles entailed in it, as with the use of this particular type of architectural design and its conventions. Cardinal Alessandro Farnese himself had a great fascination with architectural projects and he was very much conversant with architectural technicalities.[27]

In these terms, I would argue that the synergy between the text and the architectural design suggests Mercuriale's endeavour to offer a *model* of the ancient palaestra in its practical as well as its ethical qualities according to the Vitruvian values and principles, shared by Mercuriale's audience in early modern Rome. Mercuriale deploys this model in his medical discourse, promoting his medical gymnastics as a decorous –therefore appealing to the elite– method of medical practice and treatment, aiming to correspond to the contemporary medical, but as well as cultural and other needs, challenges, aspirations or trends as manifested by the Roman elite at the time. Surrounded in Rome by members of the religious and lay nobility who were interested in the ancient Roman and Greek past and bodily culture, interested in antiquarianism and in architecture, familiar with Vitruvius, as well as involved in building and architectural projects, Mercuriale, a University educated practicing physician, draws from the think-tank of this milieu in the making of his medical discourse. At the same time, with the same resources of thinking and conversing he addresses this milieu back promoting his professional agenda. In Mercuriale's *De arte gymnastica* we can see how a set of 'non-medical' elements (texts, practices, methods, ideas, etc.), as these were circulated and manifested within a particular local context, were deployed as resources in the making of a medical discourse, articulating and communicating practical as well as ethical aspects and qualities of the suggested medical treatment to an initiated audience.

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[24] See Ingrid D. Rowland, *The culture of the High Renaissance: ancients and moderns in sixteenth-century Rome*, (Cambridge: Cambridge University Press, 2000).

[25] See *Andrea Palladio, The Four Books on Architecture*, transl. by Robert Tavernor and Richard Schofield, (Massachusetts: The MIT Press, 1991), pp. vii-viii.

[26] See Bert S. Hall, 'The Didactic and the Elegant: Some Thoughts on Scientific and Technological Illustrations in the Middle Ages and the Renaissance' in Brian S. Baigrie (ed.), *Picturing Knowledge. Historical and Philosophical Problems Concerning the Use of Art in Science*, (Toronto: University of Toronto Press, 1996).

[27] See Clare Robertson, *op. cit*.

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AMATO LUSITANO AND HIS PILGRIMAGES TO EUROPE –HIS CONTRIBUTION TO THE DEVELOPMENT OF EUROPEAN MEDICINE IN THE SIXTEENTH CENTURY

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Abstract

This study covers a remarkable period of European history –The Renaissance– and its main objective is to study the contribution of the Portuguese doctor Amato Lusitano (1511-1568) to the development of European Medicine in the sixteenth century.
In terms of methodology we used the content analysis, using data collection to the Portuguese edition of Seven Centuries of Medicinal Cures*, published by the Faculty of Medical Sciences, New University of Lisbon, in 1980.*

Amato Lusitano is one of the major references of European Medicine of his time. It was worth the frontispiece portrait of books from colleagues (as, for example in *Portada de História Plantarum* of João Bauhino). Its services were ordered by characters as diverse and distinct as the Pope, the King of Poland, the city-state of Ragusa, or the Grand Vizier of the Ottoman Empire (Correia *et al.*, 1968; Dias, 1936).

Amato Lusitano was born in Castelo Branco, with the given name of João Rodrigues, he was only 14 when he went to Salamanca to study medicine. It is possible to deduce that he was a brilliant and hard-working student, for, when he was only eighteen, their teachers didn't hesitate to give him the management of wards with several patients. From his stay in Salamanca, he left us some references to teachers and fellow colleagues. It is possible that some of its teachers were António de Nebrija[1], Fernando Nunes, also called the Pinciano, Pontano and Olivares[2], and the one who Amato more recalls

[1] V *Centúria*, Cura C, p. 293.

[2] VI *Centúria*, Cura C, p. 163.

in one of his works, Lourenço Alderete. As for his colleagues, he mentions, among others, the names of Luís Nunes, João Aguilera, and André Laguna.

After graduation he returned to Portugal to, shortly afterwards, start a long pilgrimage to Northern and Southern Europe. His dedication to Medicine, but above all the desire for new knowledge, led him to long journeys in Portugal and several European countries. Amato mastered perfectly eight languages including Spanish, Portuguese, Latin, Greek, Arabic, and Hebrew. Building on this, he could well and freely transfer himself to any cities or countries where he found favorable conditions for the exercise of the medical profession (Jorge, 1914).

In 1533 he left for Antwerp, where he remained for about seven years. At this stage he began creating a reputation that transcended borders, and his medical services were repeatedly asked for in other countries and cities. While practicing medicine, he also worked in scientific research. In 1536, in Antwerp, he publishes his first work, the *Index Dioscorides*. This is the only work where Amato uses his given name, João Rodrigues de Castelo Branco, as it is possible to see on the cover of the existing copy in the Library of Évora (Correia *et al*., 1968; Dias, 1936).

Of the many people he was related to in Antwerp, it is possible to mention the humanists Erasmus (1584-1653), Conrado Goclenio, and Luiz Vives, tutor of Princess Mary of England.

In 1541, he left Antwerp and headed for the ducal court of Ferrara, probably attracted by promises of Hercules II d'Este, which allowed for the practice of Jewish religion. In this town, one of the brightest in all over Italy in sciences, letters, and arts, Amato amounted to the biggest celebrity that a doctor could aspire: the rank of professor. It is possible that, at Ferrara, Amato was the court physician, since he refers to a disease that Diana d'Este, a relative of Hércules II, was suffering from. He also mentions some contacts with famous doctors who had worked in the same town, including Savoranola, Hugo de Sena, Leoniceno (1428-1524), and Nicolas Monardes (1493-1588). It was during this period that Amato studied the properties of simple, counting for this purpose on the cooperation of the influential anatomist Gabriel Fallopio (1523-1562), who taught medical botany at Ferrara (Rodrigues, 2005).

António Musa Brasavola (1500-1555) and Giambattista Canano (1515-1579) were two doctors from Ferrara with whom Amato had friendly relations. The first was a disciple of Leoniceno and Monardes (1493-1588), was physician to several distinct figures, such as Charles V, Henry VIII, and the Popes Paul III, Leon X, and Clement VII. As for Canano, Amato considers him an expert anatomist, comparable to Vesalius. It was in the presence of this colleague, together with an assembly of other doctors, that Amato made the dissection of 12 human and animal bodies, where he encountered, at the opening of the azygos vein in the vena cava, valves that prevented the backward travel of blood (Rodrigues, 2005).

Meanwhile, Amato was invited to practice medicine in Ragusa but this was slow to materialize. Amato goes to Ancona, by then an important commercial town, whose development seems to have been closely associated with the Jewish community. At Ancona, Amato continues his professional practice and the preparation of Works for publication, concluding the first of the *Centúrias Medicinais*, *Curationem Medicinalium centuriae septem*, on December 1st, 1549.

Shortly after being established in Ancona, Amato went to Venice where he resided for a short period. In this town, he contacted doctors that he considered particularly skilled, namely Baptista Montano and Victor Trincavelle. It is known that both of them were professors at the University of Padua and that they left some works of reference (Correia *et al*., 1968; Dias, 1936).

In 1550, Amato is asked to go to Rome to look after Pope Julius III. Although the ills that afflicted the Pope are, as a rule, described as pleurisy, pneumonia, or other respiratory problems, evidence recorded in the *Centúrias* indicates that the Pope was rather suffering from syphilis, since Amato treated him with the root of China, a plant used in the treatment of this disease (Jorge, 1914).

Amato lived in Rome until the end 1551. Then, he went to Florence where he met Cosimo de Medici.

In 1552, perhaps due to a security issue, he went back to Ancona, then part of the papal territory. After the death of Pope Julius III (1555), whose pontificate was marked by a favorable treatment to the Jewish community, the new appointment fell on Paul IV. Numerous decrees were immediately promulgated against the Jews and, once more, their lands and fields were expropriated. Jewish doctors were forbidden to treat Christians, "ghettos" were settled and many Jews suffered imprisonment and torture (Lemos, 1889). When he received the announcement of the arrival of the emissaries of the new Pope, Amato left immediately for Pesaro. On withdrawal, he lost all his possessions and the considerable personal library, which he greatly regretted, as well as two important manuscripts, the *V Centúria* and some *Comentários sobre O livro de Avicena* [Comments on The book of Avicenna].

Amato remained only a few months in Pesaro, since, in August 1556, he finally went to his post in Ragusa. During the time he was in Ragusa, Amato pursued private practice, which enabled him to write several clinical cases that are depicted in the VI *Centúria*. We know that it was during his stay in Ragusa that Amato had a misunderstanding with serious consequences with Pietro Mattioli.

In May 1559, Amato finally gives up to find peace and security in Ragusa, then heading out to Thessaloniki. There, he actively exercises the clinic until his death from plague, on January 21, 1568.

Of the three works he left us: *Índex Dioscódires*, *Ennarraions* and *Curationum Medicinalium centuriae septem, varae multiplicique rerum cognitione referte; quibus praemissa est commentatio de introitu medici ad agrotantem, deque crisi et diebus decretoriis*, Burgos, 1620 –Seven Centuries of medical cures, the third one was the most publicized and that had more impact at the time.

The *Seven Centuries of medical cures*, the object of our analysis, is a collection of valuable observations of Surgery and Medicine, collected throughout his travels through Europe.

This work was first published in 1580 in Leon, when Amato had already died, and was reissued, complete or fragmentary, at least 57 times (Correia *et al*., 1968).

This number of reprints shows the influence that the *Centúrias* had, for several years, within several academic groups, namely doctors and Medical students from multiple universities in Europe. Each *centúria* includes 100 clinical cases (cures). It consists of two parts: in the first part, the presentation, patient's history, treatment, clinical; and then the Comments which evokes the classic and modern, discusses the effect of drugs, changes in treatments.

It is worth noticing that the comments and interpretations of Amato, although influenced by the philosophical and scientific mentality prevailing in Europe at the time, are remarkable. We can see from this table where references to Galen and Hippocrates dominate, which are usually the starting points for the presentation of clinical cases as well as to the type of therapy to be applied. He follows Avicenna, that Amato considers a "most learned man" and that, in his opinion, must be placed right after Galen. Amato often presents some contemporaries, such as Monardes, Andre Vesalius (1514-1564), Andre Laguna (1510-1562), or the anatomist Giambattista Canano. This means that Amato accompanied the works and discoveries of his colleagues (Rodrigues, 2005).

We found that the themes related to sexuality represent a significant proportion of the clinical cases. It is with this work that sexology is defined as a clinical matter, by the abundance of cases and the naturalness of their presentation –that occurs alongside other matters requiring the attention of the doctor– and by the uniformity of the clinical terminology used, completely identical to that used for other cases.

The *Centúrias* can be read as one of the first medical works where the rudiments of sexual disturbances begin to be drawn, that later on would become integrated into the science currently identified as clinical sexology (Rodrigues, 2005).

The approach to these matters has raised some controversy at the time. And the works as well as its author were subject to censorship. It is possible to find the presence of Amato Lusitano in the Index of forbidden books by the end of the 16th century. The name of Amato Lusitano appears, as a forbidden author, in the Indexes of 1581, 1612, 1632, 1640, and 1707. Amato is again the subject of censorship in the *Index auctorum damnatae memoriae*, published in Lisbon, in 1624. This Index contains several other authors, such as Alberto Magno, Arnaldo Villanova, Leonardo Fuchs, and André Laguna (Front, 2001; Tomás, 1991).

Conclusions

By the end of the 15th century, due to various social, economic, academic, and political factors, the whole European culture had undergone a profound transformation. This awakening of Europe to new values and ideas was characterized by what was conceived, at the time, as a renewal of the human spirit, leading to a new conception of life.

Amato Lusitano actively participated in this hectic time of European knowledge. Like all men living in critical times, in which traditions are questioned and generating new conceptions of humanity and the world are generated, he left us the legacy of a troubled soul, curious, eager to learn and improve. Although in his work dominates the spirit of inquiry and empirical observation, his angle of approach was necessarily affected by the contradictions of his time: on one hand, he had been formed within the framework of thought of ancient authorities, such as Galen and Hippocrates, or the scholastic tradition; but, on the other hand, he was attracted by the "new science", with its objective attitude, creative and free, focused on nature and man –adding that often his own observations led him to deny the Classic authors.

We consider, for all that we just said, that Amato Lusitano and his works were references in 16th century medicine.

Referencies

CORREIA, *et al*. (1968). Amato Lusitano: Miscelânea de Cartas e Documentos, *Estudos de Castelo Branco*, Castelo Branco: Estudos de Castelo Branco *de Farmacología e Terapêutica Experimental*, 8º vol, pp. 1-29.

DIAS, J. L. (1936). *Dr. João Rodrigues de Castelo Branco - Amato Lusitano. Ensaio Bio-Biográfico*, Lisboa: Congresso Da Actividade Científica Portuguesa, pp. 92-178.

DIAS, J. L. (1946) .Terapêutica da Sífilis em Amato Lusitano, *Arquivos do Instituto.*

FRONT, D. (2001).The expurgation of medical books in sixteenth-century Spain. *Bull History of Medicine*, 001. 75 (2): pp. 290-296.

JORGE, R. (1915) Comentos á vida, obra e época de Amato Lusitano, *Arquivo de História da Medicina Portuguesa*, 51.

LEMOS, M. (1899). *História da Medicina em Portugal. Doutrinas e Instituições*. Vol. I. e II, Lisboa: Publicações Dom Quixote/Ordem dos Médicos.

LVSITANI, Amati, (1620) *Curationum Medicinalium centuriae septem, varae multiplicique rerum cognitione referte; quibus praemissa est commentatio de introitu medici ad agrotantem, deque crisi et diebus decretoriis*, Bordéus.

PINTO-CORREIA, C. (2004), *O Testículo Esquerdo*, Lisboa: Relógio D'Água.

PUERTO, F. Javier (1991). História De La Ciencia y De La Tecnica. *El Renacimiento*, 11, Madrid: Ediciones Akal, S. A, pp. 7-54.

RODRIGUES, I. T.(2005). *Amato Lusitano a as Problemáticas Sexuais – Algumas contribuições para uma nova perspectiva de análise das Centúrias de Curas Medicinais*. Doctoral thesis, Vila Real: UTAD.

Tomas, J. P., (1991). *Ciencia y Censura, La Inquisición Espanõla y los libros científicos en los siglos XVI y XVII*, p. 193.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 975.

WANDERING *EXOTICA*. THE ILLUSTRATIONS IN NIEREMBERG'S *HISTORIA NATURAE* (1635)

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Abstract

Juan Eusebio Nieremberg's Historia Naturae *(1635) constitutes a rare contribution to natural history and natural philosophy coming from Spain during the first half of the seventeenth century. Featuring a theologically oriented approach to nature, mainly focused on the curious and the rare, the book is particularly noteworthy for the way it makes use of the materials (texts and illustrations) gathered by Francisco Hernández during his expedition to the New World in the 1570s. The story of these materials is well known among scholars, especially in relation to the Roman edition of Hernández's texts and illustrations produced by the* Accademia dei Lincei*. Nieremberg's treatment of these materials, however, has not received as much attention, despite some significant work being done on the American flora featured in his book. The purpose of this paper is to focus on the visual content of* Historia Naturae *and interpret it as a reflection of the kind of processes involved in the production and distribution of natural knowledge, in particular, circulating images representing unknown subjects such as New World animals and plants.*

The aim of this presentation is to examine a particular case of circulation of knowledge by visual means: the corpus of illustrations featured in the book *Historia naturae, maxime peregrinae* (Antwerp, 1635), by the Jesuit scholar Juan Eusebio Nieremberg (1595-1658).[1] Active in Madrid for most of his life, Nieremberg was a well-known figure in seventeenth-century Spain, mainly due to his numerous religious writings.[2] Interestingly for the history of science, Nieremberg was also the first holder of the chair of natural history at the Reales Estudios –inaugurated in 1629– of the Jesuit Colegio Imperial in Madrid. In

[1] This paper is based on materials discussed in my ongoing Ph.D. project devoted to Nieremberg and Baroque science in seventeenth-century Spain. I would like to thank the FPU Grant Program of the Spanish Ministry of Education and research projects HUM2007-63267 and HAR2010-15099 for their support, and the attendants at the ESHS meeting in Barcelona for their comments.

[2] Juan Pimentel, "Baroque Natures: Nieremberg, American Wonders and the Preter-Imperial Natural History." *Science in the Spanish and Portuguese Empires, 1500-1800*, Daniela Bleichmar *et al*, eds. Stanford Calif.: Stanford University Press, 2009.

connection with these teaching duties, he produced several works devoted to natural philosophy and natural history, including *Historia naturae,* the most important and internationally oriented of them all.[3]

Nieremberg's aim in this book is twofold. On the one hand, it intends to present a description of New World *naturalia*, with a special emphasis on the rare and curious. This constitutes the content of Books VIII-XVI, which begin with human beings and end with minerals and the elements. But before that, in the first seven books, he provides a natural philosophical account of the New World in order to explain –this still remained an important question at the time– the origin of men, animals, plants, natural elements in general, there. How did they reach these lands? Did they exist before the Flood? As it is stated in the title of the book, the presence of unknown creatures in the New World posed the question of how this evidence agreed with the Biblical account of the Creation, which for Nieremberg provided the appropriate framework to understand the origins and constitution of the natural world. Above all, by looking at rare and curious phenomena, the ultimate aim of the book is to praise God's creation and God's masterly design.[4]

The book is a fine example of bookish accumulation. Nieremberg, who hardly left his studio in Madrid, quotes from many authors and many sources: books, manuscripts, reports, etc. In this regard, one of the most important aspects of this book is Nieremberg's use, for most of the treatise, of a particular set of materials: those gathered by the Spanish physician Francisco Hernández during his expedition to New Spain. The expedition was probably the most important project of sixteenth-century Hispanic science.[5] In 1569, Philip II nominated Francisco Hernández (1515-1587) his «chief medical officer of the Indies», and sent him on a mission to collect as much information as possible on the people, the fauna and the flora of New Spain and Peru. The expedition lasted from 1571 to 1577, and only New Spain was explored. Still, Hernández was able to provide descriptions of thousands of new plants, as well as hundreds of descriptions of animals, minerals and other phenomena. The materials he sent from Mexico amounted to a sixteen-book work of about nine hundred pages and around two thousand pages of illustrations. Hernández, however, did not edit these materials. In 1580, the Neapolitan physician Nardo Antonio Recchi was commissioned by Philip II to «put in order» the content of these admittedly messy volumes, in order to «make them useful». Recchi thus made a selection based on medical usefulness, and finished it by 1582. He took a copy of his selection with him, as well as a copy of around six hundred images, and returned to Naples. The idea was to publish his selection (text and images) in Spain but the project did not succeed. The original manuscripts and illustrations gathered by Hernández, who died in 1587, remained at the Library of the Real Monasterio de El Escorial until 1671, when a great fire destroyed them.

The story of these materials is certainly convoluted and fascinating, particularly in the way it runs parallel to the story of the Roman *Accademia dei Lincei*.[6] Highly devoted practitioners of any form of natural knowledge, the Linceans became very interested in the materials from the Hernández expedition, to the point that they bought Rechhi's selection from his heir, and made copies of his illustrations, with the aim of publishing a treatise on the natural history of the New World. The result would be the *Rerum Medicarum Novae Hispaniae Thesaurus* (Rome, 1651), also known as the *Tesoro Messicano*, the *Mexican Treasury*. The genesis of this book was long and complex. Its editing process, which began around 1610, under the direction of Federico Cesi, the founder of the Academy, did not finish until 1651, when Cesi and the majority of the Linceans had already died.

It is in this context, several years before the publication of the final version of the *Mexican Treasury*, that we have to consider Nieremberg's *Historia naturae*, a book which should be regarded as a rivalling work. Unlike the Linceans, which were using Recchi's selection as their main source, Nieremberg not only had had access to the original materials kept at the Escorial but had also been able to use Hernández's own manuscripts, which had been kept at the library of the Colegio Imperial since the doctor's death. The *Carteggio* –the compilation of Lincean correspondence edited by Giuseppe Gabrielli– contains only one reference to the Jesuit's work, in a letter by Johannes de Laet, director of the Dutch West India Company,

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[3] Other books devoted to natural knowledge are *Prolusion a la doctrina y historia natural* (1629), *Curiosa Filosofia, y tesoro de maravillas de la naturaleza, examinadas en varias questiones naturales* (1630), and its second part *Oculta Filosofia* (1633).

[4] See chapters by Millones Figueroa and Ledezma in Millones Figueroa and Ledezma, eds. *El saber de los jesuitas, historias naturales y el Nuevo Mundo*. Madrid: Iberoamericana, 2005.

[5] For the most recent work on Hernández and the expedition, see Simon Varey et al, Th*e Mexican Treasury. The Writings of Dr. Francisco Hernández and Searching for the secrets of nature. The life and works of Dr. Francisco Hernández*. Stanford, California: Stanford University Press, 2000. In connection with Nieremberg see also José María López Piñero and José Pardo Tomás, *La influencia de Francisco Hernández en la constitución de la botánica y la materia médica modernas*, Valencia: Instituto de Estudios Documentales e Históricos sobre la Ciencia,1996.

[6] Irene Baldriga, *L'occhio della lince: i primi Lincei tra arte, scienza e collezionismo, 1603-1630*. Roma: Accademia nazionale dei Lincei, 2002; David Freedberg, *The eye of the lynx: Galileo, his friends, and the beginnings of modern natural history*. Chicago: University of Chicago Press, 2002; Sabina Brevaglieri, "Il cantiere del 'Tesoro Messicano' tra Roma e l'Europa. Pratiche di comunicazione e strategie editoriale nell'orizzonte dell'Accademia dei Lincei." *Sul 'Tesoro Messicano' e su alcuni disegni del Museo cartaceo di Cassiano Dal Pozzo*, S. Brevaglieri et al, eds. Roma: Edizioni dell'Elefante, 2007, pp. 1-68.

addressed to a friend of the Linceans in Rome.[7] The aim of the letter is to get updated information about the editing process of the Lincean book. In particular, Laet wants to confirm the news that such an important and long-awaited volume would not see the light due to the publication of a similar book –Nieremberg's *Historia naturae*. In fact, he seems surprised, for, in his view, this new work is not important enough to stop the publication of the Lincean treatise. Why? It is not about the text, he claims, but about the illustrations that would complement the descriptions of American *naturalia*: «[Nieremberg] has provided no images besides those which Clusius and others have already given». Novelty, originality. For a book about the natural history of the New World –Laet is saying– not only it was crucial to feature images; without novel ones, the book was not as worthy.

Against this backdrop of circulating materials, in the following pages the illustrations in *Historia naturae* will be described, with the double aim of examining to what extent is Laet's evaluation of Nieremberg's book accurate, and in which sense the value of Nieremberg's contribution to natural history could be measured in terms of its visual content.

Historia naturae features seventy images, all of them woodcuts. Twenty six are devoted to animals, eighteen to birds, seven to aquatic creatures, four to reptiles and fifteen to plants. None of them are devoted to human beings, insects or minerals. The few references in the literature describe these images as a unitary corpus, when in fact they constitute a rather heterogeneous group. We will be referring to four types of images.

The larger group is made up of thirty two woodcuts, based on images included in the book *Exoticorum libri decem*, by Carolus Clusius, published in 1605 in Leiden by the Plantin press. These illustrations feature next to many passages where Nieremberg either makes reference to Clusius or quotes directly from his text. Due to the similarities between the woodcuts in Clusius and Nieremberg's works, it is highly possible that the same woodblocks were used. Reusing older woodblocks had been and was still common practice in printing houses, either in preparation of further editions of the same book or editions of different works devoted to a similar theme.[8]

The second important group of images is made up of twenty seven woodcuts, which feature the signature of the Flemish engraver Christoffer Jegher, an important collaborator of Peter Paul Rubens.[9] Jegher worked for the Plantin-Moretus Press for almost two decades, until 1643, mostly devoted to the design of title pages for religious works. As part of Rubens' team of engravers, he was responsible for a sort of revival of the woodcutting technique, in a period, the 1630-1640s, dominated by copper engraving and etching. Mentioned somehow loosely in the literature about Nieremberg and the Hernández materials, Jegher's contribution to the visual content of *Historia Naturae* is more problematic that it seems. It is generally assumed that he was the author of *all* the woodcuts in the treatise. However, it is not possible to conclude that he was the only responsible for all the engravings. Even if we focus only on the images that feature his signature, Jegher's contribution is still problematic. To what extent is his production original, in the sense expressed by Laet in his letter? A close look at the images reveals that many of his woodcuts are just very close versions of other engravings. The figures are practically identical. In terms of visual content they are no more novelties than the woodcuts taken from Clusius. Some of them are even more outdated, for they are taken from books that were published much earlier than 1605. Other engravings by Jegher, however, do offer novel images.[10] Which leads to the question: which materials did Jegher use to produce his woodcuts? The generally accepted answer is that he based his designs on the original images of the Hernández expedition. This is, again, problematic. It is rather unlikely, though not impossible, that Jegher travelled to the Escorial to work with the original images. There is no record of his presence in Madrid at the time. Besides, Nieremberg's text does not include any reference that could suggest a sort of collaboration between the Jesuit and the engraver. So, on the whole, we do not know which materials (copies of the originals) Jegher used. It seems that the original images that he had access to were not many, and their provenance is unknown.

The third group of images is made up of six engravings without signature. Two of them represent two birds of paradise.[11] The first one is a slightly modified version of the woodcut published by Conrad Gessner in the third volume of his *Historiae animalium* –dedicated to birds–, from 1555.[12] Reproduced in many other books, this image was for a long time the most important visual reference of the bird of paradise. The second image is very similar to the first of five engravings of birds of paradise published by Aldrovandi in his *Ornithologiae* of 1599.[13] Interestingly, the original watercolour on which the

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[7] Leiden, 10 october 1636. Giuseppe Gabrieli, ed. *Il Carteggio Linceo*. Roma: Accademia Nazionale dei Lincei, 1996, n. 1038, pp. 1242-1243; translation from Freedberg 2002, pp. 286-287.

[8] Dirk Imhof, ed. *The illustration of books published by the Moretuses*. Antwerpen: Plantin-Moretus Museum, 1997, esp. pp. 53-63.

[9] Mary L. Myers, "Rubens and the Woodcuts of Christoffel Jegher." *The Metropolitan Museum of Art Bulletin* 25, nº. 1 (Summer 1966): 7-23; Max Rooses, *Le Musee Plantin-Moretus*. Anvers: G. Zazzarini, 1913, pp. 299-302.

[10] Some examples: «Tlaquatzin» (*Historia naturae*, p. 156), «Pinuum dasypus» (p. 153), «Mapach» (p. 175), «Mater formicarum, sive Tzicatlinan» (p. 272), «Acaltetepon, seu Monoxilus Mucronatus» (p. 275).

[11] *Historia naturae*, pp. 210, 211.

[12] *Historiae animalium liber III qui est de auium natura*, Tiguri: apud Christoph. Froschouerum, 1555, p. 612.

[13] *Ornithologiae, hoc est de avibus historia libri XII* (1599) Bononiae, Franciscum de Franciscis,1599, p. 810.

latter engraving was based –which is extant, and kept at the Aldrovandi Archives in Bologna– is identical to the image of the bird of paradise included in the so-called *Pomar Codex*, the illustrated album that Philip II gave to the physician and professor of *simples* in Valencia, Honorato de Pomar.[14] In fact, many images included in the *Pomar Codex* are similar to those kept at the Aldrovandi collection, which makes this an interesting case of circulating images: were they copies from the same source, or one group of images was based on the other?[15] The third unsigned image, which features the «reverse» fish or *gaicanus*, is based on a woodcut from the fourth volume of the history of animals by Gessner, published in 1558.[16] Also from this book, the fourth image represents the *mors piscis* or walrus.[17] The fifth one, which represents the *granadilla* or Passion flower –complete with the signs of Christ's Passion– constitutes a slightly modified version of a model of this plant that became very popular in the first decades of the seventeenth century –as in *Cultura ingeniorum*, by Antonio Possevino, for example, published in 1610.[18] Finally, the sixth engraving depicts the «queen of snakes» or «teuhtlacocauhqui», whose discussion opens the twelfth book of *Historia naturae*.[19] Quite significantly, this image is very similar to the one included in the *Mexican Treasury* regarding the same snake, a fact that suggests a form of iconographic transfer from one book to the other –a plausible hypothesis, given that most of the visual material for the Lincean edition was prepared, even printed, by 1628.[20]

Finally, the fourth group of images is made up of five botanical illustrations, which already in 1954 Germán Somolinos d'Ardois identified as «those featuring Amerindian iconography».[21] In fact, only three of them feature the «water» and «earth» pre-Columbian symbols, a testimony of the local artistry that was employed during the Hernández expedition. How did these images reach the press remains unknown. They could belong to the corpus of preliminary sketches that Hernández kept in his possession until his death, which eventually were used to decorate –in a sort of collage– one of the royal chambers at the Escorial, the «Sala del Mediodía». In any case, these illustrations constitute a rare instance of a hybridised pictorial representation in a book, in fact in a whole discipline, natural history, that relied mostly on Europeanized illustrations.

In Nieremberg's time, images circulated much better than the objects they were meant to represent.[22] Natural elements would suffer from corruption and decay, whereas images, as Giuseppe Olmi has pointed out, «furnished not only a complete non-corrupted picture of the specimen, but also one that was incorruptible over time».[23] Images bridged huge geographical and temporal gaps. They turned distant and inaccessible entities into visible and manageable objects of study. They allowed for an alternative for first-hand observation, especially if the image was made «ad vivum», from the natural.[24] As the letter from Johannes de Laet reveals, the presence/lack of original images in a book on natural history was a key feature. Images, more than texts, constituted the novelty factor of a publication: new knowledge was represented chiefly

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[14] José María López Piñero, ed. *El "Atlas De Historia Natural" Donado Por Felipe II a Jaime Honorato Pomar*. 2 vols. Valencia: Vicent Garcia, 1990.

[15] In this regard, it is interesting to note that four decades later, Francis Willoughby and John Ray would include both images – the one in Nieremberg's book and the one in Aldrovandi's- in the same table devoted to the bird of paradise in their *Ornithologiae libri tres* (1676).

[16] *Historia naturae*, p. 250. Gesner, *Historiae animalium liber IIII qui est de piscium & aquatilium animantium natura*. Tiguri, apud Christoph Froschouerum, 1558, p. 483.

[17] *Historia naturae*, p. 257. Gesner 1558, p. 1265.

[18] *Historia naturae*, p. 299. On the history of the passionflower and its iconography, Emil E. Kugler and Leslie A. King, "A Brief History of the Passionflower." *Passiflora: passionflowers of the world*, Torsten Ulmer and John MacDougal, eds. Portland: Timber Press, 2004, pp. 15-26; on Nieremberg and the passionflower, Jorge Cañizares-Esguerra en *Puritan conquistadors: Iberianizing the Atlantic, 1550-1700*. Stanford University Press, 2006, pp. 147-150, 271-274.

[19] *Historia naturae*, p. 268.

[20] *Mexican Treasury*, p. 329. The engravings depicting the *hoitztlaquatzin* (*Historia naturae*, p. 154; *Mexican Treasury*, p. 322) and the *zainus* or *coyametl* (*Historia naturae*, p. 170; *Mexican Treasury*, p. 637) also look very similar. At Peter Mason's suggestion, the circle of Balthasar Moretus, Jegher and Rubens could be a good place to look for an answer. Rubens was a good friend of Johannes Faber, one of the most active Lincean working on the edition of the *Mexican Treasury*.

[21] Germán Somolinos d'Ardois, "Sobre la iconografía botánica original de las obras de Hernández y su sustitución en las ediciones europeas." *Revista de la Sociedad Mexicana de Historia Natural* XV, nº. 1 (1954): 73-86. The engravings are: «Atatapalacatl» (p. 306), «Teoamatl, vitae et mortis index» (p. 308), «Ayotli» (p. 309), «Tuna, sive nopalli saxis innascens» (p. 310) and «Capolin» (p. 344).

[22] Pamela H. Smith and Paula Findlen, eds. *Merchants & marvels: commerce, science, and art in early modern Europe*. London: Routledge, 2002; Peter Mason, *Before disenchantment. Images of exotic animals and plants in the early modern world*. London: Reaktion Books, 2009.

[23] Giuseppe Olmi, "From the marvellous to the commonplace: Notes on natural history museums (16th-18th centuries)." *Non-verbal communication in science prior to 1900*, Renato Mazzolini, ed.. Firenze: L.S. Olschki, 1993, pp. 240-241.

[24] Peter Parshall, "Imago contrafacta: Images and Facts in the Northern Renaissance." *Art History* 16 (1993): 554-579; Claudia Swan, "Ad vivum, naer het leven, from the life: defining a mode of representation." *Word and Image* 11, nº. 4 (Diciembre 1995): 353-372.

through new images. Following William Asworth, certain images appeared and reappeared persistently in modern natural visual culture: they circulated through books, and through versions of these images in other books or, in fact, in other visual media, such as paintings.[25]

In this regard, the images in *Historia naturae* constitute a rather heterogeneous and slightly unusual corpus. Copies next to original representations; versions of older, *classical*, illustrations mixed with depictions featuring non-European, *pre-classical*, symbolism. There is a strong sense of disparity, eclectic accumulation: some images have travelled from far, others have not even left the printing house; some reflect the European-centred, book-based tradition of natural knowledge, other speak for decentred, peripheral, local, forms of knowledge-making.

More specifically, what can we say about these images? Firstly, their number. There are not that many engravings. The images from the Hernández expedition amounted to two thousand botanical illustrations. And those devoted to animals, birds, fishes or reptiles could add up to several hundreds. Nieremberg's books features seventy images only. Comparing this quantity with the visual content in the *Mexican Treasury* the difference is noteworthy.

Secondly, originality. As Laet rightly noticed at the time, most of the visual elements in *Historia naturae* are based on previous images. The novelty of the Hernández materials is mostly missing. «There are lots of them, and so different one from another with respect to colours and forms, leaves as well as flowers, that one cannot find anything more curious than this, and if published it would be a great contribution to medicine», Cassiano dal Pozzo wrote in his diary after examining the original illustrations kept at the Escorial.[26] Surely, Nieremberg's book does present some images of certain species for the first time in European natural history. But these are almost like exceptions, given the extraordinary amount of novel illustrations that could have been included.

Thirdly, quality. These engravings are woodcuts, a declining technique by the middle of the century. As it soon became clear to the Linceans, this was a rather limited method to portray the natural world. The choice of such a reputed engraver as Jegher may seem as the sign of commitment for good quality illustrations, but the end result is not as outstanding compared to Jegher's other works in collaboration with Rubens.

On the whole, the corpus of images in *Historia naturae* constitutes a poor, and to a great extent failed, iconographical and epistemic project. Among the reasons for this we could single out the lack of resources at a time of deep economic crisis in Spain, the lack of appreciation of these materials on the part of patrons –as Fernando Rodriguez de la Flor has argued, Spain, at this time, was in the process of turning into a «metaphysical peninsula», devoting itself to poetry, drama, painting[27]– and, more importantly, the type of difficulties the Linceans encountered: printing a large collection of ever-circulating images was a gigantic task. *Historia naturae*, in this regard, could be seen as a reflection of the contingent nature of knowledge-making practices in the first decades of the seventeenth century.

[25] William B. Ashworth Jr., "The Persistent Beast: Recurring Images in Early Zoological Illustration". *The natural sciences and the arts*, Allan Ellenius, ed. Stockholm: Almqvist, 1985, pp. 46-66. Even woodblocks circulated: just when Nieremberg's work was being printed, most of the Plantin-Moretus stock of woodblocks was not available, for it was in London, lent for the printing of the second edition of John Gerard's *Herball* (Imhof 1997).

[26] Cassiano dal Pozzo, *El diario del viaje a España del Cardenal Francesco Barberini*. Alessandra Anselmi, ed. Madrid: Fundación Carolina, 2004, p. 227.

[27] Rodríguez de la Flor, Fernando. *La península metafísica. Arte, literatura y pensamiento en la España de la Contrarreforma*. Madrid: Biblioteca Nueva, 1999.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 980.

COMMEMORATING THE 250TH ANNIVERSARY OF THE ROYAL COLLEGE OF SURGERY OF BARCELONA (1760 – 1843): THE BEGINNINGS OF MODERN SURGERY

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Abstract

Taking the Royal College of Barcelona (1760-1843) as a case study, this communication shows the development of modern surgery in Spain initiated by Bourbon Monarchy founding a new kind of institutions by means of its academic activities of diffusion of science.
Founded 250 years ago, in 1760, the Royal College of Surgery of Barcelona was the second college of surgery created in Spain. The new college was erected near the old Hospital of Santa Creu. Learning human bodily structure by performing hands-on dissections in the anatomical theatre has become a fundamental element of modern medical education.
Antoni Gimbernat worked with tenacity from Madrid improving the quality of the royal colleges in Spain, but before that, he was professor of Anatomy in the College of Barcelona and the most famous Spanish surgeon with international recognition.
Gimbernat favoured the study of natural sciences, the new chemistry of Lavoisier and the experimental physics in the academic programs of surgery. As a result of that, the academic program developed in the new colleges of surgery was different from the traditional formal education for surgeons. The new physician-surgeons trained there were not only interested in therapeutics, but also wanted to know as much as possible about the bodies' anatomy and physiology, being as they were in competition with its pathology.
According to the study of a very relevant set of documents preserved in the library, the so-called "Juntas Literarias", we have notice about the main subjects debated in these clinical sessions about the concept of human being and diseases in relation to the development of the new experimental sciences. These documents evidenced that chemistry and experimental physics were considered crucial tools to understand the unexplained processes that happened in the human body in a medico-surgical context.

Introduction

The development of modern surgery in Spain was initiated by Bourbon Monarchy founding a new kind of institutions by means of its academic activities of diffusion of science. Founded 250 years ago, in 1760, the Royal College of Surgery of Barcelona was the second college of surgery created in Spain[1]. The new college was erected near the old Hospital of Santa Creu. Learning human bodily structure by performing hands-on dissections in the anatomical theatre has become a fundamental element of modern medical education. Taking the Royal College of Surgery of Barcelona (1760-1843) as a case study, first of all, I am going to introduce the purposes and the aims for founding this kind of institutions in our country. After that, I am going to look a very relevant activity of communication of science performed at the college, the so called "Juntas Literarias"[2]. Because, it's through these scientific manuscripts preserved until nowadays that we know the reception and development of the new sciences well as its application to the healing arts at the end of 18th century in Spain. According to the study of this very relevant set of documents preserved we have notice about the main subjects debated in these clinical sessions about the concept of human being and diseases in relation to the development of the new experimental sciences. These documents evidenced that chemistry and experimental physics were considered crucial tools to understand the unexplained processes that happened in the human body in a medico-surgical context. Professor Antoni Gimbernat favoured the study of natural sciences, the new chemistry of Lavoisier and the experimental physics in the academic programs of surgery. Then we will have a look of the uses of the experimental physics applied to medicine[3]. According with that, first, I will summarize the theoretical facts, controversies and debates electricity aroused, a natural phenomena the origins of which remained unknown. Secondly, I would like to stress the instrumental use of this knowledge applied to medicine and its practice. Thanks to the works performed about this subject at that moment we will see the application of this knowledge in practice. The conclusions will be expounded at the end.

The Royal Colleges of Surgery as an enlightened innovation

Founded 250 years ago, in 1760, the Royal College of Surgery of Barcelona was the second college created in Spain[4]. This institution was erected next to the Hospital of Santa Creu, an age-old health institution founded in1401. The Hospital provided the college with patients and corpses to be studied. The development of modern surgery was determined by an increasing demand for medical well-trained personnel able to attend the soldiers on the various fronts open in Europe. The Royal College of Barcelona was founded in a country bordering France (Catalonia, located in north-eastern Spain) and occupied by Spanish military forces. The Royal Colleges of Surgery represented the most important enlightened medical institution created by the Spanish Bourbon monarchy.

Working with tenacity, Antoni Gimbernat improves the quality of the Spanish royal colleges from his position in Madrid. Before, he was trained as a surgeon in the Royal College of Surgery in Cadiz and then he was professor of anatomy in the College of Barcelona. Later on he will be the most famous Spanish surgeon with international recognition. Thanks to a Royal grant he visited France, Great Britain, Scotland and Holland. When he came back to Spain he was nominated director of the third Spanish college of surgeons, The Royal College of San Carlos in Madrid. Later on he became Royal Surgeon of Carlos IV.

Gimbernat, with a good position, favoured from Madrid the study of natural sciences, the new chemistry of Lavoisier and experimental physics in the academic programmes of surgery. The new physician-surgeons trained there were not only interested in therapeutics, but also wanted to know as much as possible about the bodies' anatomy and physiology, being as they were in competition with its pathology. As a consequence of that these new medical-surgeons were not only interested in therapeutics, but wanted to know as much as possible about the parts of the body and its function, its physiology and in front of the illness, its pathology being in competition with physicians.

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[1] These documents have been preserved and digitalized in the Ancient Book Collection at the University of Barcelona. Núria PÉREZ-PÉREZ, Antonio SITGES-SERRA (2009), "Juntas Literarias: legado de la cirugía ilustrada" *Cirugía Española*. doi:10.1016/j.ciresp.2009.05.006.

[2] PÉREZ-PÉREZ, Núria (2010), "A new procedure to spread science in Spain: the "Juntas Literarias" at the Royal College of Surgery of Barcelona (1760-1843)", *Proceedings of the III European Society for History of Science* (EHSH) Vienna (Austria).

[3] PÉREZ-PÉREZ, Núria (2010), "Medicine and Science in a new medical-surgical context: the Royal College of Surgery of Barcelona (1760 - 1843)", *MEDICINE STUDIES: International Journal for the History, Philosophy and Ethics of Medicine & Allied Sciences*, *DOI: 10.1007/s12376-010-0039-z*.

[4] MASSONS, Josep Maria (2002) *El Reial Col·legi de Cirurgia de Barcelona*, Barcelona, Fundació Uriach 1838.

Spreading science in a new scenario

The academic programme developed in the new colleges of surgery was different from the traditional corporative education for surgeons. In this setting, and in the same way as it was being implemented in other enlightened scientific institutions belonging to this period, for instance societies and academies, a new form of transmission of knowledge and in our case a new method of teaching as well, was proposed in the regulations of the new colleges of surgeons in Spain. The rules of the Spanish colleges of surgery established to hold the so-called "Juntas Literarias". This means the regulation favoured activities in which a dissertation was read periodically and, after that, censored in a public session in front of both the students and the board of professors of the college. So both professors and students attended the dissertations that were read periodically by the professor in front of the audience. Then, the following session, another member of the College Board assigned by the vice-president of the college was in charge of elaborating a critical assessment, reading in public session again plus a critical writing about the dissertation. At the end of the session each professor member of the board gave a short written assessment or comment.

It is important to stress that these manuscripts were never published, but fortunately are available in the archives and constitute one of the most important primary sources to know the role and the reception of the new sciences applied to healing arts.

Chemistry and allied sciences to interested audiences

Thanks to the regulations promoted by Antoni Gimbernat in 1795[5], chemistry and experimental physics were included in the academic programs of the Spanish Royal colleges of Surgery. According to "Juntas Literarias" documentation it is clear that chemistry became especially present in the dissertations expounded at the Royal College of Surgeons of Barcelona[6]. The first reference to chemistry found in the college's files dates from 1797[7]. It is a dissertation about the use of carbonic acid to remove gallstones from the urinary bladder. In effect, the development of the study of airs crossed the boundaries between chemistry, physics, and medicine. The so called "natural philosophers" studied some problems that led them into chemistry but also into domains outside the normal scope of chemical practice. For example, the therapeutic use of aerostatic balloons was subjected to discussion in the sessions at the Royal College of Surgery of Barcelona[8]. The pros and cons, controversies aroused by these and other topics given at the sessions (vaccine, tumours, galvanism, syphilis, etc.) are already present in this important set of documents. The "Juntas Literarias" showed in public session the topics which aroused controversy, expectation and anticipation in the Spanish enlightened society in those days.

Electricity: from chemistry to experimental physics

I would like now to discuss some details about the emergence of Experimental Physics at the Royal College of Surgery of Barcelona. First of all I am going to have a look at the theoretical facts leading to a debate about this subject in scientific sessions. Secondly I would like to show some dissertations presented at the sessions of the college and some works published in relation with that. And finally I would like to emphasize the uses of experimental physics applied to medicine.

Regarding the nature of electricity, it was included among the chemical principia of the bodies in the beginnings but after Alessandro Volta's experiments electricity was considered an inherent force of the nature. In the middle of the eighteenth century, the abbot and professor of experimental physics Jean- Antoine Nollet (1700-1770) defined electricity as something with both irritating and stimulating character simultaneously[9]. The electrical fluid and electricity were analogous in relation to light and fire. For this reason, some scientists thought that the origins of all of these elements should be the same. The experiments presented in the Royal Society by Henry Cavendish (1771-1772) or the public demonstrations by John

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[5] *Ordenanzas de S.M. que deben observarse por el Real Colegio de Cirugía de Barcelona Cuerpo de cirugía militar, Colegios subalternos y cirujanos del Principado de Catalunya*, Madrid, 1795. APARICIO SIMON, José (1956), *Historia del Real Colegio de San Carlos de Madrid*, Madrid, Aguilar.

[6] PÉREZ-PÉREZ, Núria (2007) *Anatomia, química i física experimental al Reial Col·legi de Cirurgia de Barcelona (1760 – 1808),* PhD dissertation, Philosophy Deparment at Universitat Autònoma de Barcelona. Published in TDX [Tesis Doctorals en Xarxa]. http://www.tdx.cat/TDX-1203107-162239 [last entry: 25th February 2010].

[7] ARTIGAS, Francesc (1797), *Observación sobre un cálculo en la vejiga urinaria curado por el ácido carbónico, 16 de noviembre de 1797*, Ms. 45, bundle 6, UB.

[8] BOVER, Domènec (1800), *Disertación sobre el uso de los globos aerostáticos aplicados a la medicina, 8 de Mayo de 1800*, Ms. 78, Bundle 9, University of Barcelona.

[9] NOLLET, Jean-Antoine (1746), *Essai sur l'electricité des corps,* París, Freres Guerin. Translated into Spanish in 1747 by Joseph Vázquez y Morales.

Hunter using torpedo fish led to think that electricity could be innate in living beings. In 1791, Galvani, professor of Obstetrics in the Italian university of Bologna, described how a frog muscle prepared for dissection was able to experience contractions when the crural nerve of this amphibious was connected to the metallic arc[10]. The new observed phenomenon was denominated "galvanism", "galvanic fluid" being a substance of the same nature as the "nervous fluid".

Both Galvani and Volta observing the same phenomena –the muscular contraction in frogs prepared for their dissection– reached different conclusions. It's the so-called Galvani-Volta controversy and the matter of the dissertation "On the muscular fibres" presented at the College in 1806. Galvani, physician and physiologist, thought that there was some kind of animal electricity inherent to the organization of living bodies and responsible for muscular contraction. The lack of an electrometer able to measure the minimal amount of electricity involved in this physiological phenomena made its demonstration difficult. From 1800 to 1805 Giovanni Aldini reproducing Galvani's experiments was able to produce electricity signals in animals without the aid of metals, which corroborated the hypothesis that living bodies could be considered as true batteries able to produce and to accumulate electricity. However, the physicist Alessandro Volta, thanks to an apparatus developed by himself, the so called battery of Volta, was able to induce muscular contractions experimentally. This fact reaffirmed the possible external origin of the phenomenon.

The expectation opened by the nature of electricity and its application to the healing art was present at the Royal College of Surgery of Barcelona according to the dissertations read in its meetings. At least I would like to emphasize two of them: "On Galvanism" read in public session in 1802[11] and "On the muscular fibres" read in 1806[12]. "On Galvanism" was written as an extensive review about galvanism where the novelties of electricity were expounded to the audience reviewing the main characters that had made its development possible (mainly Galvani, Perkins and Volta). "On galvanism" presented as a dissertation at the Royal College of Surgery of Barcelona explained the development of "metallic tractors" developed by Perkins and his son in 1798. They used electrified metallic needles to relieve several kinds of health disorders "drawing off the noxious electrical fluid that lay at the root of suffering". After the evaluation of Galvani's experiments, in the dissertation "On galvanism" the coexistence of two different types of death is proposed: the animal death in relation to the chemical principles that conforms the living bodies, and the death of the vitality related with the vital principles that animate living beings. In this context, electricity was intimately associated with vitality. Living beings had been used to identify the effects of electricity on animal economy. For that purpose and in spite of the ethical and political problems, the use of corpses of people condemned to capital punishment has been documented. These debates could constitute a good source of inspiration for Mary Shelley, author of the famous *Frankenstein* novel (1818)[13]. Alfred Smee (1818-1877) surgeon of the St. Bartholomew Hospital of London defended the theory according to which the bodies themselves could regulate the intrinsic electricity they had. For this statement he was the object of a lot of criticism. The Galvani–Volta controversy was exposed in the public session "On the muscular fibres" at the Royal College of Surgery of Barcelona, and that dissertation displayed in detail the structure of the battery of Volta applied to treatments. The reception of the theories of Galvani by the professors of the College was divided. Some of them manifested to be more in agreement with Volta's postulates; others with Galvani, others with the chemical changes proposed by Alexander Humboldt, and others expressed their scepticism.

Antoni Cibat Arnautó introduced experimental physics

In order to demonstrate the great importance of the literary meetings "Juntas Literarias" celebrated at the College I would like to show the statement of professor Antoni Cibat Arnautó (1770 -1812), in the preface of his work *Elements of Experimental Physics* (1804, vol. 1)[14]. According to him he had started his research on the effects of electricity in living beings and by extension on human beings at the Royall School of Surgery of Barcelona some years before. In his treatise, Cibat devoted more than 150 pages to electricity. In the afterwards of the book Cibat has a special mention to the scientific sessions performed at the college of Barcelona as crucial activity that configured his knowledge about experimental physics in general and about electricity in particular. At least we can find ten manuscripts related with that subject. These documents contains both some practical aspects related to electricity applied to healing art and some theoretical parts contextualizing this new and controversial corpus of knowledge.

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[10] GALVANI, Luigi (1791), *De viribus electricitatis in motu musculari commentarius*, a De Bononiensi Scientarum et Artium Instituto atque Academia Comentarii, vol. VII, Bononiae, Ex Typographia Instituti Scientiarum. There is a digital edition by Marco Bresadola, P. Fezzi.

[11] BOVER, Domènec (1802), *Discurso sobre el galbanismo,* Ms. 99, Bundle 11. University of Barcelona.

[12] BOVER, Domènec (1806), *Sobre la fibra muscular.* Ms. 136, Bundle 15, University of Barcelona.

[13] MORUS, Iwan Rhys (1998), *Frankenstein's Children. Electricity, Exhibition, and Experiment in Early-Nineteenth-Century London,* New Jersey, Princenton University Press. SELIGARDI, Raffaella (2001), "What is the electricity? Some chemical answers, 1770-1815": 181-208 In: BERTUCCI, P.; PANCALDI, G., (eds.) (2001), *Electric Bodies. Episodes in the history of medical electricity*, Bolonya, Università di Bologna.

[14] CIBAT, Antoni (1814-1815), *Elementos de Física Experimental*, Barcelona, Antonio Brusi. 2 vols.

However, Cibat and his colleges had tried to reproduce the crucial experimental works performed by Galvani, Volta, Aldini, Humboldt and others in his laboratory, he explains in his textbook. From 1798 to 1792, Cibat studied surgery at the college of Barcelona. After that, he went to Scotland and he obtained his PhD at the Marischal College in Aberdeen[15] and later on he came back to the Royal College of Barcelona again and was professor of Experimental Physics from 1797 to 1807. At least two dissertations in 1798 and in 1799 about the utility of electricity in the healing art had presented by professor Cibat in *junta literaria*. Despite the fact that Cibat was a defender of Galvani's postulates, he specifies in *Elements of Experimental Physics* the difficulties with the achievement of the results obtained by his predecessors. Cibat and his counterparts tried to reproduce the same experiences, trying to verifying his works at the laboratory, unsuccessfully.

Instrumental uses of electricity and devices applied to healing arts

At this moment, following the works mentioned before some illnesses were considered caused by some electricity dysfunction in the human body. For that reason professor Cibat devoted his research to the study of this subject. In his opinion, some kind of illness convulsions and trismus were caused by an imbalance of electricity in the body[16]. Trismus is defined as a tonic contraction of the chewing muscles, moreover in the past this word was often used to describe the effects of tetanus. Whenever electricity was in excess in the jaw a trismus could appear as a consequence of an imbalance of electricity in the body. More recently, this term has been used to describe any restriction of the mouth opening, including restrictions caused by trauma, surgery or radiation. On the other hand, a convulsion is a medical condition where body muscles contract and relax rapidly and repeatedly, resulting in an uncontrolled shaking of the body. The manuscripts of the sessions contain a lot of information about the therapeutic uses of electricity. I would like to stress the instrumental use of this knowledge applied to healing art. One episode is described in detail by Cibat: some electric discharges were applied to a patient thanks to an electrostatic machine near the sea. Moreover, in his dissertations he describes several procedures of electrification applied to several parts of the body depending on the illness that he wanted to treat. Cibat in his *Elements of Experimentals Physics* explained when electricity should be applied, extracting or adding this substance according to the disease and depending on the illness and if it was in excess or not. He considered this kind of procedures as a suitable alternative to other treatments. In general, therapies using electricity aim to counteract the unhealthy problem related to electric imbalance in the human body.

Electrostatic machines became part of the healing arts when Van Martinus Marum said: "the history of electrical science teaches us that progress in this science has been made in step with the use of ever larger electrical instruments giving a more powerful electrical force". Some machines described by Pieter van Musschenbroek in his book *Essai de Physique* (1751), were reproduced by Spanish artisans directed by Jaume Bonells, physician and deputy director of the Conferencia Físico Matemática Experimental, predecessor institution of the Royal Academy of Natural Sciences and Arts of Barcelona, founded in 1770. In one of his dissertations, Cibat states that Antoni Gimbernat had contracted artisan members of the above mentioned institution in order to build specific instruments for the Royal College of Surgery of Barcelona. Juan González Figueras (1731-1807) was the most famous artisan not only in Catalonia but in Spain, and his well-known skills were spread in the Spanish generalist newspapers. Several dissertations presented at the College contain technical information about description and maintenance of this kind of devices, always in the context of the healing arts.

In summary, in the middle of the 18th century all the elements required for the emergence of a new discipline, Experimental physics, was present at the Royal College of Surgery of Barcelona. In Spain, as well as other developed countries, for instance Italy, France, England or Scotland; theoretical facts and practical uses were taken into account. Expectation opened by the nature of electricity and its application to the healing art was present according to the dissertations read in the meetings performed in the College.

To conclude

Taking the Royal College of Surgery of Barcelona (1760-1843) as a case study, after I introduced the purposes and the aims for founding this kind of institution in our country I made special mention of one relevant activity of communication of science held in these institutions, the scientific sessions called "juntas literarias". As I tried to demonstrate, among others documents available, through this manuscripts –the "juntas literarias"– we have noticed the development of this new discipline in Spain: the emergence of Experimental Physics and its reception always in accordance with international

[15] *List of Diplomas M.D. granted by the Marischal College from January 1792 to January 1793*. M397/5/8. Special Libraries and Archives King's College. Aberdeen University (Scotland).

[16] CIBAT i ARNAUTÓ, Antoni (1798), *Memoria o sea ensayo el trismus traumático, 12 de julio de 1798*. Ms. 55, Bundle 7, University of Barcelona; (1799), *Solución a las Objeciones que se hicieron a la memoria o ensayo sobre el trismus traumático que leyó en este Sabio Congreso en el año próximo pasado de 1798 y experimentos que corroboran el modo con que discurrí en aquella*. Ms. 67, Bundle 8, University of Barcelona. [12th July 1799].

development of this new discipline at the end of the 18th century. The experimental physics application to healing arts was associated with the expertise, managing and maintenance of electric machines and devices. Despite the controversies and theoretical debates aroused by electricity, its use for therapeutic purposes has been shown. Its development required both the knowledge for its construction and maintenance of new instruments as an essential condition for its application to the healing art. The emergence of experimental physics was in parallel with the development of the new chemistry of Lavoisier. Since chemistry and experimental physics were included in the academic programs of the Spanish colleges of surgery this kind of knowledge was considered a crucial tool to debate and to understand the unexplained processes that happened in the human body in health as well as in illness. This subject of debate occurs just when the legitimization of surgery in the healing art is being consolidated. In this setting, the new concepts of chemistry and experimental physics constitute instrumental knowledge for understanding the observed phenomena which happened in healthy human bodies as well as in illness condition, with respect to anatomy, physiology, pathology and therapeutics.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 986.

CIRCULATION AS TRANSLATION OF BOOKS: THE CASE OF ARDINGHELLI IN 18TH-CENTURY NAPLES

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Abstract

In 1727 Stephen Hales published the seminal work Vegetable Staticks *which initiated a new idea of the "air": no longer a mere physical instrument, but rather a chemical capable of binding. Most of Italian scholars probably learned that by the first and only Italian translation published in Naples in 1756.*
In fact, in 18th-century Naples there was a brilliant young woman, whose name was Mariangela Ardinghelli (1728-1825), who had already dedicated herself successfully to translating Hales since 1750, when she was only 22 years old. Indeed, she had even published the Italian text of Haemastatics *(1733), becoming somewhat famous amongst scholars and we know that all foreigners in the Kingdom of Naples wanted to meet her. She was in contact with numerous academies and in her salon lively conversations, of Newtonian inspiration, took place.*
So Ardinghelli had a central role in the circulation of many scientific books in Italy through her translations; with her notes added to the text she also fit the theory to her local context converting, e. g., all English weights and measurements into their Neapolitan equivalents.
Ardinghelli is also linked, in the history of the circulation of ideas and techniques, to the 18th century fashion for electricity: she was the addressee of the Abbé *Nollet's first* Lettres sur l'electricité *(1753) and for the whole life she informed him about scientific news from Naples.*
This paper reveals Ardinghelli's peculiarities and her role in circulation, reception and appropriation of scientific theories in the Kingdom of Naples (i.e. Southern Italy), through an analysis of her writings and of those (both coeval and later) who wrote on her.

In 1727 Stephen Hales[1] (1677-1761), the famous English physiologist, published the seminal work *Vegetable Staticks*[2] which initiated a new idea of the "air": no longer a mere physical instrument, but rather a chemical capable of binding. Most of Italian scholars probably learned that by the first and only Italian translation published in Naples in 1756.

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[1] GUERLAC Henry, *The continental reputation of Stephen Hales*, in «Archives internationales d'histoire des sciences», n. 15 (1951), pp. 393-404.

In fact in 18th-century Naples there was a brilliant young woman, whose name was Mariangela Ardinghelli (1728-1825), who had already dedicated herself successfully to translating Hales since 1750, when she was only twenty two years old. Indeed, she had even published the Italian text of *Haemastatics*[3] (1733), becoming somewhat famous amongst scholars, and we know that all foreigners in the Kingdom of Naples want to meet her.[4]

Ardinghelli is one of the women-scientists whose fame has reached us just because they devoted themselves to the translation of scientific works; in this way we also have the opportunity to rediscover her role in the European debate about electricity.

In 1749 she met the famous Jean Antoine Nollet[5] who provided Ardinghelli with her first ‘professional’ contact with the European scientists such as Darthenus, Jean Baptiste Le Roy (1720-1800) and Jean Jacques d'Ortous de Mairan (1678-1771), with whom she later exchanged letters. Nollet himself relates in his travel journal that during one of their first meetings Mariangela gave him a couple of geometrical problems to be solved by Alexis Claude Clairaut (1713-1765).

In fact, her correspondence with S. Hales, who we can reasonably call “Her Author”, started after the publication of her *Haemastatics* translation in 1750, when he sent her all his works in the hope she would translate them.[6]

According to Ardinghelli's nephew, when they met, Nollet was the Dauphin's tutor. Along with the Marquis de l'Hôpital[7], to whom Mrs. Ardinghelli dedicated her *Haemastatics* translation, he tried to get her to come to Paris as the tutor to the King's daughters, but her mother kept her in Naples.[8]

In 1753 Nollet addressed her the first of his famous *Lettres sur l'electricité*[9]. At least seventy letters of their private correspondence have been lost. In several *memoires* of the *Academie des Sciences* Nollet mentions news and words written by his Neapolitan correspondent, such as when he was informed by her about curious medical cases examined by Doctor Carlo Curzio, which Nollet had translated and published in France[10].

At this point, it is finally possible to say something about her famous translation of *Haemastatics or, an account of some hydraulic and hydrostatical experiments made on the blood and blood-vessels of animals. Also an account of some experiments on stones in the kidneys and bladder*. It does not intend here to go deeply into the well-known and, to some extent, already studied contents, since they don't help to clarify the role of the translator activity as a medium for circulating scientific ideas, but it is important to remember that she translated from the French version.

In Mariangela Ardinghelli's opinion, translating means correcting mistakes too, to avoid the danger of transmitting wrong scientific contents or the thinking of the French translator to the Italians readers. So where the French text does not make any sense, she consults the original English to discover that whole lines are missing. François Boissier De Sauvages[11] (1706-1767), translating the same text into French, changes the English units of measurement into French ones, without modifying the correspondent numerical values. She calculates all the equivalences again, and since she realizes most of

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[2] HALES Stephen, *Vegetable staticks, or, an account of some statical experiments on the sap in vegetable: being an essay towards a natural history of vegetation: also, a specimen of an attempt to analyse the air, by a great variety of chymiostatical experiments: which were read at several meetings before the Royal Society*, London, W. and J. Innys & T. Woodward, 1727.

[3] HALES Stephen, *Statical essays: containing haemastaticks, or, an account of some hydraulic and hydrostatical experiments made on the blood and blood vessels of animals: also an account of some experiments on stones in the kidneys and bladder: with an enquiry into the nature these anomalous concretions: to which is added an appendix containing observations and experiments relating to several subjects in the first volume: the greatest part of which were read at several meetings before the Royal Society*, London, W. Innys, R. Manby & T. Woodward, 1733.

[4] The astronomer De Lalande, having met her during his travels around Italy in the years 1765-66, wrote Mrs. Ardinghelli was at the forefront of all honorable women in Italy and Europe, distinguishing herself not for her graces or her class as all the other women, but thanks to her knowledge of Physics and Mathematics. DE LALANDE Joseph Jérôme, *Voyage d'un français en Italie, fait dans les années 1765 & 1766. Contenant l'Histoire & les Anecdotes les plus singulières de l'Italie. Nouvelle Edition corrigée & considérablement augmentée*, Yverdon 1769-1770, t. 6, p. 237.

[5] TORLAIS Jean, *L'abbé Nollet (1700-1770)*, Paris, Sipuco, 1955.

[6] LOMBARDI Antonio, *Storia della letteratura italiana nel secolo XVIII*, Modena 1832, p. 72.

[7] Paul Françoise Gallucci de L'Hopital (1697-1776) marquis of Chateaneuf, field marshal, arrived in Naples the 7th of July 1740 as new ambassador of France and he remained there until 1750 when he went to Petersburg. In TANUCCI Bernardo, *Epistolario (1723-1746)*, COPPINI R. P., DEL BIANCO L., NIERI R. (eds.), Roma, Edizioni di storia e letteratura, 1980, vol. I, p. 394; 483.

[8] VITRIOLI Diego, *Elogio diAngela Ardinghelli, in Opere scelte di Diego Vitrioli*, Reggio Calabria 1930, vol. I, p. 280.

[9] NOLLET Jean Antoine, *Lettres sur l'électricité: dans lesquelles on examine les dernieres découvertes qui ont été faites sur cette matière, & les conséquences que l'on en peut tirer*, Paris, H.-L. GUERIN & L.-F. DELATOUR, 1753, Lettres II-VII.

[10] CURZIO Carlo, *Dissertation anatomique et pratique sur une maladie de la peau d'une espece fort rare et fort singulièr*, Paris, chez Vincent, rue S. Severin, à l'Ange, 1755, Translated by Charles Augustin VANDERMONDE.

[11] FRENCH Roger Kenneth, ‘Sauvages, Whytt and the motion of the heart: aspects of eighteenth century animism', *Clio medica*, vol. 7 (1972), pp. 35-54.

them are wrong, she feels free to correct them. Sometimes she says she has paraphrased instead of translating, because the concepts are too difficult and she thinks they may be misprints. Moreover, she informs the readers that she has added a few notes in italics for her own explanations.

> Translating Mr. de Sauvages's notes, I tried to clarify them wherever possible; but I wasn't always able to do that, because this Editor sometimes is not very clear. He says few things that belong to the text; his main purpose is to restore the ancient system, according to which spontaneous motions of the body derive from the Soul.[12]

In the second volume, published two years later, where she translates some of Hales' writings on kidney stones there seems to be a willingness on her part to communicate a social message, in this case as a translation of a scientific work. As Ardinghelli explicitly says, her aim is to enable a wider public to benefit from the medication for kidney stones and «in order to make it easier for those who would like to use this medication, I have converted all English weights and measurements into their Neapolitan equivalents.»[13]

On page 39 (volume 2) of her translation, she personally examines Neapolitan water sediments as Hales have done for the water sediments around London. She reports with a degree of frankness to have used a candle kept in a kind of small stove and accurately describes it. As far as she knows, no one has done this before, so she decides to do this just because it would have been pointless for her readers to know only about the far away city of London.

Then in 1756 Ardinghelli published *Vegetable staticks and analyse of air*, and although only twenty years later, in the 1776 edition, her name appeared on the title page. This time she translates directly from English and, when in doubt, she looks at Buffon's French translation. In this second edition she does all the calculations again. She repeats the experiments, reports Newton's Latin passages, and changes into 'Neapolitan' all the words for animals and plants so that everyone can recognize them.

It is interesting to note her intuition, which is the base of Hales' importance in modern chemistry, on p. 246 note (m)

Air did not only have a physical but also a chemical property; no one had thought about air before; why, she asks, has no one noted that air combine with other chemicals?

> It is sure that many useful discoveries come from physicians in natural history, if they go on working in these so smart new Hales' experiments on "air". But no one I find, who has tried to repeat or to study on them until today, neither to reflect on the general system, erects on them.[14]

This is exactly what Antoine Laurent Lavoisier (1743-1794) would do some years later, and what pneumatic chemistry had done before him, which was to bring on the famous Chemical Revolution. However, if we reflect for a minute on the fact that Lavoisier didn't understand English and came to know Hales through Buffon's translation into French, then Ardinghelli's work looks even more significant to our eyes.

Ardinghelli's only real scientific acknowledgment came from the 'Accademia dei Fisiocritici' of Siena[15] (Tuscany). In 1759, in line with the new 18th-century spirit, the Academy accepted a woman, the *celebrated philosopher* Mrs. Ardinghelli, who soon sent a letter to express her gratitude (July 2, 1759)[16].

It is unlikely that Ardinghelli worked on anything for the Academy of Siena. In the Academy dissertation indexes I've consulted there is nothing about or by her. But thanks to this admission, Mariangela Ardinghelli could finally put a title to her long and valued scientific activity in her latter publications, poetical compositions for public events.

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[12] "Nel tradurre le note del Signor de Sauvage, ove ho potuto, ho cercato ancora di rischiararle; ma non sempre mi è riuscito, perché questo Comentatore mostra talvolta di non farsi intendere. Egli poche cose dice, che appartengano al testo; ma il suo principale istituto è di ristabilire l'antico sistema, che i moti spontanei del corpo dipendono immediatamente dall'Anima." HALES Stephen, *Emastatica, o sia Statica degli animali: esperienze idrauliche fatte sugli animali viventi dal signor Hales... tradotta dall'inglese nel franzese, e commentata dal signor De Sauvages... e dal franzese nuovamente trasportata nell'italiano idioma*, Napoli, presso Giuseppe Raimondi, 1750-1752, vol. 1, *A chi legge*, without page number.

[13] Ivi, vol. 2.

[14] "Egli è certo, che molte utilissime scoperte potrebbero fare i Fisici nella Scienza Naturale, se colla scorta di questa ingegnosissime nuove Sperienze del Signor Hales travagliassero intorno all'aria. Pure nessuno io ritrovo, che siasi sinora applicato né a ripeterle, né ad esaminarle, né a riflettere al general sistema, ch'egli sopra vi fonda." HALES Stephen, *Statica de' vegetabili, ed analisi dell'aria... tradotta dall'inglese con varie annotazioni*, Napoli, nella stamperia di Giuseppe Raimondi, 1756.

[15] The *Accademia dei Fisiocritici* was founded in Siena in 1691 by Pirro Maria Gabrielli (1643-1705) its venue was the Hospital "Santa Maria della Scala".

[16] Archives of the Accademia dei Fisiocritici, Correspondence, 1759, 156 [12] in TROMBETTA Vincenzo, *Storia e cultura delle biblioteche napoletane*, Napoli, Vivarium, 2002, p. 89, n. 42.

De Lalande claimed that she had also written some works about physics, which were not published because of her modesty, and that she started to translate De Buffon's *Histoire naturelle*. These are words we can trust, since the French astronomer had a long correspondence with her, which is also proved by the mentions she makes of him in the few letters still available.

Another manuscript attributed to Ardinghelli is now in a miscellaneous volume in the library in Palermo of the Società siciliana di Storia Patria[17]. The title is *Dissertazione del Sig.r D.n Giuseppe Sembrano sopra il Trattato del Movimento degli Animali, nell'opera del Sig.r Gio' Borelli*[18]. We cannot be certain Ardinghelli wrote this or not, but her notes in *Haemastatics* show familiarity with Borrelli's work.[19]

We have to admit that Ardinghelli's choice is translating Hales' works is connected to her intent of arousing greater interest around Newtonian philosophy, and that translation allowed Mariangela Ardinghelli to show her skills as a mathematician and experimental scientist to a wider audience than that of the Spinelli Library, which was the place where she had her scientific education. It was a Public Library which was owned by Prince Ferdinando Vincenzo Spinelli of Tarsia (1685-1753). On occasion of its opening, Mariangela, who was seventeen at the time, (July 22, 1747) is said to have given a highly praised speech in Latin on 'electrical force'. De Lalande described the sumptuous Spinelli Library as unique, due to the great number of books and to its scientific apparatus, even better than those owned by the University, but the French traveler adds, that even if the library was open three days a week, both in the morning and in the afternoon, its librarian was often alone: maybe Ardinghelli was alone with Hales' books too.[20]

So it's undeniable that translation and study of languages were for her the only means she had of living as a scientist beyond the Kingdom's borders, outliving herself, and above all the activity of translation was her best means to circulate new scientific ideas and to validate her Newtonian belief among Italian scholars.

After the effort of calculation, translation and experimentation Mariangela Ardinghelli used to conclude her forewords with these beautiful words:

> Take pleasure now in my work, if you find it worthy of your pleasure. And live happy.[21]

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[17] Sicilian Society Library of Palermo National History.

[18] *Mr. Giuseppe Sembrano's dissertation on Treatise about Animal Movements in Mr. Giò Borrelli's work*.

[19] BRIGAGLIA Aldo & NASTASI Pietro, 'Bologna e il Regno delle Due Sicilie: aspetti di un dialogo scientifico, 1730-1760', *Giornale critico di filosofia italiana*, a. 63 (65), ser. VI, vol. 4, fasc. 1 (1984), pp. 145-178.

[20] DE LALANDE Joseph Jérôme, *Voyage d'un françois en Italie*, Yverdon, 1769, t. 6, pp. 96-97.

[21] "Gradisci intanto questa mia fatica, se degna ti pare del tuo gradimento. E vivi felice."

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CORRECTING LAZZARO SPALLANZANI. ANTONI MARTÍ I FRANQUÈS' CONTRIBUTION TO THE KNOWLEDGE OF SEXUAL REPRODUCTION OF PLANTS (18TH CENTURY)

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Abstract

In Expériences pour servir l'histoire de la génération des animaux et des plantes *(1786), Lazzaro Spallanzani (1729-1799) concluded that some plants, including hemp, could reproduce without fertilisation. Antoni Martí I Franquès (1750-1832), a Catalan naturalist, repeated Spallanzani's experiments with hemp, concluding that the Italian scientist was wrong. According to Martí, Spallanzani had not been sufficiently rigorous and careful in his testing. He did not realise that there were male or hermaphrodite flowers present in the female plants. Martí found that the intervention of both sexes was needed for successful reproduction in hemp, as in all superior plants. At that time, experiments on plant sexuality were crucial to make a definitive determination among the three alternatives surrounding the mechanism of generation: the ovist theory (pre-existence of germs in the egg), the animalculist theory (pre-existence of germ sperm) or epigenesis (meeting-with-fertilisation of two principles contained respectively in the egg and the sperm). In this paper, we shall outline Martí's method and explain how he reached his conclusions.*

The sexual theory of plants. The debate during the 18th century

The debate on plant sexuality is a long, controversial episode in the history of science. From the ancient world until its proof well into the 19th century, theories in favour of plant sexuality were put forth, certified by experiments that seemed definitive but that were soon rejected by other experiments which also purported to be definitive. Indeed, plant morphology itself did not do much to help resolve the issue. We should bear in mind that in the majority of plant species, the male and female organs are together not only in a single stem but also in a single flower, thus making it more difficult to clarify any kind of sexuality based on two genders. Until the exact meaning of stamens and pistils was recognised, it was very difficult to properly interpret a state of hermaphroditism, which can only be noted exceptionally in animals. And this was coupled with many botanists' persistent predisposition to deny sexuality in plants. Along these lines, we can find an early argument in the Aristotelian tradition which encompassed scientific activity throughout many centuries that denied the existence of sexuality in plants. Furthermore, the majority of botanists' preference until well into the 19th century was for systematic studies, relegating physiological questions to secondary status. Indeed, in the midst of the 18th century Linnaeus himself, a prince among botanists, distinguished "true botanists" that is, those who worked on the classification, systematics and description of the different plant species, from "botanophiles", amateurs who were only concerned with secondary, accessory issues, such as plant physiology, fertility and sexuality.[1]

Even though plant sexuality had been guessed at since the ancient world, no experimental confirmation was available until the late 17th century. In 1694, Rudolph Camerarius (1665-1721), a Botany professor at Tubingen, wrote *De sexu plantarum epistola* in which he set forth the list of experience which led him to conclude that stamens are male sexual organs while the style and ovary are the plant's female organs. He also noted that there could be no production of fertile seeds unless both sexes took part in the fertilisation process.[2] However, the fact that this process of fertilisation was not wholly clarified and that some of Camerarius' experiments yielded results that seemed contradictory meant that the sexual theory never gained ground and the controversy instead endured throughout the entire 18th century. Even though a respected authority like Linnaeus acknowledged the existence of sexes in plants and founded his system on observations of stamens and pistils, the detractors from the sexual theory argued strongly against it, reaching their maximum success in the opinions of Lazzaro Spallanzani (1729-1799), who used an implacable battery of experiments which cast doubt on the sexual theory in the late 18th century.[3]

Even though Spallanzani had noted that in a vast number of cases, when isolated from the males of their own species, female plants blossomed without producing fertile seeds, he also observed some cases that contradicted this rule. His first results came from hemp (*Cannabis sativa*). Spallanzani cultivated two female stems of this plant isolated in a closed chamber in order to prevent any contact with pollen that might come from a male plant. To further secure this isolation, he even placed several blossoming branches from these female stems, with their flowers still closed buds, in bottles with the necks sealed with plant resin. Despite these conditions of extreme isolation which made contact with pollen impossible, the majority of the seeds produced were fertile. He conducted experiments with similar results using spinach (*Spinacia oleracea*) and mercury (*Mercurialis annua*). In order to reject any objection due to the possible arrival of pollen through the wind or insects, or the fact that the stamens that are sometimes found in female flowers were ignored, Spallanzani chose the watermelon (*Citrullus lanatus*) as his new subject of experimentation, whose flowers are large enough to prevent this kind of error. After eliminating the male flowers, he still obtained fertile seeds. To demonstrate that there had been no contamination from external pollen, he cultivated watermelons in a greenhouse in the middle of winter, when no male flower could exist in this season outside the greenhouse. In this case, too, the experiment yielded fertile seeds. With these results, Spallanzani, who was becoming one of the most reputed experimenters at that time, sowed serious doubt as to plant sexuality among the natural philosophers of his day. At the same time, this doubt opened the doorway for new studies and experiments to prove the certainty of the Italian scholar's results. The curious spirit of our main character, Antoni Martí i Franquès, set out to cross the threshold of this door with his own experiments. However, before embarking on a description and analysis of Martí's experiments, let us learn more about this naturalist.

Antoni Martí i Franquès: Brief biographical sketch

Antoni Martí i Franquès was born in Altafulla on the 14th of June 1750. He was the son of Antoni de Martí Gatell, a landowner and businessman, and Maria Franqués Gatell, who was also from a wealthy landowning family.[4] After his primary school, guided by tutors who went to his house, at the age of 14 he entered the University of Cervera. This university experience was apparently not fully satisfactory, as we have proof of no degree that would enable us to verify his completion of any kind of programme at this educational institution. Therefore, we must choose to think that the vast knowledge that Martí demonstrated having was achieved through his own means. Apart from a deep-seated knowledge of Latin and Greek, he was also quite familiar with French, German, English and Italian, which over the course of his life would serve him as instruments for acquiring new knowledge from foreign publications and for interacting with scholars from other European countries. From a very young age, he revealed an inclination to study the natural sciences, especially botany and chemistry. He thus applied to join the Royal Academy of Sciences and Arts of Barcelona, a dream which came true in 1786. Shortly thereafter, in 1790, he was also admitted to the Royal Academy of Medicine of Barcelona as a free member. A businessman, he administered his family wealth, which encompassed vast tracts of vineyards and hemp fields, as well as numerous investments in textile factories and other commercial enterprises. He participated in the flurry of activities aimed at spurring the economy, such as the 1787 creation of the Economic Society of Friends of the County of Tarragona, the project to build a motorway between Tarragona and Lleida and the Drawing and Sailing School of Tarragona. Committed to the historical

[1] For more on this debate, see: Magnin-Gonze, 2004; Delaporte, 1982; Morton, 1981; and Ritterbush, 1964.

[2] A good description of Camerarius' experiments can be found in the classic Sachs, 1890.

[3] You can track Spallanzani's experiments on plant reproduction in Spallanzani, 1784.

[4] For the biographical details of Antoni Martí i Franquès, see: Quintana, 1935 and Rovira, 1982.

events of his day, he economically contributed to defray the expenses caused by the military mobilisations during the Great War against the French Convention and participated actively in the patriotic movements and institutions during the French War. In fact, he was injured in a leg in this latter conflict. Antoni Martí i Franquès died in Altafulla on the 20th of August 1832.

Martí always kept abreast of everything that happened in the world of science in his day. With this purpose in mind, he amassed a fantastic library containing the most recent works in physics, chemistry and botany. His library also contained the leading works from classical literature as well as a number of treatises on agriculture, philology, geography and other disciplines (Duch, 1998), a veritable corpus of knowledge that reflects Martí's wide range of intellectual interests. He was also an avid reader of the top scientific journals circulating throughout Europe at that time. He regularly read journals like *Annales de Chimie, Annales de Chimie et de Physique, Journal de Physique, Archives des decouvertes, Journal des nouvelles étrangères et politiques, Le Publiciste, Magasin Encyclopédique de Nivose* and the science bulletin of the *Société Phylomatique* (Quintana, 1935). The friendships that Martí had cultivated with Parisian booksellers such as Girard, Freuttel and Fuchs, as well as others from Perpignan, proved to be highly instrumental to him in keeping abreast of the latest developments in the scientific literature (Quintana, 1935).

However, Martí was not contented to only examine the theoretical aspects of science, as many of his contemporary intellectuals and even some professors of the scientific disciplines were. Instead, he wanted firsthand proof of what he read in books and journals. To this end, he mobilised into action, making observations and experiments to check the results reported in the scientific stories published in the journals. This practice led him to ask new questions that could only be answered by new experiments. Martí thus became a modern scientist, with experimentation as the foundation of his working method. He conducted his experiments in his houses in Tarragona or Altafulla, and his venue was a laboratory equipped with the instruments needed to rise to the challenges posed by experimentally verifying the results. This experimental proof was what led him to detect anomalies in certain results published by European scientists who set forth their theories. Martí was not above checking what was being claimed in the scientific debates that were the subject of controversy Europe-wide. Two examples of Martí's desire to participate in the scientific discussion of his day can be found in the debate on the composition of the air in the atmosphere[5] and the mechanism of plant sexuality. The latter subject is the focal point of this study, which we shall now analyse in detail.

Antoni Martí i Franquès' experiments

Indeed Martí, who had read the study in which Spallanzani explained his experiments, and who was totally cognizant of the European debate on the sexual theory of plants (Quintana, 1935), designed and conducted an entire series of tests with hemp and other plants to check whether the Italian cleric's results were valid or erroneous.

He reported on these experiments and their results in the report he submitted to the Royal Academy of Medicine of Barcelona in 1790 (Martí, 1790). According to Martí himself, the reasons inspiring him to embark on this experimental endeavour were first the importance of the issue itself, secondly its possible application to agriculture and thirdly and most importantly the possibility of proving or disproving Linnaeus' ideas on plant sexuality. In this latter sense, throughout the entire report Martí came out strongly in favour of the Swedish botanist, often citing his work on plant mating. Linnaeus' experiment with hemp, whose results contradicted those of Spallanzani, were precisely what picked Martí's curiosity, spurring him to check for himself which natural philosopher was right. The easy availability of the hemp plant on Martí's own estates even further predisposed him to take a step towards securing his own proof.

Martí's series of experiments began simply by sowing hemp seeds in two pots. When the plants emerged and began to blossom, Martí eliminated all of the male stems and the stamens that appeared in some female flowers. By doing this, he aimed to ensure the absence of male elements in the stems. The first round of flowers wilted without being fertilised. However, the second round, which had dovetailed with the natural blossoming of the hemp fields around Martí's home, ended their cycle with the majority of specimens fertilised. According to Martí, the first flowers did not have the chance to be fertilised because there was no pollen anywhere. The second ones did because the wind carried this pollen from the nearby hemp fields, only 55 feet steps away, to the place where the experimental plants were located. To these results he added his own observation in the hemp fields, where when the male specimens were lacking, the female specimens remained unfertilised. To Martí, this fact was striking proof of the need for pollen in order for the hemp plant to be fertilised.

To reinforce this idea and discard the influence of environmental circumstances in the process of fertilisation, Martí came up with a new test which consisted of cultivating hemp plants in pots placed under different conditions. He placed a pot with male and female specimens in a window facing north and another pot with only female specimens in a window facing south. The plants were thickly sown in the first pot, while there were only four specimens in the second pot. It was understood that the northern exposure thick with plants would entail less favourable conditions than those in the second pot, which faced south and left room for all the specimens sown. The experiment began in late September, when the hemp plants

[5] On Martí's participation in the debate on the composition of the air in the atmosphere, see: Quintana, 1935 and Grapí, 2001.

on the estate and nearby plants had been harvested; therefore, the only pollen influence could come from the stamens of the male specimens in the pot facing north. With all of these conditions, the plants in the pot facing south, isolated and away from the pollen in the north-facing pot, yielded no fertile seeds, while the specimens in the north-facing pot did. Martí deduced that better or worse cultivation conditions did not matter as much; rather that what really made hemp plants fructify was the presence of male specimens. He confirmed this idea by artificially forcing fertilisation. With the pollen removed from the male stems in the north-facing pot, he inoculated the flowers in another series of female plants, also facing north and got almost all of them to yield fertile seeds.

Given these unanimous results in favour of the sexual theory, Martí provided several explanations interpreting Spallanzani's results. He recalled that in his experiments, Camerarius had obtained fertile hemp seeds in isolated female specimens, a phenomenon that he attributed to the arrival of pollen from nearby hemp plants carried by the wind, or to the presence of male flowers in these same stems. However, Spallanzani had taken the precaution to confine the female stems in glass containers, which were impossible for any male influence to penetrate. Martí, then, could only explain the fertilisation of the female flowers subjected to this confinement by the inadvertent existence of male flowers. He noted that as the result of constant, methodical observation, he had discovered male and hermaphroditic flowers in the female stems. He claimed that this phenomenon might have escaped Spallanzani's notice.

This particular hemp flowering had yet another possible source of error. According to Martí, there existed the possibility of fertilisation in very early stages of blossoming. Very young female flowers might be fertilised by also young male flowers. He proved this with an experiment in which he placed a pot with a female stem with incipient flowers near a pot with male stems whose flowers were also just beginning to bud. The female flowers that were exposed to the young males yielded fertile seeds despite the precociousness of their sexual relations. In this sense, Martí cast doubt on one of Spallanzani's own experiments which he claimed yielded fertile seeds from female stems separated from any male flowers, which had in turn been eliminated in a very early stage 18 days earlier. With the help of artificial fertilisation, Martí fertilised female flowers, yielding seeds that he never demonstrated to be fertile before 23 days after conception, and which achieved their maximum fertility one month after they were inoculated with pollen. With this experiment, Martí noted that Spallanzani's fertile flowers must have been fertilised before he eliminated the male flowers in a period longer than 18 days when even though the male flowers had not manifested their sex with all their attributes, incipient yet active pollen had undertaken its reproductive mission.

Martí used the technique of artificial fertilisation to reinforce the sexual theory. He fertilised female flowers chosen from a group subjected to isolation from any male specimen. The fact that these fertilisations yielded fertile seeds and that he observed that the flowers that had not been inoculated remained sterile were yet further arguments questioning Spallanzani's claims, thus reinforcing the idea of the universality of the necessary participation of both sexes in the reproduction of hemp.

Martí also experimented with other plants. He was unable to get an exclusively female specimen in spinach. When he eliminated the male flowers, they soon reappeared again. For this reason, he was unable to demonstrate that a totally female specimen remained infertile when isolated from the male influence. However, he did observe that in the lapse of time between when the male flowers were eliminated and when the new male flowers reappeared, the pistils of the female flowers on the same specimen lengthened further than normal when the female flowers were accompanied by male flowers. He had also observed this phenomenon in hemp, and Martí attributed it to the virginal condition of these flowers, which demonstrated, to his mind, the female flower's reaction to the absence of pollen. Somehow, with these observations Martí noted a natural "need" for complementation between the sexes, yet further corroboration of the sexual law in plants.

Spallanzani's experiments with watermelon, melon (*Cucumis velo*) and pumpkin (*Cucurbita melopepo*), all of them species with large enough flowers to facilitate their identification, confirmed cases in which female specimens totally isolated from the male presence managed to produce fertile seeds. With the goal of checking these results himself, Martí observed the blossoming of these plants, noting that at least in the southern latitudes of Altafulla, all three species contained what appeared to be female, male and hermaphroditic flowers in their stems. He placed special emphasis on the case of watermelon, a species in which, though quite rare, some male flowers could be found in female stems, which cast doubt on the purported total isolation to which Spallanzani claimed to have subjected the female flowers of this species in his experiments.

With this series of experiments and observations, Martí cast doubt on the experimental apparatus that Lazzaro Spallanzani had wielded several years earlier to question the sexual theory of plants. Spallanzani had also taken this questioning to the animal kingdom, relativising or even denying the role of male seminal agents in the process of fertilisation. Always cautious and respectful of Spallanzani, Martí noted the particularities of the blossoming of the plant species used in his experiments. These particularities made it more difficult to identify the flowers and made it necessary to conduct a painstaking, exhaustive exploration of the female stems in order to discard the possibility of any intruding male flowers. Implicitly yet with the solidity of experimental results that disproved the arguments wielded by Spallanzani, Martí was capable of correcting him and pointing to the possible causes of error. These errors lay in the "careless" or at least not very attentive action of a skilful and highly reputed experimenter who had not paid enough attention to detecting male flowers in the specimens which he believed only had female flowers. Without wanting to enter into the debate of the pre-existence of embryos, Martí never stopped questioning this interpretation of the reproductive process of living beings. The results of his experiments confirmed the need for pollen to take part in the reproduction of the plant species that Spallanzani himself had used precisely to evaluate the pre-formation of organisms in the "eggs" of the females. Somehow this was an empirical discrediting of a debate to which Martí supplied arguments which, precisely because they were also empirical, were difficult to discredit. For all of these reasons, we must regard Martí's contributions as retold in his report on the sex of plants as an important milestone in clarifying the mechanisms of reproduction in plants. Despite the fact that these contributions were not widely disseminated among the European scientific community of his day, these contributions were joined by those that strove to reveal the mechanisms governing life using the strictest experimental practice.

References

DELAPORTE, François (1982) *Nature's Second Kingdom. Exploration of Vegetality in Eighteenth Century*, London, The MIT Press.

DUCH PLANA, Montserrat (1998) "Aproximació al coneixement de la biblioteca d'Antoni Martí i Franquès", *Antoni Martí i Franquès: recull d'articles*, Altafulla, Centre d'Estudis d'Altafulla, pp. 9-17.

GRAPÍ, Pere (2001) "Antoni de Martí i Franquès i la química del segle XVIII", *Estudis altafullencs*. Altafulla : Centre d'Estudis d'Altafulla. No. 25, pp. 51-58.

MAGNIN-GONZE, Joëlle (2004) *Histoire de la botanique*, París, Delachaux et Niestlé.

MARTÍ I FRANQUÈS, Antoni (1790) *Experimentos y observaciones sobre los sexos y fecundación de las plantas*, [The transcription of the manuscript can be found in Quintana, 1935].

MORTON, Alan G. (1981). *History of Botanical Science: An Account of the Development of Botany from Ancient Times to the Present Day*. London: Academic Press.

QUINTANA MARTI, Antoni (1935) *Antoni de Martí i Franqués: memòries originals, estudi biogràfic i documental*, Barcelona, Nebots de López Robert i Cia.

RITTERBUSH, Philip C. (1964) *Overtures to Biology. The Speculations of Eighteenth Century Naturalists*, New Havens-London, Yale University Press.

ROVIRA I GÓMEZ, Salvador (1982) *Antoni Martí i Franquès i l'Altafulla del seu temps: 1750-1832*, Altafulla, Centre d'Estudis d'Altafulla.

SACHS, Julius (1890) *History of Botany*, London, Clardon Press [translation from German].

SPALLANZANI, Lazzaro (1784) A dissertation concerning the generation of certain plants," in *Dissertations Relative to the Natural History of Animals and Vegetables*, London, J. Murray, pp. 249-347.

SCIENCE AND TECHNOLOGY IN THE 18TH AND 19TH CENTURIES

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 996.

NEWTON'S QUEST FOR A MATHEMATICAL-DEMONSTRATIVE OPTICS

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Abstract

In this talk, I take up and develop the suggestion made by the late I. Bernard Cohen, Casper Hakfoort and Alan E. Shapiro that Newton's methodological ideal of "deducing causes from phenomena" was not equally attainable in the study of optical phenomena. If this suggestion is correct, then in the apex of Newton's optical researches, The Opticks*, which in fact contained a set of interrelated theories on optical phenomena, Newton failed to rigidly deduce these theories in the sense he had done in the* Principia*.*

By contrasting Newton's methodology in the Principia *with the way in which theoretical conclusions are established in* The Opticks*, I shall be able to explain why Newton was less successful to accommodate optical phenomena according to his own methodological desiderata of deducing causes from phenomena. After having commented briefly upon Newton's methodology in the* Principia*, I will review the kinds of trouble that Newton ran into when trying to methodize optics in a Principia-Style fashion. My focal point will be Newton's arguments for the thesis that white light consists of rays differently 'refrangible'. Special attention will be paid to Newton's presumed application of Rule II of the* regulae philosophandi *in establishing that part of his optical theory. It is shown that Rule II licenses the identification of instances of causes of the same kind which have been shown to be true and sufficient to explain their phenomena. Thus, on the basis of Rule II we identify two instances of causal parameters of the same kind, which have separately been derived from phenomena. The disanalogy involved is thus that in the experimentum crucis (and its related sections in* The Opticks*) we use an argument for uniformity to establish a single causal claim, while Rule II licences the identification of similar causal parameters which were independently established and were deduced from phenomena by systematic dependencies.*

In the Principia *there are systematic dependencies, derived from the laws of motion, between causes and effects. Given the absence of systematic dependencies, Newton could offer only sufficient causes in* The Opticks*. Moreover, by means of what I call 'macro-micro inference tickets' (i.e. Props. LXX-LVI, Book I), Newton was able to license conclusions about the inverse-square centripetal forces of each of the individual micro-particles that constitute a macroscopic body from the overall inverse-square centripetal force exerted by that body –in this way, Newton was thus able to back up transductive inferences about the particles constituting a macroscopic body. In* The Opticks *none of the above was at hand. Newton clearly wanted to do more than to simply establish the phenomenological laws regulating optical phenomena: he also wanted to provide a solid physical account of optical phenomena. However, given the empirical and methodological problems Newton later encountered when methodizing optics in a Principia-style, it turned out that establishing non-hypothetical physical interpretations of optical phenomena was quite a difficult matter.*

It is the aim of this talk to pinpoint the dynamics between method and 'phenomena' in Newton's optical research.

Introduction

My basic premise is that Newton was somehow dissatisfied with his own optical work. Here I shall not further persuade you of the adequacy of this particular premise, as its case has, in my opinion, been made quite successfully elsewhere. In this talk I seek *to explain* why Newton was less successful to accommodate optical phenomena *according to his own methodological desiderata* of deducing causes from phenomena. In order to do so, I shall be contrasting Newton's methodology in the *Principia* with the method by which theoretical and causal conclusions were established in *The Opticks*. *Ab initio*, I wish to emphasize that, when I occasionally use the word "failure," I refer to Newton's inability to accommodate optical phenomena according to his *own* methodological ideals –so nowhere, do I use the word in a derogative sense.

As I shall argue in what follows, *the difference between* The Opticks *and the* Principia *does not lie, respectively, in the absence or presence of causal explanations, but in the different ways in which theoretical and causal statements were established*.

I shall focus on how Newton drew theoretical conclusions from the *experimentum crucis* and contrast this with how he concluded that inverse-square centripetal forces were causing the Keplerian motion of the primary and secondary planets. For presentational reasons, I have decided to focus on the *experimentum crucis*, which is *a symptomatic case* of Newton's "failure" in optics.

Newton's Argument for the Heterogeneity of White Light

Here I shall not dwell on the set-up of the *experimentum crucis*, which is, I suppose, rather well-known. I shall only discuss the conclusions which Newton drew from his *experimentum crucis* and investigate whether these conclusions can be justified by Newton's own methodological desiderata.

Newton's new theory of light and colours contained basically two claims: *one the one hand*, it encompassed the claim that there is a one-on-one correspondence between refrangibility and colour; *on the other hand*, it contained the claim that white light *consists* of a heterogeneous mixture of different colours. The former claim, *i.e. the claim on the one-on-one correspondence*, was based on observation which had shown that the rays emerging from prism *ABC* at the greatest angle of refraction are equally re-refracted by prism *abc*; similarly, rays emerging at the least angle of refraction are equally re-refracted by the second prism. And so on, for all intermediate rays of the spectrum. The latter claim, *i.e. the claim on the heterogeneity of white light*, entails that the colours, which are originally contained in white light before refraction, become visible when separated by a prism. According to Newton, prismatic colours are thus *not created* but *rendered visible by separation*.

Now, given the experiments he had performed, Newton could indeed understandingly claim that to every colour there corresponds a specific degree of refrangibility. However, his claim was based on experiments in two different refracting media only: glass and water, which seems to testify of Newton's belief in the uniformity of nature. Nowadays, we know that there are media in which red colours are more refracted than violet ones (e.g. in dye fuchsine or in iodine), and that the refrangibility of a ray can be modified (e.g. in the Doppler-effect). Nevertheless, given the media which Newton had studied, his claim on the one-on-one correspondence between colour and refrangibility was based on experiments –albeit on a limited range of experiments.

But what about Newton's generalization that white light *consists* of a heterogeneous mixture of different colours? Unfortunately, in his first optical paper, Newton did not dwell much on the specifics of how this generalization follows from the *experimentum crucis*. We do know that Newton was quite convinced of the correctness of his conclusion on the heterogeneity of white light. In a letter to Henry Oldenburg on 6 February 1671/2, Newton pointed out the following: "For what I shall tell concerning them is not a Hypothesis but most rigid consequence, not conjectured by barely inferring 'tis thus because not otherwise or because it satisfies all phænomena (the Philosophers universall Topick,) but evinced by ye mediation of experiments concluding directly & without any suspicion of doubt."[1] In view of several criticisms on his first optical paper, Newton never denounced his thesis on the heterogeneity of light. In response to Huygens' criticism, Newton did, however, restrict the *scope* of the theory. Whereas in the original paper he had made claims on the innateness of white light in general, in a letter to Oldenburg on 23 June 1673 he now restricted his theory to the innateness of the sun's light.

Much later, in a comment on Experiment 9 of Part I of Book I of *The Opticks*, which remained identical in all editions, Newton made the accompanying supposition more explicit:

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[1] Newton, *Correspondence*, I, pp. 96-97.

> And that all such reflected Light is of the same Nature with the Sun's Light before its Incidence on the Base of the Prism, no Man ever doubted; it being generally allowed, that Light by such Reflections suffers no Alteration in its Modifications and Properties. […], So then, the Sun's incident Light being of the same Temper and Constitution with his emergent Light, and the last being compounded of Rays differently refrangible, the first must be in like manner compounded.[2]

Note that, if we do not accept that white light consists of rays differently refrangible, then we have to assume that a different refraction process occurred at the first prism than at the second prism of Newton's *experimentum crucis*: one for the *creation* of colours and one for the *common refraction* of colours. On this assumption, the rays where created at the first prism, while at the second prism the created rays were refracted. If we suppose, on the other hand, that white light consists of rays differently refrangible, then the effects in both prisms can be explained by the same refraction process. So it seems that causal parsimony and the belief in the uniformity of nature are what motivated Newton's endorsement of the view that white light consists of a heterogeneous mixture of colours. Unfortunately, the problem is that observing coloured rays in white light is impossible –for coloured rays become visible only after refraction. My point is that *Newton's claim that white light is a heterogeneous mixture of colours is utterly dependent on this specific argument of causal parsimony*.

At this point, one may point out that Newton might have relied on the Rules I or II of his *regulae philosophandi*, as Alan E. Shapiro has suggested in his *Fits, Passions, and Paroxysms*. Newton's first *regula philosophandi*, as formulated in the third edition of the *Principia*, states that: "No more causes of natural things should be admitted than are both true and sufficient to explain their phenomena [*quam quae verae sint & earum phenomenis explicandis sufficiant*]." The second *regula philosophandi*, which is in fact a corollary to the first, states, as formulated in the same edition, that "Therefore, the causes assigned to natural effects of the same kind must be, so far as possible [*quatenus fieri potest*], the same."[3] At first glance, reference to the *regulae* seems to be a reasonable suggestion.

However, this move will not do. Rules I and II might on a *prima facie* basis seem to express the idea of causal parsimony, to wit: the number of causes in natural philosophy should not be inflated beyond necessity, because nature operates economically. Upon closer consideration, in Rules I and II Newton also provides two desiderata that a proper cause in natural philosophy should meet: a cause should not merely be explanatory, it should also be true. Put differently, a true cause should not merely explain its effect; it should also be shown that its observable effect *necessarily follows* from this very cause. Once this is established, then, when its corresponding effect is observed, the cause can be safely inferred to exist. The question, then, that Newton is trying to answer in Section 2 of Book I of the *Principia*, is not so much *Which forces entail Keplerian motion?*, but rather *What are, given the laws of motion, the necessary and sufficient conditions for Keplerian motion?* Establishing that inverse-square centripetal forces are the necessary and sufficient conditions for Keplerian motion warrants that, *given the laws of motion*, Keplerian motion, in general, is produced by inverse-square centripetal forces, and inverse-square centripetal forces alone. On this reading, Rules I and II assert that causes shown to be necessary and sufficient of their effects *–and only such causes!–* are to be kept minimal. Rules I and II license the identification of instances of causes of the same kind which have been shown to be necessary and sufficient to explain phenomena of the same kind. In other words, in the context of the *Principia*, this implied that, once we have deduced from phenomena that the motions of Jupiter and Saturn are both produced by an inverse-square centripetal force, we may assume that these phenomena are produced by the same force: gravity.

On the basis of Rules I and II, we thus identify two or more instances of causes of the same kind, which have been derived separately from phenomena. *The disanalogy involved*, which is visualised on the screen, *is thus that in the* experimentum crucis *we use an argument of causal parsimony to establish a (single) causal claim, while Rules I and II in fact licence the identification of similar causes which were separately deduced from similar phenomena by means of the systematic dependencies established in Book I of the* Principia.

What I see Newton doing in his comments on Experiment 9, Part 1 of Book I of *The Opticks* is showing –at least on an implicit level– that the innateness of white light cannot be directly deduced from phenomena and that the strongest argument adducible for its establishment is his argument from causal parsimony which we have just spelled out. As I have also argued, this argument could not be licensed by *regulae philosophandi* I and II.

Looking with Unseeing Eyes into the Invisible Realm: The Problem of Transduction

Transduction refers to the reasoning process whereby inferences are made about the *microscopic components* of bodies from the observed laws and properties of the *macroscopic bodies*, which these microscopic components constitute (or are supposed to constitute).

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[2] Newton, *The Opticks*, pp. 55-56.

[3] Newton, *The Principia*, pp. 794-795.

Newton, however, who insisted in the General Scholium (1713) that scientific propositions should be based on phenomena directly or on deductions from phenomena (for "whatever is not deduced from the phenomena must be called a hypothesis"), *could not*, as I shall explain in what follows, *infer statements about the invisible realm of optical phenomena in the demonstrative part of optics without introducing hypotheses*.

In the context of transduction, the "Analogy of Nature" was a guiding principle in Newton's research, for without the uniformity between microscopic and macroscopic components, it would be impossible ... "to derive the qualities of imperceptible bodies from the qualities of perceptible ones," as Newton observed in manuscript material on Hypothesis III (which would later become Rule III) in 1692. While the "Analogy of Nature" suggested to Newton that the micro-constituents of optical phenomena are similar or analogical to the bodies that fall within the reach of our observation, these micro-constituents are in themselves unobservable and, moreover, there is no way to justify the "Analogy of Nature" in this particular context. Therefore any characterization of them in corpuscular terms would remain utterly speculative. In different manuscript material, which was composed in the same period as Hypothesis III, Newton stressed that:

> Here only sensible [bodies] and their parts are treated [and it is] therefore that the argument of induction may have [its] place in them only. Distant [bodies] that cannot be perceived, but which are nevertheless hypothetically called bodies by some, should be more adequately treated in hypothetical Metaphysics and Philosophy.[4]

The tension between this quote and the previous quote from Hypothesis III, could hardly have escaped his attention. When Newton increasingly began to emphasize "Deductions from Phenomena" shortly after 1700, this tension became all the more stronger. Notwithstanding, in his optical research Newton *made* claims on the micro-constituents of optical phenomena (for instance, in his explanation of coloured bodies in Book II, Part III or in his hierarchical account on the structure of matter at the end of Proposition VIII, Part III, Book II of *The Opticks*).

Transduction in the *Principia*

Up to this point, I have discussed some of the problems surrounding Newton's optical research, however, I should also add some short observations on how transduction could be legitimately carried out in the *Principia*. On the basis of the propositions on the attraction of spherical bodies, i.e. Propositions LXXI-LXXVI (71-76) of Book I of the *Principia*, Newton could argue that the overall inverse-square centripetal force of a sphere on an external particle (or sphere) results from the summation of the individual inverse-square centripetal forces of each of the particles composing that sphere. In this case, transduction is rather unproblematic because the constituents of bodies share the same theoretically relevant property with the bodies they constitute: namely, mass. Allow me to elaborate this point. Newton clearly conceived of mass as an additive property: a body's (total) mass consists of the masses of its parts and it increases (or diminishes) as the number of parts increases (or diminishes). Since the gravity towards all planets is proportional to their mass, it follows that their gravity is also additive: the gravity of the whole results from the gravity of its parts. Given this subtle process of decomposition, Newton could bridge the gap between the micro- and the macro-level in the *Principia*. In optics, by contrast, transduction is problematic because it amounts to endorsing a particular hypothesis on the nature of light.

To avoid giving the impression that transduction was always unproblematic in the *Principia*, I should also point out in Section VII of Book II of the *Principia* Newton's attempt at transducing the micro-constituents of the inertial component of fluid resistance failed. Given the time-constraints, I will not further discuss this.

The Asymmetry between the *Principia* and *The Opticks*

Optical phenomena did not easily lend themselves to a *Principia*-style physico-mathematical treatment. As Newton experienced for instance in his study of diffraction, optical phenomena can behave in odd ways –rendering their study and characterization particularly resilient. In mechanics, the affected entities, i.e. the *explananda*: *bodies* moving along specific trajectories, and their constituting elements, i.e. the *particles* constituting these very bodies, all have a theoretically salient property in common: *mass*. In optics, by contrast, we do not know –at least not without speculating on the matter– the constituting elements of the *explananda*.

In order to get a grasp on the difference between *The Opticks* and the *Principia*, let us take a closer look at the axioms of *The Opticks*, which remained unchanged in all editions. The eight axioms in *The Opticks* basically deal with the geometrical properties of reflected and refracted light. Here I shall, however, not discuss all of these axioms. Rather, I shall provide some examples, which are required to make my next point. The first axiom states that the "*Angles of Reflection and Refraction, lie in the same Plane with the Angle of Incidence*;" the second axiom states that the angle of reflection is equal to

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[4] My translation of CUL Add. Ms. 3965, f. 422^{r}.

the angle of incidence; the third axiom states that if a refracted ray is returned to the point of incidence it will be refracted into the initial line by the incident ray; the fifth axiom contains the sine law of refraction which holds "either accurately or very nearly."

In contrast to the laws of motion in the Principia*, the axioms in* The Opticks *characterize phenomenological relations described in geometrical terms and they do not carry information about the proximate causes producing these phenomena*. The laws of motion served as a theoretical toolbox that allows one to gather information about the forces acting upon bodies. By Law I, Newton could infer the presence of a centripetal force from non-inertial motion; by Law II, he could establish the magnitude and direction of a centripetal force producing such non-inertial motion; and, by Law III, he was able to relate the centripetal force to its counteracting reaction force.

Another way of putting it is that *The Opticks* could not transcend the tradition of the *scientiae mixtae* and of geometrical optics more particularly *–at least not without considerable abductive and inductive risk*. In contrast to the physico-mathematical theory of the *Principia*, which provides causal explanations of the *explananda* from "within" the theory, a mixed-science treatment describes a given phenomenon mathematically, without an accompanying explanatory story. This contrast is again visualized on the screen. Newton, however, wanted to do more than to simply establish the phenomenological laws regulating optical phenomena: he also wanted to provide a solid physical account of optical phenomena. In order to be explanatory, a mixed-science treatment requires the addition of a causal story, which by definition falls outside of the mathematical description.

In the Principia*, which stood in the tradition of physico-mathematics, such causal stories could be licensed by the systematic dependencies between cause and effect, which were derived from the laws of motion. In other words, the proximate causes to be adduced in Book III of the* Principia *are constrained by the laws of motion*. As a way of reducing the risk of wild speculation, Newton demanded that the causes of orbital motion ought to be derivable from the laws of motion, i.e. from principles that have already shown their usefulness in natural-philosophical inquiry. In other words, to avoid introducing arbitrary causes into natural philosophy, Newton required that causes should be derivable from non-arbitrary principles or laws. The motivation behind this was to get rid of the problem of causal under-determination which vexed the method of hypothesis, to which Newton referred to in a letter on 10 June 1672 to Henry Oldenburg for Ignace Pardies: "For if the possibility of hypotheses is to be to test the truth and reality of things, I see not how certainty can be obtained in any science; since numerous hypotheses may be devised, which shall seem to overcome new difficulties."[5]

Newton's derivation of causes in the *Principia* contrasts significantly with his explanation of the *experimentum crucis*, viz. that white light consists of rays differently refrangible. This claim was established by an external consideration, i.e. a consideration independent of the mathematical *exposé* of the reported refraction patterns: Newton's argument of causal parsimony which we have spelled out previously. Given the absence of systematic dependencies, Newton could only offer *sufficient causes* in *The Opticks*. In a letter to Newton on 13 October 1676, Anthony Lucas, one of Newton's most acute commentators, pointed out that the heterogeneity of white light is only a sufficient cause of the effects described in the *experimentum crucis*. According to Lucas, Newton did not prove "*that this unequall refraction necessarily implyes an unequall refrangibility in rays differently coloured*," therefore "not evinceing that this inequality ariseth from an unequall *refrangibility*, *intrinsecall* to *rays differently coloured*." More generally, given the empirical and methodological problems which Newton encountered while trying to methodize optics in a *Principia*-style manner, he found out that establishing non-hypothetical causal interpretations of optical phenomena was quite a difficult matter. As I have documented in the preceding sections, Newton's ideal of deducing causes by their effects turned out to be unattainable in optics: 1. *the fundamental principles in optics remained uncertain*, 2. *no methodological justification for the use of transduction within the realm of optics could be provided*, and 3. *the causal claims introduced in* The Opticks *could not be constrained by theory*.

In *The Opticks* Newton occasionally transcended the strictly observational features of optical phenomena and this did not exactly fit hand in glove with his methodological standards. Therefore Newton's "failure" in *The Opticks* was his failure to meet the very standards he had formulated himself. In *The Opticks*, therefore, Newton was genuinely facing the limits of deducing from phenomena.

[5] Newton, *Correspondence*, I, p. 164.

References

NEWTON, Isaac. (1979 [1952] [based on the 1730 edition]) *The Opticks, or A Treatise of Reflections, Refractions, Inflections and Colours of Light* (New York, New York, Dover).

NEWTON, Isaac (1959-1977) *The Correspondence of Isaac Newton* (7 vol.), edited by H.W. Turnbull, J.F. Scott, A. Rupert Hall, and Laura Tilling (Cambridge, Cambridge University Press).

NEWTON, Isaac (1999) *The Principia, Mathematical Principles of Natural Philosophy, A New Translation by I. Bernard Cohen and Anne Whitman, assisted by Julia Budenz, Preceded by A Guide to Newton's Principia by I. Bernard Cohen* (Berkeley, University of California Press).

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THE STETHOSCOPE. HOW THE PRESENTATION OF A MEDICAL INNOVATION INFLUENCED ITS SUCCESS

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Abstract

Laennec's De l'Auscultation Médiate *(1819) has been criticized even by its translator, John Forbes, for being "written in a diffuse and verbose style by no means commendable in a work of science". The modern reader would wholeheartedly agree, especially since the book also seems to indiscriminately mingle case reports, pathogeneses, diagnostics, post-mortem analyses, physiology and numerous other elements including anecdotes from the author's life. Only a very small proportion is dedicated to the stethoscope and its use.*
It is interesting to observe that in fact Laennec's contemporaries perceived the work above all as a treatise on anatomical pathology and not so much as a handbook for a new means of diagnosis.
For his first English translation, Forbes consequently rearranged and considerably shortened the book, "restoring" it to "what I humbly conceive it ought always to have been".
The most common criticism was, however, the sheer length of the work; two volumes of 470 and 472 pages. Soon concise versions, even tiny "pocket versions" and overviews in tabular form, came out. Their great variety seems to suggest a considerable demand.
But not all objections were based on presentation. The new method of diagnosis was criticized "because its whole hue and character is foreign, and opposed to all our habits and associations." It was considered too complicated to learn, too mechanical and against the old art of Hippocratical medicine. But this all didn't stop the stethoscope from becoming inevitable within only a few decades.
On the basis of books, press and journal reports, the paper seeks to examine how far the way of presentation chosen by Laennec served as inhibitor or promoter to the international success of his invention, with special focus on France, Britain and Germany.

In 1819 a book was published which was meant to change the world of medical diagnostics forever. Its title was *De l'auscultation médiate ou traité du diagnostic des maladies des poumons et du coeur, fondé principalement sur ce nouveau moyen d'exploration*. (On mediate auscultation, or treatise on the diagnostics of illnesses of the lungs and heart, mainly based on this new means of exploration.) By "this new means of exploration", of course the stethoscope or 'cylinder' was meant, an invention made by the author, René Théophile Hyacinthe Laennec, in 1816.

The work contained two volumes of 456 and 472 pages –plus tables– and at first sight seems equally disappointing and confusing to the modern reader. Only a small proportion is dedicated to the stethoscope and its use, the rest indiscriminately mingles case reports, pathogeneses, diagnostics, post-mortem analyses, physiology and numerous other elements including anecdotes from the author's life.

In this paper, I will examine how this form of presentation influenced the success of the medical innovation presented in these two volumes, the stethoscope.

Even though the history of the stethoscope and the life of Laennec are among the better researched topics in Medical history, with several excellent monographs and countless articles under numerous different approaches and focuses,[1] this particular aspect seems to have received very little attention so far. This is quite surprising, since the form of presentation was a major concern of contemporary reviewers –and to them clearly presented an obstacle to the success of the stethoscope and the technique associated with it, mediate auscultation.

This paper is mainly based on reviews of Laennec's work in the corpus of contemporary books and journals from France, Germany and the UK that is available online and also on the reviews mentioned in Patrick James Bishop's extremely useful –though not always accurate– bibliography of Laennec, which he assembled during decades of research.[2]

The Status quo ante

Before the invention of the stethoscope, medical diagnosis was principally based upon the patient's own narrative, combined with some very few methods of physical examination like palpation or pulse-taking.

Percussion –diagnosis by tapping on the patient's body– an invention that the Viennese physician Leopold Auenbrugger published in his *Inventum novum* in 1761 had become out of use in Germany and was little known in England. In France, however, the situation was different since the translation of Auenbrugger's *Inventum novum* by Jean-Nicolas Corvisart, the primary physician of Napoléon Bonaparte, in 1808[3] and it seems that percussion was also used to some extent in Edinburgh.[4]

The Stethoscope and mediate auscultation

The stethoscope has experienced a considerable evolution from the first trial model made of cardboard over the first professional versions made entirely of wood to the modern, binaural stethoscope we all know. Even though other means of diagnostics have become far more important, it still is a symbol of the medical profession and it already was as early as the mid 19th century.

The diagnostic technique involved in the use of the stethoscope is called "mediate auscultation", from Latin *auscultare* –to hear with attention, listen to, give ear to. **Immediate** auscultation, with the ear directly applied to the patient's body, had already been used by Hippocrates. Laennec's innovation therefore consisted in adding a wooden cylinder as intermediary –as he claimed to improve the aural effect, but also for modesty reasons when examining female patients– and a classification system for the sounds obtained.

The main fields of criticism

The book has missed its topic

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1 Notably Jens Lachmund, Der abgehorchte Körper: *Zur historischen Soziologie der medizinischen Untersuchung*, Univ.-Diss. Bielefeld, 1995. (Opladen: Westdt. Verl., 1997); Jacalyn Duffin, *To see with a better eye: A life of R.T.H. Laennec* (Princeton, New Jersey: Princeton University Press, 1998). and several other works by these two authors.

2 Patrick James Bishop, *A bibliography of R.T.H. Laennec (1781-1826): An attempt to list all publications by or about him, together with a list of manuscripts, letters and other related material* (Sydney NSW: Royal Australian College of Physicians, 1981).

3 Wutzer, 'Ueber das vom Herrn Dr. Laennec in Paris neuerdings bekannte gemachte Verfahren zur Exploration des Zustandes der Brustorgane, nebst einer Analyse des von ihm über denselben Gegenstand herausgegebenen Werkes', *Journal der Chirurgie und Augenheilkunde*, 1820, 359–79, 359–60; Saul Jarcho, 'A Review of John Forbes' Translation of Laennec', *Am J Cardiol*, 10 (1962), 859–63, 863.

4 Malcolm Nicolson, 'The introduction of percussion and stethoscopy to early nineteenth-century Edinburgh', in William Frederick Bynum and Roy Porter (eds.), *Medicine and the five senses. Based on a Symposium on Medicine and the Five Senses, held at the Wellcome Institute for the History of Medicine on 11-12 June 1987* (1. paperback ed., Cambridge: Cambridge University Press, 2004), 134–53.

By quite a few of Laennec's contemporaries, the treatise was seen as a work of pathological anatomy or on chest diseases:

> Le livre que nous avons à faire connoître, étant formé presque en entier des descriptions de maladies et de faits anatomiques (…) [qui] peuvent intéresser les médecins, et sur-tout les élèves, parce qu'ils y trouveront les effets liés avec les symptômes qui devoient les présager, si l'on en excepte des anomalies impossibles à soupçonner. (…) l'auscultation médiate, que M. Laennec appelle l'objet principal de son livre, et que nous regardons comme l'accessoire, eu égard à la totalité de l'ouvrage, qui est un bon traité des maladies de la poitrine.[5]

I only found one isolated author, Italo-English physician August Bozzi Granville, who in his 1820 review thought that Laennec had used pathological anatomy as mere "accessories" to the description of mediate auscultation, since the book was aimed at introducing mediate auscultation into the praxis of medical diagnosis. However, Granville found the parts of the book dealing with pathological anatomy "very important".[6] [7]

The book is too long

The sheer length of Laennec's treatise was commonly criticised, even in France, and it was seen as a great obstacle to its practical use.

> L'ouvrage de M. Laennec est un livre éminemment pratique; mais nous avons craint que son étendre ne fut un grand obstacle à son utilité.[8]

One French reviewer was convinced that Laennec would have abridged the post mortem reports, if only he had had the time:

> Il eût peut-être possible à M. Laennec de donner moins de longueur à son ouvrage, en abrégeant un peu des descriptions d'ouvertures de corps, sans omettre les lésions importantes. Il l'aurait fait, sans doute, si d'autres occupations le lui eussent permis.[9]

Among those who criticised the length was Laennec's personal enemy, François Broussais (1772-1838), like Laennec also a former pupil of Corvisart, but follower of a different medical school. Laennec commented on this in the preface to his second edition: "I never thought that one should learn everything by heart."

> Je n'ai jamais pensé qu'on dût les apprendre par cœur.[10]

Even though Laennec himself did not find it necessary, the medical profession soon developed its coping strategies. Countless condensed versions appeared within the first decade of the first publication of Laennec's treatise, most of them in medical journals, sometimes as a whole series, for example in *The Lancet* in England and the *Medicinisch-chirurgische Zeitung* in Germany.

Some of them clearly stated that the purpose of their summary was to free the busy practitioner from the lengthy lecture of the original, allowing him to form an opinion or even serving as a practical manual.

> Nous avons (…) cru rendre un véritable service aux gens de l'art, aux praticiens surtout, qui n'ont pas le temps de s'adonner à des longues lectures, en leur donnant une analyse de cet ouvrage suffisamment détalillée pour qu'ell puisse leur tenir lieu de l'ouvrage lui-même, et assez succincte pour qu'elle puisse leur servir de manuel pratique. [11]

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[5] Tessier, 'De l'Auscultation médiate. Ou Traité du diagnostic des maladies des poumons et du coeur fondé principalement sur ce mouveau moyen d'exploration', Par R.T.H. Laennec, *Journal des Savans*, 1820, 52–5, 52–3.

[6] Bishop, *A bibliography of R.T.H. Laennec (1781-1826)* (above, n. 2), 362–3. - 27.11 Granville (Augustus Bozzi): Rezension: On mediate auscultation, London med. phys. J. 1820, xliii, 164-70 (No. 252, February).

[7] Augustus Bozzi Granville, 'On mediate auscultation… [review]', *The London medical and physical journal*, 1820, 164–70, 165.

[8] Rezension L. Rouzet, Rev. med. hist. philos. 1820, 1, 1-82 (1re livraison, janvier); see Bishop, *A bibliography of R.T.H. Laennec (1781-1826)* (above, n. 2), 361.

[9] Tessier, 'De l'Auscultation médiate. Ou Traité du diagnostic des maladies des poumons et du coeur fondé principalement sur ce mouveau moyen d'exploration' (above, n. 5), 55.

[10] René Theophile Hyacinthe Laennec, *Traité de L'Auscultation Médiate et des Maladies des Poumons et du Coeur* (Bruxelles: Librairie Médicale et Scientifique, 1828), xiv., footnote nr. 4.

[11] Rouzet, *Rev. med. hist. philos*. 1820, 1, 1-82 (1re livraison, janvier); see Bishop, *A bibliography of R.T.H. Laennec (1781-1826)* (above, n. 2), 362.

> L'ouvrage de M. Laennec est long, trop long peut-être; et les praticiens occupés n'ayant pas le temps de lire un livre de si longue haleine, voulant néanmoins profiter des signes nouveau au moyens desquels ils pourront mieux apprécier les diverses altérations thoraciques, trouveront sans doute avec plaisir dans quelques pages l'extrait de tous ces signes. Bien q'un peu aride, un pareils travail sera lu par l'attrait de la nouveauté du sujet, l'éspoir d'en retirer un grand avantage et de pouvoir le juger soi-même. Tel sera le but de cet article, qui ne sera mêlé d'aucunes réflexions critiques.[12]

Auscultation manuals appeared, like William Stokes' "Introduction to the use of the Stethoscope" in 1825 or Adam Raciborski's (1809–1871) "New and complete manual of auscultation and percussion" in 1835. Raciborski's work appeared almost simultaneously in French, English and German and was especially popular in France.[13]

The most extreme examples of abridged versions are probably Richard Townsend's "Tabular View of the Prinicipal Signs Furnished by Auscultation and Percussion" from 1832 and the "Laennec in der Westentasche" (Waistcoat-Pocket-Laennec) which enjoyed at least 3 editions in the 1860s and 1870s.

The structure of the text

The translator of the English version of Laennec's treatise –which came out as early as 1821– , John Forbes, both rearranged and considerably shortened the book, restoring it to "what I humbly conceive it ought always to have been". He was convinced that both the English reader and the original author should be thankful for this.

Forbes had in fact created two separate works and divided pathology from diagnosis.

Reviews of Forbes' translation generally welcomed this rearrangement and saw it as an important improvement compared to the original,[14] but not all agreed with the considerable abbreviation of the case reports[15] –usually condensing several pages into just a few lines.

> Dr. Forbes is entitled to much praise for the laborious task he has imposed on himself of re-modelling the arrangement of the original work, and concentrating the language, so as to occupy but one volume instead of two - a double improvement, that saves both time and expense. He has also enriched the work by the addition of a sensible preface, and many valuable notes.[16]

> We consider the translator's arrangement as a considerable improvement, and as tending in a most essential manner to facilitate the reference of the Practitioner.[17]

Forbes himself had second thoughts on this and included longer case reports in the second, 1827 edition of his translation and he also published a separate volume of "Original Cases with dissections and observations illustrating the use of the stethoscope and percussion in the diagnosis of diseases of the chest" in 1824.

Part of the somewhat confuse structure of the original French edition might be due to the fact that Laennec had agreed to 'deliver' his work to the publisher bit by bit, the last part no later than June 30th, 1819.[18]

Some French critics, on the other hand, praised the order observed by Laennec in his work as "corresponding to the true principles of analysis":

> L'ordre suivi par M. Laennec dans la rédaction de son ouvrage est conforme aux vrais principes de l'analyse, puisqu'il commence par donner l'état anatomique positif des maladies dont il veut recueillir les signes par l'auscultation immédiate (sic!), et qu'il appuie ensuite par des fait ce qu'il a dit précédemment.[19]

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[12] L. Rostan, 'Review of De l'Auscultation…', *Nouveau journal de médecine, chirurgie, pharmacie*, 1819/6 (September), 256.

[13] 'review on Raciborski, M. Adam, *New and complete manual of auscultation and percussion: applied to the diagnosis of diseases* (London: A.H. Baily and Co, 1835).', The Lancet, 582.

[14] Bishop, *A bibliography of R.T.H. Laennec (1781-1826)* (above, n. 2), 369. - 27.26 A Treatise on the Diseases of the Chest ... Translated ... By John Forbes ... London, 1821, Lond. med. Reository, mthly j. and Rev., 1822, 17, 209-18 (no 99 - March 1).

[15] *Ibid*., 368. - 27.23 - A Treatise on the Diseases of the Chest ... Translated from the French ... by John Forbes, London 1821, Med. Intell. 1822, NS, 3, 68-69 (No 28 - February).

[16] Review on Forbes' translation in *Medico-Chirurgical Review and Journal of medical Science* (Analytical Series), 2 (1821-22), p. 795; see also Jarcho, 'A Review of John Forbes' Translation of Laennec' (above, n. 3), 859.

[17] Review on Forbes' translation, *The London medical repository: monthly journal and review*, 1822, 209–18, 210–1.

[18] Bishop, *A bibliography of R.T.H. Laennec (1781-1826)* (above, n. 2), 30.

[19] I. Bricheteau, *Journal complémentaire du dictionnaire des sciences médicales*, 1820, 245–55, 254–5.

For his second edition, which appeared in 1826, Laennec considerably changed the structure and apparently also picked up some of Forbes' alterations, separating pathology more clearly from diagnosis. Even though Laennec did not speak any English –or, in fact, any other modern languages besides French and Breton–, we know that he was aware of Forbes' edition and that the two actually exchanged letters.

As you would expect, the alterations in the second French edition were warmly welcomed in England, it was said to be "so much more distinct and systematic, that its value is greatly increased".[20]

Language and Style

Laennec's writing style found very little sympathy in the English-speaking world. His translator John Forbes stated in his introduction to the second edition: "(…) my translation (…) will be more valuable than if it had been strictly literal; a good deal of the original being written in a diffuse and verbose style by no means commendable in a work of science."[21]

It seems his compatriots were pretty much in line with this judgment. On the literary character of Forbes' translation, one reviewer stated: "It appears, upon the whole, to be both perspicuous and succinct, and to possess but little of the French idiom. These are great requisites as the translation of a medical work, which is strictly descriptive, ought to aspire to." A certain lack of linguistic variation was diagnosed, but that was considered as beyond the translator's responsibility.[22]

The French strongly disagreed. Even in 1925, Laennec was still praised for his excellent style, which was "severe, condensed, clear, elegant –real medical writing" ("sévère, condensée, claire, élégante (…), véritable écriture médicale"[23]).

A French 1933 thesis stated that Laennec's elevatedness of thinking harmonizes with the grave, austere and meditative form. His language is compared to that of Pascal or Bossuet in its affluent power, its balanced rhythm and also what he calls *allure combative*:

> La hauteur de la pensée s'y harmonise à la forme grave, austère, méditative. (…) Cette langue de Laennec nous semble très proche de celle d'un Pascal et d'un Bossuet par la puissance aisée, l'équilibre des périodes, aussi par l'allure combative (…)[24]

Even Michel Foucault in his Birth of the Clinic (1963) praises "the extraordinary beauty" of Laennec's description of liver cirrhosis.[25]

So, just like nowadays there were considerable national differences in what was seen as an appropriate academic style.

Terminology

Laennec probably would have loved to have the multimedia possibilities of this mid-1990s educational software [the "Laennec CD-ROM"[26]] but this was of course not possible in the early 19th century. So he chose to describe auscultation sounds in metaphorical, sometimes rather poetic terms, in order to achieve a 'graphic' effect.

Of course these were not self-explanatory and it is –for instance– quite difficult to explain why something that sounds like "the cooing of a wood-pigeon" should be classified as "râle" (which means rattle or stertorous breathing)[27].

John Elliotson took his medical doctorate in 1821. He was very much drawn to auscultation but had to struggle to master it. As he told years later in a clinical lecture delivered at St. Thomas Hospital in London, he once heard what he took to be a "metallic tinkling" and a "click" and said he never could have imagined before what Laennec could possibly mean with those descriptions. So he undertook to learn right from the master himself in Paris, but Laennec was already too ill to teach.[28]

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[20] 'On Auscultation and Percussion', *Glasgow Medical Journal*, 1 (1828), 62.

[21] René Théophile Hyacinthe Laennec, *A treatise on the diseases of the chest and on mediate auscultation* (4th edn., London, 1834), xi.

[22] Review on Forbes' translation (above, n. 17), 218.

[23] Henri Bon, *Laënnec*: *1781 - 1826* (Les grands catholiques des 19e & 20e siècles, Dijon: Publ. 'Lumiére', 1925).; cited after Marie-Joseph-Raoul-Marcel Jaubert, *Evolution des idées de Laënnec touchant l'auscultation médiate, la séméiologie des affections pulmonaires et les productions accidentelles développées dans le poumon* (Bordeaux: Imprimerie Librarie de l'Université, 1933), 11.

[24] Foucault, Michel (2003) *The Birth of the Clinic*. London: Routledge, p. 208; cf. *Ibid*., 15.

[25] John G. Simmons, *Doctors and discoveries: Lives that created today's medicine* (Boston: Houghton Mifflin, 2002), 63.

[26] http://www.amazon.de/Springer-Berlin-LAENNEC-CD--ROM/dp/3540145354/ref=sr_1_1?ie=UTF8&qid=1296658848&sr=8-1
http://openlibrary.org/books/OL12771866M/Laennec_CD-ROM
For details and screenshots see http://ditwww.epfl.ch/SIC/SA/publications/FI94/special-ete/94-SP-page24.html

[27] A. John Robertson and Robert Coope, 'Before Our Time Râles, Rhonchi, and Laennec', *The Lancet*, 1957, 417 ‐ 423, 418.

[28] Lester S. King, 'Auscultation in England, 1821-1837', *Bulletin of the History of Medicine*, 33 (1959), 446–53, 447; Clinical Lecture by Dr. Elliotson, delivered at St. Thomas's Hospital, *Lancet* 1829-30, 1, 398-403, here 402.

Gabriel Andral (1797-1876), who had incurred Laennec's anger for having experimented with auscultation without his authorisation, was the first to develop a new systematic of auscultation sounds in his 1821 Paris thesis on diseases of the chest.[29]

Peter Mere Latham (1789-1875) invented his own classification of auscultation sounds (mainly distinguishing dry from moist sounds for the lung) in his "Lectures on subjects connected with clinical medicine" in 1836.[30]

In 1839, Andral's pupil Jules Fournet (1812-1888) developed an extremely elaborated system of auscultation sounds, in a two-volume book based on his 1837 thesis. The classification was organised in different layers and resembled the taxonomy of the botanists of the Enlightenment period. Criteria were –amongst others– intensity and pitch of the different sounds.[31]

Also in 1839, Viennese physician Josef Skoda (1805-1881) developed yet another system of auscultation sounds. Just like Laennec, he also tried to match sound phenomena and anatomical alterations, but he did not suppose a simple 1 to 1 relation.[32] Skoda made a distinction between the 'chemical behaviour' of the organs, which lead to anatomical alterations and the 'mechanical behaviour' that produced auscultation sounds. Acoustic principles were the major guideline for the analysis of auscultation signs. If there was no physical explanation, the sign was to be considered as 'unreliable'.[33] Whereas Laennec had presented an abundance of clinical cases and postmortems, Skoda's book was based on theoretical reasoning and descriptions of acoustic experiments.[34] The quasi-visual depictions that Laennec used so frequently are quite absent in Skoda's work.[35]

So now the French school "which cultivated the senses" was facing a new, "rational" German one.[36]

As one could almost expect, Skoda's approach wasn't much of a success in France.[37]

In Germany, on the other hand, Laennec's signs were considered as too complicated around 1840 and Fournet's system as completely unusable.[38] Skoda had reduced the number of stethoscopic signs and was seen by many as the grand simplifier of the method. [39]

Skoda had a considerable success with his diagnostic, soon became head of department and in 1846 professor of medicine. Students of all ages and all nationalities began flocking to Vienna.[40]

With all the competing systems, it is no wonder that the correct classification and interpretation of auscultation sounds remained an issue of controversy well into the mid-nineteenth century.[41]

The written text cannot replace teaching

Learning to distinguish the different sound phenomena was generally considered a long and assiduous task,[42] taking more patience than most readers had.[43] Only part of the phenomena could be learnt through self-teaching,[44] an

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[29] In his thesis "Récherches sur l'expectoration dans les différents maladies de poitrine, Paris 1821, Thèse no 89; cf. Robertson and Coope, 'Before Our Time Râles, Rhonchi, and Laennec' (above, n. 27), 419.

[30] Peter Mere Latham, *Lectures on subjects connected with clinical medicine* (London: Longman, Rees, Orme, Brown, Green, and Longman, 1836); cf. Robertson and Coope, 'Before Our Time Râles, Rhonchi, and Laennec' (above, n. 27), 420.

[31] Jules Fournet, *Recherches cliniques sur l'auscultation des organes respiratoires, et sur la première période de la phthisie pulmonaire: faites dans le service de M. le Prof. Andral*, 2 vols. (Paris: J.S. Chaudé, 1839).; cf. J. Lachmund, 'Making Sense of Sound: Auscultation and Lung Sound Codification in Nineteenth-Century French and German Medicine', *Science, Technology & Human Values*, 24/4 (1999), 419-450, 427.

[32] Joseph Škoda, *Abhandlung über Perkussion und Auskultation* (Wien: Mösler & Braumüller, 1839); Lachmund, 'Making Sense of Sound: Auscultation and Lung Sound Codification in Nineteenth-Century French and German Medicine' (above, n. 31), 430.

[33] Lachmund, 'Making Sense of Sound: Auscultation and Lung Sound Codification in Nineteenth-Century French and German Medicine' (above, n. 31), 430.

[34] *Ibid*.

[35] *Ibid*., 432.

[36] *Ibid*., 433.

[37] *Ibid*.

[38] *Ibid*., 437.

[39] *Ibid*., 438.

[40] Horace Marshall Korns, 'A brief history of physical diagnosis', *Annals of medical history*, 1939, 50–67, 58.

[41] Lachmund, 'Making Sense of Sound: Auscultation and Lung Sound Codification in Nineteenth-Century French and German Medicine' (above, n. 31), 419.

[42] Bricheteau (above, n. 19), 247.

[43] *Ibid*., 250.

apprenticeship in Parisian hospitals was generally considered more favourable –but this meant a disadvantage to non-French practitioners and students of medicine.[45]

But soon pupils of Laennec and other young physicians acquainted with the new technique offered both private and university courses in the use of the stethoscope, placing adverts in medical journals and the daily press.

> [advertisement] *Specielle Pathologie* der Lungenkrankheiten nach *Laennec* Hr. Dr. *Lorinser* zwei Stunden wöchentlich, unentgeltlich[46]

Real life presentation: PR activities

Some reviewers noted with a certain enervation that Laennec left out no possibility for promoting himself and the stethoscope. He presented his invention wherever he could and wherever it seemed favourable to him,[47] be it the Academie Royale des Sciences de l'Institut de France[48] or the Societé de Medicine, where he even took a patient for demonstrations.[49]

François-Victor Mérat de Vaumartoise (1780-1851) was present at one of these events and even though the patient was clearly suffering from a very high degree of tuberculosis, he could not hear the *pectoriloquie* that was so evident according to Laennec –nor could other participants.

> Nous devons avouer que les essais que nous avons faits avec le pectoriloque ne sont point à l'avantage de cet instrument. Nous avons cherché à nous assurer de la pectoriloquie sur une malade que M. Laennec a présentée lui-même à la société de la faculté, comme pectoriloque, et qui était effectivement dans un degré très-avancé de phtisie : nous n'avons pu l'entendre; d'autres membres n'ont pas été plus heureux que nous[50]

Foreigners, who had visited Laennec in Paris and joined him during his hospital rounds, reported the same.[51]

Laennec was once again not amused and in his preface to the second edition of his treatise ranted about "deaf people" and that there was "nothing worse than those who do not want to hear".

> Il y a d'ailleurs des sourds, et, comme l'a remarqué un des auteurs du *Dictionnaire des Sciences médicales* (article *stéthoscope*), il n'y en a pas des pires que ceux qui ne veulent pas entendre.[52]

He also presented free copies to his treatise to institutions like the Académie des Sciences (27. November 1819)[53] or to Matthew Baillie (1761-1823), personal physician to George III and George IV, a specialist in morbid anatomy who also practised percussion.[54] Baillie's copy is preserved and was digitised by the Bibliothèque Nationale de France for their Gallica project.[55]

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44 'On Auscultation and Percussion' (above, n. 20), 60.

45 J. H. Kopp, *Aerztliche Bemerkungen, veranlasst durch eine Reise in Deutschland und Frankreich im Frühjahre und Sommer 1824* (Frankfurt: Hermann, 1825); A. Mühry, *Darstellung und Ansichten zur Vergleichung der Medicin in Frankreich, England und Deutschland. nach einer Reise in diesen Ländern 1835* (Hannover: Hahn, 1836); C. Otto, *Reise durch die Schweiz, Italien, Frankreich, Grossbritannien und Holland: Mit besonderer Rücksicht auf Spitäler, Heilmethoden und den übrigen medicinischen Zustand dieser Länder* (Hamburg: August Campe, 1825); cf. Lachmund, 'Making Sense of Sound: Auscultation and Lung Sound Codification in Nineteenth-Century French and German Medicine' (above, n. 31), 437–8.

46 *Journal der practischen Heilkunde, hrsg. v. C.W. Hufeland*, 1822, 125. (Lorinser was a pupil of Laennec).

47 Tessier, 'De l'Auscultation médiate. Ou Traité du diagnostic des maladies des poumons et du coeur fondé principalement sur ce mouveau moyen d'exploration' (above, n. 5), 54–5.

48 Bishop, *A bibliography of R.T.H. Laennec (1781-1826)* (above, n. 2), 183.

49 *Ibid*., 185.

50 Mérat, 'Pectoriloque', in , *Dictionnaire des sciences médicales, par une société de médecins et de chirurgiens*, 60 vols. (Paris: Panckouke, 1812-1822), 10–35. – Mérat had to admit, though, that the conditions were not ideal, since the room was rather noisy.

51 Wutzer, 'Ueber das vom Herrn Dr. Laennec in Paris neuerdings bekannte gemachte Verfahren zur Exploration des Zustandes der Brustorgane, nebst einer Analyse des von ihm über denselben Gegenstand herausgegebenen Werkes' (above, n. 3), 365.

52 Laennec, *Traité de L'Auscultation Médiate et des Maladies des Poumons et du Coeur* (above, n. 10), xiii. – The are in fact two articles on the stethoscope in the Dictionnaire des sciences médicales, one not very favourable one by Mérat under the heading "pectoriloque" (see footnote 50) and the one that Laennec refers to here, which is written by one of his pupils: De Lens, 'Stethoscope', in , *Dictionnaire des sciences médicales, par une société de médecins et de chirurgiens*, 60 vols. (Paris: Panckouke, 1812-1822), 586–90. However, the author doesn't mention "those who are not willing to hear", only lazybones, ignoramuses and anti-progressives.

53 Bishop, *A bibliography of R.T.H. Laennec (1781-1826)* (above, n. 2), 188.

54 Forbes, 1st edition, preface, 19.

55 *http://gallica.bnf.fr/ark:/12148/bpt6k987580/f2.image.pagination.r=laennec+auscultation+1819.langEN*

Conclusion

Even though the form of presentation in Laennec's "De l'Auscultation Médiate" was commonly seen as a big obstacle, it had relatively little influence on the success of the stethoscope and of mediate auscultation. The medical profession took enough interest in the new invention to come up with compensation strategies against its main weaknesses –be it length, structure, language and style, terminology or the need for practical teaching– and Laennec's promotional campaign and personal network –amongst others his several hundred foreign students– played a role that cannot be underestimated for the first years after the invention of the stethoscope. In the longer run, other factors played a more important role like the open-mindedness of the medical profession –in Scotland even medical professors attended auscultation classes– or the approach to medical teaching. Bedside teaching was common practice both in France and Scotland where the stethoscope found its way into everyday practice a lot earlier than in England and Germany, where medical teaching was almost exclusively academic and theoretical.

The success of a medical invention is a multi-factored process in which presentation is only one –and as it seems not always the most important– element.

Acknowledgements:

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ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 1010.

THE ROLE OF PHOTOCHEMICAL PROCESSES ON THE DEVELOPMENT OF COLOUR PRINTING IN 19TH-CENTURY CARTOGRAPHY

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Abstract

During the decade 1860-70, the French chemist Alphonse Poitevin (1819-1882) did research work on the photographic sensitizing properties of dichromated gelatin, which led to the development of several printing methods by photomechanical processes. This allowed significant advances in several scientific and technical fields such as cartography; production of printing maps was done with more precision and in an easier and economical way. The printing of maps by photomechanical processes led to remarkable changes in cartographic cameras, lenses and printing instruments produced by European makers.
The study of the chemical and physical principles of the most important photomechanical techniques used in the second half of the 19th century cartography will be presented, namely the three-colour photographic printing processes developed by Charles Eckstein in the Netherlands and José Julio Rodrigues (1843-1893) in Portugal. We also intend to relate their work with the photocollographic process (tricromy) developed in France by Louis Ducos du Hauron (1837-1920). In 1878, Ducos du Hauron presented at the Universal Exhibition of Paris his process of tricromy, but without obtaining any impression using photomechanical processes. At the same time Rodrigues researched and obtained coloured photomechanical prints. A parallel study of the optical, photographic and printing instruments specially designed for the 19th-century cartography will be done.

Introduction

In the first half of the 19th century the evolution on the technology of paper making, metallurgy and the invention of photography had a great impact on map production.

The first maps were printed in letterpress or relief printing. Engraving or intaglio printing on metal plates, usually on copper, was used for the period 16th-18th centuries. Both these printing processes were expensive and tedious to accomplish. After the invention of lithography around 1798, this was the main method used for map printing production[1]. Lithography was the most manageable and the reproduction of the maps was achieved by direct drawing or transfer on a heavy stone.

In a paper submitted to the Geographical Congress held in Paris in 1876, Captain James Waterhouse (1842-1922), Assistant Surveyor General of India stated:

> *"Among the many useful and important artistic and scientific applications of photography one of the most valuable is the reproduction by its means, in absolute facsimile, of maps and plans, speedily and cheaply and on any scale –either the same, larger or smaller (…) most of the civilized countries now possess special photographic studios for the reproduction of maps, plans, etc, (…) for fiscal, military and other purposes."*[2]

The experiment done by Joseph Nicéphore Nièpce (1765-1833) in 1826 on the light-sensitivity properties of bitumen of Judea with a heliographic engraving on a pewter plate provided a method of photomechanically reproducing line drawings on paper[3]. In 1852 Talbot invented what he called a "photoglyphic print"[4]. These two first experiments were later followed by several other methods of engraving using the photographic process[5]. The discovery of Mungo Ponton[6] (1801-1808) in 1839 of the light-sensitivity of silver chromates was the turning point in the development of the photomechanical processes. Alphonse Louis Poitevin (1819-1882) studied the behaviour of dichromated albumen exposed to light on a lithographic stone. With this method he won in 1867 the prize for photomechanical printing offered by the Duc de Luynes. He was also the inventor of the collotype, a practical photolithographic technique which used the controlled reticulation of exposed and heated gelatin on a glass plate.[7]

The photomechanical processes shared the following principle: a photosensitive substance applied to a hard surface (stone or metal), became insoluble after exposing it to light through a photographic negative. The parts protected from the light remained soluble and after washing it the support retained a positive impression of the image, ready to be inked and printed. As an alternative, a transparent positive image could be used on metal. In this case the areas under the dark part of the image disappeared and the metal was afterwards etched out with an acid solution and inked for printing. They were categorized in the same way as the taxonomy of print: relief, intaglio and planographic.

Photomechanical processes and the printing of maps

The attention of cartographers was directed to the new photomechanical methods. During the period 1850-1870 the photographic and photomechanical methods on map making were mainly used for the printing and reproduction of maps as well as reductions and enlargements of the map scaling. They had several advantages over the time-consuming tradicional processes: easier printing, the scale-variation achieved without having to employ the difficult and tedious process with the pantograph and the introduction of the half-tone printing[8]. This led to an increase in the use and diffusion of maps.

The first serious attempt using photography for the reduction of maps was done in 1855 by Colonel Henry James (1803-1877), Director of the Ordnance Survey of Great Britain and Ireland,[9] who proved in front of the English parliament that

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[1] Koeman, C., *In* Woodward, D. (edit) (1975), *Five Centuries of Map Printing*, Chicago and London, the University of Chicago Press, pp. 137-155.

[2] Waterhouse, J. (Captain), (1878), *The application of photography to the reproduction of maps and plans by the photomechanical and other processes*, Calcutta, G.H. Rouse, Baptist Mission Press, p. 1.

[3] Newhall, B. (1982), *The History of Photography*, New York, Bulfinch Press, pp. 13-15.

[4] Talbot obtained the "photoglyphic" print (photogravure) by producing images with chromated gelatina on a steel plate. He etched it with a solution of platinum (II) chloride, thus obtaining an intaglio surface from which impression could be made on a copper plate printing press. See Ostroff, E., (1969), *Journal of Photographic Science*, vol.17, pp. 101-105.

[5] Eder, Josef M., (1945), *History of Photography*, New York, Dover Publications, pp. 552-560.

[6] Ponton demonstrated that, as for silver chloride, the spectral sensitivity to light of potassium dichromate was mainly to the violet end of the spectrum. In Ponton, M. (1839), *Edinburgh New Philosophical Journal*, vol. 27, n.53, pp. 169-171

[7] Poitevin, A. (1862), *Traité de l'impression photographique sans sels d'argent, contenant l'histoire, la théorie et la pratique des méthodes et procédés de l'impression au charbon, de l'hélioplastie*, Paris, Leiber.

[8] Half-tone was a photomechanical printing process that translated the tones and details of a photographic image into a printed pattern of tiny dots.

[9] According to Mumford in the early period of mapping by lithography and other processes, the principal output was for the military and not for sale. In Mumford, I. (1972), *Cartographic Journal*, vol. 9, pp. 30-35.

using these new processes, the reduction of a map could be obtained with better results than the traditional ones[10]. James together with J. W. Osborne (1828-1902) also invented photozincography, in 1859, a planographic printing from zinc plates "*by which impressions were produced in an analogous way to photolithography*"[11] (fig. 1).

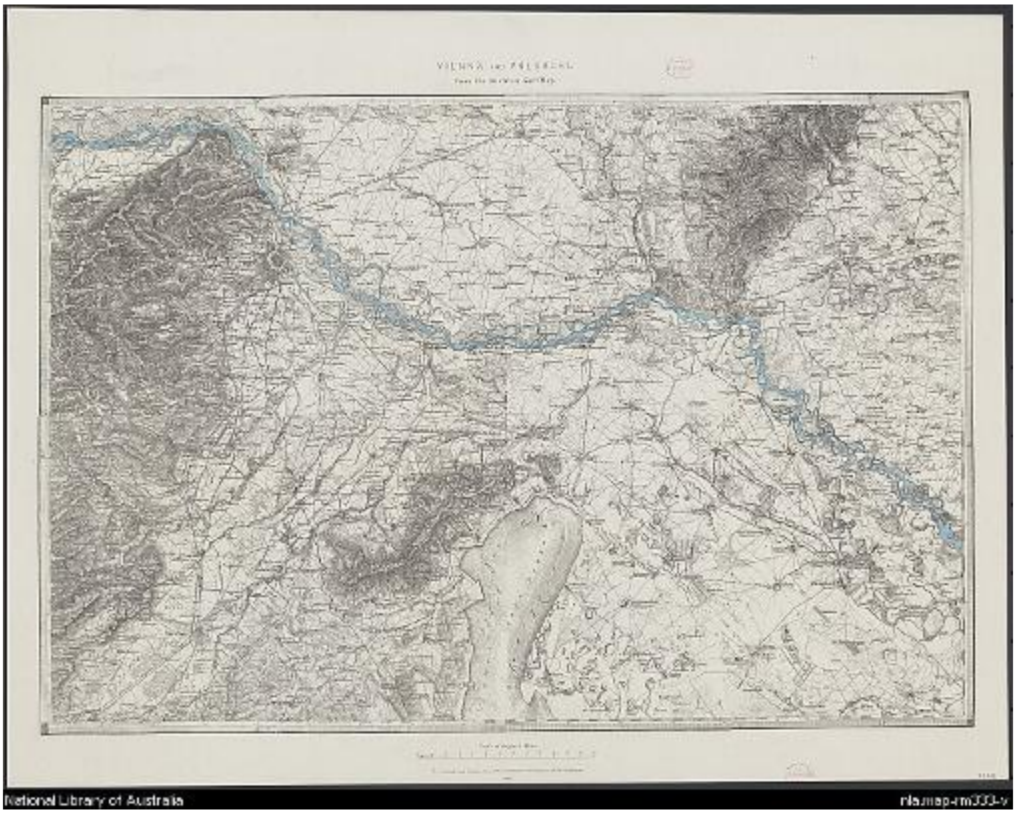

Fig. 1: Southampton: Photo-zincographed at the Ordnance Survey Office, Southampton, Col. Sir H. James, 1866.[12]

The Austrian-Hungarian Ordnance Survey was one of the first Geographical Institutes to adapt the new photomechanical methods for map reproduction. In the Universal Exhibition in Vienna (1873), "*an impressive collection of maps executed by heliogravure and photolithography was exhibited*"[13]. The Military Geographical Institute, founded in 1806 in Vienna, played an important role in the history of the graphic reproduction processes. This was mainly due to the work of Ottomar Volkmer (1839-1901), director of its photographic department and president of the Vienna Photographic Society and to his successor Baron Arthur Hübl[14] (1853-1932), a remarkable researcher on colour photographic processes.

The first cartographical cameras were installed in the Geographical Institutes throughout Europe around the 1860s. The main qualities demanded of these cameras were that they should be equipped with distortion-free lens and that parallel planes of copy and negative should be achieved[15]. However progress was slow due to the imperfection of the images obtained, derived mainly from the poor quality lenses available at the time. In 1859 John Henry Dallmeyer (1830-1883) formed his own company producing very high quality optical and photographic instruments[16]. By 1861 he had designed the

[10] Waterhouse, J. (Captain), (1878), *Op. Cit.*, p. 2.

[11] Nadeau, Luis, (2006), *in Encyclopedia of Printing, Photographic and Photomechanical Processes,* Vol. 1 & 2, Fredericton (Canada), 1st eBook ed., V. 1.1., p. 171.

[12] Southampton: Photo-zincographed at the Ordnance Survey Office, Southampton, Col. Sir H. James R.E., F.R.S. &c Director, 1866. 1 map ; 49.9 x 74.9 cm. Map of Vienna (Austria) and Presburg (Slovakia) regions showing cities and towns with relief shown by hachures. Source: National Library of Australia *http:/-/-nla.gov.au/-nla.map-rm333*, accessed at 7th March 2011.

[13] Davanne, L-A. (1875), *Bulletin de la Société Française de Photographie*, vol.21, pp. 264-265.

[14] Eder, Josef M., (1945), *Op. cit.*, p. 590.

[15] Koeman, C., *In* Woodward, D. (edit) (1975), *Op. Cit.*, p. 139.

[16] Gee, Brian (1998), Bulletin Scientific Instrument Commission, no. 56, p. 24.

triple-achromatic lens, an arrangement of three achromatic doublets. The Rapid Rectilinear lens, or RR lens, was introduced by Dallmeyer in 1866 and as Dallmeyer wrote[17], "*For copying purposes the rapid rectilinear is without a rival. It has already been supplied to all the government topographical establishments in Europe, India and Australia"* (fig. 2).

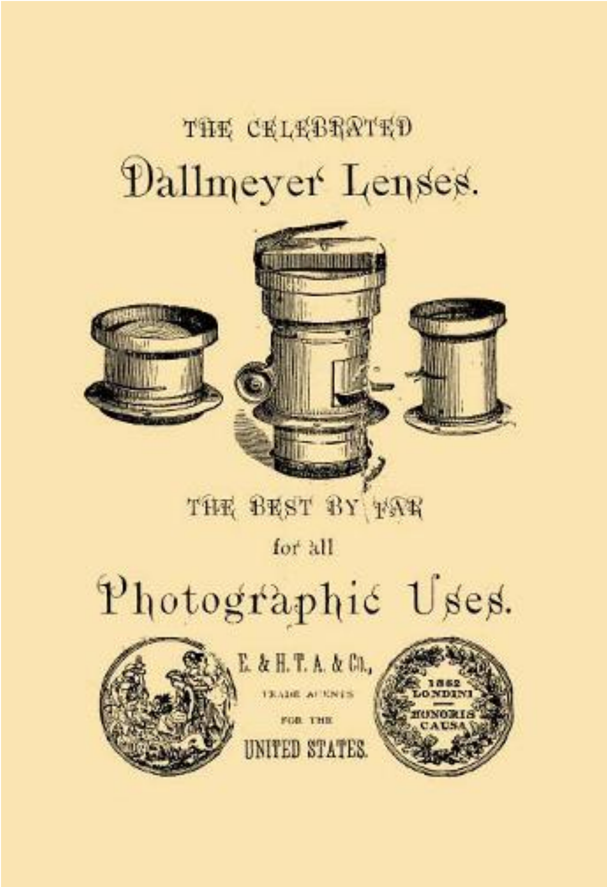

Fig. 2: Dallmeyer's cover book on photographic lenses.

Around the same time Hugo Adolphe Steinheil (1832-1893) invented a similar design to the RR lens, the aplanat, most probably due to his friendly relation with the Munich mathematician Phillips Von Seidel (1821-1896) who established at the time the theory of lens aberration (Seidel sums)[18]. Both lenses were constructed under the same principle, with a pair of achromatic lens element groups, arranged symmetrically before and behind the diaphragm. They were made of flint glass with an aperture of f/8. This lens construction reduced radial distortion[19] (figs. 3 and 4).

The camera was usually fixed on a stand, it could be moved along rails so that the distance of the camera from the plan-board where the map to be reproduced was, would be adjusted according to the scale required. With such a camera, a master copy of the map was duplicated, providing also the possibility of enlargements or reductions of certain parts of the map (fig. 5).

In the 1860s there were still some problems concerning the map printing processes to be solved[20]: adequate lighting (around 1860 when the camera was starting to be used for the cartographic work, electric lighting was an exception), substitution of heavy stones by a lightweight and cheap printing surface and better polychromatic printing. Some of the problems were, to some extent, partially solved the following years by several Surveys and geographical Institutes in Europe. The Portuguese Geographical Institute and the Dutch topographical Department at The Hague played an important role in this historical evolution in map printing during this period.

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[17] Dallmeyer, J. H., (1874), *On the choice and use of photographic lenses*, New York, E.& H. T. Anthony & Company, p. 21.

[18] Brachner, Alto (1987), *Bulletin Scientific Instrument Commission*, no. 12, pp. 3-7.

[19] Nugent, E. (1878), *Optics: light and sound*, London, Strahan Company, p. 161.

[20] Koeman, C., *In* Woodward, D. (edit) (1975), *Op. cit*., pp. 139 -145.

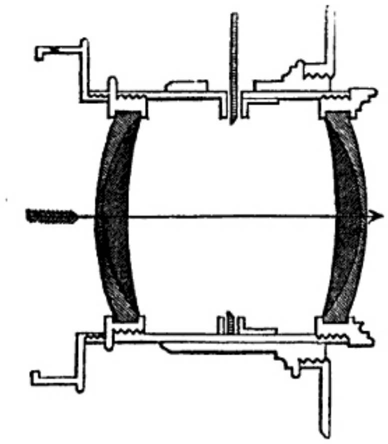

Fig. 3: Dallmeyer's rapid rectilinear lens (Nugent[19], 1870).

Fig. 4: Dallmeyer's lenses (IGP archive, no. M167 and M168).

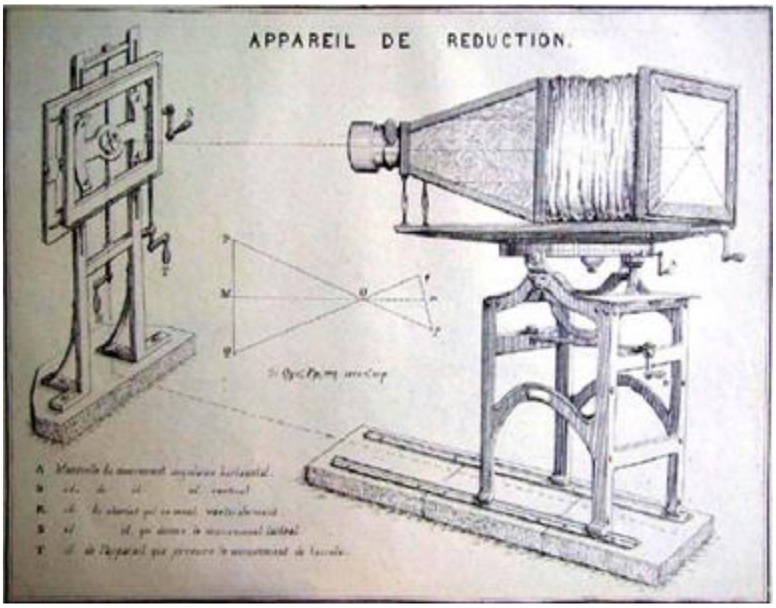

Fig. 5: Cartographic camera (Hannot[21]; 1881).

A case study: the Photographic Section of the Portuguese Geographical Institute and the colour printing of maps

In 1872 under the directorship of general Filipe Folque (1800-1874) the Photographic Department for cartographic reproduction is created in Portugal. Jose Julio Rodrigues (1843-1893), a professor of chemistry at the Lisbon Polytechnic School, is appointed its Director (1872-1879). The main photographic processes practiced there were the wet and dry collodion on glass (negative and positive) and the albumen silver print. Dichromated gelatin and bitumen of Judea were employed in several photomechanical processes such as photolithography, photogravure and typogravure using different supports (stone, copper and zinc). Rodrigues introduced a new material for his technique of photolithography: a thin piece of tinfoil was coated with an ammonia dichromated gelatin. This was put under the negative, and altering exposure to light it would give a photographic image. Greasy ink was then applied to the wet surface for a lithographic print. He considered this

[21] Hannot, A. (1881), *Communications de l'Institut Cartographique Militaire. N°17. Notice sur les travaux de la reproduction de la carte de Belgique au 20 000e et au 40 000e*., Bruxelles, A. Cnophs fils, p.34.

method the best for producing photolithographed maps as tin plates were inextensible and impermeable. In 1874, a communication about this process was sent to the *Société Française de Photographie* and was presented by Louis-Alphonse Davanne (1824-1912) at a meeting of the Society held on the 5th of June of that year; it was published in the society's bulletin[22]. Another important innovation done as early as 1878 by Rodrigues was the typo-autographic method for the lettering on maps: moveable types were printed on special paper which was cut out and pasted on to maps and reduced photographically[23] (fig. 6).

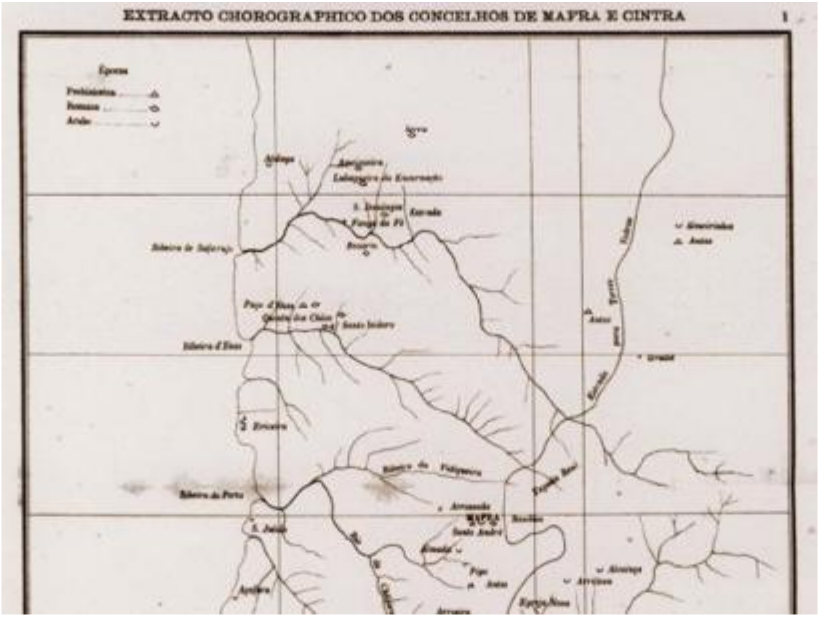

Fig. 6: Detail of *"Extracto Chorografico dos Concelhos de Mafra e Sintra"* done by the typoautographic process (IGP archive, c.a. 1878).

In the Portuguese Geographical Institute a big Dallmeyer camera with a focal distance of more than 3m was in use, allowing to obtain photographic reproductions of 75cm square, with either the triplet or the rapid rectilinear from Dallmeyer. With a second smaller Dallmeyer camera, photographs of 44cm square were obtained with a triplet objective. A Steinheil prism was also used for taking negatives for collotype printing. By placing a reverse prism in front of the lens, the image passed through the lens reversed and was impressed directly on the sensitive plate[24]. Usually these cameras were under a shelter receiving natural illumination. In very few cases, such as in the Portuguese Geographical Institute, artificial light was used. In this case electricity was produced by a magneto-electric Gramme machine[25]. This was probably the first establishment of its kind to use electricity for this purpose. Examples of printed maps from the Photographic Department are presented in fig. 7 and fig. 8.

Among other printing instruments the Photographic Department had a lithographic press constructed by H. Voirin (1827-1887)[26] (fig. 9 and 10). This press was a wide-angle model employed for the printing of black and white and chromo chorographic and topographic maps.

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[22] Rodrigues, J. J. (1874), *Bulletin de la Société Française de Photographie*, vol.20, p. 148.

[23] Rodrigues, J. J. (1879), *Procédés Photographiques et Méthodes Diverses d'Impressions aux Encres Grasses*, Paris, Gauthier-Villars, pp. 58-61.

[24] Rodrigues, J. J. (1879*), Op. cit.*, pp. 6-7.

[25] Rodrigues, J. J. (1879*), Op. cit.*, p. 7.

[26] Henri Voirin (1827-1887) was technical director at the instrument maker Rousselet - Normand. In 1860 he built in collaboration with Paul Dupont a lithographic press *«à cylindre avec système de repérage".*
It was awarded a gold medal at Universal Exhibition in Paris (1878) in http://www.imprimerie.lyon.fr/imprimerie/sections/fr/documentation/fonds/marinoni/marinoni/marinoni3, accessed at 15th March 2011.

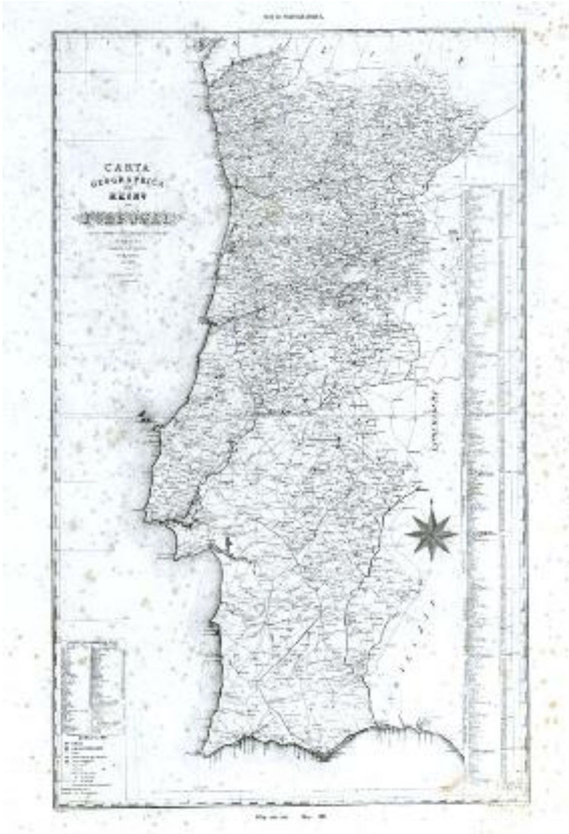
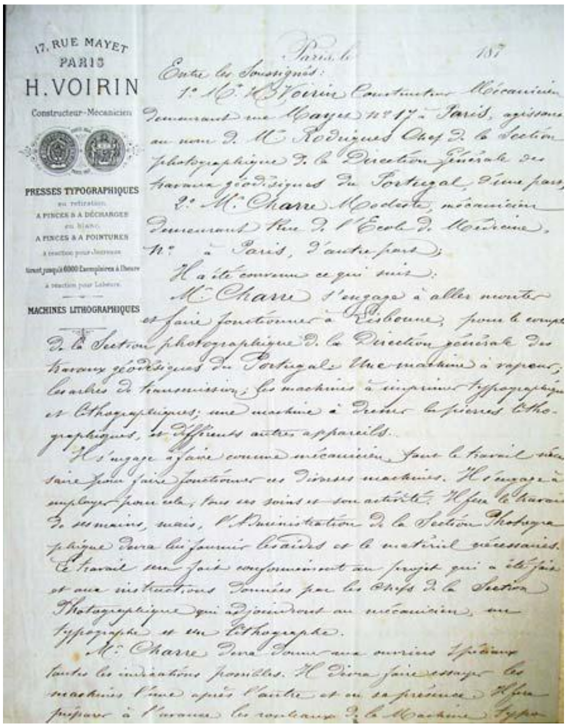


Fig. 7: Portugal's geographic map. Half-tone relief process in copper (IGP archive, c.a. 1878).

Fig. 8: Goa's geographic map in two different scales (IGP archive, 1876).

Fig. 9: Voirin's press, albumen print (IGP archive).



17, RUE MAYET
PARIS
H. VOIRIN
Constructeur-Mecanicien
PRESSES TYPOGRAPHIQUES
en retiration
A PINCES & A DÉCHARGES
en blanc
A PINCES & A POINTURES
à réaction pour Journaux
tirant jusqu'à 6000 Exemplaires à l'heure
à réaction pour Labeurs
MACHINES LITHOGRAPHIQUES

Paris, le 187

Entre les Soussignés:

1° Mr. H. Voirin Constructeur Mécanicien demeurant rue Mayet n° 17 à Paris, agissant au nom de Mr. Rodrigues Chef de la Section photographique de la Direction Générale des travaux géodésiques du Portugal, d'une part,

2° Mr. Charre Modeste, mécanicien demeurant Rue de l'Ecole de Médecine, N° à Paris, d'autre part;

Il a été convenu ce qui suit:

Mr. Charre s'engage à aller monter et faire fonctionner à Lisbonne, pour le compte de la Section photographique de la Direction générale des travaux géodésiques du Portugal: Une machine à vapeur, les arbres de transmission; les machines à imprimer typographiques et lithographiques; une machine à dresser les pierres lithographiques, et différents autres appareils.

Il s'engage à faire comme mécanicien tout le travail nécessaire pour faire fonctionner ces diverses machines. Il s'engage à employer pour cela, tous ses soins et son activité. Il fera le travail de ses mains, mais, l'Administration de la Section Photographique devra lui fournir les aides et le matériel nécessaires. Ce travail sera fait conformément au projet qui a été fait et aux instructions données par les Chefs de la Section Photographique qui adjoindront au mécanicien, un typographe et un lithographe.

Mr. Charre devra donner aux ouvriers [illegible] toutes les indications possibles. Il devra faire essayer les machines l'une après l'autre et en sa présence. Il fera préparer à l'avance les rouleaux de la Machine Typo-

Fig. 10: Voirin's letter to J. J. Rodrigues, 1876 (IGP archive).

The drawbacks and costs of printing polychromatic maps in the traditional way were quite high as it implied the use of many stones. A new process of map colour printing with the aid of photography was devised by Charles Eckstein, director of the Dutch Topographical Department at The Hague. The combined prints of 4 stones (one for each primary colour: red, blue and yellow and one for black), allowed the printing of up to forty colours and tints. A carbon copy of a negative in wet collodion of the map was transferred to the light-sensitive surface of each stone. The stone was covered with an acid resistant coating of wax and bitumen of Judea. The hardened coating was put under a ruling machine which would cut grooves crosswise with parallel lines (8 to 20 lines on a millimeter). After being etched by a solution of diluted nitric acid, through a selective timing process, different tints of each colour would be produced on each stone after being rubbed on with one of the primary colours (blue, yellow and red) or black. Afterwards the stone was ready for the lithographic impression (fig. 11). The etching process was also applied to contoured maps (fig. 12). This half-tone process was similar to the one patented in 1882 by the German printer George Meisenbach (1841-1922)[27].

Eckstein attended the International Exhibition at Philadelphia (1876) presenting there a booklet with samples of maps and the description of his technique[28]. In the Universal Exhibition at Paris (1878) he exhibited maps photomechanically printed in colour. The Photographic Department of the Portuguese Geographical Institute and Rodrigues were also present at these two international exhibitions. The two cartographic institutions exchanged correspondence and a very skilled Portuguese cartographer, Manuel Dias dos Santos, was sent to The Hague to learn the Eckstein process[29].

Rodrigues improved an analogous photochromolithographic process, named chromocuprography, which used a copper plate instead of a stone as the support surface; as an etching solution he used a solution of iron (II) chloride (commonly known as perchloride of iron) and improved the halftone process by adding iron (II) oxide. These chromocuprographic essays were exhibited at the Universal Exhibition in Paris[30] (fig. 13).

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[27] Eder, Josef M.,(1945), *Op. cit.*, pp. 630-632.

[28] Eckstein, C. (1876), *New Method for Reproducing Maps and Drawings*, Hague, Giunta d'Albani.

[29] Manuscript Letter sent to Rodrigues from J. Klerck, Minister of War, the Netherlands (IGP Archive).

[30] Exposition universelle, Paris (1878), *Rapports du jury international*, group II, Class 16, p. 371.

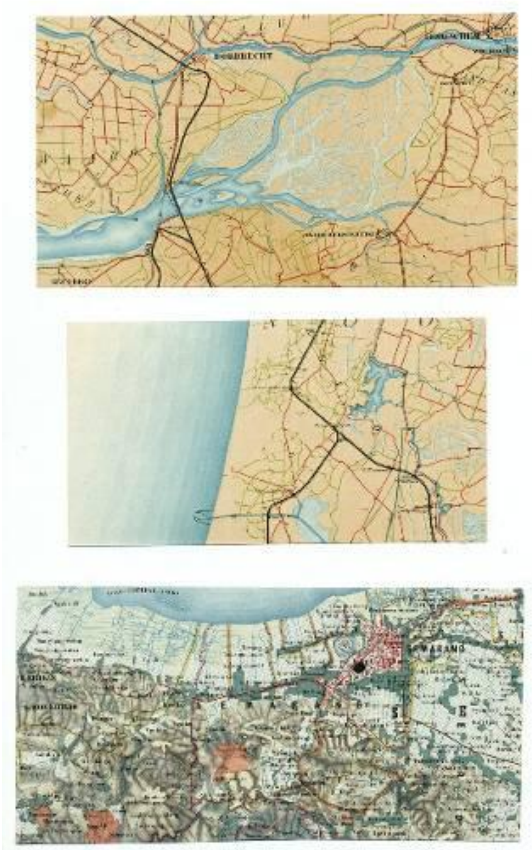

Fig. 11: Dutch photochromolithographic topographic maps (Eckstein,1876). (IGP archive).

Fig. 12: Topographic map (Eckstein; 1876).

The French researcher and photographer Louis Ducos du Hauron (1837-1920) attended the Universal Exhibition at Paris (1878), presenting several specimens[31] of his invented colour photographic system based on the trichromatic subtractive method[32]; he was awarded a silver medal. Hauron intended to produce low-cost three colour book illustrations using his indirect carbon colour separation process and in the 1870s developed with success the collotype technique with dichromated gelatin on glass and on copper. After several experiments he sent in 1891 a communication to the *Academie de Sciences de Paris, "Photography of colours: photomechanical reproduction of colours in unlimited number of copies"*[33] and filed a patent in 1892.[34] The principle of superimposing red, yellow and blue images was similar to the Eckstein process. The pigments were carefully chosen according to their compatibility with the wax used in the photographic technique and for their spectral qualities. They were organic (madder alizarin for red) and inorganic (lead chromate for yellow and ferric ferrocyanide for blue).

The research done on the development of cartography in the 19th century would allow us to conclude that the invention of photography and photomechanical processes were fundamental in the reproduction, scaling and colour printing of maps and charts. Furthermore:

- Photolithography and photozincography were mostly applicable to all maps in dot or line, except for very fine drawings; planographic prints from zinc displaced the use of lithographic stones as these were heavy and difficult to obtain in big sizes; the photoengraving methods were suitable for high quality printed maps.

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[31] Exposition universelle, Paris (1878), *Rapports du jury international*, Group II, class 12, p. 34.

[32] Du Hauron, L. Ducos (1878), *Traité pratique de photographie des couleurs, système d'heliochromie*, Paris, Gauthier-Villars et fils.

[33] The full description of du Hauron's methods is in the book written by his brother Alcide, a scientist who collaborated with Louis Ducos du Hauron ; Du Hauron, A. Ducos (1897), *La triplice photographique des couleurs et l'imprimerie*, Paris, Gauthier-Villars et fils.

[34] French Patent of 15 years under No. 233,817, 20th August 1892.

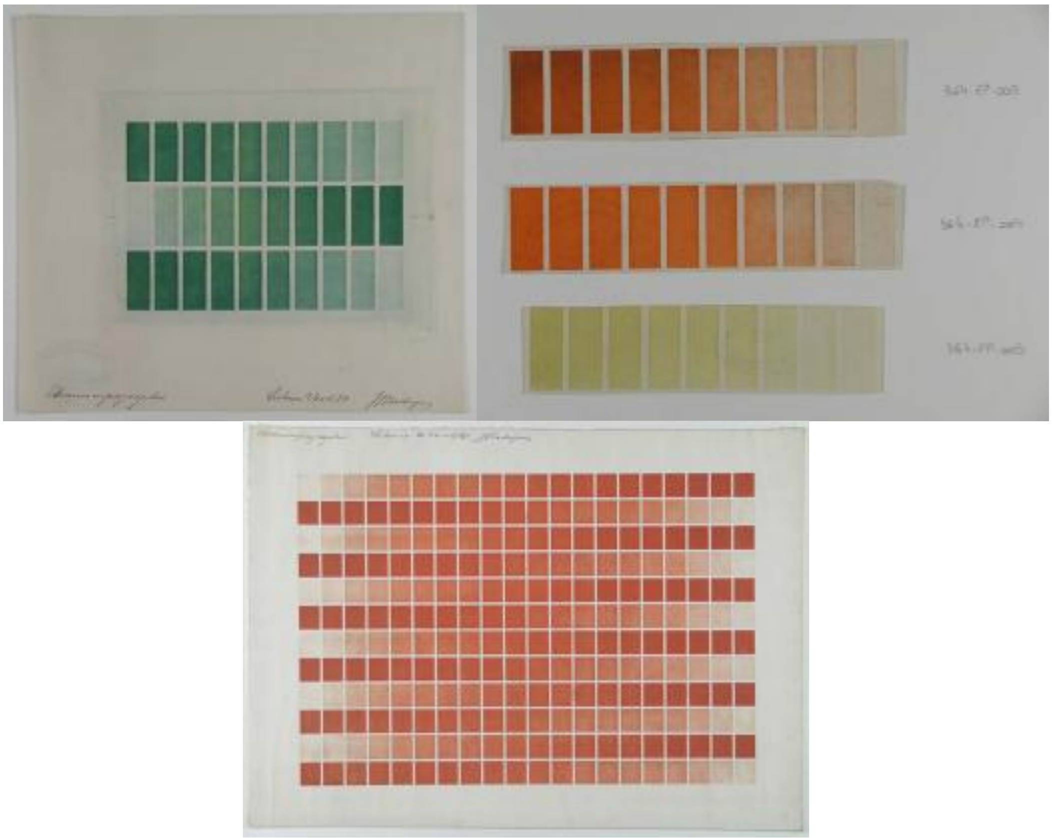

Fig. 13: Chromocuprographic essays (J. J. Rodrigues, 1877).

- Optical and photographic instruments were specially designed for the 19th-century cartography. However even before special cartographic cameras were installed, photography was also used to transfer line drawings into the sensitized stone or plate.
- Colour printing was essentially for geological maps. The Eckstein process, although time-consuming, was probably one of the best available at the time. This process was used in several European countries and received numerous awards in International Exhibitions at Philadelphia (1876) and at Paris (1878).
- The Photographic Department of the Portuguese Geographical Institute was considered one of the best in Europe. Important 19th-century photographic journals and books as well as geographical reports attested to its achievements. Several innovations on cartographic printing were introduced by José Julio Rodrigues: the use of tinfoil for photolithography, the typo-autographic process for the lettering on maps. He also did some improvement in the half-tone phototypographic process[35].

Acknowledgements:

We are most thankful to the *Portuguese Geographical Institute (IGP)* and to the *Société Française de Photographie*, for their invaluable assistance in the consultation of the respective archives and libraries. A grant from the *Fundação da Ciência e Tecnologia (FCT)* is acknowledged.

[35] Waterhouse, J. (Captain), (1878), *Op. cit.*, p.66; see also: Gravier, C. (1893), *Bulletin du Photoclub de Paris*, p. 42.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 1020.

MOSCOW SOCIETY OF FRIENDS OF NATURAL SCIENCES AND DISSEMINATION OF SCIENTIFIC AND TECHNOLOGICAL KNOWLEDGE IN 19TH-CENTURY RUSSIA

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Abstract

The Moscow Society of Friends of Natural Sciences, established by Moscow University in 1863, played a major role in popularization of science and technology in Russia. It was one of the first Russian learned societies to adopt large-scale scientific exhibitions as means of dissemination of scientific and technological knowledge. The exhibitions were aimed at popularization of certain branches of scientific knowledge that were developing or had been developed in the West but were unknown to Russian public. All of them were accompanied by scientific congresses to which eminent foreign scientists were invited. All provided Moscow with new science museums.

It is common knowledge that Western science was introduced into Russia by Peter the Great. Obviously, it was not an easy task, as in Peter's times in Russia, to cite A. Vucinich, "The scientists were few in number and their scholarly skills were rudimentary, to say the least".[1] Peter established in St-Petersburg an Academy of Science and Arts with gymnasium and university, which were to lay the basis for scientific inquiry in the country, to educate Russian scholars and to overcome generally negative attitude toward science and learning in the upper strata of Russian society and to interest lower classes in education and learning. He also invited to Russia a number of reputable European scientists, among them Nicolaus and Daniel Bernoulli, Christian Goldbach, George Bernhard Bilfinger, Joseph-Nicolas Delisle and many others. These efforts led to significant development of national science and education. During the next century a whole number of universities (among them Moscow University, 1755; Kazan and Kharkov Universities, 1803, *et al.*), schools (Konstantin Surveying School, 1779; Central Peoples School with Teachers' Seminary, 1783, *et al.*) and learned societies (Free Economic Society, 1765; Moscow Society of Naturalists, 1805; St. Petersburg Mineralogical Society, 1817; Russian Geographic Society, 1845, and many

[1] Vucinich A. *Science in Russian Culture. A History to 1860*. Stanford, Calif.: Stanford University Press, 1963, p. 69.

other) appeared in Russia and the practice of inviting foreign professors and academicians gradually came to an end. Nevertheless the problems inherited from the previous period still persisted.

The most serious hindrance to the further progress of science was the generally accepted attitude that science was a kind of foreign matter, strongly alien to Russian mind, all the more so as the languages, adopted in Russia for presentation of scientific ideas were Latin, French and German. In the beginning of the 19th century Academician Johann Philipp Krug (he was an expert in Russian history and numismatics) complained that in Russia there was no public for scientific writings, and that "generally everything that even savors of learning and exact research frightens readers here or is lamely omitted."[2] It is sticking enough, that even in the second half of the 19th century considerable part of Moscow liberal professors shared the idea that Russian people were not ripe for the understanding of intricate scientific ideas and deprecated organization of scientific exhibitions and international scientific congresses in Moscow.[3] So when in 1863 Professor of zoology and Director of Zoological Museum of Moscow University Anatoly Bogdanov established his Society of Friends of Natural Sciences (since 1867, Imperial Society of Friends of Natural Sciences, Anthropology and Ethnography –OLEAE) that, according to its Regulation,[4] was aimed at dissemination of scientific knowledge but not only at scientific research and advancement of natural sciences; adopted Russian as the only language of its publications; and proclaimed wide membership not limited by age, gender, social position or educational level (the only condition was that any member had to share the society's interests and aims and participate in its activities), it was no wonder, that OLEAE met strong opposition on the part of some Moscow professors and certain members of Moscow Society of Naturalists. They considered this to be destructive for Russian science and coined the new society not "friends" (*ljubiteli*) but "fiends" (*gubiteli*) of natural sciences.[5]

Nevertheless, Bogdanov managed to find support in many reputable Russian scientists, among them geologist, Professor of geognosy and paleontology of Moscow University Grigory Shurovsky who found among charter members of the society understanding and appreciation of his own ideas concerning development and dispersal of scientific knowledge in Russia. Shurovsky believed that "accessibility or popularization of natural sciences" was "a requirement of any educated country" and that "popularization of science is a method to influence the thinking of the whole modern society".[6] At the first meeting of OLEAE on May 14th, 1864 he was elected the first President of the new society and occupied this post for about two decades. In his Presidential address he emphasized: "…It is not deficiency in special works or scholarly issues that we feel. It's rather in people that could use these issues and further develop them…".[7] And the Society under leadership of Bogdanov and Shurovsky did its best to attract to scientific research as many people as possible. Scientific and especially popular activities of OLEAE also met with support from participants of the First Congress of Russian Naturalists (December 28, 1867–January 4, 1868) and especially its President, zoologist K.F. Kessler. The Congress even voted an appeal calling all Russian universities to establish learned societies on the model of OLEAE.

The first steps of the new society in popularization of scientific knowledge were not too impressive. At the first meeting of OLEAE Shurovsky offered that the society should organize excursions for people who were interested in natural sciences and presented "Rules for Geological Excursions", describing in detail the necessary equipment, sites of geological interest in the vicinity of Moscow, methods of geological research and presentation of geological data. In a year they were followed by "Brief Rules for Collecting Natural Objects", including zoological, botanical, paleontological and mineralogical collections,[8] prepared by a number of society members (A. Bogdanov, N. Senger, A. Fedtchenko, N. Kaufman and others), who also compiled educational collections of plants, insects, fossils, that could be bought from some Moscow booksellers. Nevertheless, Shurovsky's geological excursions, which started in the summer of 1864, were the most popular in Moscow. Shurovsky himself mentioned in his lecture at the First Congress of Russian Naturalists that sometimes these excursions announced in Moscow newspapers gathered up to 200 people of various age and rank and, in spite of the difference in their educational level; he always managed to keep up their interest. At the end of the excursions, to cite Shurovsky, "I invited the public to offer their questions to make clear the things that seemed to them not evident enough, and then started confabulation…"[9]

[2] Cited from: Val'kova O.V. 'Jazyk fundamental'noi nauki v Rossii' [Language of fundamental science in Russia]. In: *Rossiiskaya Akademiya Nauk. 275 let slugeniya Rossii*. Moscow: Janus-K, 1999, p. 324.

[3] Krivosheina G.G. 'Liberaly i conservatory v Moskonskom universitete' [Liberals and conservatives in Moscow University]. In: *Istoria sociocul'turnykh problem nauki i tekhniki*, No 2. Moscow, 2004, pp. 139–162.

[4] [Regulations of the Society of Friends of Natural Sciences]. In: *Izvestiya Obshestva Luybitelei Estestvoznaniya*, 1866, vol.3, No 1.

[5] Krivosheina G.G., 'Liberaly i conservatory…'; Raikov B.E. 'Anatoli Petrovich Bogdanov'. In: *Russkie biologi-evolutsionisty do Darvina* [Russian biologists-evolutionists before Darwin], vol. 4. Moscow-Leningrad: AN SSSR, 1959, pp. 203–467.

[6] Shurovsky G.E. 'Ob obshedostupnosti ili popularizatsii estestvennykh nauk' [On accessibility or popularization of natural sciences]. In: *Trudy Pervogo s'ezda russkikh estestvoispytatelei*. St-Petersburg, 1868.

[7] [Minutes of the Society of Friends of Natural Sciences]. In: *Izvestiya Obshestva Luybitelei Estestvoznaniya*, 1866, vol.3, No 1, col. 7.

[8] *Ibid*., col. 123–134.

[9] Shurovsky G.E. '*Ob obshedostupnosti*…'

In 1864 OLEAE received as donation from Moscow archeologist and publicist A.Gatsuk two strangely looking skulls found during excavations of burial mounds in Podolsky uezd, not far from Moscow. This was the beginning of Bogdanov's interest to anthropology, which he considered to be an important part of zoology in general. Bogdanov was full of enthusiasm to compile a craniological collection of ancient inhabitants of Russia and Moscow region in particular. And though this idea was not completely new for Russian science (it was time and again expressed by Academician R. Baer, the first honorary member of OLEAE) Bogdanov with all his energy set this plan in motion. Together with some members of the society, he managed to do a really incredible thing –to make a whole number of Russian landowners explore their estates in search of burial mounds and grant permission for excavations on their lands. Some of them got so interested in anthropology, that they made excavations themselves (in spite of the fact that the Orthodox church would think this a highly questionable occupation) and sent to the society the skulls they found (this is testified by numerous letters published in OLEAE minutes). As a result, during the summer of 1865 OLEAE's anthropological collection was supplemented by 300 new items. Essentially this was the starting point of regular research in physical anthropology in Russia.

Also in 1864, when the Division of Anthropology was established in OLEAE, the society at Bogdanov's suggestion put forward an initiative to organize an Anthropological exhibition in Moscow and to establish a Department of Anthropology with Anthropological Museum by Moscow University.

Bogdanov already had some ideas concerning exhibitions. When in 1857 he made his first trip to Europe, he visited London and was greatly impressed by the Chrystal Palace and its exposition.[10] By that time this famous exposition hall of the 1851 Great Exhibition had been already moved from Hyde Park to Sydenham in the south outskirts of London. He derived from the story of the Palace some principal conclusions. First of all, he realized that an exhibition was a good means for popularization of scientific knowledge as it actually could attract a lot of people belonging to different social classes and having different educational levels, and that exhibitions brought money that could be used to develop certain fields of scientific education and research (Victorian England had already proved this by constructing the so called Museum land in South Kensington).

Besides that, he had some personal experience in exhibition-making.[11] As secretary of the Committee of Acclimatization of Animals and Plants of the Moscow Society of Agriculture, he organized two Acclimatization Exhibitions in 1858 and 1863. Both of them were a success, but if the first one was rather unpretentious, the Second Acclimatization Exhibition, held in the Moscow Manezh in 1863 was extraordinarily representative by Moscow standards. It was a real event (actually it was the first large exhibition in Moscow) and, what was more important, it helped the Committee to gather enough money to start the construction of the Moscow Zoological Garden.

It's obvious enough why Bogdanov suggested organizing an Anthropological Exhibition: it had to popularize anthropology as a science in Russia and it also had to bring money for the new Department of Anthropology. Unfortunately due to some financial and technical reasons, the society had to change its plans and the anthropological exhibition turned into an exhibition of ethnography and anthropology, and finally into an ethnographic exhibition with a small anthropological section (the Anthropological Exhibition was held in Moscow only in 1879).[12] So the first exhibition held by OLEAE was the All-Russia Ethnographical Exhibition. It was the first and very successful attempt of a large-scale scientific exhibition undertaken by OLEAE. During preparatory work for this exhibition the society worked out basic principles of its exhibition-making strategy which was used during organization of the two other great scientific exhibitions held by the society (Polytechnic Exhibition, 1872 and Anthropology Exhibition, 1879) and more unpretentious Zoological and Geographical Exhibitions of 1892.

In brief, these principles can be represented as follows:

1. Scientific exhibitions were aimed at popularization of certain branches of scientific or technological knowledge that were developing or had been developed in the West but were alien to the Russian public.
2. OLEAE attracted to preparatory work as many people as possible. Though the exhibitions were held in Moscow the society tried to encourage and instruct people even from far off regions of the country to collect necessary exhibits. This was especially helpful in the recruitment of new members and in establishing new affiliations in different parts of the country (in this OLEAE followed the example of the Russian Geographical Society which had its branches all over Russia). Sometimes these contacts were very useful for the society itself, as it was with the famous Turkestan expedition of A. Fedtchenko, which became possible only due to Turkestan governor K. von Kaufman, an active participant of the Ethnographical Exhibition.

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[10] Bogdanov A.P. 'Zametki o zoologicheskikh sadakh' [Notes on zoological gardens]. *Izvestiya Obshestva Luybitelei Estestvoznaniya*, 1876, vol. 25, No 1, pp. 1–40.

[11] See: Raikov B.E. 'Anatoli Petrovich Bogdanov'…

[12] On the history of these two exhibitions see: Raikov B.E. 'Anatoli Petrovich Bogdanov'…; Krivosheina G.G. 'Rol' A.P. Bogdanova v razvitii zoologii v Rossii' [Role of A.P. Bogdanov in development of zoology in Russia]. In: *Istoria sociokul'turnykh problem nauki i tekhniki*, No 3. Moscow, 2004.

3. Before and during the exhibitions high-level lectures on the topic for educated public were combined with gratuitous scientific demonstrations and lectures for people with lower educational level.
4. Decoration of exhibitions was always impressive thus helping to memorize the main ideas of the exhibition: beautiful mannequin groups in traditional natural or household environment, giant plants, dinosaurs, mammoth, etc. Maybe it was not too scientific, but certainly memorable.
5. Each exhibition was accompanied by important scientific or cultural events. They also served as a good means to demonstrate achievements of Russian science as eminent foreign scientists were invited. In this respect political events, associated with exhibitions, appeared to be less effective, as it happened at the Ethnographical Exhibition, which turned into bacchanalia of Slavophilism.
6. Each exhibition provided Moscow with new science museums (Dashkov Ethnographical Museum, housed together with Rumyantsev Museum in the Pashkov House, the Polytechnical Museum, and the Museum of Anthropology of the Moscow University).

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 1024.

THE DIFFUSION OF NEW PAINTING MATERIALS IN 19TH-CENTURY PORTUGUESE TECHNICAL COURSES: THE CASE OF THE OPORTO INDUSTRIAL SCHOOL

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Abstract

Technical education in Portugal had its official beginning in 1852. Oporto Industrial School (in present time known as Instituto Superior de Engenharia do Porto – ISEP*) was then created. Its history is a testimony of the troubled early life of industrial education and training in the country. The survey of its historical archives and museum allowed pigments designation study and prices' comparison between traditional and modern synthetic products in the last years of the 19th century. These were analysed by colour range. The compilation of brands and provenance in fine arts materials acquisitions was also possible.*

Introduction

The outset of industrial and technical education in Portugal can be traced back to 1759 with the implementation of *Aula do Comércio* (Commerce lecture) in Lisbon, followed by nautical science (1765 in Oporto) and drawing (1779 in Oporto and 1781 in Lisbon). The *Academia Real das Ciências de Lisboa* (Lisbon Royal Academy of Sciences) is created in 1779. The outset of *Sociedade Promotora da Indústria Nacional* (National Industry Promotional Society) occurs in a politically and economically troubled period in 1822. In 1836 the polytechnic experiences are adopted in the country. The importance of secondary education is recognized and sciences applied to the arts and crafts are regarded as means to prosperity. In 1837 the *Conservatório Portuense de Artes e Ofícios* (Oporto Arts and Crafts Conservatory) and the *Academia Politécnica do Porto* (Oporto Polytechnic Academy) appear in Oporto, Portugal's industrial capital during the 19th century. The *Associação Industrial Portuense* (Oporto Industrial Association) is implemented in 1849. This association aimed at reaching prosperity through instruction of working class and the preparation of craftsmen. In order to achieve this, a school was also created. Finally in 1852, legislation determined officially the beginning of technical and industrial education and private initiatives were abandoned. Two schools appear in the following year: Lisbon Industrial Institute and Oporto Industrial School. Both schools suffered several reorganizations during the 19th and first half of the 20th centuries. Yet throughout this period, among other areas of study, courses and curricula of Oporto school included drawing, machines and models design, geometry, painting techniques or decorative painting as well chemical technology and chemistry applied to arts such as dyeing, stamping or ceramics. Pragmatism, labour market orientation and learning by imitation were some principles adopted. The School had a museum and laboratories for instructional purposes (fig. 1) [1-4].

Fig. 1: *Laboratorio Chimico* (chemistry laboratory) of Oporto Industrial School in the second half of the 19th century.

Integrated in a broader project concerning the introduction and circulation of new painting materials in 19th-century Portugal, the study of the historical archives of Oporto Industrial School/*Instituto Superior de Engenharia* (ISEP) and its museum allowed the compilation of information concerning materials, their prices and places of acquisition (figs. 2-7) in the last quarter of the century. Objects in the collection, cash books, bills and catalogues were elements analysed and discussed within the European context.

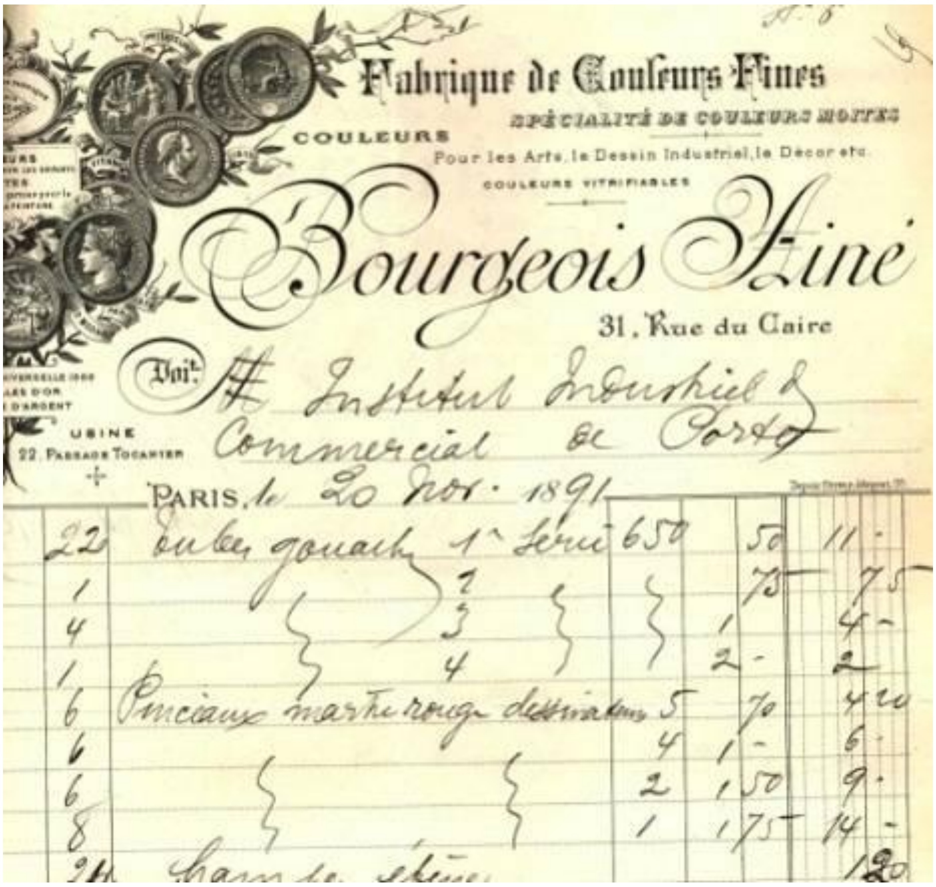


Fig. 2: Receipt of materials' purchase to *Bourgeois Ainé* (Paris) 1891 (detail).

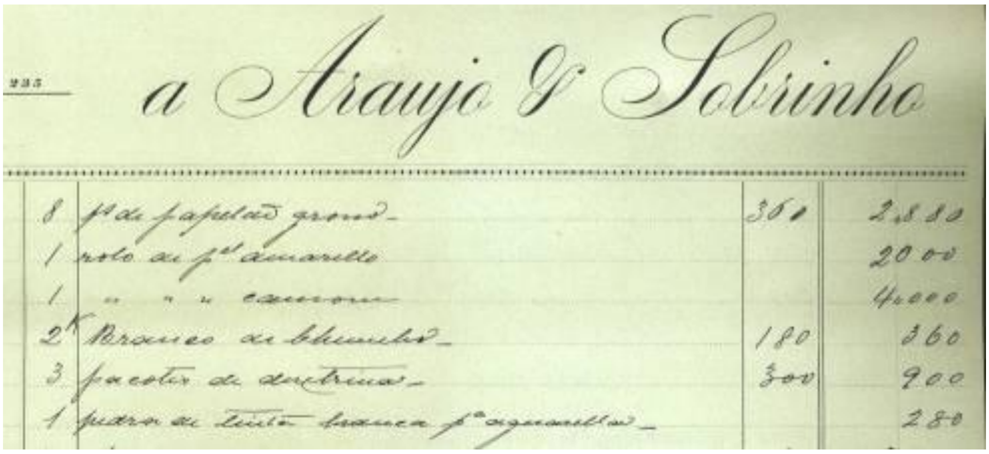
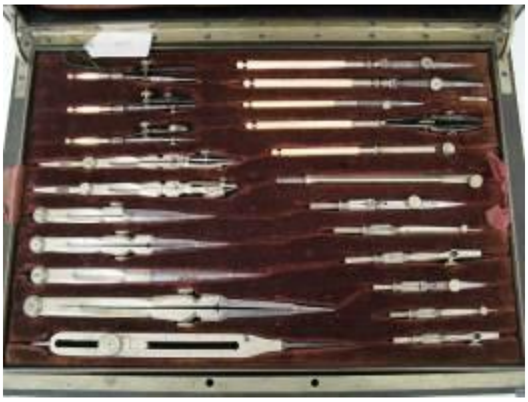


Fig. 3: Receipt of materials' purchase to *Araújo & Sobrinho* (Oporto) 1896 (detail).



Figs. 4 and 5: Drawing set (left) and detail of its brand, J.&A. Molteni (right).

Figs. 6 and 7: Watercolour set (left) and detail of its brand, Reeves & Sons (right).

New pigments synthesis in the 19th century

The discovery of new elements such as cobalt (Brandt, 1739), zinc (Marggraf, 1748), strontium (Crawford, 1790), chromium (Vauquelin, 1797), Barium (Davy, 1808), cadmium (Strohmeyer, 1817) led to the discovery of new pigments mainly in the period between 1770 and 1900 [5]. The synthesis of modern colours answered some basic and urgent issues: the need of a broader palette (chromium, cadmium and cobalt-based colours allowed the enrichment or replacement of poor options in the ranges of yellow, green and blue colours) and the need of less costly materials (e.g. ultramarine blue, the only good blue pigment before synthetic pigments appeared was extremely expensive) (table 1). The textile industry demanded cheaper and larger quantities of new dyes and the growing awareness of some traditional colour materials' toxicity (pigments based in lead, mercury, arsenic, for instance) gave rise to the search of less or non-harmful colour agents [6-8].

Table 1: Main new synthetic pigments developed in 1700-1900.

Colour	Pigments	Year of discovery, author
WHITE	Zinc white Barium white	1781, Guyton de Morveau 1782, Guyton de Morveau
BLUE	Prussian blue Cobalt blue Artificial ultramarine Cerulean	1704-1707 Diesbach and Dippel 1803-1804, Thenard 1826-1828, Guimet 1789, Hoepfner
YELLOW	Mars yellow Turner or patent yellow Zinc yellow Chromium yellow Barium yellow Strontium yellow Cadmium yellow Cobalt yellow	1780, de Massoul? 1775, Scheele c.1800, Vauquelin 1804, Vauquelin 1809, Vauquelin 1809, Vauquelin 1817, Strohmeyer 1831, Fischer
GREEN	Scheele green Schweinfurt green Chromium oxide green Viridian Cobalt green	1775, Scheele 1814, Sattler 1797, Vauquelin 1838, Pannetier 1780, Rinmann
RED	Mars red Chromium red Silver red Iodine scarlet Cobalt violet Manganese violet	Early 19th century 1804-1809, Vauquelin 1807, Chaptal 1811, Courtois 1859, Salvétat 1868, Leykauf
BROWN and BLACK	Prussian brown Prussian black	Unknown unknown
All colours	Synthetic dyes	

Pigments in Oporto Industrial School: a 1889-1896 portrait

The survey and the data collected led to the study of prices and their relation among each colour range. Only pigments whose price per weight unit (kilogramme) was determinable could be considered. For the majority of the colours analysed, this meant the period from 1893 to 1896. The exception was yellow colours; the gap for these was 1889-1896. Pigments' historical names could also be compiled. The French names collected correspond to imported materials by *Araújo & Sobrinho*, a shop located in Oporto that began its activity in 1829 as a paper dealer and evolved selling a variety of goods. They represented *Winsor & Newton* in the second half of the century. Although the brand of these pigments was not mentioned in receipts, the fact that their names are written in French means that they also received different brands and from other countries of provenance, namely France, since the conversion is made from *francs* to *reis*, the Portuguese currency in late 19th century.

White pigments

Two classes of white pigments could have their price per kilogramme (Kg) determined. Among traditional whites, lead white appeared under the names *branco de chumbo* and *blanc d'argent* (*alvaiade* was also found but no data to establish unitary price was available). Zinc white was the only modern white present, under the names *branco de zinco* and *blanc de neige*.

At the end of the 19th century traditional and modern whites could be equally expensive, depending on their quality (average prices: $345/Kg –*blanc de neige*/zinc white; and $368/Kg –*blanc d'argent*/lead white). Yet modern zinc white ($190/Kg –*branco de zinco*) could be less costly than lead white ($240/Kg –*branco de chumbo*) (fig. 8). This is confirmed by importation in commerce statistics, price regulation of pharmacies' goods and other publications. Zinc white price dropped and lead white's was rather stable throughout the century [9-10].

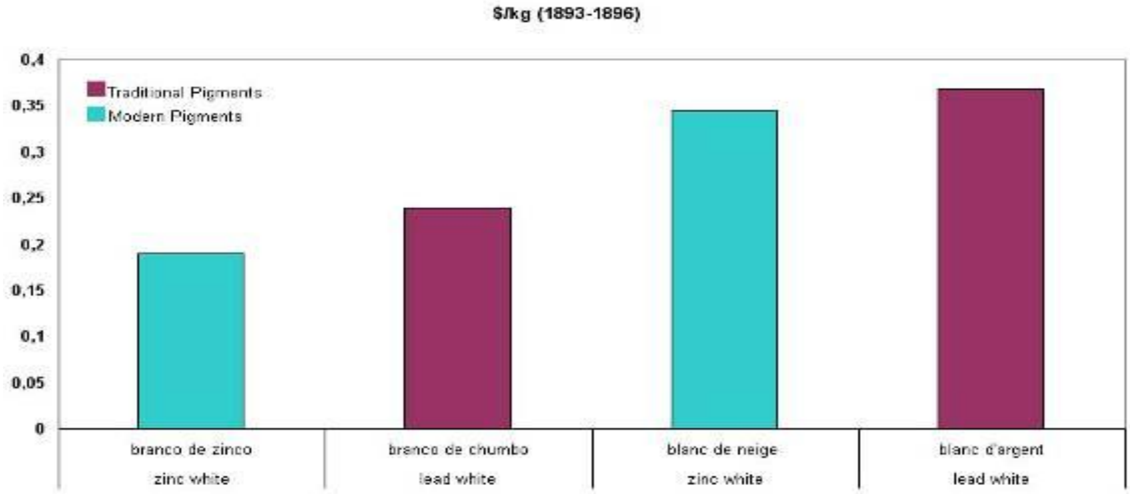


Fig. 8: White pigments: prices per weight unit (*reis / kilogramme*).

Blue pigments

Among traditional blues, only indigo could be priced per unit weight. It was named *azul indigo*. Blue ashes, mentioned as *cinza azul*, were also bought, but price/Kg was not possible to calculate. Four different classes of blue synthetic pigments were found: Prussian, artificial ultramarine, synthetic dye and cobalt blue. Prussian blue appears as *azul da Prússia*, *azul mineral* and *bleu de Chine*. Artificial ultramarine blue is named *azul ultramar* and *bleu outremer*. These could be classified as the artificial version of ultramarine because of their distinctive low price. A blue synthetic dye was also acquired, *bleu lumière*. Cobalt blue is referred as *azul cobalt*.

At the end of the century, there is a clear dominance of modern pigments in blue colour range. Artificial ultramarine was then one of the cheapest blue pigments (average price $700/Kg –*azul ultramar*/artificial ultramarine). Prussian blues prices showed larger variations depending in their purity and manufacture. *Bleu de Chine*/Chinese Blue, a high quality Prussian blue, was the dearest in this class (3$825/Kg) [11-13]. Cobalt blue, as other cobalt pigments, was confirmed as an expensive product still. Indeed, sold at average price 12$900/Kg, it is the most expensive blue pigment, 18.4 times more expensive than *azul ultramar*. Indigo, the only traditional blue pigment included was an expensive material too, sold at 9$600//Kg (fig. 9).

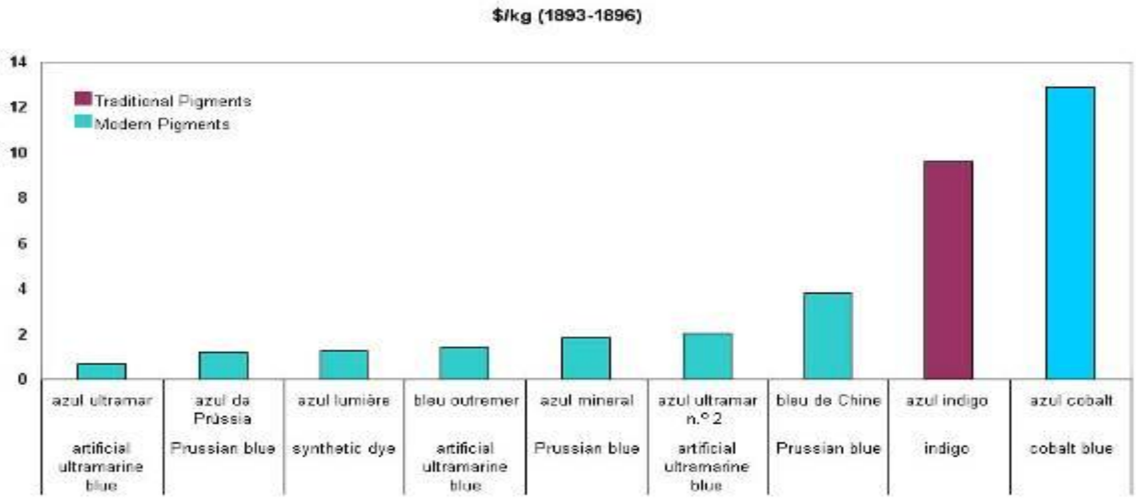


Fig. 9: Blue pigments: prices per weight unit (*reis / kilogramme*).

Yellow pigments

Four traditional classes of yellows were found: yellow ochres, Naples yellow, Indian yellow and gamboge. Yellow ochres are *ocre*, *ocre fino*, *ocre amarelo*, *amarelo de Itália* and *ocre d'ouro*. Naples yellow is referred as *amarelo de Nápoles*, indian yellow as *indien* and gamboge as *gomma indiana*. Two modern classes of yellow were present: chromium yellow, named *chrome foncé* and *chrome clair*, and cadmium yellow, listed as *cadmium*. Yellow synthetic dye *anilina amarela* was also present but was not considered since no price per weight unit was determinable. Yellow ochres and Naples yellow were clearly inexpensive traditional pigments (average prices eg $060/Kg –*ocre fino em pó*/yellow ochre; $920/Kg –*amarelo de Nápoles*/Naples yellow) as chromium yellow was among modern ($870/Kg –*chrome clair*/chromium yellow). Cadmium yellow was the most expensive modern yellow pigment in the group. Sold at average price 9$200/Kg, it was 153 times more expensive than the cheapest ochre. Two traditional classes of pigments have the dearest prices, namely Indian yellow and gamboge. More than twice the value of cadmium yellow, their average prices were, respectively, 20$700/Kg and 21$000/Kg. These two materials' high prices are confirmed in foreign technical literature [14-15] (fig. 10).

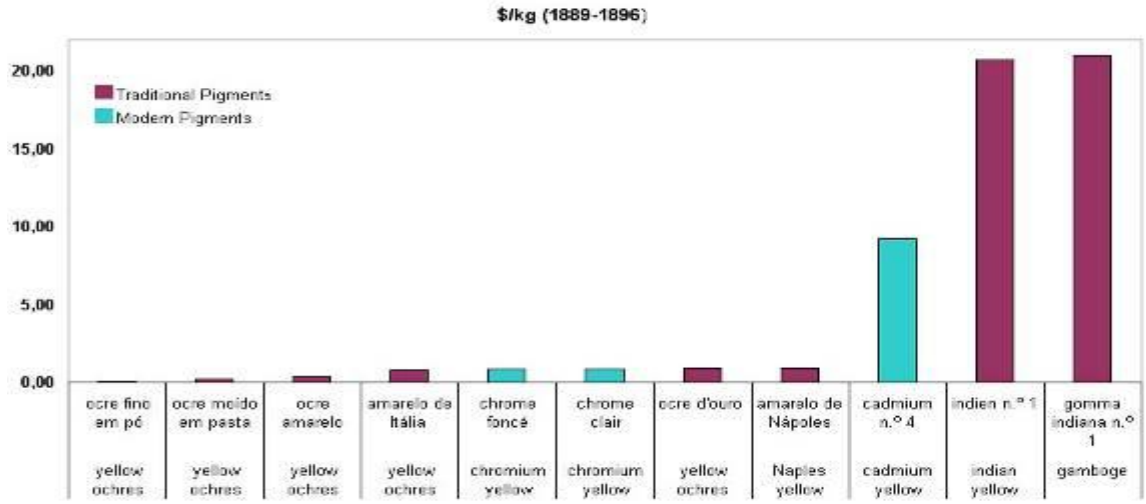


Fig. 10: Yellow pigments: prices per weight unit (*reis / kilogramme*).

Green pigments

Three different types of traditional greens were identified: buckthorn, mentioned as *vert Chine*; verditer was referred as *cendre verte* and *vert mineral*; and sap green appeared under the name *verde bexiga*. Also three modern greens were found: Schweinfurt green was present as *verde de Paris* and *vert Veroneze*; chrome green, a composition or mixed green, is listed under the names *vert de chrome* and *verde de chromo*. Finally viridian is *vert emeraude*. The vegetable green colour buckthorn is the most accessible ($046/Kg –*vert Chine*), followed by Schweinfurt greens and green verditer. Schweinfurt greens (*verde de Paris*, *vert Venoneze*) and verditer (*cendre verte* and *vert mineral*) prices were variable. Chromium-based greens like chrome green and viridian were expensive (3$220/Kg –*vert de chrome n.º 1*/chrome green; 5$750/Kg –*vert emeraude*/viridian). Surprisingly, traditional sap green presented a high price (5$300/Kg –*verde bexiga*). This raises the possibility it might be of different nature, unrelated to its designation [12,16] (fig. 11).

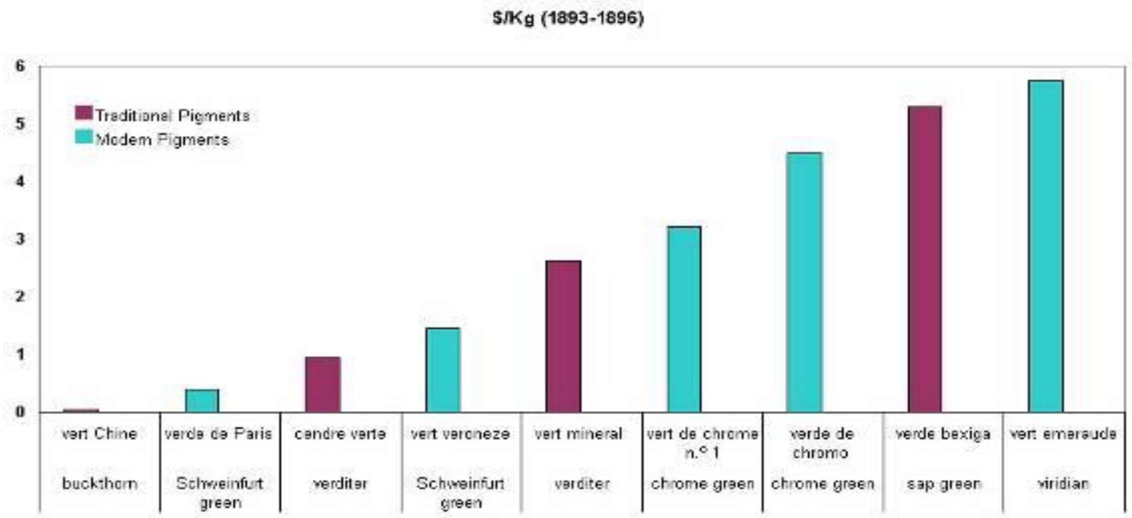


Fig. 11: Green pigments: prices per weight unit (*reis / kilogramme*).

Red pigments

A larger number of classes and dispersed prices were detected in red pigments: five traditional classes and four modern. Traditional reds include red ochres, red lead, dragon's blood, vermilion and cochineal lakes. Red ochres appear under the designations *roxo rei*, *sanguínea*, *vermelho indiano* and *vermelho de Veneza*. Red lead is *rouge Saturne* and *azarcão*. Dragon's blood is *sang dragon*. Vermilion is *vermilion* and cochineal lakes are named *laca carminada* and *carmim*. The four modern red classes found were: chromium red that appears as *vermelho de chromo*; synthetic dyes include different materials and names like *laque dahlia*, *magenta*, *rosa bengala* and *laque geranium*; Mars violet is listed *violeta de Marte* and *violet Mars* and finally cobalt violet is *violet cobalt*. Red ochres had different prices but can be considered inexpensive pigments. Referring exclusively to this class, *roxo rei* was cheaper than *sanguínea, vermelho indiano* and *vermelho de Veneza*. Their prices are similar to red lead's and to chromium red, the only new pigment belonging to the less costly. *Almagre*, another red ochre, has a price at a different level, together with vermilion and dragon's blood but cheaper than cochineal lake *lacca carminada* (fig. 12-a). A division in red pigments is noticeable relating prices and categories. The major distinction is that the group of more expensive pigments (3$500-20$000/Kg) is mostly constituted by modern synthetic red pigments. The exceptions to this rule are, on one hand, the accessible chromium red (*vermelho de chromo*), and on the other hand, safflower (*rouge cartham*) and cochineal lake (*carmim*), traditional yet expensive materials. Once again, as in blue pigments, a cobalt pigment, cobalt violet, is the most expensive material. It was sold at 18$400/Kg. This value is 307 times more expensive than *roxo rei*/red ochres ($060/Kg) and 6 times the price of *lacca carminada n.º 2*/cochineal lakes (2$950) (fig. 12-b).

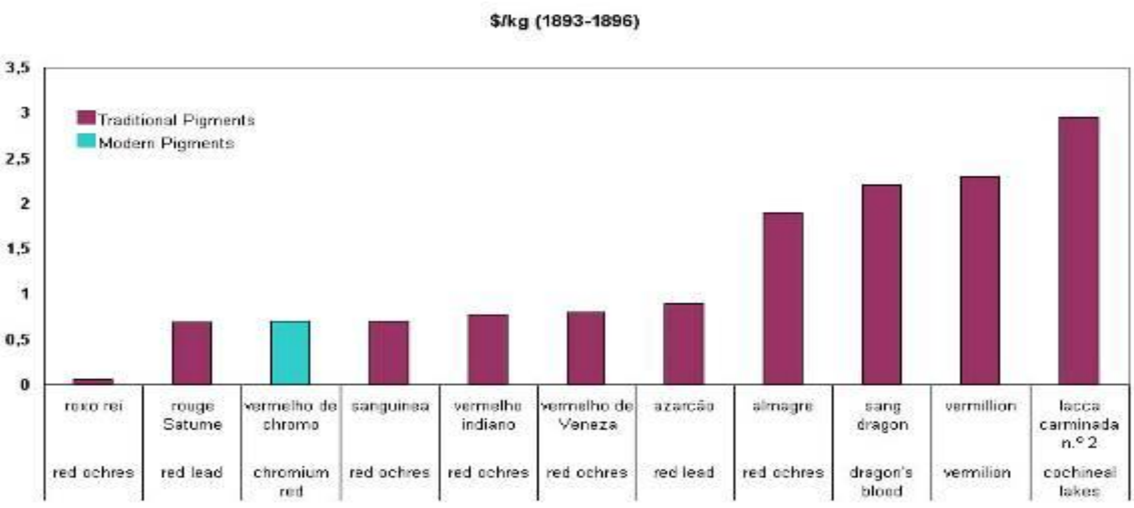


Fig. 12-a: Red pigments: prices per weight unit (*reis / kilogramme*). Values 0-3$500.

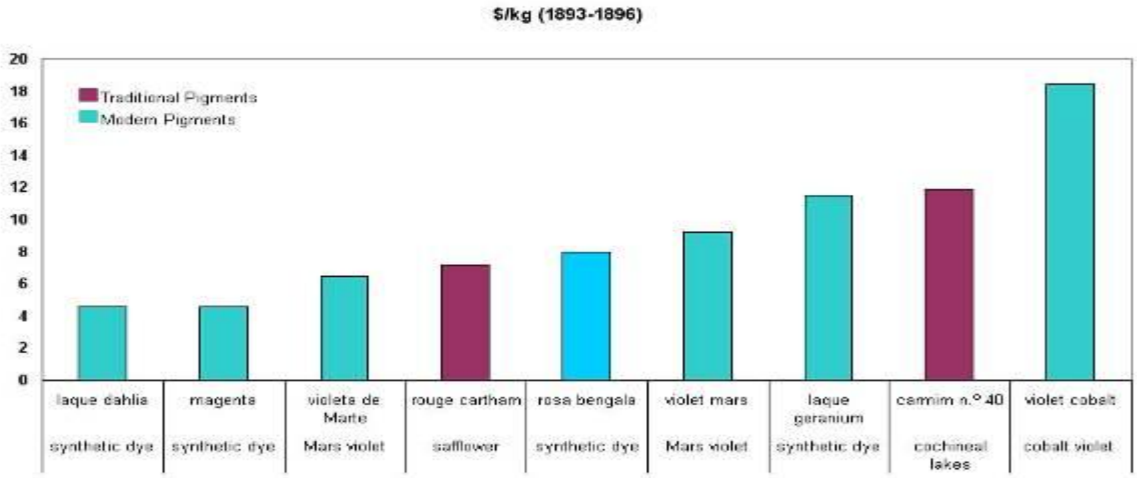


Fig. 12-b: Red pigments: prices per weight unit (*reis / kilogramme*). Values 3$500-20$000.

Brown and black pigments

The comparison of prices could only be made for traditional black and brown pigments. Umber was named *terra de sombra* (*natural*) and *terre ombre*, Sienna earth was *terre sienna*, *terra de Sienna* (*natural* and *queimada*) and *terra de Itália* (*natural*) and finally, ivory black was present as *negro de marfim*. Although not included because their price per weight unit could not be determined, other pigments, both traditional and modern, were found in bills. Considering traditional classes, black lead was named *plombagina*, black earth was *terra preta*, Indian ink was *pau de Nankin*, sepia was *sépia* and umber was *sombra*. Regarding modern pigments, synthetic dyes both brown and black were present respectively as *anilina castanha* and *anilina preta*. The prices variation is small as brown and black were usually inexpensive pigments. The cheapest, *negro de marfim*/ivory black was sold at the average price of $340/Kg and the most expensive, *terra de Itália natural*/Sienna earth at $780/Kg (fig. 13).

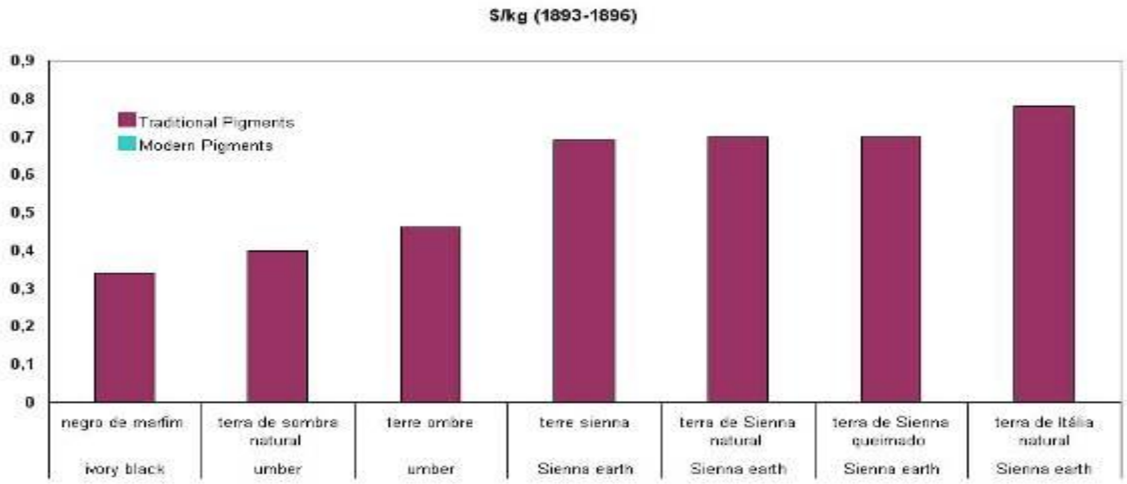


Fig. 13: Brown and black pigments: prices per weight unit (*reis / kilogramme*).

Oporto Industrial School acquisitions: Shops and Brands

In 1817, in a publication about drawing and painting, Ferreira da Silva describes the preparation of paints. The author has one clear goal: "*avoid dependence from English and French paints*" [17, p.116]. This short observation testifies the ongoing revolution in painters' craft as specialized tradesman and colourmen make ready-to-use painting materials available. International influence is found in Portuguese 1840's publications as French, English or German books and newspapers circulate in Portugal making knowledge accessible and diminishing the gap between periphery and European countries central to the development of these products. Progress and prosperity were expected results from political investments in education and arts by several governments in the second half of the century. Authors repeat that economical advantages to the country would arrive from production effort and innovation [18-19]. In this context, documentary sources include information about machines, factories, production of goods, diagrams, recipes and secrets for arts and crafts. Instructions to make painting materials were also included in this general trend. Industrial development incentives can also be seen in measures as the creation of industrial and technical schools, the opening of industrial museums and regular industry exhibitions.

Table 2 shows the acquisitions made by Oporto Industrial School of fine arts materials in the period 1876-1896. National, mainly local, and occasional French stores were found. Table 3 depicts brands, products and date of fine arts materials acquisition. French brands are preponderant. The probable importation of materials is not detected in most receipts.

Table 2: Painting materials acquisitions by Oporto Industrial School (1876-1896).

Shop	**Purchase year**	**Address**	**Product**
Benjamin Barral	1876-1882	Faubourg Saint-Denis, 80, Paris, France	Pigments and dyes
António Fonseca de Moura	1888	Largo de S. Domingos, 99, Oporto, Portugal	Stucco plaster
Joaquim António Cardoso de Almeida	1888-1892	Praça Carlos Alberto, 9-10; Oporto, Portugal	Pigments and dyes, varnishes, solvents, chemical products
Araújo & Sobrinho	1888-1896	Largo S. Domingos, 50, Oporto, Portugal	Pigments, drawing and painting materials, paper
Frederico Augusto Ribeiro Cardoso (succ. of Custódio dos Passos)	1888-1889	Praça D. Pedro, 111-3, Oporto, Portugal	Varnishes, solvents, chemical products
Librairie Charles Delagrave	1889	Rue Soufflot 15, Paris, France	Perspectographer, models
Pinto & Meirelles	1890	Rua Sá da Bandeira, 165-7, Oporto, Portugal	Painting materials
Paul Rousseau & C.ie – Fabrique de products chimiques	1890	Rue Soufflot, 17, Paris, France	**Dyes**
Georges Meusnier – Objects d'art, expertise dessin au fusain	1890-1891	Rue Saint-Augustin, 27, Paris, France	**Cours elementaire de paysage au fusain**
Bourgeois Ainé – Fabrique de couleurs, specialité couleurs moites	1891	Rue du Caire, 31, Paris, France	**Pigments and painting materials**
Papeterie Hurier	1892	Rue du Caire, 53, Paris, France	**Drawing papers**
Silva Guimarães & Irmão	1893	Rua de S. João, 126-8, Oporto, Portugal	**Painting materials**
Favrel Portuense	1894	Campo dos Mártires da Pátria, Oporto, Portugal	**Gold leaf**
Antão de Almeida Sampaio (succ. of José M.ª Costa Araújo)	**1894**	**Praça dos Voluntários da Rainha, 23-4, Oporto, Portugal**	**Painting materials**

Table 3: Fine arts materials' brands in Oporto Industrial School acquisitions (1876-1896).

Brand	**Product**	**Year**
Bourgeois Ainé	pigments	1891
Hurier	paper	1892
Favrel Portuense	gold leaf	1894
Rives	(photographic) paper	1893
Ingres	paper	1892
Canson	paper	1892, 1896
Thomar	paper	**1889, 1893**
Whatman	paper	**1889, 1891**
A. W. Faber	pencils and erasers	**1888-1893**
Conté	pencils	**1892, 1896**
Field	ink	**1888, 1891, 1893**
Antoine	ink	**1893**
Robert	crayons	**1890**
Petit Buisson	Drawing charcoal	**1892**
Venitum pour le trait	Drawing charcoal	**1892**
Reeves	Drawing and watercolour set	**- (1889? 1891?)**
J.& A. Molteni	**Drawing set**	**- (1889? 1891?)**

Conclusions

Traditional pigments persist throughout the century. The main pigments referred in contemporary foreign technical literature were found in Oporto Industrial School receipts.

Regarding fine arts materials, French provenance was dominant in the School acquisitions in the studied period.

The access to synthetic materials at the end of the century is confirmed and some modern pigments were then less expensive materials than traditional ones. For instance, among blue colours, artificial ultramarine had become the cheapest blue pigment.

Considering modern pigments, chromium-based colours were sold at low prices, with the exception of viridian. Instead, cadmium- and cobalt-based pigments were expensive colour materials throughout the entire century as one can confirm in the late years.

Regarding traditional pigments by colour range, materials such as Indian yellow, indigo and cochineal were among the most expensive materials respectively in yellow, blue and red pigments.

Technical literature shows the availability of materials from the French supplier *Lefranc* [20-21] and in the second half of 19th century *Araújo & Sobrinho* sold Winsor & Newton. In early 20th century other brands such as *Morin & Janet* and *C. Bourgès* are documented [22-23].

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References

[1] ALVES, L. A. M. (2005) *ISEP – 150 anos: Memória e Identidade*. Porto: Instituto Superior de Engenharia do Porto/Edições Gémeo.

[2] ALVES, L. A. M. *et al*. (2009) *Ensino Técnico (1756-1973)*. Lisboa: Secretaria-Geral do Ministério da Educação.

[3] ALVES, L. A. M. (2003*) O Porto no Arranque do Ensino Industrial (1851-1910)*. Porto: Afrontamento.

[4] COSTA, M. A. N. (1990) *O ensino industrial em Portugal de 1852 a 1900*. Lisboa: Academia Portuguesa de História.

[5] KREBBS, R. E. (2006) *The History and Use of our Earth's Chemical Elements: A reference guide*. (2nd ed.). Santa Barbara: Greenwood Publishing Group.

[6] BOMFORD, D. (1999) The history of colour in art. In LAMB, T. & BOURRIAU, J. (eds.), *Colour: Art and Science*. Cambridge: Cambridge University Press.

[7] CALLEN, A. (2000) *The Art of Impressionism: Painting Technique and the Making of Modernity*. New Haven and London: Yale University Press.

[8] BALL, P. (2003) *Bright Earth: Art and the Invention of Color*. Chicago: University of Chicago Press.

[9] COHEN, D. X. (1880) *Bases para orçamentos, seguidas d'uma série de preços*. (1.ª ed.) Lisboa: Tipografia Gutierrez.

[10] COHEN, D. X. (1895) *Bases para orçamentos, seguidas d'uma série de preços*. (2.ª ed. Lisboa: João António Rodrigues.

[11] BERRIE, B. (1997) Prussian Blue. In FITZHUGH, E. W. (ed.), *Artists' Pigments: A Handbook of Their History & Characteristics*, vol. 3. Oxford: Oxford University Press.

[12] EASTAUGH, N.; WALSH, V.; CHAPLIN, T.; SIDDALL, R. (2004) *Pigment Compendium. A dictionary of historical pigments*. Oxford: Elsevier Butterworth-Heinemann.

[13] DELAMARE, F. (2007) *Bleus en poudres: De l'art à l'industrie. 5000 ans d'innovations*. Paris: École des Mines de Paris.

[14] WINTER, J. (1997) Gamboge. In FITZHUGH, E. W. (ed.), *Artists' Pigments. A Handbook of Their History & Characteristics,* vol. 3. Oxford: Oxford University Press.

[15] BAER, N.S. *et al*. (1986) Indian yellow. In FELLER. R. L. (ed.), *Artists' Pigments. A Handbook of Their History & Characteristics*, vol. 1. Oxford: Oxford University Press.

[16] CARLYLE, L. (2001), *The Artist's Assistant: Oil painting instruction manuals and handbooks in Britain 1800-1900 with reference to selected eighteenth-century sources*. London: Archetype Publications.

[17] SILVA, R. F. (1817) *Elementos de Desenho e Pintura e Regras Gerais de Perspectiva*. Rio de Janeiro: Eduardo e Henrique Laemmert.

[18] Anon. (1841) *Segredos Necessários para os Officios, Artes, e Manufacturas*. New edition entirely revised and added by J.A.A.S. Lisboa: Typographia de José Baptista Morando.

[19] LÚCIO, J. B. (1844-1845) *Collecção de Receitas, e Segredos Particulares*, 6 vols. Coimbra: Typographia de M. Caetano da Silva.

[20] VARELLA, J. N. (c. 1902) *Favrel Lisbonense*. Lisboa.

[21] VARELLA, J. N. (1904) *Favrel Lisbonense*. Lisboa.

[22] CRUZ, A. J. (2005) A pintura de Columbano segundo as suas caixas de tintas e pincéis. *Conservar Património*, 1, pp. 5-19.

[23] SANTOS, S. B. & CRUZ, A. J. (in press) Green pigments: Tradition and modernity in painting according to Portuguese 19th century technical literature. In *Proceedings of the Youth in the Conservation of Cultural Heritage 2010. Palermo, 24-26th May 2010* (in press).

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 1035.

THE CIRCULATION OF KNOWEDGE ABOUT SYNTHETIC COLORINGS IN THE COMPANY *LA ESPAÑA INDUSTRIAL*

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Abstract

The present communication is a study about the circulation of textile chemistry knowledge in the moment of transition from the use of natural to synthetic colourings for textile printing. The company La España Industrial*, the most important textile factory in Spain (1847-1980), progressively incorporated new synthetic colourings, which during the second half of the 19th century coexisted with natural colourings.*
The discovery in England in 1856 of new synthetic colourings revolutionized the production in factories and changed the forms of communication of knowledge on the subject of chemistry applied to printing.
In the case of natural colourings, the transmission of knowledge was traditionally made under a great secrecy. With the discovery of new synthetic colourings, the forms of communication changed and new tools of communication on formulation and application of colourings were born.
The Museu de l'Estampació de Premià de Mar, a museum specialized in textile printing, preserves an extended collection of formularies and a documental archive of the company La España Industrial. In this collection there are several witnesses of the incorporation of the new chemical knowledge.
In our journey, various aspects of the period of natural and synthetic colourings are revealed on La España Industrial*. In the first place, we present a collection of unedited formularies of the museum, witnesses of the progressive incorporation of synthetic colourings and of the coexistence with natural colourings. We also present the catalogues of colourings edited by chemical industries that were used in the factory.*
Other unedited documents are also discovered, like the correspondence that the company sustained with chemical companies, Spanish and foreign scientists. In our route, we will reveal the main actors of the change in the factory that implanted the new discoveries, and the tools that they used for the transmission of this knowledge.

Introduction

The knowledge on natural and artificial dyes has been spread during the history in different ways. On one hand, the exchange and transmission of knowledge on traditional colours from plants, animals and minerals were established between the artisans grouped in guilds. The transmission of the technical process for dying and secret colour recipes in many cases were given orally; ateliers and manufactures preserved with extreme caution the formulation to obtain textile colours. We can find technical written documents and books of formulas of the 17th, 18th and 19th centuries in the Biblioteca Nacional de Catalunya, Arxiu de la Junta de Comerç and Arxiu de la Casa Ardiaca de Barcelona.

The present communication pretends to announce unpublished documents preserved in the archives of the Museu de l'Estampació de Premià de Mar, having as reference *La España Industrial* factory, which with a different purpose have been tools for knowledge about the use of natural and synthetic dyes within the company.

With this communication we also want to show the technical and scientific evolution in the factory, in Catalan and European context, analysing how *La España Industrial* (henceforth abbreviated: LEI) applied new chemical discoveries on textile colours to its laboratories and to the final product, the printed and dyed fabrics.

Introduction to natural colours

When we speak about textile colours, it's necessary to establish a clear distinction between the colours for dyeing or dyes used in liquid state submerging the clothes, and the colours for printing designs, previously engraved on moulds, over the fabrics. While the first ones were already known and applied in Europe since the Neolithic, the others began to be used by European printers in the 17th century, thanks to the import of calicos, a multicolour printed cotton from India and China.

Until 1856 printers and dyers used only natural colours, commercialized in dust, kneads or solid blocks, that little by little happened to be substituted by artificial ones. So much, the difficulty in the arrival of natural colours from the colonies and the overhead cost of some of these products are factors that drove the studies of chemists to elaborate synthesis colours in the laboratories.

Some natural and local colours were substituted during the second half of the 18th century for exotic products from American colonies. And one century later synthetic colours would take the place of natural local and also exotic ones.

In the case of Mediterranean blue, extracted of the leaves of the plant *Isatis tinctoria*, used since the Neolithic, the European import of indigo blue during the 16th century threw down whole regions devoted to the farming of *Isatis*. After their recovery during the 18th century, *Isatis* was again substituted, because of the interest by chemists in the 19th century to know the composition of indigo blue trying to imitate it to the laboratory. Nowadays *Isatis tinctoria* has been replanted in Tolouse, France.

Since ancient times Europe and Orient extracted red colour from an insect called *Quermes* and also from the root of the plant *Rubia tinctorium*. The cultivation of this plant in Spain –Catalonia– and France –Alsace and Vauclusse– was gravely affected in the 18th century because of the introduction of a red exotic pertinent insect from Central American colonies, called *Dactylopius coccus*, that became a sign of distinction between the richest classes.

In Catalonia, apart from the blue *Isatis* and the red *Rubia*, dyers had used many other autochthonous plants to elaborate natural textile colours.

Catalonia and the knowledge on science of colour

Catalonia stands for having been during the 19th century and until the last quarter of the 20th century, a textile producer centre of first order and, between the many specialities, takes a relevant role in the printing industry. Since the beginning of the Catalan textile printing industry, there was a worry to be up to date on the scientific novelties to equate to the European level.

The finding of synthetic colours in the 19th century supposed a revolution in a process that modified the internal and external relations of the manufacturing and factories of textile printing and dyeing. The findings changed the ways of transmission of these knowledges into new channels of communication (universities, trips to foreign countries).

In many documents we can see how knowledge crossed borders and how technicians established contact with companies from other countries. Knowledge travelled with manufacturers and scientists. In Catalonia, Carles Ardit and Josep Vallhonesta left an important bequeathed writing. Ardit worked in factories of France and Switzerland (1814-1817) and in 1919 published in Barcelona *Tratado teórico-práctico de la fabricación de pintados e indianas*, in two volumes, where he collected the formulas of all the local and exotic colours. However, he considered better to promote fields suitable to the Catalan climate. The historian Agustí Nieto defines Carles Ardit as a mediator of knowledge, between the South-European culture and the Northern industrial culture, and also as a mediator in the context of the Catalan industry.[1]

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[1] NIETO I GALÁN, Agustí, "Secrets i receptes. Carles Ardit i el "Tratado teórico-práctico de la fabricación de pintados e indianes." Conference course *La indústria de les indianes a Barcelona, 1730-1850*, Barcelona, 2011.

Another protagonist of the innovations and changes in Catalonia, is the industrial engineer in chemistry Josep Vallhonesta (1835-1899), who introduced in Catalonia the system of determination of colours invented by the French chemist Michel Eugène Chevreul. In one of Vallhonesta's works, *El Arte del tintorero*, in 1880, the coexistence between natural and artificial colours is obvious.[2]

The factory *La España Industrial*

In this study we have taken as a reference LEI, the biggest factory in Spain, which initiated its activity in 1847 in Barcelona and was active for more than 100 years. It outstood for being the most prosperous textile company in the country, and one of the first one involved in all the textile processes, spinning, weaving and finishing, which include dyeing and printing. In the printing and dyeing department there were ten active cylinder machines printing continuously.

LEI is a perfect example of industrial and scientific development, as the factory endeavoured a constant process of research towards innovation and implementation on both technical and scientific fields.[3]

LEI had an outstanding position among calico manufacturers as a number of foreign experts were hired, coming mostly from Alsace. Local Catalan workers received the necessary training. Among the first ones stands Martin Ziegler.

The Museu de l'Estampació de Premià de Mar preserves an important collection of textile sample books and colour formulation books for dyeing and printing; also original drawings and administrative, scientific and technical documentation. A group of 120 formulation books, from 1860 to 1915, give us a lot of information about the products employed in the colour processes, the names of some technicians, and the origin and name of the providers.[4]

The use of dyes in LEI

For more than 50 years, natural and synthetic dyes were used at LEI indistinctly and simultaneously. New synthetic dyes did not set aside traditional natural colorants. We can establish three periods in regard to the use of different kinds of dyes. The first one would start at the very foundation of the factory at 1847 until 1866. The time span of 1867-1900 will see a progressive inclusion of new synthetic dyes. Finally, by 1900-1910 the old methods will be progressively abandoned and the use of natural dyes will be discontinued.

From the first period –when only natural dyes were used– the museum keeps a small sample collection featuring some similarities. Within these sample documents a letter dated from March 1860 mentions the purchase of several pigments, like garance, pomegranate flower, gall black, quercitron and sumac, all of vegetal origin.

In 1856, the English chemist Sir William Henry Perkin accidentally discovered the first synthetic colorant when he tried to synthesise quinine. From then on, Perkin developed the first synthetic dye: mauveine. Three years later, Verguin found the fuchsine, and Lauth methyl violet (1861). In 1862 August Wilhelm von Hofmann, the violet bearing his name; and Bismarck a new brown. By 1863, Lightfood invented the aniline black and in 1868 the German chemists Graebe and Liebermann synthesised alizarin.

During an extended period of time, spanning until the early 20th century, natural and synthetic colours will coexist on the factory, like other European factories will. LEI started adopting these new discoveries and substituted traditional methods gradually.

The first documented evidence about the application of synthetic dyes at LEI dates from 1867. The form "Colores vapor", displays a printed sample with fuchsine and another one with Bismarck brown. On the same form aniline green can as well be found. That same year, new dyes had already been introduced, although there is no written evidence of a possibly

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[2] Duran i Pineda (DURAN I PINEDA, R., 2004, 53-72). Duran i Pineda emphasizes the figure of Pere Roqué Pagani (1822-1880), the author of an important text about dying in the 19th century. P. ROQUÉ i J. ARBÓS, *Tratado práctico del blanqueo y tintura de la lana, redactado conforme a los principios, práctica y demostraciones de la química moderna y acompañado de un atlas conteniendo 100 colores fijados sobre dichas materias*, 2 vols., Barcelona, Imprenta de la viuda e hijos de Mayol, 1846.

[3] "LEI was one of the first manufacturers of printing fabrics. It was possible because of the figure of the chemist director and many foreign special technicians. (...) Alsatians (...) local workers, who learnt the processes from the foreign ones." (GUTIÉRREZ I MEDINA, M.LL., 1997, 233).

[4] Most of the products came from Germany and France, although in Catalonia we can also find providers. Ramon Monroig's factory, in Barcelona since 1844, was the first manufacture of colours for the Catalan textile printers. Leopold Sagnier found the first factory of synthetic colours in Catalonia in 1881, providing Spain and Portugal, but the factory depended too much on German providers. (CABANA, Francesc, 1992, 263-272). Joan Timoleó Cros, chemist manufacturer, provided chemical substances to *La España Industrial*. Timoleó was often a mediator of foreign colours. (GUTIÉRREZ I MEDINA, M.LL., 1997, 242-244).

earlier adoption, since there are no records prior to 1850. The use of Hofmann's purple was documented at LEI only 10 years after its discovery, but we cannot discard an earlier use.

However, from all the available documentation, no reference is made to the mauveine. Probably its steep price discouraged LEI to use it.

On the other hand, aniline black was adopted quite early. Aniline black is the residual result of industrial processes applied on coal tar. Coal tar was obtained from the reaction with hydrochloric acid yielding an intense black pigment. This new dye implied an important breakthrough for its resistance to light and washing. In 1868, this dye is referenced to as "Negro A" on form number 6064.

Much like aniline, products derived from distillation of tar are numerous. Many of them are suitable for the preparation of dyes and colorants, for instance: benzene, naphthalene, anthracene, from which many colours can be extracted.

1868 was a crucial year in this revolution. German chemists Graebe and Liebermann succeeded in preparing artificial alizarin. This was an important benchmark as it could substitute natural pigments, resulting on lower prices although this influenced negatively the production of madder.

Artificial alizarin found a great commercial success although it impacted deeply on the farming of madder, necessary to produce alizarin. Although the usage of artificial alizarin was ceased on many factories, LEI continued to employ it until 1880 along with its new artificial substitute.

The first attempt to use artificial alizarin was probably around two years after its discovery. The accounting books, with information about dye providers from 1870 to 1873 (MEP 7061), reference the Chancel alizarin, also called "penkoffin", an allegedly artificial pigment. However, we have actual proof that in 1874 LEI purchased some important amounts of artificial alizarin from the German company BASF.

LEI hired Spanish and foreign workers alike aiming to introduce new discoveries. An evidence of these innovations are the research work and notes taken down on the test record books.

It is interesting to observe that the aim of the tests was to find a substitute for natural pigments. On form MEP 7062 (from 1872) some test notes show an attempt to change a base colour to aniline grey using albumin. The results were not quite satisfactory. On the next page of the notebook, Heinrich Siegle (from Stuttgart) would try to "*remplacer le cachou de l'albumine pour une lacque*".

The museum preserves a similar document from the same year (MEP 7060), which brings together a series of tests with both synthetic and natural colours. It is remarkable a study dedicated to Prussian blue, discovered in 1774, and which is considered to be the first chemical pigment. The second part of this notebook is dedicated to studies about the properties of the water used, tests with natural pigments and aniline dyes, Hofmann's purple, etc. By the end, a study by M. Courton et Cie. from Mulhouse (France) can be found about the application of the naphtylamine maroon.

It is remarkable to observe how the tests tried to replace a natural pigment for synthetic substitutes. On two separate tests, carried out by Heinrich Siegle and by Courton, both strive to find new colorants in order to substitute old formulae. The main purpose was to reduce production costs and obtain more solid and resistant colours and improve fastness against water and light.

Two of these mentioned forms conclude with a study about the water used at LEI, since its quality is paramount to obtain optimal printing results on textiles. The factory took great care on analysing the water. Therefore, chemists played a central role in the factory. They were required to have a solid education and experience. It is not casual that some chemists achieved the direction of the company.

New discoveries are not adopted right away and the implementation of new colorants is gradual. This coexistence of traditional and newer methods is reflected on the documentation preserved by the museum, with special attention to document MEP 6078, the most revealing document about this coexistence.

The book summarises the time span between 1860 and 1880, providing evidence of the implementation of new colours. This manuscript contains the lists of colours and materials used to obtain the colouring paste, a moment where we can pinpoint exactly when synthetic dyes started to be employed.

Over the 1870s a few more discoveries will appear on stage. On 1872: methyl green. On 1873 cachou de Laval, 1874 Eosin, chrysoidine on 1875-6, alizarin blue and malachite green in 1877, briebrich scarlet in 1878. The same year Baeyer synthesised indigo, although it was not commercialised until 9 years later (1897).

Form number MEP 6068, spanning from 1878 to 1880 records for the first time the use of methyl green, a dye still being utilised nowadays.

By 1876, Mr Caro, an important chemist devoted to colour synthesis, finds the methyl blue which will be applied on LEI until the very end of the 19th century and which is repeatedly reported on samples and forms. At that moment, it was a good alternative to indigo. The most remarkable achievements at the time were the application of methyl blue and green.

From this period of time, it is important to highlight the document MEP 4957, a quite elaborated writing where we can find studies and solutions aimed to create the colouring paste, the preparation of dyes as well as colours applied by steam. This document allows us to understand the complex processes involved on tincturing and printing fabrics, especially on a very valued and bright colour: Adrianopolis red.

MEP 4957 reflects the abundant use of Prussian blue or Guignet green, which could be considered as a previous step towards a total adoption of chemical dyes. Eosine and chrisodine (which will be employed some years later) are mentioned as well.

Concerning the other dyes discovered during that decade, from Brieblich scarlet, malachite green and Cachou de Laval, we have found no trace of them. Cachou, in its origin is extracted from trees bark, and it is widely documented on the papers, but references to Laval are not made. Probably this is due to being a rather matte brown tone not allowing distinguishing whether it is synthetic or not. Any further information should be taken by a specialist in chemistry.

The museum preserves other documents providing evidence of tincture application and printing, including formulae and the development of a theory toward printing. Usually, the forms were meant to be exclusively for internal documentation aimed to serve as a knowledge database on the laboratory. However, this specific document seems to have been addressed to the director of the factory, Sr. Muntadas, who was always very attentive to innovation and new discoveries.

Over the 1880s some discoveries were made: vacanceine, benzopurpurine, Congo red (1885), Para red, alizarin yellow (1887), rhodamine and paranitriline (1889).

Congo red was discovered in 1884, but the only evidence of its use at LEI is recorded on MEP 5748 during the 1890s. On the other hand, benzopurpurine was copiously used over these years.

On the incoming record book “Entrada de materiales” (MEP 6067) we can find a comprehensive inventory of all purchased materials during 1885-86 until 1900. This document has allowed us to establish when new dyes started being used, like rhodamine, and alizarin yellow, discovered around 1887. LEI will swiftly adopt these new discoveries.

A significant piece of information –the form reference MEP 4959– shows us that paranitraniline was introduced the very same year of its finding, in 1889. That year, other forms and documents state the use of derivatives of benzene, such as chrisamine, benzoaurine, azo blue; as well as alpha-naphthylamines.

As a matter of fact, the relations with foreign dye producing factories were solid and stable. We can attest this on a book from the mid-1890s (MEP 6087) quoting the most outstanding dye producers like BASF and its branch Granier & Co., DV Rohne Leverkus and the subsidiary Pfaltz Hahn & Co. and Friedrich Bayer & Co, through its salesman Alfredo Riera.

The 1890s will bring new dyes and will consolidate former materials. It is relevant to mention alizarin bordeaux, diamine green, direct dye-sky blue, direct black, vidal black, etc.

Black and blue were important tones to understand the changes on those times. Until the late 19th century, aniline black was widely used, but it will be replaced by new discoveries, especially by direct black around 1908 (form ref. MEP 6115).

A similar situation applies to indigo. On 1878 Von Baeyer found the formula to synthesise indigo but it was not commercialised until 1897. The alternatives until then were mainly: natural indigo, methyl blue, alizarin blue and aniline, as well as other chemical derivatives.

Although chemical and natural dyes available options were quite wide, tincturing with natural indigo will prevail for some time. On form MEP 6139 from 1885 we find a detailed explanation of a traditional tincturing process with natural indigo based on oxidation. This document is of some importance as the main concern addressed is the endurance of dyes. An additional note corroborates the use of direct blue and the application of naphthaline blue on 1897. However, we will not find any mention to synthetic blue until 1912.

When chemists attempted to use synthetic dyes, they obtained the raw materials from the providers including even catalogues with exhaustive instructions. Even so, it was necessary for the workers at LEI to have an extensive knowledge of chemistry to take into consideration the weight, timing and density of mixtures, and thus, obtaining a perfect result of tone and saturation.

The factory kept as well some incoming letters in which the material providers deliver accurate instructions to develop dyes. For instance, a letter explains how solid blue B must be applied, with a printed sample appended.

Over the late 19th and early 20th centuries, LEI will usually use diamine, and synthetic dyes will gain ground to natural substances. Although we cannot quote the whole list of dyes used –most of them are listed on form 6110– the most commonly used were chloramine, toluylene orange, clorophenine, direct grey, metanilline, dianisidine, benzoazurine.

The discovery of new synthetic dyes contributed to change the world of fashion and home decoration. The 19th century would end with a wide spectrum of dyes of all tones; many of the natural variants will almost completely disappear. The 20th century will bring new breakthroughs on the history of dyes. In 1901 René Bohn patents idanthrene blue, the first synthetic anthraquinone dye, featuring some excellent washing and sunlight fastness. The copious discoveries to come will cast away the use of natural dyes, with some minor exceptions.

On document MEP 7063 from the early 20th century, we find a long list of new dyes, namely: naphthene, rhodamine, nitrazol, diamine, primuline, diphenyl, chloropherine, chrisamin, oxymin, rosophenine, chlorantine, benzoaurine, toluylene and thiophenol, among others.

The most important dye providers are Geigy, Leonart, Meister Lucius, Clayton, Casella, BASF, J. Bale, Thibaudier, Monegal, Riera, Bayer, V. Vidal, Kirchofer and Sandoz, some of the most outstanding and relevant chemical companies on the textile printing industry. Although most dyes are purchased outside Spain, Catalan experts as Josep Tay, Carles Casanoves and Manuel Solé are also mentioned and employed.

Since there is a direct relation between the fabric and the print to be applied, every kind of textile will require a different dye. This is why in the 20th century more dyes will be developed, mainly to colour new kinds of fabrics: polyester, rayon, polyamid and viscose.

This is the first study disclosing unpublished objects and documentation from the Museum, a series of scientific knowledge data and internal communications. With this documentation at hand we have been able to obtain a deep understanding of how dyes were used in the factory and trace them along a timeline, a sequence that has been compared with other printing manufacturers in Europe. The Museu de l'Estampació de Premià de Mar puts its collection to the disposition of researchers and experts wishing to investigate and study this historic period of industrial development. Additionally, the Museum preserves an extensive collection of dye catalogues dating from the 20th century from the main European factories, allowing establishing a bridge between both periods of time.

References

ARDIT, C. (1819) *Tratado teórico práctico de la fabricación de pintado ó indianas*, Vol. I II, Imprenta de la Viuda de D. Agustín Roca, Barcelona.

BARGALLÓ, A.; BARGALLÓ, M.; BENAUL, J.M.; ENRICH, R.; MARTÍN I ROS, R.M.; CANALS, M.T.; MUSET, A. (2007) *Indianes Estampats*, Museu d'Història de Sabadell, Sabadell.

CABANA, Francesc (1992) *Fàbriques i empresaris: els protagonistes de la Revolució Industrial a Catalunya. Metal·lúrgics – Químics*, Barcelona, Enciclopèdia Catalana.

DURAN I PINEDA, R. (2004) Plantes tintòries i indústria química a la Catalunya del segle XIX: Josep Vallhonesta i Vendrell (1835-1899), a *Recerques*, Barcelona, núm.49.

GUTIÉRREZ I MEDINA, M.L. (1997) *La España Industrial 1847-1853. Un model d'innovació tecnològica*, Ed. Associació d'Enginyers Industrials de Catalunya, Barcelona.

HOMS, A. (2009) *Tenyir en l'època romana*, Col·lecció Badalona Ciutat Romana, Museu de Badalona, Badalona.

MARTÍN I ROS, R.M. (s.d.) *La Real Fábrica de Esteve Canals y la indústria de los estampados en Barcelona, en el siglo XVIII*, Museu Tèxtil i d'Indumentària de Barcelona.

ODOL, J. (1995) *Lauragais pays des cathares et du pastel*, Editions Privat, Tolouse.

RIQUELME SÁNCHEZ, M. (1936) *Química aplicada a la industria textil, Estampado de fibras textiles*, Ed. Manuel Marín, Barcelona, Vol. IV.

VV.AA. (1999-2000) *Teintures précieuses de la Mediterranée, Pourpre-Kérmes-Pastel*, Musée des Beaux Arts de Carcassone i Centre de Documentació i Museu tèxtil de Terrassa.

ZUNDEL, M.J-J. (1975) Les couleurs et leur mise en oeuvre, *Musée de l'Impression sur Étoffes de Mulhouse*, bulletin n.761, Société Industrielle de Mulhouse, Mulhouse.

Arxives and Museums:

Arxiu Casa de l'Ardiaca de Barcelona.

Arxiu del Museu de l'Estampació de Premià de Mar.

Biblioteca de Catalunya. Fons Junta de Comerç.

INSTITUTIONS FROM THE 18TH TO THE 20TH CENTURIES

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 1042.

THE CONGRESSES OF RUSSIAN NATURALISTS AND PHYSICIANS AND THE MAKING OF ACADEMIC COMMUNITIES IN THE RUSSIAN EMPIRE, THE 1860S-1910S[1]

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Abstract

In the 19th century academic congresses, both national and international, became critical institutional means facilitating the traffic of knowledge. Since the 'Great Reforms' era scholars in the Russian empire set up two major periodic conventions –the Russian archaeological congresses and the congresses of Russian naturalists and physicians– that served as important sites of academic socialisation and exchange of knowledge. In the early years of the 20th century they were supplemented by a few minor conferences on applied geology, meteorology and applied entomology. The paper examines changing institutional affiliation of congress members (both scholars presenting their papers and their audience) and their places of residence, as these transformations reflected the changing understanding of 'professional' and 'amateur' research, and the rise of professional middle-class strata in the provinces. It compares these processes across a number of disciplinary fields (life sciences, earth sciences, proto-social sciences), thus highlighting different roles played by university faculty, civil servants and professionals, major academic centres and provincial societies in structuring these fields of knowledge. Also the paper compares geographical configuration of emerging academic networks, exploring the impact of various regions and regional centres of research and education within the Russian empire. Finally, the paper examines the historical geography of congresses: it addresses the role of local field sites, leading local practitioners and provincial institutions in attracting the congresses to different urban locations, and considers the reception of congresses in local civic and intellectual context.

[1] The paper is based on research undertaken as a part of a project 'The making of fundamental biological research in Russia: institutional framework, funding, results, 1860s-1920s' funded by the Russian Foundation for Basic Research (ref. no.10-06-00093a).

In the 19th century academic congresses became important institutional means facilitating the traffic of knowledge and the making of academic communities and networks. National academic congresses, which emerged in Western Europe after the Napoleonic Wars, were a part of a broader trend towards institutionalisation of science at a national level. They were instrumental for the making of new disciplinary fields, expanding its audience, enhancing prestige of scholars, and linking the 'centre' and 'periphery' of science –provincial societies and major universities, amateur enthusiasts and established academics (On academic congresses in the 19th century Europe see: Fox, 1980; Casalena, 2006, Casalena, 2007; Withers, Higgit and Finnegan, 2008). The paper examines the congresses of Russian naturalists and physicians –a major academic forum for natural sciences that was established in the Russian empire in the late 1860s and convened periodically until the outbreak of World War I. It analyses a changing composition of the congress audience aiming to explore the relations between the university faculty and a broader public in a period when universities were unanimously considered as the major 'carrier' of science and scholarship in the Russian empire (Bastrakova, 2007).

Like many other academic institutions, national (or rather empire-wide) congresses were established in the Russian empire with some delay, when compared to Western Europe. In Russia they were a product of the 'Great Reforms era' of the 1860s –the decade of major political, social and economic reforms, epitomised by the abolition of serfdom in 1861. The idea to set up periodic conventions of naturalists was first conceived by Professor Karl Kessler (1815-1881) in the late 1850s. In the 1860s he emerged as one of the principle advocates and lobbyists of the congresses. The Ministry of Education eventually succumbed to his and his colleagues' entreaties and authorised the first congress of Russian naturalists to be convened in the late December 1867 - early January 1868 in St. Petersburg. The St. Petersburg University, where Kessler served as Professor of Zoology, hosted the event (Pogozhev, 1887; Tikhonovich, 1953).

Until the very end of the imperial period the congresses of Russian naturalists were run by the universities with financial support provided principally by the Ministry of Education. The Ministry encouraged the university and other higher school faculty members, as well as secondary school teachers to attend the congresses by authorising paid leaves for those employees who wished to attend the event. Yet the access was not restricted to these two categories: anyone who paid a small conference fee could sign up as a registered participant. Indeed, from the very first convention the congresses attracted substantial number of people with no recent connection to universities. Already the early congresses, which took place in St. Petersburg and Moscow in the late 1860s, drew several hundred participants. In the next decade, as the congresses moved to Kiev (1871), Kazan' (1873) and Warsaw (1876), the number of attendants perceptibly declined; however the return of the 6th congress to St. Petersburg in 1879 was marked by a dramatic increase of its audience. The growth continued in the 1880s-1910s, although each time a congress was held outside of the two capitals -in Odessa (1883), Kiev (1898) or Tiflis (Tbilisi, 1913)- the numbers dropped down again. By the early years of the 20th century the congress audience reached mammoth proportions, exceeding five thousand members.

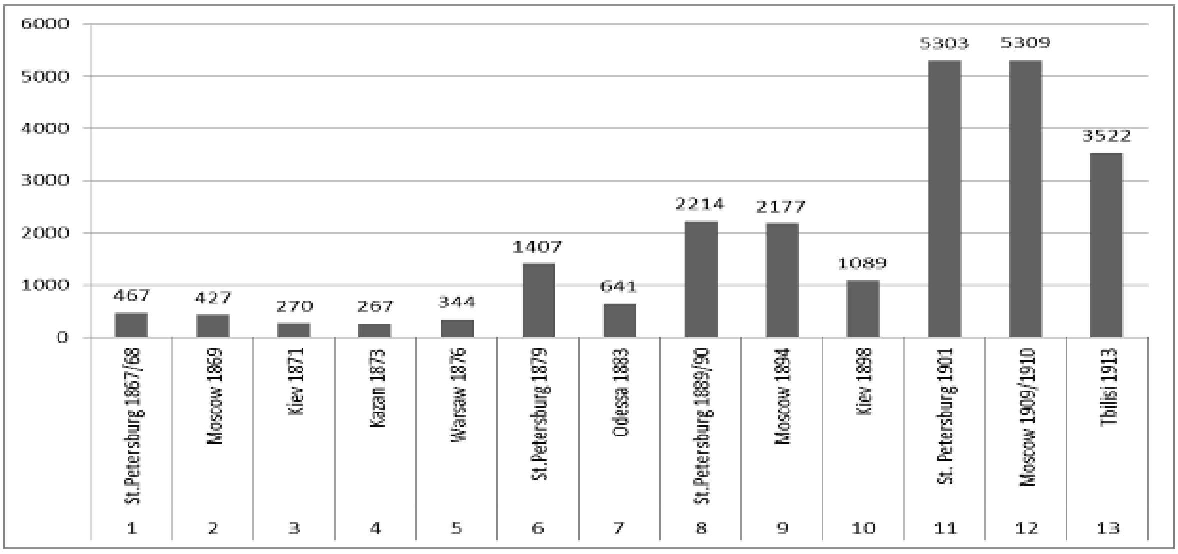

Fig. 1: Number of registered participants at the Congresses of Russian naturalists.[2]

[2] Sources: *Trudy pervogo s''ezda*, 1868, pp. xv-xxxiv; *Vtoroi s''ezd*, 1869, pp.1-21; *Trudy tret'ego s''ezda*, 1873, pp. 11-23; *Spisok chlenov IV s''ezda*, 1873; *Trudy V s''ezda*, 1877, pp. 4-16; *Rechi i protokoly VI s''ezda*, 1880, pp. 1-33; *VIII s''ezd*, 1890, pp. iv-xxx; *Dnevnik IX s''ezda*, 1894, no. 1, pp. 22-55, no. 3, pp. 15-24, no. 5, pp. 23-32, no. 10, pp. 52-62; *Vysochaishe utverzhdennyi X s''ezd*, 1898; Tikhonovich, 1953.

No doubt, the vast majority of the participants never visited more than just one congress. Of about 2,580 people who attended the first six conventions, only about 420 (or 16%) returned to take part in another congress. Later the ratio remained essentially unchanged: by 1898 no less than 7,400 people attended the congresses of Russian naturalists (the actual figure must have been even bigger, as no data exist on the participants of the 7th congress), yet it was only about 1,260 members (or 17%) who visited more than one convention.

Unfortunately, the data available on professional or institutional background of registered participants are not quite satisfactory, however there are some indicators suggesting that the university and higher school faculty composed about one quarter of the audience at the early congresses and their share remained relatively stable in the course of time, experiencing perhaps some decline at the 6th congress in 1879 but recovering by the last years of the 19th century. Another relatively stable group was formed by professional educators (mostly teachers at men's secondary schools run by the Ministry of Education) –they composed about 15-20% of all registered participants. Finally, already in the late 1860s-1870s a substantial part of the audience consisted of physicians, dentists, pharmacists and veterinary specialists. The medics became particularly visible at the 6th congress, when their share exceeded 30%. Indeed, we may assume that the first upsurge in the number of participants that took place at the same congress was at least partially accounted by a growing presence of specialists in various branches of medicine.

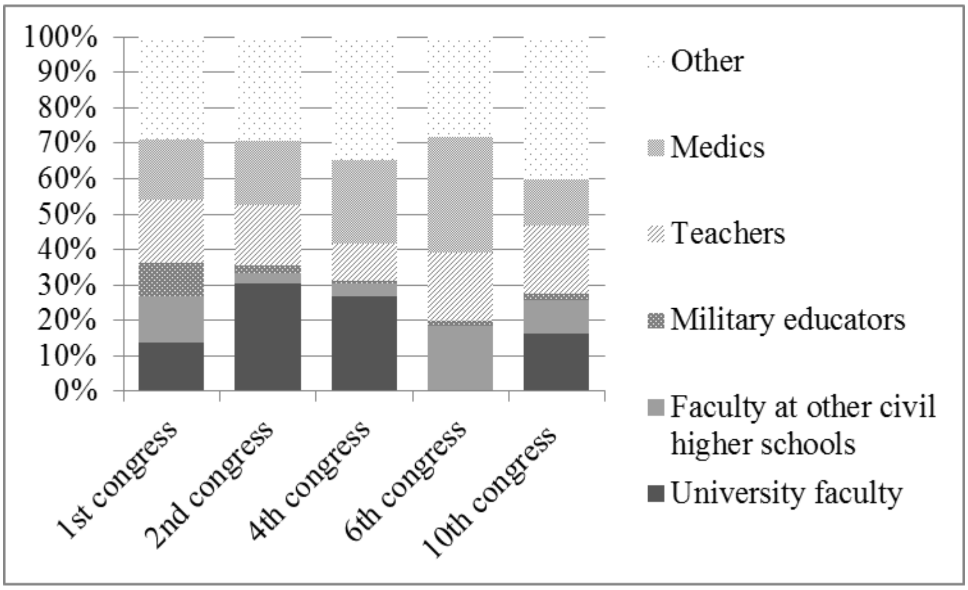

Fig. 2: Congress participants by occupation or institutional affiliation.

The audience of the first two congresses was exclusively male: it was only at the 3rd congress (Kiev, 1871) when the very first (and the only) woman showed up at the convention. Her name was Anna Volkova, she arrived from St. Petersburg and signed up for the chemistry section giving no details concerning her occupation or institutional affiliation. Volkova opened the way for other women: a few more attended the next congress in Kazan' in 1873. Yet at the Warsaw congress (1876) their number dropped down again to just two ladies. It was only at the 6th congress at St. Petersburg in 1879 when they began to form a sizable group (61 female participants on the list); even if proportionally their increase did not match the enlargement of the congress audience in general. A real growth occurred later, at the 8th congress (St. Petersburg 1889-90): the number of female participants increased more than three-fold, while their share among the audience grew up from 4.3 to 9.6%. The next congress in Moscow (1894) confirmed their increasing visibility, yet the move to Kiev, where the 10th congress took place in 1898, apparently discouraged women more than men from taking part in the event: not only the overall number of participants fell down twice, but also the proportion of women among them decreased to 5.1%. Quite predictably, the women who attended the congresses of Russian naturalists gravitated towards 'softer' subjects, such as geography and anthropology, while they were underrepresented at the sections on physics, mathematics and chemistry. Like in many other countries their role was initially rather passive: the very first woman who did not only attend a meeting but presented her paper and chaired a session at the 6th congress was Sofia Pereiaslavtseva (1851-1903), a graduate of Zurich University who would later earn an international reputation in marine zoology (On her see Mitrofanova, 1905). At the 9th congress in Moscow one more woman ventured to address the meeting as a speaker: this time she was Maria Pavlova (1854-1938), a graduate of Sorbonne, the wife of the leading Russian geologist Aleksei Pavlov, and a future prominent Russian palaeontologist on her own right (Borisiak, Menner, 1939). Yet even fifteen years later, at the 12th congress in Moscow (1909-10) there were just ten women among 478 speakers. (*Dnevniki XII s''ezda,* 1910).

The opening of the Russian naturalists' congresses for female speakers and audience could be seen as rather belated and slow, yet it was quite comparable with the advances in other areas of public scholarship in the Russian empire. Indeed, one way to assess the changing composition of the congress audience is to compare it to the public, who attended similar conventions for humanities –the Russian archaeological congresses. These meetings started in the same period (the 1st archaeological congress took place in Moscow in 1869) and met even more regularly than the naturalists' congresses until 1911. For the very first time women showed up at the 5th archaeological congress (Tiflis, 1881), i.e. even later than it was the case with naturalists, and with the course of time their visibility at the archaeological congresses (measured by their proportion to the congress audience as a whole) remained quite comparable with the situation at the naturalists' conventions (*Trudy V arkheologicheskogo s"ezda*, 1887, vol. 1, pp. xi-xii; *Trudy VI arkheologicheskogo s"ezda*, 1886, vol. 1, p. liii-lxii; *Trudy XIV arkheologicheskogo s"ezda*, 1911, vol. 3, pp. 32-42 pagination).

The fact that the naturalists' congresses were organised by the universities determined the choice of their venue: except the very last meeting (Tiflis 1913) all previous assemblies took place in the university centres of the empire. Predictably, St. Petersburg, as the imperial capital, hosted the largest number of conventions (1867-68, 1879, 1889-90, 1901), it was followed by Moscow (1869, 1894, 1909-10); twice the congresses met in Kiev (1871 and 1898), once in Kazan' (1873), Warsaw (1876) and Odessa (1883). In this respect the naturalists' conventions differed from the Russian archaeological congresses. The archaeological congresses also originated in the two capitals of the empire: the first one convened in Moscow (1869), the second one in St. Petersburg (1871), then followed the meetings in Kiev (1874), and Kazan' (1877). Later on the archaeological congresses occasionally returned to the university seats (Odessa in 1884, Moscow in 1890, Kiev in 1899, Khar'kov in 1902). However from the 1880s their geography rapidly expanded to encompass Tiflis (1881), Iaroslavl' (1887), Wilna (Vilnius, 1893), Riga (1898), Ekaterinoslav (Dnepropetrovsk, 1905), Chernigov (1908) and Novgorod (1911) (Lebedev, 1992, pp. 100-101).

Much wider geography of the archaeologists' conventions can be accounted by the fact that they were much smaller events (even in the 1910s the number of their participants did not exceed three hundred people) run by the Moscow Archaeological Society -an independent public association, very loosely tied to the university milieu. Another factor might have been at play as well: at least from the 1890s the choice of location for archaeological congresses was very much determined by the advancement of field research in a particular region. Potential interest, which a city could generate with its archaeological monuments and excavation sites, played an important role when the venue for a next convention was decided upon (Lebedev, 1992, pp. 100-101). As for the congresses of Russian naturalists, apparently, their organisers were most eager to show laboratories and museum collections. Characteristically, when congress participants had to vote for the location of the next naturalists' convention, the sections on chemistry or mathematics tended to favour St. Petersburg, while botanists and zoologists were more inclined to support the idea of changing the location (*Trudy V s"ezda*, 1877, pp. 66-68; *Rechi i protokoly VI s"ezda*, 1880, pp. 281 1st pagination, 26, 74, 123-124, 169, 238 2nd pagination).

The choice of the venue had serious implications for the composition of the congress audience. For obvious reasons the majority always came from the very city which hosted the convention. Since all the congresses of Russian naturalists and physicians, except the very last one (Tiflis, 1913), convened in the university centres, it reinforced the university domination not only in academic research and education but in the sphere of public science as well. Indeed, among non-local residents quite a number of people arrived to the congresses from the two capitals of the empire and a few other university cities. Nevertheless, there was always a sizable group among the audience, who resided outside of these major centres of scholarship, in the provinces. Naturally, the location of a particular congress was an important factor: these events always functioned as a powerful magnet for a much broader region outside the city where the meeting took place.

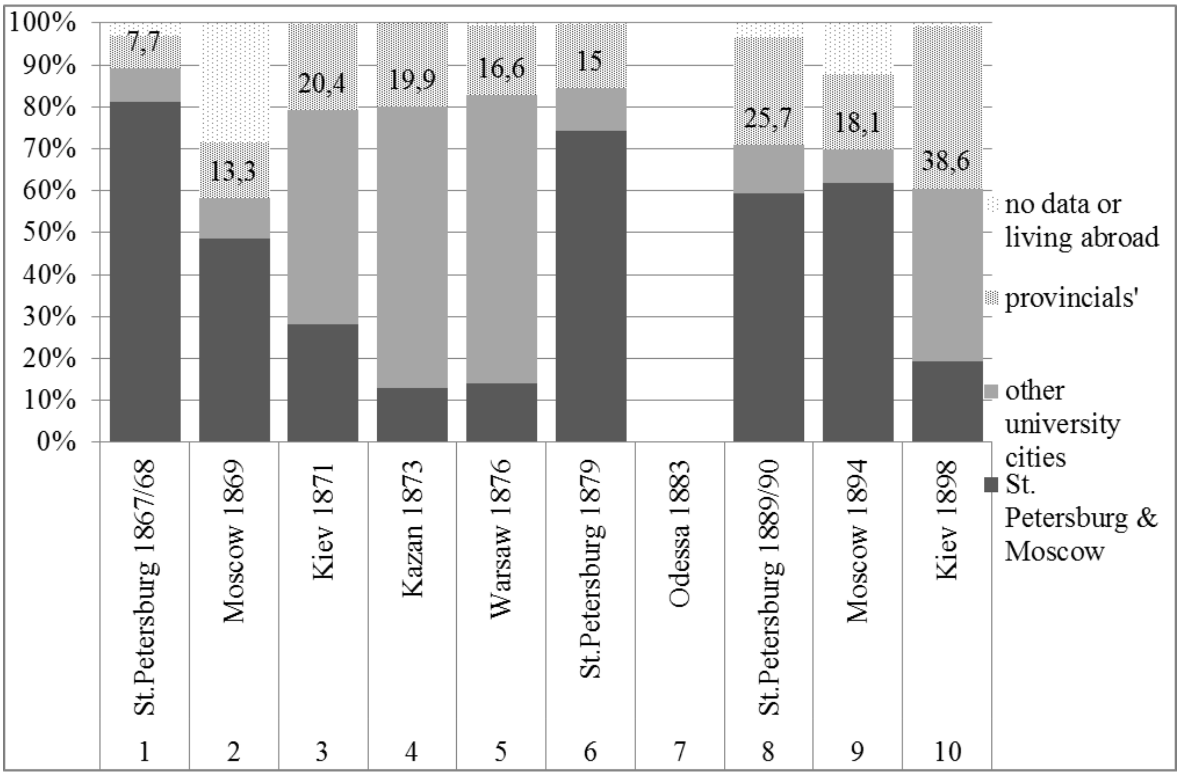

Fig. 3: Congress participants by place of residence.

Overall, there are some indicators that by the late 1890s the share of provincials among the public was on increase. Perhaps even of a greater importance was a steady albeit uneven expansion of interest in the congresses expressed by professionals and members of general public from a geographic perspective. Compare, for example, the two congresses, which took place in Kiev with the time gap of almost thirty years in between them. Among the participants of the 3rd congress less than thirty people lived outside of the capitals, university cities and provincial administrative centres, while the list of geographic names referring to their places of residence consisted of fifty entries. At the 10th congress there were no less than 415 provincials, while the same list contained 224 entries.

A substantial increase of the audience was paralleled by the expansion of the disciplinary field. Apart from plenary meetings, the program of the 1st congress encompassed subjects, which had already been institutionalized as independent chairs at the universities of the Russian empire, such as mathematics and astronomy; physics and chemistry (this section would be split into two different panels from the second congress onwards); botany; zoology; anatomy and physiology; geology and mineralogy. Sections for medicine and for technology were established at the 2nd congress. From the 3rd congress at Kiev (1871) the physics section encompassed physical geography, while the section on geology and mineralogy included palaeontology. A separate section on anthropology was established at the 4th congress (Kazan' 1873); later, at the 6th congress it was transformed into a section on geography, anthropology and ethnography. At the 8th congress (St. Petersburg 1889-90) new sections on 'scientific hygiene' and agronomy (plus veterinary) were added to the list. At the 9th congress (Moscow 1894) a subsection on statistics was established as a part of a broader unit for geography, anthropology and ethnography, while the 10th congress (Kiev 1898) provided space for separate subsections on meteorology and aeronautics.

Most of these disciplines had been to some extent reflected in the university curricula, or were in the process of their institutionalization as independent university chairs. However such sections as scientific hygiene, statistics, agronomy or anthropology had much broader appeal, as it can be demonstrated by a careful analysis of their audience. Indeed, the proportion of provincials who signed up for the sections on agronomy or statistics at the 10th congress was perceptibly larger than the proportion of provincials at the congress as a whole, while the share of provincials interested in botany, on the contrary, decreased and was smaller than the proportion of provincials at the congress as a whole.

Table 1: 'Provincials' at the congresses of Russian naturalists, by sections.

	3rd congress, Kiev 1871	10th congress, Kiev 1898
Life sciences (botany, zoology, anatomy and physiology)	31.3%	27.6%
Geology and mineralogy	35.3%	33.7%
Agronomy	-	64.2%
Statistics	-	57.4%
Medicine	9.1%	22.8%
Congress audience as a whole	20.4%	38.1%

A more detailed analysis of the botany and agronomy sections highlights a few more interesting trends concerning the institutionalization of these disciplines and the making of academic communities in the Russian empire.

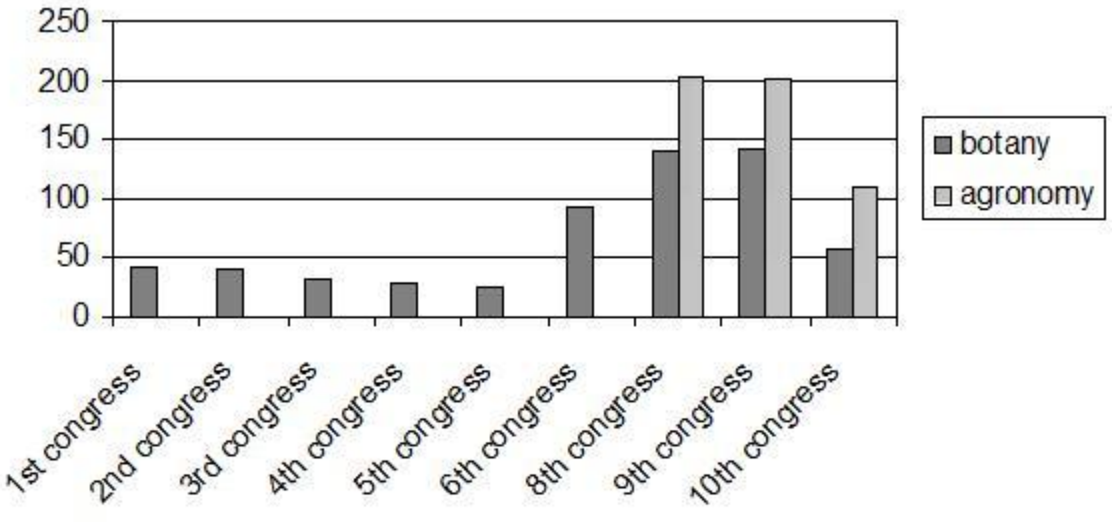

Fig. 4: Number of participants at the botany and agronomy sections.

The botany section was established already at the very first congress. The growth of its membership corresponded to the increase of the congresses' audience in general. Unlike the section on zoology, the botany section was heavily dominated by the faculty of St. Petersburg University, particularly at the early congresses, while it took quite a number of years for the faculty of Moscow University to get engaged in the events. The 'core' of the section was formed by a small group of people who missed very few conventions (2-3 out of 9), and who almost always presented a paper or chaired a session. All these men were the leading botanists from St. Petersburg -Andrei Famintsyn (1835-1918), Andrei Beketov (1825-1902), Ivan Borodin (1847-1930), Christopher Gobi (1847-1919) from the University, Aleksandr Batalin (1847-1896) from the Academy of Sciences, and Mikhail Voronin (1838-1903), who for the most part of his career was an independent scholar but who graduated from St. Petersburg University and maintained close ties with the University and the Academy of Sciences. By contrast, the only faculty member of Moscow University who attended more than three congresses was Kliment Timiriazev (1843-1920). Characteristically, after the first congress (where he was present) he failed to show up at these meetings until 1879, missing four conventions in a row.

The agronomy section, on the contrary, was established only at the 8th congress at St. Petersburg in 1889-90. From the start it attracted many more people than the botany section, and the proportion of provincials was much higher. Its audience proved to be not particularly stable, yet there was a small group of people who attended all three congresses (the 8th, 9th and 10th), for which we have data. Only one of them, Nikolai Adamov (1861-1912), was a member of the St. Petersburg University faculty; however in the 1890s he was still at the early stage of his career, serving as a custodian of the agronomy research room at the University. The two others, Aleksandr Kliucharev (1866-1932) and Aleksei Fortunatov (1856-1925) had established themselves at the Moscow Agricultural Academy. By the late 1890s, however, Fortunatov would move to New Alexandria (Pulawy) in Russian Poland. The Institute for Agriculture at New Alexandria that employed Fortunatov provided institutional affiliation for another member of the same group –Nikolai Sibirtsev (1860-1900). Sibirtsev began his academic career in 1882, when he took part in a famous soil survey of the Nizhnii Novgorod province, led by his mentor, Vasilii Dokuchaev (1846-1903). After the completion of the survey Sibirtsev remained in Nizhnii Novgorod for a few more years, taking up a position at a local museum of natural history. In 1892 he briefly returned to St. Petersburg, took part in another expedition headed by Dokuchaev, and then moved to New Alexandria, where Dokuchaev had already been serving as the director. The other three members of the 'core group' at the agronomy section were provincials –teachers or agronomists.

The same conclusion is applicable to a larger, much more volatile part of the audience: unlike botanists, very few members of the agronomy section had a university affiliation: those who taught at higher schools were employed either by the Moscow Agricultural Academy, or (less frequently) by the Institute for Agriculture at New Alexandria. While secondary school teachers were equally prominent at both panels, a substantial part of the audience on the agronomy section was composed by practicing agronomists, statisticians, and veterinary specialists.

This observation supports an argument recently advanced by some scholars who work on the history of various disciplines in the late imperial Russia. Apparently, by the turn of the 19th - early 20th century new fields of knowledge were emerging outside of the university framework. These new disciplines, like physical anthropology or plant breeding, challenged established academic hierarchies: they were much more committed to the idea of public science being more open to amateurs or practicing professionals, and pursing objectives, which were explicitly connected to immediate social problems. (Cf. Elina, 2008, vol.1, pp. 310-334; Mogilner, 2008, pp. 39-53, 162). The rise of these disciplines signalled the end of an epoch when the universities were the leading segment of research institutions in the Russian empire.

Certainly, further study is needed to understand behind-the-stage mechanisms, which determined the inclusion and exclusion of certain figures, institutions, social and professional groups in/from the congress network. We need to know more about formal and informal ways in which speakers and their audiences were recruited, and decisions were made to establish new sections. What is clear at the moment is that in the late 1860s-1880s the university faculty played the dominant role in the congresses providing the key speakers, while the most visible groups among the public were secondary school teachers and doctors. By the 1890s-1900s, as the audience expanded dramatically and new disciplines were introduced into the program, the conventions of Russian naturalists and physicians began to attract a very different social stratum of professional people, experts in applied disciplines, such as agronomy, veterinary, forestry, and statistics. These people, who often lived and worked outside of the university centres, were instrumental for the dissemination and popularisation of science in the Russian empire in the early decades of the 20th century.

References

BASTRAKOVA, M.S. (2007) 'Nauchnoe soobshchestvo Rossii vo vtoroi polovine XIX v. (stanovlenie, osobennosti, oblik)', in *Arkhiv istorii nauki i tekhniki,* 3, Moscow: Nauka, pp. 32-64.

BORISIAK, A.A., MENNER, V.V. (1939) 'Mariia Vasil'evna Pavlova', *Vestnik AN SSSR*, no. 6, p. 78-80.

CASALENA, M.P. (2006) 'Opposizione e integrazione. La scienza nazionale nelle capitali e nelle province (XVIII-XIX secolo), *Storicamente*, no. 2, http://www.storicamente.org/02casalena.htm.

CASALENA, M.P. (2007) 'The congresses of Italian scientists between Europe and the Risorgimento (1839 - 75)', *Journal of Modern Italian Studies,* vol. 12, no. 2, pp. 153-188.

Dnevnik IX s''ezda russkikh estestvoispytatelei i vrachei (1894), Moscow.

Dnevnik IX s''ezda russkikh estestvoispytatelei i vrachei, nos. 1-10 (1894), Moscow.

Dnevniki XII s''ezda russkikh estestvoispytatelei i vrachei v Moskve s 28-go dekabria 1909 po 6-e ianvaria 1910 g. (1909-1910), Moscow.

ELINA, O. (2008) *Ot tsarskikh sadov do sovetskikh polei: istoriia sel'skokhoziaistvennykh opytnykh uchrezhdenii, XVIII –20-e gody XX v.*, 2 vols., Moscow.

FOX, R. (1980) 'The savant confronts his peers: scientific societies in France, 1815-1919', in *The organization of science and technology in France 1808-1913*, eds. R. Fox and G. Weisz. Cambridge: CUP, pp. 241-282.

LEBEDEV, G.S. (1992) *Istoriia otechestvennoi arkheologii, 1700-1917*, St. Petersburg: St. Petersburg University Press.

LIUBIMOV, N (1869) *S''ezdy estestvoispytatelei v Shveitsarii, Germanii i Anglii*, Moscow, 1869.

MITROFANOVA, E.E. (1905) *Pamiati S.M. Pereiaslavtsevoi*, Warsaw.

MOGILNER, M. (2008) *Homo imperii: Istoriia fizicheskoi antropologii v Rossii (konets XIX – nachalo XX v.)*, Moscow: Novoe literaturnoe obozrenie.

POGOZHEV, A.V. (1887) *Dvadtsatipiatiletie (1861-1886) estestvennonauchnykh s''ezdov v Rossii*, Moscow.

Protokoly zasedanii Vtorogo meteorologicheskogo s''ezda pri Imperatorskoi Akademii nauk 11-17 ianvaria 1909 g. (1910), St. Petersburg.

Rechi i protokoly VI s''ezda russkikh estestvoispytatelei v vrachei v Sankt-Peterburge (1880), St. Petersburg.

Spisok chlenov IV s''ezda russkikh estestvoispytatelei (1873), Kazan'.

TIKHONOVICH, N.N. (1953) 'S''ezdy russkikh estestvoispytatelei i vrachei', in *Ocherki po istorii geologicheskikh znanii*, 1, pp. 95-119.

Trudy III s''ezda russkikh estestvoispytatelei v Kieve, proiskhodivshego s 20 po 30 avgusta 1871 g. (1873), Kiev.

Trudy pervogo s''ezda russkikh estestvoispytatelei v Sankt-Peterburge, proiskhodivshego s 28 dekabria 1867 po 4 ianvaria 1868 g. (1868), St. Petersburg.

Trudy V arkheologicheskogo s''ezda v Tiflise, vol. 1 (1887), Moscow.

Trudy V s''ezda russkikh estestvoispytatelei i vrachei v Varshave, proiskhodivshego s 31 avgusta po 9 sentiabria 1876 g., part 1 (1877), Warsaw.

Trudy VI arkheologicheskogo s''ezda v Odesse (1884), vol. 1 (1886), Odessa.

Trudy Vserossiiskogo s''ezda deiatelei po prakticheskoi geologii i razvedochnomu delu (1908), St. Petersburg.

Trudy XIV arkheologicheskogo s''ezda v Chernigove, 1909, vol. 3 (1911), Moscow.

VIII s''ezd russkikh estestvoispytatelei i vrachei v Sankt-Peterburge ot 28 dekabria 1889 g. do 7 ianvaria 1890 g. (1890), St. Petersburg.

Vtoroi s''ezd russkikh estestvoispytatelei v Moskve s 20-go po 30-e avgusta 1869 g. (1869), Moscow.

Vysochaishe utverzhdennyi X s''ezd russkikh estestvoispytatelei v vrachei v Kieve (1898), Kiev.

WITHERS, Ch., HIGGITT, R., and FINNEGAN, D. (2008), 'Historical geographies of provincial science: themes in the setting and reception of the British Association for the Advancement of Science in Britain and Ireland, 1831-c.1939', *British journal for the history of science*, vol. 41, no. 3, p. 385-415.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 1050.

THE INTERNATIONAL CONGRESS OF ELECTRICIANS OF PARIS AT 1881 AND ITS CONSEQUENCES IN COIMBRA

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Abstract

We analyze the repercussions in Portugal of the International Congress of Electricians, held in Paris in 1881, which was attended by the Portuguese representative, Professor of Physics, António dos Santos Viegas. We focus on the analysis of the motivations underlying its realization and the impact of its resolutions on the development of Physics at the University of Coimbra, since the Portuguese representative in this Congress was professor of physics at this institution for more than 50 years.

Portugal in the International Congresses of unit systems

In 1852 Queen Maria II adopted the decimal metric system, already introduced in France in 1791, establishing a stated period of 10 years for its general application. Simultaneously it was created in the Ministry of Public Works, Commerce and Industry, the Central Commission of Weights and Measures. In 1855 it was created the General Inspection of the Weights and Measures of the Kingdom, having been its first General Inspector, *Joaquim Henrique Fradesso da Silveira* (1825-1875), professor of Physics and Chemistry of the Polytechnic School of Lisbon. In 1859 he published the Compendium of the Metric System, which helped to implement this system in the country.

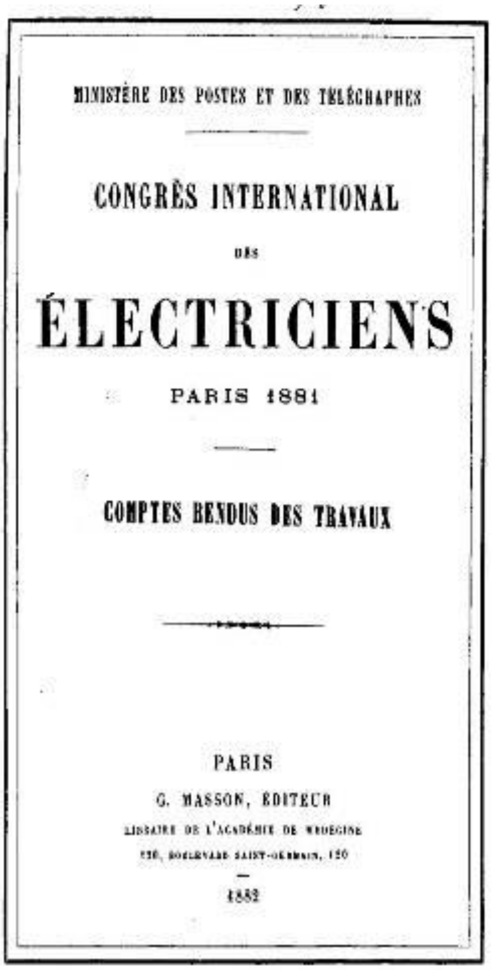
The *Convention du Mètre* was signed on 20th of May 1875 by 17 countries. Portugal was represented by the plenipotentiary minister in Paris, Jose de Silva Mendes Leal (1820-1886). This Commission aimed to establish an international authority in the metrology field, responsible for the development and safeguard of the measurement standards. Mendes Leal, who was also the Portuguese Commissioner in the Universal Exposition of 1878, had an important role in the negotiation of the Telegraphic Convention between Portugal, Spain and France, signed in Paris in 1880.

The International Congress of Electricians held in Paris in 1881 was the first step for an important series of accomplishments of international level. Between 15th September and 5th October, this meeting gave official and international recognition to the proposals of the British Association for the electric units Volt and Ohm, and added the Ampere, Coulomb and Farad to the two previous units. [1]. Later on, in the Congress of Chicago (1893), the nine electromagnetic units were adopted in successive steps: the Ohm, Volt, Ampere, Farad, Coulomb, Joule, Watt, Henry and Weber. In 1933, Giovanni Giorgi (1871-1950) displayed the system that he already had presented in 1901, in Rome, which had as basic units the meter, the kilogram and the second –the MKS system– that should be applied in 1940 for deliberation of the *International*

Electrotechnical Commission (IEC), but that this only happened eight years later. This system was called *International System* in 1960.

The International Congress of Electricians of 1881

Scientists and telegraphers congregated in Paris in 1881 (Figure 1), under the sponsorship of the French *Ministère des Postes et des Télégraphes*, recognized the necessity to fix electric units, so that all the measures were comparable in all countries. The choice of a French city became obvious because France played a central role in the international metrology based in the creation of the metric system and in the presence, in the city of Paris, of the *Bureau International des Poids et Mesures*.



MINISTÈRE DES POSTES ET DES TÉLÉGRAPHES

CONGRÈS INTERNATIONAL

DES

ÉLECTRICIENS

PARIS 1881

COMPTES RENDUS DES TRAVAUX

PARIS

G. MASSON, ÉDITEUR

LIBRAIRE DE L'ACADÉMIE DE MÉDECINE

120, BOULEVARD SAINT-GERMAIN, 120

1882

Fig. 1: *Comptes rendus des travaux* of the International Congress of Electricians of 1881.

This congress had a strong contribution of the Telegraphic Engineers. The Telegraph Engineers Society had been established on 17th May 1871, in a meeting in Westminster - Victoria Street, London, and Charles William Siemens (1823-1883) had been invited to be its first president. On 22nd of December 1880, this British institution started to be named *Society of Telegraph and of Electricians*. Alexander Trotter Pelham (1857-1947), who joined the institution in 1885, thinks that the term "electric engineering" started to be used in the exposition of Paris in 1881. On 10th November 1887, the previous Society of Telegraph Engineers received the name of *Institution of Electrical Engineers*. Nowadays it is known as *Institution of Engineering and Technology*, currently the second biggest institution of engineering of the world.

The Congress established the definitions of the following physics units: Ohm, Volt, Ampere, Coulomb and Farad, but they could not obtain the definition of the unit of force that, not being considered basic, had also to be defined in the scope of the electricity. A proposal was also made to a definition of a luminous unit and an adjusted standard was chosen to measure the intensity of light.

In the last day of the Congress, on 5th October, the Permanent Secretary of the Academy of Sciences, Jean Baptiste Dumas (1800-1884) a French chemist, dedicated his speech to the conquests of the *British Association*, standing out that the vast industry based on the electric power (…), could only effectively progress if the measures used by several countries were standardized [2]. So, we can affirm that the Congress of the Electricians in Paris emerges as one of the most excellent examples of the globalization of science.

Three goals were reached with this event: the normalization of the electric units, the presentation of important works in the scope of terrestrial magnetism, meteorology and telecommunications and the conscience by electricians of the necessity of interfaces that bound electric installations between them. In this Congress the presence of German, Belgian and even British members was very strong including also the French. From Portugal, the plenipotentiary minister in Paris was invited as a extraordinary guest, as well as the Honorary State Minister and Pair of the Kingdom of Portugal, João Andrade Corvo (1824-1890), the Council Guilhermino Augusto de Barros (1835-1900), general Director of the Post Offices, Telegraphs and Lighthouses of the Kingdom of Portugal, and also António dos Santos Viegas (1836-1914), professor of Physics at the University of Coimbra, UC, and its rector twice [3]. Barros would become an important figure because he had a preponderant role in the concession contract with *Edison Gower Bell* on 13thJanuary 1882, that gave this company the opportunity to install all the telephonic stations in Lisbon, Oporto and other suburban areas.

Fig. 2: António dos Santos Viegas, professor of Physics at the University of Coimbra.

The 2nd session of the Congress, on 17th September, congregated the discussion about globe's physics, terrestrial magnetism and atmospheric electricity, under the presidency of Dumas. The Director of the Magnetic Observatory of the UC, Santos Viegas (Figure 2), affirmed that this institution used the methods of Gauss with the modifications introduced by the German physicist Johann von Lamont (1805-1879) and the instruments recommended for the Kew Observatory. He considered, as a starting point for an international standardization, that two things should be determined: the nature of the observed phenomena and the possibility to choose standard instruments to be used in the observations. After some interventions, Éleuthère Mascart (1837-1908) read the final resolutions that seated on the measures that would have to be made by some telegraphic administrations in order to organize a systematic study of terrestrial electricity. It was considered that those measures should be collected under an International Committee, and in the impossibility to get this organization it would be desirable that, at least, the observations were made in specific days chosen by the International Commission of the Polar Year established in 1879.

This Commission was in charge of using the observation methods for the atmospheric electricity with the intention of generalizing the study of the terrestrial surface. These resolutions suggested that Santos Viegas' intervention was an important contribution in the systematization of the studies related to Earth Sciences. The Commission integrated different country representations: Denmark, Norway, Russia, Sweden, Finland, Germany, the Austro-Hungarian Empire, the Netherlands, France, the United States and Great-Britain, with the support of the new Domain of Canada. It also established that the first International Polar Year would be carried through in 1882-1883, to coincide with a transit of Venus foreseen for day 6th of December 1882.

Resolutions of the Electricians Congress of 1881 in the UC and its Repercussions

The Congress of Paris of 1881 had strong repercussions in the development of Physics in the UC. António dos Santos Viegas was, after the Congress, one of the founders of the *Société Internationale des Electriciens*, renamed later on *Société Française des Electriciens*. During the second half of the 19th century, the international contacts made by Santos Viegas, Professor of Physics for more than 50 years, had been fundamental for the development of the scientific instruments collections of the Physics Department and the Meteorological and Magnetic Observatory of the UC. In 1883 he was member of the Scientific Commission of the International Exposition of Electric Engineering, in Vienna, led by Jozef Stefan (1835-1893), director of the Institute of Physics of Vienna. In his visits to France, Belgium, England, Scotland, Germany, Austria, Italy, among other countries, Viegas knew and worked with many famous scientists such as Bourbouze, Bianchi, Jamin, Verdet, Cornu, Regnault, Secchi, Thomson, Koenig, Helmholtz or Kirchhoff.

At the *Conservatoire des Arts et Métiers*, Viegas also knew Alexandre Edmond Becquerel (1820-1891), Henri Becquerel's father (1852-1908) who discovered radioactivity. Under his influence, teaching experimental Physics at UC become very similar to French schools and being much close to the *École Polytechnique* of Paris. Santos Viegas' supervision allowed the instruments collection at the Physics Department to be continuously updated. In its vast collection there were several instruments made by Duboscq and Pellin, Hardy, Salleron, Deleuil, Ducretet, Carré, Breguet, Bianchi, Koenig, Ruhmkorff, Ernecke, Muller-Unkel, Geissler, Siemens & Halske, Casella, Kohl, among others.

Santos Viegas had much influence in the scientific panorama of the UC during the second half of the 19th century, mainly in the area of Physics. The subjects studied by his students were characterized by an excellent scientific actuality. Until the 60s, the theses defended in the Faculty of Philosophy, FPh, in Coimbra, a subject dealt with some relevance was meteorology, a fact that is related with the foundation of the Meteorological and Magnetic Observatory in 1864. From that point on, all subjects of academic papers had started giving importance to electromagnetism, such as the academic thesis of Adriano de Paiva Brandão (1847-1907), in 1868, as well as the ones of Henrique Teixeira Bastos (1861-1943) in 1884. Here we can notice an increasing interest for this subject, already evidencing the influence of the resolutions of the Congress of Paris. In 1897, in the António Vellado Pereira da Fonseca's theses (1873-1903), electromagnetism starts occupying a central place in the subjects studied. In this work Röntgen rays are also mentioned, which shows modern aspects in the physics syllabus in the FPh of the UC.

António Meirelles Garrido presented in 1878 a study on the Mathematic theory of propagation of light in a homogeneous environment [5], basing his study on the theory of mechanics' propagation of light. In 1879, Garrido presented another study: *the Radiometer*, in which he considered to make one brief summary of the facts established by the experience and to argue the value of different theories of radiometric movements [6].

Electric Units [7] was the theme of Henrique Teixeira Bastos' thesis in 1884. He was António Santos Viegas' disciple and this work could represent the first relevant influence of the Congress of Paris in 1881 on the Experimental Physics Department of the UC. In this study, the author displayed the system of electric units used by the time, dividing the book in three basic parts: Definition of the units, their comparison with the arbitrary units and their material representation. In the introduction, Teixeira Bastos made reference to the Congress of Paris of 1881, in which electric units would have been generalized. *Electromagnetic Theory of Light* (1885) was the subject of his dissertation for admission to university career [8].

In the sequence of the subsequent developments in electromagnetism, in 1897, Vellado Pereira da Fonseca studied electric oscillators [9]. This work was published in two parts: the first one refers to the optics of oscillations and the second one, which was a scientific study submitted to his admission to university career, he analyzed the effects of electric oscillations. The second half of this work had a more practical source. It talked about electric oscillations' potentialities used for illumination through the Tesla effect . Fonseca concluded his work with an entire chapter dedicated to wireless telegraphy, making reference to some wireless message transmission methods, based on electromagnetic radiations.

Anselmo Ferraz de Carvalho (1878-1955) presented academic theses, where we can see subjects related to electromagnetism, meteorology and X-rays. In 1901 he defended his scientific study on *Magneto-Optic Phenomena* in which he studied two different Physics' areas: electromagnetism and light [10]. He showed knowledge of recent works carried by other physicists in this area.

Beyond the academic theses, some scientific articles had been published in the scientific and literary magazine *O Instituto* (*IC*), published in Coimbra since 1852. Some different articles were related with electricity or electromagnetism, but some of them were distinguished by its relevance on electric telegraphy, as Jose Maria Abreu's articles [15].

At the end of the 70s new articles had started to appear in *IC* related with electromagnetism applications: Adriano de Paiva Brandão in 1878 and 1880 –*Telephony, telegraphy and telescopy* and *electric telescopy* [16]. In the 1878's article, Paiva argued about the advantages of Bell's telephone and its ability to transmit sound messages. He concluded that, in the future, we would obtain a technology based in photoconductive properties of selenium, discovered in 1876 by Werner Siemens (1816-1892), foreseeing the possibility to convert optical images into electric impulses that would be transmitted by telegraphic wires. Therefore, Paiva is considered a precursor of television [16]. Effectively, his successors only appeared some time later. English physicist Shelford Bidwell (1848-1909) published on 10th February 1881, the article entitled 'Tele-Photography' in *Nature*. About one month later, on 11th March, he presented to the London *Royal Institution* the communication on *Selenium and Its Applications to the Photophone and Telephotography*, coming back to the subject in the same magazine on 4th June 1908, with the article *Telegraphic Photography and Electric Vision*. A few days later, on 18th June, the Scottish Campbell Swinton (1863-1930) published an article in *Nature*, with the title *Distant Electric Vision*, in which he suggested the use of cathode rays to obtain images.

In 1903, Álvaro José da Silva Basto published six articles under the subject *Phenomenon and experimental disposals of wireless telegraphy* [17].

The first studies on radiation Physics in Coimbra

Henrique Teixeira Bastos also wrote some articles about the first experiences on X- Rays. In his article [18], he described the experiences made at the UC in January and February of 1896. The discovery of X rays originated an immediate scientific interest in Coimbra. Only about one month after the announcement of Röntgen's discovery, the first essays were made in the Cabinet of Physics of the UC, in contribution with the professors of the Faculty of Medicine, guided for its application in clinical diagnosis. It started a tradition of contributions between these two institutions that lasted until nowadays. A result of this was the creation of the Laboratory of Radiology of the Faculty of Medicine of the UC. In 1901, the decree nº 4 of 24th December, established the *Bases for the Reorganization of the University of Coimbra* and promulgated the creation of the Radioscopy and X-ray Department in the UC's Hospital.

In the experiments made by Bastos, a Ruhmkorff bobbin was excited by six electric batteries of Bunsen and an electrical discharge was produced in a Crookes tube, next to which was placed a photographic plate and on the top of it the experimental object, for about 20 minutes of exposition. He recognized that the obtained results were good; even so the experiences were delayed. At the time Teixeira Bastos could not foresee all the practical applications of this process. He just refers that it started to be used in UC Hospitals, more properly in the clinical diagnosis.

In the same year, in April, Teixeira Bastos published a new paper on the same subject [19] describing the recent works of Becquerel. He mentioned the *Scientific American* magazine of that same year, that explained the studies made in Edison's laboratory which showed that by using a new process, the human bones could be see using a fluoroscope, with bigger commodity for the observer. This technique was soon introduced in Coimbra.

Álvaro José da Silva Bastos (1873-1924) presented the academic thesis entitled *Cathodic Rays and X Rays of Röntgen* in May of 1897 [4]. In this work, the author mentioned the experiences which he carried out with electric discharges in gases, establishing the comparison between X rays and the rays of Becquerel, and analyzed several theories of the X rays' nature, describing several production and application techniques of these rays. In this work he presented references to some foreign publications, such as Perrigot's in April of 1897 in the Academy of Sciences of Paris published in the *Comptes Rendus*, Gustav Le Bon's publications in April of the same year, and Henri Becquerel's on 10th May. It equally showed a deep knowledge of the evolution technique and the experimental results concerning this subject.

In its appliance paperwork to teaching in the Polytechnic Academy of Oporto, entitled *Becquerel Rays and Polonium, Radium and Actinium* (1902), Alexander Alberto de Sousa Pinto (1880-1982), who had recently concluded his studies at the UC, recognized that the scientific development, subsequent to December 1895, promoted an impressive series of research on Becquerel Rays [11]. By that time this subject was considered extremely important by the international scientific community. There was a raised number of published works that Sousa Pinto had access to, little before printing his own dissertation. Some of the cited works are: Henri Becquerel's publications in the *Comptes Rendus* t. CXXXIII, 199 and 977 of 22nd July and 9th December 1901; Pierre Curie's publications in the same magazine in the t. CXXXVIV of 17th February 1902. In its final note, he gave special relevance to Madame Curie's work on the determination of radium's atomic weight published in the same magazine, t. CXXXV, p. 181, of July 1902; Hoffmann and Strauss' work about obtaining preparations of radioactive lead published in the magazine *Berichte Deutschen Chemical Gesell* of 18th January 1902 and in the *Chemical News* in February of the same year; denoting not only actuality, but also access to several works made by physicists all over the world.

The radioactivity subject continued to deserve some importance in the UC Physics Department. In 1906, João de Magalhães presented a thesis: *Radio and Radioactivity* [12]. In 1906 and 1907 he published eleven papers about this matter in *O Instituto* of Coimbra [13]. Francisco Martins de Souza Nazareth submitted, in 1915, the paperwork for his appliance to Second Assistant for UC's Faculty of Sciences which subject was: Ionization of gases in closed vases [14]; it was the first experimental work in Coimbra involving radioactivity detection.

Conclusion

In conclusion, the International Congress of the Electricians of Paris in 1881 changed everything and had an extreme impact on people's life, and brought some academic consequences in Portugal. Professors and investigators started to invest more in the study of electromagnetism, which allowed to the development of this science in teaching and/or research, or in its applications. The access to the information of what was happening in foreign countries, as well as the representation of the Faculty of Philosophy of UC in scientific congresses and other scientific events had influenced academic interests in knowledge areas such as electricity, magnetism and electromagnetism, specially in the 70s; that can be easily proved through published works.

References

[1] JANET, Paul M. (1909) l'Histoire el l'état actuel de la question des unités électriques fondamentales*, Journal de Physique*, 4ª série, t. VIII, p.37-38.

[2] TURNBRIDGE, Paul (1992) *Lord Kelvin, His influence on electrical measurements and Units* (Peter Peregrinus Ltd. on behalf of the Institution of Electrical Engineers).

[3] FRANCE, MINISTERE DES POSTES ET DES TELEGRAPHES (1882) *Congrès Internationale des Electriciens, Paris, 1881, Comptes Rendus des Travaux*. Paris: G. Masson.

[4] BASTOS, Álvaro José da Silva (1897) *Cathodic Rays and X Rays of Röntgen*, Inaugural Paperwork to Magnanimous Conclusions Act in College of Nature Philosophy of University of Coimbra.

[5] GARRIDO, António Meirelles Pereira Coutinho (1878) *Mathematic Theory of Light propagation in homogeneous environment*, Inaugural Paperwork to Magnanimous Conclusions Act in College of Nature Philosophy of University of Coimbra.

[6] GARRIDO, António Meirelles Pereira Coutinho (1879). *Radiometer*, Appliance Paperwork, presented to College of Nature Philosophy of University of Coimbra.

[7] BASTOS, Henrique Teixeira (1884) *Electric Units*, Inaugural Paperwork to Magnanimous Conclusions Act in College of Nature Philosophy of University of Coimbra.

[8] BASTOS, Henrique Teixeira (1885) *Electromagnetic Theory of Light*, Appliance Paperwork, presented to College of Nature Philosophy of University of Coimbra.

[9] FONSECA, A.A.M. Vellado Pereira da (1897 a) *Electric Oscillations, I, Optic of Oscillations,* Inaugural Paperwork to Magnanimous Conclusions Act in College of Nature Philosophy of University of Coimbra.

[10] CARVALHO, Anselmo Ferraz de (1901) *Phenomenos Magneto-Optics,* Inaugural Paperwork to Magnanimous Conclusions Act in College of Nature Philosophy of University of Coimbra.

[11] PINTO, Alexandre Alberto de Sousa (1902) *Becquerel Rays and Polonium, Radio and Actinium*, Appliance Paperwork for Teaching in Polytechnics Academy of Port.

[12] MAGALHÃES, João de (1906) *Radio and Radioactivity*. Coimbra: University Press.

[13] MAGALHÃES, João de (1907) Radio and Radioactivity. *O Instituto*. Coimbra: Press University. Vol.53, p. 309-314, 357-365, 433-440, 485-493, 551-561; 614-622, 684-694, 726-737, 1906; Vol.54, p.37-46, 98-111, 154-155.

[14] NAZARETH, Francisco Martins de Souza (1915) *Ionization of gases in closed vase*, Appliance Paperwork to Second Assistant of the 1st group of 2nd section College of Sciences of the University of Coimbra. Coimbra: University Press.

[15] ABREU, José Maria de (1855) Electric Telegraphy. Electric Telegraphy Origins. *O Instituto*. Coimbra: Press University. Vol. 4, p. 44-45, 110-112, 118-120, 141-142 , 1855; Vol. 5, p. 11-12, 43-44.

[16] PAIVA, Adriano de (1878) Telephony, telegraphy and telescopie. *O Instituto*. Coimbra: University Press. Vol. 25, p.414-421.

[17] BASTO, Álvaro José da Silva (1903) Phenomenos and experimental disposals of wireless telegraphy. *O Instituto*. Coimbra: Press University. Vol. 50, p. 279-284, 354-359, 408-414, 467-473, 676-680, 734 -738.

[18] BASTOS, Henrique Teixeira (1896) X – Rays of Röntgen. *O Instituto*. Coimbra: University Press. Vol. 43, p.38-41.

[19] BASTOS, Henrique Teixeira (1896) X – Rays of Röntgen, New Experiences. *O Instituto*. Coimbra: University Press. Vol. 43, p. 275-279.

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A STROBOSCOPIC PICTURE OF EUROPEAN SCIENCE ATTRACTIVE INSTITUTIONS IN THE THIRTIES AND FORTIES –FOLLOWING PORTUGUESE GRANT HOLDERS[1]

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Abstract

To understand the circulation of scientific knowledge and scientists in Europe in the 20th century it is essential to identify the institutions that attracted foreign scientists to all kinds of training. To construct such broad picture it would be compulsory to study the dissemination of scientific culture over Europe. To accomplish this, it would be necessary to assemble as many information as possible about specific institutions and scientists.
There are several analyses on the development of science, in particular European institutions and in specific disciplines, that referred the passage of foreign scientists to those places, as in Paris. However, this is not the main goal of the authors. Some work has been made on the history of Foundations where grants attributed can be studied, as for example the Rockefeller Institution. The institutions that manage and promote national science were also studied, and these played the principal role sending apprentices abroad. This is the case of the Spanish JAE, Junta de Ampliación de Estudios*.*
Currently, we study the Portuguese scientific apprentices in Europe and pretend to contribute to picture the scientific attractive institutions in the thirties and forties. We intend to show that, through time, there were geographical changes on those institutions at a particular discipline. We want to underline several factors influencing the selection of destinations and how these factors conditioned the circulation of scientists. The success, renown and visibility of an institution or scientist act together as a factor, but they are not by far the

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only one. In the Portuguese case we can identify other factors on different levels: the country's characteristics or constraints, the national institutions to where apprentices were connected, and the orientation of particular characters. However, besides all these factors we want to enhance the leading role of JEN (Portuguese Board of National Education) promoting scientific apprenticeships in Europe.

Introduction

The *Junta de Educação Nacional*, created in January 1929, was the first Portuguese organization intended to foment and manage the scientific investigation of the country. Despite the fact that JEN changed its name to "*Instituto para a Alta Cultura*" (IAC), in 1936, this institution kept all its former goals. JEN's main attributions were to organize Portuguese scientific work, to prepare technicians intending Portuguese economic improvement and to manage the dissemination of culture and intellectual interchanges. Its main action lines were to subsidize scientific institutions, to give grants to outstanding or promising investigators, and to promote the publicity of scientific products or results. JEN invested a big slice of its expenditure in grants, almost 50%.

The support to grant holders was intended to fulfil their professional education and attain their "framing" into the modern knowledge abroad as a way to improve their productivity. It was imperative to give them a broad picture about the scientific activities of other countries, for instance, stimulating their participation in Congresses. When a grant holder come back to Portugal after its formation, JEN was supposed to settle him in proper institutions or improve their former work conditions. Besides this grants to foreign countries, JEN also attributed grants to support works in Portugal. Usually the number of grants to foreign countries (roughly 32 per year) was greater than the grants to Portugal (near 23 per year). Grants to foreign countries constituted around 39% of JEN's total expenditure and national grants constituted nearly 10%.

At this paper we focus on JEN's science grant holders that undertook their apprenticeships at foreign countries, mainly in Europe, in the 1930s and 1940s. To study JEN grant holders that were spread on the international space we have turned to first-hand documents: JEN's reports, Proceedings of JEN's Executive Council, and grant holders' archive information. As international circulation of science was the central interest of the 4th ESHS Congress, we directed our analysis to the group of European places travelled by JEN Portuguese grant holders. We characterized these places considering their attractiveness to persons that intended to undertake an apprenticeship. Several factors could contribute to their attractiveness, but they were essentially related to the existence of outstanding professors or to well-based institutions.

Extending the study about science grant holders in Europe

Analysing the works of History of Science about grant holders we can see that they followed some different ways of analysis depending on the considered study objects. (1.) A great part of those works deals with academic itineraries of specific scientists or their professional groups. As an example we can point out some studies about the "*Junta de Ampliacion de Estudios*" (JAE) grant holders, from Spain: following JAE's 2007 centenary, Spanish historians of science published *Tiempos de investigación* about JAE activities, where they developed a particular chapter about Ramon y Cajal, Ignácio Bolivar and José Castillejo. They were men compromised at JAE's development and management. (2.) Other kind of grant holders' analysis focuses on the development of a specific scientific area at a country or region. For instance, this constitutes the major part of the book *Tiempos de investigación* (the one already referred above about Spanish JAE), as it presents different chapters about Pedagogy, Physics and Chemistry, Natural Sciences and the Botanical Garden, Philosophy, Archaeology, Biomedical science, Technology and Applied science, etc. These works are showing grant holders' preponderant role to the development of science, mainly in Spain. In another book, Yves Gingras' *Physics and the Rise of Scientific Research in Canada* (1991), its author studied the creation of Canada Physics Community and he emphasised grant holders' roles. (3.) There are some historical works studying a specific scientific institution, its actors and activities, mainly on local context. (4.) Others are studying the philanthropic foundations that supported scientific activities, both learning and investigation, as Rockefeller, Ford and Carnegie Foundations. The book by John Farley *To cast out disease: a history of the International Health Division of the Rockefeller Foundation (1913-1951)* (2004) is an example, but it didn't focus on grant holder's activities or enhance their importance to the international scientific movement. The book *Rockefeller and the internationalization of mathematics Between the Two World Wars* (2001) by Reinhard Siegmund-Schultze is focusing on Rockefellers' mathematics grant holders. Siegmund-Schultze refers to Rockefeller's scientific policy and its selection criteria regarding grant holders and institutions subsidized. Our present work has similar aspects to this Siegmund-Schultze book as analysing grant holders' selection process. However, there are differences between JEN and Rockefeller, for instance on their managing role and objectives, and of course, on the historical contexts beneath their grant's services. Siegmund-Schultze has focused on the thematic of internationalization of science, based on the scientific activity of Rockefeller's grant holders and subsidized institutions. At our present study we are taking for granted that grant holders have somehow participated at the international circulation of science. We have a different goal from Siegmund Schultze's book: we intend to exploit the information about grant holders' staging places to outline a geographic representation of the "Scientific Europe" and most importantly, a moving picture of the International Circulation of Science.

Changing science apprenticeship places

Analyzing the geographical destinations of JEN grant holders, its relation with the grant scientific area, and its variation over the time (1929-1938) we can identify some tendencies and thus endeavour some brief conclusions.

At first sight we can see that the number of JEN grant holders to foreign countries has geographically changed. It is important here to clarify that JEN never specified any annual number of grants destined for each country. The three most chosen countries to go abroad were France, Germany and England. Besides these, grant holders have gone to Switzerland, Italy, Belgium, Poland, Yugoslavia, Spain and the United States. Considering all the grants given to all the countries, France was the most chosen country, followed by Germany. If we consider only the average numbers, at France there were present 12 Portuguese grant holders per year and at Germany there were 11 of them. At England there were only 6 grant holders per year.

Some grants of a specific scientific discipline have been undertaken at the same staging places. So, selecting the staging places was certainly related with the scientific themes of the grants. At Physics, Chemistry, Mathematics and Astronomy, all together, Paris was the most chosen destination (49 grants out of a total 98, i.e. 50%). Generally, the stages on Engineering, Medicine and Biology were spent in Germany. Some places corresponded to just a scientific discipline, as Physics at Dahlem and Giessen (Germany), Medicine at Dublin (Ireland) and Stockholm (Sweden), Geology at Lille (France), Malariology at Skopje (Yugoslavia), etc. Three grant holders on Sciences of Animal and vegetable Production have made their training at the USA (California, Wisconsin, New York and Providence), and they were the only ones going there.

If we take a specific discipline, the staging places selected were not the same along time, as we can see in figure 1, considering Medicine (joining all medical practice, surgery and Biomedicine).

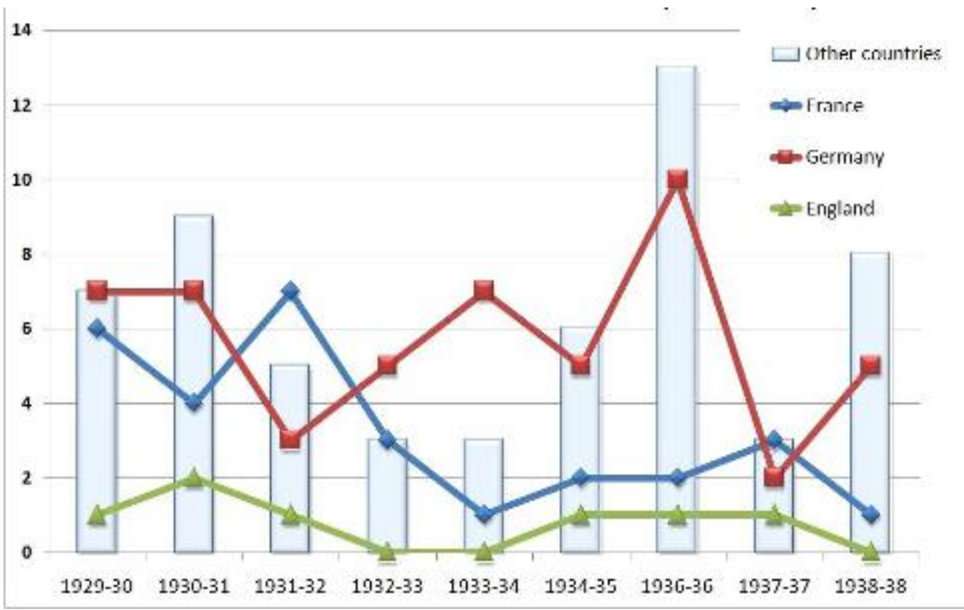

Fig. 1: Number of grants to Medicine (1929-1938).

From figure 1 we can see that in 1929-30 JEN' major number of grant holders was in Germany, and in 1931-32 they diminished there to rise in France. After 1933-34 most of grant holders were going to Germany again. At medicine there were always less grant holders at England than in other countries. We can display a similar analysis to other disciplines.

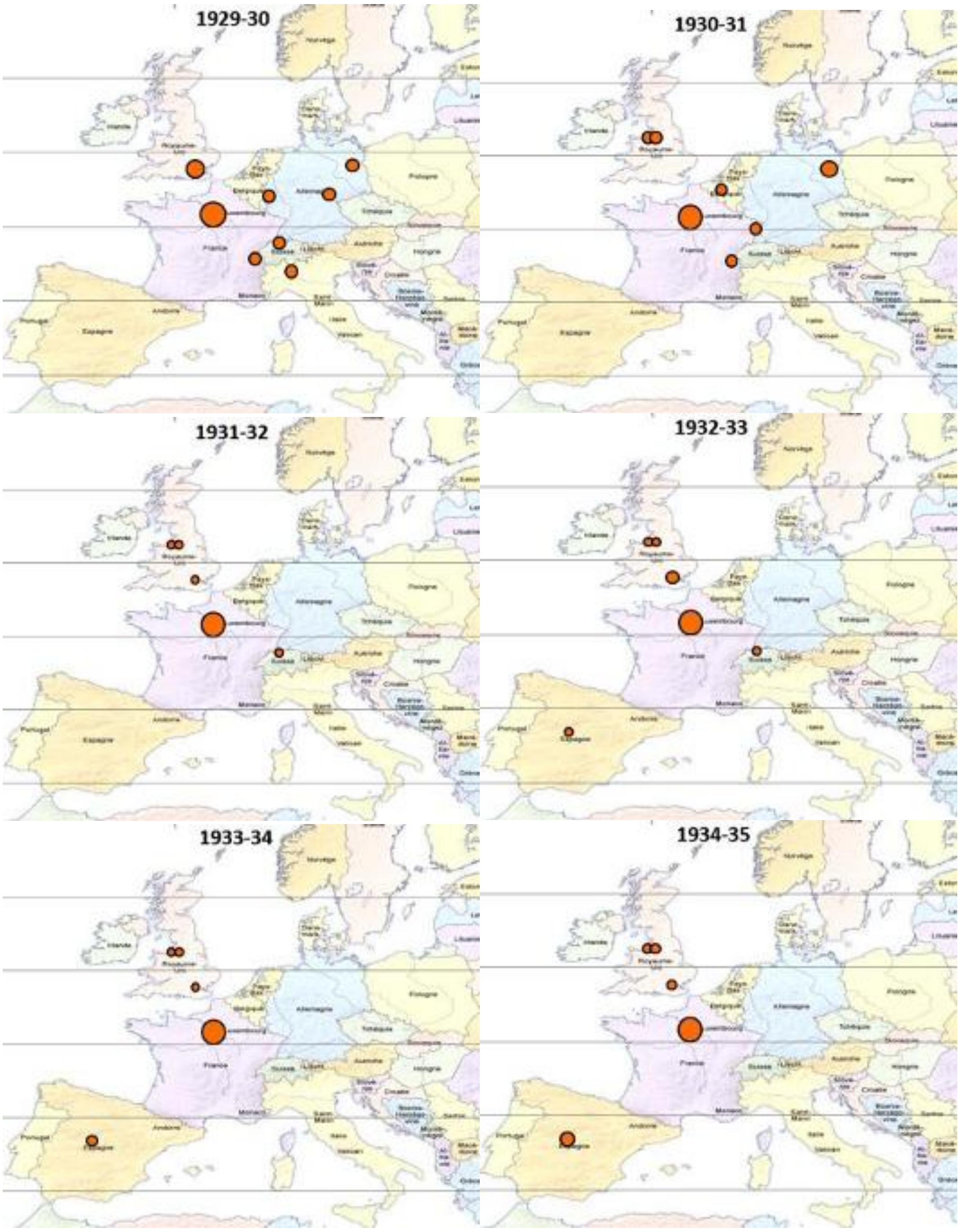

Fig. 2: Sequence showing the destinations for grant holders of Physics, Chemistry, Mathematics and Astronomy.

A dynamic picture of grant holders in Europe as a visible sign of science circulation

We can represent on a geographical map the places where JEN grant holders have been, considering a specific scientific discipline at a given year. Then we can use the stroboscopic representation method to illustrate the flow of time. This is similar to what happens when we are studying the movement of an object through a set of photos taken one after another in the same break of time.[2] If we watch the sequence of maps with the grant holders' places, we can get an idea of

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[2] We have annual information about the number of JEN grant holders in foreign countries for the period 1929-1938, which is different from having continuous information through time. In Physics, the science that studies movement, when there isn't enough data to study the movement of an object, one usually recurs to a series of pictures taken at regular time periods, framing different positions of an object

the grant holders' movement. We are showing this process for a group of grant holders of Physics, Chemistry, Mathematics and Astronomy, using maps of Europe (see figure 2).

The above maps sequence can be shown in movement using a computer animated image (GIF). This sequence represents both the movements of grant holders towards their multiple destinies of apprenticeships in Europe and the circulation of grant holders in the European space.

Influencing grant holders' movements in Europe –To select an apprenticeship place

The variation of grant holders in space along time depended at least on two big factors: (A) the European and Portuguese historical context and (B) the motives underlying the selection of each apprenticeship place. Regarding this second theme, we have analyzed the primary Bibliography with the aim of **characterizing the factors** that could influence in the selection of apprenticeships places. This is obviously a subjective construction, but it allowed us to typify some reasons directly influencing that selection.

(1.) The most meaningful motivation to select a place was its scientific proficiency at an international level, due to its outstanding professors or to its great scientific establishments. A grant holder working on a particular theme surely identified the most suitable staging place to him, for instance, an institution where he could enlarge his technical skills. Engaging on a specific professor was meaningful to some grant holders. For instance, the grant holder Francisco de Paula Leite Pinto "Asked to attend the Sorbonne Courses on «Astronomie Approfindie» by professors Andoyer and Lambert and the death of professor Andoyer compelled the grant holder to reconsider its apprenticeship plan".[3] Different personalities could advise grant holders regarding to what they thought to be outstanding places to stage. Those could be, for instance, renowned national professors or another Portuguese grant holders being at foreign places.

(2.) Previous personal contacts among Portuguese personalities and foreign scientists or institutions could influence the staging selection. The Portuguese interlocutor could be some professor related to the grant holder or even himself, as some grant holders staged at foreign places before their JEN's grant. For instance, before 1929 the Botany professor Aurélio Quintanilha had an apprenticeship in Berlin, supported by his Faculty. Having a JEN's grant in 1929-30, Quintanilha was intended to continue his studies in Berlin. For example, in the *Pflanzenphysiologisches Instituti* he would continue his work directed by professor Kniep. He had also grants to Berlin from JEN for the following years 1930-31 and 1931-32.

(3.) Sometimes the grant holder selected an apprenticeship place because the scientific activities undertaken there were similar to his employment institution. At that place he could learn some new aspects to improve his performance. The grant holder Arnaldo Rocha was an example:

> "Before asking JEN's grant he was studying the problem of corrosion in Oporto water […]. The complete range of possible studies couldn't be studied in just one institution […]. JEN gave him a 3 months grant […] to the following institutions […] where a lot of water analysis was performed."[4] JEN's Report to 1930-1931.

Another grant holder, António Costa Cabral, engineer at the Laboratory of Forest Technology of the Superior Institute of Agronomy, made a stage at the Madrid Forest Institute to study a specific method to produce wood pulp because it's application in the national [Portuguese] laboratory would be financially advantageous (JEN's Report to 1930-1931). Two naturalists from the Zoology Museum of the University of Lisbon, Amelia Vaz Bacelar and Fernando Frade Viegas da Costa undertook their stages in Natural History museums from Paris, London and Berlin and visited other European ones (JEN's Report of 1930-1931).

Sometimes it was the grant holders' institution that sent them abroad to get a specific training because they needed to apply the acquired knowledge. The stage of Arnaldo de Almeida Dias was an example: he was Chief of the Neurological Laboratory of the Faculty of Medicine of Lisbon and the Scholar Council of this Faculty charged him, within an official mission, to study "pathological anatomy of the nervous system in Germany" (JEN's Report of 1930-1931).

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in the same frame –this is a stroboscopic representation. In our case, we can watch a quick sequence in the European maps with the staging places represented using a computer; we compare this to a stroboscopic representation. At this paper we just present the sequence of maps elaborated as we cannot give movement to the sequence.

[3] "Havia pedido para frequentar na Sorbonne os Cursos «Astronomie Approfondie» que os profs. Andoyer e Lambert regiam naquele estabelecimento […]. O falecimento do prof. Andoyer obrigou o bolseiro a requerer à Junta modificação do seu plano inicialmente traçado." JEN's Report of 1930-31.

[4] "Antes de pedir a bolsa de estudo da Junta vinha já a ocupar-se do estudo do problema da acção corrosiva das águas do Porto […] O estudo completo dos métodos de investigação aplicáveis, não podia ser levado a cabo num único instituto […]. Foi-lhe concedida uma bolsa de 3 meses […] nas duas instituições seguintes: […] onde se executa um grande numero de analises de águas […]". JEN's Report of 1930-31.

(4.) Foreign institutions or organizations offered specific courses to Portuguese scientists and JEN grant holders took profit of them. Those institutions usually stipulated the place and even the scientific theme of the apprenticeship. For instance, the Society of Nations organized a Medical Course on Malaria destined to scientists from several countries and JEN grant holder Fausto Nunes Landeiro was engaged in it (1929-30). The General Direction of Health collaborated in this grant as he wanted to profit the grant holder's new knowledge to combat malaria in Portugal. The British Council was another institution that offered JEN its collaboration to grant holders in specific courses, as the two vacancies to Cambridge Colonial Course in 1938. The nominated grant holders were the colonial state workers Octávio Ferreira Gonçalves from Angola and Afonso Henriques Ivens Ferraz Freitas from Mozambique. The British Council supported them by, for example, giving them accommodation at a Cambridge college.[5]

JEN's process to select grant holders could have influenced the staging places selection. At first sight we notice that JEN didn't explicitly assume those influences as it didn't establish explicit rules to manage the grants selection of place or theme. Its regulations only required that the grant holder returned to Portugal after his staging abroad. JEN did not announce to have a bourse on any specific theme or country, but just waited for the candidate's suggestion.[6] This has limited the scientific areas covered, as was told by JEN's secretary Simões Raposo in 1932. However, we should consider that JEN possessed a Science Policy that could influence the selection of apprenticeship places.[7]

Conclusions

Our study allowed us to identify the international scientific places whose professors or institutions attracted JEN grant holders for apprenticeships, in the period 1929-1938. We have analyzed these places considering the grants' scientific areas and its occurrences through time. Focusing on a particular discipline, we can see that the staging places changed geographically through time. Some grant holders of the same scientific discipline chose the same staging places, so there exists a correspondence between a scientific discipline and its characteristic set of places.

If we serialize the maps of Europe with the staging places pinpointed we can construct a "stroboscopic representation" during a sequence of time. This kind of "picture in motion" represents the movement of Portuguese grant holders during the 1930s into the European space and the geographical variation of their staging places through time. This shows that JEN grant holders participated on the scientific knowledge circulation in the international sphere. We are working to discern concrete fluxes of grant holders through space during the referred time period. We also aim at distinguishing the institutions or places that fixed grant holders for most of the time. This is a very complex and lasting analysis that we has just been started.

Having identified a group of scientific places that attracted foreign scientists with learning objectives, we are contributing to understand the circulation of scientific knowledge around 20th-century Europe. However, this conclusion is limited by the fact that we are just considering the Portuguese grant holders. If we congregated the data of grant holders from several European countries, we would get a more comprehensive representation than ours. Then we could watch the geographical changing of apprenticeship places using a motion picture, and this would be a dynamical representation of the international circulation of scientific knowledge.

References

AASERUD, Finn (1990) *Redirecting Science - Niels Bohr, Philantropy and the Rise of Nuclear Physics*, Cambridge University Press, Cambridge.

AUSEJO, Elena (1993) *Por la ciencia y por la patria: La institucionalización científica en España en el primer tercio del siglo XX. La associación Española para el progresso de las ciencias*. Siglo veintiuno de España editores, Madrid.

CRAWFORD, Elisabeth (1992) *Nationalism and internationalism in Science, 1880-1939 Four Studies of the Nobel population*, Cambridge University Press, Cambridge.

FARLEY, John (2004) *To cast out disease: a history of the International Health Division of the Rockefeller Foundation (1913-1951)*, Oxford University Press, Oxford.

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[5] Letter by Oswald Scott from the British Embassy to F. De P. Leite Pinto, JEN secretary, 20 September 1938. Process entitled "Colonial Grants with collaboration of Btitish Council", Instituto Camões Archive, Number 1222/7.

[6] JEN used to publish in *O Século* Journal when the admission period for grant applications started, but it didn't explicit any limitation on themes or places.

[7] At our Project about JEN (FCT project HC/0077/2009) we are studying JEN's scientific policy.

Instituto para a Alta Cultura, [*Relatórios* diversos para 1936-1938].

Junta de Educação Nacional, [*Relatórios* diversos para 1928-1936].

KRIGE, John; BARTH, Kai-Henrik (2006) "Introduction: Science, Technology, and International Affairs", *OSIRIS*, n.º21, pp.1–21.

SIEGMUND-SCHULTZE, Reinhard (2001) *Rockefeller and the internationalization of mathematics Between the Two World Wars - Documents and Studies for the social History of Mathematics in the 20th century*, Birkhauser Verlag, Basel, Boston and Berlin.

SCHROEDER-GUDEHUS, Brigitte (2003) "Nationalism and Internationalism", *Cambridge History of Science, V. 5 - The Modern physical and Mathematical sciences*, Mary Jo Nye (ed.), pp.909-919.

PHYSICS IN THE 20TH CENTURY

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 1065.

THE PRINCIPLE OF POPULARIZATION OF ENERGY

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Abstract

We will examine the emergence of the principle of conservation of energy and its relation to the rise of science popularization.

On the one hand, the principle of conservation of energy is an uncommon example of widespread and simultaneous discovery. In fact, more than ten authors can be mentioned as discoverers or inventors of this fundamental principle, all during the middle of the 19th century. Although its genesis was closely related to research on the nature of heat, energy conservation did not arise to solve any specific age-old problem, nor did it belong to any specific theory. However, once firmly established, it has grown to become one of the most unquestioned principles of science.

On the other hand, popular science publishing underwent a notable, if not a radical transformation in the second half of the 19th century. Not only did this consist of a quantitative increase in scientific content; there was a significant qualitative change as well. In the 20th century, especially after World War II, popularization experienced again a radical change. Today, mention in the media of new scientific advances is far from scarce. In all of this proliferation the concept of 'energy' plays a crucial role. Moreover, its use has grown beyond scientific spheres, and we can now find it even in the most varied contexts. 'Energy' has become a ubiquitous word.

Is 'energy' an especially well-suited concept for popularizing purposes? It has been stated that the concept of energy had already existed in the minds of some of the authors involved prior to its 'discovery' as a metaphysical concept. Therefore, is this a case where society at large has come to influence technical and specialized research? Influences in both directions will be considered.

That the word *energy* is probably the most ubiquitous among those who live halfway between technical and common use is apparent at every turn. Perhaps the term *force* is one of its most serious competitors. However, with 'force', we usually can distinguish between its technical and common uses. On the contrary, the malleability of the meaning of energy is such that in many cases it is supposedly used with the same meaning. In a certain sense, energy is "malleability" itself, the pure "transformability," or "convertibility". One would never find "pure" energy, except in sport journalists' clichés. As Max Planck suggested in his book *On the Principle of Conservation of Energy* in 1887, little more can be said –or must be said– about energy besides that its amount is conserved.[1] Hendrik Kramers, who took active part in the construction of quantum mechanics, admitted that «the most fruitful concepts are those to which it is impossible to attach a well-defined meaning.» Energy is one of them.[2]

Digital age allows us to prove in few seconds that there's not a day in which the term «energy» does not appear in the newspapers. Ecology and energy industry have placed the word in the foreground of the news. There, it appears very often in relation to energy waste, global warming, or actions that a certain Minister of Energy or Energy Committee have decided to carry out.

Also the success of self-help manuals and recipes on how to live a full life has contributed to the omnipresence of the word.

Last but not least, the continued growth of scientific popularization industry allows the word to be over and over again a liaison between the jargon of the specialists and that of the common.

A History of Energy Popularization

As suggested by the title of our communication, the question we asked ourselves at the very beginning of our research was when and how this successful process of popularization took place. In other words, we wanted to do a history of the popularization of energy.

After our first steps in this direction, we realized that this approach needed to be nuanced in depth, if not radically changed. Firstly, because as Johnathan Topham and Bernadette Bensaude-Vincent, among others, have argued, the pattern *first-discovery –then-popularization*, can be naive or, in many cases simply false.[3] When scientists popularize for scientists –which happens quite often– they are forced to find new metaphors and reformulations which can really help themselves to rediscover what they were eager to popularize. Summarizing, we shouldn't understand popularization as a passive stage in the scientific development. This perfectly suits to the emergence of the conservation principle. Its consolidation as one of the firmest principles of physics took place for 20 or 30 years in which communications among scientists and communications to a wider audience –almost always educated people– played a crucial and active roll.

Even more. It's not appropriate to talk of «popularization» in the period in which the principle of conservation took shape. Certainly, at that time the expression "popular science" had been coined, but not «popularization.» Quoting once more Bensaude-Vincent, in those days «popular science» did not necessarily entail «science popularization». The gap between the professional and the amateur was not yet as wide as it became at the turn of the century, and much less than it is today, when at Geneva records on energetic collisions are broken in experiments in which more than 40 countries are involved and the same paper is signed by thousands of professional scientists.

This gap came a little earlier in the field of physics than in other disciplines. In the prologue of the first edition of the above-mentioned book by Planck, he expressed his displeasure with the fact that up to then, in 1887, the conservation principle had only been treated by specialists with a too much narrow focus, and in general books with a non-technical approach, with the exception of Hermann Helmholtz's memory.[4] Dan Wegener has seen in this work by Planck an attempt to demarcate the incipient field of theoretical physics.[5] We believe that Planck's intention can be further taken: He was attempting to distance physics from popular science through giving energy the rigor it lacked.

Following Buchwald and Hong, it can be said that «physics circa 1900 was the general science of matter and energy, exclusive of the properties by specific substances which may combine to form new substances».[6] Planck himself said, in 1905, that "besides the concepts of space and time, the concept of energy is the only one that is communal to all domains of physics."[7] Therefore, the emergence of energy doctrine is closely related to the birth of the discipline we nowadays call *Physics*.

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[1] PLANCK (1921), *Vorwort zu Ersten Auflage*.

[2] Quoted in ELKANA (1974), 161. According to Elkana, this is a "motto by H.A. Kramers quoted by Prof. Tisza in his 'Conceptual Structure of Physics', *Rev. Mov. Phys*. 35 (1962), p. 343."

[3] TOPHAM (2009), BENSAUDE-VINCENT (2001).

[4] HELMHOLTZ (1847).

[5] WEGENER (2010).

[6] BUCHWALD & HONG (2003).

[7] Quoted in WEGENER (2010), 150. It belongs to the lecture "Die Einheit des physikalischen Weltbildes", given by Planck in December the 9th, 1908, in Leiden.

The Principle of Conservation of Energy

Turning now to the History of the Principle of Conservation of Energy, we can say –again with Buchwald and Hong– that we lack a "comprehensive treatment" which includes the various studies devoted to different countries, disciplines, traditions and scientists. We can revive Planck's complaints, now referring them to the history of energy. Anyway, our communication is greatly indebted to the previous works by Kuhn, Elkana, Myers, Rabinbach, Smith, Harman, Brush, and Clarke, among others.[8] Also the papers by Pohl-Valero on the Spanish case are being quite helpful.[9]

Note the amount of different issues related to the question we raised. This wide scope affects not only traditional historiographical divisions such as internalism vs. externalism, but also to a tremendous complexity in the mutual influences of different factors in a crucial period of the History of Science. Thus, we are forced to escape specialization. What we present in this brief paper are not our definitive conclusions, but the current state of our research. Needless to say, any suggestion or objection from the improbable readers will be more than welcome.

As for the conservation, it is widely known that many authors made independent statements with very similar undertones in a relatively short time span, say between 1840 and 1850. Roughly speaking, the sources can be reduced to two: Germany, where the main character is Herman Helmholtz, and Great Britain, where it is more difficult to point a leader. In Germany, the interest in the preservation of what Helmholtz initally named "Kraft" came mainly from the fields of physiology and medicine. Many researchers were then interested in animal heat and vital dynamics. To this tradition belong the chemist Justus von Liebig, Dr. Robert Mayer –for many the true discoverer–, and also the polymath Helmholtz. The latter presented in 1847 the most technical and complete version of the principle, which included many of the previous results on the subject.[10] Helmholtz was fully aware that rather than an empirical discovery, his work was a kind of a general research program. He presented his report in the *Physikalische Gesellschaft* in Berlin, founded only two years earlier. The attempt to distance from the *Naturphilosophie* and its metaphysical speculations in the field of natural sciences had led to six enthusiast scientists (only three of them physicists) to found a new institution.[11] The old guard of Berlin did not support the proposal by Helmholtz. Johann Poggendorff refused to publish Helmholtz's memoir in the *Annalen*.

But the idea settled down in the next few years, especially after Clausius joined the Society in 1852. Three years later, for example, the academicians launched a competition to get an accurate measure of the mechanical equivalent of heat.

In Britain, also a young institution saw the emergence of the principle of conservation: the *British Association for the Advancement of Science*. It was founded in 1831 with the certain intention of regenerating British science and "to incorporate a love of knowledge with the very frame of the country."[12] There, James Joule presented his first measures of the mechanical equivalent of heat in 1843, in Cork. There, William Thomson knew about Joule's results in 1847, in Oxford. And there, the use of the word "energy" was definitively coined. Unlike the Germans, the British were deeply involved with the industrial world. For instance, William Rankine, one of the main architects of thermodynamics, was for many years Regius Professor of civil engineering and mechanics at the University of Glasgow, and had also worked for shipbuilding companies.

Very soon, interactions among the two traditions took place. Some of these interactions were in the form of priority disputes and restatements. It is interesting to note the fast conjugation of traditions that were based on seemingly such disparate interests: in the islands, increasing profitability of machines; in Germany, the desire to finish with the mystique of the vital forces.

Thermodynamics and the Second Principle

The consolidation of the conservation principle is closely related to the emergence of thermodynamics. In mechanics, since Helmholtz memory, the principle had been demonstrated (for conservative systems, of course). After that, what was needed was the quantitative relation of ancient *vis viva* to other forces of nature. There is little doubt that the mechanical equivalent of heat was one of the mostly quoted evidences in this sense. To be sure, the growing concern in heat-machines

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[8] KUHN (1959), ELKANA (1974), MYERS (1989), RABINBACH (1990), SMITH (1998), HARMAN (1987), BRUSH (1978), CLARKE (2001).

[9] POHL-VALERO (2006).

[10] HELMHOLTZ (1847).

[11] SCHREIER (1998).

[12] Quoted in CANNON (1978), 169. It is an excerpt of the speech given by the first president of the Association, Earl Fitzwilliam, at the third meeting, held in Cambridge in 1833.

research was probably the main reason for this interest, which can already be found in the foundational memory by Sadi Carnot in 1824.[13]

Thermodynamics –another term coined by the British– cannot be considered as completely formulated before the enunciation of its second principle. Although anticipated to some extent by Carnot, it was only clearly stated in the works by Thomson and Rankine in the fifties, and through the introduction of the concept of entropy by Clausius in 1864. Clausius justified the choice of this term for its resonance with "energy." Unlike the latter, 'entropy' was a neologism.

The successful spread of the principle of conservation and of the term energy itself was often accompanied by the idea of irreversibility and degradation which the second principle introduced. That law, smaller in scope than the first but much more attractive for transpositions or cosmological applications, contributed a great deal to shape the principle of conservation.

The allegory of the "Heat Death" of the universe was recognized in the fifties by Clausius, Helmholtz and Thomson. For instance, in the famous reversibility objection raised by Joseph Loschmidt against the probabilistic interpretation of the second law by Ludwig Boltzmann –objection, by the way, previously formulated by Thomson–, the Viennese physicist clearly showed the «terrifying» character that the second principle had already taken in 1876:[14]

> [My intention is to]…destroy the terroristic nimbus of the second law, which has made it appear to be an annihilating principle for all living beings of the universe; and at the same time open up the comforting prospect that mankind is not dependent on mineral coal or the sun for transforming heat into work, but rather may have available forever an inexhaustible supply of transformable heat.

In the context of spiritual affairs, the idea of degradation of energy was clearly expressed by reverend Thomas Hill, ex-president of Harvard College. In his article «On the Correlation of Forces» he wrote that:

> A ditcher or hod-carrier may use as much muscular power in the twenty-four hours as the most graceful and accomplished acrobat; and some dull student, stumbling for weeks in the vain attempt to cross the 'pons asinorum,' will use as much brain power, measured in the amount of phosphorous assimilated from his food and eliminated again from his body as William Rowan Hamilton used in creating the new science of kinematics, or the new calculus of Quaternions.

In this excerpt revered Hill points the place for the second law of thermodynamics: even though energy is conserved, one has to make good use of it.

We have found a review of this paper –whose exact reference we don't know– literally stuck inside the cover of a copy of the anthology on *The Correlation and Conservation of Forces*, by Edward Youmans.[15] We assume that who ever posted this review had the intention to add a forceful support to Youmans' purpose, openly anti-materialist. The anthology, edited in New York in 1868, contains expositions by different authors, and was intended for the spreading of the new ideas in the States.

His specific audience was American science teachers and, indirectly, textbooks editors. In this anthology we can find references not to entropy itself, but to dissipation and irreversibility. For instance, in the conference by Helmholtz, held in 1854 in Könisberg before a non-scientific audience, we already find the fiction of eternal death. William Grove did not dwell on this aspect, but did mention dissipation and irreversibility as a very probable fact (this volume contains the fourth edition of Grove's book, from 1862).

Popularization of Thermodynamics

The concept of energy joined the journalistic and artistic vocabulary during the seventies. According to the physicist Balfour Stewart, when intellectuals started writing books in which the two laws of thermodynamics were mentioned, energy was already a perfectly intelligible word in British society. He stated:

> By energy, we mean the power which a man possesses of overcoming obstacles; and the amount of his energy is measured by the amount of obstacles which he can overcome, by the amount of work which he can do.[16]

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[13] CARNOT (1824).

[14] Quoted in BRUSH (1978), 66. It belongs to LOSCHMIDT (1876), 135.

[15] YOUMANS (1868).

[16] Quoted in MYERS (1989), 323. It belongs to a paper by Balfour Stewart and J. Norman Lockyer of 1868 entitled "The Place of Life in a Universe of Energy," *Macmillan's* 20, 319.

Cosmovisions inspired by thermodynamics conceived nature as a machine capable of producing mechanical work or –as Helmholtz would call it– workforce. The industrial machine was adopted as a model of the universe and nature became the source of endless power for society.

Popular scientific literature took profit of this image. According to Myers, Stewart and Tait thought that laws of physics and those of capitalism were entangled as soon as 1875.[17] Quoting Wegener:

> Psychiatrists such as Beard spoke in 1881 of mental exhaustion as the "bankruptucy" of nervous energy. Similarly, Krafft-Ebing used, in 1885, terms like "Nervenkapital," "Nervenarbeit" and "Arbeitskapital" interchangeably... But, by far, the most common analogy was comparing exchange of money in the society with an exchange of energy in the physical world.[18]

In fact, in the above-mentioned conference by Helmholtz in 1854, he already had used this analogy.

The term 'energy' was immediately endowed with metaphysical considerations. We think that the generalization of the concept of entropy precipitated its insertion in the social imaginary of the period. Balfour Stewart published one of the first popular books entitled *The Conservation of Energy*, in 1873, in which he already discussed the second principle. Two years later, along with Peter Guthrie Tait, he published *The Unseen Universe*, where the authors distinguished between the imperishable, invisible and eternal world, and the visible, perishable, and mortal.[19] In Germany, materialists also echoed the second principle. Nietzsche himself quoted William Thomson and the heat death in his writings of the eighties.[20] He had known about this result through the book *Die Kraft*, from the German physiologist Johannes Vogt.[21] In a newspaper article published in one May the first, the Spanish Nobel laureate José Echegaray advocated for a peculiar application of Carnot's principle: only social inequalities allow work performance.[22] It was not the only time someone in Spain put heat death at the same level of egalitarianism. A similar argument was defended at the *Ateneo de Madrid* by Laureano Calderón in 1891.[23] He was claiming to avoid the state interference in the affairs of citizens.

Science Fiction, at least as futuristic speculation, reached its peak, if not its birth, in this same period. In 1871, Edward Bulwer-Lytton published an odd novel entitled *The Coming Race*, which set the action in an underground world, inverting the Victorian Platonism. Allegoric visions of thermal death were also reflected by Flammarion's scientific positivism in *La fin du monde* in 1893: the figure of Death reigning over a planet without life or the spectral representation of a human race perishing of frozen cold are some of the allegories that came from Thomson and Clausius ideas.

However, paradoxically, popularization of energy did not entail popularization of physics; on the contrary, physics became less accessible at the end of the century, and its authority increased by lots. The claim by Planck with which we began our communication was a response to the great variety of meanings which were attributed to energy in popular science, and different scientific disciplines. Planck's attempt to separate Physics from amateur speculations is the best evidence of the presence of the conservation principle in the popular and artistic imagery of the third quarter of the nineteenth century.

Concluding Remarks

Many of the current responses to the periodic energy crisis are the distant echoes of the foundational nature of energy: the second law and the idea of degradation and profitability have accompanied energy almost since its first appearances. The catastrophic implications of energetic collapses are not new. 'Energy' is nowadays as ubiquitous as still indefinable. Although we cannot properly say what energy is, we do know how much it costs, and we think that we know what would happen if we don't manage it properly.

References

BENSAUDE-VINCENT, B. (2001): "A genealogy of the increasing gap between science and the public." *Public Understanding of Science* 10, 99-113.

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[17] MYERS (1989), 327. He is referring to STEWART & TAIT (1881).

[18] WEGENER (2010), 149.

[19] STEWART & TAIT (1875).

[20] Quoted in BRUSH (1978), 72-75.

[21] Quoted in *ibid*. He is referring to J. Vogt, *Die Kraft. Eine real-monistische Weltanschauung*, 1878, Haupt & Tischler, Leipzig.

[22] ECHEGARAY (1905), 493-500. The title of the newspaper article is: "Para las Ciencias Físicas."

[23] Quoted in POHL-VALERO (2007), 10.

BRUSH, S.G. (1978): *The Temperature of History. Phases of Science and Culture in the Nineteenth Century*. Burt-Franklin, New York.

BUCHWALD, J.Z. & HONG, S. (2003): "Physics." In D. CAHAN, *From Natural Philosophy to Science: Writing the History of the Nineteenth–Century Science*, 2003, University of Chicago Press, Chicago, 163-195.

CANNON, E.F. (1978): *Science in Culture. The Early Victorian Period.* Dawson and Science History Publications, New York.

CARNOT, S. (1824): *Réflexions sur la puissance motrice du feu et sur les machines propres à développer cette puissance*. Bachelier, Paris.

CLARKE, B. (2001): *Energy forms : Allegory and Science in the Era of Classical Thermodynamics*. University of Michigan Press, Ann Arbor.

ECHEGARAY, J. (1905): *Ciencia Popular*. Imprenta de J.A. García, Madrid.

ELKANA, Y. (1974): *The Discovery of the Conservation of Energy*. Harvard University Press, Cambridge (Massachusetts).

HARMAN, P.M. (1987): *Energy, force and matter: the conceptual development of nineteenth-century physics*. Cambridge University Press, Cambridge.

HELMHOLTZ, H. (1847): *Über die Erhaltung der Kraft. Eine physikalische Abhandlung, vorgetragen in der Sitzung der physikalischen Gesellschaft zu Berlin am 23sten Juli 1847*. G. Reimer, Berlin.

KUHN, T. S. (1959): "The conservation of energy as an example of simultaneous discovery." In: Marshall CLAGETT (ed.), *Critical Problems in the History of Science*, 1959, University of Wisconsin Press, Madison, 321-356.

LOSCHMIDT, J. (1876): "Über den Zustand des Wärmegleichgewichtes eines Systems von Körpern mit Rücksicht auf die Schwerkraft. I." *Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften in Wien. Mathematisch-Wissenschaftliche Klasse* 73, 128-142.

MYERS, G. (1989). "Nineteenth-Century popularization of thermodynamics and the rethoric of social prophecy." In P. BRATLLINGER (ed.), *Energy & Entropy. Science and Culture in Victorian Britain*, 1989, Indiana University Press, Indianapolis, 307-338.

PLANCK, M. (1921): *Das Prinzip der Erhaltung der Energie*. Teubner, Leipzig. 4th edition (1st from 1887).

POHL-VALERO, S. (2006): "La termodinámica como elemento legitimador de la física teórica y aplicada en la España de la segunda mitad del siglo XIX." *Quaderns d'Història de l'Enginyeria* VII, 73-114.

POHL-VALERO, S. (2007): "La termodinámica, su historia y sus implicaciones sociales. Una revisión historiográfica." *Grafía* 5, 58-69.

RABINBACH, A. (1990): *The Human Motor. Energy, Fatigue and the Origins of Modernity*. University of California Press, Berkeley.

SCHREIER, W. (1998): "Das Energieprinzip als Thema in der Physikalischen Gesellschaft." In *150. Jahrestag des Vorträges „Über die Erhaltung der Kraft" von Hermann Helmholtz*. Preprint 130, Max-Planck-Institut für Wissenschaftsgesichte, 35-39.

SMITH, C. (1998): *The Science of Energy. A Cultural History of Energy Physics in Victorian Britain*. The Athlone Press, London.

STEWART, B. & TAIT, P.G. (1881): *The Unseen Universe; or Physical Speculations on a Future State*. London, MacMillan. 5th edition (1st 1875).

TOPHAM, J.R. (2009): "Rethinking the History of Science Popularization/Popular Science." In F. PAPANELOPOULOU, A. NIETO-GALÁN and E. PERDIGUERO (eds.), *Popularizing Science and Thechnology in the European Periphery*, 2009, Aldershot, Ashgate, 1-20.

WEGENER, D. (2010): "De-anthropomorphizing energy and energy conservation: The case of Max Planck and Ernst Mach." *Studies in History and Philosophy of Modern Physics* 41, 146-159.

YOUMANS, E. (1868): *The Correlation and Conservation of Forces: a Series of Expositions*. Appleton, New York.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 1071.

THE NEW NON-CLASSICAL

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Abstract

The paper addresses a possible shift in the classification of science into classical and non-classical. It has been widely accepted that Galilean-Newtonian mechanics is the basis and best example of classical science, while quantum mechanics and the theories of relativity play the same role at the non-classical side. Some methodologists (most notably Prigoginians) claim that in addition to the mentioned distinction one should speak about post-non-classical science. The latter case of Ilya Prigogine's conception (theory?) of self-organization has been taken as the model case. The third member in the classifying sequence, however, has failed to gain general recognition. It is obviously possible to drop the post-non-classical story as an overestimation of the influence of some relatively marginal approach to the scientific method. However, another solution is possible as well. As a matter of fact, one can view both the theories of relativity and quantum theory as classical and move the dividing line between two types of science closer to the present day in the history of science. Quantum mechanics is quite different from the classical one in several significant ways, i.e. indeterminism and the role of the observer, to name just some of the most influential ones. It may look as if self-organizationists did not really do anything else but put a stronger stress on some aspects already put forward by the big wigs of the non-classical approach. However, it is the issue of irreversibility that may well turn the tables here. The Schrödinger equation is reversible. For Einstein, time was just an illusion. It is Prigogine, who has been consistently claiming that irreversibility is an objective fact inherent in nature and not something that subjectively seems to us humans due to the limited character of our knowledge. It appears that we cannot get rid of irreversibility in principle just like the Eleatics were not able to arrest the Heraclitean change.

Introduction. Why Was Classical Science Classical?

It is normally held that Modern European Science started with Galileo. The reasons for this claim to hold are sometimes considered self-evident. Galileo was an experimenting scientist. Of course, the role of the experiment is very important for that matter. However, just experimentation could hardly bring anything new into the intellectual activities of the time. Alchemists had been furiously experimenting for quite a while. Still, the methodologists and philosophers of science of today do not tend to call them the initiators of the Modern scientific method. The experiment is part and parcel of the scientific activity of modernity. But not any kind of experiment can do the job. The experiment has to be systematic. It has to be based on a hypothesis which is scientific, i.e. which can be verified or falsified. The alchemists normally did not care for the latter task. This gives us evidence that the priority of Galileo is due to the ability to present hypothesis that were testable, that could be verified or falsified by experiment. This does not mean that he succeeded in his task of verification. But the pace was set.

We still didn't answer the main question here. Namely, what is classical about Galileo's approach to the experiment? Systematic approach to the issue based on the scientific hypothesis is an important part of the matter. The core question, however, is reversibility. Galileo stands for the requirement that each and every scientific result has to be achieved repeatedly. Reversibility becomes the core of the classical scientific method. Time is thrown overboard. It has been declared nothing more than an illusion, an example of human ignorance.

The position of the Eleatics prevailed over the Heraclitean world view for centuries. Common sense and sense perception lost the battle to pure reason just like Achilles was never able to catch up with the slowly moving tortoise in the famous paradox of Zeno, the follower of Parmenides for whom nothing existed except the one and only unchangeable whole. Still, this was the beginning of theoretical thinking. By all means a great achievement of the human intellect that enabled us to progress towards a high-tech civilization. Still, the approach has to be changed in order to avoid a dead end as we'll see below.

Why is Quantum Mechanics Still Classical?

Classical mechanics, started by Galileo and elaborated by Newton, was classical by definition. It still serves as the model case for the ideal of being scientific. Quantum theory seems to be quite different and really is such. It is normally taken to be the model case of non-classical science.

We need to answer two symmetrical questions here. Namely, what is new and what is old in quantum theory? We are back to the experiment. Now we need to pay special attention to the observer. In the very classical case the observer is there but she is not part of the experimental situation. She is rather in a God-like position arranging the situation from above and observing it repeatedly. As a matter of fact, repetition of an experiment in the strict sense of the word is an Achilles-tortoise type illusion. But this does not prevent classical scientists to apply the concept and make it a centre-piece of the method from the application point of view.

In quantum mechanics, as we know, the observer necessarily intervenes into the system disturbing it, actually breaking it, making measurement impossible. In a way, this makes the process under study irreversible. Strangely enough, the disastrous intervention of the observer into the quantum world still means a step further in the development of science itself and even its methodology.

Another novel moment is bringing in probabilities into decision making concerning experimental results. This move enables fluctuations to come in. But the need to reverse, to repeat is still there. Statistics won't turn it around. After all, the Schrödinger equation is reversible. The breakthrough was never achieved.

Ludwig Boltzmann and the Arrow of Time

The conception of the arrow of time comes into the methodology of science through the developments concerning entropy associated with the second law of thermodynamics. The role of Rudolf Clausius cannot be overestimated in this respect.

After Clausius came Ludwig Boltzmann, the Charles Darwin of physics. It was Boltzmann who identified the time paradox in the second half of the nineteenth century. For Boltzmann, the whole nineteenth century was the century of Charles Darwin. Thus, he tried to formulate an evolutionary approach to physics as well. In doing so, he came face to face with the problem of the objectivity of knowledge. The laws of Newtonian physics had represented the ideal of objective knowledge for a long time. Any attempt to confer a fundamental meaning on the arrow of time was resisted as a serious threat to this ideal. In Boltzmann's time, Newton's laws were still considered final in their domain of application.

Boltzmann was well aware that nothing analogous to irreversibility exists in classical dynamics. He thought that he had successfully proved the determination of the arrow of time by the evolution of the dynamical systems from less probable conditions to more probable ones. He concluded that irreversibility can be derived only from assumptions about the initial conditions in the early stages of our universe. Boltzmann was convinced that in order to understand nature we have to include evolutionary features and that irreversibility, as defined by the second law of thermodynamics, was a decisive step in this direction. But he was also heir to the grand tradition of dynamics and realized that it stood in the way of his attempt to give a microscopic meaning to the arrow of time. Boltzmann's probability-based interpretation makes the macroscopic character of our observations responsible for the irreversibility we observe. If we could follow the individual motion of the molecules, we would see a time-reversible system in which each individual molecule follows the laws of Newtonian physics. According to this interpretation, irreversibility is not a basic law of nature but merely a consequence of the approximate, macroscopic character of our observations.

The objections of Ernst Zermelo and Henri Poincaré made Boltzmann change his position. As the result, Boltzmann gave up the idea of the objectivity of the arrow of time, reducing it to the level of our subjective conceptions, which we as human beings impose on reality. As a result, the notion of the arrow of time was relegated to the realm of phenomenology.

"We, as imperfect human observers, are responsible for the difference between past and future through the approximations we introduce in our description of nature" (Prigogine 1997: 2). Thus, irreversibility would correspond only to an appearance that is devoid of any fundamental significance.

According to Ilya Prigogine, the failure of Boltzmann was caused not only by his inadequate response to criticism but also by his incorrect interpretation of the second law of thermodynamics. Boltzmann considered the second law a practical principle expressing only practical improbability of one process or another. Prigogine himself, however, postulates the second law of thermodynamics as a fundamental theoretical fact, as a principle of selection leading to the breaking of time symmetry (Näpinen and Müürsepp 2002: 467).

It is Boltzmann's big tragedy that he is often credited with having shown that irreversibility is only an illusion. True, he really made the logical error relying on trajectories. Trajectories are time-reversible, however numerous.

Ilya Prigogine on the Arrow of Time and Irreversibility

Ilya Prigogine has called the denial of the *arrow of time* the main problem addressed in his book *The End of Certainty* (Prigogine 1997). According to Prigogine, it is the so-called *physics of nonequilibrium processes* that describes the effects of unidirectional time and gives fresh meaning to the term 'irreversibility'. Prigogine holds that irreversibility leads to a host of novel phenomena, such as vortex formation, chemical oscillations, and laser light, all illustrating the essential constructive role of the arrow of time. From this, it is clear already that irreversibility serves as the physical basis of the arrow of time for Prigogine. He illustrates his position with a metaphor: matter at equilibrium, with no arrow of time, is "blind", but with the arrow of time, it begins to "see". Prigogine adds that we are actually the children of the arrow of time, of evolution, not its progenitors (Prigogine 1997: 3).

The other crucial development in revising the concept of time was the formulation of the physics of unstable systems. Classical science emphasized order and stability. Now we see fluctuations, instability, multiple choices and limited predictability at all levels of observations. When appropriate initial conditions are given, we can predict with certainty the future or "retrodict" the past. Once instability is included, this is no longer the case and the meaning of the laws of nature changes radically, for they now express possibilities or probabilities, not certainties.

It is interesting to note that Prigogine traces the modern history of the arrow of time back to the eighteenth century, when Jakob Bernoulli presented his pioneering work in games of chance. Sensitivity to initial condition is really observable in Bernoulli's work. Two numbers differing only very slightly initially will differ by 0.5 after 40 steps. Thus, the so-called Bernoulli map introduces an arrow of time that can only point in one direction. Here, the time symmetry is broken at the level of the equation of motion, which is thus not invertible. This is in contrast to the dynamical systems described by Newton, whose equations of motion are invariant with respect to time inversion. There is no indication, however, that Jakob Bernoulli himself paid any attention to irreversibility as a concept in the context of his studies. In this sense, it is still Boltzmann, who has the priority. In any case, the arrow of time already exists at the level of equations of motion. However, the baker map provides a more general description of the emergence of irreversibility in invertible dynamical systems.

In a celebrated statement, Max Born asserted that "irreversibility is the effect of the introduction of ignorance into the basic laws of physics" (Born 1960). Prigogine's firm position is, however, that the laws of physics, as formulated in the traditional way, describe an idealized, stable world that is quite different from the unstable, evolving world in which we live. The main reason to discard the banalization of irreversibility is that we can no longer associate the arrow of time only with an increase in disorder. Recent developments in non-equilibrium physics and chemistry unambiguously show that the arrow of time is a source of order.

The constructive role of irreversibility is clearly seen in far-from-equilibrium situations where non-equilibrium leads to new forms of coherence. Prigogine believes that it is precisely through irreversible processes associated with the arrow of time that nature achieves her most delicate and complex structures (Prigogine 1997: 26). Therefore, we can see that Prigogine associates irreversibility with the arrow of time. There is no indication, however, that we can obtain the arrow of time only in the irreversible world of non-equilibrium physics. Moreover, Prigogine's formulations raise the question, whether he imagines that there can be irreversibility not associated with the arrow of time and if so, in what sense?

This problem concerns an important aspect of the time paradox, "interface between mind and matter". If the arrow of time existed only because our human consciousness interfered with a world otherwise ruled by time-symmetrical laws, the very acquisition of knowledge would become paradoxical, since any measurement already implies an irreversible process. If we wish to learn anything at all about a time-reversible object, we cannot avoid the irreversible process involved in measurement, whether at the level of an apparatus or of our own sensory mechanisms. Thus, in classical physics, when we ask how we can understand "observation" in terms of fundamental time-reversible laws, we get "nonsense". In classical physics we can neglect this "nonsense" due to the great success of classical dynamics. In quantum theory, however, the situation is quite different. Here we need to include measurement in our fundamental description of nature. It seems that we have an irreducible duality: the time-reversible Schrödinger equation and the collapse of the wave function. By going beyond a reductionist description we can give a realistic interpretation of quantum theory. This is possible by incorporating Poincaré resonances into a statistical description and deriving diffusive terms that lie outside the range of quantum mechanics in terms of wave functions. The measurement device has to present the broken time symmetry. In this sense, there is a privileged direction of time, as there is a privileged direction of time in our perception of nature. This *arrow of time* is the necessary condition of our communication with the physical world. It is the basis of our communication with our fellow human beings.

Stephen Hawking takes an analogous position, answering the question, why do we observe that the thermodynamic and cosmological arrows point in the same direction. "Conditions in the contracting phase would not be suitable for the existence of intelligent beings who could ask the question: Why is disorder increasing in the same direction of time as that in which the universe is expanding?" (Hawking 1989: 159-160). Still, in the end, for Hawking time remained just an accident of space, supported by the *anthropic principle*. However, there is an important position that Hawking holds. He emphasizes that in addition to the thermodynamic and cosmological arrows of time there is the psychological arrow. Stephen Hawking argues that the psychological arrow of time is determined by the thermodynamic arrow (Hawking 1989: 153). His position is that psychological arrow is actually essentially the same as the thermodynamic arrow. They always point in the same direction (Hawking 1989: 160). This idea is an important step forward in the understanding of how human beings can make a better sense of the universe. "The progress of the human race in understanding the universe has established a small corner of order in an increasingly disordered universe (Hawking 1989: 161).

Ilya Prigogine takes an even broader look. According to him, the arrow of time plays an essential role in the formation of structures in both the physical sciences and biology. However, there is still a gap between the most complex structures we can produce in non-equilibrium situations in chemistry and the complexity we find in biology.

Irreversible processes describe fundamental features of nature leading to non-equilibrium dissipative structures. Such processes would not be possible in a world ruled by the time-reversible laws of classical and quantum mechanics. Dissipative structures require an arrow of time. The existence of the arrow of time is not a matter of convenience. It is a fact imposed by observation.

As already mentioned above, Prigogine postulated the second law of thermodynamics as a fundamental *theoretical* fact, as a principle of selection leading to the breaking of time symmetry. Just for this reason he creates the new physical and mathematical concepts. He treats the breaking of time symmetry as an *inner* quality. "Only the treatment of time as an *operator* by Prigogine can make it possible to explain the inner breaking of time symmetry simultaneously in mechanics, quantum physics and relativity theories" (Näpinen and Müürsepp 2002: 467). The word "inner" means that the breaking of time symmetry is not forced by new interactions with the outside world. The *time operator* is closely connected to the *microscopic entropy operator*. Through non-canonical (non-unitary –in quantum mechanics) transformations, the *evolution operator* with the inner time symmetry breaking is reached. This chain of novelties leads to the physical-mathematical description with unidirectional –from past through present into future– time. Many scientists still believe that some events are evolving in one direction only because the evolution in the opposite direction would have a very small probability but is still possible. The conviction of Prigogine is different. Only because some states are strictly forbidden and they cannot be discovered in nature and artificially prepared, the probabilistic character can be ascribed only to the states that are allowed. According to Prigogine's idea, the inner breaking of time symmetry leads to physical evolution that is describable by a representative non-unitary *semigroup*. The latter expresses the second law of thermodynamics.

It is important to emphasize that Prigogine does not connect this law with the experimental situation and thermal machines. The scope of this law is much broader. In Prigogine's interpretation, this law claims, in its most essential sense, that there is a quantity in nature which, during all the changes occurring in nature, changes only in one and the same direction. If there are quantities in the world, which change only in one direction, there are also states, which change only in one direction.

It is important to understand that trajectories and even wave functions are not enough to account for irreversibility. We need to apply the so-called *Poincaré resonances* (coupling of degrees of freedom that lead to divergent expression due to small denominators if there is resonance between them). Accepting the considering of *irreversibility* as the basic element of description of the physical world, referring to the position of the mathematical description of large physical systems that lies in the infinity of a number of degrees of freedom (Näpinen and Müürsepp 2002: 477).

Obviously, Prigogine is interested only in the description of reality in the most comprehensible way, invoking the best of the scientific method. Thus, he treats irreversibility and the arrow of time in close contact or even as synonyms. Prigogine may not have been aware of the logical possibility of separating the arrow of time from irreversibility. It is more probable, however, that this possibility did not have any significance for him. The arrow of time is just a metaphoric expression for irreversibility, which he understands in a broad physical sense.

Conclusion

The analysis provided above yields evidence that the positioning of the demarcation line between classical and non-classical science can be and probably has to be disputed. Obviously, quantum theory cannot be taken as just an extension to the classical Galilean-Newtonian approach. However, the breakthrough into principally new understanding of nature, new dialogue with it for humans has not been achieved by the quantum theorists. They lacked the necessary dose of innovative thinking. The genius of Stephen Hawking, however, has been focused on mathematical models of the universe and only occasionally made excursions to the philosophical plane with no intention of making his presence really felt in this field. Still, the breakthrough was achieved in the second half of the twentieth century by Ilya Prigogine and his followers.

There is another idea present to set the terminology of the methodological changes by calling the latter post-non-classical approach to science. However, such solution would distort the picture by displaying the great breakthrough of Prigogine in a negligibly insignificant way. The approach deserved to be called the new non-classical one.

References

BORN, M. (1960) *The Classical Mechanics of Atoms*. New York: Ungar

HAWKING, S.W. (1989) *A Brief History of Time.* New York, Toronto, London, Sydney, Auckland: Bantam Books.

NÄPINEN, L., P. MÜÜRSEPP (2002) The Concept of Chaos in Contemporary Science: On Jean Bricmont's Critique of Ilya Prigogine's Ideas. *Foundations of Science*. Volume 7. Kluwer Academic Publishers, pp. 465-479.

PRIGOGINE, I. (1997) *The End of Certainty*. New York, London, Toronto, Sydney, Singapore: The Free Press.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 1076.

CIRCULATION OF IDEAS AND FORMATION OF SLOVAK PHYSICS

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Abstract

The formation of Slovak physics is a pregnant example of the importance of idea circulation. Before 1918, no university existed in the territory of modern Slovakia. After the birth of Czechoslovakia, Comenius University was established in Bratislava, but without a faculty of science. So whoever was interested in the study of science was forced to move to Prague, Brno or foreign cities. Three young men were studying at Charles University in Prague in 1925-29 – Dionýz Ilkovič and Ján Fischer from Slovakia, and Vilém Kunzl from Czech city of Plzeň. The first one was appointed (1940) professor of technical physics in Bratislava's Technical University, and after a faculty of science was established in Bratislava he was also appointed professor of physics there. He founded the undergraduate study for future physics teachers and future physicists, and started physical research. First, his followers moved to Košice at the beginning of the 1950s to found a second Slovak centre of physics.
Owing to Ilkovič's effort, V. Kunzl was appointed professor of physics at Comenius University in 1947. In 1948-52 Kunzl founded four physics branches which are cultivated there up to now. So he is considered the founder of experimental physics at CU. Besides that, owing to his effort Ján Fischer came to CU. In 1928-31 Fischer finished his PhD study at ETH Zürich under supervisor prof. G. Wentzel. J. Fischer completed two papers here which became foundation stones of quantum theory of interaction between radiation and matter. He is deservedly considered to be the founder of Slovak theoretical physics.
No doubt, D. Ilkovič, J. Fischer, and V. Kunzl became founders of Slovak physics owing to their high-quality training at Charles University in Prague. Moreover, creating a new branch of science and forming undergraduate training they used fairly all experience, methods, and ideas which they met in Prague. In the 1950s Czech theoreticians promoted to build up a Slovak school in theory of elementary particles.

The formation of Slovak physics is a pregnant example of the importance of idea circulation. Before 1918 neither acollege nor a university existed in the territory of modern Slovakia. Hungarian Elizabeth's university, established in the year 1914, did not start lectures before the end of World War I. In 1919 Comenius University was established in Bratislava, originally as a traditional university with four faculties, but for three decades there were only three faculties: during the first school year 1919/20, the Faculty of Medicine functioned. The second and the third ones –the Faculty of Philosophy and the Faculty of Law– started to prepare university-educated specialists two years later. However, the Faculty of Natural Sciences was not established at all during the pre-war Czechoslovac republic.

So anybody who was interested in studying science, including mathematics and physics, was forced to move out to Prague, Brno or any foreign cities. However, in November 1939 all Czech universities were closed because of students' disorders connected with German occupation. After that, all Slovak students had to go back to Slovakia as Bohemia had turned into a part of Germany as the Bohemia and Moravia Protectorate, and Slovakia became an autonomous country under the auspices of the Third Reich. In the school year of 1939/40, as there was no Faculty of Sciences at Comenius University, future mathematicians and physicists or future teachers of these subjects had to enter the Faculty of Philosophy. As usual, they enrolled to some subjects closer to their interests. Fortunately, at that time there was lecturing on physics at the Institute of technical physics of the Slovak Technical University in Bratislava.

However, in the winter semester of 1939/40 there was a theoretical opportunity only because prof. Josef Sahánek, the first head of the Institute of Technical Physics of the Slovak Technical University, had left Bratislava as he was appointed professor of technical physics at the Technical University in Brno. Things became more favorable when the new head of the Institute of Technical Physics was appointed. It was Dionýz Ilkovič. He took the lead of the Institute at the beginning of February 1940. It is the starting point for the forming of Slovak physics. Why? And how had circulation of ideas taken place in this case? To answer these two questions we have to say a few words about his study.

Dionýz Ilkovič had studied at the Faculty of Sciences of Charles University in Prague in 1926-30. From the point of view of our subject, several factors had been very important:

Young Dionýz completed his education in Prague on chemistry, teaching of chemistry, physics and mathematics, at an actuary. Moreover, he was well educated in philosophy of science too. They were very strong fundamentals not only for scientific research, but also to become later the founder of Slovak physics. For Ilkovič's future mission, it was important that he very early met prof. Jaroslav Heyrovský, discoverer of polarography and Nobel Prize winner in chemistry in 1959. Dionýz had joint Heyrovský's group as a young student. He had listened to Heyrovský's lectures. He had worked in practical classes and individual research headed by Heyrovský. Additionally, Ilkovič was deeply influenced by prominent Czech physicist prof. Václav Dolejšek. One can say: in what way was he influenced?

Firstly, chemist Heyrovský and physicist Dolejšek had started in the mid-1920s to organize common seminars, the so-called Chemical-physical dialogues. Current problems of chemistry and physics were discussed there. Dionýz began to attend these dialogues as a third-year student. It was a very important and useful school for him. He became acquainted with the current situation in both physics and chemistry. He also had the opportunity to discuss many problems solved by Heyrovský, Dolejšek and other chemists and physicists at Charles University. Secondly, working at practical classes with Dolejšek Dionýz, he was intensively trained in methods and skills of physical experiments.

Thirdly, Dionyz met two young men at Charles University in Prague in 1925-29: Ján Fischer from Slovakia and Vilém Kunzl from the Czech city of Plzeň. They attended common lectures, seminars and practical classes, and step by step they became friends. During the 1930s Dionýz and Vilém were working at Charles University –Dionýz as an outside assistant of prof. Heyrovský, Vilém as an assistant of prof. Dolejšek. So they knew each other very well.

The 1930s period was very fruitful for Dionýz Ilkovič. He had published about 10 papers in first-rate foreign journals. The most important result was the so-called Ilkovič equation –a basic formula in polarography. Due to this mathematical expression and several other papers, Ilkovič in collaboration with prof. Heyrovský built the theoretical foundations of polarography. In the school year 1937/38 Dionýz Ilkovič made use of Denis' scholarship offered by the French government to gifted Czechoslovak young researchers. Ilkovič was working at the Institute for physical chemistry of Paris University, headed by prof. Audubert. The study of ultraviolet radiation by photoelectric counters was the main theme of Ilkovič's research in Paris. His experiences and findings obtained in the laboratory of prof. Audubert were later used by Dionýz Ilkovič in Bratislava. In the late 1940s he had guided PhD student Štefan Luscoň, who successfully defended his PhD theses on photoelectric cells.

Back again in Prague, Dionýz Ilkovič applied for the veniae docendi. Unfortunately, the process of habilitation was interrupted as a consequence of the closing of Czech universities. This process ended only in 1946.

As mentioned, in the winter semester of 1939/40 prof. Josef Sahánek, head of the Institute of Technical Physics, had left Bratislava, so university authorities called Dionýz Ilkovič for professorship. He accepted such an attractive offer and in February 1940 the president of the Slovak republic appointed him to the post of professor of technical physics and head of the Institute of Technical Physics of the Slovak Technical University. Working there he founded undergraduate study for future physics teachers and future physicists in Slovakia, and started physical research. Dionýz Ilkovič lectured on both theoretical and experimental physics.

From 1939 Slovak young people had no opportunity to study natural sciences or to teach these subjects. So the Faculty of Natural Sciences was established at the Slovak University at October 1940. Dionýz Ilkovič was appointed professor of physics at FNS. Apart from physics, he lectured on chemistry and physical chemistry too. During the 1940/41 school year Ilkovič's sole assistant left Slovak Technical University. Hence, prof. Ilkovič decided to involve several young students as his assistants and mates, first of all Imrich Staríček. That way, he was training, preparing and educating a new generation of Slovak academicians. I would like to say that several of Ilkovič's assistants became later professors as well. At the beginning of the 1950s two of them moved to Košice in eastern Slovakia to constitute the second physical centre in Slovakia.

The very tool for physics and chemistry education at the Faculty of Natural Sciences was Chemical-physical dialogues. In Bratislava they were organized by physicist prof. Ilkovič and chemist prof. Valentín. It was an analogy of the Dialogues organized by prof. Heyrovský and prof. Dolejšek in Prague before World War I. Although the Dialogues were special seminars for researchers, they were included into the students' curriculum, so students had the opportunity to take part in them. As a curiosity we can say that in March 1943 Werner Heisenberg had presented a lecture on atomic physics in the frame of these Dialogues.

In August 1944 the Institute of Physics was established at the Faculty of Natural Sciences. Dionýz Ilkovič had been appointed as its head, so he was heading two institutes simultaneously: the Institute of Technical Physics at Slovak Technical University and the Institute of Physics at Slovak University; Imrich Staríček, his first student auxiliary, was appointed assistant of prof. Ilkovič. In March 1945 Staríček defended his PhD thesis on theoretical physics, and he started to lecture modern subjects of theoretical physics, especially quantum mechanics. From December 1945 he spent one year as visiting researcher at the Radium institute of prof. Iréne Joliot-Curie in Paris.

At that time Ilkovič was very tired of teaching physics and heading two institutes at both Slovak Technical University and Slovak University. He decided to focus his attention only to activities at STU, so he asked Slovak University authorities to invite a new professor of physics for SU. Ilkovič was very familiar with the scientific competence, organizational skills and nice personal qualities of Vilém Kunzl, as he knew him in Prague. Moreover, Ilkovič intended to develop modern branches of experimental physics in Bratislava. Hence, he recommended inviting namely Dr. Kunzl for professorship, member of spectroscopic scientific school of prof. Dolejšek.

Vilém Kunzl completed his education in Prague on experimental physics, teaching of physics and mathematics. Apart from these subjects, he was educated in history and philosophy of science. He was also listening to lectures on physical chemistry presented by prof. Heyrovský. However, he was greatly influenced by prof. Dolejšek, and young Vilém jointed Dolejšek's group interested in spectroscopy.

In 1937 V. Kunzl was habilitated. Between 1930 and 1937 he published 12 papers in various foreign physics journals. It is very interesting and important that his competence for research and lecturing was evaluated by top European spectroscopists –prof. Laue, prof. Paschen and prof. Fajans from Germany and prof. Trillat and prof. Dauvillier from France. All reviewers were satisfied with Kunzl's results in scientific investigation. For example, prof. Laue wrote in his appraisal: "Dr. Kunzl could apply for habilitation at any university in Germany with full success." The habilitation committee noted in its evaluation "the exceptional ability of Dr. Kunzl to improvise in searching for new solutions, to assemble any devices and to set new experiments". It has to be said that all the mentioned qualities will be used by Vilém Kunzl in Bratislava.

The six months which Kunzl spent in Paris in 1939 were important for his future scientific career. He was working in the spectroscopic laboratory of prof. Cotton. Unfortunately, he was forced to return to Prague untimely due to the political situation in Czechoslovakia. He completed his scientific results only in 1947, during his second stay in Paris. He published two papers there.

Coming back to Prague, Kunzl worked at Charles University very shortly, because in November all Czech universities were closed. After that he worked in an aircraft conditioning house up to the end of World War II.

Now we have to go back to Bratislava. As we said, Vilém Kunzl was invited to become professor of physics at the Slovak University at 1946. He accepted this very attractive offer, so owing to Ilkovič's effort V. Kunzl was appointed professor of physics at Comenius University in 1947. In February 1948 he became head of the Institute of Physics of the Slovak University. Scientific activities at the institute were determined by Kunzl's research interests. So vacuum physics, acoustics and ultra-acoustics, X-ray spectroscopy a high-frequency electro-technology was started and developed in the years 1948-52 at the Faculty of Natural Sciences. I would like to pay attention on the fact that these four physics branches are cultivated in our faculty up to now. Vilém Kunz is rightly considered the founder of experimental physics at Comenius University. However, he also contributed to the development of physics at Comenius University in another way: owing to his effort Ján Fischer came to the Faculty of Natural Sciences.

Ján was the third member of a student group which was formed at second half of the 1920s at Charles University in Prague. As his friends Dionýz Ilkovič and Vilém Kunzl, he was educated at the Faculty of Natural Sciences of Charles University. He studied physics and teaching of physics and mathematics, and his attention was focused on theoretical physics. In 1927 he attended a lecture on quantum mechanics, and another one on the Compton effect in 1928. Both lectures were presented by Viktor Trkal. As Ján was a very gifted theorist, he was recommended by prof. Záviška for PhD study at the University of Zürich. In 1928-31 he completed his PhD study under supervisor prof. Gregor Wentzel. In Zürich J. Fischer completed two papers in which he picked up the threads of Wentzel's work on quantum-mechanics description of Compton effect and photo effect. Fischer had developed a new method to describe these effects in more detail. One can say that his mathematical description concerning the process of absorption in language of quantum mechanics was complete. Thanks to it Fischer's papers became foundation stones of quantum theory of interaction between radiation and matter, so we can consider Ján Fischer as the first Slovak theorist.

Coming back to Prague, Fischer intended to work at Charles University. Unfortunately, at that time he had no opportunity to become an assistant at the Institute of Theoretical Physics. Moreover, he was taken ill with tuberculosis, so he moved back to Slovakia. After pulling through and recovering, Ján started to teach physics and mathematics at various Slovak secondary schools. Only after World War II did he go to Bratislava. In 1950, owing to efforts by Vilém Kunzl, Ján Fischer was appointed assistant professor at the Faculty of Natural Sciences of the Slovak University. When prof. Kunzl came back to Prague in 1953, Ján Fischer took over administration of the Department of Physics. It was divided in 1961 and prof. Fischer became head of the Department of Theoretical Physics. He administrated it between 1961 and 1975, and created very suitable conditions for forming Bratislava's school in theory of elementary particle.

To sum up, Dionýz Ilkovič founded undergraduate study for future physics teachers and future physicists at both Slovak University and Slovak Technical University of Bratislava. He started physical research in Slovakia. First his followers moved to Košice at the beginning of the 1950s and founded a second centre of physics. Vilém Kunzl started and developed four physics branches at the Faculty of Natural Sciences, which are cultivated there up to now. Vilém Kunz is considered a founder of experimental physics at Comenius University. Ján Fischer became the first Slovak theorist and founder of theoretical physics in Bratislava.

No doubt, D. Ilkovič, J. Fischer, and V. Kunzl could become founders of Slovak physics, mainly owing to their high-quality training at Charles University in Prague. Moreover, creating the new branch of science, seeking its orientation, forming undergraduate training of future physics teachers and physicists, they used fairly all their experience, methods, opinions, and ideas which they met in Prague. Many of their scholars moved from Bratislava to other Slovak cities to find new physical centres.

The development of physics in Slovakia was affected by Vienna University too. As prof. Ilkovič was not interested in quantum mechanics, he recommended Imrich Staríček for study of quantum mechanics and other modern branches of physics in the University of Vienna. Returning to Bratislava, Staríček started lecturing a course of quantum mechanics at Comenius University. I. Staríček begun in 1951 to collaborate with Dr. Václav Votruba, professor of theoretical physics at Charles University in Prague. Votruba had studied (1945-47) at ETH Zürich under supervisor prof. Wolfgang Pauli. After coming back home, he started cultivating elementary particle theory in Czechoslovakia. Unfortunately, collaboration between Staríček and prof. Votruba was ended very soon, as I. Staríček was arrested by the communist police for fictitious espionage and committed to prison for ten years.

For this reason, prof. Votruba continued to lecture quantum mechanics in Bratislava. During his lectures, he paid attention to the gifted student Milan Petráš. In such way collaboration between prof. Votruba and Milan Petráš started. Václav Votruba gave young Milan directions for his diploma thesis and recommended him to specialize in elementary particles theory. Between 1952 and 1955 Milan Petráš, a student of Comenius University, completed his PhD study in Charles University under supervisor V. Votruba. Returning to Bratislava, M. Petráš continued his investigation, and step by step he created Bratislava's school in elementary particles theory. It is not an exaggeration to say that an overwhelming majority of Slovak theoreticians who were or are interested in elementary particle theory are Petráš's scholars –either directly or indirectly. Clearly prof. Votruba vicariously contributed to such success.

Conclusion

As we said at the beginning, the formation of Slovak physics is a pregnant example of the importance of idea circulation. All founders of Slovak physics –Ilkovič, Kunzl, Fischer, Staríček and Petráš– were educated in Prague or in Vienna. Moreover, Ilkovič, Kunzl and Staríček were developing their physical competence in Paris, Fischer in Zürich, Petráš in the Joint Institute of Nuclear Research at Dubna. Returning to Bratislava they all used experiences gained outside Comenius University in educating many generations of Slovak physicists and physics teachers. We have to thank them for their efforts.

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THE INVENTIONS OF JOSÉ RUIZ-CASTIZO: THE PLANIMETER

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Abstract

The aim of this work is to describe the invention of a planimeter by the Spanish physicist-mathematician José Ruiz-Castizo Ariza (1857-1929). He was a professor of Mecánica Racional *in the Universities of Zaragoza (1896-1905) and Madrid (1905-1929). Before getting his chair, being assistant professor of* Física Superior *in the Faculty of Sciences in Madrid (1892-1896), he invented a planimeter the analytical basis of which was different from others known then. During his first years in Zaragoza, he published an extensive article explaining the theoretical foundations and the phases followed in its design, from the first prototype to the final result. Ruiz-Castizo's planimeter, which ameliorated the performance qualities of the ones by foreign authors that were being used in Spain, was built in Switzerland in 1897, tested in the Public Works Office (*Oficina de Obras Públicas*) in Zaragoza and used in other technical Offices, including the local delegations of the National Geographic Institute (*Instituto Geográfico Nacional*).*

José Ruiz-Castizo y Ariza (1857-1929)

Ruiz-Castizo was born in Fuentes de Andalucía (Sevilla), and attended High School in the *Instituto provincial de Sevilla*, where he graduated in 1876 with excellent qualifications. He started a degree in Mathematics in this town and finished it in the Central University in Madrid in 1881, taking the doctor's degree in 1887.

Afterwards he started, in the capital too, an academic life with the following jobs: Assistant in the General Preparatory School for Engineers and Architects (1888-1892), Assistant of *Física Superior* in the Faculty of Sciences (1892-1896) and Auxiliary professor in the Physic-Mathematical Section in the same institution (1889-1896). Since March 1896, he was Professor of *Mecánica Racional* in the University of Zaragoza and at the same time he taught *Elementos de Física* in the Arts and Crafts School (*Escuela de Artes y Oficios*). Up to 1905 he completed his academic life as Professor of *Mecánica Racional* in Madrid, where he died. Ruiz-Castizo was member of the *Ateneo of Madrid* since 1891, and elected member of Royal Academy of Sciences in December 9, 1914, without ever taking possession.

Fig. 1: A photograph of Ruiz Castizo[1]

During this teaching time, he published in physics, mathematics and disclosure issues in several journals, and also, some monographs and books such as[2]:

Articles in journals:

- *La Naturaleza:* La electróptica y las ideas de Maxwell, El centro de gravedad, 1895.
- *Revista de la Real Academia de las Ciencias:* Los principios fundamentales de la Mecánica racional. Un primer capitulo de la dinámica, 1909.
- *Revista Trimestral de Matemáticas:* Algunas fórmulas para el empleo de ejes coordenados oblicuos en la Mecánica analítica, 1903; Sobre las hipótesis que sirven de fundamento a la Mecánica Racional, 1903; Nota sobre una propiedad de las formas cuadráticas, 1904; Resolución algebraica de la ecuación de cuarto grado, 1905.
- *Revista Matemática Hispano-Americana:* Prontuario de las cúbicas planas, 1925.

Books

- *Estudio analítico de un lugar geométrico de cuarto orden*, Madrid 1889.
- *El Eclipse total de sol de 1905* (with Gabriel Galán), Zaragoza 1905.
- *Tratado de Mecánica Racional*, Madrid 1907-08.

It's interesting his facet as inventor. His most important invention is the planimeter, the object of this article, but at least we want to mention a regulating mechanism for vapour machines (1919)[3] and a transmission through gears (1920)[4].

Planimeters

During the first half of the 19th century, it was necessary for an instrument to measure with precision an area represented in a plane. This need inspired several ingenious devices. Planimeters are apparatus that allow to measure areas enclosed by plane curves having the integral calculus as its theoretical basis. Various scientists invested time and effort to the design, construction and improvement of these instruments.

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1 See [1, p. 249].

2 We don't include publications referred to the planimeter because they will be cited along the text and also, in the references at end of the article.

3 *Boletín Oficial de la Propiedad Industrial*, November 16, 1919.

4 Proyectos de aparatos: año de 1928. Carp. 15. Instituto del Material Científico, CSIC.

Johann M. Hermann invented the first planimeter in 1814, which was improved by Lämmle in 1816 and built in 1817. The Swiss engineer Johannes Oppikofer invented another planimeter in 1826, which was built in 1834 by M. Ernst in Paris. As the Hermann planimeter, the design is based on a cone that rotates on an axis, which supports a wheel that rotates with it. In 1849, Kaspar Wetli invented another one that was built by Georg Christoph Starke.

But the planimeter that involved an essential change, in relation to the design and concept of these machines, was the one invented by the Swiss mathematician Jakob Amsler-Laffon (1823-1912). It is considered to be the first modern planimeter. It was built in 1854 and has two variants: polar and lineal. The polar planimeter is composed of two articulated arms. One of them has the unarticulated end fixed, so the articulation point moves along a circle. In the lineal planimeter, this last point moves along a straight slot.

An atypical planimeter was the one designed in 1875 by Holder Prytz (1848-1930), mathematician and Danish cavalry officer. It was made in Denmark in 1887. It had a simple design, but the precision of its measures was limited[5].

Interest in Spain by planimeters is evidenced by the publication in the journal *Revista de Obras Públicas*[6] of the description and use of several of them (Amsler, Wetli and Starke, Duprez) between 1861 and 1872. Ruiz-Castizo took these planimeters as reference and proposed to improve its performance in several aspects, which he finally achieved after a process of successive improvements from a novel theoretical basis.

Ruiz-Castizo's planimeter

When Ruiz-Castizo started publishing articles about his planimeter in 1896, he was already testing a prototype that he had patented with the name "Planímetro é integrador cartesiano de evaluación tangencial" in August 21, 1895. This patent expired in December 11, 1897 in order not to pay the second annual payment. The reason was that he had filed a new patent application for his final model of "planímetro e integrador tangencial", which was granted on December 18, 1897. Moreover, in order to push forward its innovative project of planimeter, Ruiz-Castizo sought the support of the Royal Academy of Sciences , which issued favourable reports in 1896 and 1898 for each of the successive models.[7]

The author published the "principio analítico" or the mathematical basis of his planimeter in the journal *La Naturaleza*, in which number of June 18, 1896 appeared a short article [2]. Ruiz-Castizo considers a curve $y=f(x)$, a point on it varying M, P the projection of M on the horizontal axis (Fig. 2)[8] and MQ a line whose distance to P is constant and equal to a; then he shows that the envelope of the family of straight lines MQ is "una verdadera cuadratriz del area comprendida entre la curva $y=f(x)$ y el eje de las x", ie, that the ordinates that are limiting the area enclosed by $y=f(x)$ and the axis x , intercept an arc in the envelope curve, whose length multiplied by a is equal to this area.

Indeed, if (x,y) are the coordinates of M, the family of straight lines MQ (with slope μ) has as equation $Y–f(x)=m(X–x)$, being $m=\mathrm{tg}\mu$, $\cos\mu=a/y$, so the differential arc length of the envelope curve is $ds=(1+m^2)^{½}dx=(y/a)dx$ and then $A=aL$, where A is the area under the initial curve and L is the length of the envelope between the same abscissas.

Fig. 2 shows that the envelope proposed by Ruiz-Castizo is for a family *NN'* parallel to *MQ*, *N'N''* parallel to *M'Q'*, etc., all them with the same vertical displacement, which is an envelope moved vertically of the previous one therefore with the same length between points with the same abscissas. When he raised the problem with this vertical displacement, he was thinking in the practical use of this analytical principle, because he knew, and so he writes, that the envelope curve can be drawn and measured with great accuracy through a kinematic combination, not difficult to get. Also, the previous analytic relationship gave rise to a new mechanical integrator, a subject that he began to develop at the end of that same year in a journal with wider spread[9], when he had already got a favourable report from the Royal Academy of Sciences.

At the end of 1896, the first appeared of the six parts in which Ruiz-Castizo divided the article titled "Un Nuevo planímetro", published over more than a year in the *ROP* [5]. The second one took nearly a year to appear, which happened immediately after the author sent, in September 26, 1897, the second request for the report to the Royal Academy of Sciences. Thereafter the parts were approximately monthly until the end of the article. In this work, besides repeating the theoretical foundations developed in *La Naturaleza*, he explains the design and construction of the planimeter in various phases, the first of which was already in trial operation. The chronological scheme of successive deliveries can be seen in Table 1.

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[5] You can see a historical outline about these planimeters in [6] and [7, ch. 9].

[6] From now on, we'll refer to this journal as *ROP*.

[7] See *Anuario* of the Royal Academy of Sciences, 1896. The second report is included in the memoir [5].

[8] See [3, p. 271].

[9] He published in *La Naturaleza* a second article on the planimeter [3], but its information is contained in the one published in *ROP* we are going to discuss.

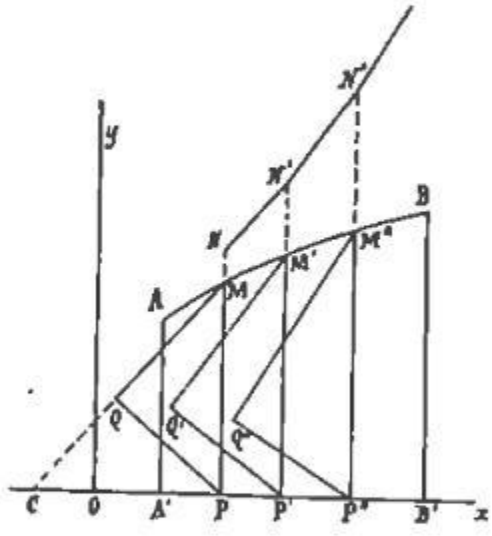

Fig. 2: Generation of the "cuadratriz" (envelope curve).

Table 1: Chronology of the successive deliveries of the article "Un Nuevo planímetro".

	Original date	Published	Content
1	11/1896	5/11/1896	Proof of the "Principio analítico". Schematic-theoretical description
2	07/1897	30/09/1897	Discussion of the model. Kinematic questions. Inclination of appraising wheel
3	08/1897	28/10/1897	Guiding reciprocating mechanism: articulated parallelogram and gear system
4	12/1897	23/12/1897	Two prototypes: parallelogram and wheel
5	01/1898	27/01/1898	The final model
6	01/1898	24/02/1898	The "integrómetro general"

We describe now the contents of each part:

1. The author begins by highlighting the practical importance of planimeters, but he argues that their results are somewhat insecure, so that to choose a trustworthy planimeter is a trouble. Then, he justifies his proposal for a new instrument which provides no little interest because it has an analytical principle totally different from those that have so far been used, reducing the problem of squaring an area to the rectification of a well-defined curve. Then he exposes the analytical principle repeating what he has already published in *La Naturaleza* a few months earlier. The new thing he shows now, with a graph (Fig. 3) as above (Fig. 2), is a first sketch of the instrument, referred to as "descripción *teórico-esquemática*", that embodies the theoretical explanation, so *YDD'* corresponds to *PQM*.

The instrument consists of two rules, *XX'*, *YY'* plus a square *YDD'* with right angle at *D*. The rule *YY'* moves perpendicularly to *XX'*, which is fixed. The end Y of the square moves on *XX'* at the same time as *YY',* and the square has also a movement inside the slide *M,* when this moves along *YY'*. The slide *M* carries a pointer to be moved along the given curve. Another slide *R* is moved on *YY'* connected to *M* by a mechanism that forces *R* to move in the direction *DD'*, describing the envelope by a wheel whose rotation is recorded to measure its length, equal to the area of the curve given. Ruiz-Castizo finishes explaining that an appropriate mechanism will transmit the movement of the square *YDD'*, when the pointer located in *M* moves along the curve given, to the slide *R* in which the wheel will move without slipping on the role, because of its suitable inclination. After this general idea, Ruiz-Castizo postpones until the following parts other details of the instrument and the results obtained with the already made models. The photograph of one of them, which regulates the movement of the square with an articulated parallelogram, appears in the first page of the article, although its description will be undertaken in the fourth part.

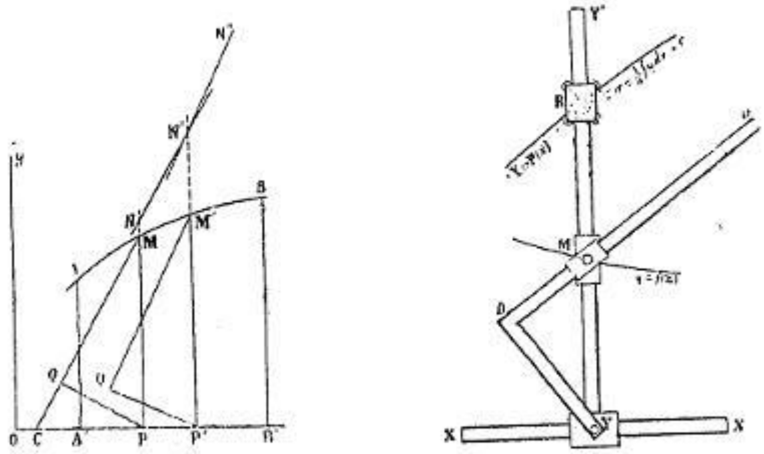

Fig. 3: Graph and sketch of the planimeter.

2. The author explains that in his first request to the Royal Academy of Sciences, he named the instrument as "*planímetro cartesiano de valuación tangencial*", a name inspired by the wheel on *R* which moves tangentially to the envelope curve. Afterwards he emphasizes the great simplicity of his theory, which considers very important. He believes that the easier the theoretical principle is, the easier it will be also to permanently check its regular working.

In the three examples in Fig. 4, the shape of the "cuadratrices" are shown depending on the complexity of the curve limiting the area. To the left, you get the area under the curve through two discontinuous arcs, requiring a shift of planimeter to allow the pointer to go through the whole curve. In the centre, the envelopes for a closed curve with a very regular form. When the planimeter moves along the arc *ACB*, the envelope *αβ* is generated, and along *BDA*, the appraising wheel rotates in the opposite direction giving the envelope *βα'*. So the counter will provide, by the difference between the lengths of the sections, the area enclosed by the curve *ACBD*. To the right, an irregular closed curve's area can be obtained by the algebraic sum of several arcs of envelope.

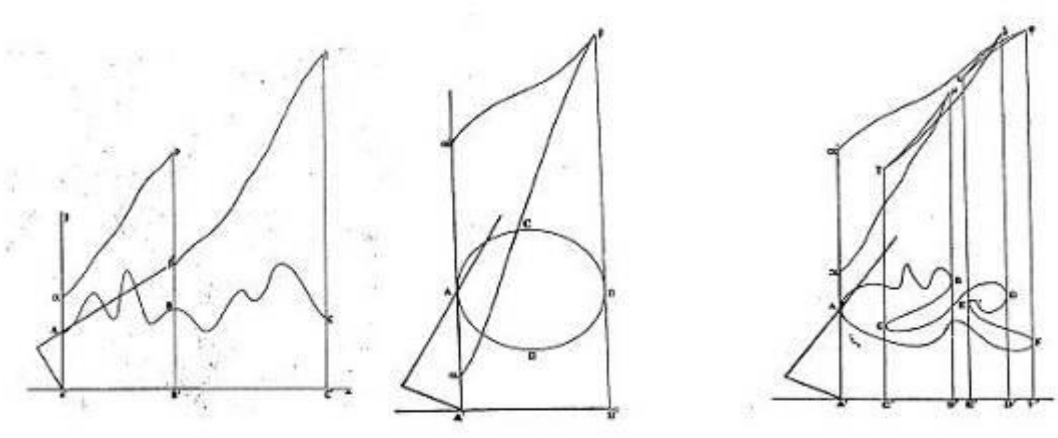

Fig. 4: Examples of curves with their envelopes.

The author notes that in making these operations it may be necessary to move the planimeter to complete the figure, which represents a reduction of time, work and possible errors in relation to other ones used. Another interesting quality is that the accuracy of his instrument can be checked *a posteriori* because it also draws the envelope curve, i.e., it's an "intégrafo" and no other has this quality. He considers this opportunity of rectification important, because its movements were not completely sure and could cause "errores accidentales de importancia". This observation gives him the opportunity to explain that the device is easy to make in the workshops and its perfect operation depends on two aspects: the friction and the appropriate mechanisms to convey the proper orientation to the appraising wheel located in *R*. Ruiz-Castizo proceeds to a technical study, kinematic and dynamic, of these aspects, noting in conclusion that the maximum tilt of the wheel admissible in practice will mainly depend on the base material on which this one is supported and the degree of sharpening of its edge.

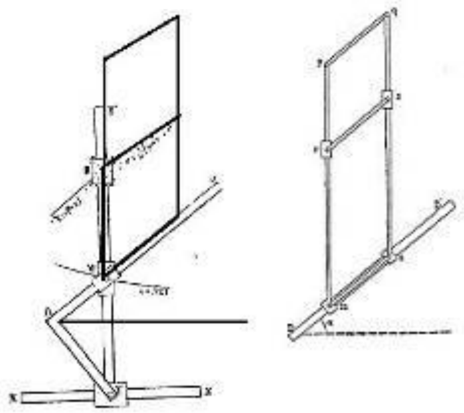

Fig. 5: The articulated parallelogram.[10]

3. Ruiz-Castizo focuses the third part to explain the mechanism to ensure that the inclination of the axis *DD'* coincides with the appraising wheel's, located in *R,* regardless of the distance between both elements. After calculating the errors that can be made by mismatches at this angle, he proposes and studies two modes of transmission, "un paralelogramo articulado, ó un juego de engranajes", to choose depending on the angle of rotation. He explains the parallelogram method around the Fig. 5 and concludes its good efficiency for amplitudes of rotation smaller than $\pi/2$, assuming a careful construction of the apparatus. He says he already has two prototypes with this system, but postpones their description for the next part. He devotes the end of this part to describe the gear set, although this time he has ruled out this system.

4. The planimeters appear at the end of the fourth part. Their photographs are exhibited and Ruiz-Castizo makes a detailed description of each element and of its functioning, starting with the one shown in Fig. 6.

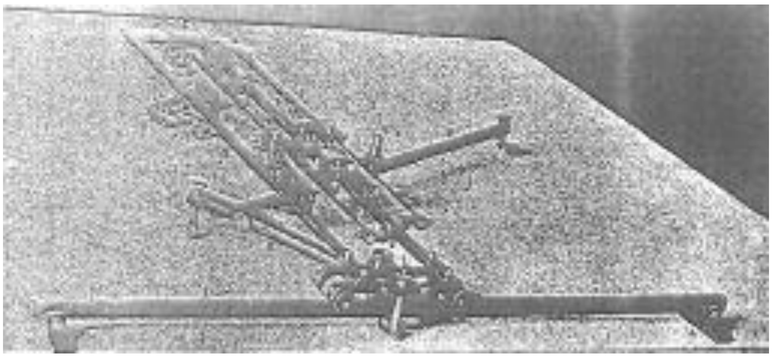

Fig. 6: First prototype.[11]

Ruiz-Castizo recognizes that the planimeter was made in Madrid by two technicians with limited technical recourses. Then, although he supervised it piece by piece, it had manufacturing defects that produced significant errors. But even properly made, he also indicated that it would work accurately, but was still uncomfortable in its practical use. There were some problems, for example, with the type of paper to use so that the wheel recorded the envelope curve without damaging it. Also, there were practical drawbacks of some importance, such as the movement range of the mechanism, that could cause the drawing of too many partial arcs to certain "cuadratrices" or an inconvenient access to the counter.

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[10] The right figure is in the article; the left one has been added to indicate its placement on the general sketch of the planimeter.

[11] See: [5, 1897, nº 1161, p. 671]

While the previous model was being built, the inventor contacted the *Société Genevoise pour la construction d'Instruments de Physique et de Mathématiques*, and came to introduce a simplification that produced an important progress. The square set was replaced by a wheel, whose radius was equal to the base of integration as shown in Fig. 7, with a rod permanently resting on it and passing through the point where the punch was located.

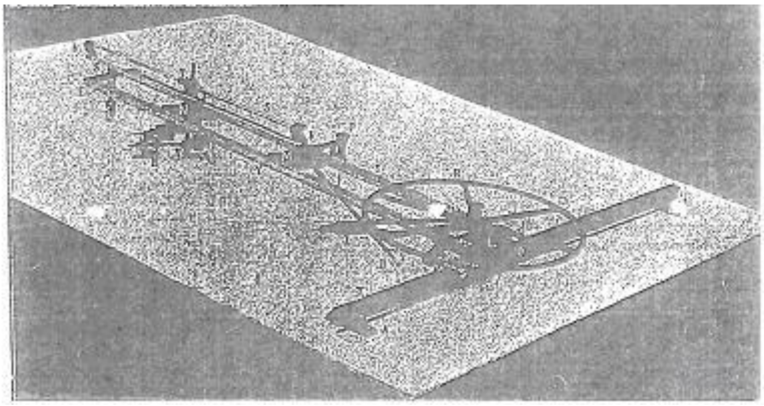

Fig. 7: Second prototype.[12]

With this change, the instrument movement became softer and had a much more satisfactory performance, with very accurate results "por defecto siempre". But the practical management problems, already mentioned for the previous model, persisted. So he addressed a final reform of the instrument which led him to change the details of its construction. Ruiz-Castizo says that this new model, already built by the *Société Genevoise* and tested by the staff of the Public Works offices in Zaragoza, has satisfied the most demanding expectations. The author leaves for following delivery a detailed description of this brilliant final model.

Before concluding, we note that Ruiz-Castizo had recognized in this part that his article consisted, in the first fourth parts, of "algo así como una sucinta historia de mis trabajos, un archivo, diríamos, del proceso de mi humilde creación", i.e. the article was a summary of the process that led him to the final model.

5. In this fifth part, Ruiz-Castizo presents the final model of the planimeter, which successfully completes the process begun years ago. With the photograph of the instrument, Ruiz-Castizo makes its description in detail, including the kinematic mechanisms and the one that ensures the prospered orientation of the appraising wheel any time. The inventor highlights that the fundamental change introduced is to draw the envelope curve on a cylinder instead of the picture plane. He chose boxwood for the cylinder.

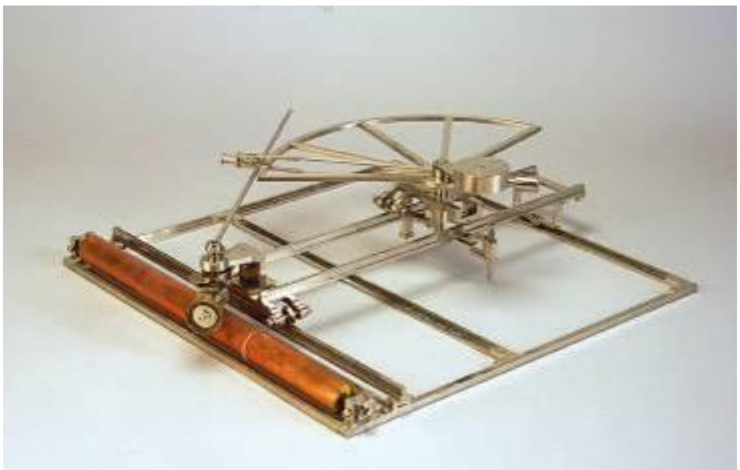

Fig. 8: Ruiz-Castizo's planimeter.[13]

[12] See: [5, 1897, nº 1161, p. 671]

He describes the main dimensions of the instrument and the whole amplitude of its movements. Then, he goes on to considerate the numerous advantages achieved with this new planimeter and to give precise data about its high precision: “Es lícito ya sentar que ofrece excelentes condiciones como mamuable, expedito, seguro y exacto”.

Finally, Ruiz-Castizo says that his instrument had been supported by the Public works staff (that already examined his first model), other technical agencies in Zaragoza[14] and professors. Also, he adds that the *Inspección del Canal Imperial* and the *Jefatura provincial de Obras Públicas* had acquired several copies.

Thus, Ruiz-Castizo managed to put on the market an instrument of national design and Swiss manufacture which competed advantageously with other imported planimeters.

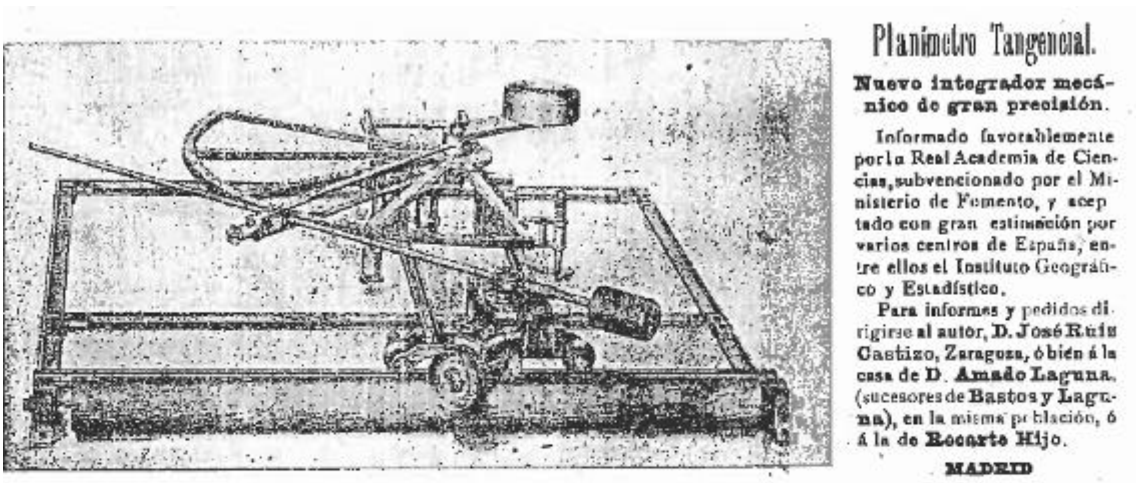

Fig. 9: Advert published in *La Naturaleza*, October 8, 1903.

6. The final part refers to a new instrument that, based on the planimeter, extends its functions to be an “integrador applicable a los tres órdenes”, that can calculate the typical integrals of the planimeter, $\int ydx$, and the ones of second and third order: $\int y2dx$, $\int y3dx$. Ruiz-Castizo adds that this new planimeter has not been built yet, therefore the current work is rather a memoir or a draft of it where critical comments could be made about its construction and applications in the future. For this reason, and to limit the extent of our work, now we confine ourselves to the image of the instrument (not complete) that he introduced to give a graphic idea of its composition.

That same year (1898), he published a memoir [5] that presents the integrator, and, as a particular case, the planimeter, reproducing the contents of *ROP*, with different organization in the text except in its historical parts.

[13] This photography can be seen in the following page:
http://www.ign.es/ign/layoutIn/museoListadoInstrumentos.do?area=gabinete&indice=7

14 Such as: *Oficina del Canal Imperial de Aragón*, *Distrito Forestal*, *División Hidrográfica del Ebro* and *Centro Topográfico*.

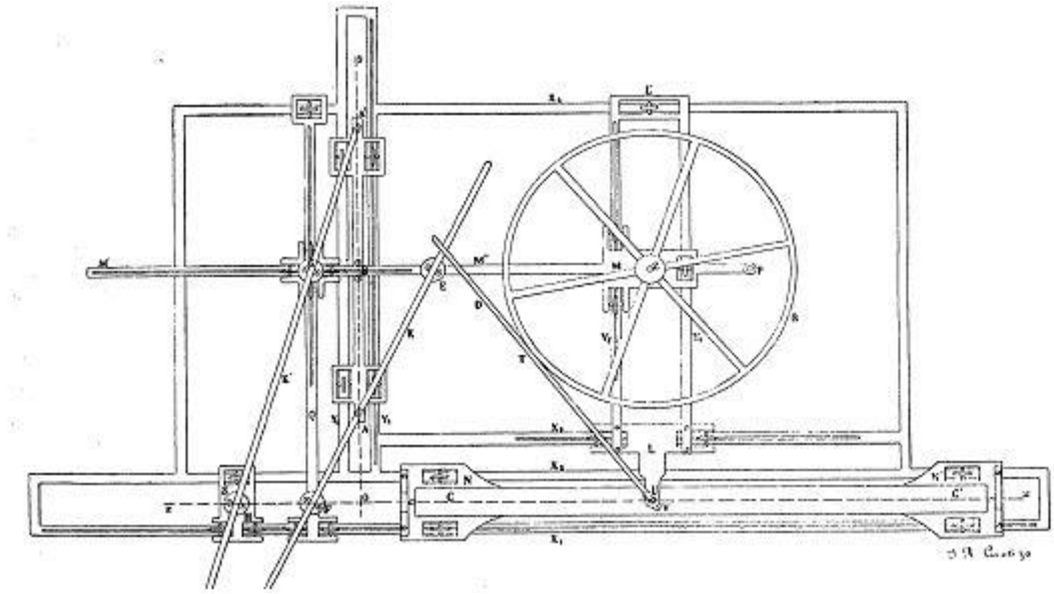

Fig. 10: Integrator for the three orders.

References

[1] GONZÁLEZ REDONDO, Francisco A., FERNÁNDEZ TERÁN, Rosario E., DE VICENTE LASECA, Lourdes (2007): Los Catedráticos de Matemáticas en la Facultad de Ciencias de la Universidad Central de Madrid durante el primer tercio del siglo XX: una historia gráfica. *La Gaceta de la RSME*, 10 (1), 241–260.

[2] RUIZ-CASTIZO, José (1896): Sobre la cuadratura de las áreas planas. *La Naturaleza*, VII, 270-271.

[3] RUIZ-CASTIZO, José (1897): Sobre la cuadratura de las áreas planas. Un nuevo planímetro. *La Naturaleza*, VIII, 410-414.

[4] RUIZ-CASTIZO, José (1896-1898): Un Nuevo planímetro. *Revista de Obras Públicas*: (1896) II (19), 374-376; (1897) II (1149), 362-365; II (1153), 471-473; II (1161): 669-673, (1898) I (1166), 55-59; I (1170), 130-136.

[5] RUIZ-CASTIZO, José (1898): *Teoría de un nuevo integrador mecánico general para los tres órdenes*. Madrid, Imprenta y Encuadernación de G. Juste.

[6] SAVOYSKY, Serge (2002): Les planimètres. *Arts mécaniques*, nº 23, 1-16.

[7] TOURNÈS, Dominique (2004): *Autour d'un mémoire de Vincenzo Riccati : histoire de la construction tractionnelle des équations différentielles*. París, L'Université Pierre et Marie Curie.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 1089.

UNSUCCESSFUL TRANSMISSION OF KNOWLEDGE. AN EXAMPLE OF THE ASTRONOMICAL SOCIETY OF INDIA

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Abstract

After some parts of Indian territories came under the British East India Company, in 1784 William Jones from Oxford founded the Asiatic Society of Bengal *in Calcutta. In the following years the Society was immensely successful in spawning a network of scientific organisations and institutions like the Royal Botanical Garden, the Indian Museum, the Meteorological Department of the Government of India and the Medical College of Bengal. So far the astronomical research based on the "western methods" were concerned, the East India Company established an observatory at Madras for promoting the knowledge of astronomy, geography and navigation. The building for the observatory was completed by 1792. Institutions like the Asiatic Society and Madras Observatory (founded in 1879) played a crucial role for the transmission of knowledge from West to East and vice versa. Contrary to them the Astronomical Society of India (founded in 1910) failed. The present communication speculates about the reasons behind the failure in social and cultural context.*

Introduction

The *National Geographical News* on September 19, 2010 reported that *"India unveils world's highest observatory – The latest hot spot for international astronomers is perched on a lonely peak in the western Himalayas."*[1] Today India possesses 16 observatories and belongs to nations taking active part in astronomical researches.[2] She has the Astronomical

[1] http://news.nationalgeographic.com/news/2001/12/1227_020104indiaobs.html, accessed Feb. 19, 2011.

[2] For more detail see, J.C. Bhattacharyya, A. Vagiswari, 'Astronomy in India – the 20th century', in: *History of Astronomy in India*, Eds. S.N. Sen, K.S. Shukla, Indian National Science Academy, New Delhi (1985). R. Kochhar, J. Narlikar, *Astronomy in India: A Perspective*, Indian National Science Academy, New Delhi (1995).

Society of India (established in 1972).[3] The less known fact is that already in 1910 the Astronomical Society of India (henceforth ASI) was founded. After more than a decade it died its own death. In the following paragraphs a short history of the activities of the ASI and reasons behind its "disappearance" are discussed.

The Astronomical Society of India and its activities

In April-May, 1910 the famous Halley's Comet was observed in India (see Figure 1). It evoked interest of the general public in astronomy. Consequently it led to the foundation of the Astronomical Society of India.[4]

Fig. 1: Halley's Comet photographed at Kodai Kanal Observatory on 3rd May 1910.
Courtesy of the *Journal of Astronomical Society of India*, 1, 1910-1911.
Archive – Indian Institute of Astrophysics, Bangalore.

The main objective of the ASI was to foster interest in general and practical astronomy. For this purpose, it took various steps like the following:

- In order to be informed about the international researches in the field, the Society subscribed journals from the Royal Astronomical Society of London, the Astronomical Society of Italy, the Royal Observatory of Belgium, Paris Observatory and others.
- The Journal of the Astronomical Society of India was started. In it the annual/monthly meetings reports, popular and general lectures, scientific papers and notes of the Society were published.
- The Society bought instruments from abroad. For astronomical observations the Society's Observatory installed an 8.5-inch telescope, which was housed at the roof of the Treasury Buildings.
- To acquaint the members with the methods of observations, practical classes were held.
- During the meetings, the members were informed about the construction of a cheap telescope and the erection of an observatory.

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[3] http://www.astron-soc.in/, accessed Feb. 19, 2011.

[4] For more details about the foundation and activities of the Astronomical Society of India, see, R. Singh, 'The 100th anniversary of the Astronomical Society of India', *Science and Culture* (forthcoming).

- To motivate the students, a prize for the best essay was initiated.
- Popular lectures were organised by the society.

Astronomy –a field without perspectives

The causes for the disappearance of the society were as follows:

- In general, when a new organization is started the motivation is high. It fades away as time passes, if its members and supporters do not get any benefit. At the *Annual Meeting of the Society* in November 17, 1919 in the Presidential address it was pointed out that most of the members had their own profession (such as Bank officer or clerk) to have any opportunity for carrying out original investigations. Thus interested members could not adopt astronomy as their profession.
- India lacked the class of amateur which had made England famous in science, as was once said by the President of the ASI.[5]
- The East India Company, for promoting the knowledge of astronomy, geography and navigation in India, established the Kodaikanal Observatory in 1879.[6] According to the author D.C.V. Mallik, *"The Government of India had supported the work in the belief that a study of the Sun would help in the prediction of the monsoons, their success and failure, the latter often leading to famines that caused such a havoc. The Solar Physics Committee also suggested to the Surveyor-General, Government of India 'that photographs of the Sun should be taken frequently in order that India might assist changes in the solar surface'."*[7] In the following years the observatory played an important role for the study of stars, and in particular the sun. These observations were important for the colonial rulers as the knowledge won there could be used for sailing purposes.[8] This was contrary to the ASI and its work, thus not much importance was given to it.
- As far as climatic and geographical conditions were concerned, Calcutta was not suitable for astronomical observations as its geographical position is little raised from the sea-level. Also vapour-laden atmosphere, sooty vapours poured forth from an ever-increasing number of chimneys, the glare from the city lights made the observations difficult.[9]
- In the West astronomy boomed with the discovery of the telescope in the 17th century. In the following years a number of telescopes were built. In general the making of scientific instruments was accepted as a profession. In India glass as the backbone of optical instruments did not enjoy a social status similar to that of metals and pottery, which were preferred to glass vessels, especially on religious occasions. No doubt the Indian craftsmen mastered the glass technology, but the society accepted it only for decoration and ornaments.[10] Apart from that a caste ridden society made it impossible that a glass-maker worked together with a metal-specialist. For generations a profession was practiced by families. A French writer, Jean Baptist Tavernier, once observed as follows *"A goldsmith would not work in silver, nor a silversmith in gold. In the looms, a weaver would weave only one single sort of stuff during the whole life, unless he was compelled to take another in hand."*[11] Obviously, making a telescope by local workers was not an easy task. The only alternative was to import expensive instruments from abroad.[12] Some of the active members of the ASI like C.V. Raman (who in 1928 along with his student K.S. Krishnan discovered the effect named after him, and in

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[5] 'Annual Meeting Report ASI, Nov. 17, 1919', in: *Journal of the Astronomical Society of India* 10 (1919-1920), p. 1-12.

[6] http://www.ias.ac.in/jarch/jaa/21/103-106.pdf, accessed Feb. 13, 2011.

[7] D.C.V. Mallik, 'The solar physics observatory at Kodaikanal and John Evershed', *Resonance* 14 (2009), p. 1032-1039.

[8] S.S. Hasan, D.C.V. Mallik, S.P. Bagare, S.P. Rajaguru, 'Solar physics at the Kodaikanal Observatory: A Historical Perspective', *Indian Institute of Astrophysics*, Bangalore, India, arXiv:0906.0144v1 [physics.hist-ph] (May 31, 2009), p. 1-25.

[9] 'Meeting Report ASI, Nov. 26, 1912', in: *JASI* 3 (1912-1913), p. 31-37.

[10] http://www.infinityfoundation.com/mandala/t_es/t_es_goyal_glass_frameset.htm, accessed Feb. 19, 2011.

[11] Cited after A.K. Bag, 'Technology in India in the 18th –19th century', *Indian J. Hist. Sci*., 17 (1982), p. 82-90.

[12] For more details on the making of metallic scientific instruments and the importing of glass instruments, see R. Singh, 'Designing, making and using scientific instruments in India: The example of Jagadis Chunder Bose', *Proceedings of the Scientific Instrument Commission of the IUHPS*, Athens 2002 and Newport News, 2003 (Ed. Jim Bennett), Leo S Olschki Editore MMV, Firenze, (2005), p. 713-723.

1930 won the physics Nobel Prize[13]), though very much interested in the subject, put aside his astronomical researches as he could not afford proper instruments.[14]

- In the *Annual Report* for the Year 1919-1920 the financial position was called satisfactory by the Business Secretary.[15] In general the rich community of Calcutta was less interested in astronomy as compared to physics and chemistry. For instance, in the 1910s Palit Professorships were created at the University of Calcutta with huge donations. None came forward to spend such a huge amount on astronomical researches.

The Astronomical Society of India did not produce any scientific research of international level. Almost within a few years, its name disappeared from the memories of prominent physicists like M.N. Saha, who delivered popular lectures at the Society. At the 25th anniversary of the Indian Science Congress, Saha wrote on the *"Progress in physics in India during the past twenty-five years"*. He reported the work done in various fields of physics including astronomy and astrophysics. He did not mention the name of the Astronomical Society of India.[16] In 1948 Raman gave popular lectures in which he did not mention about his activities within the ASI.[17]

Concluding remarks

- The modern policy makers, in particular in the field of international politics, may learn from the history of the Astronomical Society of India that the transmission of knowledge might not be successful if the local community is devoid of either financial or scientific or technological benefits.
- Before starting a project, it is necessary to make a geographical analysis of the place, taking for instance the projects related to wind and solar energy.

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[13] A similar effect was observed by the Russian physicists GS Landsberg and LI Mandelstam nearly at the same time. They published their results later and quoted Raman and Krishnan publications. Later, it led to dispute not only for the credit for the discovery, but also for the sharing of the Physics Nobel Prize in 1930. For details, see R. Singh & F. Riess, 'Seventy years ago – the discovery of the Raman effect as seen from German physicists', *Current Science* 74 (1998), p. 965-971; 'The 1930 Nobel Prize for physics – a close decision?', *Notes and Records of the Royal Society of London* 55 (2001), p. 267-283.

[14] For more details on CV Raman's researches in the field of astronomy see, R. Singh, 'CV Raman and his astronomical researches', *Current Science* 99 (2010), p. 1127-1132.

[15] 'Annual Meeting Report of the Astronomical Society of India 1919-20', in: *JASI* 11 (1920-1921), p. 3-7.

[16] Saha, M.N., 'Progress of Physics in India during the past twenty-five years', in: *The Progress of Science in India during the Past twenty-five Years*, Ed. Prashad, B., The Indian Science Congress Association, Calcutta (1938), p. 674-741.

17 C.V. Raman, *Aspects of Science*, Nalanda Publicaitons, Bombay (1948), p. 88.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 1093.

DAYTON C. MILLER'S ETHER-DRIFT EXPERIMENTS

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Abstract

One of the most important cases of circulation of a physical idea in first half of the 20th Century concerns the relativity theory. Miller's experiments and their international reception are an example of how the circulation of relativity theory and some physicists changed the scientific research programmes.
Between 1902 and 1926, Miller made several ether-drift experiments similar to the well-known 1887 Michelson-Morley one. The 1920s Miller's experiments, on the contrary to Michelson-Morley's ones, showed a periodic second-order effect that he interpreted as a proof of the absolute motion of the Earth. Since the fringes shift he measured corresponded to a velocity of the Earth lower than the one expected in a fixed-ether theory, he developed a theory of absolute motion of the Solar System and published it in 1933.
He thought that his results disproved the theory of relativity, but most of the international scientific community did not believe him and preferred a different explanation, like temperature variations, wrong data analysis, experimental errors, etc. After Miller's claims, the Michelson-Morley experiment was repeated many times from 1926 to 1930 by other experimenters with different instruments. All these experiments gave a null result.
In 1955, Shankland and his co-workers explained Miller's data as depending on temperature variations, in the context of the relativistic research program.
My study has the aim to broaden the understanding of what happened between 1925 and 1955:

1) Knowledge and interpretation of Miller's data inside the international physics community;
2) If there was a controversy inside American or European scientific communities;
3) The relation between Dayton C. Miller and other American or European physicists.

The themes I am willing to analyze are important in developing the history of relativity, in particular the history of the reception and circulation of relativity in the American scientific community.

Introduction

In 1925 Dayton C. Miller announced that his repetitions of the Michelson-Morley experiment had shown an effect corresponding to an ether wind of about 10km/s.[1] This finding was the most acknowledged fact in contradiction with special

[1] Miller summarized all his work about these ether-drift experiments in D. C. Miller, "The Ether-Drift Experiment and the Determination of the Absolute Motion of the Earth", *Reviews of Modern Physics*, 5 (1933): 203-42.

relativity theory during the 1920s. A minority of scientists are still claiming that this discovery was fundamental and that it was discarded without an acceptable explanation.[2] This communication has the purpose to analyse some features of the US reaction to Miller's announcement, which has not found an adequate study in historiographical descriptions of the events.

It could be useful to begin with a short chronology of the principal events linked to the MM experiments before Miller's claims. With the name "MM experiment" I mean the kind of experiment that was conceived and realized by Albert A. Michelson in 1881 in Potsdam. He observed a null effect and affirmed that it was in accord with the Stokes' ether theory, that is an ether that was totally dragged by the earth near its surface.[3]

In 1886 Michelson, with the help of Morley, confirmed the result of the Fizeau experiment and, one year later, confirmed the null result of the MM experiment. The conclusion of the 1887 paper was that no ether theory could explain both these experiments.[4]

In 1892, Lorentz found a possible theoretical solution that could account for the null result of the MM experiment. He hypothesized a contraction of the bodies in motion in a stationary ether in a way dependent on their velocity in a stationary ether: the so called Lorentz-Fitzgerald contraction.[5]

In the period between 1902 and 1906 Morley, with the help of his new colleague Miller, repeated the MM experiment in Cleveland and at the Euclid Heights with the aim of discovering if the Lorentz-Fitzgerald contraction was different for various materials. Here again the published conclusion was that these experiments confirmed the null result.[6]

In the same period, Einstein published his famous paper about the so called special relativity theory. This theory is in agreement with the null result of the MM experiment and, above all, Einstein clearly proposed the dismissal of the ether with the sentence: "*Die Einführung eines 'Lichtäthers' wird sich insofern* […] *überflüssig erweisen*".[7]

Between 1907 and 1915 Einstein developed the theory of general relativity, whose final structure was exposed in November 1915, during the First World War. A few months after the end of the War, in November 1919, Eddington claimed that the photos of the stars around the sun, which were made by the British eclipse expedition, confirmed the prediction of general relativity theory. Eddington's announcement was accompanied by an enormous press campaign that gave Einstein and his theory an immediate fame all around the world.[8]

Between 1906 and 1920 Miller became and expert of acoustics and did not repeat the MM experiment. Only after Eddington's announcement Miller decided to repeat the MM experiment since, in his words, "the Theory of Relativity postulates an exact null effect from the ether-drift experiment which had never obtained in fact".[9]

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[2] J. P. Vigier, "Relativistic Interpretation (with non-zero photon mass) of the small ether drift velocity detected by Michelson, Morley, and Miller", *Apeiron*, 4 (1997): 71-76; F. Selleri, "On the Anisotropy Observed by Miller and Kennedy & Thorndike", in M. Duffy, and M. Wegener (eds.) *Recent Advances in Relativity Theory 2: Material Interpretations* (Palm Harbor, Hadronic Press, 2000): 281-83; H. A. Munera, "Michelson-Morley Experiments Revisited: Systematic Errors, Consistency among Different Experiments, and Compatibility with Absolute Space", *Apeiron*, 5 (1998): 37-53; M. Allais, "Des régularités très significatives dans les observations interférométriques de Dayton C. Miller 1925-1926", *Comptes Rendus*, 327 (1999): 1405-10; M. Consoli, and E. Costanzo, "From classical to modern ether-drift experiments. The narrow window for a preferred frame", *Physics Letters A*, 333 (2004): 355-63.

[3] A. A. Michelson, "The Relative Motion of the earth and the Luminiferous Ether", *American Journal of Science*, 22 (1881): 120-29.

[4] A. A. Michelson, and E. W. Morley, "Influence of Motion of the Medium on the Velocity of Light", *American Journal of Science,* 31 (1886): 377-86; A. A. Michelson, and E. W. Morley, "On the Relative Motion of the Earth and the Luminiferous Ether", *American Journal of Science*, 34 (1887): 333-45.

[5] H. A. Lorentz, "De Relative Beweging van der Aarde en den Aether", *Verslagen Koninklijke Akademie van Wetenschappen,* English translation in H. A. Lorentz, "The Relative Motion of the Earth and the Ether", in P. Zeeman, and A. D. Fokker, (eds.) *Collected Papers,* vol. 4 ('s-Gravenhage: Nijhoff**,** 1935-1939): 220-23.

[6] E. W. Morley, and D. C. Miller, "Report of an experiment to detect the Fitzgerald-Lorentz Effect", *Philosophical Magazine*, 9 (1905): 680-85; E. W. Morley, and D. C. Miller, "Final Report on Ether-Drift Experiments", *Science*, 25 (1907): 525.

[7] A. Einstein, "Zur Elektrodynamik bewegter Körper", *Annalen der Physik*, 17 (1905): 891-921, p. 892. The English translation is: "the introduction of a 'light ether' will prove superfluous", in A. Einstein, "On the electrodynamics of moving bodies", in J. Stachel et al. (eds.) *The Collected Papers of Albert Einstein V.2 The Swiss Years: Writings, 1900-1909,* English Translation (Princeton: Princeton University Press, 1989): 140-71, p. 141.

[8] A. S. Eddington, F. W. Dyson, and C. Davidson, "A Determination of the Deflection of Light by the Sun's Gravitational Field, from Observations made at the Total Eclipse of May 29, 1919", *Philosophical Transactions of Royal Society of London Series A*, 220 (1920): 291-333; p. 332.

[9] Miller (note 1), p. 217. Douglas L. Darnell, who was a student of Miller, stated that *"[Miller] said that Einstein had based his theory on incomplete experimental data and that he would have to get busy and complete the aether-drift"*. Darnell to Shankland, 10 April 1963, Cleveland, *Case Western Reserve University* (*CWRU*) Archives, Robert S. Shankland Papers (RSP), A 08-19, box 1.

He performed these new repetitions at the top of Mount Wilson (one thousand seven hundred meters above sea level) thanks to the hospitality of the *Mount Wilson Observatory* (MWO)and its directors G. E. Hale (between 1904 and 1923) and W. S. Adams (1923-46).

After the first observations (April and December 1921) Miller was not sure about the result since the effect he observed was very slight. But in 1925 he eventually announced that his latest observations at Mount Wilson showed a real ether-drift effect in two US scientific meetings: the April meeting of the *National Academy of Sciences* (NAS) and the December joint meeting of the *American Physical Society* (APS) and of the *American Association for the Advancement of Science* (AAAS).

Miller maintained his conviction for all his life and, in 1933, he published a long paper in which he summarized all his works about the MM experiment and repeated his strong previous conclusion: “These observations all show a positive periodic displacement of the interference fringes as of an ether-drift, of the same magnitude, about 10 kilometers per second […]. The effects were shown to be real and systematic, beyond any further question”.[10]

After Miller had announced his positive result many physicists began to plan a repetition of the MM experiment in the USA as well as in Europe with the specific purpose to test Miller’s conclusion. These experiments were performed between 1926 and 1930 and no one showed the same effect observed by Miller. After 1930 the MM experiment was never repeated until the 1950s. In other words, after 1930 the experimental issue was considered a closed question and Miller’s interpretation of his observations was discarded by the scientific community. In this rejection of Miller’s results, the last two experiments played a decisive role: the Michelson-Pease-Pearson experiment performed in the USA between 1926 and 1928 and Joos’s, which was performed in Jena between 1929 and 1930.[11]

Thus, Miller’s data remained an unexplained result until 1954, when Robert S. Shankland (Miller’s successor at the *Case Physics Department* in Cleveland) and three colleagues at the *Case Institute of Technology*, found a reasonable solution based on the temperature variations in the optical paths of Miller’s interferometer.[12]

Historiographical descriptions

The *standard* version of the history of this subject, found in the scientific treatises, is enough simple and coherent. It can be summarized in this way: Miller’s results were discarded because other MM experiments, which were performed with more sensitive instruments, did not find the same effect; after these repetitions the majority of the scientific community thought that they were due to other systematic causes or some sources of error.[13] Moreover, the majority of scientific books that mentioned Miller’s results after Shankland’s re-analysis affirmed that this solution was the correct one.[14]

In addition to this *standard* history we can find other implicit as well as explicit historical accounts of this issue. Interesting implicit histories of Miller’s ether-drift experiments were in some important writings of the philosophers of science. In particular, M. Polanyi, Lakatos and Feyerabend concerned themselves with the reaction of the scientific community to Miller’s claims with opposite judgements. Feyerabend and, in particular, Polanyi underlined the irrationality of the rejection of Miller’s results, whereas Lakatos affirmed that “the tenacity of the Einsteinian research programme in the face of alleged contrary evidence was a completely *rational* phenomenon”.[15]

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[10] Miller (note 1), p. 221.

[11] A. A. Michelson, F. G. Pease, and F. Pearson, “Repetition of the Michelson-Morley Experiment”, *Nature*, 123 (1929): 88; A. A. Michelson, F. G. Pease, and F. Pearson, “Repetition of the Michelson-Morley Experiment”, *Journal of Optical Society of America*, 18 (1929): 181-82; G., Joos, “Die Jenaer Wiederholung des Michelsonversuchs”, *Annalen der Physik,* 7 (1930): 385-407.

[12] R. S. Shankland, S. W. McCuskey, F. C., Leone, and G. Kuerti, “New Analysis of the Interferometer Observations of Dayton C. Miller”, *Reviews of Modern Physics*, 27 (1955): 167-78.

[13] See R. G. W. Brown, and E. R. Pike, “A History of Optical and Optoelectronic Physics in the Twentieth Century”, in L. M. Brown, A. Pais, and B. Pippard, (eds.) (1995) *Twentieth Century Physics Volume III* (Bath: IOP and AIP Press, 1995): 1385-1504. This version is also accepted by Popper in his fundamental book *Logic of Scientific Discovery*. See K. R. Popper, *Logik der Forschung* (Tübingen: J. C. B. Mohr, 1934). The sentences about Miller’s experiments were unchanged in its revised English version K. R. Popper, *The Logic of Scientific Discovery* (1st English ed., London: Hutchinson & Co, 1959).

[14] W. K. H. Panofsky, and M. Phillips, *Classical Electricity and Magnetism* (1st ed. Reading, MA: Addison Wesley, 1955) and (2nd ed., Reading, MA: Addison Wesley, 1962); G. Holton, “Einstein, Michelson and the ‘crucial’ experiment”, *Isis*, 60, (1969): 133-97; M. A. Tonnellat, *Histoire du Principe de Relativité* (Paris: Flammarion éditeur, 1971); A. Pais, *‘Subtle is the Lord…’: The Science and the Life of Albert Einstein* (Oxford: Oxford University Press, 1982).

[15] I. Lakatos, “Falsification and the methodology of scientific research programme”, in I. Lakatos and A. Musgrave, (eds.) *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970): 91-195, p. 163 [emphasis in the original]. The epistemological discussions about this case study of M. Polanyi and Feyerabend are in: M. Polanyi, *The Logic of Liberty: Reflections and Rejoinders* (London: Routledge and Kegan Paul Ltd, 1951); M. Polanyi, *Personal Knowledge, Towards a Post-critical Philosophy* (London:

A different mention of Miller's experiments is in some biographies about Einstein, with a specific, but not deepened, description of Einstein's attitude towards them.[16] In very few textbooks of theoretical physics and optics there were some quotations of Miller's experiments in the paragraphs concerned with the null result of the MM experiment. They reported either the *standard* history or an inaccurate version of the history.[17]

The anti-relativists reacted to the rejection of Miller's results with historical analyses based on the prejudice that the scientific community was dogmatic towards relativity and claimed that Shankland's analysis was incorrect and that his principal aim was to complete a work with the support of Einstein. Thus they stated that the *standard* history is false.[18]

Apart form these implicit histories there are a few historical writings about Miller's experiments. The most acknowledged treatise about the history of Miller's experiments is the book *The Ethereal Ether* by Loyd Swenson, but this writing is often used in the anti-relativistic literature, because it shows some ambiguities and utilizes the 1933 Miller's paper and 1955 Shankland's paper as principal sources without a critical analysis.[19]

Miller's experiments found also a place in the treatises *Understanding Relativity* of S. Goldberg and, in particular, *Einstein's Jury* of J. Crelinsten in the historiographical context of the US reception of relativity during the 1920s.[20] A third interesting account of this subject is in the very thorough analysis of Einstein's attitude towards Miller's experiment made by Klaus Hentschel.[21]

The historiographical studies summarized in this section left us with several questions related to the history of Miller's experiments and of their eventual rejection:

- Why, when and how did the MM experiment end?
- Is it possible to speak of a US scientific community related to this issue? If so, what was the reaction of the US scientific community to Miller's claims? Which role (if any) did sociological features play in this national reaction?
- What was Einstein's attitude towards these experiments?
- Which were the different features of the repetitions of the MM experiment performed in the 1920s?
- Why did Shankland decide to redo the difficult re-analysis of Miller's data? How did Shankland's re-analysis reach a possible solution?
- An interesting question could also be asked in relation with philosophical analyses of this history: Which behaviour could be called rational and which could be called irrational in the history related to Miller's strange results?

I try to answer all these questions in my Ph.D. thesis, but in this communication I will focus the attention on the second point related to the reaction of the US scientific community to Miller's claims thanks to scientific literature and private documents found in several US archives.[22]

Routledge & Kegan, 1958); and P. Feyerabend, *Against Method: Outline of an Anarchistic Theory of Knowledge* (1st ed.: London: New Left Books, 1975).

[16] D. Brian, *Einstein: A Life* (New York: John Wiley and Sons, 1996), p. 127; A. Fölsing, *Albert Einstein: A Biography* (New York: Penguin, 1997), p. 503; W. Isaacson, *Einstein: His Life and His Universe* (New York: Simon & Shuster, 2007), p. 297.

[17] In F. K. Richtmyer, E: H. Kennard, and J. N. Cooper, *Introduction to Modern Physics* (6th New York: McGraw-Hill, 1969), the authors stated (p. 52): "Observations made by the Michelson method at various times of day and at different seasons of the year by physicists in different parts of the world always yielded the same negative result". This description of the MM experiments that did not consider Miller's experiments is present in several textbooks. See, for example, E. F. Taylor, and J. A. Wheeler *Spacetime Physics* (San Francisco: Freem & Co., 1963), p. 14; and H. M. Schwartz, *Introduction to Special Relativity* (New York: McGraw-Hill, 1968), pp. 34-35.

[18] See bibliography in note 2.

[19] L. S. Swenson Jr., *The Ethereal Ether A History of the Michelson-Morley Aether-Drift Experiments, 1880-1930* (Austin/London: University of Texas Press, 1972).

[20] S. Goldberg, *Understanding Relativity: Origins and Impact of a Scientific Revolution* (Oxford: Clarendon Press, 1984); J. Crelinsten, Ei*nstein's Jury: The Race to test Relativity* (Princeton/Oxford: Princeton University Press, 2006).

[21] K. Hentschel, "Einstein's Attitude Towards Experiments: Testing Relativity Theory 1907-1927", *Studies on History and Philosophy of Science*, 23 (1992): 593-624.

[22] R. Lalli, "Esperimenti di ether-drift nel XX secolo. Casi storici a confronto: Effetto Sagnac (Francia, 1913) ed Esperimenti di Miller (USA, 1921-26)" (Ph.D. thesis, Università degli Studi di Milano, 2011).

US reception of Miller's announcements

The 1920s' scientific literature shows that the US scientific community's reception of Miller's results was very different quantitatively as well as qualitatively from that of the European scientific communities. If we asked why, my historical research and other analyses of the US reception of relativity lead to two principal answers:

- In the US scientific community the opinion was diffused that the theory of special relativity was an inductive generalization of the MM experiment.[23] This opinion was due to the epistemological attitudes of the US scientific community as well as to the fact that the experiment was performed in the USA by the first US Nobel prize winner in the sciences. This implied that the MM experiment was an important national scientific discovery for the US scientific community.
- The second reason was a sociological one. Miller was an eminent member of several US scientific societies, the US scientific community regarded him as a skill experimenter and the most important authority in acoustics. In particular, he was the President of the APS when he announced his positive result in 1925. In other words, Miller had an authoritative voice within the US scientific community in the field concerning with the ether-drift experiments.

There are two opposite descriptions of the reaction of the scientists to Miller's public announcement in the 1925 meetings. On the one hand, it is reported that "nobody doubted relativity" and that all scientists thought that "some unknown source of error […] had upset Miller's work".[24] On the other hand, there are at least three pieces of evidence as to a positive response to Miller's announcement: (1) the AAAS awarded Miller with a thousand dollar prize after his lecture on the ether-drift experiments in December 1925; (2) many scientists began to repeat the MM experiment after his announcement in the USA as well as in Europe; (3) the director of *Science Service*, Edwin E. Slosson, wrote to Einstein that Miller's paper "made a deep impression upon the National Academy of Science when it was read [at the April 1925 meeting]".[25]

Thus, it is necessary to analyse the scientific literature of the period and the private documents in order to understand which features this reaction had. By these documents we learn that there were at least three different theoretical responses in the period 1925-26:

A. A strong anti-relativistic reaction, by L. Silberstein, D. C. Miller, C. L. Poor, H. D. Curtis, A. Reuterdahl and others;[26] according to them, Miller's results completely disproved relativity theories.

B. A strong relativistic one by several European theoreticians (A. Einstein, M. Von Laue, M. Planck, Epstein, A. Eddington, H. Thirring, M. Born, Weber, E. Cohn).[27] According to them, Miller's data had nothing to do with an ether-drift and they were very sceptic about the validity of Miller's results.

C. A weak relativistic one by some US relativistic theoreticians who tried to find some modifications of relativity theories that could explain Miller's observations (Page, Sparrow, Swann, Davis, Carmichael).[28]

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[23] See, for example, R. A. Millikan, "Albert Einstein on His Seventieth Birthday", *Reviews of Modern Physics,* 21 (1949): 343-45. The historian of science G. Holton underlined that this way of seeing was common within the US scientific community: Holton (note 14).

[24] C. G. Darwin, "Logic and Probability in Physics", *Philosophy of Science*, 6 (1939), 48-64, p. 51. Darwin's attestations were fundamental for the epistemological analyses of M. Polanyi and Lakatos (note 15); in particular, they assumed a radical meaning in M. Polanyi, *The Logic of Liberty* (note 15), p. 12; and in R. W. Clark, *Einstein, the Life and Times* (London/Sydney/Auckland/Toronto: Hodder and Stoughton, 1973), p. 316. Moreover, this description was very similar to that of Popper (see note 13).

[25] E. E. Slosson to A. Einstein, 26 June 1925, *Collected Papers of Albert Einstein* (*CPAE*) 17 259 courtesy *Einstein Paper Project* (*EPP*), *California Institute of Technology* (*Caltech*) in Pasadena.

[26] See Crelinsten (note 20); it is interesting to underline that historical researches shows that all the physicists who used Miller's experiments against relativity had already shown that they did not appreciate Einstein's theories for other epistemological or metaphysical reasons.

[27] These names appeared in Einstein's correspondence on this issue. M. Von Laue and M. Planck were quoted in Einstein to Lorentz, 22 June 1926, *CPAE* 16 608; P. Ehrenfest expressed his scepticism in the letter Ehrenfest to Einstein, 16 October 1925, *CPAE* 10 110; A. Metz in Metz to Einstein, 8 January 1926, in A. Einstein, *Ouvres Choises 4 - Correspondances Français: Letters Choises et presentées par Michel Biezunski* (Paris: Editions du SEUIL-Editions du CNRS, 1989), p. 212; P. Epstein in Epstein to Einstein, 19 September 1925, Caltech, Papers of Paul Sophus Epstein (*PPE*), fol. 3.34; E. Cohn to Einstein, 11 September 1925, *CPAE* 17 263; Weber to Einstein, 23 December 1925, *CPAE* 23 284; the wife of M. Born wrote to Einstein that Born doubted the reliability of Miller's instrument after his visit to Mount Wilson in Hedi Born to Einstein, 11 April 1926, quoted in Albert Einstein – Hedwig and Max Born, *Scienza e vita, lettere 1916-1955* (Torino, Boringhieri, 1973), p. 107.

[28] W. F. G. Swann, "The Relation of the Restricted to the General Theory of Relativity and the Significance of the Michelson-Morley Experiment", *Science*, 62 (1925): 145-49. L. Page, and M. Sparrow, "Relativity and Miller's Repetition of the Michelson-Morley Experiment", *Physical Review*, 28 (1926): 384-91; Carmichael et al. (1927) *A Debate on the Theory of Relativity* (Chicago/London: Open Court, 1927).

In this short list it is evident that the US reaction was different from the European one in two principal facts: first, the reaction (C) was a peculiarity of the US theoreticians that has no similarity in Europe; second, the anti-relativistic reaction (A) was not so strong in Europe. For example Sir Oliver Lodge believed in the reality of the ether but he accepted the null result of the MM experiment and did not regard Miller's result as a real issue for relativity.[29]

In any way, the most common public attitude in the period 1925-26 was to wait for other experiments performed by other scientists, before giving a conclusive opinion. Einstein agreed on the necessity of ulterior MM experiments and appeared to be sure that they would confirm the null result.

Theoretical and experimental issues

Miller was not a theoretician and he needed the help of someone else in order to calculate the earth's cosmic velocity that could be in accord with his data. In 1925 he affirmed that his data were consistent with an absolute earth's velocity of 200km/s, which was proposed by the MWO astronomer Strömberg in order to explain asymmetries in stellar radial velocities, but the ether-drift showed only a *reduced* velocity of 10km/s at the top of Mount Wilson.[30]

After Miller's April 1925 announcement, Silberstein claimed that relativity theories were disproved and Miller's data could be explained with the Stokes-Planck ether, which was a compressible ether dragged by the earth's surface.[31] Silberstein's proposal was a general justification, but Miller was very hesitant about the effective behaviour of the ether that could explain his observations at Mount Wilson:

- In the April 1925 meeting, Miller claimed that the drag changed with altitude;
- On the contrary, in the December 1925 meeting, he affirmed that the ether could be dragged from the opaque and massive shielding (for example the walls of the laboratory room);
- In the same meeting he also proposed a third possibility, that is a modified Lorentz-Fitzgerald contraction with a stationary ether.

In addition to the confusion about these explanations there was also an unexplainable effect periodic in one turn of the interferometer, and Miller's ether-drift interpretation of his data could not explain a variation of the azimuth of 30 degrees since the algebraic sum of the east-west components of the drift during a sidereal day had to be zero.

Apart from these theoretical problems, in the same period in which Miller announced his finding other US experiments were all in favour of general relativity theory. In fact, in the same April 1925 meeting, Gale reported that the Michelson-Gale experiment showed a result in agreement with general relativity theory as well as the stationary ether theory, but it disproved an ether-dragged theory (the only one that could be used to explain Miller's data).[32] The Michelson-Gale experiment played a relevant role in reducing the significance of Miller's experiments for several reasons, in particular, because it was proposed by Silberstein in order to confute relativity and to prove the Stokes-Planck ether theory, and because its result was contemporary to Miller's one.

Moreover, before Miller's claims, other US tests of general relativity theory were all in agreement with Einstein's prediction: in 1922 Campbell confirmed Eddington's conclusion about the light deflection around the sun, in 1923 St. John changed his opinion about the red-shift of solar spectral line and in 1925 Adams confirmed Eddington's relativistic prediction about the stellar red-shift of Syrius B.[33]

At the beginning of 1926 the situation about Miller's experiments was: Miller's results could be used against relativity theories, but there was not an advanced ether theory that could compete with relativity, because (1) an ether that could

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[29] O. Lodge, "Scientific Worthies: XLIV. – Albert Abraham Michelson", *Nature*, 117 (1926): 1-6; and, in particular, O. Lodge, "On Prof. Miller's Ether Drift Experiment", *Nature,* 117 (1926): 854. In this short paper Lodge exposed his convictions that Miller's effect depended on the variations of temperature in the optical paths. The beliefs that Miller's effect depended on temperature was shared by many physicists, among which Einstein.

[30] G. Strömberg, "Miller's Ether Drift Experiment and Stellar Motions", *Nature*, 117 (1926): 482-83.

[31] L. Silberstein, "D. C. Miller's Recent Experiments, and the Relativity Theory", *Nature*, 115 (1925): 798-99. L. Silberstein, "Ether Drift and the Relativity Theory", *Nature*, 116 (1925): 98. The Stokes-Planck ether hypothesis was never published by Planck, but it was discussed by Lorentz in H. A. Lorentz, "Stokes's theory of aberration in the supposition of a variable density of the aether", *Verslagen Koninklijke Akademie van Wetenschappen* (1899): 443-48; and H. A. Lorentz, *The Theory of Electrons and Its Applications to the Phenomena of Light and Radiant Heat: a course of lectures delivered in Columbia University, New York, In March and April 1906* (2nd ed., Leipzig: B. G. Teubner, 1916).

[32] A. A. Michelson, and H. G. Gale, "The Effect of the Earth Rotation on the Velocity of Light", *The Astrophysical Journal,* 61 (1925): 140-45

[33] See Crelinsten (note 20). For a thorough analysis of St. John's conversion, see K. Hentschel, "The Conversion of St. John: A Case Study on the Interplay of Theory and Experiment", *Studies in Context*, 6 (1993): 137-94.

explain Miller's data was not in accord with other known facts (in particular, the Michelson-Gale experiment and stellar aberration), and (2) Miller's interpretation needed some *ad hoc* hypotheses about the ether-drag. Furthermore, Miller proposed different *ad hoc* hypotheses in different reports: the relation with altitude in April 1925, and an ether dragged by the walls of the laboratory room or a modified Lorentz-Fitzgerald contraction in December 1925.

Other repetitions and Michelson's role

In spite of these theoretical problems, there was a sort of confusion about Miller's results and some institutions planned a repetition of the MM experiment. All these experiments concluded with the confirmation of the null result.

In the USA the Caltech as well as the MWO decided to re-perform the experiment. The first US repetition of the MM experiment was realized by Roy J. Kennedy of the Caltech in September 1926.[34] Kennedy's instrument was very sensitive. In order to eliminate temperature and pressure effects he enclosed the optical system in a sealed metal case containing helium at atmospheric pressure. This procedure was methodologically opposed to Miller's, who attempted to eliminate all the alterations of the ambient condition, which were able to interfere with the detection of the ether-drift effect. In fact, Miller enclosed the optical path with transparent glasses in order to not interfere with the free ether wind. After one year Kennedy's experiment was repeated by K. K. Illingworth with an improved apparatus. His conclusion was that his observations were in sharp disagreement with Miller's; no shift dependent on orientation was observed.[35]

Despite the excellence of their apparatus, the experiments of Kennedy and Illingworth were not sufficient to end the debate about Miller's ether-drift experiments. Furthermore, their names were not quoted very often in scientific literature, unlike Miller, whose observations appeared in a number of textbooks as the most extensive series of measurements of the MM experiment.[36] The reason for the limited impact of the Caltech experiments is not very clear; in any case, it seems that Michelson's work had a larger influence on the US scientific community's eventual rejection of Miller's results.

We can now focus our attention on Michelson's role in the eventual conclusion of the MM experiments in the USA. The director of the MWO, Walter Adams, asked him to redo the MM experiment in 1926. Adams wrote a letter in which he tried to convince Michelson that the repetition of the MM experiment was still more important than the velocity of light measurements. This letter was written after Kennedy's announcement because Michelson regarded Kennedy's experiment as conclusive.[37] In this letter, other private documents and public announcements it clearly appears that Michelson was not very interested in the repetition of the MM experiment for several reasons, but Adams urged him to continue the task since the scientific world wanted an ether-drift experiment performed by the authoritative Michelson. The expressions used by Adams are indicative of relevance of Michelson's work within the US scientific community. In fact, Adams wrote: "I understand that Kennedy got zero effects […] but what the scientific world wants is your final word on the subject".[38]

Thus Michelson, with the help of Pease and Pearson, made some observations between June 1926 and March 1928. At the beginning of 1929 they published two very short reports, in *Nature* and in the *Journal of the Optical Society of America*. There was no data at all and a very brief description of the apparatus was provided. They were almost identical with a slight, but strange, difference in the conclusion: in the *Nature* paper, published in January 1929, the authors affirmed that "the results gave no displacement as great as one fifteenth of that to be expected on the supposition of an effect due to a motion of the solar system of three hundred kilometres per second", whereas in the *JOSA* paper the amount became "one-fiftieth".[39]

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[34] R. J. Kennedy, "A Refinement of the Michelson-Morley Experiment", *Publications of the National Academy of Sciences,* 12 (1926): 621-29.

[35] K. K. Illingworth, "A Repetition of the Michelson-Morley Experiment Using Kennedy's Refinement", *Physical Review*, 30 (1927): 692-96.

[36] See for example F. A. Jenkins, and H. E. White *Fundamentals of Optics* (2nd ed., York/Toronto/London: McGraw-Hill, 1950), p. 398; F. W. Van Name, *Modern Physics: Developments of the Twentieth Century* (New York: Prentice-Hall, 1952), p. 84; F. K. Richtmyer, *Introduction to Modern Physics* (2nd ed., New York: McGraw-Hill, 1934), p. 715. Sometimes Miller's experiments were quoted as experiments that confirmed the negative result. See H. M. Schwartz, *Introduction to Special Relativity* (New York: McGraw-Hill, 1968), p. 35.

[37] There are several pieces of evidence that show Michelson's public opinion about the reliability of Kennedy's experiments. See, for example, Michelson et al. (1928) "Conference on the Michelson-Morley Experiment: Held at the Mount Wilson Observatory Pasadena California, February 4 and 4, 1927", *The Astrophysical Journal*, 63 (1928): 341-402, p. 393.

[38] Adams to Michelson, 24 November, 1926, [emphasis of Adams], *US Naval Academy* (*USNA*), *Nimitz Library Special Collection and Archives Division*, Albert A. Michelson Papers, fol. 1.

[39] A. A. Michelson, F. G. Pease and F. Pearson, "Repetition of the Michelson-Morley Experiment", *Nature*, 123 (1929): 88, and A. A. Michelson, F. G. Pease and F. Pearson, "Repetition of the Michelson-Morley Experiment", *Journal of Optical Society of America*, 18 (1929): 181-82, p. 182.

Another peculiar detail of these two reports was that Michelson and his co-authors opted not to refer directly to Miller's experiments, but to Strömberg's cosmic motion theory, which Miller had used in order to explain his data. This choice generated some confusion because Miller's data agreed with Strömberg's motion only as a result of fundamental modifications, as for example the hypothesis of an ether drag of 19/20. Thus the Michelson-Pease-Pearson data resulted not to be an explicit disproval of Miller's ether-drift of 10km/s. The fact that the authors did not directly refer to Miller's experiments leads us to a question: Why was Michelson so ambiguous in these publications?

It is possible to find some potential explanations of Michelson's behaviour in private correspondence of some scientists involved in Michelson's experiment. The US scientific community firmly trusted Michelson's last word on the MM experiments, but it does not seem that Michelson was really interested in these experiments for several different reasons. First, he had no doubts about the null result of this kind of experiment.[40] Second, he did not appreciate Miller's methodology and he never regarded his observations as valid, in particular he stated that Miller did not change the experimental conditions enough.[41] Third, he believed that Kennedy's results were sufficient to conclude the question once and for all. Lastly, he considered the new light velocity measurements as more important than a new repetition of the MM experiment. Thus, the US scientific community, thanks to Adams, put pressure on him to repeat these experiments against his personal convictions. The US scientific community's need for the authoritative position of Michelson clashed with Michelson's own behaviour who did not make many efforts in this task. It seems that these are the motivations of Michelson's alleged ambiguity, along with his reluctance in attacking Miller's expertise too directly.

Conclusion

After this short account we can try to reach some conclusions about the reaction of the US scientific community to Miller's results in the 1930s. The need for *the final word*, which we have seen in Adams' letter, means that the US scientific community could not discard the ether-drift found by Miller without an authoritative response. Michelson was the only scientist that could play this role, because he was regarded as the most important expert of this kind of experiments. But, in a paradoxical way, his confirmation of the null result was more uncertain and unclear then Kennedy's or Illingworth's. This situation created some confusion about the rejection of Miller's results, even though there were several other rational reasons for it.

This sociological attitude, linked to the importance of the authoritative word, had the consequence that the importance of Miller's experiments was overemphasized in the US scientific community, in fact Miller, as we have seen, never calculated a cosmic motion that could fit all his data. Thus we can discard some anti-relativistic claims, because sociological or irrational reasons had a role more on the excessive importance of Miller's experiments than on their final rejection.

[40] In 1925 Michelson wrote to Hale that: "the 'Ether drift' experiments finished with decidedly negative results – probably zero displacement but certainly less than one thirtieth of that which Miller found". Undated handwritten note found in *Huntington Library*, Walter S. Adams Papers, box 46, fol. Michelson 1925. Even though this note is undated it appears that it was written soon after Miller's April 1925 announcement. It shows that Michelson had no doubts about the null result of the original Michelson-Morley experiment.

[41] Epstein to Einstein, 19 September 1925, Caltech, *PPE*, fol. 3.34.

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THE APPROACHES TO THE CREATION OF TYPOLOGICAL STRUCTURES FOR THE DERIVATION OF LORENTZ TRANSFORMATION

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Abstract

The analysis of the original issues of the approaches to Lorentz transformations derivation witnesses that they had an evolutionary character which was connected with their applications to the different physical systems and mediums possibilities extensions. In this issue, through the analysis of mathematical methods which were applied while deriving the Lorentz transformations, four major directions which dominated in the first part of the 20th century are investigated. We have pointed out four founders of these directions:
- coordinate approach (V. Voight, G. Fitzgerald, J. Larmor, A. Poincaré, H. Lorentz, A. Einstein, I. Kordysch);
- group approach (H. Minkowski, A. Poincaré, V. Varicak);
- tensor approach (Einstein, Ivanickaya);
- matrix approach (H. Minkowski, A. Poincaré).
The main peculiarities are shown of these approaches and their realizations, through examples of European scientists' works.

Classical physics, based on the Newton equations, described the phenomena of causal relations through differential equations. Even the Maxwell equations implicitly implied relativistic conditions and contained the conditions of classical physics abandonment. The way to the complete construction of electrodynamics and the Maxwell equations declassification became possible only after the construction of the special theory of relativity and the consideration of the relativity principle. But it was necessary to review the basic principles of classical physics, space and time.

The periodization of the development of relativistic physics in our history of science was mostly connected with some significant historical stages: the time before the October Revolution and the post October Revolution period. In our view, this approach is not crucial, so while considering the topic, the timing from 1900 to 1924 was selected in order to highlight the first ideas of the special theory of relativity that emerged at the beginning of the twentieth century and had already been taken by the scientific community in the 1920s after the GTR establishment. Also, the intensive development of quantum theory confirmed the fairness of the mass and energy correlation formula, which indirectly confirmed the fairness of ETS. This periodization is also relevant because at the beginning of the 1920s a change of the political system took place in

Ukraine that led to fundamental changes in the structure of science, and became an important factor in the development of scientific research, including physics.

In order to determine STR trends in Ukraine and define its conceptual ideas, we would like to present, in this report, the results of the studies based on comparing the local scientists' works, their approaches to STR and the global trends of the problem analysis.

The equations called the Lorenz equations played an important role in the formation of modern physics. The "birth" of the Lorentz transformations began with a "paradox". It implies the fact that Maxwell's equations which characterize the electromagnetic field are not invariant to Galilee's transformations. Moreover, W. Vogt (Voight) showed in 1887 that the equation of the type φ=0 remains in the transition to new space-time variables by transformations of the type $x'=x-vt$, $y'=y/\gamma$, $z'=z/\gamma$, $t'=t-vx/c^2$. They were practically the future Lorentz transformations up to the scale factor. W. Vogt's work was little known to the scientific community. But J. Fitzgerald and H. Lorenz realized that in order to explain the Michelson-Morley experiments it is necessary to introduce a new postulate (reducing the size of moving bodies). In 1900, ten years after H. Hertz and O. Heaviside gave a beautiful mathematical form to the Maxwell equation, J. Larmor found the transformation in which the equations remained invariant. The method proposed by J. Larmor was reduced to a two-stage scheme: at first it is the Galilean transformation, and then the restoration of the Maxwell equations' invariance through kinematic transformation, which was explained by the author as follows: «The size of moving electrodynamics system declines along the movement direction compared with a system that is at rest with respect to $(1-v^2/c^2)^{-1/2}$». H. Lorentz, regardless J. Larmor, proposed a method of transforming one system framework to another at which the Maxwell equations retain their form. In fact, this scheme was similar to that of J. Larmor's. The transformations, $x'=klx$, $y'=ly$, $z'=lz$, where $k^2 = \frac{c^2}{c^2-\omega^2}$ where ω is the speed of the system as a whole, along the x direction, describe the transition between two different systems that were at rest relatively to aether. Therefore, if there was no electromagnetic field in one system, there was no electromagnetic field in the other. This fact explained through the Lorentz theory the result of the Michelson-Morley experiment.

A. Poincaré improved H. Lorentz's aether-field theory removing the Galilean transformations as a separate stage of transformations and immediately entered the equation as a single transformation. A. Poincaré's theory mathematically corresponds to the STR of A. Einstein, but A. Einstein himself «dared» to give physical meaning to the Lorentz transformations. As H. Lorenz wrote in 1912, «Einstein's merit is that he was the first to demonstrate the principle of relativity as a general strictly and precisely acting law» [refer to 1]. However, despite full understanding of the mathematical side of the issue H. Lorenz could not accept the conclusions that resulted from the kinematic interpretation of STR.

As physical objects and their corresponding geometric images cannot depend on where the system recorded their coordinates, studying approaches to the Lorentz transformations derivations one can introduce the classification based on the principles of physical phenomena mathematical descriptions. Considering the nature of the mathematical transformations of the coordinates of these objects, one can say to which class they belong: vectors or tensors.

Historically, the first was a coordinate method which can be most informatively illustrated through A. Einstein's paper [2]. Considering the proliferation of the light signal in both time and space, the equation type like $\frac{\partial\tau}{\partial x'} + \frac{v}{V^2-v^2}\frac{\partial\tau}{\partial t} = 0$ was analyzed. The distribution of light waves was considered in the terms of the two reference frames (one moved uniformly and rectilinearly; and the other was in the state of rest). Using the light speed invariance postulate, and basing on the equation analysis of light waves propagation in these systems, the Lorentz transformations equations were obtained. Accordingly, in the same work physical values of the obtained equations for moving bodies and clocks were obtained, and a velocity addition theorem was given. The given results were applied to the transformation of Maxwell-Hertz equations in void, the theory of aberration and the Doppler principle, the theory of light pressure, dynamics of accelerated electrons and so forth. As part of this approach, L. Kordysch and A. Hruzintsev made the research in the Ukraine in the early 20th century. It should be noted that their issues were published at the same time (1910-1911). Although the articles were devoted to the same topic, the approaches to the derivation of the Lorentz transformations were *not identical*.

In spite of the fact that Kordysch's issue "Elementary derivation of basic formulas of the theory of relativity"[3] was printed in 1911, it had been reported much earlier (15 February 1910) on the conference in Kiev Polytechnic Institute's scientific and technical society. L. Kordysch did the derivation of the Lorentz transformations in a way that was completely covered by the coordinate method definition. (Under the coordinate method we mean the description of a physical phenomenon which is based on linear transformations of the rectangular coordinate system.) Let us suppose that the light signal was released at the time of coordinate systems I and II with their coordinate origin coincidence. Based on the definition of the relativity principle, both the observer of the reference frame I and the observer of the reference frame II will need to see the second light wave spreading from the centers *A* (for coordinate system I) and *B* (for coordinate system II). System I moves with respect to system II with the speed *v*. The wave propagation sphere equation for observer I is:

$$x^2 + y^2 + z^2 - c^2t^2 = 0 \qquad (1)$$

For observer II the equation is:

$$x'^2 + y'^2 + z'^2 - c^2 t'^2 = 0 \qquad (2)$$

System II clock reading is a function of the coordinates, so *t'* can be represented as:

$$t' = At + Bx + Cy + Dz \qquad (3)$$

where, *x*, *y*, *z* are the clocks coordinates relative to I, and *A*, *B*, *C*, *D* are some constants.

Let us assume that the dependence may be more complicated:

$$t' = At + Bx + Cy + Dz + Kt^2 + Mx^2 + ... + Ltx \qquad (4)$$

Let the time t=0, the coordinates $x=l$, $y=z$=0, the moving clocks reading $t'_1=Bl+Ml^2$. Since after *t* seconds the system will shift to the distance *vt*, the reading of the moving clocks will be as follows:

$$t'_2 = At + B(l + vt) + Kt^2 + Ml^2 + Lt(l + vt) \qquad (5)$$

$$\Delta t = t'_2 - t'_1 = At + Bvt + Kt^2 + M(2vtl + t^2) + Lt(l + vt) \qquad (6)$$

As reading of the clocks cannot depend on their location *l* in the system, the condition *M=L=0* is supposed to be performed.

According to our analysis, L. Kordysch, while deriving the Lorentz transformations, based on the equality of inertial reference frames, and this does not contradict the linear coordinates and time transformation changing one inertial reference frame to another. Under these conditions he determined which coefficients would be zero and which ones were to be identified. After finding zero coefficients L. Kordysch received the following dependencies:

$$x' = A(x - vt)$$
$$y' = My \qquad (7)$$
$$z' = Nz$$

Using the conversion formulas (7) and (3) and considering that (1) must be identical to (2), L. Kordysch determined the coefficients: $A = \dfrac{1}{\sqrt{1 - \dfrac{v^2}{c^2}}}$, $B = \dfrac{v}{c}\sqrt{\dfrac{1}{c^2 - v^2}}$, $M = N = 1$.

This way he obtained the Lorentz transformations:

$$x' = \beta(x - vt) \qquad y' = y$$
$$z' = z \qquad t' = \beta\left(1 - \frac{v}{c^2}x\right)$$

where $\beta = \dfrac{1}{\sqrt{1 - \dfrac{v^2}{c^2}}}$

Let us note that L. Kordysch adhered to the view that A. Einstein's second postulate was redundant, as it was simply a consequence of the first postulate. This way he supported the view of European scientists. He noted in the preface of this work that a similar conclusion was presented by M. Planck in by that time just printed "Lectures on theoretical physics" (no wonder as L. Kordysch was M. Planck's apprentice). V.S. Ihnatovskyy, F. Franck and G. Rothe reached independently the same conclusions about the second postulate as M. Planck and L. Kordysch did. As E. Whittaker remarked: "when the principle of relativity, to which all physical nature is subject to, was recognized, scientists tried to submit it in a form free from any ties with the electromagnetic theory, so that it could be derived from a more or less defined set of credible axioms. [4, p. 72]

Let us also mention that the approach offered by L. Kordysch is easier than A. Einstein's for the derivation of Lorentz transformations, and readily used in modern physics textbooks (Yavorskyy, Dyatlaf Physics course: High School, 1999 - p. 89-90). We can find the indication to the easier derivation (compared to that of A. Einstein's) in the editor's comments on the article translation "On the Electrodynamics of Moving Bodies": "they are derived directly from the easier terms by these formulas ratio $\xi^2+\eta^2+\delta^2-V^2\tau^2=0$ which must lead to the relation $x^2+y^2+z^2-V^2t^2=0$ [5]. L. Kordysch was the first to offer this approach.

Gruzintsev A, well-known for developing and compiling the Hertz electrodynamics of light waves propagation in non-conducting media, was an adherent of the aether theory, developing the perspective of aether as the structure closely connected with the electromagnetic field. Therefore he approached the issues of the theory of relativity from the position of electrodynamics in the article "the Lorentz transformations and the principal of relativity" [6]. His approach is associated, as shown in our analysis, with the coordinate method for the methodological scheme. Unlike L. Kordysch, who ran prior to the Lorentz transformations through the intervals comparison, Gruzintsev A. chose A. Einstein's way and derived the Lorentz transformations considering time in one system against the other as a function of coordinates and time, i.e. $t'=\hat{O}(z,t)$.

The issue begins with the derivation of the Lorentz transformations using the *A* system coordinates and time *x*, *y*, *z*, *t* dependence of the coordinates and time *x'*, *y'*, *z'*, *t'* of the *B* system (which relatively moves with constant velocity) by defining time as a physical factor phenomena observed. Though the approach to this problem in general coincides with that of A. Einstein's, there is a significant difference. The speed of light ω which appears in Gruzintsev A. paper is "speed of light in the medium space is filled with" [6, p.2].

When entering t'_0, t'_1 which is the time of the beam output from *A* system indicated through system *B*; t'_2 is the reflection time from *B* and returning to *A*, Gruzintsev put down the functional dependence of time and broke t'_1, t'_2 after the Taylor method:

$$t'_0=\hat{O}(z,t)$$
$$t'_1=\hat{O}\left(z-u,t+\frac{u}{\omega+v}\right)=\hat{O}(z,t)-\frac{\partial\hat{O}}{\partial z}u+\frac{\partial\hat{O}}{\partial t}\frac{u}{\omega+v}$$
$$t'_2=\hat{O}\left(z,t+\frac{u}{\omega+v}+\frac{u}{\omega-v}\right)=\hat{O}(z,t)+\frac{\partial\hat{O}}{\partial t}\frac{2u\omega}{\omega^2-v^2}$$

The signal in A. Gruzintsev's theory comes from an arbitrary point in space but not from the origin like in A. Einstein's theory. Inserting these values into the formula $t'_1=½(t'_0+t'_2)$, he received the electromagnetic wave equation $\frac{\partial\hat{O}}{\partial u}+\frac{v}{\omega^2-v^2}\frac{\partial\hat{O}}{\partial t}=0$. Further, solving it relating to a Φ function and following A. Einstein's path, A. Gruzintsev found the Lorentz transformations.

Since A. Gruzintsev, as mentioned above, developed the electromagnetic field theory, he was one of the first scientists in Ukraine who applied STR to describe phenomena in the electromagnetic field. The foreword noted that the Lorentz transformations, which were analyzed by A. Einstein, A. Poincaré and H. Minkowski, were considered for the aether only. The aim of A. Gruzintsev's issue was to prove that "H. Lorentz's proposal appears valid for every physical medium which is characterized by the dielectric constant and magnetic permeability coefficient different from unity (for the aether *K*=1, μ=1), moreover, it is valid for mid-absorbing medium (metal)" [6, p.1]. When he was solving this problem he imposed the conditions so that "the speed of light was not the universal constant, i.e. the speed in the aether, for example as A. Einstein supposed, but in general, the speed of light in the examined medium" [6, p.2].

Further, the Lorentz transformations in the form $\frac{\partial U}{\partial t}=k\left(\frac{\partial U}{\partial t'}-v\frac{\partial U}{\partial z'}\right)$; $\frac{\partial U}{\partial z}=k\left(\frac{\partial U}{\partial z'}-\frac{v}{\omega^2}\frac{\partial U}{\partial t'}\right)$; $\frac{\partial U}{\partial x}=\frac{\partial U}{\partial x'}$; $\frac{\partial U}{\partial y}=\frac{\partial U}{\partial y'}$ were applied to the equations of the electromagnetic field in the dielectric as

$$AK\mu\frac{\partial\alpha}{\partial t}=4\pi\left(\frac{\partial g}{\partial z}-\frac{\partial h}{\partial y}\right)$$
$$AK\mu\frac{\partial\beta}{\partial t}=4\pi\left(\frac{\partial h}{\partial x}-\frac{\partial f}{\partial z}\right)$$
$$AK\mu\frac{\partial\gamma}{\partial t}=4\pi\left(\frac{\partial f}{\partial y}-\frac{\partial g}{\partial x}\right)$$
$$\frac{\partial\alpha}{\partial x}+\frac{\partial\beta}{\partial y}+\frac{\partial\gamma}{\partial z}=0$$

where *K* is the dielectric constant, μ is the magnetic permeability, α, β, γ are the components of magnetic force that is calculated per unit of magnetic masses, [6, p.8] *f*, *g*, *h* are the components of the electric displacement. After that

$$\left.\begin{array}{l} AK\mu\dfrac{\partial}{\partial t'}\left(k\alpha+\dfrac{4\pi kv}{AK\mu\omega^2}g\right)=4\pi\left[\dfrac{\partial}{\partial z'}\left(kg+\dfrac{AK\mu kv}{4\pi}\alpha\right)-\dfrac{\partial h}{\partial y'}\right] \\ AK\mu\dfrac{\partial}{\partial t'}\left(k\beta+\dfrac{4\pi kv}{AK\mu\omega^2}f\right)=4\pi\left[\dfrac{\partial h}{\partial z'}-\dfrac{\partial}{\partial z'}\left(kf-\dfrac{AK\mu kv}{4\pi}\beta\right)\right] \end{array}\right\} \text{ was received.}$$

The equation $AK\mu\frac{\partial\gamma}{\partial t}=4\pi\left(\frac{\partial f}{\partial y}-\frac{\partial g}{\partial x}\right)$ from the transformation moved to $AK\mu k\left(\frac{\partial\gamma}{\partial t'}-v\frac{\partial\gamma}{\partial z'}\right)=4\pi\left(\frac{\partial f}{\partial y'}-\frac{\partial g}{\partial x'}\right)$ (*). And the equation $\frac{\partial\alpha}{\partial x}+\frac{\partial\beta}{\partial y}+\frac{\partial\gamma}{\partial z}=0$ moved to $\frac{\partial\alpha}{\partial x'}+\frac{\partial\beta}{\partial y'}+k\left(\frac{\partial\gamma}{\partial z'}-\frac{v}{\omega^2}\frac{\partial\gamma}{\partial t'}\right)=0 \Rightarrow \frac{\partial\gamma}{\partial z'}=\frac{v}{\omega^2}\frac{\partial\gamma}{\partial t'}-\frac{1}{k}\left(\frac{\partial\alpha}{\partial x'}+\frac{\partial\beta}{\partial y'}\right)$, so the scientist put this expression in the equation (*) $AK\mu k\frac{\partial\gamma}{\partial t'}=4\pi\left[\left(\frac{\partial}{\partial y'}\left(kf-\frac{AK\mu kv}{4\pi}\beta\right)-\frac{\partial}{\partial x'}\left(kg+\frac{AK\mu kv}{4\pi}\alpha\right)\right)\right]$ (**), and marked $kf-\frac{AK\mu kv}{4\pi}\beta=Nf'$; $kg+\frac{AK\mu kv}{4\pi}\alpha=Ng'$; $k\alpha+\frac{AK\mu kv}{4\pi}g=N\alpha'$; $k\beta-\frac{AK\mu kv}{4\pi}f=N\beta'$ (***), $h=Nh'$, $\gamma=N\gamma'$ and as N is an arbitrary coefficient, after putting the above marked equations (***) in the equation (**) he obtained:

$$Ak\mu\frac{\partial\alpha'}{\partial t'}=4\pi\left(\frac{\partial g'}{\partial z'}-\frac{\partial h'}{\partial y'}\right)$$

$$Ak\mu\frac{\partial\beta'}{\partial t'}=4\pi\left(\frac{\partial h'}{\partial x'}-\frac{\partial f'}{\partial z'}\right)$$

$$Ak\mu\frac{\partial\gamma'}{\partial t'}=4\pi\left(\frac{\partial f'}{\partial y'}-\frac{\partial g'}{\partial x'}\right)$$

Making the same research for the system of equations which described the electrical component of electromagnetic oscillation, A. Gruzintsev came to the conclusion that the equations system of the electromagnetic field was transformed by the Lorentz transformations in a similar system if the relation $A^2K\mu\omega^2=1$ was valid. If we marked $\omega_0/\omega=n$, we could see that the scientist obtained $K\mu=n^2$, the Maxwell formula, where n "is the index of refraction of our medium". We would like to draw your attention to the fact that A is equal to $1/\omega_0$ in A. Gruzintsev's signs, where ω_0 is "the velocity in the vacuum".

The next step made by the scientist was the consideration of the medium: "there are electrons with charges which move, i.e. the medium has dispersion". Considering the electromagnetic field equations for this case with the contribution dispersion, the scientist received the expression $A^2K\mu\omega^2D_i=1$ where he got "the universal dispersion relation generally accepted at present (…). Thus the Lorentz transformations lead to the important results which are usually derived from the integration of electromagnetic field equations". [6, p.14]. When A. Gruzintsev took into account the conduction currents "in the general sense of the word» for the case of "periodic changes of kinetic medium" and conductivity coefficient C, he received the equity $A^2K\mu\omega^2E=1$. Under the circumstances he derived "Maxwell's formula for conductors (metals) or medium which absorb." [6, p.15]

Actually, A. Gruzintsev, one of the first in the world, proved the invariance of the Maxwell equations for the medium from the Lorentz transformations. This conclusion made by the scientist regardless other researchers was very important in terms of the theory of relativity justification. As he noted "it is clear that the Lorentz transformations for general cases take place only for periodic changes in the electromagnetic field and this is the case for all kinds of optical phenomena and for the most part, if not all, of electrodynamics." [6, p.19]

The Lorenz transformations began their life as those which "allowed" the Maxwell equations to remain invariant. The physical meaning founded there was spread by A. Einstein to the conceptual ideas of space and time. The evolution of the Lorentz transformations' derivation approaches has been finished at this point.

In 1912 N. Umov proved the mathematical meaning of the principle of relativity is the propagation of light wave equation invariance considering the wave process, "without making any preliminary supposition" concerning the law combining x', y', z'. The scientist was the one of the first who in his issue "predicted" the generalized Lorenz equations introduction noting that "whereas x, y, z mean rectangular coordinates, x', y', z' may be curvilinear [7, p. 499]. He found the general transformation. Finding a common transformation for the system $\left.\begin{aligned}\frac{1}{\omega^2}\frac{\partial^2\varphi}{\partial t^2}=\frac{\partial^2\varphi}{\partial x^2}+\frac{\partial^2\varphi}{\partial y^2}+\frac{\partial^2\varphi}{\partial z^2}\\ \frac{1}{\omega^2}\frac{\partial^2\varphi}{\partial t'^2}=\frac{\partial^2\varphi}{\partial x'^2}+\frac{\partial^2\varphi}{\partial y'^2}+\frac{\partial^2\varphi}{\partial z'^2}\end{aligned}\right\}$, which describes the phenomenon characterized by the function $\varphi(x,y,z,t)$ in two isotropic equivalent worlds, he derived the general transformation (for which, as he noted, "introducing the intermediate coordinate" [7, p.498] one could obtain the Lorentz transformations). But M. Umov mentioned that it could simplify the situation by introducing the single coordinate dependence of time. He got the Lorentz transformations as the result of his reasoning.

V. Fock expounded in the issue [8] a strict mathematical proof of the Lorentz transformations' properties. He proved that the requirement of the invariance wave-front equation $\left(\frac{\partial\omega}{\partial x_0}\right)^2-\left\{\left(\frac{\partial\omega}{\partial x_1}\right)^2+\left(\frac{\partial\omega}{\partial x_2}\right)^2+\left(\frac{\partial\omega}{\partial x_3}\right)^2\right\}=0$ (where $\omega(x_0,x_1,x_2,x_3)=0$ the surface equation of wave front), regarding the systems *S* and *S'* did not lead to the Lorentz transformations itself, because it assumed Möbius' transformations. In order to avoid this it is necessary to impose the condition maintaining the straightness and uniformity of movement.

The opportunities for clarification of the Lorentz transformations' derivation using other approaches continued. As we can see, at the beginning the validity of transformations was proved only for the transitions between the two systems moving uniformly and rectilinearly with respect to one another. The process of improving the approaches to the Lorentz transformations derivations (which first were only valid for the transition between two Cartesian systems moving uniformly and rectilinearly with respect to one another) continued.

The formulated four-dimensional concept of space-time gave another approach to the Lorentz transformations' derivation. The key role in this belonged to A. Poincaré, H. Minkowski and W. Varychak [9-12]. Putting away the question of priorities in the special theory of relativity creation, we would like to mention A. Poincaré's contribution. We know that Poincaré's paper which presented the ideas similar to the ideas of Einstein's special relativity came out a few weeks later in a not well-known Italian journal. But before A. Einstein, A. Poincaré, at the congress in Saint-Louis in 1904, put forward the idea that the speed of light could not be exceeded by the other speeds. The scientist discovered the group character of the Lorentz transformations, thus he initiated a new type of symmetry connected with a group of linear space-time transformations and introduced the concept of relativity principle. In an earlier work, A. Poincaré anticipated the special theory of relativity advent paying attention to the need of defining the notion of simultaneity. As U. Frankfurt noted, "In the form in which the Lorentz transformations were recorded before Poincaré, the full covariance equations theory was not achieved. To improve the Lorentz formulas of charge density and speed conversion, Poincaré achieved the total covariance of the equations theory." [1, p. 1927]

In 1910 V. Ignatowsky in [13] issue received the Lorentz transformations avoiding the second postulate of the speed of light constancy but applying the theory of groups. F. Frank and G. Rothe reached the same conclusions in the [14] issue. They noted that "Of all the transformation equations corresponding to the same parametric linear homogeneous groups, there are three types in which the value reduction does not depend on the direction of motion in absolute space. Only one type of them has the effect of the actual length reduction namely the Lorentz transformations (…). In the case of the Lorentz transformations the speed of light in all moving systems in any directions of propagation has the same final value ..." [14, p. 855].

H. Minkowski considered a geometric shape $c^2t^2-x^2-y^2-z^2=1$, where *c* is a positive parameter associated with the group of transformations G_c, where the group of transformations includes a space-time null point shift. Considering the properties of the group in the case of the boundary values of the parameter $c\to\infty$ (Newton approximation), he preferred G_c to G_∞, and further identified the speed of light with this constant *c*.

In 1909 A. Somerfield showed that the composition law for velocities referred to geometry of the imaginary radius sphere. In 1910 V. Varichak on the analogy of the composition law for velocities and intervals addition in Lobachevski geometry plane [10], simplified STR formulas by rewriting them using those for hyperbolic geometry of velocities space: $ct'=-x\cdot shu+ct\cdot chu$, $x'=x\cdot chu-ct\cdot shu$, $y'=y$, $z'=z$. Using 4-dimensional approach proved to be very productive which allowed simplifying considerably the equation and switching to the matrix approach in describing the space-time phenomena. Then the Lorentz transformations can be written as

$$\begin{bmatrix} ct \\ x \\ y \\ z \end{bmatrix} = \begin{bmatrix} \gamma & -\frac{v}{c}\gamma & 0 & 0 \\ -\frac{v}{c}\gamma & \gamma & 0 & 0 \\ 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & 1 \end{bmatrix} \begin{bmatrix} ct' \\ x' \\ y' \\ z' \end{bmatrix}$$

initiating a matrix approach to the Lorentz transformations derivation.

The analysis of the approaches to the Lorentz transformations' derivation suggests that they were of evolutionary type which was associated with the potential of their applications for various physical systems and mediums. This analysis of different issues enabled us to make a classification of the process. Thus we can distinguish three main trends which were dominant when establishing the Lorentz transformations in the period from 1900 to 1924 as follows:

- A coordinate approach (V. Vogt, J. Fitzgerald, J. Larmor, A. Poincaré, H. Lorentz, A. Einstein);
- A group approach (V. Ignatowsky, H. Minkowski, A. W. Poincaré Varichak);
- A matrix approach (A. Einstein).

The coordinate approach being historically the first was generalized through the introduction of 4-dimensional values which enabled to extend the methods of tensor calculation using in the general theory of relativity to the special theory of relativity. Therefore we can see the process of gradual transition from considering the possibilities of the Lorentz transformations for vacuum to spreading them on the processes in mediums which have the properties different from those of empty space. The further attempts to apply these transformations (with certain restrictions) to speed up –the systems with the appropriate search criteria of proximity were undertaken. It is evident that the methods used for the derivation supplementing each other have "inherited" properties. From the perspective of the STR conceptual ideas, each of the above stages has given a new confirmation of the STR principles' validity and testified the STR new opportunities. The Lorentz transformations have become an everyday tool in studying physical systems to which the special theory of relativity can be applied.

References

[1] FRANKFURT U.I. (1968) *Special and general relativity: historical essays*. Moscow: Nauka. - P. 330.

[2] EINSTEIN A. (1905) 'Zur Elektrodynamik bewegter Körper', *Ann. d. Phys*. – № 17. – P. 891–921.

[3] KORDYSH L. (1911) 'Elementary derivation of basic formulas of a relativity theory', *Izvestiya Kievskogo politekhnicheskogo instituta*. – book 1. – p. 43-51.

[4] WHITTAKER E.A. (1951 – 1953) *History of the Theories of Aether and Electricity*. London.: Nelson and Sons. – V.1. – 434 p.; V.2. – 319 p.

[5] EINSTEIN A. (1965) *The collection of scientific works*. – V. 1. – p. 700.

[6] GRUZINTSEV A. (1911) 'Lorentz's transformation laws and relativity', *Soobscheniya HMO*. – s 2. – Kharkiv. –v. 12. – № 6. – p. 269-288.

[7] UMOV N. (1910) 'Uniform output transformations, joint with the principle of relativity' – *Selected works*. – 1953.

[8] FOCK V. (1964) *Theory of Space, Time and Gravitation*. 2nd Revised Edition. – 411p.

[9] POINCARÉ A. (1974) *Selected works*, - V.3. - Moscow: Nauka. – 771p.

[10] VARICAK V. (1911) 'Anwendung der Lobachevskyschen Geometrie in der Relativitatstheorie', *Phys. Zeit*. – P. 93–99.

[11] VARICAK V. (1911) 'Die Relativitatstheorieund die Lobachevskijsche Geometrie', *Phys. Zeit*. – P. 287–293.

[12] MINKOWSKI H. (1908/09) 'Raum und Zeit', *Jahresberichte der Deutschen Mathematiker-Vereinigung*. – P. 75–88.

[13] IGNATOWSKY, W. (1910) 'Der starre Körper und das Relativitätsprinzip', *Annalen der Physik*. – 338 (13). – P. 607–630.

[14] FRANK P., ROTHE H. (1911) 'Über die Transformation der Raumzeitkoordinaten von ruhenden auf bewegte Systeme', *Ann. der Physik*. – Ser. 4. – Vol. 34. – № 5. – P. 825—855.

SCIENCE AND TECHNOLOGY IN THE 20TH CENTURY

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A 'SCIENCE OF EXPORTATION'? INTERNATIONAL SCHOLARSHIP IN THE PROFESSIONALIZATION OF PREHISTORY IN SPAIN (1902-1922)

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Abstract

The evolution of prehistory as a scientific discipline in Spain is marked by the contribution of German and French scholars in the first decades of the 20th century. Their role in the study of the Stone Age, and particularly of prehistoric cave art, was paramount; however they were mostly perceived as 'intruders' by the leading Spanish archaeologists. The latter defined prehistory as a patriotic endeavour and felt threatened by the 'foreign' researchers, whom they accused of stealing Spanish past, both materially and symbolically. In this paper, I intend to explore in which ways the rising Spanish prehistorian community defined itself and established its control on the discipline by constructing the 'foreign archaeologist' as antagonist. In doing so, I aim at throwing some light on the origins of this perception, and its lingering influence on the historiography of Spanish archaeology.

Introduction

Recent historiography has highlighted to what extent the process of construction of scientific disciplines has been informed by opposing narratives on the international dimension of science on the one hand and the role of scholars in the nation-state building process on the other. The consolidation of prehistoric archaeology in Spain in the first decades of the 20th century provides an excellent example of the kind of tensions created by those apparently divergent forces. Several European scholars, French and German in particular, contributed largely to the field of Stone Age, and notably to the study of prehistoric cave art. Nevertheless, or perhaps as a result of their prominent role, they were perceived as 'intruders' by Spanish scholars, and particularly by those who were leading the institutionalisation of prehistory in those years.

In what follows, I explore to what extent the presence of those scholars was utilized by the Spanish scholars to demand the establishment of institutions for the practice of prehistory and the protection of archaeological sites and artefacts, stressing their significance as elements of national heritage. In so doing, this paper analyses a historiographical construct, the alleged 'imperialist archaeology' done by those scholars in Spain, in the light of the process of professionalization, as well the instrumental use of nationalist narratives by scientists for the purpose of promoting social and official recognition for their work.

International scholarship: the recognition of prehistoric cave art

1902 marks a turning point in the development of prehistoric studies in Europe. On that date, one of the consolidated voices in the field, Émile Cartailhac (1845–1922) recognised the authenticity of the figurative representations of the cave of Altamira (Santander, Spain). This event inaugurated a new research field, the study of *prehistoric art*, but its significance is enhanced by the fact that Cartailhac was precisely one of the scholars that had denied the authenticity of those representations when the cave was first discovered in 1879. This rejection, supported by the largest sector of the international (and Spanish) scientific community, can be explained by a combination of scientific, philosophical and social factors, in which the fact that the discoverer of the cave, Marcelino Sanz de Sautuola (1831–1888), came from the periphery of prehistoric field, also played a role (Moro and González 2005).

Furthermore, the defence of the paintings was championed by the Spanish geologist Juan de Vilanova (1821–1893); in line with his Catholic beliefs and creationist standpoint, he considered those representations as the first examples of art produced by men, and therefore the proof that humanity was created by God with full intellectual and spiritual capacities. This idea was contested by some leading French prehistorians, under the influence of Gabriel de Mortillet (1821–1898). According to the latter, prehistoric humans, as modern 'primitive peoples' were incapable of high art, which confirmed his strict evolutionist position. In the hands of de Mortillet and those who agreed with him in matters of science and politics, prehistory became a weapon of social combat, as it was brandished in an attempt to reduce the influence of religious ideas in society and advance the cause of materialist philosophy (Hammond 1980; Richard 2008).

The change in the opinion expressed by Cartailhac after the turn of the century is grounded in the redefinition of prehistory's social and scientific credentials, and the shift in its general epistemological paradigm. On the one hand, new approaches in the fields of anthropology and history of religion led to a reappraisal of the intellectual and symbolic capacities of primitive (and prehistoric) populations. On the other hand, the uni–linear and teleological evolutionist paradigm that had dominated the field of prehistory in the 19th century was substituted by the cultural-history approach, which allowed assessing the creative forces of particular human groups and therefore the regional variations in archaeological record (Kaeser 2006). Finally, in the last decades of the 19th century, prehistory was gradually defused from its revolutionary potential; in trying to avoid what they considered partisan uses of knowledge, a new generation of scholars, amongst them some Catholic priests, strove to transform prehistory into a 'neutral' scientific field (Defrance-Jublot 2005).

As a result, after 1902, prehistoric cave art became one of the most promising fields within the prehistoric discipline, for it allowed the exploration of the mental, social and spiritual skills of the prehistoric populations. Cartailhac and the young Henri Breuil (1877–1961) channelled the interest in natural sciences of Prince Albert I of Monaco (1848–1922), who in 1910 created the *Institute de Paléontologie Humaine* (Institute of Human Palaeontology; IPH) in Paris. The new research centre was linked to the *Muséum Nationale d'Histoire Naturelle* (National Museum of Natural History; MNHN), from whence came its director, Marcelin Boule (1861–1942). The IPH enrolled as professors two rising scholars in the field of prehistory, the French Henri Breuil and the German Hugo Obermaier (1877–1946), both trained naturalists, and both Catholic priests.

The new institution aimed at studying the origins of humanity and more particularly prehistoric cave art, and from the very beginning both professors set out to explore the Spanish territory. Whilst Breuil travelled all over the Iberian Peninsula in search of new art stations (Ripoll 1994), Obermaier's activities concentrated on the excavation of El Castillo cave (Santander, Spain), considered as one of the best Palaeolithic sites of Europe, due to its very complete stratigraphic sequence. For this reason, this site became, in economic and scientific terms, the IPH's largest project in its first years, and it attracted a large number of international scholars, who visited the site or collaborated on the digging seasons. Moreover, the work led by Obermaier in El Castillo contributed to the methodological definition of stratigraphic excavation as the prehistorians' method *par excellence,* and the measure of its professionalism (Lanzarote 2011).

Even if the IPH reputed itself as an international institution, it was also deeply rooted in French scientific structures. Indeed its creation implied a decisive move towards the institutionalisation of prehistory and its professionalization, done by means of centralisation in Parisian institutions. In this sense, the IPH opposed the large community of French amateur archaeologists, scattered in the whole country and organised since 1904 in the *Société Préhistorique Française* (French Prehistoric Society; SPF). The excavation activities of a German-speaking Swiss scholar, Otto Hauser (1874–1932) in the prehistoric sites of the Dordogne (France), and the selling of human fossils and prehistoric artefacts, including artworks, to German museums, confirmed the division between those two ways of conceiving prehistoric research. Alarmed by what they considered a looting of the 'underground national archives', French 'official' scholars (Breuil, Boule, Cartailhac) asked their government for a law on excavations which would curtail the 'freedom of excavation' that the amateurs of the SPF proclaimed. Even if the law failed, the claims of those 'official' scholars to control the field were satisfied, at least to some extent, with the creation of the IPH (Hurel 2007).

Colonial sensibilities: counteracting 'foreign science' in Spain

Since their first missions in Spain, the professors of the IPH encountered a country immersed in an intense process of renovation of its scientific and cultural structures, influenced by the consequences of the 1898 colonial crisis. After the military defeat in the Spanish-American War and the subsequent loss of the colonies, Spanish intellectuals had insistently demanded an official effort to redraft scientific policy, oriented towards the regeneration of the country. In this wave of renovation the idea of 'Europeanising' Spain became central, implying the emulation of scientific achievements of the other European nations, but also their colonial potential. This policy led to the reform of the university system and to the creation of new research institutions, such as the *Junta para la Ampliación de Estudios* (Board for the Extension of Studies; JAE) in 1907, which promoted academic exchange by awarding scholarships to study abroad to both students and professors.

Similar goals informed the establishment of the first institution devoted to prehistoric research in Spain, the *Comisión de Investigaciones Prehistóricas y Paleontológicas* (Commission of Prehistoric and Paleontological Research; CIPP). It was created in Madrid in 1912 under the initiative of Eduardo Hernández-Pacheco (1872–1965), chair of geology at the University of Madrid, who had just benefited from a JAE scholarship to visit the MNHN in Paris, at the precise moment in which the IPH started its activities. Upon his return, he proposed to the JAE the creation of an institution within the *Museo Nacional de Ciencias Naturales* (National Museum of Natural Sciences; MNCN), devoted to the geological and prehistoric research in caves and sites. The CIPP mirrored the Parisian *Institut* in its organisation and its research objectives concerned precisely what had attracted the IPH researchers to Spain: the study of archaeological sites and cave art from the Quaternary (Rasilla 2004).

A 'grand amateur' of archaeology, Enrique de Aguilera y Gamboa, marquis de Cerralbo (1845–1922) was chosen as the president of the new institution. Cerralbo was also a political figure, being the leader of the Traditionalist party. In fact, the marquis was welcomed by the JAE, which was accused of being ideologically dominated by progressive politicians, as his participation evinced that the JAE's scientific and patriotic goals went beyond concrete political choices. Finally, Cerralbo attracted to the new institution his protégé, Juan Cabré (1882–1947), a trained artist and archaeologist, who had collaborated with Breuil in the study of the Peninsula's cave art since 1909.

Moreover, Cerralbo using his position as Senator, had contributed largely to the drafting of the *Ley de Excavaciones Arqueológicas* (Law on Archaeological Excavations), passed in 1911. This legal text defined a protectionist framework for the practice of archaeology, and limited the right of non-Spanish citizens to become owners of the discoveries or to export them. In this manner, the law was a crucial step in the definition of prehistoric and paleontological remains as Spanish 'national heritage'. In order to regulate archaeological practice, the law created the *Junta Central de Excavaciones Arqueológicas* (Central Board of Archaeological Excavations), the administrative body entitled to award permits of excavation and to allocate funds (Rasilla and Santamaría 2006). Designated vice-president of this *Junta*, Cerralbo established himself as the guardian of Spanish archaeology and agent of institutionalisation, to which he contributed using his political power and his aristocratic networks.

The passing of the Law on excavations in 1911 and the creation of the CIPP in 1912 are the expression of a desire to regulate archaeological activity and to create scientific structures for its development. Furthermore, those men (Cerralbo, Hernández-Paheco) justified their existence as a defensive effort against 'foreign science'. In this way, Spanish prehistorians used the rhetoric of nationalism to legitimate their discipline in the eyes of society and to consolidate their academic position vis-à-vis the archaeologists that came from abroad. This strategy paid off in a context of hypersensitivity towards what some called 'scientific imperialism', in the aftermath of the colonial crisis, as it had also worked in the French case with the Hauser affaire, in the context of rising anti-Germanic feelings. Singling out 'foreigners' as the antagonist allowed the creation of a group identity, surpassing the social or ideological differences between those who constituted the emerging prehistorian community in Spain, whether noblemen or commoners, official scholars or amateurs, conservative or liberal[1].

For this reason, the activities of the IPH were perceived by Spanish scholars with a mix of admiration (for its methodology and economic means) and mistrust (for their leading role at an international level). In the beginning the relationship between IPH and CIPP was cordial, and there were some attempts to establish formal links between them, but the initiative did not lead to concrete results. Lacking collaborative initiatives, the confrontation finally came about as a dispute between Breuil and Cabré. In August 1913, after a mission in Andalusia, Breuil was informed that Cabré had bribed one of his prospectors, so that he would be informed before the French abbot of the discovery of any new decorated cave (Díaz-Andreu 2000). Breuil's rage was accentuated when he found out that Cabré was preparing a book for the CIPP, *El Arte Rupestre en España* (Cabré, 1915) on a topic that Breuil considered his own scientific preserve (Breuil, 1916). The scientific pride of being the first publisher of new archaeological discoveries, a matter that affected personal ambitions and scientific agendas alike, poisoned the relations between those scholars.

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[1] These issues are considered in detail in my PhD thesis: *Prehistoria Patria. The construction of prehistoric archaeology in Spain between nationalism and Europeanisation (1900-1936)*, prepared under the supervision of Prof. Antonella Romano, at the European University Institute in Florence.

National identities: Spain as the 'world museum of prehistoric art'

The breach between the institutions deepened in the context of the First World War (1914–1918). Due to the hostilities, archaeological activities by most international scholars, and particularly those of the IPH, ceased or were severely reduced: Breuil was drafted into the French army, and had to combine his work as prehistorian with other activities such as propaganda activities in Spain in favour of the Allies. In turn, Obermaier, a German citizen working in a French institution, lost his position. When the war broke out in summer 1914, he was digging in El Castillo; invited to join the CIPP by Hernández-Pacheco, from then onwards his career developed within Spanish research structures. The War provided neutral Spain with a chance to catch up with Europe in scientific terms; as a result, Spanish prehistorians could affirm their leadership in the discipline, while fostering the definitive shaping of prehistory as a patriotic discipline.

Speaking in front of the *Asociación Española para el Progreso de las Ciencias* (Spanish Association for the Advancement of Sciences) in 1915, Hernández-Pacheco exposed the results of the last years' effort to investigate 'Spanish prehistory and palaeontology'. In his intervention, Hernández-Pacheco established a narrative in which the shortcomings of the 19th century were contrasted against the 're-birth of national science' in the 20th, and claimed a protagonist role for Spain in the field of prehistory on account of the fact that it 'constitutes the world's museum of prehistoric art'. He did not miss the opportunity to remind that the artefacts discovered in the excavations led by the IPH had been taken to Paris, and accused its researchers of conquering 'the Peninsula' for the benefit of French Science by the physical and intellectual appropriation of its national past. He concluded on a somehow positive note, affirming that the presence of 'foreign researchers' had triggered a reaction and that 'the archive of the primitive civilisations, which fortunately for Spanish science, belongs to our homeland, and which, as Spaniards and cultivated people, we ought to preserve and study'[2].

Those ideas were staged at the *Exposición de arte prehistórico español* (Exhibition of Spanish Prehistoric Art) organised in 1921 by the CIPP, along with the aristocratic members of the *Sociedad Española de Amigos del Arte* (Spanish Society of Friends of Art). Organised in Madrid and inaugurated by King Alphonse XIII, the exhibition featured sketches of cave art which were loaned by both institutions, even if the role of the IPH was barely acknowledged. In line with the narrative developed by Hernández-Pacheco, the Exhibition served as a showcase of the work done by Spanish prehistorians, and it presented cave art as the first chapter of the Spanish art tradition, placing the Peninsula at the cultural origins of Western civilisation. Moreover, the exhibition paid tribute to the Spanish 'pioneers' that contributed to the discovery of prehistoric art in the 19th century, Sanz de Sautuola and Vilanova (Hernández-Pacheco 1921). Finally, the display was inspired by a long running history of national unity, and avoided dealing with sensitive issues such as the ongoing scientific debate on the ethnic composition of the Peninsula in prehistoric times, which informed two opposing interpretations on the chronology of the so-called Levantine cave art.

The collaboration of those two institutions at the Exhibition was possible due to the mediation of Obermaier. As a member of the CIPP, he had researched intensively on the geology and prehistory of the Iberian Peninsula during the years of the War. In his book *El Hombre Fósil* (1916), he drew on his previous knowledge and experience in Central Europe and France, to insert the prehistory of Spain into that of the Continent. Even if his relations with Hernández-Pacheco deteriorated to the point that he quit the CIPP, by the end of the First World War, Obermaier had developed close ties with aristocratic patrons and members of the Spanish intelligentsia, particularly those who admired the achievements of German science. Finally, in 1922 he became the first chair of prehistory in Spain at the University of Madrid.

His designation was not achieved without resistance, notably by Hernández-Pacheco, who impeded, from his chair in the Faculty of Sciences, the creation of a new position in this faculty. Finally, Obermaier's chair was established in the Faculty of Humanities, under the name *Historia Primitiva del Hombre* (Primitive history of man), confirming the disciplinary shift of prehistory from natural sciences to humanities. Commenting on his recent appointment, Obermaier wrote to Breuil that the JAE had organised a counter-course and accused the anthropologist Manuel Antón (1849–1929) of affirming: 'foreign prehistorians have built in Spain a science of exportation rather than imported by foreigners'[3]. Ironically, it was him, a 'foreign researcher' who had indeed smuggled archaeological artefacts out of the country[4], who became the first university

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[2] "(…) es el archivo de las primitivas civilizaciones, el cual, para suerte de la Ciencia española, poseemos en nuestra Patria y que, como españoles y pueblo culto, estamos en deber de conservar y estudiar". Hernández-Pacheco 1921: 149.

[3] 'los estudiosos prehistóricos han construido en España más una ciencia de exportación que importada por extranjeros', Letter. H. Obermaier to H. Breuil. Madrid. 30 January 1921. Paris, Muséum Nationale d'Histoire Naturelle, Archive Centrale. Fonds Breuil (Br. 38).

[4] In 1912 and 1913 Obermaier sold a collection of artefacts from El Castillo to the American Museum of Natural History in New York. Obermaier acted in this way not just by hiding from the Spanish authorities, who had just implemented new heritage legislation, but also from the Parisian IPH, which asked for exclusivity of research results. The secrecy of those transactions meant that those materials have been kept in storage, unknown by most specialists until recently. See: White 2006.

professor of prehistory in the country. Obermaier acquired Spanish citizenship in 1926, and became the authority on Peninsula's Stone Age at home and abroad until 1936, when the Spanish Civil War again forced him into exile.

Conclusions

The process of professionalization of prehistory in Europe in the first decades of the 20th century resulted from a complex interplay of scientific, social and economic interests, and was inspired by both the internationalism of the academic world and the imperatives of the nation-state building process. For this reason, the alleged 'scientific colonialism' performed by European scholars in Spain seems like a narrative construct of those Spanish prehistorians who strove to consolidate their field by means of legal regulations, institutions and the conversion of archaeological artefacts into 'national heritage' items. Furthermore, those scholars transformed prehistoric cave art into a mighty element for national definition; by so doing, they contributed to a historiography that praised painting tradition as one of Spain's most relevant contributions to Western civilisation, which compensated for what was perceived of as a less decisive contribution in scientific or philosophical terms.

Going beyond the historiography of the alleged Spanish scientific backwardness, the presence of those international scholars can be better grasped as a consequence of the intensification of the country's opening to international influences, the much desired 'Europeanization' of national science. In this context, 'foreign scholars' were perceived as both, eminent scholars to emulate (and to attract as in the case of Obermaier), and competitors to counteract (as for Breuil). The tensions provoked by their presence can be understood as resulting from the strategy of a self-proclaimed national community of prehistorians to establish their control on the discipline. As opposed to the French case, where amateur archaeologists managed to wrestle official ones and hampered the implementation of a law on excavations, the professionalization of archaeology in Spain was driven by official initiative, in which scholars from different social backgrounds collaborated in the name of patriotic regeneration.

Bibliography[5]

BREUIL, H. (1916). "Algunas observaciones acerca de la obra de D. Juan Cabré, titulada El arte rupestre en España", *Boletín de la Real Sociedad Española de Historia Natural*, 16, pp. 253-269.

CABRÉ AGUILÓ, J. (1915). *El Arte rupestre en España: regiones septentrional y oriental* Memorias de la Comisión de Investigaciones Paleontológicas y Prehistóricas, 1, Madrid, Junta para la Ampliación de Estudios.

CARTAILHAC, É. (1902). "Les cavernes ornées de dessins. La grotte d'Altamira, Espagne. *Mea culpa* d'un sceptique", *L'Anthropologie,* 13, pp. 348-354.

DEFRANCE-JUBLOT, F. (2005). "Question laïque et légitimité scientifique en préhistoire: La revue *L'Anthropologie* (1890-1910)", *Vingtième Siècle. Revue d'histoire*, 87 (3), pp. 73-84.

DÍAZ-ANDREU, M. (2000). "La arqueología imperialista en España: extranjeros vs. españoles en el estudio del arte prehistórico de principios del siglo XX". In: *3º Congresso de Arqueología Peninsular: UTAD, Vila Real, Portugal, setembro de 1999*, t. 1, pp. 367-382.

HAMMOND, M. (1980). "Anthropology as a weapon of social combat in late-nineteenth-century France", *Journal of the History of the Behavioral Sciences*, 16 (2), pp. 118-132.

HERNÁNDEZ-PACHECO, E. (1915). "Estado Actual de las Investigaciones en España respecto a la paleontología y prehistoria", *Actas del V Congreso de la Asociación para el Progreso de las Ciencias*, Valladolid, 1915. Madrid, pp. 1-60.

HERNÁNDEZ-PACHECO, E. (1921). "Exposición de Arte Prehistórico Español", *Arte Español,* V (7), pp. 315-339.

HUREL, A. (2007). *La France préhistorienne de 1789 à 1941*, Paris, CNRS Éditions.

KAESER, M.-A. (2006) "The First Establishment of Prehistoric Science: The shortcomings of Autonomy". In: CALLMER, J. et al., *Die Anfänge der ur- und frühgeschichtlichen Archäologie als akademisches Fach (1890-1930) im europäischen Vergleich*, pp. 149-160.

[5] Due to the length limitation of this paper, the bibliography refers exclusively to those works directly related to the topic.

LANZAROTE GUIRAL, José María (2011). “La stratigraphie d’une vie consacrée à la préhistoire. L’abbé Hugo Obermaier, sa chaire de l’IPH et les fouilles du Castillo”. In: A. HUREL (Ed.), *Livre du centenaire de l’Institut de Paléontologie Humaine*, Paris (forthcoming).

MORO ABADÍA, O. and M. R. GONZÁLEZ MORALES (2005). “Presente-pasado. Definición y usos de una categoría historiográfica en historia de la ciencia: El arte prehistórico como paradigm”, *Complutum*, 16, pp. 59-72.

OBERMAIER, H. (1916). *El Hombre Fósil*. Memorias de la Comisión de Investigaciones Paleontológicas y Prehistóricas, 9. Madrid, Junta para la Ampliación de Estudios.

RASILLA VIVES, M. d. l. (2004). “La Comisión de Investigaciones Paleontológicas y Prehistóricas (1912-1939): algunas consideraciones sobre su andadura y su economía”, *Zona arqueológica*, 4 (4), pp. 402-407.

RASILLA VIVES, M. d. l., Santamaría Álvarez D. (2006). “La institucionalización de la arqueología prehistórica en España durante el primer tercio del siglo XX”, *Revista de historiografía*, 5, pp. 112-133.

RICHARD, N. (2008). *Inventer la Préhistoire. Les débuts de l'archéologie préhistorique en France*, Paris, Vuibert.

RIPOLL PERELLÓ, E. (1994). *El abate Henri Breuil (1877-1961)*, Madrid, Universidad Nacional de Educación a Distancia.

WHITE, R. (2006). “From Puente Viesgo to Central Park West: Hugo Obermaier, Nels Nelson and the American Museum of Natural History’s collections from Cantabrian Spain”, *Zona arqueológica,* 7, pp. 59-76.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 1116.

WAYS OF DOING CHEMICAL PRODUCTION AT CUF, "COMPANHIA UNIÃO FABRIL" (1909-1972)

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Abstract

When, in 1906, the Portuguese chemical enterprise Companhia União Fabril *decided to establish its Barreiro works for a European-sized competitive production of phosphate fertilizers, the use of Portuguese pyrites was immediately selected as the S-source for the required sulphuric acid. Another important feature marks the chemical production at Barreiro, which is that from 1909 to 1950 all the sulphuric acid produced was "chamber acid", obtained by the classical lead chamber process.*
With the approach of the end of World War II, there was a need to equate urgent technological reforms for the development of production of acid chemicals (sulphuric, hydrochloric, phosphoric), metals (cooper, lead, gold and silver) and fertilizers (phosphate and nitrogenous). Consequently, by the beginning of the 1950s several important facts happened in CUF, but we stress two among others: the adoption of the contact technology and the implementation of organizational and structural reforms.
Progressing from the former industrial model where, with a simple structure, hierarchically made a centralized command, CUF introduced a dynamic where the technical performance of its employees was not desirable, but also inevitable. In the industrial frame, several Study Centres were introduced, where engineers carried out the specialized technical work to production activities, and a Documentation Centre was also created.
The work which is being proposed aims at understanding the process of CUF's technology adoption between the beginning of sulphuric acid production (1909) and the erection of the first contact plant with fluidization technology (1972), not only in terms of the technologic consumption concept, always present, but also in terms of development of its own technological capacities (Barreiro, chemical production) related with the structures and dynamics established in the meantime.

Introduction

The history of CUF, *Companhia União Fabril*, begins in the second half of the 19th century when, in 1865, was set up the corporation which was responsible for operating the manufacturing plants originally owned by an important businessman in Lisbon trading centre, the Viscount of Junqueira. The chemical production at CUF has its origin there as well. The manufacturing plants concerned produced vegetable oils, soaps and candles (fig.1). It is assumed that the essentially solid residual cake, unintentionally obtained from raw-material pressing, was given a commercial destination already at that time. A few years later, it was apparent that such utilization was made as the material was advertised as organic fertilizers and feed for livestock.

Fig. 1: General scheme of candles, oils and soap integrated productions.
19th century CUF factories.

The currently existing knowledge of the manufacturing procedures and the technical policy used in the initial period of CUF, that is to say, from 1865 until the time when primary inorganic chemistry-based production started (Barreiro, 1909), is still limited. Nevertheless, it is possible to say that it was on the productive pattern beginning at CUF with the 20th century that the bases were laid which enabled it to follow a technological development consistent with the factor of modernity.

Thus, the following text explores those facts linked with the history of the great chemical-industrial manufacturer that CUF became, which are more directly related with that progressive evolution, where the expansion and diversification of the manufactured products were, among several others, determinant factors for the corporate overall growth. Some aspects will be brought into focus, namely those connected with the corporate industrial policy, the structural and organizational reforms performed, the production and dissemination of technical knowledge, the connection with schools, etc.

The chemical-industrial CUF in the first half of the 20th century

Ever since the great turning point, which consisted on building up the manufacturing structures at Barreiro, with which CUF started the primary inorganic chemistry-based production, that a major technology adoption and adjustment style was also marked at this corporation. It is in this initial period of the 20th century, of establishing manufacturing units for the production of acids (sulphuric and hydrochloric acids), metals (copper) and chemicals (sodium sulphate, copper sulphate) that the direct intervention of the "foreign entity" in CUF technology adoption process is expressed in an unambiguous manner. And although he wasn't the only non national technical expert who participated in the construction of Barreiro cluster, it is a fact that the greatest responsibility for its implementation is assigned to the French advisor A. L. Stinville (Auguste Lucien Stinville).[1]

In addition to Stinville, in the primary inorganic chemistry sector, there were further foreign technical consulting services, also provided by persons of recognized proficiency in the corporate working area under development. People who diversified their functions thanks to the wide range of their know-how and of their personal experience in contact with companies and institutions, and who brought to Barreiro site the inflow of technical and scientific knowledge from abroad,

[1] For detailed information about Stinville see SILVA, José Miguel; YOLLANT (2008) – Introducing A. L. Stinville (1868-1949). *In* José Ramón Bertomeu-Sánchez, Duncan Thorburn Burns, Brigitte Van Tiggelen (eds.) – *Proceedings of the 6th International Conference on the History of Chemistry "Neighbours and territories: the evolving identity of Chemistry"*. Louvain-la-neuve, Mémosciences asbl.

thereby being more or less successful. By the end of the decade of the forties - early years of the fifties of the 20th century, a process of profound reorganization of the corporation was in progress. CUF began then to enlarge its technical staff by engaging the personnel necessary for the structures that were in progress and which indicated not only an increasing complexity in the organization, but also a greater critical performance capacity on the technological system.

Fig. 2: Auguste Lucien Stinville. Portrait in "50 Anos da CUF no Barreiro", 1958.

The first technical staff people hired in this 1st stage of the enlargement, essentially carried out for Barreiro manufacturing plants, in the years between the end of the Second World War and the beginning of the fifties, witnessed an industrial dynamics at CUF based on the control made by foremen responsible for the progress of important sectors of the industrial complex, sometimes with some technical qualifications, but who essentially had a consistent practical and empirical knowledge of same.

Besides this new handful of men, mostly engineers assigned to be service heads in Barreiro industrial complex, another and still more reduced senior group, ranking above them in length of service at the corporation, carried out the superior, technical, commercial and industrial management of same as from the headquarters in Lisbon.

Among this group, the engineers Eduardo Madaíl and João da Rocha e Melo enabled for several years, through the Foreign Section and in the so-called 4th Division (Manufacturing Services), one of the two different and historically recognizable ways of accomplishing the technological development at CUF: namely that one, already traditional in the corporation, that was centred on the pattern of the foreign advisor, the intermediary who ensured the dialogue between the two worlds, the world of CUF and that of the available technological knowledge of foreign origin.

Fig. 3: Eduardo Madaíl. Portrait in "50 Anos da CUF no Barreiro", 1958.

Fig. 4: João da Rocha e Melo. Portrait in "50 Anos da CUF no Barreiro", 1958.

This system that articulated the different parties involved in this way worked whenever it was necessary to assess solutions for new problems that interrupted the everyday work of the plants, usually those arising from expansionary industrial policies, such as the increase in production capacity, the new plants and even issues linked with equipment change and operation. Gradually, however, as technological development became more and more urgent, difficulties associated with the adoption of technology, as those related with customs procedures, were made worse in the war context. Moreover, other important problems came about, such as the sudden death of the English advisor Percy Parrish, the disturbance caused in the dialogue with foreign companies resulting thereof and the problems caused by his "transfer of powers". Delays in equipment shipment and in fulfilling purchase orders and even failure to observe established deadlines, as well as troubles in sending information concerning the ongoing projects, among other facts, induced a great desire in the technical staff of CUF - Barreiro in the forties-fifties to attenuate the encumbrance of this systemic dependence.

It has already been explained in another document[2], through the presentation of a "case study", how the technical "staff" of CUF at Barreiro initially dealt with the countless difficulties that hindered the setting up of a mechanical furnace for sodium sulphate, the production of which urged to be increased at CUF in order to respond to the new consumer market requirements, due to the recent expansion of the country's paper pulp industry. That study upheld the importance of the meanwhile created structure at Barreiro industrial complex, of production and maintenance technical services, with their intermediate management positions held by a new generation of licentiates, whose academic degree was given by the *Instituto Superior Técnico* (Higher Technology Institute), the *Faculdade de Engenharia do Porto* (Oporto Engineering Faculty) or by the *Instituto Industrial de Lisboa* (Lisbon Industrial Institute). From this new organization, dated February 17, 1950, six groups arose of related manufacturing plants, called "Zones" (Acids Zone, Fertilizers Zone, Copper Zone, North Zone, Metalworking Zone and Textile Zone). And because each one of these "clusters" of plants, associated with each other by an industrial logic of their own, brought necessarily together the specific problems of that mesh which made them up, it soon started to be defined, within those Zones, Study Centres for the corresponding areas or subjects of study, where those who provided technical service to that production sector were in motion. This cooperation would have supported the initiatives of gradual autonomy of the personnel in these services in relation to external action on technical problems which they could not only identify, but also criticize, evaluate and point out solutions to solve them.

Production, development and investigation at CUF – 2nd half of the 20th century

To this very same subject referred the Battelle Memorial Institute, in its report[3] drawn up in the beginning of 1955, by stressing the importance of developing technical staff and technical services within CUF. The documentary sources that have been consulted show that the contacts with the Battelle Memorial Institute go back at least to the year 1949, the time when CUF would have ordered an opinion from this company concerning the implementation of a technical progress programme.

The new section recommended in this first opinion, and the true prime mover of the technical development to be created at the corporation, is later on (1955) referred to in other Battelle documents as the "research section of CUF", an entity whose initial concern should be to ensure technical services to the production zones. In this incipient stage of introducing a research programme at CUF, the dissociation between routine studies and profound studies did therefore not exist. However, as these production problems were gradually solved and with the adequate personnel training, the demarcation between routine and innovation was expected to occur, as it was necessary for a more advanced stage in the programme development.

The "central group of technologists" would thus start from the solution of production problems like, for example, corrosion, presence of blowholes in big castings, low performances or bad quality of fertilizers and the frequent fibre rupture in textile machines. The services provided by the group would be technical production and control services and they would include industrial engineering and cost control, engineering work in industrial operations (performance studies, troubleshooting, etc.) and accessory work with the purpose of helping the heads of production sectors and to increase productivity. Besides, an enlargement of the already initiated work should be made concerning the establishment of standards for the selection, purchase and preventive maintenance of the equipment (equipment and materials standardization). In its ultimate stage, the duties of this "Production Support Service Sector" would be reduced to an intermittent verification of the work carried out previously and to the work on new types of materials and equipment.

These were some of the orientations given by the Battelle Institute as concerns the development of what will be hereinafter called "CUF research project".

From 1949 to 1955, however, CUF had not given up the intention to proceed with the "research project". As a matter of fact, in 1953, the "Studies and Projects Section" is formalized at CUF. The subject matters comprised in its sphere of

[2] CRUZ, Isabel; ALVES, Maria do Carmo (2010) – An approach to the ways of adopting and adjusting technologies in Portugal, by taking as an example CUF plants at Barreiro. *In* Miguel Figueira de Faria; José Amado Mendes (coordinator) – *Proceedings of the International Conference "Industrialization in Portugal in the 20th century – the case of Barreiro".* Lisbon, EDIUAL, pp. 251 – 275.

[3] The following references made with respect to the Battelle Institute reports were withdrawn from: Luís Alves. *Memorandum* dated March 9, 1955 of the Studies and Projects (CUF-Barreiro) to the Technical Management (CUF- Lisbon). [Evaluation of the Battelle Memorial Institute report concerning the evolution highly recommended for the Studies and Projects Section]. Proc. 215, Studies and Projects. Former DQIM (Inorganic Chemicals and Metals Division) Documentation, CUF. CUF/QUIMIGAL Archives.

action –technology; industrial accounting; evaluation and construction; materials and equipment standardization; production control– and the fact of being followed up by the Documentation Service and the Research Laboratory (the latter without a well defined individuality) placed it half-way between the "Production Support Service Sector" and the 1st stage of the "Technical Progress and Service Sector", both designed by the previously cited Battelle Institute report of 1955. The "Studies and Projects" were a research embryo created at Barreiro, in 1952, by the engineers António Gouveia Portela and Luís Alves.[4]

Fig. 5: António Gouveia Portela.
"Indústria Portuguesa", Abril, 1965.

Fig. 6: Luís Alves. Portrait in
"50 Anos da CUF no Barreiro", 1958.

The structure and function of this section in its 1st stage determined that all studies and projects carried out at the Plant were part of such section and that all studies and projects would have the contribution of every section's experts and of the central body.

Concurrently with the Battelle recommendations, the "Studies and Projects" started-up at CUF, its first purpose being the organization of the industrial reality, but having its horizon in the future development of products and processes. Once the routine problems of the manufacturing plant were identified and adequately controlled, in the first stage, the routine work would be otherwise ensured and the engineers would be freer to do their creative work. However, the technical staff at CUF-Barreiro was still insufficient and it was taking too long to get more engineers for the Zones, which was necessary both for its expansion and for feeding the service concerned[5]. So, the training of experts for the "Studies and Projects" was going on at slow pace.

The following years were characterized by a close cooperation between the different Zones and the "Studies and Projects". As evidence thereof both established and held monthly meetings with the double purpose of evaluating the progress of the various projects and studies under execution and of raising the discussion of problems of common interest. Therefore, at this moment it is relevant to investigate into further detail of the Zones and their problems, taking as example the one that can be of greater significance at that time, from a technological point of view, at CUF-Barreiro.

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[4] Burkhart – Interview with Engineer Luís Alves, manager of the Research and Documentation Centres at CUF, of June 22, 1070. MK [McKinsey] Dossier on CUF Reorganization. Documentation extracted from the Archives of CUF Board of Directors. CUF/QUIMIGAL Archives.

[5] As an example of initial recruitment, a reference should be made to the transfer from the Acids Zone of the graduate technician Orlando Pedro, who began to act with the purpose of standardizing the equipment and reducing the variety of spare parts, under Eng. Luís Alves' guidance, at the subsection Equipment Standardization, that was being organized at that time in the Studies and Projects Section. Edgar Wahnon. Memoranda of March 10 and 11, 1953. Proc. 215, Studies and Projects. Former DQIM (inorganic Chemicals and Metals Division) Documentation, CUF. CUF/QUIMIGAL Archives.

Fig. 7: Main body of the “Studies and Projects”. Experts provided solutions for Zones common problems. Periphery –All the studies and the projects related to each Zone activity. By the middle of the 1950s, several Zone Study Centres were already created, and the Zone’s specialized personnel managed the specific problems of their own.

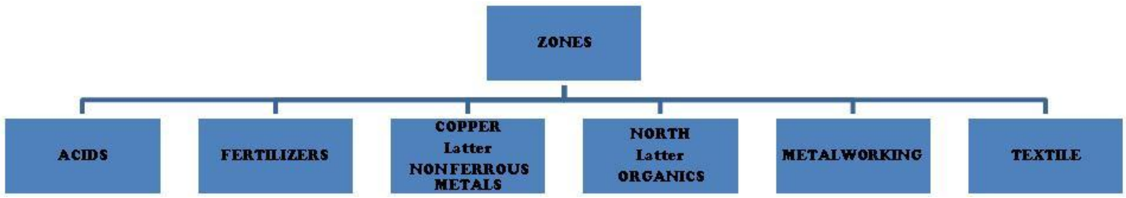

Fig. 8: CUF-Barreiro “Zones”. “Zones” provided industrial performance and also technical services; The technical services of each Zone were performed by the “Studies Centre”.

The Acids Zone

By analyzing the replies to a request made by Battelle, in 1954, about every Zone of CUF-Barreiro, aiming at being provided with relevant information on each one of them, namely in terms of productions, technologies and more pressing problems, it is possible to characterize the paradigmatic case of the Acids Zone.

In 1954, the Acids Zone defined its activity as follows:

A – Productions and consumptions

1. Manufactured products and respective annual productions – a) sulphuric acid at several concentrations, including “oleum” with 20% free SO_3. Total annual production of about 130,000 (metric) tons expressed as monohydrate (MHS); – b) Sodium sulphate: 3,000 tons; – c) Hydrochloric acid 29.7% to 33.3%: 5,100 tons.

2. Internal and external consumptions – a) Sulphuric acid, 75.5% for internal consumptions – 1.1% Acids zone; 70.6 % Fertilizers Zone and 3.8 % Copper Zone; 24.5% for external consumption. – b) and – c) 1,360 tons each, supplied to different customers.

3. Raw materials used: cupriferous pyrites from the Portuguese mines of S. Domingos, Aljustrel and Lousal, both in sulphuric acid manufacturing plants by the chamber process and in sulphuric acid plants by the contact process; marine sodium chloride (from Tagus and Sado rivers); ammonia liquor at 25% supplied by UFA (*União Fabril do Azoto*, an affiliated company of CUF).

4. Manufacturing plants: Pyrite milling plants (2); Chamber process plants (8); Acid contact plants (2); Acid dilutions (2); Sodium sulphate and hydrochloric acid plant; Sodium sulphate milling plant.

B – By-products

Purple ore – about 74,500 tons per annum; electrostatic precipitator (ESP) dust with 40% lead content (from Contact Plant no. 1) or 12% lead content (from the remaining sulphuric acid plants) – about 2,200 tons per annum, in total of the acid plants considered; selenium sludge (≈ 3% Se) obtained from the electrostatic precipitator (ESP) cooled gas of Contact Plant no. 2 – 10 to 15 tons per annum; lead sludge, in variable compositions, from tank and chamber cleaning.

C – Types of research

Research to be performed by Zone 9, of interest for the Acids Zone:

A. Studies and chemical tests, including "standard" tests for defining the characteristics of the materials used in the Zone (ceramics, plastics, special steels, etc.), search for interesting elements in by-products, corrosion studies, etc.

B. Studies and technological tests on production problems, with design and drawings for modification of the presently existing plants.

C. Technical information retrieval, standards and laws relating to industries and productions of the Zone, in order to ensure know-how updating and to enable the modernization of the manufacturing plants.

In 1955, the list of studies and projects carried out by the experts of the Acids Zone, which embraced the following subjects – furnaces; – sintered raw pyrites, silica, copper, lead and zinc content; – carob bean industry; – Contact Plant no. 3 (preliminary study); – removal of lead from pyrites during roasting; – plants with Towers; – Acids Zone office; – automatic beating of electrostatic precipitators in the Chamber-process Plants; – change in maintenance of burnt pyrite in the Chamber-process Plants and in Contact Plant no. 1; – identification of the supply of milled pyrite to Groups 1 to 4; – utilization of Copper Metallurgy gas; – de-dusting system of Symons Milling unit; – shipment and auxiliary units of Contact Plant no. 2; – revamping of the cleaning system of Contact Plant no. 1; – sodium sulphide, – sodium sulphate; – iron chloride; – ammonium chloride, reveals the evolution achieved within just a few years.

The studies and projects proceeding from this Zone are an evidence of a new endogenous ability to reflect on, to approach and treat the technological problem by the Plant technical staff.

The «Studies and Projects» and the study centres of the zones

In March 1956, the "Studies and Projects" were made up by following sections: SP – Technology; SP – Materials; SP – Documentation; SP – Laboratories; SP – Codification and Standardization and SP – Drawing Room. These sections represented the spheres of action of the experts before the remaining CUF Departments. In October of the same year, a new structure of the SP (Studies and Projects) presents 3 different sections: 1. Studies and Projects; 2. S.P. – Drawing Room; 3. S.P. – Codification and Standardization. According to this new document, the first one was "the basis for the Future Research Institute of the corporation". Its aim was "the accomplishment of the chemical process project as well as the resolution of all matters related with that project stage. It embodied the former fields of (Chemical) Technology, Materials (and Corrosion) and Laboratories, and it had in view the creation of new ones: Mechanical Technology; Chemical Mechanics; Instrumentation and Control; Technical-Economical Field; Statistical Control. Additionally, a regulation proceeding from the Technical Management Department of CUF (Headquarters –Lisbon) determines how the corporate studies and projects shall work and presents not only one, but two entities that have legitimacy to carry them out, the "Studies and Projects" (already existing with a structure of its own) and the Study Centres (newly created, directed by the head of the Zone). At this time there are five Zones that developed a Study Centre: Acids; Fertilizers; Copper; North and Textile. It can be assumed with some consistency that the Zone experts, instead of going to the "Studies and Projects" in order to establish there their activity, remained linked with the zone structure, increasingly defining a study and research territory of local scope. The demarcation, here in evidence, between the Zones and the "Studies and Projects" occurred only three years before the changing of the latter into Research Centre (1959). Engineer Luís Alves used to tell that in 1956, following these separations, "the 15 years war" had begun because the corporate policy had determined that every "high level" research should be reported solely to this body, which was first called "Studies and Projects" and then Research Centre. In the Zones (or Divisions) nobody felt that advanced research was not being carried through, therefore no one asked the central body for studies and the latter began to live on work performed by external entities.

The occurrence that took place in 1957 regarding the construction of the third contact-process sulphuric acid plant, the "Contact Plant no. 3" makes clear this superposition resulting from some badly settled confusion of duties: in March, the Acids Zone, having its Study Centre under full organization, and concerning the technological calculation of said plant, suggests to CUF Technical Department that this Centre be provided with the necessary means so that, in the future, CUF might have the capability of carrying out the project of sulphuric acid plants. With these means it would be possible to thoroughly analyze the plants and to recover a set of data, upon which the technological calculation of a furnace, of catalysis or of an absorption tower could be based. Strengthening the technical personnel of the Zone was obviously one of the prime factors to attain this capacity: a new chemical engineer, in order to discharge the current one from his tasks at the production part, an analyst and an assistant who would be the staff of the Acids Zone Laboratory to be set up too. The reply of the Technical Department to this *memorandum* enhances the intent to proceed to the strengthening of the technical personnel of this Zone, but it stresses that the Technological Calculation is reserved to the experts of the Studies and Projects and not to the Study Centres. Thus, "in these terms", that Study Centre should concern itself mainly with specific problems of the Zone which, in this case, is skilled to manufacture Acids. To do so, it must avail of a small Zone Laboratory and other facilities, but it is not necessary that it includes a department or an individual specialized in Technological Calculation, who would not even have enough work for a full time occupation (…) we kindly ask you to request the Studies and Projects to perform the Technological Calculation for Contact Plant III and to provide them with the necessary means, so that such calculation may be made within a short time as required".

By the end of 1958, following studies were being performed, among others, by the Study Centre of the Acids Zone: Preliminary studies - Study of documentation on fluidized bed furnaces (DORR), Study of documentation on turbulence furnaces (BASF), Study of documentation on plants with towers, economic Study aiming at the replacement of the Chamber-process Plant, Study of hydrochloric acid absorption, Study of the possibility of manufacturing potassium sulphate in the existing furnaces, Viability Study of titanium dioxide manufacture in Portugal; Study of lead recovery in the form of concentrated dust, Study of the operation of electrostatic precipitators (ESP) of the Chamber-process Plants.

The documentation

Until 1954, the circulation of technical and scientific information at Barreiro industrial complex consisted of a bulletin containing the abstracts of the 50 technical-economic magazines then subscribed by CUF, of some basic books and of the *Chemical Abstracts*. The development imposed by the structure of the production and of the technical work gave rise to the creation of a Documentation Service. The first activity of this Service was purchasing books with the purpose of setting up a Library. From that time on, the Bulletin that formerly circulated in the Plants published *abstracts* of the most interesting articles and transcribed the summary of the received books. The documentation soon assumed such proportion that, in 1956, a classification system was adopted, in this case the Universal Decimal Classification, UDC. Simultaneously the number of magazine subscriptions increased to 200 and the manual selection of information became unbearable. The FILMOREX equipment was then purchased for keeping records and selecting articles.

The Documentation Service was an integral part of the Studies and Projects and cooperated with the Zones (which also had a library of their own) in various fields, such as the search for and dissemination of technical and economic information, works requisition and lending, translation of articles and correspondence.

Conclusion

In 1959, the wide and important reorganization in progress at the corporation, which created the Divisions, these being entities which gathered the commercial part and the manufacturing plants part, extinguishes the Technical Department and creates the Research Department. This Department aggregates the various CUF centres: Project Centre; Documentation and Patent Centre; Standardization Centre and Research Centre, the latter being the result of the "Studies and Projects" extinction. Alongside of this separation, the great technological achievements at CUF-Barreiro in the decades of the fifties to the seventies of the 20th century always had the close cooperation of the Zones experts.

The structural changes at CUF-Barreiro which will still go on during the decades of the sixties and seventies of the 20th century will sometimes lead to modifications in the name, constitution and even, to some extent, in the scope of the Study Centres of the decade of the fifties. However, the importance of that "technological movement" and the significance assumed by the research and development activity performed is a fact that deserves much more "Studies and Projects".

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RUSSIA AND MONGOLIA: TRANSFER OF SCIENTIFIC KNOWLEDGE IN POLITICAL CONTEXT (1920S)

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Abstract

Russian-Mongolian scientific contacts began in the middle of the 1920s. It was impossible for Mongolia –just until recently a feudal-theocratic country without any system of secular education– to start organizing of national scientific institutions without foreign support. Taking into consideration all historical and political factors, the first possible candidate for granting such a support was the Russian Academy of Science (RAS).
The latter had an interest of its own in the exploration of Mongolia for several reasons: further explorations in frontier regions; or systematization of materials, having been previously collected during several Russian expeditions to Central Asia and then located in Academic institutes. The intentions of the Russian scientists, interested in further explorations of Mongolia, and the support of the Mongolian Scientific Committee coincided with the geopolitical situation in that region and the striving of the Soviet government for obtaining political authority there. That is why RAS' initiatives were supported by the government.
Russian-Mongolian scientific contacts took various forms, such as: joint Russian-Mongolian expeditions and publications of collected data, support in establishing a national museum in Mongolia, exchange of scientific editions, formal visits of RAS officials to the Mongolian Scientific Committee, training of young Mongolian specialists in Russia, and so on. They developed from the gratuitous aid on the part of Russia to the bilateral party relationship on the basis of various agreements and conventions in which both countries took part. Their format, content and intensity varied due to the international status of Mongolia, the nature of Russian-Mongolian relations, and the internal political situation in both countries. Today, according to many researchers, an ideological component left the academic interaction of both countries giving place to national interests.

Modern science emerged in Mongolia in the first half of the 20th century. Russian scientists played a key role in this process. They transferred the European system of scientific knowledge, ideas and research methods to Mongolia, helped create scientific communities, research and educational institutions. The fact that Russian scientists became a kind of "donors" was based on a complex of historical reasons and an undefined international status of Mongolia in the first place.

The independence of Mongolia, declared in 1921 in a unilateral order from China, was officially recognized only by Soviet Russia, as for Soviet Russia, Mongolia was an important strategic partner in the Far East and a buffer between

Russia and China[1]. Due to this fact, Russia provided Mongolia with various aids aimed at establishing the economy of the country, research and development of its natural resources, formation of national state institutions, and fortification of defensive capacity. An important element of the Soviet foreign policy was also a comprehensive support to the growing scientific potential of Mongolia.

It was impossible for Mongolia, which only a few years earlier was still a feudal-theocratic country without any system of secular education, to start organizing national academic institutions without foreign support. The first possible candidate for granting such a support was the Russian Academy of Sciences (RAS) due to its geographic neighboring and the deep historical connections between Mongolia and Russia.

In November 1921 the first scientific institution was founded in Mongolia, it was a Scientific Committee. It was created on the basis of typological features of the Russian Academy of Sciences and in its activity it made use of the Russian Academy's experience.

In particular, following the example of Western-European and Russian academic societies, one of the initial steps of the Scientific Committee was the foundation of a Natural History Museum as a first step towards accumulating knowledge about nature and history of the country and as a research laboratory. Replenishment of the Museum funds became a must for all foreign expeditions in Mongolia. The Scientific Committee actively developed another traditional form of activity for Communities, electing foreign scholars as honored members in its rows[2].

For Mongolia, which had stepped on a path of radical social changes, it was vital to have the "flavouring of Western science". Due to this task, the officials of the Scientific Committee turned to RAS with a request to send specialists in different fields to give lectures, as well as books for organizing a scientific library. Throughout the 1920s and the 1930s Russian specialists in various areas worked in the Mongolian Scientific Committee.

The second most important task which the Mongolian Government had set for the Scientific Committee was the reconnaissance of natural resources of the country. Limited funding, poor basic material means and a total lack of specialists did not allow expanding research activity in this field. Russian scientists were interested in studying Mongolia as vast materials on Central Asia had been accumulated in Academic institutes and museums by that time, and for their systematization it was required to continue research in the neighboring country. Taking into account the Soviet Government's interest in strengthening its political influence in Mongolia, RAS authorities initiated a discussion on providing Mongolia with scientific help.

They stressed the necessity to develop previous investigations not only because of scientific, but also economic and political advantages for Mongolia and the Soviet Russia. The arguments provided by the academic society turned out to be convincing and the Government ordered to organize a large-scale research expedition to Mongolia and to support the Scientific Committee. Political factors also played an important role in the decision: one of the top governmental officialswho supervised this project in the Soviet government, N.P. Gorbunov, emphasized in his speech that it was necessary to conduct research in Mongolia for "cementing our friendly relations with the Mongols and for the penetration of our political influence in Mongolia"[3].

As a result of the discussion the special Mongolian Commission was formed, which was to lead research works in Mongolia. Employees of the Mongolian Commission provided the Scientific Committee with a great help in organizing research activity, training national specialists and accumulating scientific knowledge in Mongolia[4].

It should be noted that for more than 60 years (1930-1990) Russian-Mongolian relations were defined as relations between a patron and a satellite. However, throughout the 1920s Mongolian statesmen resisted the Bolshevik's ideological influence defending a national way of development for their country[5]. It resulted in correcting the directions of scientific support provided by RAS. That was the reason why friendly relations with the Scientific Committee were a priority for the Mongolian Commission.

The tasks that the Soviet government set for the Mongolian Commission varied according to the foreign policy goals:

1925 - Academic research of Mongolia, which was necessary for "penetration of our political influence in Mongolia";

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[1] For more details see: B. Baabar. *History of Mongolia*. University of Cambridge, 1999.

[2] P.K. Kozlov. 'The travel journal of Mongolo-Tibetan Expedition of 1923-1926' (Kozlov P.K. Dnevniki Mongolo-Tibetskoi ekspedicii 1923-1924 gg.), ed.T.I. Yusupova, A.I. Andreev. St. Petersburg: *Nauka*, 2003, p. 127.

[3] State Archive of the Russian Federation. F. 5446. Op. 37. D.10. P. 67.

[4] Yusupova T.I. M*ongol'skaya komissiya Akademii nayk. Istoriya sozdaniya i deyadel'nosti (1925-1953)* (The Mongolian Commission of the Academy of Sciences of the USSR. History of Organization and Activities. 1925-1953). SPb., 2006.

[5] Barkmann U. *Geschichte der Mongolei oder die Mongolische Frage: die Mongolen auf ihrem Weg zum eigenen Nationalstaat*. Bonn, 1999; Murphy G. *Soviet Mongolia. A Study of the Oldest Political Satellite*. Berkeley, 1966; Rupen R. *Mongols of the Twentieth Century*. Part. I. Indiana University, Bloomington, 1964.

1926 - “Research of Mongolia is a kind of duty for Soviet Russia, Mongolia’s closest neighbor that is equipped with the necessary means for research”;

1930 - “Our [Soviet Russia’s] duty is to help developing and improving economic and cultural construction in Mongolia”;

1947 - It is necessary “to build a foundation for practical steps of the Mongolian Government in the field of improving economic and cultural construction”.

It should be noted that the ideological and political motivation of Russian expeditions in Mongolia did not affect their methods and academic credibility of research, as it is demonstrated by the reports of the expeditions. The style of the reports, by the way, conforms to established norms of academic literature, with its characteristic logic of argumentation.

The period of 1925-1933 became the most prolific one in the activity of the Mongolian Commission. During these years 41 expeditions were organized where more than 30 people took part. As a rule, members of the Mongolian Scientific Committee took part in Russian expeditions, which was a unique opportunity for them to broaden their professional horizons. The expeditions led archeological, botanical, geological, geochemical, zoological, soil and geographical, ethnographical and linguistic research. Moreover, all research results obtained by the expeditions were published in the issues of the Mongolian Commission and were handed over to the Mongolian Scientific Committee.

The activity of the Mongolian Commission closely followed legal regulations of international interaction and was subject to treaty engagements between the academic societies; however, until 1929 they were not official. The first formal treaty between the Academy of Sciences and the Mongolian Scientific Committee was signed in 1929. This agreement was aimed at eliminating all legal misunderstandings if they were to happen in the work of research expeditions in Mongolia.

The preparation of the treaty highlighted various contradictions between the Russian Academy of Sciences and the Mongolian Scientific Committee. For the Russian Academy of Sciences, as a rule, research work in Mongolia was a basis for further theoretical studies. The Mongolian party was more interested in the works of applied character, especially in economic and agricultural fields, which would allow realizing plans of quick sociopolitical reorganization of the country. The reorientation of research programs of the Mongolian Commission towards salvation of economic problems begun in the 1930s, as a result of intense state and ideological control of research activity in Soviet Russia. Soil, geological, geochemical, agricultural development and –first and the basis of Mongolian’s economy– cattle breeding became a priority in the activity of the Mongolian Commission in the following years.

In 1932 a sharp reduction of expedition research happened due to the aggravation of the internal situation both in Mongolia and in the Far East. And so, in June 1935 the Political Bureau of the Bolshevik party took the decision to suspend the expedition and other activities of the Mongolian Commission.

Since then and up to the Second World War, publishing became the main focus of the Mongolian Commission. The issues published by it became study guides in Mongolian nature, history, language, literature and economy. All materials published were handed over to the Mongolian Scientific Committee and later on they became textbooks on Mongolian history and nature for the next generation of Mongolian researchers. In various Russian academic institutions a vast material, a database in its own way, is accumulated as a result of research work conducted within the framework of these cooperative activities.

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'GERMAN SCIENCE' IN PORTUGAL, 1933-45

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Abstract

The period in analysis raises several crucial questions which revolve mainly around the Science-Society-State triad. This particularity calls for an historical analysis that takes as its cornerstones the relations between: 'Science, Culture, State (Nation) and Society', 'Science and Nationalism' or 'Science and Propaganda'.
The National-Socialist State takes advantage of Science, in an ideological and propagandistic manner and brings new nuances into the institutional relationship between Science, Society and State that had, at least since the end of the 18th century, appeared quite steady and balanced.
Science in National-Socialist Germany –and the term 'Science' should here be understood in the broadest sense of 'Wissenschaft', including both 'Naturwissenschaften' and 'Geisteswissenschaften'– has been the object of several studies. These have been mostly developed within a German historiographic framework and consequently, mainly concerned with demystifying internal structures and problematics. They therefore paid less attention to the circulation (and influence) of 'German science' outside Germany.
This paper will try to bring into focus the circulation of Science and Techonology between Portugal and Germany of that period, thus tentatively providing a wide and general picture of the persons and institutions involved in a complex –social, scientific, political– network of influence.

> «[...] the bomb has persuaded historians that they must take some account of the role of science»
>
> (Kuhn 1977: 132).

One will hardly find a moment in the History of Mankind which so incisively enlightens Bacon's famous aphorism *Scientia potentia est* as the period between 1933 (or 1939) and 1945 does. Sure enough, the fact that World War II was decided rather by laboratory research than bravery on the battlefields meant a global (and, in a way, radical) change of perspective of Science as well as of History. 'German Science' is undoubtedly (on both sides of the Atlantic) at the heart of this transformation.

This period raises several crucial questions which revolve mainly around the Science-Society-Politics triad. This particularity calls for a historical analysis that takes as its cornerstones the relations between:

- Science, Culture, State (Nation) and Society
- Science and Nationalism
- Science and Propaganda

The National-Socialist State takes advantage of Science in an ideological and propagandistic manner and brings new nuances into the institutional relationship between Science, Society and State that had, at least since the end of the 18th century, appeared quite steady and balanced. One needs only to remember, for instance, the interdiction of the periodical *Nature* in nazi Germany (cf. Rügener 1938).

Science in National-Socialist Germany –and the term 'Science' should here be understood in the broadest sense of 'Wissenschaft', including both 'Naturwissenschaften' and 'Geisteswissenschaften'– has been the object of several studies (cf. among many others, Kuhn et al. 1966, Erdmann 1967, Meinel/Voswinckel 1994, Hausmann 2002 or Cornwell 2003). These have been mostly developed within a German historiographic framework and therefore mainly concerned with demystifying internal structures and problematics. Hence less attention has been paid to the external use that nazi propaganda made of an internationally recognized and reputable tradition: Science produced in German academies, universities and laboratories.

The 3-year research project, that this paper intends to briefly present, aims to bring into focus the many ways of representing 'German Science' in Portugal between 1933 and 1945, be it in a more political field (where the scientific publishing was overshadowed by the propagandistic and ideological one) or in the more scientific and institutional field of specialized interaction, with 'German Science' influencing the building of several academic disciplines in Portugal (from Ethnography to Engineering, not forgetting the Philologies). The analysis will therefore focus on:

- scientific works of German authors published in Portugal;
- exhibitions (at the National Library in Lisbon, for example); representations in media;
- institutional and non-institutional relations between both States in the academic and university context.

It is above all from the mid-sixties, i.e., 20 years after the end of World War II, that Germany begins to reconsider the role of science and technology as well as the role of universities during the nazi period. The essays written by Saller (1961) or Erdmann (1967) and the several public university lectures (*Ringvorlesungen*) that took place in some German universities (see for example Kuhn et al. 1966) can be considered without a doubt an important step towards a first outline of a History of Science in National-Socialist Germany. Other essays and collective works followed (Mehrtens/Richter 1980, Beyerchen 1980, Hermann 1982, Meinel/Voswinckel 1994, Hausmann 2002) until the more recently published book by Cornwell (2003) and the biographical lexicon by Gruettner (2004).

Most of these writings share a common feature: they are primarily concerned with inner landscapes and networks of the History of Science in Germany. They tend, therefore, to overlook the characteristic 'universalism' of the scientific enterprise –after all, one of the most important values of scientific knowledge–, neglecting the internationalisation of 'German Science' of that period, and thus not mapping a complex network of individuals, hybrid scientists/politicians, that take on these two different roles according to the particularity of their activity at a given time: as scientists they promote science 'made in Germany', they follow its theories and principles, devoting special attention to the newest German research developments; on the other hand, as political agents they encourage and decisively support the creation of institutional networks with German scientific organizations, or take German institutions as a model for creating new ones abroad.

If one understands propaganda as some kind of *Anxiety of Influence* (Bloom), as «the promotion of a State in foreign countries so that its national creations are *recognized* and *imitated*» (Giese 1939/40: 163, our italics), what is, then, the influence of 'German Science' in Portuguese contemporary Science? What are the parallelisms and affinities of these two national 'Epistemic Cultures'? Which of the scientific institutions were connected? How did those connections develop? What was the role of ideological and political propaganda in that exchange?

The project will basically try to deal with these questions and its possible answers focusing on the German-Portuguese networks of *institutions*, *individuals* and (academic) *disciplines* during this period.

Within the broader context of a European Cultural History of Science (and more specifically in the framework of a Portuguese-German relationship) some works have already been produced on prior phases of intense scientific contact, as in the Epoch of the Discoveries or in the 18th century (cf. among others Cardoso 2003, Carneiro *et al*. 2000, Carvalho 1996, Clara 2008, Opitz 1983); similarly, in recent years (and in the wake of an increasing attention to Portuguese History in the second half of the 20th century), more studies about the complexity of the relationship between these two authoritarian regimes have been published and become notable (cf. Rosas 1990, Louçã 2005, Rosmaninho 2001, Pimentel 2006, Loff 2008 or Torgal 1995). However, with a few exceptions (e.g.: Branco 2007, Branco/Barros 2007, Brito *et al*. 2002, Matos 1996 and 2005, Grossegesse 1996) those studies hardly explore in a systematic way the prolific scientific connections between the two States at that time.

In fact, and from a pure historiographic point of view, there exists a lack of studies about the relations between Portugal and Germany during the Second World War. In spite of the growing interest in this period (cf. among many others Rosas 1990 or Loff 2008) –which has to be understood against variegated and complex backgrounds, some of them national-mediatic, other historical-fictional (like the 'discovery' of the role played by Aristides de Sousa Mendes in helping Jews to flee from the Nazis or the novels that in more recent years have been staging Lisbon as a city of spies, some sort of European Casablanca)–, there are only a few authors that have written specifically about the subject (Louçã 2005, Medina

1998, Pimentel 2006, Torgal 1995). The dominant perspective remains however historical-political. There are also scattered papers focusing on the subject, but indeed Portuguese historiography has paid scant attention to the relations between Portugal and Germany beyond the stereotypical. The reason is well known: the German language continues to be an obstacle in the access to vital sources and bibliography.

Some of the essays mentioned before (Grossegesse 1996, Branco 2007 or Matos 2005), for example, can be seen as exceptions in this context as they analyze cultural institutions (respectively the *Mocidade Portuguesa*, the influence of German Ethnography in Portugal and the *Kraft durch Freude* travels in Portugal) from a much broader point of view, a point of view which, even though not being specifically concerned about scientific matters, allows us to catch a glimpse of the rich and densely interwoven networks established during the period in question between Portugal and Germany.

The image produced by the Portuguese historiography of science does not differ significantly from the one provided by Portuguese historiography in general. In fact, it shares the same silencing of certain areas of interaction and interface, whether because of political sensitivity rarely overtly discussed or the alleged linguistic obstacles. The bibliography by Tavares/Leitão (2006) or the essays collected by Nunes/Gonçalves (2001) have a few references to the international networking of science; the case of the German-Portuguese scientific relations between 1933 and 1945 remains until nowadays a kind of no man's land.

A proof of this State of the Art is the absence of any systematic approach to make an inventory of personalities and institutions involved in the interaction with personalities and institutions of Germany throughout the 20th century that would be a relevant contribution in order to recover a chapter of the patrimony of European History with repercussions still visible but rarely recognised in the present.

Focusing on the scientific relationships between Germany and Portugal between 1933 and 1945, this project aims to fill a twofold gap: firstly, the aforementioned one thereby contributing to the making of a European Cultural History of Science of that period; and secondly, the one that directly derives from it, that is: the deficit in research on the decisive and prolific scientific Portuguese connections to Germany.

References

BEYERCHEN, Alan D. (1980), *Wissenschaftler unter Hitler. Physiker im Dritten Reich*. Köln.

BRANCO, Jorge Freitas (2007), «Jenseits des Rheins: Über periphere Beziehungen in der Ethnologie». In: *Streitfragen – zum Verhältnis von empirischer Forschung und ethnologischer Theoriebildung am Anfang des 21. Jahrhunderts*. Tagung 2007 der Deutschen Gesellschaft für Völkerkunde, https://repositorio.iscte.pt/handle/10071/1149.

BRANCO, Jorge Freitas; BARROS, Miriam (2007), «Etnografia dum lugar tecnológico. O campus da Alameda (ist, Lisboa)». In: *Intersecções Ibéricas. Margens, Passagens e Fronteiras*. Eds. CUNHA, Manuela; CUNHA, Luís. Lisboa: 90 Graus Editores (pp. 205-224).

BRITO, José Maria Brandão de; HEITOR, Manuel; ROLLO, Maria Fernanda (Eds.) (2002), *Engenho e obra: uma abordagem à história da engeharia em Portugal no século XX*. Lisboa: Pub. D. Quixote; IST.

CARDOSO, José Luís (2003), «From natural history to political economy: the enlightened mission of Domenico Vandelli in late eighteenth-century Portugal». *Studies in History and Philosophy of Science*, 34 (pp. 781-803).

CARNEIRO, Ana; SIMÕES, Ana; DIOGO, Maria Paula (2000), «Enlightenment Science in Portugal. The Estrangeirados and their Communication Networks». *Social Studies of Science*. 30 / 4 (August 2000). London: Sage (pp. 591-619).

CARVALHO, Rómulo de (1996), *Actividades científicas em Portugal no século XVIII.* Coord. Secretariado Editorial da Universidade de Évora. Évora: Universidade.

CLARA, Fernando (2008), «Deutsche Wissenschaft – Portugiesische Natur: Die Entstehung einer 'Flore Portugaise'». In: *Wissen im Netz: Botanik und Pflanzentransfer in europäischen Korrespondenznetzen des 18. Jahrhunderts*. Eds. DAUSER, Regina *et al*., Berlin: Akademie Verlag (pp. 65-78).

CORNWELL, John (2003), *Hitler's Scientists: Science, War, and the Devil's Pact*. London: Viking.

ERDMANN, Karl Dietrich (1967), *Wissenschaft im Dritten Reich*, Kiel: Veröffentlichungen der Schleswig-Holsteinischen Universitätsgesellschaft.

GIESE, Wilhelm (1939/40), «Französische Kulturpropaganda». *Hansische Hochschulzeitung*. 21/7, Hamburg (pp. 163-171).

GROSSEGESSE, Orlando (1996), «A lição alemã na Mocidade Portuguesa». In: *Portugal-Alemanha-África. Do colonialismo imperial ao colonialismo político*. Eds. OLIVEIRA MARQUES, A. H. *et al*., Lisboa: Colibri (pp. 185-197).

GRÜTTNER, Michael (2004), Biographisches *Lexikon zur nationalsozialistischen Wissenschaftspolitik*. Heidelberg: Synchron.

RÜGENER, H. (1938), «Die *Nature* eine Greuelzeitschrift». *Zeitschrift für die gesamte Naturwissenschaft*, 3/12, Braunschweig (pp. 475-479).

HÅRD, Mikael; JAMISON, Andrew (2005), *Hubris and Hybris. A Cultural History of Technology and Science*. New York / London: Routledge.

HAUSMANN, Frank-Rutger (Ed.) (2002), *Die Rolle der Geisteswissenschaften im Dritten Reich (1933-1945)*. München: R. Oldenbourg Verlag.

HERMANN, Armin (1982), *Wie die Wissenschaft ihre Unschuld verlor. Macht und Mißbrauch der Forscher*. Stuttgart.

KNORR-CETINA, Karin (1999), *Epistemic Cultures. How the Sciences Make Knowledge*. Cambridge / London: Harvard University Press.

KUHN, Helmut; PASCHER, Joseph; MAIER, Hans et al. (Eds.) (1966), D*ie deutsche Universität im Dritten Reich. Eine Vortragsreihe der Universität München*. München: Piper.

KUHN, Thomas S. (1977), *The Essential Tension. Selected Studies in Scientific Tradition and Change*. Chicago/London: University of Chicago Press.

LOFF, Manuel (2008), *O Nosso Século é Fascista. O Mundo visto por Salazar e Franco (1936-1945)*. Porto: Campo das Letras.

LOUÇÃ, António (2005), *Portugal visto pelos nazis. Documentos 1933-1945*. Lisboa: Fim de Século.

MATOS, Mário (1996), *As viagens marítimas da organização nazi Kraft durch Freude a Portugal (1935-1939): turismo, literatura e propaganda*. Lisboa: FCSH-UNL.

MATOS, Mário (2005), «Tourismus und 'Totale Mobilmachung' oder Kraft durch Freude-Auslandsreisen als interkulturelle Inszenierung». In: *Mobilität – Raum – Kultur. Erfahrungswandel vom Mittelalter bis zur Gegenwart*. Eds. REHBERG, K.-S. *et al*., Dresden: w.e.b. Thelem (pp. 247-264).

MEDINA, João (1998), «Salazar na Alemanha: acerca da edição de uma antologia salazarista na Alemanha hitleriana». *Análise Social*, XXXIII/145 (pp. 147-163).

MEHRTENS, Herbert; RICHTER, Steffen (Eds.) (1980), *Naturwissenschaft, Technik und Ideologie. Beiträge zur Wissenschaftsgeschichte des Dritten Reichs*, Frankfurt a. Main: Suhrkamp.

MEINEL, Christoph; VOSWINCKEL, Peter (1994), *Medizin, Naturwissenschaft, Technik und Nationalsozialismus: Kontinuitäten und Diskontinuitäten*. Stuttgart: Verl. für Geschichte der Naturwiss. und der Technik.

NUNES, João Arriscado; GONÇALVES, Maria Eduarda (Eds.) (2001), *Enteados de Galileu? A Semiperiferia no Sistema Mundial da Ciência*. Porto: Afrontamento.

OPITZ, Alfred (1983), «Durch die Wüste, Lichter tragend... Sozialgeschichte und literarischer Stil in den Reiseberichten über die Iberia um 1800». In: *Reise und Soziale Realität am Ende des 18. Jahrhunderts*. Eds. GRIEP, W.; JÄGER, H.-W., Heidelberg: Carl Winter (pp. 188-217).

PIMENTEL, Irene Flunser (2006), *Judeus em Portugal durante a II Guerra Mundial. Em fuga de Hitler e do Holocausto*. Lisboa: A Esfera dos Livros.

PRESAS I PUIG, Albert (2008), «Technological transfer as a political weapon: technological relations between Germany and Spain from 1918 to the early 1950s». *Journal of modern European History*. 6/2 (pp. 218-236).

ROSAS, Fernando (1990), *Portugal entre a Paz e a Guerra (1939-1945)*. Lisboa: Editorial Estampa.

ROSMANINHO, Nuno (2001), *O Poder da Arte. O Estado Novo e a Cidade Universitária de Coimbra*. Coimbra: Faculdade de Letras da Universidade de Coimbra.

SALLER, Karl (1961), *Die Rassenlehre des Nationalsozialismus in Wissenschaft und Propaganda*, Darmstadt.

TAVARES, Conceição; LEITÃO, Henrique (2006), *Bibliografia de História da Ciência em Portugal (2000-2004)*. Lisboa: CHC-UL.

TORGAL, Luís Reis (1995), «Salazarismo, Alemanha e Europa. Discursos políticos e culturais». In: *Portugal und Deutschland auf dem Weg nach Europa*. Eds. LOPES, Marília Santos *et al*. Pfaffenweiler: Centaurus (pp. 193-219).

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 1132.

THEMATIC MAPS AND THEIR CIRCULATION DURING THE NATIONAL SOCIALIST ERA

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Abstract

This article will focus on the circulation on thematic maps developed in the Austrian part of the German Reich (called "Ostmark") during the National Socialist Era in the context of spatial research with the prime aim of addressing the exchanges of maps between political authorities, scientific institutions and various scientists of different disciplines. One focus of the lecture will be on the question of which institutions or scientists initiated this map-circulation. The hypothesis is that it depended on the research institution as well as on different research projects.

Spatial research established itself as a multidisciplinary science in the German Reich during the 1930s, in the process assuming great and above all political significance in the context of National Socialist ethnic and cultural area research and NS expansion policy.[1] Immediately after the annexation of Austria by the German Reich, spatially oriented research projects took off notably, combined with increased thematic map production. As early as per 13 April 1938, the Viennese geographer Hugo Hassinger (1877-1952) compiled a list of persons who might do valuable work for spatial research and spatial planning concerning the Ostmark on behalf of the "Reichsarbeitsgemeinschaft für Raumforschung" established in 1935.[2] Thematic cartography had the task of providing maps urgently required for spatial research. Yet cartography not only generated basic data for other research projects; it was in its turn also markedly influenced by spatial research. A review of map production from the Habsburg era into the 1940s shows that the input required for cartography increased enormously over time. This was chiefly due to the firm establishment of spatial research, since more and more statistical data were processed and visualisation methods grew in complexity.

Cartographic research focused on the one hand on the "Ostmark" and on the other hand, and increasingly, on South-eastern Europe. Spatially oriented cartographic studies helped to identify new areas for settlement and economic expansion as well as to plan resettlement campaigns for the benefit of the German Reich. Various statements in numerous, mostly

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[1] For more about the establishment of spatial research in the German Reich see: Rössler Mechtild, 'Die Institutionalisierung einer neuen Wissenschaft im Nationalsozialismus: Raumforschung und Raumordnung 1935-1945'. In: *Geographische Zeitschrift* 75 (Stuttgart 1987), p. 177-194; Venhoff Michael, 'Die Reichsarbeitsgemeinschaft für Raumforschung (RAG) und die reichsdeutsche Raumplanung seit ihrer Entstehung bis zum Ende des Zweiten Weltkrieges 1945'. *Arbeitsmaterial der Akademie für Raumforschung und Landesplanung* 258 (Hannover 2000); Leendertz Ariane, 'Reichsarbeitsgemeinschaft für Raumforschung'. In: Fahlbusch Michael / Haar Ingo, *Handbuch der völkischen Wissenschaften* (München 2008), p. 520-527.

[2] UAW (Archive of the University of Vienna), unpublished works done by Hugo Hassinger, box 15 (letter Hassinger to RAG 13. 4. 1938).

unpublished reports and letters clearly show that the thematic maps and other space-centred studies developed in the "Ostmark" were explicitly compiled with political intentions so as to contribute actively towards the National Socialist "Lebensraum" policy. The initiative in this did not always originate with National Socialist politicians and authorities, but often came from scientists themselves, who submitted their research findings to National Socialist politicians of their own accord, without having been constrained in any way to do so[3]. In this context, the circulation of both existing and newly produced thematic maps increased as well. The latter were mainly compiled by arts scholars and natural scientists as well as by cartographers forming part of various research groups.[4]

We can find four different sorts of map-circulation during National Socialist Era: at first the circulation between various scientists of different disciplines within a research society, second the circulation between different research societies, third the circulation between scientists and National Socialist authorities, and fourth the circulation between different political authorities. Most of the thematic maps were made by different research societies. In the Austrian part of the German Reich many research societies existed, which were situated mainly in Vienna. The most important were:

- *"Working Groups for Spatial Research"* at the Universities von Graz, Innsbruck and Vienna: They were part of the "Reichsarbeitsgemeinschaft für Raumforschung", the most important working group was the "Working Group for Spatial Research" of Vienna University, which was founded in autumn of 1938. This Working Group was headed by the geographer Hugo Hassinger[5]. Its research mainly covered the districts of Vienna, Lower and Upper Danube as well as South-eastern Europe.[6]
- The *"Southeast German Research Association"* was founded in 1931. It was one of six ethnic-German research associations set up mainly in Germany and concerned with the systematic study of the German ethnic and cultural territory. The map department was enlarged and headed by Wilfried Krallert (1912-1969), who had studied history and geography and was a NSDAP member as early as in 1934. In 1934 Krallert was also accepted into the SS, where he was assigned the rank of SS Untersturmführer in 1938.[7]
- The *"South-Eastern German Institute"* was established in Graz in April 1938. Its tasks lay on the one hand in observing the political events in Yugoslavia inasmuch as these concerned the interests of the German-language population and on the other hand in promoting German language and culture in Lower Styria (the northern part of today's Slovenia).[8]

In creating these thematic maps, the members of such research communities made use of both statistical data and existing maps provided by other scientists or various institutions. For example, members of the Working Community for Spatial Research of Vienna University repeatedly exchanged maps among each other when preparing the atlas of Gau Niederdonau (Administrative Region of Lower Danube). But maps were likewise swapped among institutions and research

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[3] More about the connection between politics and scientists see: Ash Mitchell G., 'Wissenschaftswandel in Zeiten politischer Umwälzungen: Entwicklungen, Verwicklungen, Abwicklungen'. In: *Internationale Zeitschrift für Geschichte und Ethik der Naturwissenschaften, Technik und Medizin* 3 / 95 (Basel / Boston / Berlin 1995), p. 1; Ash Mitchell G., 'Wissenschaft und Politik als Ressourcen für einander'. In: Bruch Rüdiger vom / Kaderas Brigitte (Hrsg.), *Wissenschaften und Wissenschaftspolitik. Bestandsaufnahmen zu Formationen, Brüchen und Kontinuitäten* (Wiesbaden 2002), p. 32 f.; Ash Mitchell G., 'Umbrüche 1933 / 1938 und 1945 im Vergleich. Konstruierte Kontinuitäten'. In: Ash Mitchell G. (Hrsg.), *Hochschulen und Wissenschaften im Nationalsozialismus. Stand der Forschung und Projekte in Österreich*. CD-Rom (Wien 2003) etc.

[4] Svatek Petra, 'Hugo Hassinger und Südosteuropa. Raumwissenschaftliche Forschungen in Wien (1931-1945)'. In: Sachse Carola (Hrsg.), *„Mitteleuropa" und „Südosteuropa" als Planungsraum. Wirtschafts- und kulturpolitische Expertisen im Zeitalter der Weltkriege* (Göttingen 2010), p. 290-311; Svatek Petra, '„Wien als das Tor nach dem Südosten" – Der Beitrag Wiener Geisteswissenschaftler zur Erforschung Südosteuropas während des Nationalsozialismus'. In: Ash Mitchell G. / Nieß Wolfram / Pils Ramon (Hrsg.), *Geisteswissenschaften im Nationalsozialismus: Das Beispiel der Universität Wien* (Göttingen 2010), p. 111-139.

[5] Hassinger was born on 7 November 1877 in Vienna as the son of a bank employee. In 1896 he began his study of Geography, Geology and History at Vienna University. After his doctors degree he worked as a teacher at different schools in Vienna and Moravia. In 1914 he was appointed to a lecturer at the Department of Geography at Vienna University. Starting from 1918, he worked as a professor at the Departments of Geography at the universities of Basel (Switzerland) and Freiburg (Germany). In 1931, he returned to Vienna, where he was appointed professor of cultural geography. Hassinger died on 13 March 1952 after a road accident. (Götzinger Gustav, 'Hugo Hassinger, 1877-1952'. In: *Mitteilungen der Geographischen Gesellschaft Wien* 96, Wien 1954, p. 149-176; Zippel Christine, 'Hugo Hassinger', In: Fahlbusch Michael / Haar Ingo, *Handbuch der völkischen Wissenschaften*, München 2008, p. 226-230; Svatek Petra, Hassinger, p. 290-311; etc.)

[6] Svatek Petra, 'Wiener Geisteswissenschaftler', p. 122.

[7] More about the "Southeast German Research Association" see: Fahlbusch Michael, *Wissenschaft im Dienste der nationalsozialistischen Politik? Die „Volksdeutschen Forschungsgemeinschaften" von 1931-1945* (Baden-Baden 1999); Fahlbusch Michael, 'Wilfried Krallert'. In: Fahlbusch Michael / Haar Ingo, *Handbuch der völkischen Wissenschaften* (München 2008), p. 335-337.

[8] More about the "South-Eastern German Institute" see: Promitzer Christian, 'Täterwissenschaft: das Südostdeutsche Institut Graz'. In: Beer Mathias / Gerhard Seewann (Hrsg.), *Südostforschung im Schatten des Dritten Reiches. Institutionen – Inhalte – Personen* (München 2004), p. 93-113.

groups, e.g. between the Department of Geography of Vienna University and the "South-east German Research Association". The atlas of Gau Niederdonau was to provide a new starting-point for researching the Gau Lower Danube, assist spatial planners and create preconditions for the redesign of the German lebensraum for the purposes of the Third Reich. This cartographic work was deliberately organised in a multidisciplinary fashion by involving numerous scientists from many different institutions and fields of study.[9]

Yet not all maps received from other institutions were submitted voluntarily to the various research communities. This is e.g. documented by the map acquisitions of "South-east German Research Association" in the context of the Special Commando Künsberg. Here Wilfried Krallert and other members had the task of securing maps kept at various Belgrade institutions and considered significant for ethnicity research. During the operations conducted in mid-April 1941, they managed to obtain mostly very valuable cartographic material from the Statistical Bureau, the Institute of Military Geography and the University's Department of Geography, which all arrived in Vienna for further processing in late April. Inter alia, this material comprised the updated version of the map of Yugoslavia, formerly unknown in Germany (scale 1:25,000), maps of the country scaled at 1:50,000 and 1:100,000 as well as some remaining parts of the old map of Serbia (various scales); the latter was dispatched to the Main Cartographic Division XIV situated in Vienna. Initially, Krallert's task lay in evaluating the material captured and relaying it to different authorities for further processing. In due course, "South-east German Research Association" was upgraded to something resembling a distribution centre. Moreover, the cartographic department staff promptly made use of the data obtained to create in co-operation with the Department of Geography of Vienna University new ethnographic maps, which provided important sources for ethnographic "consolidation" by the SS and "Wehrmacht".[10]

The accuracy of the thematic data featured in these maps of the "South-east German Research Association" undoubtedly exceeded all older documents. Above all the highly detailed presentation style using an absolute value method permitted precise inferences regarding the distribution and size of the individual ethnic groups. In addition to describing the German, Slovak, Czech, Magyar, Slovene, Romanian, Bulgarian, Russian, Ukrainian, etc. ethnic groups, the maps also provided information about the Jewish population. For this reason, it cannot be excluded that they were used as sources for the deportation and annihilation of the Jewish people.[11]

After the Yugoslavian campaign, the contacts between "South-east German Research Association" staff members and SS Special Commandos continued from summer 1941 to 1944 in Russia through participation in the looting of several statistical offices and institutes of geography. Moreover, Krallert issued specific orders to seize the documents of various Leningrad institutions. In 1941, Krallert took part in these raids by joining forces with the "Einsatzkommando Nürnberg" (Mobile Commando Nuremberg) and looting a number of institutions in Kiev, Kharkov, Odessa, Simferopol and Sevastopol as well as, later on, in the Northern Caucasus region for strategic material. From this material, the staff members of the "Publication Office" e.g. developed a nationalities map of Crimea (scale 1:400,000) and an ethnographic map of the Caucasus region (scale 1:1.000,000). Inter alia, this material was used for the planned resettlement of ethnic Germans from the Volga and Black Sea regions.[12] These raids made Wilfried Krallert an excellent expert on Yugoslav and Soviet Russian cartography, whose know-how he drew upon e.g. in an article on the "Methodology in the Field of Soviet Russian Cartography" written for the "Deutsches Archiv für Landes- und Volksforschung"[13] in 1943.

Apparently, "South-east German Research Association" was ordered by the Foreign Office to produce new maps by combining older cartographic material with statistical data and to relay the output to other authorities and institutions of the German Reich and the South-eastern European states. These maps were then relayed to the Foreign Office, from where they were sent for example to the general staff of the army and air force as well as to the "Volksdeutsche Mittelstelle" (Ethnic Germans Welfare Office).[14]

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[9] More about the atlas of Gau Niederdonau see: Svatek Petra, 'Raumforschung, NS-Politik und der Gauatlas Niederdonau'. In: Brandstetter Thomas / Rupnow Dirk / Wessely Christina (Hrsg.), *Sachunterricht. Fundstücke aus der Wissenschaftsgeschichte* (Wien 2008), p. 88-93.

[10] Fahlbusch Michael, *Wissenschaft*, p. 480-493; Fahlbusch Michael, 'Im Dienste des Deutschtums in Südosteuropa: Ethnopolitische Berater als Tathelfer für Verbrechen gegen die Menschlichkeit'. In: Beer Mathias / Seewann Gerhard (Hrsg.), *Südostforschung im Schatten des Dritten Reiches. Institutionen – Inhalte – Personen* (München 2004), p. 195. More information about the Special Commando Künsberg you can find in PAAA (Political Archive of the Foreign Office Berlin) R 27531.

[11] Most of these maps you can find e.g. in the Austrian National Library in Vienna and in the British Library in London.

[12] Fahlbusch Michael, *Wissenschaft*, p. 487, 490, 659; more information about the Mobile Commando Nuremberg you can find in: PAAA R 27557 and PAAA R 27539.

[13] Krallert Wilfried, 'Die Planmäßigkeit auf dem Gebiet sowjetrussischer kartographischer Arbeiten. Ein Beitrag zur Kenntnis der sowjetischen Kriegsvorbereitungen auf einem wissenschaftlichen Teilgebiet'. In: *Deutsches Archiv für Landes- und Volksforschung* 7 (Leipzig 1943), p. 12-44.

[14] Fahlbusch Michael, *Wissenschaft*, p. 634; Fahlbusch Michael, 'Publikationsstelle Wien'. In: Haar Ingo / Fahlbusch Michael (Hrsg.), *Handbuch der völkischen Wissenschaften* (München 2008), p. 500; PAAA R 100469 (Letter Gertrud Krallert to „Reichsministerium für die besetzten Ostgebiete" 16. 9. 1943); PAAA R 27531 (note Krallert 3. 9. 1941).

Likewise, it seems that the thematic maps of the "South-Eastern German Institute" of Graz were mostly commissioned and circulated by political authorities. A key role in this process was assumed by the Graz-based geographer Helmut Carstanjen, who not only served as director of the institute but was also a member of the German-Croatian resettlement commission. This body was not only concerned with the relocation of Croatian nationals and ethnic Croatians from Lower Styria and the occupied zones of Carinthia and Carniola to the territory of the Independent State of Croatia, but also with the deportation of Slovenes and the resettlement of ethnic Germans.[15] It was likely in the context of this activity that the maps developed by the "South-Eastern German Institute" were circulated by him in co-operation with National Socialist institutions. Most ethnographic maps dealing with Croatia were created by the geographer Manfred Straka. One such example is the map "German Settlements in the Independent State of Croatia", which indicates the relative share of the German-language population in the administrative units of the country as well as the absolute number of ethnic Germans in its larger settlements.

However, scholars also made thematic maps available to NS institutions of their own accord without having been requested to do so by political authorities. These maps were mainly produced by staff members of the Working Group for Spatial Research of Vienna University. Many cartographic studies were not commissioned from the Working Group by outside parties; rather, its director Hugo Hassinger initiated politically relevant projects on his own, as is e.g. documented by the resettlement maps for ethnic Germans in South-eastern Europe. After Hassinger had been charged in 1939 by the "Verein für das Deutschtum im Ausland" (Association for Germans and German Culture Abroad) to develop his first study on the resettlement of ethnic Germans[16], he launched several initiatives in subsequent years to counteract the massive scattering of ethnic Germans in South-eastern Europe. His statement[17] in the proposal for a project on the "Re-arrangement of the Areas Inhabited by Ethnic Germans in the Inner Carpathian Region" clearly demands that science be prepared for whenever such resettlement issues should become pressing. This proves that Hassinger had intended the political slant of the study from the beginning without previous requests on the part of any authority or politician and was certainly willing to submit the study results. The same applies for the project on the "Status Quo of Ethnic German Groups along the Middle Reaches of the Danube". In both projects, thematic maps were planned partly in co-operation with ethnic German groups in South-eastern Europe; after completion, these were to be made available not only to the "Reichsarbeitsgemeinschaft für Raumforschung", but to political institutions as well.[18]

Another example is the atlas of Croatia, which likewise had not been commissioned by any political body. The atlas of Croatia was edited by Captain Alois Jaschke, who had mainly fought in Galicia and on the southern front during the First World War and later established a reputation through numerous scientific publications. The atlas was submitted for funding to the "Reichsarbeitsgemeinschaft" in the 1943/44 budget year as the research project "The economic basis of the independent State of Croatia". It was the objective of this study to provide for a general overview of Croatia's economic situation and social conditions as well as to examine the efficiency of the Croatian economy and its controllability. Hassinger submitted the atlas and other maps he had received from South-eastern European scholars to the "Forschungsstaffel zur besonderen Verwendung" (Special Research Unit, always referred to as "Forschungsstaffel z. b. V.") based in Neudorf.[19] Answerable to the Wehrmacht, the "Forschungsstaffel z. b. V." was set up in April 1943. Until the end of the war, it conducted terrain evaluations in unexplored or little known territories and also developed a number of maps in this context. The unit was composed of scholars from all disciplines of the natural sciences as well as seasoned military personnel (sappers, experts on armoured vehicles, aircraft and mountain infantry).[20] When Hassinger entered into contact with the unit, it was mainly busy with mapping parts of Dalmatia and hence found the material provided by him very useful. Due to the submission of these maps at Hassinger's behest, he was appointed to the "Working Community for Military-Scientific Terrain Research and Military Geology" in late 1944.[21]

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[15] Promitzer Christian, Täterwissenschaft, S. 105; PAAA R 100644 (Deportation of Croatians).

[16] UAW, unpublished works done by Hassinger, box 15 (note of the Working Group for Spatial Research of Vienna University 1. 11. 1940).

[17] UAW, unpublished works done by Hassinger, box 16 (request for a project 1941): "Auf jedem Fall soll rechtzeitig die wissenschaftliche Rüstung vorbereitet sein, wenn eine dieser Umsiedlungsfragen politisch angeschnitten werden sollte, um nicht von den Tatsachen überrascht zu werden, wie das teilweise im Nordosten geschehen ist."

[18] Svatek Petra, 'Hassinger', p. 308.

[19] UAW, unpublished works done by Hassinger, box 18 (letters Hassinger to the "Forschungsstaffel" 28. 9. 1944, 13. 11. 1944, 9. 12. 1944, 12. 1. 1945).

[20] Boehm Erwin, 'Aufbau und Einsatz der Forschungsstaffel z. b. V'. In: Boehm Erwin / Brucklacher Walter / Pillewizer Wolfgang, *Luftbildinterpretation und Geländevergleich. Die Tätigkeit der Forschungsstaffel von 1943-1945*. Österreichische Akademie der Wissenschaften. Berichte und Informationen (Wien 1989), p. 9-15; Häusler Hermann, *Forschungsstaffel z. b. V. Eine Sondereinheit zur militärgeografischen Beurteilung des Geländes im 2. Weltkrieg*. MilGeo 21 (Wien 2007).

[21] UAW, unpublished works done by Hassinger, box 18 (letter Hassinger to the "Reichsforschungsrat" 4. 12. 1944, letter Hans Spreitzer to Hassinger 12. 1. 1945).

Finally, I would like to draw attention to a map project involving staff members of multiple research groups: the atlas "The Danube-Carpathian Region". Collaborators in this project included not only scholars of the "Old Empire" and of Vienna University but also members of the Working Community for Spatial Research, "Southeast German Research Association" and "Southeast European Society". The new edition lobbied for by the Wehrmacht and commissioned by the "Welfare Office for Ethnic Germans" was to benefit the war policy of the period. Several sources document that the maps designed by Hassinger were sent by the geographer Egon Lendl (1906-1989) on behalf of the "Welfare Office" to the Stuttgart University of Agriculture, which co-ordinated the atlas project.[22]

References

ASH Mitchell G. (1995), Wissenschaftswandel in Zeiten politischer Umwälzungen: Entwicklungen, Verwicklungen, Abwicklungen. In: *Internationale Zeitschrift für Geschichte und Ethik der Naturwissenschaften, Technik und Medizin* 3 / 95 (Basel / Boston / Berlin), p. 1-21.

ASH Mitchell G. (2002), Wissenschaft und Politik als Ressourcen für einander. In: BRUCH Rüdiger vom / KADERAS Brigitte (Hrsg.), *Wissenschaften und Wissenschaftspolitik. Bestandsaufnahmen zu Formationen, Brüchen und Kontinuitäten* (Wiesbaden), p. 32-51.

ASH Mitchell G. (2003), Umbrüche 1933 / 1938 und 1945 im Vergleich. Konstruierte Kontinuitäten. In: ASH Mitchell G. (Hrsg.), *Hochschulen und Wissenschaften im Nationalsozialismus. Stand der Forschung und Projekte in Österreich*. CD-Rom (Wien 2003).

BOEHM Erwin (1989), Aufbau und Einsatz der Forschungsstaffel z. b. V. In: BOEHM Erwin / BRUCKLACHER Walter / PILLEWIZER Wolfgang, *Luftbildinterpretation und Geländevergleich. Die Tätigkeit der Forschungsstaffel von 1943-1945*. Österreichische Akademie der Wissenschaften. Berichte und Informationen (Wien), p. 9-15.

FAHLBUSCH Michael (1999), *Wissenschaft im Dienste der nationalsozialistischen Politik? Die „Volksdeutschen Forschungsgemeinschaften" von 1931-1945* (Baden-Baden).

FAHLBUSCH Michael (2004), Im Dienste des Deutschtums in Südosteuropa: Ethnopolitische Berater als Tathelfer für Verbrechen gegen die Menschlichkeit. In: BEER Mathias / SEEWANN Gerhard (Hrsg.), *Südostforschung im Schatten des Dritten Reiches. Institutionen – Inhalte – Personen* (München), p. 175-214.

FAHLBUSCH Michael (2008), Wilfried Krallert. In: FAHLBUSCH Michael / HAAR Ingo, *Handbuch der völkischen Wissenschaften* (München), p. 335-337.

FAHLBUSCH Michael (2008), Publikationsstelle Wien. In: HAAR Ingo / FAHLBUSCH Michael (Hrsg.), *Handbuch der völkischen Wissenschaften* (München), p. 496-504.

GÖTZINGER Gustav (1954), Hugo Hassinger, 1877-1952. In: *Mitteilungen der Geographischen Gesellschaft Wien* 96, Wien, p. 149-176.

HÄUSLER Hermann (2007), *Forschungsstaffel z. b. V. Eine Sondereinheit zur militärgeografischen Beurteilung des Geländes im 2. Weltkrieg*. MilGeo 21 (Wien).

KRALLERT Wilfried (1943), Die Planmäßigkeit auf dem Gebiet sowjetrussischer kartographischer Arbeiten. Ein Beitrag zur Kenntnis der sowjetischen Kriegsvorbereitungen auf einem wissenschaftlichen Teilgebiet. In: *Deutsches Archiv für Landes- und Volksforschung* 7 (Leipzig), p. 12-44.

LEENDERTZ Ariane (2008), Reichsarbeitsgemeinschaft für Raumforschung. In: FAHLBUSCH Michael / HAAR Ingo, *Handbuch der völkischen Wissenschaften* (München), p. 520-527.

PROMITZER Christian (2004), Täterwissenschaft: das Südostdeutsche Institut Graz. In: BEER Mathias / GERHARD Seewann (Hrsg.), *Südostforschung im Schatten des Dritten Reiches. Institutionen – Inhalte – Personen* (München), p. 93-113.

RÖSSLER Mechtild (1987), Die Institutionalisierung einer neuen Wissenschaft im Nationalsozialismus: Raumforschung und Raumordnung 1935-1945. In: *Geographische Zeitschrift* 75 (Stuttgart), p. 177-194.

[22] UAW, unpublished works done by Hassinger, box 19 (several notes of the working group "Kartenwerk Südost" and letters from Hassinger and Lendl 1944); Svatek Petra, Hassinger, p. 306.

SVATEK Petra (2008), Raumforschung, NS-Politik und der Gauatlas Niederdonau. In: BRANDSTETTER Thomas / RUPNOW Dirk / WESSELY Christina (Hrsg.), *Sachunterricht. Fundstücke aus der Wissenschaftsgeschichte* (Wien), p. 88-93.

SVATEK Petra (2010), Hugo Hassinger und Südosteuropa. Raumwissenschaftliche Forschungen in Wien (1931-1945). In: SACHSE Carola (Hrsg.), *„Mitteleuropa" und „Südosteuropa" als Planungsraum. Wirtschafts- und kulturpolitische Expertisen im Zeitalter der Weltkriege* (Göttingen), p. 290-311.

SVATEK Petra (2010), „Wien als das Tor nach dem Südosten" – Der Beitrag Wiener Geisteswissenschaftler zur Erforschung Südosteuropas während des Nationalsozialismus. In: ASH Mitchell G. / NIEß Wolfram / PILS Ramon (Hrsg.), *Geisteswissenschaften im Nationalsozialismus: Das Beispiel der Universität Wien* (Göttingen), p. 111-139.

VENHOFF Michael (2000), *Die Reichsarbeitsgemeinschaft für Raumforschung (RAG) und die reichsdeutsche Raumplanung seit ihrer Entstehung bis zum Ende des Zweiten Weltkrieges 1945*. Arbeitsmaterial der Akademie für Raumforschung und Landesplanung 258 (Hannover).

ZIPPEL Christine (2008), Hugo Hassinger, In: FAHLBUSCH Michael / HAAR Ingo, *Handbuch der völkischen Wissenschaften*, München, p. 226-230.

Archival sources:

PAAA (Political Archive of the Foreign Office Berlin) boxes R 27531, R 27539, R 27557, R 100469 and R 100644.

UAW (Archive of the University of Vienna), unpublished works done by Hugo Hassinger, boxes 15, 16, 18 and 19.

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HOW COULD AUTHOR BIBLIOGRAPHIES BE USED FOR THE HISTORY OF SCIENCE?

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Abstract

The communication will display a bibliometrical survey of personal bibliographical indexes of authors in different countries; an attempt will be made to select the science field. Universal Decimal Classification (UDC) index will be used to select records from the electronic catalogues of national libraries of the world: http://www.library.uq.edu.au/natlibs/ *(107 countries). Data will be collected only from the libraries which use UDC for systematising records. The UDC index 012 describes "Author bibliographies. Individual bibliographies" (*http://www.udcc.org*). Number of personal bibliographies will be compared to the general number of records in a catalogue of a national library of a country, so indicating the standing of a source in a country. Only records of books published after 1945 will be considered.*
The tradition of bibliographical indexes has a long history: from personal lists of scholarly works published in the first scientific magazines to a personal bibliography management software. The tradition of compiling an index of published works is well considered in academic world in many countries. Personal bibliography of a scholar, who initiated a scientific school or developed a certain trend in science, is a historical source, as it gives a chronological picture of science, as well as data about co-authorship.
Personal bibliographies are both issued as books and as internet sources, i.e. in HYLE (International Journal for Philosophy of Chemistry): "Bibliography on Boyle, –1940": 455 titles, compiled by the Robert Boyle Project (Michael Hunter); "Bibliography on Lavoisier, –1794": 1,179 titles, compiled by Panopticon Lavoisier (Marco Beretta); "Bibliography on Liebig, –1997": 242 titles, compiled by William Brock, etc.
The communication will compare data of published personal bibliographical indexes in different sciences with reference to the catalogue of the Vrublevskiai (Wroblewski) Library of the Lithuanian Academy of Sciences. Our tradition of 50 years in compiling such bibliographical indexes for famous scholars will be presented, as well as the discussion will be raised for the real need and importance of such activity. Some examples of indexes will be presented to the audience.

Introduction

Although the title question of this paper is not usually questioned by the historians of science, we shall try to evaluate the readiness of libraries for global surveys on the topic. More and more libraries launch electronic catalogues; try to open their holdings for interactive searches. The idea to include all holdings to electronic catalogues is a pursuance, though not easily achieved in some countries.

Author bibliography, as a result of bibliographical activity, is intended to register the works by and about a person; it gives a chronological picture of science, also data about co-authorship. For a scientific school, followers of historians of a particular branch of science, such bibliographical tool is usually the most valuable.

Historical background

Bibliography producers are usually libraries, scientific societies and very seldom private individuals. What encourages compilers to start and finally to complete such time-consuming, hard work? It's usually the inexorable desire to serve the community and save the labour of the followers by registering what was already achieved in the field. A primary bibliographic function is ***recording*** (enumerative) –listing individual items because, before books or articles can be studied, they must be known to exist. The vital factor in enumerative bibliography is that it should be *noncritical* and *complete*.[1]

This function has a clear distinction from a ***selecting*** activity, which turns technical activity of a compiler into a research work: select the best, relevant items from the infinite. Each single subject bibliography could be considered as a piece of subject history.

The tradition of bibliographical indexes have a long history: from author lists of scholarly works, published in the first scientific magazines to author bibliography management software.

Individual bibliographies are both issued as books and as internet sources, i. e. in HYLE (International Journal for Philosophy of Chemistry): "Bibliography on Boyle, –1940": 455 titles, compiled by the Robert Boyle Project (Michael Hunter); "Bibliography on Lavoisier, –1794": 1,179 titles, compiled by Panopticon Lavoisier (Marco Beretta); "Bibliography on Liebig, –1997": 242 titles, compiled by William Brock, etc.

Great attention was shown to the bibliographical practice of science history by the International Union of the History and Philosophy of Science, which launched the project "World History of Science Online (WHSO): databases of bibliographical and archival sources" in 2004.[2]

Methodology

Initially, bibliographical indexes are "(…) a systematic list of writings by a given author or on a given subject"[3]. Such bibliographical source usually serves as a source for the history of a given branch of science. The idea to give a world overview of author bibliographies was based on some brave presumptions to access:

1. National libraries intend to collect all published material of a country (nation).
2. Holdings of national libraries are accessed via electronic catalogues.
3. Records in electronic form are searchable using Universal Decimal Classification.

A bibliometrical survey of an author's bibliography was based on electronic catalogues held by national libraries. Criteria for investigation: the records of the electronic catalogue were based on Universal Decimal Classification (UDC). This classification system is widely used in research and academic libraries, but is not the only one. Its popularity is shared with Dewey Decimal Classification (DDC), the Library of Congress classification system (LCC), etc.

The UDC index 012 describes "Author bibliographies. Individual bibliographies"; it covers bibliographies of editions of, or writings about, the works of a particular author, or particular anonymous works.[4]

For the survey a list will be used of national libraries in 204 countries, arranged by the interactive website of the national libraries[5].

The number of national libraries with a homepage and interactive electronic catalogue will be compared, as well as the number of libraries using UCD for cataloguing.

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[1] Bibliography *Encyclopedia of library and information science*, vol. 2. New York; London, 1969, p. 407–408.

[2] WHSO Project. Accessed on 2001 Mach 30 <http://www.dhst-whso.org/description.htm>.

[3] Bibliography *The Oxford Dictionary of Literary Terms*. Chris Baldick. Oxford University Press, 2008. *Oxford Reference Online*. Oxford University Press. Accessed on 15 March 2011 <http://www.oxfordreference.com/views/ENTRY.html?subview=Main&entry=t56.e128>.

[4] Author bibliographies. Individual bibliographies *Universal Decimal Classification.* Accessed on 15 March 2011, <http://www.udcc.org>

[5] *National Libraries.* Accessed on 15 March, 2011 <http://www.nationallibraries.org/>.

Survey

The vast variety of electronic catalogues proves the need of unified means of information search. The first interactive website of National Libraries has been made possible by a partnership between Texas A&M University Libraries and the National Library of Hungary. The objective of the website was to consolidate all existing websites of the National Libraries in 204 countries world-wide to a single platform. The survey turned out to have a double result: the results of author and subject bibliographies we inseparable.

After the survey the following data were identified:

A. National libraries with a homepage: 114 (56% of 204 countries).

B. National libraries with electronic catalogue: 79 (39% of 204 countries).

C. Electronic catalogues using UDC: 20 (25% of B).

D. Author and subject bibliographies, found in interactive electronic catalogues: 7882 (total in C).

Conclusions

Although only a pilot scanning of national library catalogues was performed, such survey should be acclaimed as under-resourced. The survey of the paper should be repeated with more detail, i.e. a questionnaire for a national library and a methodological advice of the WHSO. The project "World History of Science Online (WHSO): databases of bibliographical and archival sources" (The International Union of the History and Philosophy of Science) encourages bibliographical activity of member countries, and offers methodological support[6].

39% of national libraries maintain electronic catalogue.

25% of electronic catalogues maintain UDC.

Author bibliography is a must for each scholar; it should be compiled and made ready for publishing, so saving time and efforts to pupils and followers.

Since the global search is not appropriate for electronic catalogues of the libraries, special bibliographical data should be searched only from specialized databases.

[6] Report on the first results of the Survey on current activities related to the DHS project "World History of Science Online: databases of bibliographical and archival sources" Accessed on 29 March, 2011 <http://www.dhst-whso.org/surveyactivities.htm>.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 1141.

A 'LANGUAGE OF MATHEMATICS': NEUTRALITY AS A FACILITATOR OF CIRCULATION DURING THE COLD WAR[1]

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Abstract

Computing and cybernetics are two fields with many intersections, which often leads to confusion. As Slava Gerovitch has shown (cf. From Newspeak to Cyberspeak*, MIT Press, 2002; 'Feedback of Fear', presentation at 23rd ICHST Congress, Budapest, July 28, 2009), cybernetics and its developments were heavily interconnected with politics on both sides of the Iron Curtain. Computing, on the other hand, was promoted on both sides of the Iron Curtain, and compared to cybernetics provided a more neutral ground for the exchange of ideas and concepts.*

The most neutral part of computing is connected with those parts that are close to mathematic: with programming rather than with the machines themselves. When programmers realised the benefits of sharing the outcome of their work, computer programmes or software, they opted for a programming language "as close as possible to mathematical notation". In my contribution, I will examine how the neutrality of such language –the language of mathematics– facilitated sharing practices, ideas, and results of the endeavour among computer scientists. Special attention will be paid to the spread of the ideas of the Dutch computer programmer Edsger W. Dijkstra and his Discipline of programming *and related works in the community of computer scientists in Czechoslovakia in the 1970s and 1980s.*

Introduction

In the early post-war period computing technology[2] emerged as a new field of science. Other such fields, emerging immediately after WWII, are nuclear physics[3] and cybernetics.[4] Their history, if not reduced to the technical details and

[1] This work resulted from my participation in the 'Software for Europe: constructing Europe through software' project led by Gerard Alberts, Universiteit van Amsterdam, funded as part of the European Science Foundation EUROCORES Programme "Inventing Europe". The research was funded by grant no. INE/07/E008 from the Czech Grant Agency (GAČR).

[2] Paul Edwards, *The Closed World. Computers and Politics of Discourse in Cold War America*. Cambridge, Mass.: MIT Press, 1996; Atsushi Akera, *Calculating a Natural World Scientists, Engineers, and Computers During the Rise of U.S. Cold War Research*. Cambridge, Mass.: MIT Press, 2006.

improvements, is told in the context of the Cold War. The transfer of computing technology in one direction, namely from West (or more precisely from the USA and the United Kingdom) to East, seems unquestioned. Given the origins and the potential of this new technology –this unique relation of thinking and doing[5]– in the military, the advances in the field were closely observed from both sides of the Cold War, with ideology debates and spies also involved.[6]

Analysing history of cybernetics in the Soviet Union, Slava Gerovitch speaks of computers as mathematical machines of the cold war.[7] Some sub-disciplines of computing were not under so much political pressure as others. This comparatively neutral sub-discipline, because theoretical and not bound to the provenance of the machine, is programming. Programming languages, emerging in the second half of 1950s, also grew out of the programmers' wish to share their work. Many of the computing people were advocating the use of a language "as close as possible to mathematical notation". The neutral nature of mathematics facilitated sharing the ideas and results related to theoretical programming.[8]

Very roughly, people active in computing in the late 1940s and 1950s can be divided into two groups: one with a background in electrical engineering and/or physics, and the other with a background in mathematics and/or logic. The first group's experience was of crucial importance in the design of computers, choosing the right components and material. The efforts of the second group were central to programming and implementation of specific mathematical machines as well as to thoughts on what could be automated.[9] In this paper, I analyse the circulation of ideas in these more subtle areas of computing that are connected closely to mathematics, in order to show that this relatively[10] neutral ground provided a relatively uncomplicated passage way for the transfer of knowledge between Western and Eastern Europe.

Cybernetics vs. computing

Debates about computing and cybernetics and their potential usefulness or uselessness were not limited to the Soviet bloc and the influence of ideology on science. Howard Aiken, for example, would not credit a machine with thinking;[11] von Neumann questioned the expectation that a machine made of unreliable elements would work reliably.[12] The term *cyber* has been closely intertwined with computers and their use since the beginnings of the disciplines in the 1940s, which was not just a matter of confusing the two because of personalities like John von Neumann,[13] who contributed to both. Cybernetics in the strict sense, i.e. the science based on comparing the feedback mechanisms in living organisms and in machines, was initially rejected by the ruling party in the Soviet Union, and hence also in other Soviet-bloc countries. As Gerovitch describes

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3 Gabrielle Hecht, *The Radiance of France: Nuclear Power and National Identity after World War II*. Cambridge, Mass.: MIT Press, 1998; Paul R. Josephson, *Red Atom: Russia's Nuclear Power Program from Stalin to Today*. New York: W. H. Freeman and Company, 1999; John Krige, Atoms for Peace, Scientific Internationalism, and Scientific Intelligence. *Osiris*, Vol. 21, No. 1, Global Power Knowledge: Science and Technology in International Affairs (2006), pp. 161-181.

4 Cybernetics in the strict sense derives from Nobert Wiener's seminal publication *Cybernetics; or, Control and communication in the animal and the machine* (New York: J. Wiley, 1948). For the analyses of the West German and Soviet cases, see Philipp Aumann, *Mode und Methode. Die Kybernetik in der Bundesrepublik Deutschland.* (Deutsches Museum, Abhandlungen und Berichte; Neue Folge 24). Goettingen: Wallstein 2009 and Slava Gerovitch, *From newspeak to cyberspeak,* MIT Press, 2002, respectively.

5 Michael S. Mahoney, *The Origins of the "Software Crisis", 1950-1970: Preliminary considerations* Draft of a lecture, 1986. Michael S. Mahoney Papers (CBI 213), Charles Babbage Institute, University of Minnesota, Minneapolis, box 43.

6 See for example Steven Usdin, *Engineering Communism: how two American spied for Stalin and founded the Soviet Silicon Valley.* Yale University Press, 2005. For more subtle connections between politics and science, see John Krige, *American Hegemony and the Postwar Reconstruction of Science in Europe.* MIT Press, 2006; for the role of scientific intelligence therein, see Ronald E. Doel, Does Scientific Intelligence Matter? *Centaurus* vol. 52, 2010, pp. 311-322.

7 Slava Gerovitch, 'Mathematical Machines' of the Cold War: Soviet Computing, American Cybernetics and Ideological Disputes in the Early 1950s. *Social Studies of Science*, Vol. 31, No. 2, *Science in the Cold War* (Apr., 2001), pp. 253-287.

8 One such example is the spread of the ideas of the Dutch computer programmer Edsger W. Dijkstra and his *Discipline of programming* and related works in the community of computer scientists in Czechoslovakia in the 1970s and 1980s. Jiří Hořejš (1933-2001), one of the influential computer scientists in Czechoslovakia, tried to look like Dijkstra: http://www.ics.muni.cz/25let/galerie/dkf/00200005.000_nahled.html [accessed 18 April 2011].

9 One of the human activities to be automated was translation. Cf. Janet Martin-Nielsen, 'It was all connected: Computers and linguistics in early Cold War America', presented at CHOC in Amsterdam, 16 November 2009.

10 See e.g. B. V. Gnědenko and L. Kalužin, Boj materialismu s idealismem v matematice. (The fight of materialism with idealism in mathematics). *Pokroky matematiky, fyziky a astronomie*, vol. 1 (1956), no 3, pp. 289-295 and no. 4, pp. 437-442. (In Czech, two parts, translated from German transcript of the lecture given on 30 April 1954 at Technische Hochschule Dresden.)

11 Robert Slater, *Portraits in Silicon,* MIT Press, 1989, p. 81.

12 John von Neumann, Probabilistic logic and the synthesis of reliable organisms from unreliable computers. In: Claude Elwood Shannon and John McCarthy, eds., *Automata Studies. Annals of Mathematics Studies*. University of Princeton Press, 1956, pp. 43-98.

13 John von Neumann, *The Computer and the Brain.* New Haven: Yale University Press, 1958.

in detail,[14] the situation changed around mid-1950s. Ernest Kolman's article 'What is cybernetics' is often cited as being the one of the keys in this change. The article, published in *Voprosy filosofii* in 1955,[15] triggered changes towards warm reception of cybernetics also in Czechoslovakia. In Prague, cybernetics was discussed by a group of researchers publishing under the pseudonym K. Vasspeg. The name *K. Vasspeg* was composed of the initial letters of the researchers' surnames: *K* stood for Jiří Klír, *Va* for Milan Valach, *S* for Spiro, another *S* for Petr Sehnal, *Pe* for Pelikán, and *G* for Ján Gecsei, all of them young computer researchers[16] participating in the design of Czechoslovak computers. Hence, the connection between computers and cybernetics appears even more strongly.

In contrast to cybernetics, computer was regarded as a tool, as electronic computers were expected to substitute human computers. By doing so, mathematical machines would free the hands and minds of people, thus helping the advancement of knowledge. In that sense, the purpose of computers was the same as in the West. As Slava Gerovitch has shown,[17] cybernetics and its developments were heavily interconnected with politics on both sides of the Iron Curtain. Computing, on the other hand, was promoted on both sides of the Iron Curtain and compared to cybernetics, provided a more neutral ground for the exchange of ideas and concepts.[18]

Computers as a research aid

The effects of the Cold War situation on the development of computing in Czechoslovakia cannot be reduced to the simple problem of the impossibility to freely purchase computers or spare parts for them. The Czechoslovak computer Antonín Svoboda spent most of World War II at MIT in Radiation laboratory, designing analog devices. Upon his return home, he wanted to teach mathematics at the technical university in Prague, although his background was in electrical engineering and physics. Svoboda's activities in computing in Czechoslovakia included the military (VTU), the production of computing machines (Aritma), and the theoretical design of computers. In this paper, the last of the three is the point of entry.

Svoboda presented his first proposal for an electronic digital computer in the Research Institute for Mathematics of the Czech Academy of Sciences and Arts upon his return from the UNRRA-sponsored trip in September 1947.[19] The relations between computing and mathematics in Czechoslovakia were institutionally quite strong: the Czechoslovak Academy of the Sciences, established in November 1952, had its Mathematical Institute, part of which was the department for information processing machines.[20]

In the early 1950s, Antonín Svoboda, the designer of the first Czechoslovak computers, urged researchers to find problems that can be fruitfully solved by computer.[21] The computer was developed in the vicinity of the Mathematical Institute of the Czechoslovak Academy of Sciences, which would also supply tasks for the computer and in fact were relying on the computer to carry out the tedious calculations connected e.g. to the construction of dams.[22] Svoboda's plea seems to turn the relation upside down by saying that computers need work, but interpreting his plea in this way would be derogatory. Rather, the mutual interaction between mathematicians and the emerging field of computing is a general feature: departments of applied mathematics and laboratories for computing soon grew into departments *per se*.

The strategy of the Czechoslovak Academy of the Sciences was to catch up and overcome, and thus contacts with researchers in the West were encouraged in the sciences and in technology,[23] unlike in the humanities. Furthermore,

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[14] Slava Gerovitch, *From Newspeak to Cyberspeak.* MIT Press, 2002.

[15] The article was translated into Czech by J. Klánská and published in 1956, see E. Kolman, Co je to kybernetika [What is cybernetics]. *Pokroky matematiky, fyziky a astronomie*, vol. 1 (1956), no. 2, 202-211.

[16] George J. Klir, Facets of Systems Science. *IFSR International Series on Systems Science and Engineering*, vol. 7, New York and London: Plenum Press, 1991, pp. 338-339.

[17] Slava Gerovitch, *Feedback of Fear*, presentation at 23rd ICHST Congress, Budapest, July 28, 2009.

[18] In his book Gerovitch differentiates between computing and cybernetics, claiming that computer scientists "jumped on the bandwagon of cybernetics" when asking for support for computer science at the point when cybernetics received official support from the communist party (Gerovitch, 2002, p. 98).

[19] Petr Vysoký, Professor Svoboda, professor Trnka and first courses on Computers at Czech Technical University. In: Folta, J., ed., 2001, *Computing Technology Past and Future. Acta historiae rerum naturalium necnon technicarum — Prague Studies in the History of Science and Technology, New series*, Vol. 5. National Technical Museum in Prague 2001, 25–35.

[20] See also Helena Durnová, Sovietization of Czechoslovak computing: the rise and fall of the SAPO project, *IEEE Annals of the History of Computing*, vol. 32 (2010), no. 2, pp. 11-21.

[21] Antonín Svoboda, *Z pracovní konference pořádané oddělením strojů na zpracování informací při Matematickém ústavu Československé akademie věd. Časopis pro pěstování matematiky*, vol. 78 (1953), no. 1, pp. 27-30.

[22] Masaryk Institute and Archive of the Academy of the Sciences of the Czech Republic, collection I. sekce ČSAV 1952-1961, box 4, folder 6.

[23] Masaryk Institute and Archive of the Academy of the Sciences of the Czech Republic, collection I. sekce ČSAV, box 21, folder 68.

science and technology was officially in the centre of attention.[24] Researchers from the Institute for Mathematical Machines profited from this encouragement and from their director's contacts within the computing field abroad. Similarly, foreign researchers were invited to Czechoslovakia: H. Greniewski,[25] L. Łukasiewicz, and R. Marczyński from Poland, N. J. Lehmann from East Germany,[26] and also W. Hoffmann from West Germany.[27]

In October 1955, a major international gathering[28] of computing people took place in Darmstadt.[29] This was co-organised by GAMM (Gesselschaft für angewandte Mathematik und Mechanik) and VDE (Verband deutscher Elektrotechniker) in co-operation with DMV (Deutsche Mathematikervereinigung). The local organizer was Alvin Walther,[30] director of the Institute for Practical Mathematics in Darmstadt and the conference was attended by over 500 researchers from 15 countries. A fifth of the participants were from abroad, twenty of the contributions were devoted to developments in Czechoslovakia,[31] the Soviet Union,[32] Sweden,[33] the Netherlands,[34] and other European countries.

In their keynote lectures, Alston Scott Householder, Heinz Rutishauser, and Howard Aiken all emphasized the connection of mathematics and computation. According to Householder, mathematicians had to learn something that Gauss did not know, namely programming.[35] Rutishauser further pointed out that mathematical formulation was the first step in solving a problem through automatic computation.[36] Aiken brought attention to the single objective of mechanization of numerical analysis which stood at the beginning of the modern efforts to construct large computers.[37] In short, the Darmstadt conference on electronic digital computers and information processing could also be presented as a mathematical conference.[38]

In September 1957, a member of the above-mentioned IPM, Walter Hoffmann, came to give lectures[39] on the development of electronic computing machines with regard to the situation in West Germany.[40] The lectures were co-organized by the Union of Czechoslovak Mathematicians and Physicists and two institutes of the Czechoslovak Academy of Sciences: the Mathematical Institute and the Institute for Mathematical Machines of ČSAV, which underscores the interest of mathematicians in the development of computing.

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[24] Paul R. Josephson, "Projects of the Century" in Soviet History: Large-Scale Technologies from Lenin to Gorbachev. *Technology and Culture*, vol. 36, no. 3 (Jul., 1995): 519-559.

[25] Zprávy (Notices), *Časopis pro pěstování matematiky*, vol. 76 (1951), no. 1, pp. 71-72 and no. 3, p. 230.

[26] František Svoboda, Třetí celostátní konference o strojích na zpracování informací. *Časopis pro pěstování matematiky*, vol. 80 (1955), no. 2, pp. 255-258.

[27] Ivo Babuška, Zprávy. *Časopis pro pěstování matematiky*, vol. 83 (1958), no. 2, p. 252.

[28] Antonín Svoboda's colleague Miroslav Valach described this gathering as "the first world conference" on the topic. AAV, collection I. sekce ČSAV, box 11, folder 24, Ústav matematických strojů, letter to support electing Antonín Svoboda academician.

[29] Wosnik, Johannes, ed., *Elektronische Rechenmaschinen und Informationsverarbeitung / Electronic Digital Computers and Information. Proceedings of the Darmstadt GAMM/VDE/DMV, NTF* [*Nachrichtentechnische Fachberichte*], supplement of NTZ, vol. 4, Friedr. Vieweg & Sohn, Braunschweig, 1956 (further referred to as *Darmstadt*, 1955).

[30] Alwin Walther (1898-1967) was the founder and the director of the Institute for Practical Mathematics at the Technical University of Darmstadt. Cf. Schwetlick, H., Alwin Walther (6. Mai 1898-4. Januar 1967). Ein Pionier des Wissenschaftlichen Rechnens. *ZAMM _ Z. Angew. Math. Mech.* 80 (2000) 1, 5-8; W. de Beauclair, Alwin Walther, IPM, and the Development of Calculator/Computer Technology in Germany, 1930-1945. *Annals of the History of Computing*, 1986, 8, 334-350. I would like to thank Ulf Hashagen for providing me with this reference.

[31] Antonín Svoboda, ARITMA Calculating Punch, in *Darmstadt 1955*, pp. 72, Jan Oblonský, Some features of the Czechoslovak Relay Computer SAPO, in *Darmstadt 1955*, pp. 73-75.

[32] S. A. Lebedew, BESM, eine schnellaufende elektronische Rechenmaschine der Akademie der Wissenschaften der USSR. In: *Darmstadt 1955*, pp. 76-79; J. J. Basilewski, Die universelle Elektronen-Rechenmaschine URAL für ingenieurtechnische Untersuchungen. In: *Darmstadt, 1955*, pp. 80-86.

[33] Stig Comét, Die Verwendung von BESK. In: *Darmstadt, 1955*, pp. 62-65.

[34] Adriaan van Wijngaarden, Moderne Rechenanlagen in den Niederlanden. In: *Darmstadt, 1955*, pp. 80-81.

[35] Alston Scott Householder, Numerical mathematics from the viewpoint of electronic digital computers. In: *Darmstadt, 1955*, p. 21.

[36] Heinz Rutishauser, Massnahmen zur Vereinfachung des Programmierens. In: *Darmstadt 1955*, p. 26.

[37] Howard Aiken, The future of automatic computing machinery. In: *Darmstadt 1955*, p, 31.

[38] Alvin Walther and Walter Hoffmann, Vorwort, p. v in *Darmstadt, 1955*, p. v.

[39] Ivo Babuška, Zprávy. *Časopis pro pěstování matematiky*, vol. 83 (1958), no. 2, p. 252.

[40] Karel Krištoufek, Excerpts from the lectures of Professor W. Hoffmann. *Aplikace matematiky* 3 (1958), no. 2, pp. 156-159.

Programming computers

In the 1950s, it became clear that giving exact instructions (which later crystallised into programming) to the computer would be a tiring, time-consuming, and error-prone job, and this was not just because machine code was different for different computers. Methods in numerical mathematics had been developed for human computation and other kinds of approximation and their transfer to automated computation was not as straightforward a business as it might have seemed.[41] Olga Pokorná[42] for example, encountered difficulty when implementing an algorithm that should have stopped when the result was zero. However, this zero was too loosely defined: the mathematician using the method would know when to stop, while the computer never arrived to zero, but to some very small number, hence the program would run infinitely. In short, what is now known as basic algorithms to any computer programmer had still to be developed.

The ponderings about the difficulties and tediousness of programming led to the development of automatic coding, programming programs, and systems of automatic programming. Roughly speaking, all the preceding notions refer to the same thing: as the new field developed, programming was done separately for each computer in its machine code. Also at conferences, people would report about the work done in their centre and seldom refer to work of others, as it was not directly transferable.

In Czechoslovakia, the way of giving the instructions to the computer was called *instruction sequence* or *instruction network* since the early 1950s until the early 1960s, when the term was substituted with the words *program* and later *program* or *software.*[43] These two concepts differed in their complexity: while instruction sequence ordered the instructions linearly, instruction network allowed for branching and loops. Thus, in an instruction network, the decision to take one or another branch could be taken in the course of the computation described by the same instruction sequence. Similarly, certain parts of the computation could be repeated in a loop. Like in the USA, Britain, Germany, and the Soviet Union, researchers in Czechoslovakia soon realised that there were intellectual and boring parts of programming.

Without any significant theoretical work having been conducted in the automation of programming in Czechoslovakia, the emergence of ALGOL 60, which promised to become the universal medium for exchanging programs, was warmly accepted upon the publication of ALGOL 60 reports in *Numerische Mathematik* and *Communications of the ACM.* The translation of the full ALGOL 60 report into Czech was finished in 1963 and was a joint work of a group of researchers from the Research Institute for Mathematical Machines. It consisted of three parts: the translation as such was preceded by an introduction into programming and followed by a note on compiler construction. The book soon sold out, to the regret of textbook writers. Textbooks on programming in general and ALGOL 60 in particular began to appear in Czechoslovakia in the early 1960s. Every school and every computer was to have an ALGOL compiler.[44]

Translations and reports

As stated above, the flow of computing technology is usually presented as West-East transfer. While this is the case for the actual machines, the ideas about machines and their use moved across the borders more easily. Translations of Western literature into Russian is well known: many of these translations included references to the Russian translations of the works cited in the original and sometimes, the translators would add new Russian references. The US-based Association for Computing regularly reported about computing technology in the Soviet Union as well as in Central and Eastern Europe, the first report appearing in the *Journal of ACM* in 1956.[45] When the monthly *Communications of ACM* was launched, reports from trips to the Soviet bloc countries appeared there regularly.[46] Despite this, reports published in the 1980s reveal concerns with export of computers[47] and software.[48]

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[41] See e.g. Maarten Bullynck, Liesbeth De Mol, Setting-up early computer programs: D. H. Lehmer's ENIAC computation. *Archive for mathematical Logic*, 2010.

[42] Olga Pokorná, Počátky programování v Československu (Beginnings of computing in Czechoslovakia). *25 let počítačů ve VÚMS* (*25 years of computers in the Research Institute for Mathematical Machines*).*Proceedings of the conference held in Prague, 18-20 November 1975*. VÚMS Praha, 1976. II – J – 1 (3 pages.)

[43] Helena Durnová, Instrukční sítě a fenomén ALGOL 60. To appear in Věda a technika v 60. letech 20. století (*proceedings of conference held in the National Technical Museum in Prague,7-8 December 2010*).

[44] This becomes apparent through the number of university and technical university textbooks on the subject published in the 1960s, many of them named after the local variant of ALGOL 60: Tesla ALGOL, FEL ALGOL, etc.

[45] S. A. Lebedev, The High-Speed Electronic Calculating Machine of the Academy of Sciences of the U.S.S.R. *Journal of ACM*, vol. vol. 3 (1956), no. 3, pp. 129-133.

[46] John W. Carr III, Alan J. Perlis, James E. Robertson, Norman R. Scott, A visit to computation centers in the Soviet Union. *Comm. ACM*, vol 2 (1959),no. 6, pp. 8-20; Nelson M. Blachman, Central European Computers. *Comm. ACM*, vol. 2 (1959), no. 9, pp. 14-18; Morton Nadler, Some notes on computer research in Eastern Europe. *Comm. ACM*, vol. 2 (1959), no. 12, pp. 1-2; Willis H. Ware, ed., Soviet

The development of programming in the Soviet bloc was observed in the West and vice versa: Ershov's 1958 short paper on programming was published in English the same year,[49] and the preliminary report on the international algebraic language[50] was noticed in the Soviet bloc as well. Although both are known especially for their contributions to computing, Andrei Petrovich Ershov was trained as a mathematician and the editor of *Communications of ACM,* Alan Perlis, obtained a Ph.D. in mathematics from MIT. Both Perlis and Ershov are known for their involvement in the development of programming languages ALGOL 60 and ALPHA, respectively.

Conclusion

ALGOL 60 was embraced by researchers in Czechoslovakia and work on a compiler was reported, again in the *Communications of the ACM* in 1963.[51] Beyond that, ALGOL 60 also raised theoretical questions, and as such was approached by mathematicians.[52] In the 1960s, two of them, Jiří Kopřiva and Jiří Hořejš (1933-2001)[53] organised a series of seminars on the theory and practice of programming. This seminar gradually developed into a regular school in computer science, starting in 1974 under the name SOFSEM (short for SOFtware SEMinar). The Dutch programmer Edsger Wybe Dijkstra was a hero of this group, and quite officially so. Thus, the embedding of computer programming in mathematical environment contributed to the spread of the ideas about programming across political and economical boundaries.

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computer technology -- 1959. *Comm. ACM*, vol. 3 (1960), no. 3, pp. 131-166; Nelson M. Blachman, The state of digital computer technology in Europe. *Comm. ACM*, vol. 4 (1961), no. 6, pp. 256-265.

47 S. E. Goodman. 1982. U.S. computer export control policies: value conflicts and policy choices. *Commun. ACM* 25, 9 (September 1982), 613-624.

48 Gold, Charles L. and Goodman, Seymour E. and Walker, Benjamin G., Software: recommendations for an export control policy, *Communications of ACM*, vol. 23, no. 4 (April 1980), pp. 199-207.

49 A. P. Ershov, On programming of arithmetic operations. *Comm. ACM*, vol. 1. (1958), no. 8, pp. 3-6. Originally published in *Doklady Akademii Nauk SSSR*, vol. 118, (1958), no. 3, pp. 427-430.

50 Alan J. Perlis, Karl Samelson, Preliminary report: international algebraic language. *Comm. ACM*, vol. 1 (1958), no. 12, pp. 8-22.

51 John A. Gosden, ed., Report of a visit to discuss common programming languages in Czechoslavakia and Poland, *Comm. ACM*, vol 6 (1963), no. 11, pp. 660-663.

52 Karel Čulík (1926-2002), for more details see Petr Hájek, Zemřel profesor Karel Čulík. Pokroky matematiky, *fyziky a astronomie*, vol. 47 (2002), no. 4, pp. 344-348; Jiří Kopřiva (* 1925), for more details see Jiří Hořejš, Doc. J. Kopřiva šedesátiletý. *Časopis pro pěstování matematiky*, vol. 110 (1985), no. 3, pp. 333-336. The article includes bibliography of Jiří Kopřiva, showing his interest in automatic programming in general and in ALGOL 60 in particular.

53 Renata Ochranová and Miroslav Bartošek, Průkopník informatiky – doc. Jiří Hořejš. Available online at *http://abicko.avcr.cz/archiv/2004/2/obsah/prukopnik-informatiky-docent-jiri-horejs.html* [21 April 2011].

MEDICINE IN THE 20TH CENTURY

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 1148.

SCIENCE, INDUSTRY AND IDEOLOGY IN 20TH-CENTURY CATALONIA: THE LEGITIMATING STRATEGIES FOR THE INSTITUT RAVETLLAT-PLA'S THEORY AND PRODUCTS IN LATIN AMERICA BETWEEN 1919 AND 1939

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Abstract

Besides the controversy in Spain about Jaume Ferran i Clua's Anti-alfa vaccine and Calmette and Guerin's BCG during the first third of the 20th century, the veterinary Joaquin Ravetllat i Estech developed in Catalonia an alternative theory on the tuberculosis etiological variability. His ideas, together with the scientific and economic support of the physician Ramon Pla i Armengol, became fundamentals not only for founding the new Institut Ravetllat-Pla, but also for the fabrication and commercialization of two anti-tuberculosis products: the Suero Ravetllat-Pla and the Hemo-antitoxine Ravetllat-Pla. European hegemonic science was, however, committed to the mono-causality of Koch's bacillus. Therefore, Ramon Pla i Armengol was forced to expand his market to Latin America, generating a wide scientific and commercial network in approximately twenty countries from Latin America and Europe, including Portugal and Belgium. Through local commercial agents, the Institute not only distributed its products, but also spread its scientific ideas in scientific publications, including the journal published and edited by the Institute and entitled La Clínica *(1924-1936).*

The Institut Ravetllat-Pla became a corporation with a strong international projection, mostly thanks to the constitution and consolidation of a solid scientific and commercial network. This favoured the sale of their products, which embedded Pla i Armengol's scientific and ideological principles, and the survival of the Institute until 1980. Such a network model became crucial for exchanging ideas and constructing and developing knowledge. This case study will contribute to better understand historically how industries constituted an important mechanism for scientific legitimacy. Moreover, it will provide evidence of how both market economy and the legal and political definition of consumer products, specific from each country, are significant aspects in the making of science.

The Institut Ravetllat-Pla was founded in 1924. Its main activity was the study of the tuberculosis bacillus etiopathology and also the manufacture of two anti-tuberculosis products (the Ravetllat-Pla Serum and Hemo-antitoxin). The Institute maintained its activity until 1980, surviving four different political regimes while commercializing just these two pharmaceutical products. Interestingly, they were both based upon a scientific theory that was not and has not yet been approved by the official medical institutions. This Institute became a private company when their owners tried to donate the patent to the savings bank "La Caixa", which refused this request arguing that the ideas of the Institute "were not accepted in Paris, London, or Berlin" and therefore they could not be taken seriously[1]. The aim of my research is to determine which were the legitimating strategies that allowed the Institute to successfully spread its theory and products, and at the same time to obtain the approval of many physicians who praised the efficacy of this products in the tuberculosis treatment.

The founder of the Institut Ravetllat-Pla, Ramon Pla i Armengol (1880-1956), was a specialist in tuberculosis (phthisiologist), who never neglected his ideological values. He was a physician of the Catalonian Foundation against Tuberculosis (*Patronato de Cataluña para la lucha contra la tuberculosis)* and the Hospital de la Santa Creu of Barcelona, the founder of the Physicians' Union of Catalonia (*Sindicat de Metges de Catalunya),* and director of the journal *Annals de l'Acadèmia i Laboratori de Ciènces Mèdiques de Catalunya*. He was an active socialist, a member of the Catalanist Union and the Catalonian Federation of the Spanish Socialist Worker's Party, while he was lastly elected for the Catalonian parliament in 1936 as a member of the Socialist Union of Catalonia. Finally, in the wake of the Spanish Civil War, he went into exile, ending in Mexico, only to return to Barcelona in 1948. He died detached from the medical and political circles in his Institute in 1958[2].

Beyond the question about the efficacy of these pharmaceutical products, which by no means is clear so far, I want to propose that "healing success" is a relative concept, which depends on the particular context where it is defined. Thus, to begin with, I am focusing on some specifically historical questions: How do we define the "healing success" of any pharmaceutical product in the first half of the twentieth century? Which are the most important issues involved in a particular geo-historical context: consumers and sales rates; the number of physicians that prescribed the product; or the number of published articles to prove a given scientific theory?

Before the discovery of antibiotics, there was no treatment capable to completely eliminate the tuberculosis bacteria from the patients, notwithstanding people's living conditions in the period. Neither the serum from animals infected with tuberculosis, nor the tuberculin, nor the pneumothorax procedure, nor even the French BCG vaccine, were able to do away with the bacillus. Physicians searched for preventive therapies and ways to help the body to heal itself, but these as best managed to control the spread of the disease[3]. So, the relevant historical question is not if these products did actually cure, but: who and what defined the "healing success" of a given pharmaceutical product in a context where everything somehow worked but did not live up to expectations? And how and why were these products sold until 1980?

After the discovery of *Mycobacterium tuberculosis* by Robert Koch in 1882, the etiology of the disease was established. It was a simple and reductionist approach: one infectious agent, one disease, and one cure[4]. However, the difficulty to produce a specific and safe vaccine, the clinical variability of the disease, and the inability to visualize the bacillus in tubercles and infectious secretions, led to the emergence of new theories aiming at the bacillus variability. In this context, there came about the Ravetllat-Pla theory proposing that the tuberculosis bacterium was a changeable microbial species, with three different forms that are reversible among them, and each one producing a different clinical profile[5]. The new bacterial form discovered by Joaquim Ravetllat i Stech (1871-1923) and Ramon Pla was called "strike bacterium", which they considered the most virulent form of this bacillus. Indeed, the Institute research was focused on these bacteria, and the products developed were based on this form's capacity to trigger an immunological response.

Since the official scientific institutions did not recognize the Ravetllat-Pla theory, the Institute had to seek alternate legitimating strategies for the spreading of both the theory and their products. Some of these strategies were:

1. The creation of the Institute's own scientific journal.

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[1] Pla, Ramon (1945) *Records amb comentaris. Petit pròlec a Mexic.* Barcelona, ms. Ravetllat-Pla Insititute Archive, Universidad Autónoma de Barcelona.

[2] Lugo, Sara (2008) *Ciencia, industria e ideología en la Cataluña del siglo XX. El Instituto Ravetllat-Pla en Sudamérica entre 1924 y 1936*. Available at: http://www.recercat.net/bitstream/2072/42721/1/TR_SaraLugoMarquez.pdf

[3] Molero, Jorge (1990) "La vacunación antituberculosa". *Historia 16*, 172, 81-88.

[4] Molero, Jorge (1989) "La tuberculosis como enfermedad social en los estudios epidemiológicos españoles anteriores a la Guerra Civil". *Dynamis,* 9, 185-223.

[5] Ravetllat, Joaquim; Pla Ramon (1924) *La bacteria de la tuberculosis*. Barcelona, Tipografia Catalana.

2. Unrelenting scientific advertising campaigns, targeting specific publics: general and rural physicians and pediatricians.
3. Building a worldwide scientific-commercial network to promote the circulation of scientific knowledge, mainly about tuberculosis etiology.

An account of the most important features of these legitimating strategies follows. First, the journal *La Clínica. Revista Mensual Hispano-Americana de Ciencias Médicas* [The Clinic. Monthly Journal of Hispano-American Medical Sciences] was published between 1924 and 1936, and edited and sponsored by the Institut Ravetllat-Pla, while this was never stated in their pages. Four hundred and seventy-seven original articles about diverse medical topics were published in the journal; yet, the main subject-matter was tuberculosis, including research about the Ravetllat-Pla theory and products. In this context, the commercial success of the Ravetllat-Pla medicines did not just represent a lucrative activity, as Pla was saying, but also a legitimating keystone for the Ravetllat-Pla scientific theory. This legitimation required and indeed included the empirical proof of the efficacy of the products from many physicians, mostly general and rural physicians and pediatricians. These clinical studies were published in *La Clínica* and collected like comments to be shown in advertisements[6]. In this case, advertising was not only a persuasive technique toward potential customers, but also a way to spread and legitimate the scientific theory.

Fig. 1: Pamphlet example from 1952, showing the cities where the Ratvellat-Pla products were commercialized and tested. Chilean physician Miguel Farbes Yoaga's certificate declaring he tested the Ravetllat-Pla Serum in fifty children, with forty of them improving their physical condition, is reproduced.

One of the legitimating strategies that the Institute used for its consolidation was the creation of an extensive scientific a commercial network, mainly in Latin America. This network was built through commercial agents in each country, who distributed free samples and scientific publicity, issued free subscriptions to *La Clínica* for nearly all local physicians, and directly presented the Ravetllat-Pla theory in the official medical circles. This scientific-commercial network was used in the advertising of the Institute to strengthen its legitimization. Some advertising pamphlets showed the cities where the product was commercialized and tested, as well as places where the theory was discussed and the circulation of scientific

[6] Lugo, Sara (2010) "Entre ciència i publicitat. *La Clínica. Revista Mensual Hispano-americana de Ciencias Médicas* (1924 – 1936)". *Gimbernat* (in press).

knowledge actually took place. Additionally, also in the advertising of the Institute, physicians' certificates declaring their compliance and the success, proved by them, of the Ravetllat-Pla serums were used (figure 1).

Both, the scientific network and the journal allowed a wide scientific exchange between the Institute and the Latin American countries. For instance, the Institute discussed with a Cuban physician, Francisco José Velez (1885-1953) about "nuclear inversion", a phenomenon that takes place in the lymphocytes of tuberculosis patients, by publishing several articles about this topic in the journal. Velez had worked at important hospitals in Barcelona and Paris, had been the Local Health Chief Officer in Mariel (Cuba), and was at the time a phthisiologist at the Covadonga sanatorium in Havana. He was proposing nuclear inversion as a diagnosis method for tuberculosis[7]. Ten articles about Velez's theory were published in the Institute's journal. Among them, one study was made in Barcelona at the Ravetllat-Pla Institute, and later another one was Velez's response to the Institute's approach. Another example is Armando Pareja Coronel (1896-1965), Professor at the University of Guayaquil (Ecuador), and physician at the General Hospital, who researched about the Ravetllat-Pla theory and published his results in the journal[8]. He also presented this research in the Guayas Medical-Surgical Society on July 1925. One last example, illustrating specifically a pattern of scientific knowledge appropriation, comes with the Institute adopting a staining technique invented by Dionisio Cerqueira, a physician from Rio de Janeiro, who never showed any interest in the Ravetllat-Pla theory. This becomes clear in the letters between the Institute and its commercial agent in Brazil[9].

In 1935, Ramon Pla i Armengol made a trip across the Americas, visiting medical institutions and giving lectures in twelve countries. The local press used to inflate scientific events, in this case by publishing many articles announcing the arrival of "an eminent Spanish learned man". My initial assumption was that the relationship between Ramon Pla i Armengol and Latin America was built through official science channels[10]. However, although recognized Brazilian scientists actually received Ramon Pla, they did not really consider his scientific postulates, and because of this, Pla was forced to implement strategies different from those related with the official science, to legitimate the Institute's scientific theory. One of the strategies used by the Institute was to lure renowned scientists into trying and eventually compromising with its theory. For instance, since 1927, the Institute insisted on the importance of making contact with Antonio Cardoso Fontes (1879-1943), an important bacteriologist, who was the former director of the Oswaldo Cruz Institute, and who had a similar theory about the tuberculosis bacteria[11]. Ramon Pla i Armengol studied and quoted Cardoso's theories since 1925. Meanwhile, Fontes quoted the Ravetllat-Pla theory only once and in general terms. Cardoso Fontes held a cordial relation with the Institute. He actually welcomed Ramon Pla during his trip and showed him the laboratories where he worked, but this relation was not officially registered by the Oswaldo Cruz Institute. Indeed, Cardoso Fontes had publicly discredited the advance of the Ravetllat-Pla theory in Brazil. In 1928, upon the presentation at the Medical-Surgical Society of Rio de Janeiro of a research proving the Ravellat-Pla theory by Almeida Magalhães, who was a physician at the Army Policlinic and the Coscadura hospital, also a member of the League, and collaborator of the journal *La Clínica*, Fontes made the following comment, as told by Pla's commercial agent:

> "Having verified the reversibility of the bacillus as argued by you, Dr. Almeida Magalhães presented a paper at the Medical Society in the session held on the 16th of this month, receiving, as you will see, the response of Dr. Fontes, president of the Society, congratulating him for achieving higher happiness than him as, after twenty years of research on the subject, he had not been able to reach such result. Owing to the fact that I had to show Dr. Almeida Magalhães where Dr. Fontes' clinic was located, I happened to know for sure that [Dr. Fontes] had asked him for samples in order to make the experiments himself and thus verify the truthfulness of the proof."[12]

This was taking place when tuberculosis was not a priority for the Brazilian government, as it happened in the second decade of the twentieth century, even though it actually was one of the main causes of death. Indeed, the government public health measures were basically aimed at yellow fever, smallpox and bubonic plague[13]. In 1911 the "Brazilian League against Tuberculosis" –nearly the only organization dealing with this subject– abandoned its philanthropic and charitable discourse and adopted a more progressive approach, based upon scientific knowledge, and claimed for a governmental involvement in

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[7] Vélez, Francisco José (1926) "La inversión nuclear en el diagnóstico precoz de la tuberculosis pulmonar". *La Clínica. Revista Mensual Hispano-Americana de Ciencias Médicas,* 29, p 257-262.

[8] Pareja, Armando (1926) "La tuberculosis bajo varios aspectos". *La Clínica. Revista Mensual Hispano-Americana de Ciencias Médicas,* 21, p 1-9.

[9] Juliá, Buenaventura, letter to Ravetllat-Pla Institute, May 1930, file: 24.6.1.0. Ravetllat-Pla Insititute Archive, Universitat Autònoma de Barcelona.

[10] Ramon Pla's travel reconstruction can be consulted in: Lugo, Sara (2008), *op. cit.* 1.

[11] Souza, Heráclides Cesar (1943) "In Memorian Antonio Cardoso Fontes 1879-1943". *Memorias Instituto Oswaldo Cruz*, 39, nº 2, 1-10.

[12] Juliá, Buenaventura, letter to Ravetllat-Pla Institute, on 25th October 1930, file: 24.6.1.0. Ravetllat-Pla Insititute Archive, Universitat Autònoma de Barcelona.

[13] Nascimento, Dilene Raimundo (2005) *As pestes do século XX. Tuberculose e Aids no Brasil, uma história comparada*. Rio de Janeiro, Editora Fiocruz.

the fight against the white plague. The main objectives of the League were the establishment of new sanatoriums and to make hospitals take care of the isolation of tuberculosis patients[14]. In 1907, when the first sanatorium funded by the League started to work, the most used medicines were expectorants, antiseptics, astringents, local anaesthetics or restorative products[15]. These products did not contemplate, as a scientific basis, the immunological reaction produced by the tuberculosis bacteria. The Brazilian tuberculosis prophylaxis was influenced by the central medical discourse of the time as shown by the fact that the therapy against the tuberculosis was based in the use of the tuberculin, produced by the Manguinhos Institute and later on by the Oswaldo Cruz Institute, where Antonio Cardoso Fontes actually worked, and BCG, produced since 1918 by "the League" in the Viscondessa de Moraes Institute[16].

Another legitimating strategy for the theory implemented in Rio de Janeiro was to turn big international companies into permanent consumers of Ravetllat-Pla products. This is the case of the Canadian company, Light and Power, a huge worldwide emporium, which was significantly expanding in this period. This is the account from the Brazilian commercial agent:

> "[The Light and Power Company,] owner of phone lines, tramways, electricity, and buses in Rio, Sao Paulo, Bello Horizonte, etc., has declared the Ravetllat-Pla products as the official ones in its branch of its sort of welfare society section for its employees in Rio (only in Rio, it has approximately twelve thousand members with their corresponding families). They were doing experiments with [free] samples but they will start carrying them out with purchased products. They pretend to make a contract [to secure] monthly deliveries of a determined quantity."[17]

Nevertheless, Pla kept the scientific-medical focus of the advertising campaigns. Thus, for instance, in Chile, the sales campaign of the Ravetllat-Pla products failed because of the commercial agent's negligence, precisely because he did not want to use scientific channels for advertising. In the agent's words: "The publicity has to be done not in a scientific way, as it should apparently be, but rather in the Yankee way, as they are used to. So, you should not be surprised if you see some advertisements made in this way"[18].

To which Ramon Pla replied:

> "With scientific publicity occurs the same as with good coal, as it is hard to set alight yet once it has started to burn, it is the one that heats most and burns for a longer time. On the contrary, public advertising is like splinter fire, it burns quickly yet it goes out in a moment with no time to warm anything, while to maintain the heat you must constantly feed shavings."[19]

Ramon Pla changed this commercial agent, as his strategy, that is, the lack of scientific advertising, had an impact in product sales. In Pla's words to the new agent:

> "We note that you have activated the [scientific] publicity of our products, which we celebrate. It is really necessary to activate [scientific] publicity in Chile, since it is currently the country where we are getting the lower [sale] rates, despite the fact of being one of the first countries where the products were released."[20]

All these cases show that the Institut Ravetllat-Pla was a pioneer in the use of different strategies for scientific knowledge legitimation. Some of these strategies included: the creation of a scientific journal widely distributed, free for all physicians, in Latin America; medical publicity aimed at a specific public, such as general and rural physicians and pediatricians; the building of a huge transnational scientific and commercial network, which fostered knowledge circulation; and the involvement of big companies in the legitimating process. In all, the case of the Ravetllat-Pla serums illustrates that

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[14] Nascimento, Dilene Raimundo (2002) *Fundaçao Ataulpho de Paiva. Liga Brasileira Contra a Tuberculose. Um século de luta*. Rio de Janeiro, FAPERJ y Quadratim Editoras.

[15] Nascimento (2005), *op cit. 13.*

[16] *Ibíd.* p 65.

[17] Juliá, Buenaventura, letter to Ravetllat-Pla Institute, on 1st August 1929, file: 24.4.3.0. Ravetllat-Pla Insititute Archive, Universitat Autònoma de Barcelona.

[18] Armengol, Juan, letter to Ravetllat-Pla Institute, on 28th February 1925, file: 35.2.0.0. Ravetllat-Pla Insititute Archive, Universitat Autònoma de Barcelona.

[19] Pla, Ramon, letter to Juan Armengol, on 22th June 1927, file: 35.4.0.0. Ravetllat-Pla Insititute Archive, Universitat Autònoma de Barcelona.

[20] Ravetllat-Pla Institute, letter to Ignaci Parés Serra, on 22th July 1929, file: 35.6.0.0. Ravetllat-Pla Insititute Archive, Universitat Autònoma de Barcelona.

patients consume pharmaceutical products as prescribed by physicians, even without consensual scientific approval, as shown by the Institute's successful legitimating and advertising strategies and, thus, high sales rates.

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RECYCLING PSYCHOLOGICAL INSTRUMENTS

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Abstract

When looking at a series of Albert Michotte's successive experiments on perception from the 1920s through the 1940s, it becomes clear that the instruments he used in these experiments were not fixed and stable entities. They were especially chosen and developed for being flexible and easy to adapt. Michotte's method required this easy access to the instruments in order to experiment on an extensive variety of related impressions. The instruments did not only constantly evolve within a certain series of experiments (e.g. the experiments on causality), but when a certain investigation was completed, Michotte often recycled parts of the instruments in order to create new instruments for a new experimental study. Many of the instruments, moreover, had already been borrowed from other laboratory or artistic practices, and had been adapted to the specific purposes of the experiment. The practices of adapting and recycling thus transcend the context of one psychologist or one laboratory. I will discuss these practices of adapting and recycling by looking at some concrete instruments used and developed by Michotte and his technician M.L. Roland.

This paper is about the role of *recycling* in the creation of new psychological instruments. I will look mainly at instruments of one particular psychological laboratory, situated in Leuven, Belgium, during the first half of the 20th century: that of the experimental psychologist Albert Michotte (1881-1965). Here the focus will be on instruments that were developed for showing visual stimuli to observers.

I start from the observation that new instruments often were developed by reusing, *recycling*, parts of older existing instruments, often those that were present in the laboratory. I consider as 'recycling', both the taking material parts out of older instruments in order to reuse them in a new one (which I call the *recycling of material elements*); and the reusing of certain design structures of known instruments in the new constellation of a new instrument (which I call the *recycling of structural elements, or design elements).* Both of which were common practice in the psychological laboratory.

These recycling practices can be seen as determining to a considerable extent what new instruments would be like. A new research topic often asked for a new experimental set-up, and new instruments had to be constructed. Yet this new topic was often embarked upon with instruments built up from parts of instruments available in the laboratory or as an assemblage of certain structures of already familiar instruments.

This also means that this practice of recycling determined to a considerable extent the kind of stimuli that were presented to the observers in the experiments. The new stimuli necessary for the new investigations were already largely determined by the corpus of existing instruments.

In this paper, I retrace certain paths of recycling, as a way to better understand specific instruments and to grasp why they were built the way they were. My main aim with this approach is to shed light on how a certain tradition of presenting visual stimuli was created in experimental psychology.

Recycling material elements

For the case of the recycling of material elements, the optical element of Michotte's tachistoscope provides a telling example. Tachistoscopes, most often used as short exposure devices, had been in use for about half a century in different versions in physiology and experimental psychology (Benschop 1998) when at the international Congress of Psychology in 1909 in Genève, Albert Michotte, then a young professor in experimental psychology, presented his own version of the tachistoscope (Claparède 1910; Michotte 1912, image: http://vlp.mpiwg-berlin.mpg.de/vlpimages/images/img24638.jpg). What was new about this tachistoscope with its two large rotating discs, was that two different images could be shown simultaneously or successively, in short, variable and precisely determinable intervals, and, due to a special optical element, presented at the same spot on the retina. Michotte describes many possible uses for the instrument: next to the usual tachistoscopic experiments, it could also be used for research on immediate memory, stroboscopic illusions, or consecutive images. It also could make experimental setups for research on simultaneous associations (Narciss Ach) or extensions of consciousness (Wilhelm Wirth) much simpler.

Michotte's *comparison tachistoscope*, as the instrument was called, has not been preserved. It must have been subject to some pride, as it was one of his first instruments taken into production by one of the most prominent psychological instrument makers, Ernst Zimmerman (Zimmerman 1912), yet it was dismantled. Its optical element, however, has been preserved. It consisted of a combination of prisms, some with silvered sides, so that the light rays of the objects (images on paper cards placed in cardholders on either side of the discs) are broken in such a way that they end up on the same spot of the retina. This optical element, which was the most expensive and original part of Michotte's comparison tachistoscope, was then reused in another kind of tachistoscope, a fall tachistoscope, which has been preserved. (image: http://www.erfgoedplus.be, KULppw030).

Original instrument parts travelled in the laboratory from one instrument to the other. We got to known this particular optical element as part of the last instrument in which it was used. Due to the fact that this optical element was clearly described as part of an earlier instrument, and had been presented and published as such, it can be traced back to its original place.

Recycling structural elements

Next to the recycling of concrete, materially available instrument parts for use in a new instrument, I want to look at the practice of reusing certain *design elements* from known instruments. This, I argue, is another, often recurring practice of recycling, looking at which allows to relate different instruments to one another and helps us to gain a better understanding why certain instruments were designed the way they were.

It is striking, for example, to see the recurrence of rotating discs in physiological and psychological instruments. Another recurrent structure is that of a narrow slit or window (of different forms and sizes) in a covering screen which uncovers, partially or temporarily, an object behind it. The instruments I discuss here all combined rotating discs with such slits. The combination of these two design elements, however, could take many different variations.

In Michotte's comparison tachistoscope, there were two black metal rotating discs, each 90cm in diameter, and both carrying a slit whose size was adjustable. In Michotte's laboratory, another such black metal disc with an adjustable slit could be found, although in a smaller format (40 cm in diameter), in a tachistoscope in a design of Friedrich Schumann (Image, see http://www.uclouvain.be/235628.html) which has been preserved as well. This was another and smaller version of the tachistoscope Schumann presented in 1904 at the 'Ausstellung von experimental-psychologischen Apparaten und Methoden', accompanying the first 'Kongress für experimentelle Psychologie' in Giessen.

Such rotating discs were not part of the first tachistoscopes, where rather, as e.g. in Alfred Volkmann's tachistoscope (Volkmann 1859), the slit was in a thin metal plate, which was pulled by a falling weight over an underlying image, revealing it briefly (Image: Benschop 1998, p.28). Rotating discs became part of tachistoscope designs when recurrent, periodical stimuli became important for certain research topics.

The use of rotating discs in tachistoscopic designs, in its turn, made the instrument susceptible to yet other tasks in other experiments. Max Wertheimer used an adapted version of Schumann's tachistoscope in his famous studies on seeing movement in 1910 (Wertheimer 1912). These experiments illustrated how rotation tachistoscopes could be used in experiments on movement perception, allowing to study what had been investigated with stroboscopes with now much more precision. Michotte also explicitly mentions that his comparison tachistoscope can be used especially for stroboscopic movement studies.

When three decades later, in his experiments on the perception of causality (Michotte 1941; 1946), Michotte needed not simply moving stimuli, but what he called 'dynamic' stimuli, he took recourse to these same structural elements of rotating disc and slit again. He needed a way to present moving objects in different kinds of interactions, and this in such a way that the parameters of these stimuli could be easily varied and precisely controlled. These new dynamic stimuli were created by constructing a new instrument, the *Banc Michotte* (image: *http://www.uclouvain.be/235664.html*), which was hailed by his colleagues for its inventiveness. This new instrument however was also an assemblage of many different structures already available in familiar instruments in new combination.

Michotte developed the Banc Michotte together with his technician Léon Roland. Large paper discs (50cm in diameter) with lines in different colours painted on them were rotated behind a big white screen. In the screen was a small horizontal slit. Observers had to sit at 1.5 meter from the screen, focus on the middle of the slit, and report what they saw. They described that they saw, for example, a red rectangle bumping into a black one and setting it in motion. With these abstract moving images Michotte studied the perception of interactions between objects, causal interaction in this case, as something that is directly given in perception.

The Banc Michotte could perhaps be seen as one of the last versions of the tachistoscope or short exposure apparatus before the computer largely took over as a stimuli presentation apparatus. It was, however, a version of the tachistoscope which was adapted to the presentation of moving interacting figures. It took over the elements of rotating disc, the slit and the shortly uncovering of an underlying image. In addition to this, it also included design elements from other familiar instruments, combining all these in a new constellation. Let me trace in a sketchy way some of these structural elements and their combinations.

1. In the Banc Michotte, in contrast to the comparison tachistoscope, it is the discs themselves which are the image carriers. They carry coloured lines in many different combinations. Rotating discs carrying inscriptions already had a long history in perception research. In colour research, discs with segments in different colours painted on them are mounted on different kinds of rotating devices (e.g. the colour mixer by Goethe & Otto Runge). Such colour discs and different kinds of rotating devices were present in most psychological laboratories of the time, and also Michotte's laboratory in Louvain possessed different of these rotating devices. Rotating discs with different areas painted black or white were also used in research on contrast perception (e.g. Ernst Mach 1865). Very well-known were also those discs with figures in different stages of movement, used for studying the perception of movement, such as the 'phenakistoscope' of Joseph Plateau (Plateau 1832).

2. The inscriptions on the discs are not discrete figures, or coloured segments, but continuous lines. In this respect they resemble perhaps more Plateau's famous spiral disc, with which he showed the 'spiral motion after-effect'. To make the relation with another domain of knowledge where vision was investigated, they do remind strongly of some of Marcel Duchamp's rotoreliefs (1935) as well. The lines on Michotte's discs however are no spirals or eccentric circles, but arcs of a circle, mostly in red and black, drawn with much precision in such a way that, when looked at through the slit, they could present exactly those stimuli to observers necessary for them to see one figure setting another in movement.

3. Crucial in the Banc Michotte was that the rotating discs had to be looked at through a slit. Whereas the colour and contrasts discs had to be looked at as a whole, Plateau's discs also were looked at through various slits. Whereas these slits, however, were plural and located in the disc itself, thus rotating as well, in the Banc Michotte, there was only one slit which was located in a large fixed covering screen. In contrast to most models of the tachistoscope, where a fix image is briefly revealed by a (circularly or linearly) travelling slit, here it is the images that travel, and the slit is fixed. Yet there is a similar structure to be found in that both are briefly uncovering an underlying image. Whereas in the tachistoscope the view on the whole image is shortly revealed and then cut off again, the working principle of the banc Michotte is that only part of the underlying image is revealed at each moment. It was the task of the slit to cut out a small portion of the total view, and it is only then that the intended images in the moving displays are created.

Some elements of the rotation tachistoscope were combined here with the principle of a then popular illusion: that of pulling a strip of paper with a diagonal line on it, behind a little screen with a horizontal slit. In the slit one sees a figure moving from left to right. The construction of motorized rotating discs, put in continuous movement, and the many hundreds of discs with different line combinations allowed to employ this illusion for the study of causal impressions, in a way that was flexible, variable and controllable.

With this tracing of instrument structures, I wanted to argue that, when looking at stimulus presentation instruments in experimental psychology, from the beginning of scientific psychology until about the 1960s, their seems to have been *a pool of design elements*, a corpus of structures, which were recombined and reassembled in many different combinations. The Banc Michotte is a late and complex instance of this 'recycling' tradition.

In this pool of design elements, moreover, it seemed that *some elements obtained an exemplary status*. Some became as it were *standard design elements*. I would like to argue that the rotating disc and the slit were two such important, often recurring elements. They were used again and again, in instruments of various kinds with various functions. Combined together, rotating discs with slits in them even received a specialist name, being called the Episkotister principle. But rotating discs and slits kept on recurring in many different combinations. This frequent recurrence no doubt had to do with their usefulness for various ends; and with their material presence in almost each psychological laboratory. This made that they became elements that were readily available to the *technical imagination*. For those who had to construct new instruments for newly needed presentation tasks, rotating discs and covering/uncovering slits were some of the forms or structures that came easily to mind. They were 'recycled' time and again, extracted from instruments that were not of any direct use for the envisaged experiments, and fitted into new instruments to tackle the next problem.

Recycling stimuli

I have argued how recycling was often an important factor for what new instruments would be like. This common practice of recycling instrument parts and design elements in order to construct new instruments, moreover, also determined to a considerable extent the *kind of stimuli* that were created to show at the observers. Looking at these recycling practices can help to explain how, step by step, *a particular way of showing stimuli* and also *a specific kind of stimuli* became accepted in psychology for use in perception experiments.

Nicolas Wade, in his book *Perception and Illusion* (2005), shortly addresses the question of how it became accepted in experimental psychology to use images instead of substantial objects and real life scenes as stimuli for visual perception research. As one of the explaining factors, Wade points to the import of Charles Wheatstone's work, showing that the perception of three-dimensional space can be synthesised from two dimensional stimuli. Wade argues how the acceptance of pictures for the study of vision was important to the development of psychology as an independent experimental science because it allowed to confine the research to the laboratory, and to allow for variation, control, and exactitude (Wade 2005, p126-127). My approach of looking at recycling practices presents a way to understand why *particular* kinds of images came to be used so frequently in vision experiments.

Through the recycling of instrument parts and structures, often also a *recycling of stimuli* took place. Michotte's bumping figures used in his causality research worked in a sense as surprisingly new. These apparently simple, yet very precise moving displays, with small coloured rectangles in many movement variations drawn on white paper and seen in a corridor formed by the slit, were in a new sense, yet these stimuli and the way they were framed also strongly referred to a large number of stimuli presentations from previous experiments. The schematic images, the geometrical forms, the colour palette, their precision, the corridor formed by the slit in which they were presented: many of these aspects were explicitly chosen and argued for, sometimes also supported with experiments, as aspects which could enable or advance the experiments. Yet they also referred back to many psychological images used before them.

Tracing more in depth this recycling of stimuli would need another paper. What I wanted to argue within this paper was how looking at recycling practices can offer a promising route to better understand certain instruments and the stimuli images used in them. It will need to be further explored how the recycling of material and structural elements of stimulus presentation instruments has contributed to the creation of what could be called a *stimulus canon* in perceptual psychology.

References

http://vlp.mpiwg-berlin.mpg.de/index_html, The Virtual Laboratory. Essays and Resources on the Experimentalization of Life, Max Planck Institute for the History of Science, Berlin, ISSN 1866-4784

http://www.uclouvain.be/musee-michotte.html, Le Musée Albert Michotte de la Psychologie Expérimentale de Louvain-la-Neuve, Faculté de psychologie et des sciences de l'éducation, Université catholique de Louvain, Louvain-la-Neuve, Belgium.

http://www.erfgoedplus.be, Erfgoedplus.

BENSCHOP, R. (1998) 'What is a tachistoscope? Historical explorations of an instrument.' *Science in Context,* 11(1), 23-50.

CLAPARÈDE, E. (1910) *VIme Congrès international de Psychologie. Tenu à Genève de 2 au 7 Août 1909*, Genève, Librairie Kündig.

MACH, E. (1865) ‚Über die Wirkung der räumlichen Vertheilung des Lichtreizes auf die Netzhaut.' *Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften: Mathematisch-Naturwissenschaftliche Classe,* 52, 303-322.

MICHOTTE, A. (1912) 'Description et fonctionnement d'un nouveau Tachistoscope de Comparaison.' *Archives de Psychologie*, XII (45), 1-13.

MICHOTTE, A. (1941) 'La causalité physique est-elle une donnée phénoménale?' *Tijdschrift voor Filosofie,* 3, 290-328.

MICHOTTE, A. (1946) *La perception de la causalité*. Etudes de Psychologie, vol. 8. Louvain: Editions de l'Institut Supérieur de Philosophie. (Second edition: (1954) *La perception de la causalité*. Louvain: Publications universitaires de Louvain).

PIÉRON, H. (1920) 'Sensations lumineuses par excitation fovéale (adaption à l'obscurité)' *L'année psychologique,* 22, 85-92.

PLATEAU, J. (1832) 'Sur un nouveau genre d'illusions d'optique.' *Correspondence mathématique et physique de l'Observatoire de Bruxelles* 7, 365-368.

SOMMER, R. (1904) 'Die Ausstellung von experimental-psychologischen Apparaten und Methoden bei dem Kongreß für experimentelle Psychologie Gießen 18.—21. April 1904', Leipzig: Verlag von Johann Ambrosius Barth.

VOLKMANN, A.W. (1859) ‚Das Tachistoskop....' *Berichte über die Verhandlungen der Königlich. Sächsischen Gesellschaft der Wissenschaften zu Leipzig, Mathematisch-Physische Classe*, 11, 90-98.

WADE, N.J. (2005) *Perception and illusion: Historical perspectives*. New York: Springer.

WERTHEIMER, M. (1912) ‚Experimentelle Studien über das Sehen von Bewegung.*Zeitschrift für Psychologie und Physiologie der Sinnesorgane,* 61, 161-265.

ZIMMERMAN, E. (1912) *Psychologische und Physiologische Apparate. Liste 25.* Leipzig-Berlin (Collection Jiri Hoskovec, The Virtual Laboratory).

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 1159.

FILM, MEDICINE AND EMPIRE: INCLUSION-EXCLUSION PRACTICES AND DISCOURSES IN SPANISH MEDICAL-COLONIAL DOCUMENTARIES OF THE 1940S

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Abstract

Inclusion-exclusion dynamics in contemporary societies are complex, multidimensional processes involving the active intertwining of wide-ranging social, economic, political and cultural factors.
In this regard, scientific-technological discourses, considered as a basic constituent of western hegemonic thought, play a fundamental role in the definition, classification, framing, rationalization and disciplining of distinct human clusters. This is especially apparent when conveyed through health-medical practices and policies, inasmuch as direct contact between experts (physicians) and laypeople (patients) necessarily occurs through practice itself, which in turn purportedly offers immediate solutions to pressing everyday life problems. On the other hand, mass media, particularly when image-based, such as in the case of film or television, are crucially conducive of the construction, diffusion, representation, interpretation and reinterpretation of the beliefs and values upon which human communities are shaped. Moreover, they are also technological means of contact between experts (writers, directors, producers, distributors) and laypeople (readers, listeners, viewers), offering as well multifaceted immediate solutions (information, entertainment) to modern life needs, and thus bearing an undeniable strategic importance precisely from a joint social, economic, political and cultural point of view.
Upon these premises, our aim is to contribute to the historical understanding of the role played by the concurrence of the cinematographic and scientific-medical discourses in inclusion-exclusion dynamics in the context of post-war Spain. To do so, we have analyzed from this perspective five medical-colonial documentaries produced between 1946 and 1949, three of them in fact under the directive of Franco's government. In these films, a third discourse (colonial), equally strategic for the regime, plays an essential and

complementary role as well. Thus, the joint articulation of these three discourses offers some explanatory keys for basic inclusion-exclusion traits pertaining to the categories of gender, class and ethnic group.

The aim of this paper is to contribute to the historical understanding of the role played by the concurrence of the cinematographic and scientific-medical practices and discourses in the construction, legitimating and consolidation of Franco's regime. Through the analysis of the representation and articulation of health-medical practices and discourses in post-war Spain's medical-colonial documentary films, this study offers some explanatory keys regarding the multidimensional relations between different collectives (experts and non-experts) concerning the nature and degree of intervention in processes of generation and management of scientific-medical-health knowledge, all within the particular case of the creation of a 20th-century totalitarian regime. The addition in these films of a third set of practices and discourses, i.e. colonial, equally strategic for the regime, allows to explore the processes of science, medicine and technology popularization involved in relation to inclusion-exclusion dynamics, insofar as the explicit classification, rationalization and disciplining of distinct human clusters (race, particularly in this case, yet along with class and gender) was an essential feature in the construction of the new national identity.

Indoctrination needs in post-war autarchic Spain (1940s)

At the end of the Spanish Civil War (1936-1939), Franco and his backers faced the challenge of establishing and consolidating a new regime. The task was to (re)build the social, economic and administrative structure required for survival in a territory immersed in chaos after three years of war devastation. This, logically, had to be achieved under the provisions of the regime's political, ideological and cultural legitimating needs at home and abroad, all within a context of intense power struggle between the different political, social, economic and ideological families aiming to enter the ruling layers. Such complex process led to an autarchic articulation of the regime owing to the combination of at least three key factors: first, the paternalistic structuring of the system, where survival (of the regime, i.e., of the people) was forcibly identified with the *Caudillo*'s, whose personal authority became the axis of the political, ideological and social strategy of the regime; second, the building and establishment of the new order through the rooting out of all vestige from the Republican period, including, as expected, the subjugation of any trace of dissidence; and third, the increasing isolation of the regime in the international arena as the European friend regimes in Germany and Italy were being defeated in World War II, coupled with Franco's regime non-belligerent position according to the priority of survival needs over ideological considerations[1].

Under these circumstances, a multidimensional, strong-arm policy was required for the combined exercise of building, consolidating and legitimating the regime. In this process, an efficient indoctrination of the population was a key feature and needed the articulation of a discourse leading simultaneously, on the one hand, to the justification of a paternalistic, i.e. repressive *and* benefactor, as well as unwavering and enduring system; and, on the other, to the expedient social and historical de-contextualization of the situation, so that a new national identity would be created[2]. This was achieved, among other aspects, upon the construction of a political project, the national-syndicalism, which involved the structuring of knowledge management in a strict vertical flow of information, *from one* (the One, Franco) *or a few* (the Fascist Party, the military, the Catholic Church) *to the many* (the population at large), where adherence to the rulers, in political, ideological and moral terms, was as important as subjection and obedience to the experts, in practical terms[3].

About practices and discourses: film, medicine and empire

Among the different indoctrination devices available to the regime, the use of two sets of practices and discourses, medical-health and mass media, otherwise essential for the construction and management of contemporary societies, was particularly suitable. Medical-health practices and discourses develop around the concepts of health and disease, upon which complex social dynamics are structured, from the building of wide-ranging health systems and policies, to the multifaceted dealings, at the personal level, between physicians and patients. On the other hand, mass media practices and discourses are crucially conducive of the construction, diffusion, representation, interpretation and reinterpretation of the beliefs, values, concerns and expectations upon which human communities are shaped.

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1 Monterde (2004); Tabernero (2008).

2 Medina-Doménech and Menéndez-Navarro (2005); Tabernero (2010).

3 Tabernero (2008, 2010).

Both medical-health and mass media practices and discourses revolve around knowledge circulation and management between experts (physicians, scientists, professors/teachers; writers, directors, producers, distributors; policy-makers, companies, institutions) and non-experts (the population at large; patients, students; readers, listeners, viewers; users, customers), thus bearing an undeniable strategic importance from a joint social, economic, political and cultural point of view. It is no surprise that Franco's regime regarded them both as primary means for the building of its social and political scaffolding, and insofar as indoctrination was a key feature in this process, the approach, as it would eventually be for any developing totalitarian system, albeit not exclusively, was that of an unconcealed diffusionist model of knowledge management. According to this model, the dynamics of socio-cultural construction and organization would be based upon unidirectional and asymmetrical flows of knowledge generation, transmission and circulation, where the few experts are the creators and managers of knowledge, and the many non-experts are purportedly passive, non-participant, submissive and dependent receivers of that knowledge and its consequences in the contexts of their everyday lives. Therefore, the (medical-health and media) experts would be *the* necessary source of immediate solutions (in this case, health, information, education, entertainment) for the non-experts' pressing everyday life problems (disease) and needs (the articulation of the symbolic framework and the time and space organization upon which social life develops)[4].

As a result, on the one hand, medical-health discourses and practices were used to address immediate needs in post-war Spain, such as widespread poverty, starvation and epidemics, mainly through the establishment of a public health system which, at the same time, and given their fundamental scientific-technological trait, that is, allegedly neutral, could define and control social clusters as well as their values and behaviour, thus contributing to the building and legitimating of the regime[5]. On the other, mass media were deemed as essential means of enculturation, through the combined functions of information, education and entertainment, by way of generating apt representations of the regime. Besides the press and the radio, cinema, as "the most popular form of entertainment in Spain in the post-war years", was fundamental to provide the Spanish population with new socio-cultural meanings, including those related to science, medicine and technology[6]. Moreover, documentary films, that is, the most scientific-technological form of cinema as provider of theoretically faithful, objective reproductions of reality, would be particularly apposite to enhance the most diffusionist approach of knowledge management. As it was stated,

> "It has become essential to develop a body of documentaries, in the service of our propaganda agencies, able *to reflect in an exact, artistic manner, and through a perfect technique*, the different aspects of our Fatherland's life, and, *in the most entertaining and efficient way, to educate and instruct our people*, to persuade those still possibly mistaken of their error, and to show foreigners the wonders of Spain, the progress of our industry, our natural resources, *the discoveries of our science* and, in all, the resurgence of our Fatherland in all its aspects as impelled by the new State".[7]

Indeed, along these lines, medical-health content was used in the compulsory and exclusive official newsreel of the regime (*NO-DO*) as a valuable legitimating tool and a key feature for the building of the new national identity[8].

Finally, the documentary films analyzed in this study incorporate a third set of practices and discourses, the colonial-imperial, which was equally strategic for the regime, and plays and essential and complementary role. The aims were unequivocal, as were the functions of science and medicine in the post-war context of Spain and the legitimating significance of their portrayal for the regime, both at home and abroad:

> "All countries with colonial expansion took one and many times to the screens of the world the account of their realities or perhaps their fantasies in the territories distant from the mother country. In this sphere, there is only missing the sweet melody, all indisputable truth, of the most noble heroic deed of those great Spaniards –missionaries, officials, teachers, *men of science*– who proclaimed and proclaim in the lands of Africa the religion of Christ, *the conquests of medicine*, the essences of culture, that a man needs to live his time […] this task well deserves to be broadcasted in the screens, as the screens already broadcasted the war heroism that made it possible".[9]

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[4] Tabernero (2010), in reference to debates on popularization of science as accounted for in: Cooter and Pumfrey (1994); Secord (2004); and Topham (2009).

[5] Jiménez-Lucena (1994); Molero-Mesa (1994, 1999); Molero-Mesa and Jiménez-Lucena (2000)

[6] Medina-Doménech and Menéndez-Navarro (2005), p. 394. Also: Jiménez-Lucena, Somavilla and Castellanos-Guerrero (2002).

[7] Regulations for the organization and operation of the official body for cinematographic production, edition and distribution NO-DO, September 29, 1942, as quoted in Tabernero (2010), p. 28, with emphasis added.

[8] Medina-Doménech and Menéndez-Navarro (2005).

[9] *Primer Plano*, number 83, October 17, 1942 (emphasis added), as quoted in: Fígares (2003), p. 234.

The documentary films by Manuel Hernández Sanjuán and Santos Núñez

Five medical-colonial documentary films have been analyzed in this study. All of them were produced by Hermic Films. *Médicos coloniales* (*Colonial physicians*), *Los enfermos de Mikomeseng* (*The sick people from Mikomeseng*) and *Fiebre amarilla* (*Yellow fever*)[10] were directed by Manuel Hernández Sanjuán (1915-2007), co-founder of the production company, in Equatorial Guinea in 1946. They were part of a governmental request made directly to Hernández Sanjuán by the General Director of Morocco and Colonies, General José Díaz de Villegas Bustamante, to make "documentaries about the Guinean colony, with the aim of showing to Spain the job that was being done"[11]. The result was the production of 31 documentaries (the Equatorial Guinea series) classified under seven thematic clusters: 'Woodland resources', 'Ethnology of inhabitants', 'Wildlife and natural phenomena', 'The work of missionaries and education', 'Development and technology', 'Military exaltation' and, significantly for this study, 'Tropical diseases and health', to which the three documentaries mentioned above belonged.

In spite of the governmental sponsorship, these documentaries were poorly distributed. The official premiere was held on May 22, 1946, at 7 p.m., in the Music Palace Theater in Madrid, where General Díaz de Villegas was in attendance. Later on, there were some sporadic open showings in Spain (1947) and Guinea (1948), as well as some private showings for officials in Guinea and Tetuan (1948). Some fragments were subsequently used as inserts in other films, including fiction[12].

The other two documentaries, *Enfermos en Ben-Karrich* (*Sick people at Ben-Karrich*) and *Médicos en Marruecos* (*Physicians in Morocco*)[13] were directed by Santos Núñez[14], who had been the scriptwriter of Hernández Sanjuán's Guinea documentaries, in Morocco in 1949. They are a product of the long linkage of Hermic Films with colonial cinematographic projects made in both Guinea and Morocco[15]. Núñez's documentaries, as well as Hernández Sanjuán's, were made to fit the format of *Revista Imágenes*, which was one of the forms of the regime's official newsreel (*NO-DO*), although they were not ultimately shown as such.

The documentaries made in Guinea met with critical acclaim. Ignacio Mateo, National Radio commentator during the official premiere

> "praised the importance of cinema when applied to a cultural and patriotic venture, as it is *to reflect and broadcast* Spain's *fruitful civilizing effort* in those African lands, according to its tradition".[16]

This unmistakably shows the regime's interest in film as a wide-ranging ("broadcast") and objective ("reflect") educational tool. Similarly, the official press took the chance to explicitly insist on the objective character of documentaries as opposed to fiction films:

> "Accustomed, in the easy entertainment before the screen, to those sophisticated jungles made in Hollywood studios, we are impressed by *the realism of those naked and truthful images,* devoid of shady, laboratory-made tricks"[17].

These documentaries efficiently complied with their triple enculturation function: first, the films provided information, as they documented a distant reality, from the mother country's point of view; second, they were a source of education, as we have seen concerning the colonial venture and its civilizing (social, cultural, ideological, organizational) implications, but concurrently, given the thematic focus, in relation to health policies and the associated practices and behaviors; and third, they offered entertainment, inasmuch as the distant and somehow problematic context featured was portrayed as 'exotic' and 'heroic'.

Indeed, medical-health practices and discourses are represented and articulated in these documentaries as a necessary and heroic, as well as, owing to its scientific-technological character, mostly a practical and impartial source of construction and organization of a specific kind of societies, that is, the colonies as administered by Franco's regime. In this regard, it is significant that medical-health issues as well as science and technology (resources, ethnology and wildlife) were crucial thematic axis, as shown by the focus of the seven series of the Guinean film project, along with religion, education and the military. Thus, regarding the enculturation effort, and through the combined functions of information, education and

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[10] Filmoteca Española. Reference numbers, respectively: A-7549, A-9584, A-7552.

[11] Fígares (2003), p. 234.

[12] Fígares (2003); Ortín and Pereiró (2006).

[13] Filmoteca Española. Reference numbers, respectively: A-9150, A-8820.

[14] It has not been possible so far to obtain detailed information on Santos Núñez's life beyond his participation in these and other film projects.

[15] Fígares (2003); Ortín and Pereiró (2006).

[16] *Primer Plano*, number 297, June 23, 1946 (emphasis added), as quoted in: Fígares (2003), p. 244.

[17] Florentino Soria in África, number 56-57, August-September 1946, as quoted in: Fígares (2003), p. 244.

entertainment, these documentaries not only highlight the weight of medical-health practices and discourses as building and managing tools for the regime, but also, through the combination with the scientific-technological character of documentary filmmaking, that is, of the form simultaneously with the content, they become an overt legitimating (educational, as seemingly impartial) device.

Film, medical-health and colonial practices and discourses: inclusion-exclusion dynamics in 1940s' Spain

If the scientific-technological factor of medical-health and documentary film practices and discourses is used as a source to convey certainty, cohesion and power, the colonial-imperial venture provides an explicit representation of the associated, expert-driven and, as such, purportedly impartial processes of definition, classification, framing, rationalization and disciplining of distinct human clusters. This allows exploring the processes of science, medicine and technology popularization involved particularly from the perspective of inclusion-exclusion dynamics[18], i.e. power relations as related to medicalization processes and their impact in the social, political and cultural construction of the new national identity in post-war Spain. Such an analysis is also possible insofar as the consideration of medicine, film and empire as sets of 'practices and discourses' implies an acknowledgement of their embedding in multidimensional forms of everyday social action and interaction between individuals, groups and institutions[19]. With this premise, the analysis focuses on the (medical-health and colonial) discourses articulated in the films, the related representations of the spaces and instruments employed, as well as of the (expert and non-expert) people who inhabit and use them. As a result, an account of the interrelated position of all the actors involved with respect to the regime's aims and needs is suggested, from all the people (experts and non-experts, colonizers and colonized) featured in the documentaries, to the intended audiences (mostly in the mother country) and the filmmakers themselves.

The voice-over narration kept the "voice of the One" quality featured in the pieces produced and/or dubbed for the compulsory and exclusive official newsreel (*NO-DO*). Under the primary informational disguise, the aim was ultimately the glorification (i.e. legitimating) of the regime, conveniently de-contextualized from the joint social and historical points of view. In addition, the language used, far from actually provide with valuable information and/or educational content about specifically medical-health issues, efficiently combined technical expressions concerning diseases (etiology, prevention, treatment), the consequent application of expertise and technologies, and the associated, albeit far-reaching, health policies, with colloquial phrases, from the completely informal regard precisely of diseases, technologies and policies as well, to the insertion of anecdotes ultimately enhancing the entertainment side of the films. Taken together, all these elements conveyed an apparently truthful definition of an authority whose character was outwardly expert, practical and unbiased, but also, simultaneously (along with the inescapable religious and military elements), severe, unwavering and familiarly paternalistic ("we are all under the One")[20].

The representation of medical technologies and facilities provided with the required sense of (yet again, fittingly de-contextualized) "order, strength and cleanliness" associated to the New State. However, as opposed to the static objects and empty spaces usually displayed in the official newsreel through the 1940s, these documentaries showed instruments being truly operated by physicians and technicians as well as facilities (hospitals and clinics) swarming with patients. "The ideals of modernization and efficacy [were indeed] symbolized through technical display"[21], yet the actual application of these technologies and facilities in the colonial context, as portrayed in the screen, conveyed a powerful sense of the competence and dependability, i.e. legitimating, of the regime as a genuine provider of imperative solutions, despite the difficulties associated to its international isolation[22].

Finally, the depiction of the social dynamics taking place in these crowded facilities featured a clear-cut differentiation between medical-health experts and non-experts, and thus an essentially practical classification, rationalization and disciplining of distinct human clusters across race, class and gender. According to the diffusionist model applied, as aforementioned, the experts (officials and physicians; all of them Spanish –white– men) rigorously and efficiently provided immediate solutions to pressing everyday problems afflicting the non-experts (patients, or the population in danger: natives, in this case men, women and children, explicitly from working-class and peasant families; and Spanish women and children, particularly in the Moroccan context). The only, albeit partial, exception to this scheme is the possibility for natives and

[18] Bourdeiu (1993); Luhmann (1998).

[19] Tabernero (2010), in reference to theoretical frameworks developed on popularization of science in: Cooter and Pumfrey (1994); Secord (2004); and Topham (2009); and on communication and media theory in: Thompson (1995); and Couldry (2004).

[20] Medina-Doménech and Menéndez-Navarro (2005), p. 398, in reference to: Rodríguez-Tranche and Sánchez-Biosca (2001), pp. 89, 125.

[21] Medina-Doménech and Menéndez-Navarro (2005), pp. 395, 399.

[22] In this sense, *Los enfermos de Mikomeseng* (*The sick people from Mikomeseng*) is the portrayal of the actual construction of a model and healing, albeit isolated, society in the leprosy of the same name.

women to become medical-health technicians, that is, one step closer to the expert dominion, yet still as subaltern staff always and necessarily under the close supervision of the specialists, in a sort of otherwise unambiguous 'excluding inclusion'[23]. It is noteworthy, from the point of view of the regime's needs concerning the construction and consolidation of the new order, that the non-experts' intervention is always openly portrayed as captive and submissive, where compliance with the experts' supposedly objective and unbiased medical directives and, by extension, moral and ideological principles (as constantly suggested in the screen by the religious tutelage, mostly represented by nuns), becomes, sometimes, disturbingly docile[24].

Conclusion: colonizers and the colonized

Upon these features, while the informative/educational purpose regarding the civilizing endeavour of the Spaniards is inescapable for potential native viewers, a two-sided identification effect can be argued concerning the intended Spanish –white, mostly at the mother country– audiences. On the one hand, the primary and straightforward identification would be with the colonizers, the ruling Spaniards. Yet, on the other, they could very well identify as well with the colonized, inasmuch as patients, workers, peasants, and particularly women, in deep need of solutions and information, specially pertaining to medical-health issues, but also with regard to the new regime's intentions and capabilities.

This succeeding identification pattern is achieved by the combination of a number of cinematic techniques displayed in the documentaries: first, the adoption of a distinctively didactic format, aptly combined with the entertainment trait mentioned above, through the use, as introduction or for contextualization, of animated graphics and maps, microphotography techniques, and illustrated historical accounts of the medical-health issues shown, always conveniently devised and structured according to the regime's essentialist aims and asymmetrical information model; and second, the portrayal of medical-health science and technology as spectacle, as a set of commodities ready for everyday consumption, and made possible precisely through the (as explicitly qualified) self-denying, rigorous and outstanding efforts of the experts and officials, whose guiding and civilizing function thus becomes undeniable. In this sense, the filmmakers themselves play two different and complementary roles, as colonizers, for, upon arrival, they join the ruling communities both in Guinea and Morocco, but also as 'colonized', inasmuch as they are direct witnesses, and thus, the very first public, of the regime's colonial and related medical-health endeavours, for which they offer, in their films, a primary interpretation.

As a result, the documentaries themselves are part of the solution, as they offer not only information and education, but also evasion, so much needed in post-war Spain. The movie theatre thus becomes an entertainment-driven science space, where everyday communication practices are efficiently intertwined with, in this case, medical-health instruction, featuring essential social, political, moral and ideological aspects pertaining to the seemingly unbiased definition and disciplining of human clusters according to the regime's needs.

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References

BOHN, Cornelia (2009) "Inclusion and exclusion: Theories and findings. From exclusion from the community to including exclusion". In: GESTRICH, Andreas; RAPHAEL, Lutz; UERLINGS, Herbert (eds.). *Strangers and Poor People. Changing Patterns of Inclusion and Exclusion in Europe and the Mediterranean World from Classical Antiquity to the Present Day*. Frankfurt: Peter Lang, pp. 35-54. http://www.unilu.ch/files/bohn_2008_inclusion-and-exclusion.pdf

BOURDIEU, Pierre (1993) *La miseria del mundo*. Barcelona: Akal.

COOTER, Roger; PUMPHREY, Stephen. (1994). "Separate Spheres and Public places: Reflections on the History of Science popularisation and science in popular culture". *History of Science* 32: 237-267.

COULDRY, Nick (2004) "Theorising Media as Practice". *Social Semiotics* 14(2): 115–32.

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[23] Or 'including exclusion'. See: Bohn (2009); also Jiménez-Lucena and Molero-Mesa (2011).

[24] A particularly explicit example is the application of a pneumothorax to a patient in *Enfermos en Ben-Karrich* (*Sick people at Ben-Karrich*).

FÍGARES ROMERO DE LA CRUZ, María Dolores F. (2003). *La colonización del imaginario. Imágenes de África*. Granada: Universidad de Granada.

JIMÉNEZ LUCENA, Isabel (1994). "El tifus exantemático en la posguerra española (1939-1943): El uso de una enfermedad colectiva en la legitimación del 'Nuevo Estado'". *Dynamis* 14: 185-198.

JIMÉNEZ LUCENA, Isabel; RUIZ SOMAVILLA, María José; CASTELLANOS GUERRERO, Jesús (2002). "Un discurso sanitario para un proyecto político. La educación sanitaria en los medios de comunicación de masas durante el primer franquismo". *Asclepio* LIV(1): 201-218.

JIMÉNEZ LUCENA, Isabel; MOLERO MESA, Jorge (2011) "Good birth and good living. The (de)medicalising key to sexual reform in the anarchist media of inter-war Spain". *Journal of Iberian and Latin American Studies (JILAS)* (in press).

LUHMANN, Niklas (1998) "Inclusión-exclusión". En: *Complejidad y Modernidad: de la Unidad a la Diferencia*. Madrid: Trotta, pp. 167-195.

MEDINA-DOMÉNECH, Rosa M.; MENÉNDEZ-NAVARRO, Alfredo (2005). "Cinematic representations of medical technologies in the Spanish oficial newsreel, 1943-1970". *Public Understanding of Science*, 14: 393-408.

MOLERO MESA, Jorge (1994). "Enfermedad y previsión social en España durante el primer franquismo (1936-1951). El frustrado seguro obligatorio contra la tuberculosis". *Dynamis* 14: 199-225.

MOLERO MESA, Jorge (1999). "Fundamentos sociopolíticos de la prevención de enfermedad en la primera mitad del siglo XIX español". *Trabajo Social y Salud* 32: 19-59.

MOLERO MESA, Jorge; JIMÉNEZ LUCENA, Isabel (2000). "Salud y burocracia en España. Los cuerpos de Sanidad Nacional (1855-1951)". *Revista Española de Salud Pública* 74: 45-79.

MONTERDE, José Enrique (2004). *El cine de la autarquía (1939-1950)*. En: GUBERN, Román; MONTERDE, José Enrique; PÉREZ PERUCHA, Julio; RIAMBAU, Esteve; TORREIRO, Casimiro. *Historia del cine español*. Madrid: Cátedra, pp.181-238.

ORTÍN, Pere; PEREIRÓ, Vic (2006). *Mbini. Cazadores de imágenes en la Guinea colonial*. Barcelona: Altair/WAHF.

RODRÍGUEZ TRANCHE, Rafael; SÁNCHEZ BIOSCA, Vicente (2001). *NO-DO, el tiempo y la memoria*. Madrid: Cátedra/Filmoteca Española.

SECORD, James A. (2004). "Knowledge in Transit". *Isis* 95: 654–672.

TABERNERO, Carlos (2008). "El proceso salud-enfermedad como instrumento del discurso hegemónico en el cine español de la posguerra (1939-1950)". En: ORTIZ GÓMEZ, Teresa; *et al*. (coords.) *La experiencia de enfermar en perspectiva histórica*. Granada, Universidad de Granada, pp. 361-365.

TABERNERO Carlos (2010). *Discursos y representaciones médico-sanitarias en el cine documental colonial español de la posguerra (1939-1950)*. Barcelona: Centro de Historia de la Ciencia (CEHIC) / Universidad Autónoma de Barcelona (UAB).
http://www.recercat.net/handle/2072/97216

THOMPSON, John B. (1995). *The Media and Modernity. A Social Theory of Media*. Cambridge: Polity Press.

TOPHAM, Jonathan R. (2009). "Focus: Historicizing 'Popular Science'. Introduction" *Isis* 100(2): 310-318.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 1166.

PVC AND ITS CONTROVERSIES

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Abstract

Polyvinyl chloride (PVC), a polymer, is one of the most known and used plastics in the world. This plastic has worldwide applications in sectors such as construction, consumer goods and food packaging. In spite of usefulness of this material, in the early 1970s, complaints surfaced regarding potential links between a kind of liver cancer and vinyl chloride monomer (VCM), a small chemical unit that is used in synthesizing PVC.
This paper firstly analyses the health-history of VCM and PVC, and the outbreak of first the 'hand-disease' (acro-osteloysis) and then liver cancer (angiosarcoma); and secondly how chemists, the industry, the governments, the press, and public in general reacted to this event. We will analyse in a comparative manner the (different) responses in the USA and in Europe.

Polyvinyl chloride (PVC –Figure 1) is one of the most widely used plastics in the world. However, in the early 1970s, complaints surfaced regarding potential links between a kind of liver cancer and vinyl chloride monomer (VCM –Figure 2), a small chemical unit that is used in synthesizing PVC.

During the 1940s and 1950s several cases of occupational illness among PVC workers were described in the medical literature.[1] The causes of those illnesses remained obscure, though. Initially, it was frequently thought that the unhealthy effects were caused by the plasticizers that were added to the PVC. Later, there were growing indications that VCM might have some chronic toxic effects, especially on the liver, the kidneys and the skin. These indications were

[1] For (historical) overviews of the health effects of vinyl chloride, see: Charles Levinson, *PVC zum Beispiel: Krebserkrankungen bei der Kunststoffherstellung* (Reinbek bei Hamburg: Rowohlt, 1975); [Gilles le Ricousse], *L'industrie au regard de l'environnement: Fabrication du chlorure de vinyle monomère et du chlorure de polyvinyle* (s.l. [Paris]: Ministère de l'Environnement et du Cadre de Vie, Direction de la Prévention des Pollutions, Service de l'Environnement Industriel, August 1980); Michael Brown, 'Setting occupational health standards: The vinyl chloride case,' in: Dorothy Nelkin (ed.), *Controversy: Politics of Technical Decisions* (Beverly Hills, London: Sage, 1979), pp. 125-141; Andrea Westermann: 'PVC, Dynamit Nobel und die Stadt Troisdorf. Lokale Deutungen von industriellen Gesundheitsgefahren und ihre Verallgemeinerung'. in: F.-J. Brüggemeier/ I. Engels (Hrsg.): *Natur- und Umweltschutz nach 1945. Konflikte, Konzepte, Kompetenzen* (Frankfurt am Main: Campus, 2005), pp. 249–267; Andrea Westermann, *Plastik und politische Kultur in Westdeutschland* (Zürich: Chronos, 2007), pp. 237-314.

confirmed in an experimental study on laboratory animals executed by researchers of Dow Chemical at the end of the 1950s. Dangers of VCM were not discussed, but the scientific results were published in a medical journal in 1961.

In the course of the 1960s a specific clinical syndrome was encountered with increasing frequency by general practitioners, specialists, and medical doctors associated with PVC factories. Some workers complained about severe pain in their fingers, which had become white pale, as in case of frost. Often bone changes, shortened finger limbs or clubbed fingers were diagnosed by the doctor. Serious cases of crippled workers that had to leave their jobs were reported as well. The syndrome got the name 'acroosteolysis.' After about 1965 it became increasingly clear that the illness was quite typical for workers in the PVC industry and for autoclave cleaners especially. At Tavaux, for instance, six cases of acroosteolysis were discovered between 1965 and 1970. But many uncertainties remained; there were numerous chemicals involved in PVC manufacture and workers were employed in many different positions. As a result, it lasted until 1971 before an extensive industrial hygiene and epidemiological research project, sponsored by the Manufacturing Chemists Association in the USA, irrefutably proved that acroosteolysis was caused by vinyl chloride and occurred almost exclusively among reactor cleaners in PVC plants. In the VCM plants proper the illness did not occur, because production there often took place in highly automated open air facilities, where no manual work was required. Preliminary findings of this large study were already presented in 1969 at a conference.[2]

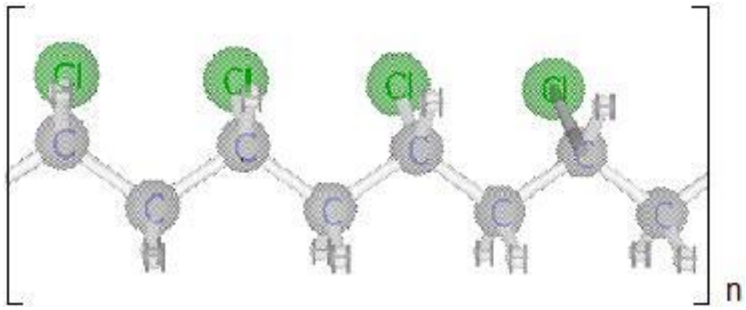

Fig. 1: A molecular structure's fragment of PVC.[3]

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Fig. 2: Vinyl chloride monomer molecular structure.[4]

VCM Incident –Its Carcinogenicity

At the 10th International Cancer Congress at Houston, Texas, in 1970 the Italian physician Pier Luigi Viola (1917-1985) gave a paper on the occurrence of cancer in rats that had been exposed to very high doses of vinyl chloride. This was the first public announcement of a link between VCM and cancer. It soon received attention and would get great consequences.[5] Viola was director of the Ospedale Aziendale, founded by Solvay, at Rosignano and connected as medical doctor to the local Solvay plant. In order to study the relation between VCM inhalation and acroosteolysis he had started in the late 1960s an experimental study on rats in the cellars of the hospital. During those studies he found that the rats developed cancers in several parts of their bodies. Immediately after the presentation of his results Viola was contacted by Professor Cesare Maltoni of Bologna University, who was involved in an extensive study on the effects of chemicals on the respiratory tracts of Italian workers. He received from Viola a manuscript of a report on 'The Vinyl Chloride Disease,' that he had written during the summer of 1970.

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[2] Brown, 'Setting occupational health standards, 128-130; J.M. Cordier, C. Fievez, M.J. Lefèvre, A. Sevrin, 'Acroostéolyse et lésions cutanées associés chez deux ouvriers affectés au nettoyage d'autoclaves,' *Cah. Med. Travail* 4 (1966), p. 14; W.A. Cook, P.M. Giever, B.D. Dinman, H.J. Magnuson, 'Occupational acroosteolysis. II. An industrial hygiene study,' *Archives of Environmental Health* 22 (1971), pp. 74-82; Susanne Jühe and Günther Veltmann, 'Zur Klinik der sogenannten Vinylchlorid-Krankheit (Sklerodermie-ähnliche Veränderungen bei Arbeitern der PVC-herstellenden Industrie), in : *Erstes internationales Symposium der Werkärzte der Chemischen Industrie*, 27.-29.4.1972 (Ludwigshafen 1972), pp. 267-276.

[3] http://www.webmoleculec.com/index.shtml

[4] *Ibidem*

[5] P.L. Viola, 'Canceragenic effect of vinyl chloride,' *Proceedings of the 10th International Cancer Congres. Abstracts* (Houston 1970), pp. 29, 20; P.L. Viola, A. Bigoti, and A. Caputo, 'Oncogenic response of rat skin, lungs, and bones to vinyl chloride,' *Cancer Research* 31 (1971), pp. 516-522; Brown, 'Setting occupational health standards, pp. 131, 140.

These results induced Maltoni to include workers in VCM and PVC production facilities in his study. Hearing about this, Professor Bartalini, director of the Health Services of the Montedison Company contacted Maltoni and suggested to extend the entire investigation to the experimental study of the biological effects of VCM on animals, with the support of his company. In 1971 Solvay, ICI and Rhône-Progil decided to join this initiative. Supported financially by the 'European Cooperative Group,' formed by those four companies, Maltoni and his team executed between 1971 and 1973 an extensive study of the health effects of VCM. They found that a specific cancer, angiosarcoma of the liver, was the most typical effect of VCM. Not only at the very high doses studied by Viola, but also at exposures of 250 ppm, or even lower. In 1974 a study by Viola confirmed these results.[6]

Maltoni frequently discussed his results with the European Cooperative Group. By September 1972, when there was sufficient hard evidence that VCM was indeed able to induce angiosarcoma, Maltoni's result were also communicated to major American manufacturers of VC and PVC, and in January 1973 also confidentially to the Manufacturing Chemists Association (MCA), who asked an American laboratory to repeat Maltoni's research. After the results had been confirmed, by July 1973 the MCA send part of the information to the responsible government institution, the National Institute of Occupational Safety and Health (NIOSH). That institute took no immediate action though. The 500 ppm threshold value remained in effect, until, early 1974, great unrest broke out among the public. Before that time only very few people, even inside the PVC companies, knew about Viola's and Maltoni's research.[7]

The industry's reaction

Reflecting back on the public exposure of VCM's dangers, an important question arises: How informed was the industry about the dangerous effects of VCM and its potential hazards? When measures to reduce VCM levels in the workplace to a limit of 1 ppm per 8-hour shift (after an initial temporary emergency status where the levels of VCM were reduced from 500 ppm to 50 ppm) started to be taken the industry did not react positively, countering that such a measure would not be feasible.[8] The VCM industry considered 1 ppm a very low limit, arguing that it was technically impossible to reach this value since it would bring grave economic consequences for the plastics industry.[9] Beyond these reasons, the industry defended itself by saying that is was impossible to positively identify the exposure level at which cancer would be induced.[10]

The responses by industry to these accumulated findings of the 1960s were mixed. After acroosteolysis had been discovered at Tavaux, Solvay almost immediately ordered the use of hand shoes among the autoclave cleaners. And B.F. Goodrich, the largest American manufacturer, started a programme of medical screening among the workers, in order to remove 'vulnerable individuals' from reactor cleaning jobs. At that time, however, no fundamental changes were made to the production installations by any of the producers, although some companies improved their cleaning practices, by using, for instance, water jets under high pressures, or organic solvents. But the results were not decisive, because PVC is difficult to dissolve, and therefore manual cleaning practices were not abandoned. Nor did responsible government bodies raise any doubts about the 500 ppm threshold value at that time. This would all change completely after a medical doctor working for Solvay published his results in 1970.[11]

Anxiety about VCM –the reaction of the press, the public and politics

In 2002, the historians Gerald Markowitz and David Rosner published a book regarding the industry's knowledge of the dangerous effects of VCM before the public disclosure of the workers' deaths in the PVC plants, that according to plastic

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[6] Brown, 'Setting occupational health standards, p. 131; Cesare Maltoni and Giuseppe Lefemine, 'Carconogenicity bioassays of vinyl chloride: I. Research plan and early results,' *Environmental Research* 7 (1974), 387-405, pp. 387-388.

[7] Cesare Maltoni and Giuseppe Lefemine, 'Carconogenicity bioassays of vinyl chloride: I. Research plan and early results,' *Environmental Research* 7 (1974), pp. 387-405, 403-404 ; Brown, 'Setting occupational health standards, p. 131.

[8] Before 1974, in the industries of VCM and PVC the concentrations most common of VCM were placed between 250 and 300 ppm. Some workers under these concentrations after months and years had fibrosis of the liver and spleen. See Ted Greenwood, "The Myth of Scientific Incompetence of Regulatory Agencies", *Science, Technology, & Human Values*, Vol. 9, No. 1, 1984, p. 90. See also Jeffrey Lewis Berger/Steven D. Riskin, "Economic and Technological Feasibility in Regulating Toxic Substances Under the Occupational Safety and Health Act", *Ecology Law Quarterly*, Vol. 7, No. 2, 1978, p. 294 and David D. Doniger, *The Law and policy of toxic substances control – a case study of vinyl chloride,* London, Baltimore, Johns Hopkins University Press, 1978, pp. 29, 30.

[9] Jeffrey Lewis Berger/Steven D. Riskin, *ibidem*, pp. 318-19. Gerald Markowitz amd David Rosner, *Deceit and Denial – The Deadly Politics of Industrial Pollution*, Berkeley, Los Angeles, London, University of California Press, 2002.

[10] Jeffrey Lewis Berger/Steven D. Riskin, *ibidem*, pp. 286-87.

[11] Brown, 'Setting occupational health standards, p. 130; W.A. Cook, P.M. Giever, B.D. Dinman, H.J. Magnuson, 'Occupational acroosteolysis. II. An industrial hygiene study,' *Archives of Environmental Health* 22 (1971), pp. 74-82.

industry and some American historians is unethical. In their influential book, *Deceit and Denial—The Deadly Politics of Industrial Pollution*, Rosner and Markowitz show that in 1973 the plastic industry had prior knowledge that VCM caused cancer in animals, even at low level exposures. Thus, revealing that American and European plastics industries withheld information regarding one of its major raw materials (VCM) and its potential cancerigenous effect.[12] Given the fact that VCM was the base for the production of hundreds of consumer products, the implications for public health would have been devastating. [13] Subsequently, the industry failed to disclose this information to either the public or to the regulatory agencies.[14]

Parallel to the confidential research going on in Europe and the USA, a left-wing journalist of the *Kölner Stadt-Anzeiger* in Germany, Ulla Jungk, had come into contact with injured workers from the PVC works of Dynamit Nobel at Troisdorf, near Cologne. She interviewed numerous workers on their medical problems and informed the local communist party. During 1973 several articles on the dangers of PVC appeared in the works magazine of communist workers group at Dynamit Nobel, and alarming letters were written to the responsible Bundesarbeitsministerium (Federal Ministry of Labour). The Ministry took the issue seriously and contacted the medical doctors of Dynamit Nobel in the Summer of 1973, but no immediate measures were taken. A few months later this sensitive issue reached the public at large, when in December 1973 an article on the 'Gefährlicher Kunststoff' (dangerous plastic) PVC was published in the influential and widely read weekly *Der Spiegel*.

On 22 January 1974, when the public debates in Germany were approaching the boiling point, a 'publicity bomb' exploded in the USA. That day, the large PVC manufacturer B.F. Goodrich informed the NIOSH, their workers and the public at large that three of its workers had died between September 1971 and December 1973 of angiosarcoma of the liver. Only in December 1973 the two responsible doctors, had related the three deaths to the same cause. The US public authorities, who had been quite passive so far, now responded immediately and organised an 'Informal Fact-Finding Hearing on Possible Hazards of Vinyl Chloride Manufacture and Use' on 15 February 1974 where Maltoni presented his results, which now were published with priority in March. The US Occupational Safety and Health Administration (OSHA) now worked at high speed. Already on 5 April 1974 the OSHA issued an emergence standard which required VCM and PVC producers to reduce exposures to 50 ppm. The temporary standard should be replaced by a permanent one within 6 months. The energetic action of OSHA was no luxury. At about the same time the American VCM industry reported that an additional ten workers had died from angiosarcoma.

The injured image

The whole debate on the carcinogenicity of VCM had important 'side-effects' that would continue to bother the industry for several decades. At the zenith of the public debate on VCM, in 1974 and 1975, headlines like the following appeared for instance in the German press: 'Tod im Plastik' (Death in plastics), and 'Werden wir auf Kunststoffe verzichten müssen?' (Shall we have to live without plastics?). Similar headlines were published elsewhere. In the press, as well as in public debate, not always a clear distinction was made between VCM and PVC, nor between the manufacture of PVC and plastic products made from PVC. As a result, the dark shadow of the VCM episode was cast over the PVC industry or even over the plastics industry in general.[15]

In the case of Germany, Andrea Westermann has analysed how the public debates gradually transformed from a discussion on occupational diseases into debates on the environmental hazards of the PVC industry. In 1976 the discussion, for instance, shifted to the issue of VCM residues in plastic foils for food stuffs. Suddenly VCM, and by implication PVC, was not only a problem for the workers, but for society at large. Later the environmental movement started to attack the waste management of PVC and the chlorine industry as a whole. In the 1970s the PVC industry had acquired a negative image, in some circles at least, and it did not easily get rid of this during the following years.[16]

The controversies and conflicts around PVC bring to the forefront important questions about risk control, scientific reliability and validity. In other words, the PVC case shares features with other controversies in the history of science. Concerning risk evaluation, many of the scientific questions brought forth during the VCM episode are still very controversial and far from being resolved. It has not yet been accurately determined what level of exposure to a carcinogenic may

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[12] Gerald Markowitz and David Rosner, *Deceit and Denial – The Deadly Politics of Industrial Pollution*, *op. cit*.

[13] John Wiener, *Why 2 Historians Now Have to Fear the Chemical Industry*, http://hnn.us/articles/9950.html, p. 2.

[14] *Ibidem*.

[15] 'Tod im Plastik,' *Der Spiegel*, Nr. 27, 1974; 'Werden wir auf Kunststoffe verzichten müssen?', *Die Zeit,* 28 June 1974, p. 37; M. Tushen, 'Disaster in plastic,' *Health/ PAC Bulletin* 71 (1976), pp. 1-16. See: Westermann, *Plastik und politische Kultur*, pp. 249-250, 264-265; Brown, 'Setting occupational health standards, p. 140.

[16] Westermann: 'PVC, Dynamit Nobel und die Stadt Troisdorf'. pp. 264–266; Westermann, *Plastik und politische Kultur*, pp. 288-310.

constitute a danger, nor whether a safe level of exposure exists or not, or if the doses applied to animals can be extrapolated to humans in order to foresee their adverse effects.[17]

[17] Jacqueline Karnell Corn, "Vinyl Chloride, Setting a Workplace Standard: An Historical Perspective on Assessing Risk", *Journal of Public Health Policy*, Vol. 5, N. 4, 1984, p. 510.

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HOW GREEK SCIENCES PASSED TO EUROPE BY THE ARABS THROUGH AL-ANDALUS AND SICILY

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Abstract

It is well-known that the European Renaissance was based on the Classical heritage transmitted to Europe by the Arabs, who did not render it the same as they had taken it.
We are concerned here with an exposition of the nature of that role, which manifested itself in 3 phases: 1) Translating and editing of the Greek text books, both scientific and philosophic. 2) Researching and analyzing through detailed studies of these works, supplementing them with introductions, interpretations, scholia and comments. 3) Application and experimentation of the scientific theories, which thus laid the principles of the scientific method.
As previous studies enumerated the Arabs' contributions in the major fields of sciences, the present survey intends to throw into relief how the Arab scholars exceeded from the theoretical speculations to praxis. By applying this experimental practical method, using instruments and apparatuses developed or invented by them, drawing maps, making tables, they opened new vistas for the appendix sciences either to medicine or mathematics to emerge and grow independently of these major sciences.
Al-Andalus and Sicily, as channels of transmission, provided the institutes and universities in Europe when the translation movement from Arabic started in the 12th century, with all these heritage and studies, where they remained open to fresh investigations and exhaustive researches. They thus paved the way for Europe to proceed into the Renaissance.
To sum up: the growth of knowledge should be looked up as the achievement of all humanity, the active nation who could benefit from the knowledge of the predecessors and then pass it to its successors, with new contributions of its own. In that process, the Arabs played their role.

Centuries separated the Latin west from the Greek thought; during these centuries great modifications had been undertaken as far as sciences were concerned. In the meantime, this Greek heritage called the attention of the Arabs, who devoted themselves from the 8th century onwards to be acquainted with that thought. The Arab Scientists[1] were not content

[1] This term denotes all those who are Arab in origin or lived under Arab rule and wrote their works in Arabic.

to accept the Greek sciences thus obtained, but made them the basis for fresh inquiries. That role played by the Arabs in recovering and reviving that heritage invoked the Latin West to reacquire the full works of the Greek sciences together with the Arab scholarship.

As various previous studies enumerated the Arabs' contributions to the major fields of sciences. The present survey intends to throw into relief how the Arab scholars, when exceeded from the theoretical speculations to praxis by applying experimental and practical method, using instruments and apparatuses developed or invented by them, compiling tables etc., opened new vistas for some appendix and allied sciences, either to medicine or mathematics, to emerge and grow independently of these major sciences.

By following them in their process especially for those appendix and allied sciences, as yet had been given little attention by previous studies, we are concerned here.

If one may follow in headlines the stages, the Greek thought passed at the hands of the Arabs, manifested themselves in 3 phases:

- Firstly, the translating and the editing of the Greek works both philosophic and scientific, which the Arabs entrusted the translators for around 3 centuries (8th to 10th), produced revised versions, helped to the verification of badly copied texts, and also preserved in Arabic versions even works whose Greek originals had been lost.[2]
- Secondly, researching and analysing through detailed studies of the translated Greek works, supplementing them with introduction, interpretations and commentaries, the Arab scholars devoted themselves successively to studies, which become upwards reference works indispensable to comprehend those Greek works.
- Thirdly, applying and experimenting the Greek scientific theories, with careful assessments of the results of the practice, its failures and complications. Thus, laying the principles of the scientific method for the rest of their scientific activities.

Throughout these stages, thus exposed the nature of the role played by the Arabs in this context, one may notice its traces on the scientific works written by the Arabs as well as on the sciences themselves. Among its main traces there are in the first place: how the theoretical speculations undertaken by encyclopaedic scholars, when the classical Greek philosophical corpus, that was understood to include such disciplines as medicine, natural sciences and mathematics, appeared as self-contained and integrated body of knowledge that could explain many varied phenomena by restoring to an all-encompassing philosophical system such as the Aristotelian system. That system was found particularly appealing by scholars, who found in themselves the ability to grasp this kind of interconnectedness of the various kinds of scientific principles. That is why we found such scholars as al-Farabi (ca. 870-950) and his student Avicenna (980-1037) exposing their concepts in encyclopaedic works.

In the second place: how old disciplines treated by refiner scholars; when works of later Greek scientists were found among the translated bulk, which have more specific scientific interests in the spheres of sciences known to the Greeks. Most of those scientists flourished in the Hellenistic era and were related somehow to the scientific school of Alexandria, embodied in Hippocrates (460-360 BC), Herophilos (280-225 BC), and Galen (ca. 129 - ca. 200) in medicine; Heron of Alexandria (ca. 55-68 BC) and Ptolemy (ca. 100-175) in mathematics. These works explored the contents and the dimensions of these sciences, which created among the Arabs a deep desire to recover and deploy them with remarks and refinements, like what Thabit ben Qurra (901) did in an abstract of Ptolemy's Almagest to make it readable, and also make it verifiable through a second book entitled *Introduction to Almagest*[3], updating the text through emendations.

In the third place: how new disciplines emerged by true scholars, whose activities included most and foremost a process of reassessment of the Greek scientific theories by revising the shortcomings resulted by time; and went further than that by creating new scientific disciplines; as was the case with the sphere of applied mathematics from which emerge such disciplines known as exact sciences like Algebra and Trigonometry and their applications on disciplines like astronomy and astrology, which we will later discuss in detail. For the moment we have to refer to scholars of these sciences who tended to express their thoughts in works dedicated to a particular kind of sciences rather than in encyclopaedic works, like Al-Khawarizmi (fl. 825) in his books on "Arithmetic" and "Algebra and Opposition".

Finally in the fourth place: how the scope of the known disciplines were enlarged and tended to specialization by experimentalists and practitioners; when the dependence on well-made apparatuses meant that theory and practice were brought together, but even made practice a more natural part of science itself. By that scientific method, those scholars

[2] For the translation movement and its achievements see: De Lacy O'leary, How Greek Science passed to the Arabs, London, 1949, pp. 155-175.

[3] Ibn al-Qifti, *Tarikh al-Hukama* (*History of the wise*), ed. Julius Libert, Leipzig, 1903, pp. 111-112.

increased the range of mathematization, not only by developing new disciplines, but also by attacking different questions through a quantitative approach. Whereas Archimedes (285-211 BC) and Hero of Alexandria were avoiding discussing openly the applications for their inventions, major Arab scientific figures such as al-Battani (fl. 870) were also expert-makers of instruments which enhanced their powers of observation and calculation, thus widening the scope of the science of astronomy into mathematical astronomy with its applications on daily-life needs.

In the other sphere of science, if the grand characteristic of the Hippocratic medicine was that he advocated the clinical observation, by which alone success in medicine was possible. This great trait in his system, together with the development of the anatomical school at the hands of Herophilos and Galen, absorbed by the Arabs, led to a further advance in the scope of medical science. Thus, nothing more obvious can be noticed than their insistence on the investigations of causes which required the knowledge of physiological and pathological processes, both in general and in the details of particular cases, as recommended by al-Rhazi, (d. 923) and adopted by later Arab scholars of medicine.

Such traces and the like we intend to follow up in detail. But we have to point out briefly how the echoes of the translation movement, together with the scientific studies based on it, were rapidly and widely circulating all over the Arab world among those who were interested, so that the Arab scholars on the western part in North Africa, Sicily and al-Andalus were provided regularly with the most recent translated editions, together with every work in all kinds of knowledge once appear in the East, either through individual acquisitions or by efforts of the authorities,[4] so as to have enough opportunity to follow the scientific activities, and also to take part in them. Thus it is not surprising to find how their writings reflect their views on every scientific issue raised by their counterparts in the East.

Throughout the bulk of the Greek works and the studies made upon them by the Arabs, remarkable points can be investigated in order to point out how the Arabs were dealing with this scientific heritage. We can deduce from some hints to what extent did the Greeks apply the principles of the scientific method known to them, and how far these principles were adopted by the Arabs; and also the impact of their applications on the scope of both the medical and the mathematical sciences, which we are concerned with here.

Nothing more obvious than finding a testimony by the very words of a Greek scholar himself in a letter written to one of his colleagues, in which Archimedes himself said *"I thought fit to write out for you and explain in detail in the same book the peculiarity of a certain method, by which it will be possible for you to get a start to enable you to investigate some of the problems in mathematics by means of mechanics. This procedure is, I am persuaded, no less useful even for the proof of the theorems themselves; for certain things first became clear to me by a mechanical method, although they had to be demonstrated by geometry afterwards because their investigation by this method fell short of demonstration… I think it necessary to expound the method… because I am persuaded that it will be of no little service to mathematics; for I suppose that some of my contemporaries or successors, once the method is established, will through it discover other theorems, which have not yet occurred to me"*.[5]

And according to a commentary by Ptolemy in his *Syntaxis* (pub. ca. 150) on Hipparchus's (fl. 146-126 BC) works on the method used to record the positions for the fixed stars, he said that *"Hipparchus, because he did not yet have in his possession such a groundwork of resources in the form of accurate observations, although he investigated the theories of the sun and moon, and to the best of his ability, demonstrated with every means at his command that they are represented by uniform circular motions, did not even make a beginning in establishing theories for the five planets. All that he did was to make a compilation of the planetary observations arranged in a more useful way, and to show by means of this that the phenomena were not in agreement with the hypotheses of the astronomers of that time"*.[6]

A similar testimony cited by Galen taken from the physician Erasistratus' (280-225 BC) work on the nature of scientific research, he said "*those who are completely unused to inquiry are, in their first attempts, blinded and dazed in their understanding and straightway leave off the inquiry from mental fatigue. But the man who is used to inquiry tries every opening as he conducts his search and turns in every direction… directing his attention to one idea after another that is germane to what is being investigated, he presses on until he arrives at his goal"*.[7]

There is also another citation of the physician Herakleides (75-20 BC) who wrote on Hippocratic exegesis, on a kind of exterior therapy for a reduction of dislocation of the thigh. He recommends that *"one ought not to judge the affair by theory, since it's useful but not completely required to investigate this"*.[8]

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[4] Concerning these efforts in al-Andalus see: *Ibn Said Al-Maghrebi, Al Mugrib*, ed. Ch. Daif, Cairo, 1955, part I, p. 45; in Sicily, see: Ibn Abi Usaybia, uyun al-Anba (*Sources of news about physicians*), 2nd ed., Beirut, 1982. p. 478; and also: Aziz Ahmed, *Islamic Sicily*, trans. A. al-Tibi, Beirut, 1980,pp. 52-53, 74-75.

[5] Georgia L. Irby-Massie & Paul I. Keyser, G*reek Science of the Hellenistic era-A sourcebook*, N.Y, 2002, pp. 28-29.

[6] *Idem*, p. 77.

[7] *Idem*, p. 301.

[8] *Idem*, P. 308.

Dioscorides (fl. 50-70) also declared his method in the preface of his pharmacy-book, *Materia Medica* saying *"I now encourage you, and any who may chance upon my book, not to look at my verbal facility but at my careful practical experience. For I have exercised the greatest precision in getting to know most of my subject through direct observation, and in checking what was universally accepted in the written records"*.[9]

While Galen himself wrote in his book on "the sects" –the schools of medicine– (ca. 166), suggesting this book ought to be among the first to be read. He explained the method of the empiricists, *"who claim that the art* [medicine] *comes about in the following way. One has observed many affections in people. It is this kind of experience which has contributed most to their art. When they find out that for the most part, it has the same effect in the case of the same diseases, then they call such a memory a 'theorem' and think that it already is trustworthy and forms part of the art. But when many such theorems had been accumulated by them, the whole accumulation was the art of medicine, and the person who had accumulated the theorems was a doctor, such an accumulation came to be called by then 'autopsy', being a certain kind of memory of what is perceived to happen many times in the same way"*.[10] He also recommends observation and dissection when he wrote about the anatomical procedures (ca. 170) saying: *"Make it your serious endeavour not only to learn precisely from books the form of each bone, but also to examine assiduously with your own eyes the human bones themselves"*.[11]

All these citations allude that the Greeks used inquiry, applying observation and practice, but only for the sake of proving the theory. They managed to make all their experience fit in with the theoretical system; they had no reason to doubt the truth of the theories and the conceptions on which the system was based. But tending to observation and practices as a method and a procedure to find solutions or for the application of the theory, that was the method adopted by the Arabs.

The Arab medical thought knows that there are fundamentals in medicine that must not be proved by logic and reasons,[12] as had been realized by Avicenna. But physicians have to start by studying the theory of medicine and to reason correctly. Then, incline to careful enquiry about all relevant symptoms (that is history and medicine). Next, consider prognosis and different diagnosis. And finally, the proper treatment was suggested with careful assessment of the results of the treatment, according to the recommendations of al-Rhazi.

This represents the characteristic contributions of the Arab physicians to medical evaluation. And decide the relation between Greek and Arab medicine, which is best known in the attitude of the clinicians, who often agree with Hippocrates and Galen when their teachings are in accordance with their own experiences.[13] But they differ with them, often very energetically, when it are not; at times they would suspend their judgment until experience indicated whether they were right or wrong. In fact there are some indications in the style of Arab writings which show irritation at the limitations imposed on medicine by philosophical speculation. So examples of their independence are common in their writings when they stated experimental technique.

All these characteristics can be noticed throughout the enormous library of works, encyclopaedias, texts on specialties and guides to medical practice, produced by the Arab scholars, which suffice here to refer to the writing of al-Zahrawi (known as Abulcasis, d. 1013) and Ibn Zuhr (Avenzoar, d. 1162).

Among other medical development was the growing critical examination of the Greek medical writings, specially the works of Galen, expressed in a genre of writings entitled with "Shukuk" (doubts), such as what had been written by Rhazi and later by Averroes (d. 1198). This critical and independent tendency enabled them to add significantly to the knowledge of medicine from the 11th century and throughout the 12th century, widening the scope of the science studying epidemics, advancing the knowledge of anatomy and physiology. Believing medicine to be the art of removing impediments to the normal functioning of nature, they extended their concepts to the study of psychotherapy, when they perceived a close connection between emotional and physical states.[14] Considering the importance of remedies, depending on field-trained herbalists and highly-trained pharmacist-authors, pharmacy first emerged as an independent profession in Islamic land during the late 8th century to reach its zenith in the 13th century, through the greatest Arab text –dealing with medicinal botany– of Ibn al-Baitar (d. 1248).[15]

If we turn to the sphere of mathematics, it will be noticed that the Greeks showed interest in number theory, numerical relationships and the arithmetical operations. In addition to geometry, descended from the principles set forth by Euclid (285 BC), and the problems in constructing interrelated geometrical figures, the character of the mathematical point,

[9] *Idem*, p. 310.

[10] *Idem*, p. 322.

[11] *Idem*, P. 324.

[12] Franz Rosenthal, *The Classical Heritage in Islam*, trans. Emile & Jenny Marmorstein, London, 1975, pp. 187 f.

[13] Donald Campbell, Arabian *Medicine and its influence on the Middle Ages*, vol. I, London, repr. 2000, pp. 4-5.

[14] Manfred Ullman, *Islamic Medicine*, Edinburgh, repr. 1997, pp. 55, 82.

[15] Howard Turner, *Science in Medieval* Islam, Texas, 1995, pp. 138-139.

line and space was given intense study in a manner both mathematical and philosophical.[16] And beyond Euclid's definitions great strides were made in theoretical works. The Arab mathematicians devoted considerable time and effort to proving Euclid's postulates, which stimulated them successively to arrive at alternate proofs including proofs of non-Euclidean theorems.[17]

The most remarkable thing is that practical application was never long over-looked. The arithmetic of daily life was essential in everything. Fundamental mathematical principles and definitions, Greek in origin, were clarified in such a way as to increase public understanding of numerical relationships and promote efficiency in all kinds of computation, since such intentions had provoked a mathematician like al-Khawarizmi to produce his works.

Moreover, the beginnings of analytical methods and the foundations of trigonometry by the Greeks have indeed a continuous history of progress when they were handed on to the Arabs.[18]

This can be best shown in some of the exact sciences like astronomy, cosmology, astrology, mechanics, which all had a strong mathematical component. In the early centuries of classical Greece the boundary between cosmology and astronomy was unclear. One Greek savant after another took his own particular look at the heavenly spheres. The Greek philosophers refined the mechanisms of movement within the Greek cosmos of spheres.[19] The Hellenistic astronomers looked at things differently, to a different nature of the earth and the heavens, once a version of Ptolemy's *Syntaxis*, better known as *Almagest*, was produced.[20]

All Arab astronomers accepted Ptolemy's theories as the definitive mathematical model of the heavens with the sun and planets. But by the 11th century astronomy was flourishing in al-Andalus, when Ptolemaic astronomy began to be questioned.[21] In the sphere of theoretical astronomy, the Ptolemaic planetary system was generally accepted, but in the 13th century Arab astronomers eventually came to object in particular to the way his epicyclical motions violated the uniformity of motion, and reached its zenith through hard attempts to come to a new kind of astronomy in which theory and observable fact could be reconciled without having to accommodate noticeable discrepancies. As exemplified in the works of Maslama al-Magiti (d. 1008) and his disciples.[22]

As a result of that, a branch of astronomy that the Arabs termed ilm al-Miqat (the science of time-keeping) emerged as an observational astronomy. In this sphere a great effort was done by increasing need for precision.

Astrology, too, was one of the old disciplines which in the Middle Ages is considered to be irrational and superstitious according to one of its distinct aspects, while the other aspect implies a scientific study of the physical influences of the heavens on earth, that makes it regarded as a science. This understanding of astrology was backed up by a wealth of scientific evidences consistent with the premises of astronomy and other sciences as Ptolemy did in his book *Tetrabiblos*, which became a basic text. Through the Arab enhancements in mathematical calculation, and their increasingly sophisticated methods of astronomical inquiry and analysis, such an evaluation, it offered the possibility that the natural world and scientific knowledge might be put to practical use to benefit humankind. With mathematical approach an attempt is made to combine theoretical knowledge with practical results. Considering the physical effects of the heavenly bodies on the earth, the Arab astrologers study their effects on weather, climate, illness, and human and animal behaviour. This evolution renders astrology more reliable, and is preserved as a science for its common utility; by the 10th century a tradition was represented by a host of Arabic texts devoted to "anw'a" as being concerned with the utility of the rising and setting of constellations for agricultural purposes, and for the general purposes of daily life. Yet, there is a tendency to insist upon the native Arabic background of that kind of knowledge.[23]

In the light of all these traces, which manifest the stages taken by the Arabs in dealing with the Greek sciences and the manner they rendered them to their successors, if we have to say a word about the transmission of the Greco-Arabic learning, whose stages and personages have already been known,[24] we can only refer here to some of its consequences:

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[16] James Gow. *A short history of Greek mathematics*, New York, repr. 1968, p. 30.

[17] Shunharo Ito. The Medieval Latin translation of the data of Euclid, Tokyo, 1980, pp. 15-16.

[18] James Gow. *op. cit*., p. 309.

[19] Howard Turner, *op. cit*, p. 60-61.

[20] Elesapeth Witiney, *Medieval science and technology*, London, 2004, p. 59 ff.

[21] Gcorge Saliba, *Islamic science and the making of the European Renaissance*, Cambridge, 2007. pp. 94 ff.

[22] For their works and contributions see: Ibn Said al-Andalusi, *Tabakat al-Umam* (*ranks of nations*) ed. Hussein Mones, Cairo, 1998, pp. 90-92.

[23] Julio Samsó, "De Nuevo Sobre La Traduccion Arabe de Las phaseis de Ptolomeo y la influencia clásica en los kutub Al-Anwa", *Al-Andalus*, vol. XLI, fasc. 2, 1976, pp. 474, 477; and also, Saliba, *op. cit*., 86.

[24] Manuel Alorso Alonso, "Traducciones del Arcediano Domingo Gundisalvo", *Al Andalus*, vol. 12 1947, pp. 296-309; G. Gabrieli, "The transmission of learning and literary influences to western Europe" in *Cambridge History of Islam*, vol. 2, Cambridge, 1970, pp. 851-889.

first of all, the recovery of classical sciences; secondly, the great interest in Arabic sciences and the development of scholasticism, a distinctive form of analysis, which presents pro and con arguments on carefully defined questions. Scholasticism had its origins in the speculative atmosphere of the 12th-century Renaissance, but soon became the basic method of intellectual argument in the universities of the 12th century and later centuries. These factors made medical science a vital and ever-evolving enterprise.

Conclusion

As the Arabs aroused the interest of the Latins in classical studies, scientific or philosophic, reference has been made to the trend towards the study of the languages of that Greco-Arabic tradition. This interest was not confined to the 12th and 13th centuries, but the movement extended to the 15th and 16th centuries and linked the Middle Ages with early Modern, and enriched the Renaissance with the ancient heritage.

In this brief overview, we pointed out the stages the Greek tradition passed through in the hands of the Arabs, through which it received an evaluating assessments, from the refinements of the perceived mistakes to the modifications that changed its parameters as a result of raising questions, and of increasing need for precision; some disciplines received transformations, others witnessed great overhauls. New disciplines emerged and were given strong pushes to its progress by the Arab scientists who pursued empirical research and its applications, and explored the utilitarian benefits of scientific investigations. Thus the scientific method, that the Arabs adopted, had specific influences on individual branches of sciences. Nevertheless they often show understanding to the different branches of sciences as having both theoretical and practical sides. This prevailed in the tradition they produced, which raise several problems and expose a variety of issues to research and analysis, and aroused the intellectual questioning to be promoted in the universities of the Latin West.

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NATURAL PHILOSOPHY AND RELIGION IN BYZANTIUM: DIALOGUE OR CONFLICT?

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Abstract

The work attempts to record the study and the evaluation of the interpretative approaches of Byzantine scholars for natural processes. These interpretations can focus certain views of the questioning which was present among the scholars in Byzantine territories from the 9th to the 14th century.
The varied interpretations for the natural processes which were derived at that period do not reflect only the aspects of their supporters but indicate the influences of the clergy as well and the apprehension of the ancient Greek conceptions within Byzantine scholars.
The views of the clergymen have special interest and importance because during the period we study Orthodox Church was transforming its attitudes towards the study of the Aristotelian corpus, from polemics to reserved acceptance.

Clarifications

The 9th century can be considered as a turning point in the history of philosophical and scientific thought in the Byzantine Empire. On one hand, that century was just after the Late Antiquity and the subsequent Dark Ages and, on the other hand, marked the beginning of the so-called *Byzantine enlightenment*. The 14th century indicates the terminating point of the Empire as it gradually collapsed and was finally conquered by the Ottomans during the 15th century[1].

Some criteria for the selection of the presented phenomena are: the frequency of the records found in the works of various Byzantine scholars, the theological initiatives and outcomes of them and the conjunction between Christian and Hellenic conceptions achieved by the scholars. The preference on them indicates the hassle between the followers of the Aristotelian tradition which was *the only* scientific method at that time –if we may use this term– and the supporters of metaphysical explanations. In other words: it indicates the conflict between rational interpretations and spiritual justifications for the natural processes which was present during medieval period.

[1] Paul Lemerle, *Le premier Humanisme byzantine, Notes at remarques sur enseignement et culture à Byzance des origins au Xe siècle*, Paris 1971, p. 253.

The background

The main figure in history of ancient sciences is undoubtedly Aristotle. He set up a scientific method trying to interpret natural processes and establish a complete qualitative, interpretative method for almost everything which could be approached by human senses. Due to its completeness, the Aristotelian method and interpretation of the phenomena inspired a lot of scholars during the ancient and medieval period and created an interpretative tradition which was active and stood until the Renaissance, when the Copernican revolution and Galilean inventions and innovations necessitated the change *Du monde de "l'a peu pres" a l' univers de la precision*" (=*moving from the Cosmos of 'approximately' towards the exact universe*) if we use the old phrase by Alexandre Koyre.

Aristotelian cosmology was the main method of interpreting the natural phenomena in the Byzantine Empire. The reasons for it were not only its completeness but also its correspondence with the Church's principles for the cosmic view. Thus Aristotelian theory remained in the mainstream of the philosophical thought of Byzantine scholars for almost the whole duration of the Empire despite some problems which occurred in the beginning by the new religion and the biblical tradition[2].

After the 9th century major theological conflicts had been solved, and a mutual understanding between the Christian Church and Aristotelians led to the formation of a new cosmology which tried to include principles from both traditions.

The main difference between these two traditions had remained the acceptance of eternity of the Cosmos from the Aristotelian philosophy and the rejection of it from the Christian theology. But this difference in view was put aside by Byzantine scholars, although they mention this difference in their works and they follow the basic principles of the Aristotelian philosophy trying to comprehend Nature or interpret observed phenomena.

Sometimes, they even put in their works exactly the same words that Aristotle had used in his works. For example if we look at the definition of space proposed by Symeon Seth (a scholar who lived in the 11th century, studied most of the corpus of ancient greek philosophy and presented their views to the Byzantine audience) in his *Synopsis ton Fysikon (Conspectus of the Physics)* written sometime after 1058[3], we see that he defines space as "the termination of the container which contains the containment[4]".

Also, if we turn at Nikephoros Blemmydes, a distinguished scholar who lived 150 years later than Seth in the exiled capital of the Byzantine Empire at Nicaea in Asia Minor (1197-1278) –he is contemporary with Thomas Aquinas– we see that in his *Epitomi Fysikis (Summary of Physics)* which was written after 1258 referring to space he uses the same Aristotelian words as Seth did 200 years earlier: Space is "the termination [end] of the container which contains the containment"[5].

But there is a great difference between these 2 scholars' words though they sound to be the same. This difference has to do with the period these 2 scholars lived. Seth lived in an era where the concepts *Greek* and *Christian* had not yet combined into a single meaning as they later did. So, although he seemed to accept the Christian principles for Nature and Cosmos and had written his *Synopsis ton Fysikon* in the Greek language, on the other hand he referred to the Greeks as pagans and non-Christians[6].

On the other hand, things were easier for Blemmydes, as he lived during an era where the Byzantine Empire had lost its capital, conquered by the Crusaders of the 4th Crusade, and Byzantines, trying to overcome this shocking situation, sought for a new identity looking westwards, to Greece and its glorious ancient times.

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[2] Nicephoros Gregoras, a distinguished scholar with theological studies and knowledge on Greek philosophy (13th century), proclaimed Aristotle as a source of wisdom. See Nicephoros Gregoras, *Historia Romana*, 3, p.256.

[3] The text was written 200 years before the major translations of the Greek texts into Latin, during a period that Seth's ancient sources were not spread in the West. The work of Seth has been edited by A. Delatte together with another work of Seth (the astronomical essay *On the Need of the Heavenly Bodies*) in the volume *Anecdota Atheniensia et Alia (tome II) textes Grecs Relatifs a l'Histoire des Sciences,* Liege 1939, p. 16-120. My references will be to Delatte's edition. Seth mentioned in the chapter on the eclipses that "here, during Comnenus' reign the whole sun was covered as we have personally observed there" (*Summary of Physics, Anecdota Atheniensia*…, p. 53). That eclipse should be a total solar eclipse visible from Egypt where he was during that period. Our enquiry has found out that during that particular period two eclipses had occurred. The one mentioned by Seth should be the one that took place on 25th February 1058. It started at 1:36 pm at South America and by 2:50 pm the shade covered the whole of Egypt. See also, Manolis Kartsonakis, "The *Synopsis ton Fysikon* of Symeon Seth", in G.N.Vlahakis – E.Nicolaides (eds.), *Byzantium – Venice – Modern Hellenism*, Athens 2004, p. 129-137 (in Greek).

[4] *Anecdota Atheniensia*…, p.64.

[5] Nikephoros Blemmydes's *Epitomi Fysikis* has been edited by J.P.Migne at his *Patrologia Graeca,* Paris 1857-1866, vol.142. The definition about space is at *PG 142*, col. 1100B and at col. 1104D.

[6] At this point we are quoting his words for the Chinese people: "… The Chinese people live at the eastern part of the earth. All of them are ***Greek*** in their faith…". *Anecdota Atheniensia…..,*p.23.

The phenomena and its interpretations

Rain and hail.

We have interpretations proposed by Michael Psellus, the distinguished scholar and nobleman who lived in 1018-1078, his contemporary Symeon Seth and Nikephoros Blemmydes. Psellus interpreted rain and hail as the result of gathering clouds occurred during wintertime, when the sun's influence on earth is less strong[7]. Seth says that it is created by frozen raindrops before they reach the surface of the earth. He also mentions that, if this procedure takes place high in the sky, away from the surface of the earth, then the created hail becomes spherical as it is fallen onto the earth (because of the friction of the air), otherwise –if hail is created closer to earth's surface– each particle of hail remains with edges in different shapes[8].

On the same subject, Blemmydes gives us a more detailed description of the creation of rain indicating that rain is created by the uprising vapor from the earth's surface. For Blemmydes, rain –being a variable quantity of Nature– is created in the sub-lunar part of the Cosmos. It becomes liquefied and frozen due to meteorological reasons and falls again onto earth as hail, rain or snow[9].

A very interesting point in Blemmydes' interpretation of this phenomenon is the reference of an ancient Greek method of interpreting natural processes called antiperistasis. This method was initially introduced by Plato at *Timaeus* under the name *periosis*[10] and was used by followers of Aristotle as a method of violent motions' explanation. The term *antiperistasis* (or *periosis*, if we follow the platonic terminology) describes the transposition of one part of air from the front side of a moving body towards the back.

The acceptance or the rejection of antiperistasis from scholars can be considered as an indication of the way they treated the existence of vacuum: Antiperistasis, as an interpreting theory of natural processes, was easily accepted for the devotees of *horror vacui,* while the supporters of the possibility of vacuum's existence could not accept it as sufficient because the main concern of this theory was to impede the appearance of a void space[11].

Earthquakes.

On earthquakes, we have several references indicated by Byzantine scholars as far as this phenomenon is very dreadful and thundering. If we focus on this phenomenon we will see a conflict that was present in the societies during the medieval centuries: On one hand we have the attempts for logical interpretations, and on the other hand we have metaphysical beliefs and superstitions for them.

We have early references for earthquakes that happened in Antioch in the 9th century, mentioned by Georgios Monahos at his *Chronikon* (written in 813-842)[12]; at the reign of Constantinus X Dukas (reigned in 1059–1067) mentioned by Michael Attaliates –a notable chronographer and historian of the 11th century– in his *Historia*[13]; in 1116-17 mentioned by Anna Comnena (1083-1153) in her historical work *Alexiad,* dedicated to her father's Alexios I Comnenos' reign[14]. The 13th-century historian George Acropolites mentioned two very strong earthquakes (on 11th March 1231 and 16th September 1237)[15], and the historian Georgios Pachymeres also mentioned strong earthquakes in 1265, at Dyrahio (present-day Albania) in 1270 and on the 1st June 1296[16].

Psellus, 300 years earlier, had noticed that the cause of earthquakes is the process of the fire which took place underground in the earth and caused dryness. This process creates a dry spirit which moves rapidly upwards and causes an earthquake[17]. His contemporary Seth exposed his explanation based on natural causes. Seth said that when earth is being

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[7] Michael Psellus*, Opuscula logica, physica, allegorica, alia*, opus. 19, 20, 22, 30, 34, 36, 55.

[8] *Anecdota Atheniensia*…,σελ. 29 – 30.

[9] *PG* 142, col. 1141 – 1151.

[10] Plato, *Timaeus*, 59a, 79b, 79c, 79e and 80c.

[11] See also, Manolis Kartsonakis, "The concept of antiperistasis: Supporters and counters of vacuum space in Late Antiquity and Middle Ages", in G.N.Vlahakis – P.Fildisis (eds.), *The views of ancient philosophers for the natural sciences and their influence in modern philosophical thought*, Athens 2007, p. 109-130 (in Greek).

[12] George Monahos, *Chronikon*, book 1 – 4, p. 22, 221, 255, 267, 311, 315 – 316, 445, 502, 543, 560 – 561, 586, 593, 604, 622, 626, 641 – 642, 744, 760, 788, 798.

[13] Michael Attaliates, *Historia*, ed. Immanuel Bekker, *Corpus Scriptorium Historiae Byzantinae* 34, (Bonn: 1853) p.88.

[14] Anna Comnena, *Alexiad*, Book 3, ch.5, sec.2 and Book 15, ch.8.

[15] George Acropolites, *Historia*,ch.120.

[16] George Pachymeres, *Historia*, Book 6 p. 15, 167, 232 & 233 (for the earthquake of 1296), 377, 457, 459, 641 and Book 7, p.6, 8, 134, 233, 234, 355, 360, 361, 36, 384, 392, 293.

[17] Michael Psellus, Opuscula logica, physica, allegorica, alia, opus. 26,29,30.

heated, steam and vapor have to be arisen. Steam is moist and vapor is warm and can move easily. When the vapor is being haltered in the earth trying to find an exit, it moves the contiguous earth and brings on earthquakes. Earthquakes take place during windless days and strong winds occur afterwards. He also mentions that only once, during Christ's crucifixion, an earthquake took place all over the earth. Otherwise earthquakes are considered to be local phenomena[18].

Blemmydes also continued this way of thinking and believed that earthquakes are created by the warm and dry vapor that exists under the surface of the earth. This vapor is gathered, fills the underground pits and, trying to shift upwards, moves the surrounding area and consequently the earth's surface. Trying to interpret this phenomenon, he agreed that most of the strongest earthquakes occur during windless periods and at nighttime (because most of the nights are windless). If an earthquake occurs during daytime, it is at noon, because noontime is the most windless of all as far as sunrays are stronger and vertical towards earth[19].

On the other hand, there were scholars and clergymen who had considered earthquakes as metaphysical appearances and had connected them with God's Will and Punishment: i.e. Monahos mentioned earthquakes as a sign of God's disagreement for human behavior on certain theological and social issues, Pachymeres seems to accept divine, superhuman origins of earthquakes connecting them either with God's wrath or a warning of bad forthcomings[20]. Nikephoros Gregoras, considered them as "God's driven phenomena for people's repentance[21].

Comets.

The various appearances of comets had attracted most of the Byzantine scholars and were mentioned in their essays. Starting from Monahos's *Chronikon,* we are informed that during Emperor's Leon V reign (813-820) there was a comet's appearance[22]. Also, Attaliates recorded a comet's appearance during Constantinus X Dukas' reign (1059-1067) in May 1066 and lasted for 40 days, as he informs us[23]. Anna Comnena in *Alexiad* mentioned that a comet was visible during February and March 1106[24]. Acropolites and Pachymeres indicated comets' appearances during the 13th century, and Gregoras referred to various appearances of comets during the period 1204-1359[25].

Apart from these historical references on comets, we have noticed attempts for interpretation of this phenomenon. Seth proposed that they are created by vapor arisen from the earth. Up in the border between sub-lunar and celestial cosmos this vapor is mixed with cosmic fire and transformed into a comet. According to Seth, people that have seen a comet consider it as a mischievous monster.

Blemmydes also followed this Aristotelian view that they are created by vapor arisen from the earth. But, likewise the case of earthquakes, there were several scholars who had proposed a direct or indirect connection between the appearance of a comet and a bad upcoming event. Even Pachymeres named comets as "a signifier of bad events"[26].

Eclipses.

Another very impressive phenomenon which attracted various scholars had been eclipses, both solar and lunar. We have marked records concerning eclipses since the 9th century, made by Monahos and Emperor Constantinus VII Porfyrogenitos himself (reigned in 913-959). Also, in the 11th century there are records for eclipses made by Psellus, Seth, Attaliates[27] and Anna Comnena[28], during her father Alexios I Comnenus' reign. Blemmydes has mentioned a lunar eclipse which was observed in the evening of 18th May 1258[29], Acropolites indicated a solar eclipse at 1238, Pachymeres and Gregoras as well.

Most of these scholars have referred on them as natural phenomena, and they had understood their origin. Seth, for example, indicated that solar eclipses take place when the moon is intercalated between the sun and the earth. He insisted that solar eclipses take place locally and he referred on the solar eclipse that occurred at 25 February 1058, which was

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[18] *Anecdota Atheniensia*…,p. 31-33.

[19] *PG* 142, col. 1171-1177.

[20] George Pachymeres, *Historia*, Book 6 p.353 & 375-377.

[21] Nicephoros Gregoras, *Historia Romana*, book 1, p.32.

[22] George Monahos, *Chronikon*, Book 1-4, p. 607,643, 661 & 777.

[23] Michael Attaliates, *Historia*, p.17-25 & 97.

[24] Anna Comnena, *Alexiad,* Book 12, ch. 4, col.1.

[25] Nicephoros Gregoras, *Historia Romana*, book 1, p. 98-99 & 542.

[26] George Pachymeres, *Historia*, Book 6, p. 295 as well as p. 11, 47, 301 and 401 and at Book 7, p. 304, 305 and 530.

[27] He referred to a lunar eclipse which took place during Michael's VII Dukas reign (1071-1078).

[28] Her reference is for the total solar eclipse which took place in the morning of 1st August 1087. *Alexiad*, Book 7. Ch.2.

[29] *PG* 142, col. 1265c.

visible at Constantinople as we have mentioned[30]. Also, referring on this phenomenon, he mentioned that eclipses may be total or partial depending on the location of the observer. This particular one was total at Constantinople, but partial at Egypt, where he was at that time.

On lunar eclipses, he opined that they were acted out when the earth was located between the sun and the moon so that the earth's shade can cover the moon. He also mentioned that lunar eclipses are either partial or total too. And he hinted that the earth is spherical because lunar eclipses can be seen only from places where it is nighttime[31]. So, he concluded, it is obvious that at the same time some places have nighttime and some other daytime at the same time.

Blemmydes believed that they are natural phenomena and agreed with Seth about the causes for solar eclipses. He declared that the only eclipse based on metaphysical reasons was the one that occurred during Christ's crucifixion. According to his words, "at that time, the moon motivated by the God's Will moved westwards and covered the Sun for 3 hours"[32].

He also agreed with Seth for lunar eclipses. He described a lunar eclipse that he saw once. We have established that this eclipse would have been the one that took place during the night of 18th May 1258.

Lightning and thunderclaps.

On lightning and thunderclaps, Michael Psellus referred to Aristotle's explanations mentioned in *Meteorologica*, where lightnings are supposed to be created by dry steam arisen from earth to heavens. He also presented a view that thunderclaps have to be considered as results of clouds' motion up in the sky. He also commented on the difference of time that takes place in noticing them. He attributed this difference at the different manufacturing of ears and eyes. So, though lightnings and thunderclaps have the same moment as starting point, we realize them with a slight gap[33].

Seth mentioned that we should not consider them as results of conflicting clouds but as results of warm spirits rushing through clouds[34]. Blemmydes described in details the mechanism of thunderclaps' creation based on the rise of the dry vapor and its thickening in clouds. Later on this thickened vapor has become spirit and is expelled from clouds with noise and this is a thunderclap. As it is smashed, it falls off the clouds colored with the color of fire, and we call this lightning[35].

Conclusions

We will outline briefly our conclusions so that we can spot more accurate on the question set in the title:

1. It seems that there was *a living tradition* in Byzantium concerning issues of Nature. This background was influenced by the corpus of the ancient Greek scientific thought represented by Aristotle himself in his various works and of his commentators' texts of Late Antiquity of the Greek-speaking world.
2. The Church did not fully approve studies on them at the first period of the Byzantine Empire: they represented the Hellenic Cosmos, which was non-Christian. On the other hand, a number of distinguished and ingenious Holy Fathers had undertaken serious efforts in order to compromise ancient Greek principles found in platonic and Aristotelian texts and major Christian principles. So this theoretical process led to mutual understanding from both sides and, as centuries went by, Church's opposition was transformed to a silent acceptance on studies of Greek texts on Nature by Byzantine scholars. All these texts had been archived and preserved not only at Constantinople's libraries, but in various libraries all over the Greek mainland[36], in their original language.
3. Distinguished scholars have recorded various phenomena in their works so that we have very useful historical references on these phenomena. Those scholars were educated officials, noble men and women, statesmen, even educated Emperors! All these records give us a significant view of certain natural

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[30] See footnote 3.

[31] *Anecdota Atheniensia*…,p. 53 & 57-59.

[32] *PG* 142, col. 1252.

[33] Michael Psellus, Opuscula logica, physica, allegorica, alia, opus. 19 & 22.

[34] *Anecdota Atheniensia*…,p. 30.

[35] *PG* 142, col. 1180 -1186.

[36] It is remarkable to mention that, during the first decades of the 13th century, during the Latin occupation of Constantinople, Nicephoros Blemmydes, who had undertaken the role of the tutor of the Crown Prince of the exiled Byzantine Empire Theodore II Lascaris, had traveled at the mainland and insular Greek territories searching for archives for his distinguished student and he had found manuscripts in several libraries. See C.N. Constantinides, *Higher Education in Byzantium in the Thirteenth century (1204-1310)*, Nicosia 1982, p. 7 – 27.

phenomena which either had appeared in the areas of the Byzantine Empire or had happened in former times.

4. Certain scholars had moved one step beyond. Having studied the corpus of Aristotelian Physics –including texts of Aristotle's commentators written during the first centuries AC and the Late Antiquity– they had been inspired by it and they tried to interpret natural phenomena using Aristotelian principles combined with their Christian beliefs. This methodological approach of interpretation took place mainly after the 11th century until the paleologian era, which indicates the final collapse of the Byzantine Empire in 1453. These four last centuries were very fruitful for the philosophical and scientific thought at the declining Empire. During that period, several scholars had contributed significantly to the interpretation of natural phenomena. Especially, efforts undertaken during the 11th century have to be considered as very important because they took place 200 years before the massive translations of the Greek texts into Latin: Psellus and Seth knew what Aristotle had said on Nature whilst contemporary Latin scholars did not have the opportunity to study Aristotelian texts at the same era.
5. Nicephoros Blemmydes has to be considered as a major figure among the medieval scholars, but the interpretative approaches on natural phenomena based on the combination between Aristotelian and Christian principles proposed by him and his followers during the 13th and 14th centuries were not sufficiently spread among the scientific community of that area, as Thomas Aquinas' similar ones did. The reasons for this can be traced on social and nonscientific fields: i) the central location of the scientific evolution was not in the Mediterranean areas, and was moving westwards through the massive oncoming translations of the Greek corpus into Latin; ii) the key role of the Greek language as the mother tongue of the scientific texts was shrinking; and iii) the communication between the Latin and the Byzantine world was lacking under the pressure of the increasing religious and political conflicts[37].

So, Byzantine scholars introduced the methodology of combining Aristotelian and Christian principles independently from similar efforts undertaken in the West.

[37] It is considerable to indicate that Nicephoros Gregoras had achieved to prepare a new calendar which was correcting the irregularities on the calculation of Easter occurred in the Julian calendar. He presented his new calendar in the Court of the Emperor Andronicus II Paleologos at 1324. His work did not have any success on replacing the Julian calendar (250 years before the Gregorian reform of 1578) because the Emperor hesitated to put it in practice as –using the Emperor's words– "it was difficult to persuade foreign leaders to accept this reform". See Nicephoros Gregoras, *Historia Romana*, book 1, p. 367 – 373.

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HISTORY OF PHYSICS AS A DIDACTIC TOOL

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Abstract

The history of physics provides the students with recognition about the history of human efforts in science. Through this discipline they become familiar with the lives of great scientists and the gradual evolution of ideas. Although there are several exact theories accepted by all, scientific theories are still open to be attacked by new experiments and new ideas. On the other hand, through this discipline, the pupils or students have the opportunity to ask and to be active in practice, to collect historical material in order to establish a museum of physics, or to set up software according to the historical periods of development of science and technology.

As an attempt to find teaching strategies to improve the teaching of science, we should consider the history of science as a teaching tool and as a description of integrated school curricula. The purpose of this article is to treat some of these problems addressed on the Course of History of Physics. This course will reflect the overall development of the main areas of physics, as we know today. It will appear as a clear linear model, and develop the main forms in each area, which will evidence that science is the attempt to give a rational explanation of the laws in nature, more accurate than magical or religious explanations.

People in different countries of the world began to develop science at different times, with different accents. Looking at the history of science as part of the culture to transmit to school can be argued that this emerged as a discipline in itself.

According to our opinion the history of science can be considered as a part of curricula and will be appropriate to observe possible results of the introduction of historical arguments.

- The first result is the transmission of information on the historical development of knowledge, with the relation science-technology-society.

- The second result is the best acquisition of physics through analyzing the physical ideas from a historical viewpoint.

- The third result is engagement of students in practical activities, projects, papers and assignments to build a museum of history of physics. It can lead to a greater integration between the human and scientific area through an interpretation of the history that realizes the ratios; man-environment, human-nature related with the development of technology through integrated analysis in social and economic form.

Introduction

Man has made efforts to recognize the world in which he has lived, not instantly and not available in definitive form. For example, recognition of the physics laws, which already are known by all the science with the name of "Newton's Laws" needed a long time for recognition: some thousands of years. It also needed two and a half centuries understanding the limits of these laws: they are not universal, and should be specified for objects that move with very large speed, comparable with light speed, and for very small objects (particles).

Humankind has passed and will always pass through a long and difficult way, from acknowledge to recognition, consistently replacing on this path the incomplete and imperfect knowledge with the more complete and more sophisticated knowledge. In a few words, knowledge has historical character, science has history.

Essentially, any researcher should be informed on what was done before him about the problem that is being studied and should evaluate critically the results obtained from his predecessors. Nothing is born in an empty space. Each new generation starts at the place where the previous has stopped and gives its obtained results to the next generation. Without Euclid and Archimedes, we wouldn't have Newton. Without Newton we wouldn't have Einstein and Bohr. The study of the development process of recognition represents the task of a specific science of the history of science. The history of physics is a part of the history of science concerned with the study of the historical development of the physics science.

The main task of the science is to discover the laws that govern each area that it is concerned with. The main task of the history of science refers to finding the laws that govern the development of science. At first sight, it might look like such laws do not exist. The appearance of Archimedes, Newton, Einstein, Planck, could not be predicted; the scientist's thinking and creating cannot be directed. Outwardly, the history of science can be visualized as a result of uncontrolled activity of the particular genius scientists. But it is incontestable that the science is the product of human activity and by far the most complex, intellectual, cognitive and creative activity. From this point of view, the development of science occurs in historical, economic and cultural conditions, which play an important determining role. These conditions represent the real basis of human activity, including the basis of their spiritual activities. In this way, the start and development of science is possible in a specific level of economic development. The start and the development of science are characterized by the production. Especially, this role in developing of science is clearly evident in today's historic stage, where the development of spacecraft and plasma physics, etc., requires a staff and super-colossal qualified tools, namely a powerful and developed education system. It is understood that a strong economy is required to ensure all conditions for the developing of science today. Economic conditions create the necessary bases for all life of society, including science. But other factors play a crucial role for the existence of these bases; so, for any researcher, the internal factors are determining: the status of scientific knowledge, the actuality of the problem, interests and personal skills, etc.

Science not only gains independence, but influences on the social production, stimulates and accelerates the development of the productive forces, being themselves transformed into the productive force. We note that the correlation of science and production has also an historical character, and it is developed with the development of the science production.

Through what we have presented, it is clearly apparent that the task of the study of science development legality, including physics, has a completely defined meaning and a great scientific value. Today, when science itself is the development factor of society, especially this task becomes prevailing. The history of science plays a principal role in the theory of recognition. Here, it consists in the fundamental scientific value of the history of science. Also, the history of science has methodological and didactic value. Quite often the historical way of the recognition of science is the most effective. Therefore, for teachers of physics, recognition of the history of physics is necessary, by providing them methodically and scientifically. The history of science educates the love and respect for science and scientists, helps fighting against dogmatism and formalism and wakes up scientific cooperation between educated people.

Historical aspects

There is much debate and discussions on the role of science history in didactics of physics. The opinions in this debate are contradictory. Some underestimate the role of the history of physics with the following argument: "Since a theory tends asymptotically towards an instant stable, logically independent, then why should I get lost in the labyrinth of history?" Others are skeptical: how can we achieve within a short time to learn everything about the history of science? A significant part (including the authors of this paper) are optimistic, seeing the history of science as a part of intellectual culture that should be taught in schools as a subject in itself. That's for the fact that the history of science is the arena for continued discussion of scientific and philosophical problems. In the learning process we can see how science is extended and deepened, introducing its own sphere of new objectives for science. So, discovering new properties and ratios, we can see how the old concepts are overturned and how recent concepts are affirmed and, based on them, the new methods, theories, and recognition procedures are created.

Considering that the history of science should be treated as an independent part of curricula, we find the worth opportunities to observe the presentation of historical arguments, limited to placing emphasis on the didactic values of the history of science. On the other hand, this independence of the history of physics as an independent subject provides us the opportunity for transmission of information in the conceptual development of science, being limited in their methodological valence.

Three important benefits are derived from the above arguments, which are closely related to a particular science discipline of physics. Firstly, a better understanding of concepts can be obtained through their treatment in a historical point of view. Historical perspective based on the source materials of scientists, correspondences between them, debates or discussions in seminars, scientific conferences and congresses, has a huge significance for two reasons: to show how the idea or theory is born, confirmed and developed facing the opinions and viewpoints, pros and cons, with all scientists in a specific constellation.

Secondly, to create good perceptions among students, that physical science is not a definitive ready product of one or several of the brightest minds, but a human real effort of several generations of physicists. Only the literature of the history can draw this picture, which also serves to convince students how much sweat, effort and desire is needed to confirm an idea, hypothesis or new scientific theory, when even the brightest, greatest minds at a period of time are often confused and accept new scientific ideas with difficulties, but before the experimental facts “force” even the stubborn conservative people to renounce old perceptions. This is the way of recognition of the scientific truth, which is always relative, and also is developed and perfected.

Another conclusion, which underlines the methodological aspect, means the whole conditions and principles which the concrete methods of study and description of physical phenomena (dynamical and statistical methods of description of physical condition) are based on. Relying on them, physical doctrines (classical and quantum) can be built, and the application limits of theories can be defined. The methodologies of physics in this sense may undergo substantial changes not only replacing a philosophical base of physics to another, but even when changes occurring in science do not doubt the philosophical basis of science that supported those changes.

Methodological systematization of the scientific thinking experience and the history of scientific thinking is, of course, not the same as the systematization of knowledge and scientific discipline procedures. In the latter part of the sentence, systematization aims at the realization of a logical and coherent order of scientific thinking in limitations of any known area. Otherwise, the methodological systematization aims at realization of an interdisciplinary relationship based on the scientific thinking experience and his historical carriage from one area of knowledge to another. Since our knowledge is not a priori, but derived from all human experience analysis and generalization, every human insight to the novelty and unexplored area of phenomena, leading to the necessity of the radical revision and the generalization of our fundamental concepts and perceptions, even such as time, space, action, motion, energy, etc.This, of course, doesn’t mean that every new stage of scientific development disclaims everything previous. The using limitations of those concepts and laws which were earlier considered universal are emerged at each new stage and the lawfulness of a more general character are discovered.

The requirements to any new theory is that it becomes increasingly stronger because it not only needs to explain continuously discovered facts, but also to include the special cases of all earlier discovered lawfulness, noting the exact limits of their use. Thus, the classical physics fundamentals are included in the more general laws of relativity and quantum theory, of which they are derived, in conditions when the object speeds are very small compared with the speed of light, whereas the space-time grades of the phenomena and the measures of objects are such that the action to be large compared to the quantum constant.

Conclusions

The treatment of the history of science in its conceptual aspect increases the logical valence of the birth of new ideas. The path toward birth of new ideas is not simple. The novelty is hardly affirmed through a “tough fight” with the older one. Across the unexplored path of new theory, firstly “the track is opened”, the hypothesis or new scientific ideas are postulated, which are not born in a vacuum space and at every man. After this moment the “crisis” is started of existing perceptions or theories which are unable to explain new experimental facts. Already, the role of the scientist appears, his intuition to connect between them two different groups of facts. This connection decides the order and consequently the simplification and perfection of the scientific picture of the world.

The above mentioned conclusions aren’t anything else, except some of the practicable possibilities. So, we find ourselves in front of a wide spectrum of opinions that ranges from a full teaching to a directed teaching in an in-depth manner in an intellectual and cultural sense, which doesn’t require ordinary knowledge of the history and methodology of physics. Based on the training establishment level of teachers, precisely to focus on above mentioned aspects, it may be valid to compare the arguments in a more clear structure in a historical and methodological viewpoint. Devoted to this idea, we have presented the following proposal, which not only serves as structure to the course of history of physics, but as well as laboratory of the history of physics in the recognition aspect.

References

[1] Loci, Mario (1970) *Istorija Fiziki*, M.Mir.

[2] Llaue, M (1958) *Istorija Fiziki*, M.GITTL.

[3] Sposkij, B.I. (1963) *Istorija Fiziki Cast 1,2 Izotovo*. MGU.

[4] Strojk, D.J. (1969) 'Kratky ocerk istorii matematiki' M. *Nauka*.

[5] Bunge,M. 'Filosofije fiziki' M. *Progress*.

[6] Frenkel, I.J. (1975) 'Nazarje novoj fiziki,Akodemija Nauk SSSR Izof'. *Nauka* Lengal.

[7] Meljuhin, S.T. (1966) *Filosofskije problemi* M.Znanije.

[8] Massey, H (1964) *L'ere nouvelle des sciens physiques*. Paris, Dund.

[9] Hajzenberg, V. (1953) *Filosofkije problemi atomnoj fiziki* M.

[10] Kudrjavcev, P.S. (1974) 'Kurs istorii fiziki'. M. *Prosvescenie*.

[11] Slavin, Allen I. (1994) *Brief History and Philosophy of Physics*. Departament of Physics, Trent University. August.

[12] Mason, S.F. (1964) *A History of Sciences*. Callier, New York.

[13] Hargon, I. (1992) 'Quantum Philosophy', *Scientific American*, July.

[14] Schneer, C.J. (1960) *The evolution of Physical Science*. Grove Press, New York.

ROCA-ROSELL, A. (ed.).(2012) *The Circulation of Science and Technology: Proceedings of the 4th International Conference of the ESHS, Barcelona, 18-20 November 2010.* Barcelona: SCHCT-IEC, p. 1188.

SCIENTIFIC EXHIBITS FROM THE VIRTUAL MUSEUM OF THE UNIVERSITY OF BARCELONA

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Abstract

In January, the University of Barcelona opened its Virtual Museum. This experiment is a pioneering one in Spain and a project that began with the aim of publicizing the University's entire heritage to students, researchers, scholars and the general public. The system that has been used is a particularly modern and highly specialized database designed for cataloguing all kinds of museum material. Linked to the program, we have also acquired the version making it possible to display the museum on the Internet.
The museum is currently organized in 11 collections and covers about 300 pieces, a small sample of the university's heritage which we have made available to the public in the form of an online showcase. We are now enriching the Virtual Museum with a second phase which introduces new pieces into the existing collections and also expands the number of collections.
This contribution is devoted to the description of the six scientific collections included in this virtual museum.

Introduction

In January 2010, the University of Barcelona opened its Virtual Museum. This experiment is a pioneering one in Spain and a project that began with the aim of publicizing the University's entire heritage to students, researchers, scholars and the general public.

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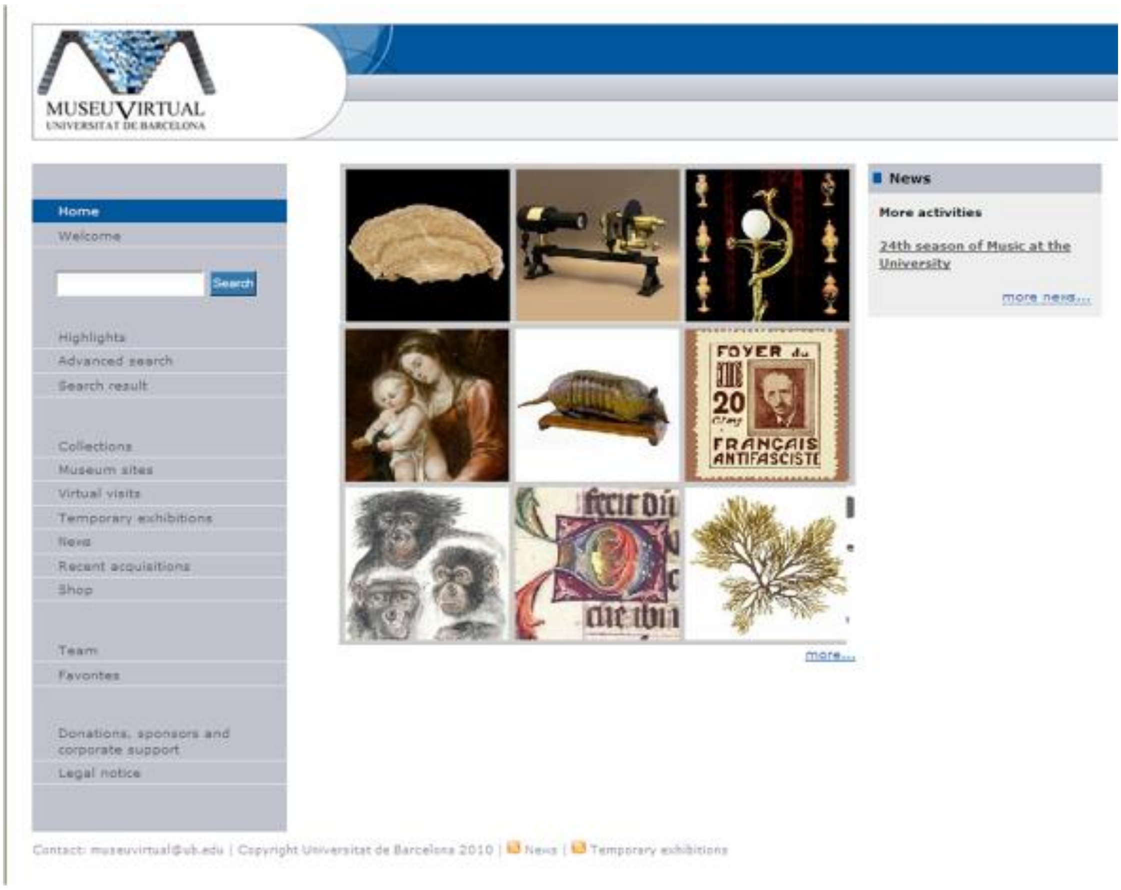
This contribution is devoted to the description of the main features of this virtual group of elements and collections belonging to the Heritage of the University of Barcelona.

Log-in

Access is easy from the introductory page of the University of Barcelona (http://www.ub.edu) on the left hand side:

Or by using the direct link: http://www.ub.edu/museuvirtual/. The general view, home page:



Eleven collections

Art

Art brings together the artistic and architectural heritage of the UB and features a series of sub-collections including the *Pinacoteca*, a gallery of three hundred paintings, fifty-six of which belong to the Prado Museum.

The Fine Arts Collection

The Fine Arts Collection of contemporary art brings together the work of students and teachers associated with the Faculty of Fine Arts and the former Sant Jordi School of Fine Arts. The collection provides a showcase for the trends and tendencies in Catalan art from the early twentieth century until the present day.

Animal Biodiversity Resource Centre Collection (CRBA)

The historic collection of the Animal Biodiversity Resource Centre (CRBA) contains exhibits of birds and mammals from the *Gabinet d'Història Natural*, which was created in 1847. The CRBA also features numerous specimens of different animal groups in its science collection and in the part of the collection providing a study and teaching resource for UB members.

Pavelló de la República *Library Collection*

The *Pavelló de la República* Library Collection brings together documents from the Centre for International Historical Studies (CEHI) and the Josep M. Figueras Library in one of the country's most important collections on the subjects of the Second Spanish Republic, the Spanish Civil War, exile from Spain and the Regime, and the period of history known as the Transition to Democracy, both in Spain as a whole and in Catalonia.

Mineralogy Collection

The Mineralogy Collection is housed in the Department of Crystallography, Mineralogy and Mineral Deposits. With a total of almost 20,000 mineral samples, 7,000 thin sections and 3,000 polished sections, the collection brings together all internationally recognized mineral species and highlights both their aesthetic and scientific value as part of our international mineral heritage. Finally, the collection is also used in classes by teaching staff in the degrees of Geology and Geological Engineering.

Sabater Pi Collection

The Sabater Pi Collection is housed in the Barcelona Science Park (PCB) and contains the scientific and artistic works of UB emeritus professor Jordi Sabater Pi.

Rare Book and Manuscript Library

Housed in the Historic Building in Plaça de la Universitat, the Rare Books and Manuscripts Library contains historical documents relating to the University of Barcelona dating up to the nineteenth century. On a national level and after the National Library and the *Biblioteca Columbina*, the reserve is the third largest collection of its kind in its collection of incunabula, and, after the National Library, the second largest in terms of its printed material from the sixteenth to eighteenth centuries.

The Herbarium BCN (CeDocBiV)

The Herbarium BCN is supported by the UB's Centre for Research on Plant Biodiversity (CeDocBiV) and contains around 300,000 specimens, mainly from the area of the Catalan Countries (*Països Catalans*) but also from the rest of the Iberian Peninsula, Northern Africa and tropical regions of South America.

Scientific Instruments

Scientific Instruments contains more than 500 exhibits of historical interest and is housed in the Faculties of Biology, Chemistry, Medicine, Pharmacy, Physics, and Psychology and in the School of Nursing. The collection offers the visitor a broad range of examples of instruments used in teaching and research from the eighteenth to the twentieth century.

Faculty of Geology Mineral Collection

The Mineral Collection is housed in the basement of the Faculty of Geology building and contains a broad range of rock, mineral and fossil exhibits used in teaching and research.

Catalan Pharmacy Museum

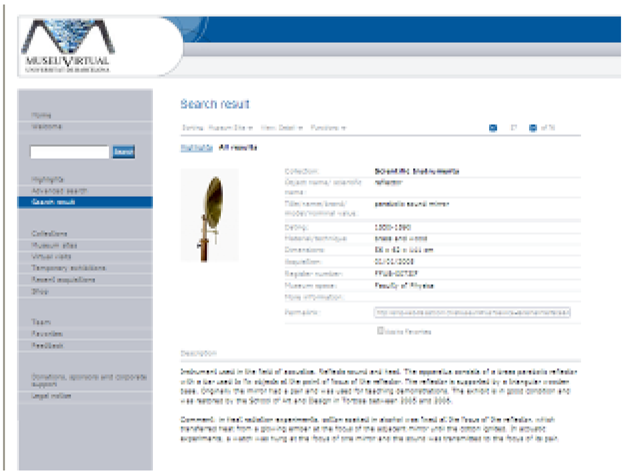
The Catalan Pharmacy Museum gathers together over two thousand objects of different kinds, as well as two emblematic collections: a series of ancient pharmaceutical specialities —the largest collection— and a group of old posters advertising pharmaceutical products.

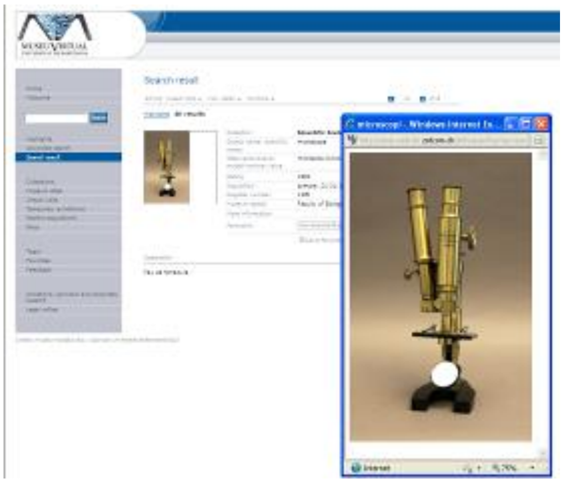
Inside the collections

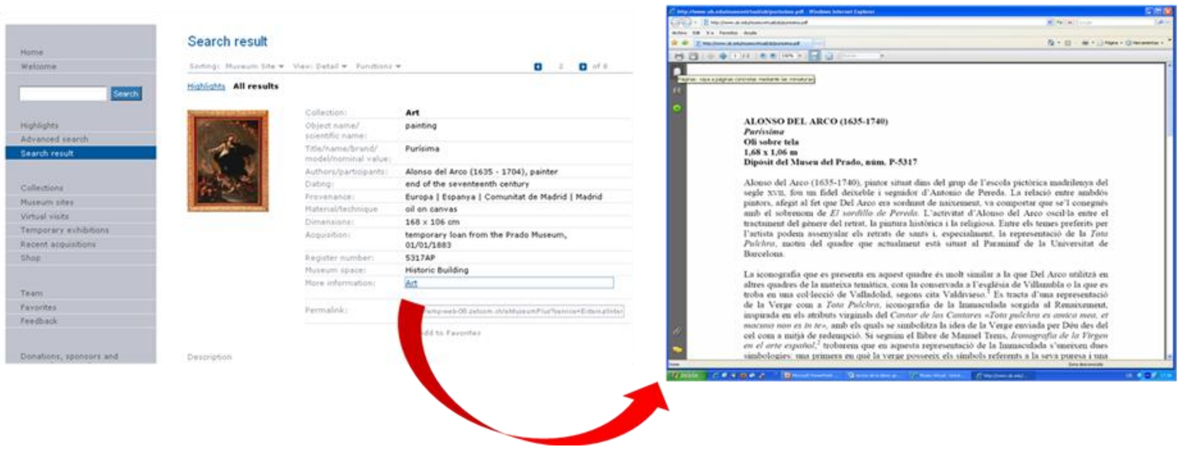
You can have a quick look at any collection by clicking on *Highlights* or you can see the *Complete Collection*. Alternatively you can use the *Advanced search* in order to find some specific item.

By clicking on each item more information should appear, such as the provenance, register number, the title/name/brand/ model, authors/maker, dimensions, materials, and other general information.

It is also possible to broaden the information and see more details by opening attached documents in pdf format. By clicking on each picture an enlarged image is shown.







The Scientific Instruments collection

The section of Scientific instruments exhibits items located in the Faculties of Biology, Chemistry, Medicine, Pharmacy, Physics, and Psychology and in the School of Nursing. At present it contains 76 instruments, offering examples of instruments used in teaching and research from the two last centuries. Among the 22 highlighted instruments there are a complex sectioning microtome, a very early X-ray machine and a magic lantern dated around 1885.

Virtual tours: the Historic Building and the Gaudí Pavilions

At present there are two possible virtual tours. One corresponds to the Historic Building in the centre of the city and the other corresponds to the Gaudi Pavilions, in the North Campus of the city.

The virtual tour of the Historic Building is divided into four main sections: the History of the University of Barcelona; the City; Elies Rogent –the architect of the building; and the Historic Building. Each part is subdivided into several photographs and descriptions providing synchronised information.

Concerning the Gaudí Pavilions, there are three sections: Ceramic and exposed brick; the Garden of Hesperides; and the Iron Dragon, besides information and photographs of several other sections.

More information: News, Temporary exhibitions, Recent acquisitions...

In all these sections all the related news is presented with the relevant information and with the links that allow connection with the original sites.

Conclusions

Since it was inaugurated one year ago, the University of Barcelona Virtual Museum has been steadily consolidating its structure and perfecting the system it uses to exhibit the various collections that comprise our university heritage. We are now enriching the Virtual Museum with a second phase which adds new pieces to the existing collections and also expands the number of collections. To this end, we are preparing the Virtual exhibitions section, which will support the Temporary exhibitions section, where we announce exhibitions organized by the different campuses of the University of Barcelona.

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