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GYORGY MARKUS

Why Is There No Hermeneutics of Natural Sciences? Some Preliminary Theses

The Argument

Contemporary natural sciences succeed remarkably well in ensuring a relatively continuous transmission of their cognitively relevant traditions and in creating a widely shared background consensus among their practitioners – hermeneutical ends seemingly achieved without hermeneutical awareness or explicitly acquired hermeneutical skills.

It is a historically specific – emerging only in the nineteenth century – cultural organization of the Author-Text-Reader relation which endows them with such an ease of hermeneutical achievements: an institutionally fixed form of textual and intertextual practices, normatively posited ways of adequate reception and criticism, etc. The same organization also explains a number of their often-discussed epistemic and cultural characteristics: their depersonalized objectivity, the social closure of their discourse and their reduced cultural significance, the shallow historical depth of their activated traditions, etc.

The cognitive structure and the social function of contemporary natural sciences are intimately interwoven with a set of sui generis cultural relations that are partially fixed in the textual characteristics of their literary objectivations. A comparative hermeneutical analysis of natural sciences as a specifically constituted and institutionalized cultural genre or discourse-type brings into relief those contingent cultural conditions and relations to which some of their fundamental epistemological characteristics are bound, or at least with which they are historically closely associated.

A. The Problem-Situation

1. A hermeneutics of the natural sciences – as an area of recognizably distinct cognitive interests – does not exist today. Writings explicitly addressed to such an undertaking are very rare, and then are usually of general, polemico-programmatic character, essentially restricted to a hermeneutically informed criticism of the "mainstream," analytic philosophy of science. Generally speaking, the situation today remains the same as in the only but outdated bibliography of hermeneutics

(Henrichs 1968). This bibliography contains hundreds of entries under the headings of historical, juridical, philological, hermeneutics, etc., but it has no section which deals with the hermeneutics of the natural sciences. Works somehow related to this latter topic appear in it only in connection with the old *Methodenstreit,* the dispute over the relationship between causal explanation and hermeneutical understanding.

2. Hermeneutics, of course, emerged as a philosophical discipline exactly in connection with this dispute, or more broadly: in the struggle of the human sciences for methodological and epistemological independence from the model of natural scientific inquiry. Modern, post-Heideggerian hermeneutics, however, has sharply attacked this restrictively methodological conception of its subject-matter, in the name of the universality of the hermeneutic approach. It has emphatically underlined that "understanding" should not be conceived as one of the possible cognitive relations between the subject and some specific objects of knowledge, but should be regarded as a basic mode of our finite-temporal existence encompassing the whole of our world-experience. It is just in respect to this claim of universality – especially in view of the earlier history of the discipline – that the silence of modern hermeneutics about the natural sciences acquires a somewhat strange character.

3. This impression is reinforced if one pays closer attention to what the initiators of a "hermeneutical turn" have in fact said about natural science as a cultural form or genre. I shall here take the example of Gadamer alone. On one hand, he unambiguously upholds the universality-claim of hermeneutics also in respect of the natural sciences themselves. These represent a form of *literature,* sharing with literary art works the fundamental characteristics of being inherently bound to language and therefore being able to be written down (*Sprachlichkeit* and *Schriftfähigkeit*), which makes the differences between them less basic than usually assumed. Gadamer (1975, 155–56) reinforces this latter point by pointing to the fact that important works of science may simultaneously also be outstanding examples of an artistic prose legitimately belonging to world literature – a remark which gives his considerations a somewhat dated character, since it is more applicable to the Galilean period than to recent works in the natural sciences. And indeed, when Gadamer explicitly deals with *modern* science, he seems to revoke the above characterization. He not only repeats Heidegger's famous (and for many infamous) dictum according to which, in the emphatic sense of the word, "science itself does not think," but adds that it "actually does not speak a proper language either" (1976, 10). He underlines the *monologic* character of scientific "sign-systems" which are, allegedly, completely determined by the realm of inquiry to which they refer (ibid., 11).[1] This would seem to deny the presence of some of the most fundamental features of linguisticity in the literary practice and works of the natural sciences: the constitution of the "matter" of talk in the very dialogue of "two speakers" and the associated world-openness of language. In short: Gadamer ultimately seems to

[1] Similar views were expressed also by H. Arendt (1958, 3), in the early writings of Habermas (e.g. 1971, 130–31), etc.

suggest that a hermeneutic approach to the natural sciences can legitimately discover their ineliminable dependence upon everyday language and communication, on the one hand, and their being in need of a higher, rational-philosophical "unification" (as an open-ended process) accounting for their role in the totality of human existence, on the other. Hermeneutics can then play an important *reintegrative* cultural role with respect to the natural sciences, but with little to say about the proper cultural-cognitive practice of autonomous scientific inquiry. This remains as the legitimate domain of an analytic philosophy of science which investigates the logic and epistemology of artificially constructed, secondary "sign-systems," the idealized "language" of the natural sciences.

4. This resigned (or at times hostile) attitude toward the natural sciences, which in a sense accepts their positivist image, is characteristic not only of Gadamer, but also of his predecessors such as Heidegger and of his critics such as Habermas (at least as far as it concerns his earlier writings). However, it is, today, opposed by several trends in the philosophy, historiography and sociology of natural science which developed a convincing critique of its predominant positivistic interpretation and which clamor (explicitly or implicitly) for a hermeneutic approach to scientific activity itself. It is quite conspicuous that the presently powerful criticism of the traditional "whig" history of the sciences (which constructed their past as a continuous series of contributions resulting in the contemporary state of the discipline) in many respects reproduces well-known hermeneutical arguments against a naive idea of progress which does not recognize the role and "creativity" of hermeneutic distance in history and in historical interpretation. Interestingly, even such historians of science – who are certainly not "revisionists" – as for instance, A. C. Crombie (1981, 279),[2] consider today the hermeneutical practice of the history of philosophy as the methodological example to be emulated in the historiography of science as well. Similar phenomena can also be observed in the sociology of the natural sciences where there is a definite shift (or at least broadening) of interests from the investigation of the informal social interaction between the scientists to the way the literary accounts of their activity are constructed.[3]

5. The few papers that directly address themselves to a hermeneutics of the natural sciences have no difficulty in demonstrating that several fundamental hermeneutical concepts and ideas can be fruitfully applied to the characterization of their proper cognitive activity. The role of a hermeneutic logic of *question and answer* in scientific inquiry has already been indicated by Popper, and has since led to the elaboration of some interrogative models of scientific activity. The presuppositional character of scientific knowledge, entailed by such varying conceptions as Polanyi's idea of a "tacit dimension," Kuhn's concept of the paradigm or Elkana's emphasis on the role of the "images of science," can be treated as a case (or specific cases) of those historically inherited "*prejudices*" (i.e., pre-judgements) which in hermeneutics

[2] In the same volume an identical point is made by Jardine (1981, 347).

[3] Gilbert 1976; Gusfield 1976; Latour and Fabri 1977; Woolgar 1980; Gilbert and Mulkay 1980; Mulkay 1981; Bazerman 1981; Knorr-Cetina 1981; Gilbert and Mulkay 1984.

constitute the precondition of any understanding. Similarly, the relationship between theory and observation can be analyzed in an enlightening way with the use of the idea of the *hermeneutic circle*. Metaphor's role in the emergence of new theories, the intimate relation between scientific production and reception shown in the history of science[4] – all these undoubtedly represent themes and problem-complexes in which investigations of natural science are in close contact with the ideas of hermeneutics.

6. Arguments of this type – which appear in the relatively few papers explicitly attempting to transpose some ideas of a hermeneutical philosophy to the study of the natural sciences[5] – have, in my view, the force and significance of successful *analogies*. They shed new light on an already established field of research by unexpectedly connecting it with an independently developed line of inquiry and its problematics. However, they also share the usual drawback of such analogic procedure: in the transfer process some of the original problem's or notion's most fundamental constituents are often lost. For example, K. O. Apel (1983, 186–87) has convincingly argued that when one regards the theory-observation nexus as a case of the "hermeneutic circle", one actually misses the whole problem-background, which this latter concept has been introduced to solve (the problem of the necessity of mediation between two meaning-intentions in incidents of communication over a cognitive distance). More importantly, however, there is, in my opinion, something contrived and artificial in all these attempts which simply transpose the readily-taken ideas of a general philosophical hermeneutics to the cultural field of natural scientific activities. The relationship between hermeneutics and natural science is not only strained from the hermeneutics viewpoint; it is equally problematic from the natural sciences' viewpoint. Bluntly put, the natural sciences, in practice, seem to be in *no need* of a hermeneutics – they succeed quite well without it.

7. This last assertion is intended to be a mere statement of fact. It attempts to focus on a situation which is perhaps best illustrated by comparing professional socialization in the humanities and in many of the "soft" sciences with that in the developed disciplines of natural science. A student of philosophy, history, and also sociology spends much time during his or her education on the actual acquisition of simple hermeneutical skills: he or she is emphatically and explicitly taught and trained to understand, interpret and use definite types of texts in definite ways. A student of physics, on the other hand, is not explicitly taught how to read the discipline's scriptures, although they can certainly appear to the layman as formidably difficult to understand. Whatever the student is taught – physical theories, mathematical techniques, the use of instruments and devices in laboratory situations and the appropriate interpretation of its results, etc. – through this learning process he or she is supposed to acquire the "language of physics." This language, once learnt, should

[4] On this latter point see some of the papers in Holton and Blanpied 1976; further Dolby 1971, 16–21; Shapin and Thackray 1974, Parts 2–3; Shapin 1974; Porter 1980.

[5] In the English language literature arguments to this effect can be found, e.g., in Healan 1972; Kisiel 1974, 1976 and 1978; Farr 1983.

make the texts of the discipline unambiguously and perspicuously comprehensible. Interestingly, this learning of physics will also involve a rather rigorous training in how to *write* texts of such kind. Thus, in the various branches of humanities there are a great variety of manuals teaching people how to read, while in the natural sciences there is a similar variety advising them how to write – but not vice versa. It is as though these two great branches of learning shared the opposed halves of the conviction of the Shakespearean Dogberry: *either* to write *or* to read "comes by nature."

8. Philosophers of science may convincingly destroy the idea of an ideally sharp and unambiguous language of physics; historians of science may discover that in all the great disputes in this field – from the reception of the Copernican theory to that of quantum mechanics – the adversaries not only regularly misunderstood each other, but these misunderstandings also played a constitutive role since they polemically influenced the way the concerned theories actually developed; "ethnomethodologists" of laboratory life (e.g. Latour and Woolgar 1979; or, in some respects, Knorr-Cetina 1981) can demonstrate that already simple "experimental reports" are underdetermined in their meaning and therefore, as a rule and without some additional conditions, cannot be replicated even by the expert reader – despite all these criticisms, the "hermeneutical naïveté" of the natural sciences persists, because it "works." That is, the "ideology" (*if* it is a mere ideology) of the natural sciences which regards any acceptable scientific text as totally self-sufficient as to its meaning (and therefore as unambigously clear to any reader with adequate competence) does succeed because the *hermeneutical* consequences of a so conceived practice seem to confirm this belief. From the viewpoint of its actually realized *hermeneutical achievements* natural science seems to be very "superior" to the hermeneutically very conscious humanities and "soft" social sciences.

Whatever one's view of the idea of a unilinear scientific progress, it is the modern natural sciences which indubitably provide at least the best approximation to what should be understood by the notion of an "accumulative historical growth" – the process of continuous *tradition-transmission* and simultaneously creative and accretive transformation of this tradition proceeds in a paradigmatic way in them. As a result, at any given historical moment, natural sciences are characterized – especially in contrast with the never ceasing "battle of sects" in humanities – by the existence of a widely shared *background consensus*. Due to this consensus, the frequently occurring disputes and disagreements at the frontier-areas of research usually prove to be "resolvable" relatively quickly (even if this truly involves a "decision," i.e. a fallible and always revocable resolution of the dispute). Lastly, whatever the frequency of the de facto occurring misunderstandings is (something which cannot be judged), it is at least true that the *argument from being misunderstood*, this perhaps most usual countermove in philosophical polemics (and in many other fields of the human sciences, too) does not belong to the "normal tone" of disputes in the contemporary natural sciences. The fear of possible miscomprehension, this *neurosis philosophicus* which, from Plato's seventh letter on, accompanies its whole history, seems to be

conspicuously absent from the public rhetoric of the natural sciences. Thus in respect of all these desiderata the modern natural sciences seem to represent a true Eden of hermeneutics: a state of fulfillment and perfection achieved without any effort. Therefore, any hermeneutical investigation of the natural sciences ought to first answer the question: why are its own cognitive interests and methods (or, at least, why do they seem to be), from the viewpoint of natural scientific practice, *unnecessary?* In answer to this question, however, it is insufficient to indicate or to demonstrate that some of philosophical hermeneutics' ideas and concepts are nevertheless applicable in some sense to the field of natural scientific inquiry as well.

9. Edenic happiness and innocence – as we know – has its own restrictions, and moreover, deprivations: there is some price to be paid for being able to dwell in Paradise. The clarification of this price is attempted in the following sections of this paper. Or, to put it less "poetically": I shall try to articulate – in a very schematic way – some constitutive features of contemporary natural science understood as a definite cultural practice and genre (or, in another terminology, an institutionalized discourse-type). These features at least partially explain both its hermeneutical "success" and "innocence." At the same time, I shall also point (even if only in broad hints) to some of those historical-cultural processes during which these characteristics were formed. (If the analysis stops at this, essentially "cultural," level, it is *not* because I would deny the interconnection between it and processes of deeper social transformation. Just the opposite. This interconnection cannot, however, be meaningfully discussed within the present paper's limits. But I should also add: this "culturologist" approach to science does indeed reflect my conviction not only in the usually conceded "relative autonomy" of cultural activities in modern society, but also in the existence of a specific, *sui generis system of relations* pertaining to the processes of cultural production, transmission, reception and innovation.)

10. The method employed in the following analysis is itself – at least in my own understanding – hermeneutical, but in a rather unusual and "revisionist" sense. In contradistinction and in opposition to the ontologizing approach of contemporary philosophical hermeneutics I would designate it as that of a historical hermeneutics of cultural institutions.[6] This latter approach focuses on the comparative analysis of the Author-Text-Reader (ATR) relationship constitutive to different cultural genres in different historical epochs. The terms of an ATR-relation are per se – no doubt – not specifically hermeneutical; they can be seen and treated, for example, as belonging to the conceptual field of a sociology of (literary) communication. Specifically hermeneutical is, however, the insistence on the following three points:

(a) The roles of the author and the reader are not solely determined by empirical – sociological and/or psychological – variables, but are co-determined by *normative* requirements posited through the genric characteristics of the specific text. Each text

[6] In this methodological respect I have borrowed and used – though in a generalized and rather transformed form – several ideas from the so-called "esthetics of reception," especially from the writings of Jauss (1970) and Warning (1975).

contains *inscribed* in it a definite authorial position and "voice," and simultaneously posits a definite reader-role and attitude (or attitudes) *prescribed* by it (as being adequate to it).

(b) The text acquires its "genric" character only through its articulated relation to other texts which appear in relation to it as its "tradition," into which it has to be – in culturally characteristic ways – inserted and which is (or can be) not only mobilized, but also partially reconstituted by the text itself.

(c) The historically conceived "production" of cultural (more narrowly literary) objectivations cannot be understood without the simultaneously on-going process of their reception which constitutes the specific *telos* of the first activities, and as an active process co-determines – in a dialogic interaction – their course.

It is the inscribed author, the posited (adequate) reader, and the text in the intertextual context of its tradition which constitute the main terms of the following schematic analysis.

B. The Inscribed Author of Natural Scientific Texts

11. Within our civilizational complex, culturally relevant texts are as a rule regarded as "authorial," i.e., they are ascribed to some particular individual (or to the collaboration of a few individuals) as his or her (or their) "creation." This cultural trait cannot be reduced to the mere fact that such texts (or more generally works) are actually the results of the intentional, relatively autonomous and non-habitual activity of some particular person(s). This may be so and a culture may nevertheless treat them as parts of an anonymous tradition. On the other hand, the compulsion to ascribe culturally significant objectivations to well-defined authors is so strong within our own culture that it can drive to a "discovery" of authorship for the anonymously inherited works of the past, even when it is realized that they were created under conditions making the applicability of such a concept highly problematical.[7]

The texts of the natural sciences are in the above, ascriptive- "proprietarian" sense strongly authorial. This is clearly demonstrated in the (presently usual) case of multiple authorship: there exist elaborate, highly formal conventions concerning "name ordering" to recognize each particular author's "assumed share" in the collaboration's literary outcome. Individual authorship in the above sense plays a pivotal role in modern science, since its social reward (and motivational) system is firmly anchored in this concept.

[7] This tendency is most conspicuously present in the practice of art history, with its strong interest in the questions of "attribution." Past works of art are often ascribed to individual artists though it is known that they were the products of a workshop with a strict division of labor, that their "program" might have been entirely due to their donors or patrons (whom the age concerned might have credited with the "making" of the work) and that they were created under cultural conditions which did not recognize our own distinction between an "original" and its "copies." The fact that attribution of authorship as a concern is especially predominant in the arts does not seem to be incidental since within our cultural ambit works of art are predominantly conceived and interpreted as expressions and self-realizations of a unique and exceptional individuality.

12. Despite this highly personalized concept of authorship (and its accompanying individualistic ideology), the author *inscribed* into the texts of contemporary natural sciences is (as a norm) a completely depersonalized one. The *depersonalization* of the inscribed authorial role is one of the fundamental traits characterizing these texts as constituting a separate and recognizable type of discourse.[8] In this respect the following points seem to be of relevance:

(a) Contemporary natural science (as a *cultural* genre) is characterized by the extreme *paucity* of its accepted *literary genres or forms* (whose diversity in general renders possible – among others – the expression of varying authorial attitudes and commitments to the communicated content in culturally codified ways). The "scientific paper" (unsharply divided into experimental and theoretical ones), the "comprehensive textbook" and the "theoretical monograph" are its main literary genres.[9] This can be supplemented by the observation that from the late nineteenth century on, the genre of "theoretical monograph" is increasingly in decline. Since the textbook's primary function is to fix the already achieved results in a field of inquiry in a comprehensive-systematic way, the "paper" remains as the nearly sole genre for the formulation (or at least public recording) of new scientific results and ideas (Kuhn 1970, 136–38; Eisenstein 1980, 461–62).

(b) The contemporary scientific paper (especially the experimental "research report") has – at least in most of the disciplines – a routinely standardized structure rigidly prescribed for the author and reflected in the well-known sequence of sections: Abstract – Introduction – Materials and Methods – Results – Discussion – References. I shall discuss the hermeneutical significance of such a structuring later (§39–40). At this point it should already be indicated that this organization has far-reaching consequences insofar as it implies a definite way the paper *ought to be understood.* The existence of the Abstract posits that it is possible to summarize its essential "content," i.e. that this latter is independent from the exposition's literary form and argumentative context. The distinction between Introduction and Discussion, on the one hand, and Methods and Results, on the other, implies the possibility to divorce "interpretation" from "description," while the division between Methods and Results indicates a similar possibility of separating the ways of investigation from its "findings."

(c) Research papers are characterized by a peculiar, idiosyncratic and highly conventional style; generally, they possess a distinct and shared "linguistic register,"[10] and the above-mentioned "training to write" essentially consists in the social-

[8] Cf. "The authors seem only to be contributing a filler for a defined slot, and they are only in competition with a few other authors who are trying to fill the same slot. The personal, though proud among colleagues, is humbled before nature" (Bazerman 1981, 365). This depersonalized character of the textual objectivations is all the more striking since the more evanescent, informal communications among scientists usually demonstrate a very strong emphasis on personalities and their clash.

[9] To this list one should perhaps also add such rather institutionally defined "genres" as the Ph.D. thesis and the "proceedings" of a symposium or conference.

[10] The concept "register" refers to those lexico-grammatical and text-organizing choices ("field," "tenor," and "mode of discourse") which are systematically realized by an item of language-use in dependence upon the character of the *social situation* in which it occurs. For the elaboration of this concept see Halliday 1978, 31–35, 63–68, etc.

ization to its active use. Especially in the last decade, sociologists (and to a lesser degree linguists) have paid considerable attention to this "literary rhetoric" of the natural sciences.[11] Since it is impossible to deal in detail with their respective analyses, I shall merely refer to those, mostly descriptive, characteristics which they share with each other. It has been indicated that the "language" of the experimental paper is, firstly, highly *decontextualized:* in its main body the specific experimental actions situationally contingent upon the laboratory's local conditions are expressed in terms of codified, laconic, general formulae chosen from a restricted vocabulary. A further sign of this decontextualization is the rarity (in comparison with other types of texts) of "essentially indexical expressions" in these writings. More particularly — and in direct connection with the depersonalized authorial role – among all the pronominal deiktica (through which different subject-positions in relation to what is conveyed in the text can be expressed) only the use of the undifferentiated "we" is allowed. Furthermore, natural scientific texts prefer the employment of a passive voice through which the *actions* of the experimenter (the "real author"), intentionally undertaken through the exercise of practical choices in the laboratory, become transformed into a sequence of *events* following upon each other. Lastly, these texts not only exclude any explicit value-judgement, but also do not use emotionally or normatively tinged, evocative expressions, with which personal authorial attitudes can be suggested.

Due to all these indicated characteristics the "inscribed author" of the natural scientific texts appears as an anonymous performer of methodologically certified, strictly regulated activities and a detached observer of their results – without any further personal identifying marks beyond possession of the required professional competence. Through this depersonalization of the author the experimental paper acquires its fundamental cultural trait of *report.*

13. The depersonalized authorial role represents, of course, a "genric" requirement; it is not a fact, but acts as a *norm* (and has normative consequences). That is:

(a) The independence of the experimental report from its author's personality is to a large extent *fictitious* in the sense that no two scientists performing the same experiment (according to the accepted criteria of "sameness," since literal replication of experiments is in principle impossible) will write it up in an identical way. What is more, the differences between the various "expositions" will reflect not only inessential personal idiosyncrasies, but can have far-reaching cognitive effects. As a rule experimental data (depending on the theoretical context they are inserted into) allow one to draw a number of different interpretive conclusions, which can be formulated again with varying "cognitive force," from the sceptically conditional to the dogmatically assertive. There are therefore – in spite of the impersonal, purely "registrative" tone of the scientific paper – always personal choices to be effected, e.g., between the strategies of maximization versus minimization of the possible knowledge claims.

[11] See Gopnik 1972; Hofstadter 1955, and the writings referred to in footnote 3.

(b) It is, however, characteristic – and already belongs to the normative effects of the indicated "author-role" – that the minimization strategy of the involved knowledge-claim (i.e., carefully taking into account all the possible objections, presenting the interpreted data in an appropriately sceptical manner, etc.) is considered to be the properly scientific one. This is not only expressed in the positive evaluation of the cautious, sceptical attitude as part of the scientific ethos, but more importantly, in case of such a minimization strategy, an experiment whose results the scientific community ultimately refuses to accept, is often not counted as the result of the author's mistake or error . It is usually regarded as a piece of "bad luck," the result of some "freaky incident" that neither could be foreseen, nor explained with the present state of knowledge, and can "happen" to any experimenter.[12] In this sense the depersonalized authorial role goes together – under appropriate circumstances – with a diminished *authorial responsibility* (in the cognitive sense) for the text published. This naturally means a "reward" for reducing the knowledge-claim contained in the paper – a strategy hardly advantageous from the viewpoint of scientific *progress*. But this tendency is counterbalanced by another normative requirement towards scientific objectivations: they must represent a *new* contribution to the existing body of knowledge. Since novelty of results is both a *constitutive* criterion for any work to be admitted into science and an evaluative criterion of its significance, from the viewpoint of this requirement strategies of maximalization of cognitive claims are to be preferred. Because of the simultaneous validity of both norms, which can produce clashing preferences, each scientist must find in every case his or her personal compromise between "scepticism" and "dogmatic" commitment.

(c) If the depersonalization of the inscribed author somewhat diminishes the responsibility of the real one for the text written by her or him, this desubjectivization also results in the reduction of her or his *authority and control* over its *meaning*. Earlier (§8) I referred to the fact that – in comparison with the humanities — charges of being wilfully or inadvertently misinterpreted occur relatively rarely in disputes within the natural sciences (insofar as the *texts* are concerned, since such charges occur quite frequently in informal communications). This, however, has now to be supplemented by the observation that another – and stranger – kind of misunderstanding is often suggested in the latter controversies: The *author* is often charged (even if not necessarily in so many words) with having misunderstood what she or he has "described." The meaning of what is reported in the main sections of the research paper is posited as beyond the author's control, belonging to an impersonal and interpersonal realm. In this sense scientific papers are truly treated in this cultural-hermeneutical practice as imperfect fragments from an infinite "Book of Nature." This perhaps also explains the enormous staying-power of this metaphor which, originally introduced in the Augustinian tradition for the articulation of "sympathic" understanding of nature as divinely created meaning-connection, has retained its

[12] For a case study well illustrating this point, cf. Harvey 1980, esp. 149–51; further Knorr-Cetina 1981, 102, 124–26; Gilbert and Mulkay 1984, Chap. 4, etc.

force even after its whole onto-theological background has first been radically transformed, and then completely lost.[13]

14. Depersonalization and desubjectivization of the authorial role brings natural scientific texts into an unexpected parallel with some works of modernist literature which consciously and programmatically aim at the elimination of the personal authorial voice (the "oeuvre pure" precisely characterized by the "disparition élocutoire du poète," in the words of Mallarmé). Just because such a comparison seems to be (and I hasten to add: essentially is) quite absurd, it is worthwhile to follow it through.

The programmatic elimination of the subjective authorial voice from "pure poetry" (or that of the narrator from "Nouveau roman") aims at making these texts completely *self referential.* That is, such a text normatively insists on being received "for its own sake": it foregrounds the language actualized in it as its *material,* instead of this language's being used as a mere *means* of communication (about something, real or fictitious). This is achieved (insofar as it is achievable at all) through a conscious and systematic destruction of the identity and unity of those directly referential relations which are spontaneously evoked by any use of the language (Warning 1983, 198–200; Zons 1983, 122–27; Riffaterre 1983, 221–39).

In all the relevant respects natural scientific texts demonstrate directly opposed characteristics. Their restricted vocabulary, pedestrianly straight syntax, the ban on the use of rhetorical and poetic figures and *topoi,* all make the language used (for the competent "speakers") unobtrusively transparent, render the text's linguistic constitution completely opaque. They fix language normatively in the role of a mere instrument of communication. The exclusion of any expression of an authorial attitude (at least from the main body of the paper) is directed again at the homogenization of its referential functions, but it homogenizes and emphasizes precisely the function of *direct (object) reference.*

These two types of equally "depersonalized" texts occupy therefore just the opposed poles in the wide spectrum of the culturally codified text-uses historically available to us. The texts, from which allegedly "language itself speaks," and those in which allegedly "the facts speak for themselves," are the extremes in that variety of hermeneutical positions from which our culture allows (or renders it possible for) *us* to speak within – and partially about – that world in which we find ourselves as a contingent fact.

15. To my knowledge, there are no historical investigations which systematically concentrate on the changes in the fundamental "genric"-literary characteristics of "scientific" texts. Nevertheless, elementary historical considerations do suggest that

[13] The latest resurfacing of this metaphor is, of course, in our talk about the "biological code," amino acid analysis as "deciphering," etc. It is interesting also to observe that this *topos* often reoccurs even with those thinkers who are theoretically completely opposed to the understanding of knowledge on the analogy with the "correct reading" of something already meaningfully articulated. So, e.g., Marx, in his first attempt to formulate a radically historicist understanding of knowledge as a specific type of production, simply transferred the metaphor of the "Book of Nature" to industry understood as the "*open* book of *essential human powers*" (Marx [1844] 1968, 543). About the history and the role of his metaphor in general, see Curtius 1948, Chap. 16; Nelson 1975; Rothacker 1979; Eisenstein 1980, Chap. 5 and Blumenberg 1981.

the depersonalized authorial voice and position does not characterize "natural science" as such, if this term is taken in its commonly accepted historical compass and meaning. A simple recall of the "genric"-textual features described earlier (§12) makes it clear that such a hermeneutical trait can only be attributed to natural sciences in relatively recent times. From the High Renaissance to the end of the eighteenth century there was a great variety of relatively well distinguished literary genres among which writer-scientists could choose – according to circumstances, authorial intentions and attitudes, etc.[14] The conventions within each of these literary forms were much less rigidly fixed than they are today. Furthermore, a well-discernible authorial voice is directly present in many of the important natural philosophy and natural history works of the seventeenth and eighteenth centuries, not the least in those of their sections which (as it is often the case) touch upon metaphysico-theological or methodological issues. Experimental reports even well into the nineteenth century often seem to demonstrate a strong "narrative" organization, with an appropriate narrator role for the author. In general, it would seem that the depersonalized authorial role, in the sense characterized above, does not emerge fully before late nineteenth century.

16. The lack of closer historical investigations concerning the changes in the literary forms of natural science can be, however, to some degree counterbalanced in an oblique manner: by recalling, in a cursory way, that better-known process through which the natural sciences have been separated from the arts. This historical separation is relevant to the emergence of the depersonalized authorial role of the writer-scientist in that within our cultural tradition, works of art are predominantly interpreted – in spite of the already mentioned modernist countertendency – as expression of an irreproducible, exceptional individuality, i.e., they are usually related to an irreducibly personalized authorial figure and role.

Therefore it is not without interest that, at the beginning of the long process of their cultural autonomization, arts and natural science appeared in close unity, and just because both were equally regarded as expressions of an individual-personal creativity. The *virtuoso* – as the man of *virtù* – of the Renaissance designated both the artist and the scholar-"scientist," and in such cases as Brunelleschi or Leonardo it is certainly impossible to draw any strict line between artistic, technical, and scientific concerns. Leonardo emphatically characterized painting as science, and opposed it to poetry on the basis that the latter has to do with moral philosophy, while the former has to do with natural philosophy (Leonardo da Vinci 1980, 200). It is usually maintained that such a "hybridization" of architectural and visual arts, on the one hand, and the "sciences" of nature, on the other, ends with the fifteenth century: "By the middle of sixteenth century," writes Ben-David, "the relationship between science and art reverted to the earlier pattern of two endeavors running widely separate courses and having few meaningful encounters" (Ben-David 1965, 29).[15]

[14] So Olschki (1922, 219–300) could fill up almost a hundred pages with the discussion of the various genres of sixteenth-century scientific literature in Italy.

[15] Similar views are expressed also by de Santillana 1959, and Ackerman 1961.

This is, however, a rather simplified picture since the process of their complete divorce was much more protracted. Insofar as those "minor arts" are concerned in which technical innovations played a significant part (like turning, medallion-making, engraving, etc.), even the unification of the roles of artist and scientist in one person has survived into the eighteenth century,[16] all the more easily since their practitioners were often the makers of the "philosophical instruments." Even in such major fields of artistic endeavor as painting, the interaction between it and some branches of natural philosophy (primarily optics) remained relatively close and direct well into the eighteenth century. This contact was both of practical (e.g. the employment of up-to-date optical devices by painters like Vermeer, Fabritius or Hoogstraaten) and ideological character (as the largely spurious use of Newtonian optics in painterly manuals), and it allowed landscape artists to continue regarding themselves during this period as something of experimenters in natural philosophy (see Gage 1983). Even in the nineteenth century Ruskin could still meaningfully advise painters to first learn to see nature as she is *from science* (characteristic of that time, the science referred to was geology).

17. From the viewpoint of our topic, however, the question of the divorce of the natural sciences from the *literary arts* is of greater importance. This problem goes beyond the effects of the development of natural science on literature, and the influence of the latter upon the reception of scientific theories,[17] and has been explicitly discussed in an interesting paper by W. Lepenies. His main conclusion: "up until the eighteenth century it is a senseless enterprise to divorce science and literature" (1979, 137) is, if taken literally, undoubtedly overstated. Certainly no contemporary reader would miss the point that the works of – let us say – Marivaux and Maupertuis belong to quite different cultural genres. He is, however, completely correct in emphasizing that until the first half of the eighteenth century the appropriateness of applying definite esthetic-rhetorical requirements and criteria to writings in natural philosophy and history was taken as self-evident. To my knowledge, it is a Mémoire presented to the Académie Royale des Inscriptions et Belles-Lettres in 1740 by De La Nauze which first explicitly raised the problem about the relationship between science and the *belles-lettres* (to protest against attempts at their separation) (see Ricken 1978, 39). Only in the second half of the century were voices raised with growing frequency (e.g., in the discussions of, and disputes about, Buffon's work) stressing the potential conflict between the demands of scientific objectivity and exactness, on the one hand, and those of stylistic "beauty," on the other. However, as long as *both* literature and science are primarily conceived as forces of intellectual and moral cultivation, i.e., are comprehended in their relation to the individual, and not as objectivations, no strict distinction is made between the two. It is therefore not accidental that a clear distinction between the sciences and the arts is first theoretically drawn by Kant.[18] The actual process in which the natural sciences shed off their

[16] For an interesting example, see Gouk 1983.

[17] As discussed in respect to Newtonianism and English poetry by Nicolson 1946, and Bush 1950.

[18] "There is no science of the beautiful, but only a critique. Nor, again, is there an elegant science

literate-rhetorical character (and the intertwined personal-narrator role of the author) proceeded at a different pace in different national-cultural environments – in France, e.g., it certainly took longer than it did in Germany. By the end of the nineteenth century the scientist's depersonalized authorial role is, however, so well established and self-evident that Flaubert can characterize his own artistic program which aims at the impersonality of narration as that of the "scientization" of literature.[19]

18. It is important to underline that the literate-rhetorical character of the early forms of "natural knowledge" did not simply mean the presence and effectivity of some *external* (and in our understanding: foreign) requirements merely concerning the character of the "exposition" in scientific literature. The pleasing and engaging character of writings in natural philosophy and natural history (as a better or worse realized cultural norm) has been intimately connected with their cognitive structure, cultural function and ways of social institutionalization. Firstly, there is – as it has been pointed out by Bachelard (1938, Chap. 2; see also Schaffer 1983; Schaffer 1980, 72–86) – a strong interconnection between the conversational-rhetorical style of the works in early natural scientific literature and the concentration of "experimental natural philosophy" upon the demonstration and explanation of the *dramatic* and *marvellous* powers of nature, with the associated focusing of experimental activity upon the publicly displayable and spectacular. This had important cognitive consequences. The variety of such qualitative experiments, usually performed with non-standardized instruments and apparatuses on non-standardized materials and reported with a belletristic *ductus,* lacked consensually acceptable criteria of replicability:[20] in general such experiments could *stimulate* theory construction, but were unable to serve as *systematic control* (falsifactory) instances between competing

(*schöne Wissenschaft*), but only a fine art (*schöne Kunst*). For a science of the beautiful would have to determine scientifically, i.e. by means of proofs, whether a thing was to be considered beautiful or not, and the judgement upon beauty, consequently, would, if belonging to science, fail to be a judgement of taste. As for a beautiful science – a science which, as such, is to be beautiful, is a nonentity. For if, treating it as a science, we were to ask for reasons and proofs, we would be put off with elegant phrases (*bons mots*)" (Kant 1790, §44, trans. by J. C. Meredith).

[19] This development did not take place, even in its semantic aspect, without resistance. So Ruskin in 1874 still wrote: "It has become the permitted fashion among modern mathematicians, chemists, and apothecaries to call themselves "scientific men," as opposed to theologians, poets, and artists. They know their sphere to be a separate one: but their ridiculous notion of its being a peculiarly scientific one ought not to be allowed in our Universities. There is a science of Morals, a science of History, a science of Grammar, a science of Music, and a science of Painting; and all these are quite beyond comparison higher fields for human intellect, and require accuracies of intenser observation, than either chemistry, electricity, or geology." (*Ariadne Florentine*, quoted by Ross 1962, 70).

[20] The *norm* of replicability – as it is clearly reflected, e.g., in the principles of Royal Society in the formulation of Sprat – was already well recognized. Under the indicated conditions, however, failure to reproduce some reported experimental result always could be simply interpreted as the lack of "art" on the side of the second experimenter. (And this was not irrational. All the four scientists, to whom Kleist originally communicated his discovery of the Leyden jar, were unable to repeat his experiment.) On the other hand, the original experiment could be discarded with an equal ease, if not by questioning the probity of the experimenter (and implying the suggestibility of his audience), then through the indication of some quite *ad hoc*, vaguely stated and uncontrollable qualitative factors (as "complicating causes") invalidating its results. It is characteristic in general that during this period the problem of replicability was articulated as a question about the adequate "policing" of science and the struggle against charlatanism, i.e. it was conceived in terms of control over individual morality.

theories. On the other hand, this concentration on the direct manifestation of hidden and marvellous natural powers was connected with a definite ontological conception of nature in general (understanding of nature in terms of pervasive, hidden and qualitatively different forces either immanent to matter or impressed upon it by God, etc.), and, simultaneously, with a definite understanding of the cultural role of science as a morally (and often also religiously) uplifting and edifying force. That again involved a particular image of the potential audience for science which, in its turn, was not independent from the prevailing forms of its actual institutionalization, in particular from the way social support for scientific activities has been solicited and secured. So the emergence of the depersonalized authorial role of the scientist was part and parcel of that transformation in which – mostly during the nineteenth century – the whole character of natural science as an institutionalized form of cultural practice has been radically changed.

C. The Intended Reader

19. As the above considerations already indicate the authorial role "inscribed" in the texts of the natural sciences is not independent from the *reader/addressee prescribed and implied* by these texts as their adequate (i.e., able to understand, judge, discuss, criticize, etc., them) recipient. As is the case with all *sensu stricto* cultural activities, a normatively defined "adequate audience" represents (at each historical moment) a constitutive element of the literary practice of science, the objectivations of which have a cultural significance only if they are comprehended/interpreted/used in some, well-defined way(s). It is only in the process of an "adequate" reception that the historically actual meaning and cultural significance of any text – including scientific ones – is established and consummated.[21] A culturally posited "public of science" therefore belongs not simply to its social context "influencing," as it were externally, the direction of natural scientific enquiry, but constitutes an imminent characteristic of it as objectifying activity.

The so-conceived "intended" (adequate/competent) reader of contemporary scientific literature is – and solely – the *expert professional*, working in the same *research area* to which the work in question pertains. True, this research area – and thereby also the circle of recognized addressees – is only defined in a diffuse way. Basically it

[21] This becomes clear if one considers, e.g., the requirement of *novelty* of results as a necessary precondition for any work to be accepted as contribution to science. "Scientific novelty," however, cannot be characterized in terms of that individual process of production (of its "originality" or "creativity") which results in the work in question. The novelty of this latter is constituted by its relation to the contemporary literature of its subject; a work of science may be the outcome of highly original research, and nevertheless "duplicate" some result which just has been published by someone else, and in such a case it will not be recognized as a relevant contribution to science at all. Since usually there are no unambiguous criteria of "sameness" in science (hence such concepts as that of "semi-duplication," often to be met in highly competitive fields), it is only in the *process of reception* that novelty (as a seemingly inherent feature of the work) becomes, often through negotiations and disputes, established at all. Understandably, with the change of the literature, some works may appear in *retrospect* as containing unsuspected, at first not appreciated "novelties." On this latter point, see e.g., Holton 1978, 43–44.

is pre-given to the author by the existing institutional structure of scientific specialization (with its finer subdivisions into recognized areas of specific concern and competence). However, it can be partially projectively redefined by the paper itself. In principle, however, the audience of natural scientific discourse is restricted to those who can equally participate in its continuation. This *social closure* of the discourse upon itself: the *specialization and professionalization of its intended/implied public* – as interconnected, but analytically quite distinct phenomena from both the specialization and the professionalization of the scientists as writer/"producers" – constitute again a specific feature of contemporary natural science as a cultural genre.

20. One can immediately object to this formulation by pointing to the fact that "professionalization of the audience" in the above sense is not specific to the natural sciences alone; under contemporary conditions it characterizes all forms and types of *scholarly* endeavors. Though this remark, especially in an English-speaking cultural milieu, sounds almost self-evident, and although it undoubtedly legitimately indicates an observable historical *tendency,* it cannot be accepted as correct.

It is certainly the case that the distinction between works of scholarship and popularization (with their quite distinct evaluative criteria) is today equally present in the natural and in the "soft," social sciences, and even in the broadly conceived humanities. Further, it must be conceded that perhaps ninety-nine percent of the scholarly works recently published, let us say, in philosophy, are intended for, and are actually read by, "professionals" (including students as aspiring professionals). It is, however, the remaining one percent which is of interest. Because this consists not only of works of indubitable "scholarly" significance, but is composed, as a rule, of such writings that the "profession" itself regards as the most important contributions to the present state of learning. One needs only to compare international publication and circulation data concerning (in respect of a longer time-span) e.g., the scholarly writings of Einstein and Dirac, on the one hand, and Wittgenstein, Heidegger or Quine, on the other, and the difference becomes immediately clear. At the same time this phenomenon is not restricted to philosophy alone. The same result will emerge if one replaces the above-mentioned philosophers with anthropologists like Malinowski, Lévi-Strauss or Geertz, or with sociologists like Durkheim, Weber, and even Merton or Lazarsfeld.

Even today the most important and influential scholarly works in humanities and social sciences regularly find an audience wider than the one comprised of the "professional experts" in the field. This public is constituted partly by scholars in *other* disciplines and specialities, partly by the elusive "cultivated reader" – and it seems to be growing rather than diminishing. Members of this audience certainly are not regarded as competent to partake on an equal basis in discussions among professionals about the works concerned, but their attitudes, evaluations and opinions do in various ways influence these discussions. They are regarded as legitimate recipients of the works in question, only of an "inferior" type (see §24).

There is no similar phenomenon for the literature of developed natural scientific disciplines.

21. The simplest and most usual explanation of this difference refers to the varying degrees of difficulty, or "unintelligibility," the two kinds of texts represent for the non-specialist reader. Natural sciences – it is often argued[22] – operate with a mode of discourse autonomous, or at least far removed and differentiated, from everyday language, just as their problems also have little to do with everyday concerns. On the other hand, humanities and social sciences, even if they do employ some specific terminology or vocabulary, are deeply dependent upon natural language and everyday interests. This may be regarded as a sign of their theoretical underdevelopment or just as a constitutive trait connected with the specific character of their cognitive interests; in any case it is seen as sufficient explanation for their easier accessibility to the layman or the non-specialist.

While this posited difference regarding everyday language may well be, in some general way, true, I doubt that it adequately explains the different constitution of audiences for the cultural genres in question. Firstly, it is not clear at all that such texts like the *Tractatus* or *Sein und Zeit* (texts undoubtedly read today by many non-philosophers, too) are in any meaningful sense more easily accessible to an uneducated layman than writings in theoretical physics or biology. It would seem that even a very elemental understanding of *both* types of texts demands a considerable educational (or self-educational) effort; that there is some significant difference in its intensity or prolongation in the two relevant cases, would need to be proven (and it seems doubtful). Secondly, even if this was the case, such considerations cannot explain why the adequate reader in the natural sciences is posited as the specialist expert in the given *area of research,* since the difficulties in question cannot be present – or at least cannot constitute a serious obstacle – for scientists within the same discipline or specialty, working, however, in unrelated research areas.

22. The answer to this last question does have something to do with the relation between everyday language and the discourse of the natural sciences, but in quite another sense than the one suggested above. Natural scientific activities involve (in our culture) not only argumentative-discursive, but also experimental-manipulative practices. Therefore new knowledge is fixed and accumulated in this field not merely in the form of textual objectivations, but also through *incorporation* into those laboratory activities which have the character of craft skills and can only be learned through example and controlled performances in the relevant situations. More particularly, the very meaning of the *sui generis* "observational" terms of experimental natural science is undivorceably interconnected with this particular (usually instrumental) action-context and action-orientation. Regarding this embeddedness of some of its basic concepts in the pragmatic contexts of manipulative activities, the discourse of natural science is rather *similar* to everyday discourse (with the important *proviso* that laboratory actions, in opposition to everyday activities, are as a rule

[22] In the relatively recent literature such argumentation occurs – with widely differing evaluative accents – in Lammers 1974; Knorr 1975, 232–35; Bourdieu 1975, 34–36; etc.

constructed as socially and morally neutral, as *eo ipso* technical activities). While the natural sciences certainly have no autonomous (from everyday talk) "language" of their own, their discourse does possess – in view of the intimate- intrinsic interconnection between practical situation, manipulative action and linguistic-conceptual articulation – the character of a *sui generis* (even if "derivative") *language game*, in counterdistinction to the humanities and social sciences which essentially represent *metadiscourses* divorced from *direct* connection with practical-manipulative activities.

As a result an adequate understanding of natural scientific texts cannot be learned/acquired in an intercourse with these *texts alone*. To adequately comprehend a research report – to understand what the experimenter has done and why, whether therefore the experiment is at all, in principle, reliable, i.e., whether it can have any claim to be scientifically relevant – presupposes an ability to translate the abstractly, formulaically indicated "methods" into concrete actions envisaged in the described laboratory situation, so that their "fitness" to the problem concerned, etc., could be judged. Understanding, therefore, presupposes some degree of shared craft skills and practical know-how: a *"tacit" knowledge* which is in fact present only among the members of a restricted circle of specialists working in the same (or closely related) research area(s) (see Polanyi 1964, 49–63; Healan 1972; Collins 1974; Gilbert and Mulkay 1980, 282–93).

23. There are, therefore, some good reasons to regard contemporary natural scientific texts (or at least some important class of them) as ones with an intelligibility inherently limited to the small circle of professional experts. All arguments, however, which would explain the restriction of the adequate audience with similar considerations of *factual* nature, are insufficient. The cultural construction of the relevant reading public definitely figures in the natural sciences as a *normative injuction*, and cannot, therefore, be represented as the mere consequence of some inescapable facts. The boundary limiting and enclosing natural scientific discourse is not pre-given, but actively *maintained*.[23] The layman and the non-specialist are posited in the natural sciences as ones whose interpretation of, and opinion about, the works of science *ought* not intrude into the relevant discussions at all. Their views are culturally fixed as being in principle irrational, or at least irrelevant. This is rather directly reflected in that (institutionally strongly enforced) norm which forbids the researcher to appeal in any way to an external public before his or her results have been accepted and "certified" by the competent professional community concerned. Proper scientific publication is in this way construed as the opposite to "seeking publicity." Deviance from this norm involves, as a rule, strong sanctions; it is often seen as legitimating a violent professional reaction which itself may seriously impinge on the usual standards of fairness and objectivity (see Barnes 1972, 283–87; Bourdieu 1975, 23 and 42; Whitley 1977, 146–48; Dolby 1982). There is no such insitutionalized norm in force in other areas of learning.

[23] For the same reason, Kuhn's account for the social closure of the discourse in the natural sciences, in terms of the socio-psychological characteristics of the scientific community (its educational homogeneity, relatively high degree of social isolation, etc.), also seems unsatisfactory to me.

24. Thus professionalization of the audience for natural science is a *normative* cultural construct, and not a simple fact. This can be demonstrated also from the other way round. It must be assumed that works of this cultural genre are even today regularly read by some non-specialist outsiders and that *in fact* this reading does influence the on-going practice of the relevant disciplines. Firstly, it would seem that some scientific publications do have an interest to scientists *outside* the given speciality, or even discipline, since there is no field of research that does not employ techniques, results and theories originating in unrelated areas. Secondly, some scientific writings (projects, reports, etc.) should be read and evaluated by those institution members who decide upon the support of various research projects, upon the selection of scientific personnel, and the distribution of economic and social resources necessary for the maintenance of scientific activities – and they are mostly not fellow-specialists.

Contemporary natural scientific practice does therefore presuppose the existence of some readers who are not "expert-professionals" in the indicated sense. But its hermeneutical constitution is characterized just by the fact that these – potential or actual – readers are not posited as *sensu stricto recipients* of the concerned texts (even with a reduced competence), but are treated as *clients-users* of the results fixed in, or the information provided by, them. They are recognized as competent to judge the instrumental significance of some result from an "external" viewpoint, but not the intrinsic value and meaning; they should accept the latter as authoritatively established by the relevant research community.

Perhaps the following, no doubt anecdotal, consideration may to some degree illuminate how this dichotomy of the adequate recipient *versus* client-user differs from the presupposition of a *multiplicity of recipient-types* which is – despite all tendencies of professionalization – still culturally accepted in the humanities.[24] Both mathematicians and philosophers (but I could have chosen other examples) are inclined to complain, at least among themselves, about the inability of physicists or sociologists, respectively, to get any mathematical or philosophical, respectively, idea straight. Mathematicians, however, will not be apt to criticize publicly the physicist's "misunderstanding" of mathematics (as clearly distinct from making technical errors in the employed mathematical procedures), the latter are just not supposed to get it right. Philosophers, however, do criticize sociologists for such interpretative sins; as an example I can refer to some rather vitriolic comments in the recent philosophical literature directed at a number of sociologists of science for their alleged misrepresentation of the views of Wittgenstein and Kuhn.[25] At the same time philosophers sometimes make direct use of ideas and viewpoints developed by sociologists in the context of such "philosophical excursions." They do recognize the

[24] This multiplicity of the implied recipient-types is, of course, even more pronounced and more clearly recognized in the arts, where it is a commonplace to distinguish between the receptive position and attitude of the fellow-artist, the critic, the connoisseur and the "naive" reader (or viewer), with important ideological battles going on concerning their relative significance.

[25] See e.g. the editorial introduction to Gutting 1980, 9–11.

latter as recipients and interpreters of philosophical thought and texts, even if of a suspect and certainly inferior type.

The multiplicity of recipient-types recognized in humanities provides them even today with a *multifunctional* cultural role. The strict, normatively posited "professionalization" of the intended, implied reader of the natural scientific literature is synonymous with its cultural *monofunctionality* in view of which all the "non-professional" use of its results and resources is reduced to the case of an external, *technico-instrumental application*.

25. In view of the fact that the demarcation of the research area, to which some scientific publication "belongs," is – as a rule – fluid and diffuse, the distinction between the adequate reader and the mere user of natural scientific texts also has a similar character. In a great many cases it is not pre-given at all, but becomes established in a complex process beginning with the definition of the genuine problematic of the research and ending with conferring the label of being competent/incompetent, relevant/irrelevant upon the various standpoints and criticisms. In this process usually *both* argumentation and social negotiations play role (see Callon 1980; Knorr-Cetina 1981, Chap. 4; Collins 1981). The line between considerations that are "internal" and "external" with respect to some scientific investigation is established during a social interaction in which not only scientists, but also some of their "clients" may in fact participate. It belongs, however, to the characteristics of contemporary natural science as an institutionalized cultural practice that regarding its objectivations and "results," such a line – somewhere and somehow – *ought to be drawn*.

26. The most important hermeneutical consequence of this professionalization of the audience consists, however, in the fact that – in conjunction with the earlier characterized depersonalization of the authorial role – it normatively posits the *complete interchangeability* of the author and the recipient. The (inscribed) author appears as only one member of that research community which is the adequate addressee of his or her paper and simultaneously the bearer of that "we," in the name of whom the text is usually formulated. Each participant in this "community" has in principle equal competence and right not only to judge the veracity of what is reported, but also the *meaning* (correct interpretation) of what is so *described*.

While this equalization of the author's and implied reader's interpretative authority is certainly a counterfactual postulate embedded in textual characteristics, it is – in contemporary natural sciences – *not completely* fictive. The now usual circulation of drafts and preprints, and the function of the "reviewers" can involve a significant part of a paper's intended audience in its formulation process since their reactions, commments and criticisms may seriously influence its final-"public" form.[26] In this sense modern natural sciences come closer than any other type of cultural practice to the direct realization of the Romantic hermeneutical postulate concerning the co-creative role of the recipient – certainly with rather unromantic consequences.

[26] This point is made by Knorr-Cetina 1981, 104–6 and 125–26.

27. The significance of this postulate of interchangeability of the authorial and reader roles comes sharply into focus when seen through the prism of some modern theories of fictionality. Rainer Warning (1983, 191–98)[27] especially has underlined the strong connection between the cultural recognition of the "fictitious" character of literary works of art, on the one hand, and the appearance in the relevant texts of an authorial (or narratorial) voice whose identity with the real person of their creator is at least problematic, on the other hand. This "dedoublement" leads to a split between the "internal" communicative situation articulated *within* the text and the "external" situation of its *actual reception,* and so it creates a pragmatic *double-bind* for the reader. As a result, texts of such type, on the one hand, force, or at least stimulate, the reader to take various positions in relation to their "message" in a play of imagination; on the other hand, they themselves thereby acquire the character of *res ficta sive fabula,* of a mere "tale." Something told by someone whose identity, vantage point, etc., *in principle* cannnot be established in an unambiguous way and which therefore *eo ipso* cannot (and *should* not) be verified.

If fictional texts in this way systematically exclude the interchangeability of the dialogic roles which is an overall pragmatic trait of everyday communicative exchanges, texts of the natural sciences fix this interchangeability as a feature of their *very textual constitution.*[28] What they tell is posited as something which could (and, under the observance of elementary rules of veracity, also should) be told by everyone who possesses the necessary (and in principle universally accessible) competences. A claim to *strict intersubjectivity* and *objectivity* pertains to the way the cultural objectivations of natural sciences are constituted in their contemporary practice.

28. In the case of "research reports" this claim of strict intersubjectivity takes on the form of the well-known postulate of replicability of the experimental results. This postulate has a paradoxical character (see Collins 1975, 1981; Harvey 1980; Pickering 1981). Those features of the scientific texts which *allow* such a claim to be made also *exclude* its fulfillment in any "literal" or ordinary sense. On the one hand the extremely stylized and typified character of the description of the procedures used as "methods," renders the information contained in the paper insufficiently specific for any veritable replication. On the other hand, only such a description makes the claim to replicability possible at all. This is not only so in the sense that in all their individual details, no experimental conditions and actions were practically reproducible (or even describable). More important, the text's mere *focusing* on the particular-local, non-recurrent aspects of the laboratory events would immediately situate its author in the position of a privileged, exceptionally placed observer whose role in principle cannot be taken up by just "anyone." Precisely here lies the

[27] See also Stanzel 1982, esp. Chaps. 4–5.

[28] "... (T)he author does not claim a specific vantage point, or viewpoint as compared to his audience.... The audience knows as much and as little as the author. They are on equal plane.... The mode of writing reduces distance and avoids claims of authority or superior judgement on the part of the author." (Gusfield 1976, 21).

difference between a "scientific report" of an experiment and a belletristic or journalistic "reportage" about the goings and doings in a laboratory.

From this viewpoint the usual "Methods" section of a research paper should be seen as a projective claim which specifies those formulaically indicated conditions under which all competent persons (with the necessary "tacit" knowledge and experimental know-how) should reach results, at the given level of knowledge counting as essentially identical with the ones described. When there is a disagreement about the results of an experiment (mostly in the form of a dispute of what should be considered as its competent replication), then it is the above claim's justified nature which is usually questioned. In the cultural practice of the contemporary natural sciences such disputes are, *as a rule,* consensually solved in a relatively short time, even though *in principle* there never can be completely conclusive argumentative grounds for such a solution (which, of course, does not imply that this latter is by necessity an irrational or cognitively unmotivated one). In this resolution of dissension *both* "internal" *arguments* (e.g. as to the legitimacy of the various *ceteris paribus* clauses silently assumed in the competing reports, etc.) and "negotiated," socially "influenced" *decisions* (e.g. concerning the relative advantages/disadvantages of continuing a series of experiments, etc.) usually play their interwoven parts. When the research community is unable to re-establish the consensus in the above way (which of course does happen), this fact often results not in the continuation of the controversy, but in its *neutralization* through a split of the original research area into two. (About "specialization" as a way to eliminate dissension in the natural sciences, see §37.)

29. The intended audience's specialization and professionalization certainly cannot be regarded as a characteristic of the institutionalized forms of knowledge of nature *before* the nineteenth century. In this respect one ought to summarily mention the following, generally well-known, historical facts:

(a) The period usually considered to be that of the emergence of natural scientific discourse was characterized by frequent conflicts between the corporatively organized, traditional, academic scholarship and the representatives of the new forms of natural knowledge. In these struggles the latter regularly appealed for support to a larger cultivated public. Their newly created institutions (Academies, etc.) also united the producers of scientific knowlege and their dilettante patrons in a single institution and largely on an equal basis.

(b) Seventeenth- to eighteenth-century "natural philosophy" still had a markedly multifunctional character[29] (see §24) and was in general successfully communicated to socially and culturally divergent groups of addressees. Even those works which represented the most formidable difficulties of understanding for the cultivated

[29] The question about the multifunctionality of eighteenth-century scientific literature is directly addressed by C. Lawrence (1979). See also the general discussion of this problem by S. Shapin (1982, 187–94) and the writings referred to by him – though Shapin, it seems to me, tends to conflate two distinct questions: the one about the variegated *roles* scientific writings have fulfilled (in a non-accidental way) in different socio-cultural settings and for different groups of cultural addressees, and that concerning the diversity of *motives and interests* determining the theory-choice of the scientist.

reader of the time, like Newton's *Principia,* quickly became not only objects of widely read "popularizations," but also exercised a deep influence upon, and were thoroughly discussed within, other, already culturally (in a fluid way) separated forms of discourse: theological, properly philosophical and even literary ones. In their turn, these discussions occurring in "alien" genres seriously influenced the more narrowly scientific impact of the works concerned, and were usually regarded as having a direct bearing upon the question of their truth.[30]

(c) The question of the proper audience for science and the "popular" *versus* "expert" character of scientific literature has turned in the second half of the eighteenth century into the subject-matter of explicit cultural struggles which acquired a directly political character during the French Revolution (see Gillispie 1959; Mendelsohn 1964, 7–13). It is only with the deep transformation of the whole organizational framework of natural scientific activities and of the ways social support and patronage is ensured for them that the audience's specialization and professionalization became established during the nineteenth century (in different disciplines with differing pace) both as a norm and as a fact, broadly speaking simultaneously with the professionalization of the scientist-author's role itself. It is in this process that the *république des savants* of the eighteenth century, still loosely uniting scientists, philosophers, publicists and cultivated amateurs, has been transformed into a multitude of separated *research communities* comprising the professional specialists in the given area and now posited as the sole public for the relevant scientific objectivations.[31]

30. This historical process in which the monofunctional character of the contemporary natural sciences has first been formed, at the same time meant a progressively *narrowing of their cultural significance.*[32] A discussion of this problem would first require an overview of the main stages through which the natural sciences divorced themselves from theology and philosophy – and this cannot be undertaken here. So I must restrict myself to some very broad hints on this account.

Early modern systems of scientific natural knowledge still had a direct claim to an onto-theological – and through it also moral and political – significance. One may recall the formulation of Fontenelle who certainly was no religious enthusiast:

[30] About the direct influence of theological disputes and discourses upon the formulation and development of early corpuscular theories and Newtonianism see Jacob and Jacob 1976; McGuire and McEvoy 1975; etc.

[31] For an early, succinct characterization of this whole process see Mendelsohn (1964). This paper, however, does not make a clear distinction neither between specialization and professionalization, nor between these processes insofar as they concern the practitioners of science and insofar as they occur in relation to its audience. In respect of the first distinction, see Porter (1978), and the literature referred to by him. The complex process in which the very audience of science became restricted to the professional specialist is partially discussed (in its wider socio-cultural setting) in Shapin and Thackray, 1974, 4–13. About the connection between these processes of institutional and cultural change with *cognitive* transformations see the comments in discussion by Diemer and Bohme (Diemer 1978, 228–31).

[32] In this consists, of course, that *paradox* of the universalization of scientific rationality which constituted one of the central ideas of Max Weber's historical sociology of modernity (and which should be distinguished from his more particular assumptions concerning the role of radical Protestantism in this process). Recent and convincing reformulations of this fundamental Weberian insight are presented by Tenbruck (1975) and Riedel (1979).

"Astronomy and anatomy are primarily those two sciences which most apprehensibly manifest two great characteristics of the Creator; the first His immensity through the distances, dimension and number of celestial bodies; the second His infinite intelligence, through the mechanism of the animals. True physics advances itself till it becomes a sort of theology" (Fontenelle 1790, 70). By discovering the "secret order" of nature, science was seen, and culturally posited, as providing a rational access to the divine plan of creation, as being a way of ascertaining God's intentions with the world at large and with man. In particular, amidst the deep religious crisis and political uncertainties of the sixteenth and seventeenth centuries the various readings of the "Book of Nature" played definite justifying and legitimating roles regarding the various competing interpretations of the "Book of God," of the Christian tradition which was in turn perceived as the "cement of society."[33]

From the mid–eighteenth century on (in different countries at different times, e.g. in France definitely earlier than in England) the natural sciences gradually lose this function of deciphering truly *meta*-physical messages. But they themselves now take up the role of providing the key element in the newly created complex and concept of *culture* which aspires to replace religion by offering man a completely inner-wordly and immanent orientation in life. Natural sciences now represent not only the most eloquent demonstration of what man can achieve by his own efforts when he acts rationally, but also, through what is actually achieved *in* them – through the discovery of the universe's eternal laws (or later: the cosmic process of evolution) – they promise to deliver those basic insights upon which a rational and just moral and social order may be built.[34] It is primarily in this interpretation that natural scientific education and self-cultivation also serves as an important avenue of social-cultural advancement for the new middle-class strata of society. The battle-cry of early, nineteenth-century positivism, that of the "natural scientific *world-view*," expressed these tendencies fully – and in its concrete content already foreshadowed their demise.

When the cultural closure of natural scientific discourse upon itself becomes a fact impossible not to recognize, when its results' and theories' meaning is culturally posited as completely intrinsic to this discourse alone and with only a pragmatical-technical *use* outside this proper sphere, then the divorce of natural scientific inquiry from general culture and cultivation is also inevitable. Natural science, having acquired the fundamental social function of opening up new and in principle unlimited possibilities for meaningful *technical* action upon, and intervention into the environment, can no longer confer some fixed and inherent meaning upon

[33] For short overviews of the relevant contemporary literature see Jacob and Jacob 1976; Heimann 1978; Shapin 1982, 180–84.

[34] This point of view is graphically formulated in Huxley's address *On the Advisibility of Improving Natural Knowledge* (1866): "I say that natural knowledge, seeking to satisfy natural wants, has found the ideas which can alone still spiritual cravings. I say that natural knowledge, in desiring to ascertain the laws of comfort, has been driven to discover those of conduct, to lay the foundations of a new morality." (Quoted by Tenbruck 1975, 30). See further the discussion of this problem in Shapin and Thackray 1974, 5–11.

natural phenomena. It can retain the role of a *methodological* ideal in respect of some other forms of cultural endeavor, but what is achieved through these methods in their proper field of application is now posited as having no significance whatsoever for orienting men's conduct in the world they live in, or their understanding of this lived world itself. Tenbruck (1975, 24) aptly formulated it: the view of nature provided by the sciences is no more a world-view. As to the naively simple question: why are the literary objectivations of the natural sciences not read today by a wider public beyond the narrow circle of professional experts? – the answer cannot stop at a reference to the grave difficulties which understanding such texts poses to the non-specialist. Even the argument which indicates that they are only normatively addressed to a specialized readership will not be sufficient. One also should add: because today they are culturally defined as of no interest or consequence for a non-specialist reader. Other than idle curiosity there is no reason why such a reader *should* today read the texts of natural sciences.

D. The Work in the Context of its Tradition

31. Like all texts of cultural significance, writings in contemporary natural sciences possess an intersubjectively understandable and culturally relevant (in this case: scientific) meaning due to, and through their relation to some fund of past texts constituting their "literary tradition." *Intertextuality* of meaning is a constitutive characteristic of all cultural objectivations (at least of textual kind). In our culture, i.e., under conditions of Western modernity to which the natural sciences as a broadly conceived cultural genre themselves belong, it stands under the fundamental postulate determining the character of this culture: the *postulate of innovation*. That is, as opposed to some other cultures in which works primarily fulfilling the function of preservation, collation or elaboration of a "tradition" (sacred or profane) have been recognized as culturally significant and valuable, in our culture a work must be *novel* within the relevant tradition, to be accepted as a *sui generis* cultural objectivation at all.

The intertextual character of any writing in the natural sciences is ensured by the fact that a work will not be regarded as *scientifically* relevant unless it contains "new results" of some sort – *in comparison* with a "literature," in whose context it will be placed by those who decide its cultural acceptance or rejection.

32. The literary tradition of natural sciences is characterized, as its most evident feature, with its shallowness ("skin-depth") in time.[35] As a rule, there is a significant difference between the time-span of the entire history of a natural scientific discipline, on the one hand, and the historical expansion of its *active tradition,* on the other (in the meaning of past works directly and consciously utilized by scientists and

[35] This is pointed out by almost all authors explicitly addressing themselves to the characterization of natural scientific tradition. See esp. Kuhn 1977, 228–29; Shils 1981, 109–13, and Dosch 1982, 51–52.

therefore usually also referred to in their writings),[36] – and this gap is constantly growing. In this respect the cultural organization of tradition-transmission and tradition-preservation in the contemporary natural sciences differs significantly not only from such human disciplines as philosophy, but also from a number of social sciences in which – at least in regard to fundamental works of *theoretical* nature – no such gap can be observed. (Not to speak of the arts, in which one can observe an enormous *expansion* of the esthetically mobilizable and mobilized tradition, especially in the last hundred years.) References, e.g., in physics do not usually extend beyond five decades from the date of publication of the citing article. A philosopher, on the other hand, may well quote or discuss Plato or Aristotle (and this in a paper or book of *non*-historical nature, dealing with some "contemporary" problem). The difference in question is well-reflected in such bibliometric indicators as the so-called "Price-index" (percentage of references made to the last five years of the literature). In physics it is about 60–70%, in sociology (actually: the American sociological literature of the sixties) around 40%, while its average in respect of philosophical journals seems to oscillate between 15-30% (de Solla Price 1970, 10–21).

However, other bibliometric data[37] strongly indicate that it is impossible to explain this difference in terms of more or less rapid progress of knowledge (whatever this means) in the respective fields of scholarship. Diachronic studies of the so-called "citation behavior" have failed to demonstrate significant variations in the *average rate of obsolescence* in the respective cases (e.g. between papers in physics and in sociology). As a common sense observation, I would also add: while it seems to make little sense to speak about "progress" in philosophy, the rate of *change* in its contemporary literature appears to be quite rapid – schools, tendencies, problematics, which dominate the academic field for a while, often disappear in a very short time, to be replaced by other ones. In any case there were certainly more (at least self-acclaimed) "turns" and "revolutions" in the last fifty years of the history of philosophy than during the entire history of physics.

The difference between physics and philosophy in the given respect is thus not to be reduced to the differing *average life span* of their contemporary scriptures – in both fields the overwhelming *majority* of literary objectivations ages quite rapidly. At least in part this difference ought to be explained by the distinct composition and structuralization of the respective "active traditions" in the two fields. The actually mobilized literary tradition in physics (and also in other natural scientific disciplines) consists of works of two types: the relevant writings in the *recent* literature (meaning the last five to ten years) and the *seminal* papers in the field. This latter comprises those publications that have played a pioneering role in founding a new research

[36] Of course, the traditions *embodied* in scientific terminology and, to a lesser degree, in scientific instruments and procedures, are usually of a longer duration than the literary traditions *actively utilized* by the scientists.

[37] Broadus 1971; Oromaner 1977. I am well aware of the fact that the methods through which these data were attained are the subject of a dispute in the relevant literature (see, e.g., Edge 1979). Nevertheless, the data can be safely taken – so it seems to me – as rough indicators of general tendencies, and only these latter are of any consequence here.

area, theory, experimental technique, etc., and that may remain frequently referred to during thirty to fifty years[38] – until the whole research frontier moves to other areas, or until they are replaced by radical reformulations in more contemporary terms.

In the philosopher's use of the works of the past one similarly encounters the same two types. "Seminal" are the works of those "standard bearers" who programmatically formulated the ideas of an identifiable "trend" or "school," and again these are discussed with great frequency as long as the school's direct cultural significance and identity is preserved. (Thus I would call Bohr's 1913 article on atomic structure and some of the programmatic papers of Carnap or Neurath in *Erkenntnis* "seminal" in the same sense.)

Philosophy, however, knows a third category of tradition as well: the *classical*. *Classical* are those literary products of the (usually more remote) past to which the *present* cultural practice ascribes an "atemporal" (or at least epochal) validity: an ability to shed light upon the questions of any age, even if it is realized that they were born out of a specific cultural context and directly addressed themselves to now outdated problematics. Accordingly these texts are re-read, referred to, discussed, argued with, etc., by philosophers *qua* philosophers (and not as historians of philosophical ideas), though the strictly taken "doctrines" (in the meaning of *Lehrmeinungen*) of their authors often are no longer considered plausible. Moreover, works will sometimes be retained as classical even though their most broadly conceived standpoint and approach is rejected today with near unanimity: they are regarded as "paradigmatically wrong." Not only philosophy possesses such a classical tradition. Many of the social sciences – in spite of their much shorter history – also seem to have their own classics. In theoretical sociology the writings of Marx, Weber or Durkheim (though in *some* respects considered to be quite outdated) will be treated as highly relevant to contemporary issues and controversies. Even in economics it makes good sense to qualify some present-day standpoints as Neo-Ricardian, Marxist or Keynesian, etc.

Since my whole point here is to indicate that *contemporary* natural sciences do *not* possess classical texts, a further elucidation of the notion of "classical" can perhaps be neglected here. It is, however, necessary to at least indicate that having (or not having) a classical tradition is the characteristics of some *contemporary* cultural practice, more particularly: of the way it actively interconnects itself with, and inserts itself into, the selected results of past activities; it should not be regarded as an inherent feature of the concerned tradition itself. In this respect it is certainly not insignificant that while being classical means being "atemporally" valid, the actual composition, the "canonic list" of the classics often radically changes in time.[39] Furthermore, a

[38] An unusually large percentage of these references may actually be "perfunctory" or purely "ceremonial." But since I am concerned not with the issue of actual "influences," but with that of the cultural organization of tradition in natural science, the point does not seem to be specifically relevant for the topic.

[39] In this context it is worthwhile to recall the following quote: "This also must be confessed, that the most durable, as well as justest fame, has been acquired by the easy philosophers; and that the abstract reasoners seem hitherto to have enjoyed only a momentary reputation from the caprice or ignorance of

cultural genre in some epoch can be posited as one in which no classical works can meaningfully occur, and then later a whole classical tradition of great antiquity may be "discovered" for it (as it was the case with the visual arts in the transitional period to the Renaissance). This is certainly also true in reverse. If the natural sciences today do not have a classical tradition, this does not imply that they never had one. Some of Newton's writings certainly fulfilled such a role for early nineteenth-century physics. Similarly Euclid's *Elements* functioned as a classical text in geometry, perhaps until the very beginning of this century. No text has, however, such a function in *contemporary* natural sciences. It is, therefore, no accident that from the second half of nineteenth century the historiography of the natural sciences and the actual scientific investigations of nature became sharply divorced from each other as completely different disciplines and cultural enterprises, though earlier they were usually accomplished by the same persons and treated in one and the same work.

33. The lack of a classical tradition provides the contemporary natural sciences – in comparison with other cultural genres – with a specifically *short-span historical memory,* institutionally endows them with a "historical amnesia" (Elkana 1981, 35–36). (Or, if this seems to be an unduly negativistic formulation, I am ready to say: it ensures that ease of forgetting without which – according to Nietzsche – life itself would be impossible.) This is, however, a one-sided formulation and not completely accurate. The natural sciences do have – fixed in their contemporary texts – their own long-term memory, only of a specific kind. Galilean dynamics, Newton's laws, Darwinian selection, Mendelian genetics, the Lorentz-transformations, the Michelson-Morley experiment, etc., etc., – all these are "literary monuments" of modern science, through which a reverential rememberance is ensured to its distant heroes whose works are no more actively used in its actual practice. Natural sciences replace a long-term historical memory with the preservation of corresponding *memorabilia*. The items of this history which are intrinsic to science and fixed in its very language, are not only "monumentalized" (there was no "Michelson-Morley experiment" as a single historical event – this expression replaces a complicated *story*),[40] but they are also relentlessly *modernized*.[41] Newton's laws as they are found in the recent textbooks of physics are something Newton *should* have written had he used modern mathematical notations, contemporary physical concepts, etc. The meaning of these

their age, but have not been able to support their own renown with more equitable posterity. . . . The fame of Cicero flourishes at present, but that of Aristotle is utterly decayed. La Bruyère passes the seas, and still maintains his reputation: But the glory of Malebranche is confined to his own nation, and to his own age. And Addison, perhaps, will be read with pleasure when Locke shall be entirely forgotten." And the author of this ridiculous misjudgement had, nevertheless, some idea of philosophy – he was called David Hume (1748, Sect. I, §4.)

[40] See its description and analysis in Lakatos (1970, 159–65), who also underlines that the meaning usually associated with "the" experiment could only be established retrospectively, twenty-five years later.

[41] This is forcefully emphasized by Kuhn (1970, 136–43). Kuhn, however, essentially interprets this fact as the outcome of an *ideology*, imbued by a pedagogic practice and functional from the viewpoint of creating a group mentality ultimately promoting "progress." On this point see §40 and §43 of the present paper. About the unconscious modernizations involved in the intrinsic "folk-histories" of science see also Elkana (1974, 175–97; and 1981, 59–60) about the retrospectively construed character of some of the best known cases of "simultaneous discoveries" in the natural sciences.

expressions, therefore, changes as science progresses, though they are, of course, posited as designating well-identifiable, singular historical phenomena and events.[42]

This intrinsic "folk-history" of a discipline suggests a very definite conception of the character of its development. Firstly, it ensures a highly individualistic picture of cognitive change in science as primarily a matter of those culture-heroes who really mattered and whose name is perpetuated. Secondly, it makes the past directly *incorporated* into the present which is seen as containing everything that was valuable (and worthy of recalling) in the past. We – pygmies or not – just stand on the shoulders of all these giants, and so we see *further* – and *not otherwise*. Thirdly, these historical *memorabilia* are also *memento mori:* science, in its relentless progress, turns even the greatest intellectual achievements into mere relicts, in it there is no other certainty besides this unlimited drive forward.

It can be pointed out that philosophy equally knows and uses such "literary monuments" of its past. No doubt, "Cartesian dualism," "Spinozist monism," "Hegelian dialectic," etc., are historical *memorabilia* of the same type, with a meaning both vague and modernized. There is, however, one difference. In *philosophical* discussions (and I underline: philosophical and not historical ones), under definite conditions, the hermeneutical legitimacy of the so associated meanings can be raised. E.g., in the critical reactions to Ryle's *Concept of Mind* – a work with no historical pretensions – the question of the adequacy of its author's conception of Cartesian dualism has been frequently and emphatically raised, and justly so. Because whether what contemporary philosophy considers to be the *classical and paradigmatic* case of a dualist metaphysics really fares so badly in providing answer-schemas to the problems discussed, had a serious relevance to the claims Ryle was making. It seems to me that nothing analogous could occur in contemporary natural sciences – nobody will be taken to task because his or her mentioning of Newton's laws has been textually inaccurate or historically anachronistic. Of course, this latter occurrence is rendered highly improbable today by the fact alone that it is only the philosopher whose professional competence is conceived of as including the knowledge of (at least some) classical texts.

34. There are, naturally, some very good reasons for this "historical amnesia" of the natural sciences. In this respect special importance pertains to the fact that the knowledge accumulated in the experimental natural sciences cannot be fully objectified in their texts alone (see §22). Especially the understanding of experimental reports presupposes a degree of shared "tacit knowledge" without which it is impossible to translate the formulaically indicated procedural rules and technically designated materials, devices, etc., into envisageable practical operations with well-

[42] In the language of hermeneutics one could say that in this implied form of historical understanding the aspect of *application* (in its Gadamerian sense) completely dominates over that of interpretation proper. In this respect the "naive hermeneutics" of the natural sciences is a kind of *dogmatic* hermeneutics, akin, e.g., to the traditional Biblical one: both *grant* the validity claim of *some* texts as the *precondition* of understanding. There is, of course, a fundamental difference between them: in the case of the modern natural sciences these texts are not some authoritatively fixed scriptures of the past, but the momentarily codified literature of the ever changing *present*.

defined objects. As experimental apparatuses, standards for materials change, measuring procedures, etc., are altered, this tacit, operational know-how necessary for the understanding of the texts *disappears* (to be replaced by another one); now it can only be reconstructed by proper historical investigations. The fact that the operational meaning of the low-level theoretical (or *sui generis* observational) terms of science changes in this way, while their reference seemingly remains the same, only complicates the situation. Reading far earlier research reports, the scientist is often unable to work out what the experimenter really did, how reliable his or her measurements were, and even the data *of what* they actually are. (History of science knows many examples when the *meaning* of some past measurement has been radically revised in retrospect; e.g., in the historiography of early research of electricity we are constantly told: what has been measured was "really" something else than the original experimenter has thought.) Related, although somewhat different considerations apply to texts of purely theoretical nature, too. *Given* the way the scientist's professional competence is culturally constituted today, the scriptures of the natural sciences have a "*built-in* obsolescence" which makes the extension of a "search for literature" beyond definite time-limits an essentially senseless enterprise.

35. Difficulties of similar type are, however, not unknown in humanities and "soft" sciences either, insofar as the understanding of their classical texts is concerned. Even if these latter represent forms of "metadiscourses," i.e., the specific problems connected with the relatively traceless disappearance of a tacit operational know-how does not emerge in respect of their comprehension, their meaning is never simply "given" to the modern reader – it has to be recovered by historical-hermeneutical means. Since this meaning is posited as being relevant and enlightening in regard to *present-day* problems, the task of interpretation emerges anew again and again. Thus the differences between the two cultural genres regarding the organization of their effective traditions ("long-term" *versus* "short-term historical memory") cannot be accounted for *merely* by the differing features of the scriptures constituting their respective histories. (All the less since in more remote cases one and the same text can sometimes be legitimately conceived as belonging to both the history of physics and to the history of philosophy, and then it often will be read by philosophers, and not by physicists.) The question rather is the following: why is it the professional obligation of a contemporary philosopher to know something about Aristotle and to have some rudimentary competences for reading his texts, and why is this not so in the case of a physicist? The answer to this question ultimately has to point to the differing ways in which the field of *contemporary research or learning* is culturally articulated in the two genres and to the ways this organization is then reinforced, legitimated and carried forward by a corresponding structuration of their activated historical past.

36. Human and social science disciplines are in general culturally articulated in a *polemic-dissensive* manner.[43] Though they are normally divided (usually in an

[43] C. J. Lammers (1974) has spoken about their "multiparadigmatic" character in a related sense.

ephemeral, overlapping and fluid way) into a number of "co-ordinated" specialties, this division is at least partially overlaid by another one: that between competing theoretical "schools," "trends" and "tendencies."[44] The relationship between these trends is *competitive-agonic*. They are usually regarded as in principle imcompatible solutions to ultimately identical or closely related problems (even if the explicitly formulated *questions*, to which they give answers, are *different*), as *alternative theoretical models or images* of what the discipline is about. Intellectual consistency (and frequently practical engagement, too) demands a choice between them.

The organization of the tradition in the discipline or branch of learning is then to *support* this polemic structuralization of its contemporary field. It traces the presently relevant theoretical alternatives (or the various conceptual components into which they are analyzed) back to their "origins," and fixes as *classical* those texts that gave a *paradigmatic* formulation to one or another of these alternatives: formulations which are posited as "forever" or at least epochally valid, since they demonstrate most clearly the reasons and motives for, and the consequences implied by, the acceptance of some fundamental theoretical model or image. The discipline's tradition is thereby organized into a number of "traditions," and each of its present "schools" – drawing on the common pool of "classics" – usually constructs a somewhat differing "list" of them and gives them a distinct (sometimes sharply opposed) interpretation.[45] Such an "agonic" reconstruction of history renders some works of the more remote past directly mobilizable for the present debates (both for legitimating and argumentative purposes), and at the same time *maximalizes* the number of presently available conceptual *alternatives*.[46] A culturally active tradition is traced back with great historical depth which in some cases may extend beyond the time-point from which onward one can meaningfully speak about the existence of the discipline at all. (There is a Marxist sociology today, though of course there was no sociology as such during the life-time of Marx.) On the other hand, *what* the so-conceived problems and alternatives are – even in philosophy which is apt to treat them as eternal-perennial questions and controversies – *ultimately* depends on the present state of scholarship. Philosophers are, e.g., inclined to trace back the dispute between idealism and materialism at least to a "conflict" between Democritus and Plato, irrespective of the fact that these tendencies of philosophical thought do not appear as explicitly recognized and opposed alternatives till the eighteenth century.

37. The contemporary cultural field of the natural sciences is structured – in opposition to the above – in a *pluralistic and consensual* manner. These disciplines are not only rather sharply divided into a number of specialties, but these latter are again informally decomposed into research areas conceived as the main loci of

[44] Usually one and the same "school" will be meaningfully discerned as being present or influential simultaneously in many, though not necessarily in all, of the specialties comprising the discipline.

[45] The very historiography of philosophy begins, in Alexandrian times, with the construction of such an agonistic- "confrontationalist" interpretation of its development (Markus 1984).

[46] As a result, the specific assertions and knowledge-claims made by the authors in these disciplines usually are evaluable only within broader contexts which, in their turn, are neither strictly fixed, nor consensually accepted. This point is specifically emphasized by Bazerman (1981, 370–73).

innovative inquiry. The various areas and specialties within the discipline are posited to co-exist in a fashion of loose *co-ordination,* as forms of inquiry directed at *different,* though interrelated problems, and sharing a background consensus concerning the (theoretical and experimental) foundations of the discipline the content of which is represented by its actual "textbook *fundus.*" Even when it is accepted – that is, taken for granted both by common sense and in the implicit ontology of science – that the various specialties in fact investigate *one and the same* "object," the different theories, results, etc., attained by them are usually not conceived as rival and alternative models, among which one ought to choose, but as conceptualizations of its *different aspects,* which are, at least in principle, compatible with each other. This assumption can even be upheld in cases when the theories in question are (in their present form) logically irreconcilable: their reconciliation is then projectively postulated as a task "future research" will solve (e.g., the relationship between the general theory of relativity and the nonrelativistic quantum mechanics). In this way *dissent* in natural sciences is, at least in periods of "normal" development, *contained, restricted* to disputes within the particular research areas as disagreement about how to answer definite questions – not as a controversy about the ways the very problem should be comprehended, and the object of the inquiry should be approached.

This consensual and pluralistic organization of its field is not so much a factual characteristic of "normal science" (i.e., a state-description that most of the time happens to be true in natural sciences), but it is rather a state which this type of cultural practice tends to "normalize." That is, these practices are *directed at* the containment and localization of cognitively relevant dissent by a number of specific, recognizable means. Disputes, for the plausible resolution of which the theoretical and technical resources of the discipline are, at the present level of knowledge, insufficient, often simply become removed as being "unscientific" or "metaphysical" (to be re-opened at a later stage, perhaps). Persisting controversies which for a longer time split the concerned research community are often "neutralized" by transforming the original disagreement of *views* into equally legitimate separate specialties investigating different *aspects* of the same phenomena. Competition between them is thereby removed from the argumentative-cognitive level to a "social" one (competition for academic and broader recognition, for financial funds, etc., on the basis of their perceived "fruitfulness" and significance). An ever progressing specialization of research thereby functions as a way of conflict resolution in natural sciences.[47] In such a manner the strong *objectivism* of science (§27) is "tempered" by an easily evoked *perspectivism* which, in a reifying fashion, transforms theoretically, methodologically, and technically differing, and sometimes conflicting, *ways of investigation* into "aspective" differences of the *investigated reality.*

[47] About the conflict-neutralizing function of scientific specialization and "segmentation" see first of all Hagstrom (1965, 187–226). For a historical case study (of the dispute between Bateson and Pearson, and the emergence of biometry) illustrating this process cf. Farrall (1975). Further examples can easily be invoked (the divorce between thermodynamics and the kinetic theory of gases, between genetics and molecular biology, etc.).

When these normalizing practices of science fail to contain disputes, the event is perceived as anomalous: a "revolution," if its consequences later are regarded as significant and positive; an "aberration" if they are judged to be fruitless. This perception depends relatively little on such occurrences' actual frequency in the history of science – judging something to be anomalous has not much to do with its being (or not being) rare.

38. The actual organization of tradition in the natural sciences *supports* this perspectivistic, pluralistic and consensual organization of their contemporary activities. Textbooks often contain (as introduction) the discipline's official history – a hagiographic story interconnecting its most important "memorabilia" into a simple sequence. But the actually mobilized and culturally-cognitively employed *tradition* is organized and structured *within* each research area and essentially *independently* from each other. Instead of a number of long enduring and alternative tendencies-"traditions" (whose argumentative competition ensures the unity of the given branch of learning), the activated past in the natural sciences is present as the multitude of co-existing, side by side, *"states of research,"* each of relatively short duration and connected only through partial overlaps. Each research paper directly contributes to the relative consolidation and the simultaneous modification of this tradition. The natural sciences can afford a lack of reflective historical consciousness, because *each* literary objectivation immediately participates in the articulation and interpretation of that (shallow) past which is relevant from the viewpoint of their present activities.

39. This tradition-organizing function is today a *formal* feature and requirement of publications in the natural sciences. This is connected with the hermeneutical function and meaning of that conventional and standardized literary structure of the contemporary research paper which has been mentioned earlier (§7/b).

The *Introduction* of the research report (together with its *References*) has the task of placing the given investigation in respect to the "contemporary literature," i.e., the literary inscriptions of a short-term past. For this purpose the corresponding tradition is reconstructed and interpreted in a characteristic manner: it is transformed into a single, but partial and open-ended *argumentative complex*. Works which may have been originally entirely unrelated, are now interconnected as either corroborating or possibly contradicting each other from the viewpoint of the chosen problem; the actual time-sequence of publications (and the lines of actual influences) is largely neglected and replaced by constructed argumentative nexuses.[48] In this way the author – as a truly creative recipient and interpreter of the past works of science – does not only select between them, but simultaneously transforms a complex, usually many-centered and heterogeneous historical story into a "logicized" one, thereby comprising it into a *momentary state:* the "current state of research." This logicization and momentarization of a short-term history serves a well-defined purpose; namely, to delimit a definite area of concerns as a relatively autonomous, legitimate field of

[48] In the mainstream analytic philosophy of science this actual practice became codified and legitimated through the conceptual distinction drawn between the "context of discovery" and the "context of justification."

inquiry and – even more importantly – to demarcate within it what is already solved, the sphere of the known, from that of the still persisting, unresolved dispute and ignorance. In this manner the past is construed as *objectively posing some question(s)*, to which the paper then addresses itself. The Introduction, therefore, through a definite construction of the tradition, provides legitimation for the paper's claim of "contributing" to the existing state of knowledge, by narrowing down an objectively pre-given area of uncertainty and ignorance. To legitimate his or her own work as relevant to science, the scientist must "acknowledge" the relevance of some earlier publications, and in this way he or she actually transforms them into, or preserves them as, a tradition culturally active in, and significant for, the present (see Gilbert 1977; Knorr-Cetina 1981, 100–101 and 110–13).

This organization of the tradition, in which each natural scientist is routinely, and mostly unreflectively, engaged, is, of course, a "subjective" interpretive activity, i.e., it involves individual (or group) decisions about the limits of the research area, the relevance, novelty, and significance of earlier literary contributions to it, etc. – all, of course, depending on the way the scientist in question interprets his or her own results and their possible significance. There are no cognitive criteria which would prescribe (or allow) an unambigous choice concerning *any* of these matters. It can well happen that two scientists, performing largely similar series of experiments, will construct the relevant literary tradition quite differently, and in such a way insert their own findings into different theoretical contexts and interpret them accordingly.[49] Nevertheless, these *(re)constructions* of the tradition are seen as simple *descriptions* of a pre-existent state (of knowledge). This is ensured not only by the injunction prohibiting the stylistically direct expression of individual choices and attitudes, but also – and first of all – by the existence of a number of normative requirements aimed at securing the "impersonality" of such a construct. The "search for literature" ought to be comprehensive, all works of influence and relevance should be acknowledged, the interpretation of the findings of other authors – except in cases of explicit polemics – should not radically deviate from their authorial interpretation, etc. Certainly, already the meaning of these requirements is diffuse, and in the actual practice even cases of flagrant deviance (simple neglect of rival theories) do frequently occur. Nevertheless, these postulates are *posited* as valid. That is, a paper can be criticized (and even denied scientific relevance) for conspicuously failing to comply with them. It is perhaps best to regard the construction of tradition (presented in the Introduction, etc.) as a *proposal for consensual acceptance* of what should count as the actual state of knowledge in the given area. It is then in the literary interaction of the on-going sequence of related publications that this tradition becomes – for a time – relatively stabilized (the "important recent works" in the field, most frequently referred to, and often in one cluster, are selected), and in this same process the given paper also succeeds or fails to insert *itself* into this tradition.

[49] Compare, e.g., with the material presented in Gilbert and Mulkay (1984, 43–51).

40. If the task of the *Introduction* is primarily the construction of a short-term history as objectively posing a question to be answered or solved, the *Methods* section serves to demonstrate that the way the given scientist proceeded to "find" the answer, his or her manner of producing the new scientific knowledge, was a warranted one, codified by the previous literature and research. The alleged "raw data" pertaining to the answer are then presented in the section of *Results*, to be interpreted as providing (or contributing to) the solution of the originally posed question in the *Discussion*. This latter section, however, usually accomplishes more: the requested sceptical tone of science gives at least strong preference to the explicit indication of the still undecided interpretative alternatives, the theoretical and empirical problems left open for *future* research. In this way the paper inserts itself not only into an immediate past, but also posits itself in some relation to the projected (proposed) future of inquiry as well.

41. Thus, already the formal-routine organization of a natural scientific paper submits its whole to the hermeneutical logic of *question and answer* – with the *proviso* that the questions appear as our own (dictated by the present state of our knowledge and ignorance), while the answer is largely objectified and naturalized (ultimately provided by the all-deciding data as "straight facts"). The so-reported experiment, therefore, truly becomes a way "to force nature to answer our question" (Kant) – the second all-powerful metaphor which, just like the simile about the "Book of Nature" (§13) directly transposes intersubjective, hermeneutical-dialogic relations to the interaction between nature and man.

This "naturalization" of a hermeneutical process is ensured partly by the fact that the logic of question and answer is applied in the natural sciences as the *direct organizing principle* of their historical tradition, through which history becomes strongly "logicized"[50] and turned (from the viewpoint of the present) into a unilinear process. Through the structuring just described each paper becomes firmly embedded into a short-term past, in which the lines of demarcation between knowledge and ignorance are firmly drawn. It then claims to push this frontier somewhat further and thereby also to open up some new questions. Thus, the institutionalized character of these literary objectivations confers upon them the significance of a *contribution* to an always open-ended, continously progressing collective enterprise. All literary works of the natural sciences are thereby posited as merely transitory stages, evanescent stepping- and stopping-points in the relentlessly forward moving process of knowing.

The idea of scientific *progress* is therefore neither a mere ideology of the scientists (and some philosophers), nor does it express some fact about the history of science: it is a *postulate*, the admittance of which is necessary to confer *meaning* upon natural

[50] This "logicization" of history is directly connected with the norm of reproducibility of the experiment. Insofar as the research paper claims to describe the actions of the experimenter only in those of their aspects which render their outcome (the "results") replicable, it also has to eliminate the real historicity of these actions in respect to their *motives*. The personally and historically contingent actual motivations of the actor have to be replaced by reasons which can be claimed to be equally compelling for everyone committed to scientific investigation in the given area of knowledge. The undertaking of the concrete experiment is to appear as a rational thing to do in regard of a given "state of knowledge" *alone*.

scientific activities as they are culturally organized today. This is so not only in the sense that the "historical amnesia" of the modern natural sciences, the shallowness of their activated tradition, can be conceived as rational only as long as one assumes that everything cognitively valuable in the works of more remote past is completely contained in the "recent" literature. As I tried to argue, the requested "adequate *understanding*" of a scientific paper, posited by its genric form, demands its comprehension as a contribution to an encompassing, irreversible process of knowing, constantly moving forward. The idea of progress is therefore to be conceived of as a *historically-culturally contingent regulative idea*[51] which is intrinsically connected with, and undivorceable from, the contemporary cultural organization of natural scientific activities and their literary objectivations. Of course, the question: whether there is "progress" in any given branch of science or research, "progress" in the sense of criteria, implied or suggested by its regulative idea – is an empirical question. But the presupposition that natural scientific knowledge in general is capable of accumulative progress is a historically specific assumption connected with the contemporary cultural organization of this form of knowing. Its "contingency," however, does not imply its arbitrariness. Natural science is "able to progress" as long as the so-organized cognitive practice can actually satisfy the basic social expectations, demands, and needs addressed to it, at least as long as it is not challenged effectively by a form of practice otherwise organized.

42. There are, of course, strong *social grounds* which can account for the strikingly different ways in which the cultural fields and the associated traditions are structured in the humanities, on the one hand, and in the natural sciences, on the other. Works in the human and social sciences remain strongly connected and associated – either in an avowed, or in an unreflected way – with differentiated, partly opposed social interests, with the legitimation or the criticism of various extant social insitutions and practices divergently affecting the social position of different groups in society. Our relation to nature, on the other hand, is posited within our culture as being predominantly a technical one which should only be judged on the basis of the graded criterion of effectiveness usually conceived as neutral both morally and socially. The fact, that there are good social-cultural grounds which make the existence of the above difference comprehensible, does not mean, however, that there are *compelling epistemic-cognitive reasons* for it. In the latter sense it is not necessary that the tradition in humanities *should* be organized into competitive, opposed "trends" of long historical duration, and in the natural sciences organized into many co-existing and merely partially overlapping "states of research" with shallow time-compass.

In fact the historical tradition of philosophy (to take this, in the given respect certainly most extreme example) *can* well be ordered according to a schema of continuous growth of philosophical knowledge. This has been done by most of the

[51] I use here the term "idea" in a quasi-Kantian sense. "Ideas" are non-arbitrary *Sinnbegriffe*: forms of self-interpretation which fulfill a normative and orientative role by conferring a definite meaning upon practices, a meaning, however, which is not a "free invention" of the acting individual, but bound to the cultural-social constitution of the concerned practice.

great "philosophical histories of philosophy" (mostly depicting their own authors' system as the immanent *telos* of this whole evolution), from Aristotle through Hegel to the neo-Kantian historiography. One ought not only to acknowledge the historical effectivity (in the sense of a *Wirkungsgeschichte*) of these writings, but – I think – should also hesitate to call them, even strictly interpretively, unilluminating. Furthermore, it is, of course, quite possible at any given historical moment to "reconcile" the fighting "sects" of philosophy and to neutralize their dispute, precisely through that method of perspectivistic relativization and appropriate restriction of their opposed cognitive claims (to differing "spheres" or "aspects") which is, as we have seen, often exercised in the natural sciences. In the history of philosophy there has always been present a strong impulse towards such a syncretism. Today such attempts are evaluated pejoratively as "eclectic." This negative judgement, however, hardly accurately reflects either their past significance (some of the greatest philosophical achievements of the past – to mention only Leibniz or Kant – were seriously motivated by such an aim), or their present role (in all probability the majority of contemporary academic philosophers are not orthodox adherents of one or another clearly formed "school," but are "eclectic syncretizers"). This evaluation does, however, correctly express the cultural irrelevance – under present conditions – of syncretism as a *philosophical program* of solving the "dispute of schools" forever. To this, however, one has to add: there *were* whole cultural periods (e.g., late republican and imperial Rome) when syncretic attempts of some kind did achieve a relatively lasting dominance. (True, these were hardly the most fruitful epochs in the history of philosophy.)

43. The same point can be argued the other way around, from the side of the natural sciences. Until the early eighteenth century, accepted forms of natural knowledge were in general embodied in a number of culturally (and often also nationally) specific, multifunctional theories which opposed each other competitively as alternative and irreconcilable "models" of the world of nature of which one had to choose. The relationship between Cartesian, Newtonian and Leibnizian "physics" (to mention only one case) was not in principle different from that existing between rival metaphysical systems (from which, of course, they cannot be divorced but only in a modernizing, ahistorical abstraction). This situation has then progressively changed during the entire eighteenth century. In spite of the predominantly Newtonian rhetorics of the age, it was *not* one paradigm's consensual triumph and general acceptance over all the others (as it is sometimes argued in a too easy application of Kuhn to real history) which has actually occurred during this period. Rather a "hybridization" of these models, conceived earlier as mutually exclusive, has taken place; an "opportunistic eclecticism" which blended and combined their various features and constituents in ways depending primarily on the central research interest dominant in one or the other case (and also on the cultural traditions prevailing in the given milieu).[52] One should perhaps date the emergence of a

[52] For a convincing marshalling of the evidence and concise argumentation to this effect see Elkana (1971), Guerlac (1977), and Schofield (1978).

cognitive strategy of "perspectivistic reconciliation" in the sciences of nature from this time onward. Earlier, in the dispute over the interpretations of Copernican theory, this had been definitely rejected. In any case, it was this "eclectic hybridization" of the various paradigms which prepared the conceptual grounds for a theoretically and methodologically more rigorous joining of the concerns, results, and models of "experimental natural philosophy" to the mathematical principles of a "general physics," primarily to the (appropriately reinterpreted) Newtonian laws of mechanics, and allowed the emergence of physics as a unified (and growingly professionalized) discipline in the first half of the nineteenth century. This process also involved far-reaching cognitive changes in the understanding of the relationship between both experiment and theory, and between experience and mathematics, changes which influenced both the operational (experimental) and the literary practice of the discipline (see Silliman 1973; Smith 1978; Cannon 1978, Chap. 4; Bellone 1980).

44. One cannot identify, however, the cultural constitution of tradition even in the nineteenth-century natural sciences with the relevant characteristics of their present practice. The mere fact that Newton was treated during this period as the "classic of physics," in the full sense of the word, already indicates the difference. The concept of an endless scientific progress, which already then had been firmly anchored in the cultural practice of the natural sciences, was still bound together with an equally firm belief in a definite (achieved or soon achievable) "scientific world view," whose principles were beyond any reasonable doubt and provided the guarantee for an *extensive* growth of knowledge. It was again Kant who first clearly articulated *both* aspects of this progress-concept in his theoretical philosophy.

It is only from the late nineteenth early twentieth century onward that the conception of an endless growth of knowledge in science has become interwoven with that of a principled *fallibilism*. Scientific progress now meant an irreversible process constantly approximating towards some inachievable end which was also uncharacterizable and unpredictable in any essential trait of its content. At about the same time the literary objectivations of the natural sciences, and the genric conventions and rules concerning their appropriate constitution and literary use also acquired their contemporary, modern form. In particular, the presently known rules of referencing, together with the specific "short-term historical memory" of the natural sciences described above, have been slowly established from the second half of the nineteenth century. Historians of science recently began to speak with growing frequency about a "second scientific revolution" which occurred during the nineteenth century – meaning *either* some radical changes in the theoretical orientation and methodological standards of science (first of all physics), *or* a fundamental transformation in the forms of social organization of scientific activities in general.[53] A hermeneutical analysis of the natural sciences suggests that these two types of

[53] The first view is exemplified in the works of Bellone (1980), and Cannon (1978). For the second view see Mendelsohn (1964).

transformation were interconnected and integrated with each other through a series of simultaneously occurring changes not merely in their literary practice, but also more broadly in the set of cultural (ATR) relations which sustain this practice. Natural science as the cultural genre which *we* know, as the familiar form of institutionalized discursive activities, is the product of a nineteenth-century development in which the cognitive structure, institutional organization, cultural forms of objectivation and its global social function have changed together.

E. Some Presumptive Concluding Remarks

45. Any substantive conclusion regarding the natural sciences in general which can be drawn from an hermeneutical analysis of the type attempted above must be conjectural and tentative. Such an analysis can deliver primarily a *phenomenological description* of those cultural conditions which are necessary to confer *meaning* upon the literary objectivations of contemporary natural scientific discourse. Its specific cultural constitution can thus be brought to sharper focus in contradistinction to other cultural genres, on the one hand, and to historically earlier forms of "natural knowledge," on the other. Since modern natural science, as has been emphasized, is comprised of not only literary-discursive activities, this analysis cannot, in principle, exhaust the subject-matter. Furthermore, by concentrating on the historically-culturally contingent conditions of culturally codified *meaning*, it cannot replace the more traditional enterprises of epistemological and sociological analyses, with their focusing on the problems concerning conditions of truth and social efficiency, respectively.

These three large problem-areas are, however, certainly not independent, even analytically, from each other. A hermeneutics of the natural sciences can, in particular, provide a needed corrective against often encountered biases of traditional epistemological and sociological approaches.

Philosophers of science often treat – or at least did so for a long time – the outcomes of natural scientific inquiry as disembodied "theories," i.e. systems of *propositions* pertaining to some "idealized" language. Sociologists of science in the Mertonian tradition, on the other hand, used to look at them as if they were *utterances* of a speaker, motivated by his or her interiorized values and goals, and aimed to influence some group of interlocutors to achieve these ends. A hermeneutics of the natural sciences can serve as a useful antidote against both these views, insofar as it insists that the products of this type of cultural practice are *texts* of a well-defined type: literary objectivations with strong institutionalized "genric" characteristics that normatively circumscribe the way of their production, transmission, reception, and interpretation within the given historical-cultural context.

46. At the same time such a hermeneutics can also perhaps temper the force of those, today often vocal "revisionist" attacks upon the above, mainstream philosophical and sociological views, which seem to lead either to a radical epistemological relativism or to a strong sociological externalism, or both. The claims of objectivity, replicability, communality, novelty, and advance of knowledge are not simply ideologies, that is forms of a false consciousness making the recognition of the proper character of a practice – in the interest of some agents – impossible; just as they are not (à la Merton) interiorized conscious maxims and standards actually motivating the activity of the scientist and forming his or her expectations towards the conduct of the others. They are rather *normative requirements* impersonally imposed upon the activity and literary interaction of the actors by the specific way this type of cultural practice and its objectivations are historically constituted, and this largely occurs independently of the actual motives and rules of conduct of the agents in question. Certainly it is true that in an absolute sense, irrespective of the cultural-social context, these requirements can never be fulfilled. Moreover, there are no historically constant methodological or epistemological criteria for deciding unambiguously and with certainty in any concrete case whether these requirements had in fact been complied with, even *relative* to the available intellectual-cognitive and technical resources. This must always remain a matter of *decision* for some group of concerned agents. This does not mean, however, that the decision in question is in principle arbitrary, though it always will depend – to a larger or lesser degree – on the nature and character of the individual case, i.e. it cannot be but *prudential, fallible and revocable.* It is again true that not only cognitive-argumentative, but usually also some "external" considerations will influence the decision actually taken. It does not make it, however, *eo ipso* non-rational, nor does it invalidate the very distinction between "external" and "internal" factors, because it belongs to the cultural organization of the contemporary natural scientific practice that such a distinction – again in a tentative and negotiated, but non-arbitrary way – *ought* to be made. In general, a "prudential" fulfillment of the "internal" requirements of scientific activity in the on-going process of cultural interaction between the members of scientific community is a condition of the meaningfulness of these activities, *given* the present constitution of natural scientific practice.

47. A hermeneutics of the natural sciences renders, in my view, highly implausible (though it certainly does not *prove* them wrong) all those attempts that endeavor to account for, and to justify, the cognitive characteristics of contemporary natural science in terms of some universal conditions of rationality, be they understood either in a strictly transcendental sense, or in the meaning of "quasi-transcendental" anthropological constraints pertaining to human knowledge in general. Hermeneutical analysis brings into relief those contingent cultural conditions and relations to which these epistemic characteristics are bound, or at least with which they are historically associated. It indicates that even within the post-antique Western intellectual development there have been a plurality of forms of "scientific" knowledge of nature as differently constituted cultural genres which fulfilled not only dissimilar

socio-cultural functions, but each possessed also a distinct epistemological structure (with an associated understanding of experiment, theory, scientific proof, criteria of novelty and advance, etc.) as well. It is certainly possible to *reconstruct* the sequence of these forms (and that of the theories in which they were embodied) as constituting a *progressive* development in our rational knowledge concerning natural phenomena; contemporary natural science as a culturally constituted form of activity even demands that we do so. And we can successfully make such "evolutionary" reconstructions, *once* we accept the present state of scientific knowledge as the *telos and criterion* of this whole development. The fact that we succeed to do so – that "Whiggish" histories of science are for us both more convincing and more illuminating than, e.g., "evolutionary" histories of painting are – is not inconsequential; it says something *both* about our culture and about science. This success and its "ease," however, should not obliterate an awareness of the fact that these are interpretive reconstructions of history determined by our own cultural premises, and that the actual course of the so construed past "scientific evolution" in fact changes (sometimes dramatically) with each significant change in the composition and character of our *present* knowledge. Given the socio-cultural preconditions of modernity, natural science is an intellectual enterprise with the inherent ability to "progress," but any attempt at the definition of the criteria of this progress within some framework independent from transient historical-cultural variables seems to me doomed to failure and leading only to the hypostasis of some particular cultural characteristics as universal constituents of human rationality.

48. Such a standpoint of a "strong historicism" cannot deny the possible (*in principle*) meaningfulness of the idea of an "alternative" natural science. A hermeneutical approach to the contemporary natural sciences is itself able to indicate (though this has not been attempted within the framework of the present paper) definite strains within their actual practice, and it certainly bares those features that are far removed from, or even contrary to, those expectations which the great tradition of Enlightenment has organically connected with the idea of scientific progress. Such an approach, however, lends as little support to a romantic critique of science in practical respect as to an epistemological relativism in theoretical respect. It certainly indicates that there is a broad historical simultaneity and definite affinity between the various, epistemic, cultural and socio-functional traits of modern natural scientific practice. In particular, it emphasizes that the natural sciences have *lost* their direct and general cultural significance (in the Weberian sense, i.e. in the meaning of an encompassing cognitive orientation in the lived world as nature), their discourse became self-enclosed (i.e. one among experts alone) in the very same process in which they *acquired* those epistemic and social characteristics which made them able to fulfill a direct function in technical development (transformed them into a "productive force proper," in the Marxian sense). The historical simultaneity and cultural affinity of these traits does not yet prove them to be undivorceable from each other under all conditions, but it seriously undermines the relevance of those wholesale criticisms of science which concentrate upon some culturally disquieting

features of its present practice, tacitly presupposing that they can be changed without perhaps sacrificing some of its characteristics and continuing achievements that may be fundamental to modern societies. The idea of an "alternative science" formulated in terms of (desirable) generalities remains, at the very best, a completely empty possibility which cannot be discussed rationally. A systematically evolved, social-cultural practice can be effectively criticized only from the perspective of a meaningful and concretely articulated alternative which can transform or replace it. A strongly historicist standpoint is just the opposite of the comforting relativist belief that "anything goes": if history teaches us anything, it is – unfortunately – that among the great many imaginable and perhaps desirable things at any historical moment, only a very few have any chance of a practical-social realizability.

49. A hermeneutics of the natural sciences can only render explicit those characteristics which under contemporary conditions make a reflexive hermeneutical awareness unnecessary for the successful practice of the natural sciences; it merely indicates the "price" for the ease of their hermeneutic achievements. To the question: "Is this price right?" it can provide no answer, since it is not a problem with which a philosopher would (and could) have more competence than anybody else. A hermeneutics of the natural sciences can only attempt to contribute – as philosophy should – to the clarification of what is at stake in asking this question, to elucidate what we do – as cultural beings – to ourselves, when we practice natural science the way we now do.

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Department of General Philosophy
The University of Sydney