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ECONOMICS IN PHILOSOPHY OF SCIENCE: A DISMAL CONTRIBUTION?

ABSTRACT. This paper draws a connection between recent developments in naturalized philosophy of science and the Buchanan research program in economics. Economic approaches in naturalized philosophy of science can be combined to form an economic philosophy of science. After giving an overview of some of these approaches, I lay out the fundamentals of the Buchanan research program. I argue that its main elements are a theory of interactions and a normative foundation in consensus which help to answer some important criticisms of economic philosophy of science.

1. INTRODUCTION

A glance at the development of philosophy of science during the last two decades shows that a growing number of its leading protagonists such as Kitcher (1990; 1993), Rescher (1978b, 1989, 1996), or Goldman and Shaked (1991) employ methods from economics.[1] These applications of economics to the methodology of science, however, have been criticized heavily. Most of these critiques were directed against the supposed abstract character of economic methods which, as the critics claim, have nothing substantial to contribute to philosophy of science. Moreover, the economic basis was questioned for its own unresolved methodological problems. In this paper, I argue that there is a direction of economic thinking, James Buchanan's "constitutional economics", which has not previously been applied in this context but which could be a very useful tool and could avoid many of the difficulties of other approaches. I am going to apply this approach to an important episode in the history of science, the "Great Devonian Controversy", which is best understood in Buchanan's constitutional terms. I thereby hope to show that there is a lot of potential in economics as a resource for philosophers of science provided one uses the constitutional economics. I argue that the constitutional approach could provide a basis for developing an "*economic philosophy of science*" in a systematic way.[2]

This article is laid out as follows: In Section 2, I give a brief overview of some 'economic' approaches to philosophy of science and their critics.

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In Section 3, I lay out the fundamentals of the Buchanan research program. Its consequences for philosophy of science will be dealt with in Section 4 before drawing a conclusion in Section 5.

2. MOTIVATING THE ECONOMIC APPROACH TO PHILOSOPHY OF SCIENCE

Science is a social enterprise. This is generally acknowledged by sociologists, economists, and also by many philosophers of science. As a social institution, science cannot escape the dimension of costs and benefits. Like every other human activity, the production of knowledge is subject to economic constraints.[3] It should therefore prove fruitful to employ economics as an analytical tool in philosophy of science.

Of course, there are other approaches by social scientists to understand science. Most important are the sociology of science and the sociology of scientific knowledge.[4] One important difference between the sociological and the economic approach to science seems to be that sociologists mostly emphasize the epistemic deficiencies of scientists and point to inadequate scientific institutions.[5] While this approach is, of course, completely legitimate, economics can view science from a different, and equally legitimate angle: Economics could help reconstitute science as social AND at the same time as epistemically privileged, and thereby show that science can, after all, deliver relatively reliable knowledge despite all its sociological inadequacies.[6]

The first general idea is that economists can reconstruct social institutions as rational solutions to problems of social interaction. The classic example is Hobbes' argument for the social contract: A social contract to form a state primarily solves the problem of how rational actors can rely on each other's actions. Such a contract creates rules and institutions and ways of enforcing these rules. If enforcement is sufficiently guaranteed, then actors can calculate the other's reactions to their own moves. Without a social contract – in an anarchic state – the enforcement of rules would not be guaranteed. There would be incentives to steal, to plunder, to kill – in general, to exploit others. In the long run, nobody would profit from such a situation. Therefore it is rational for all to consent to a social contract which is profitable for everyone. Economics has reconstructed many institutions in this way.[7] So it seems 'rational' to try to use economic tools on science as well.[8] There is no reason why scientific institutions should not have evolved in the same way, as rational solutions to problems of social interaction.

The second general idea is that economics deals with many problems in which each actor serves his own interest while at the same time serving collective rationality. This could be used in the analysis of science as well. So while sociologists of science often talk about self-interested scientists, they do not necessarily allow for the possibility that despite this individual self-interest, the overall outcome might be rational. Economics can provide arguments for the rationality of science despite – and even *due* to – the social character of science.[9] I am going to deal with this idea – which has been used by Philip Kitcher and others – in Section 4. But first, I want to give a more detailed account of the kind of economic framework I am going to use.

3. THE BUCHANAN RESEARCH PROGRAM IN ECONOMICS

The approaches to an economic philosophy of science by Kitcher, Rescher and others all make use of methods and results from economics, albeit in different ways. The question arises whether a systematic basis for an economic philosophy of science can be found.

I would like to explore the consequences arising from the application of a certain view of economics to the philosophy of science. I call this view the *Buchanan research program*, which is based on James M. Buchanan's writings on "constitutional economics" (Buchanan 1975; Brennan and Buchanan 1985). This approach is directed against the central ideas of welfare economics, especially against the existence of a social welfare function.[10] Constitutional economics aims at a fundamental change in economic theory along the following points:

1. Economics is not the science of market processes and does not deal with material goods only. Rather, economics is the science of costs and benefits which provides us with a universal theory of human behavior. This means that new elements can be integrated into utility functions, such as psychological or intellectual costs.
2. Economics deals primarily with interactions, especially with the analysis of dilemma situations (such as the prisoners' dilemma), and not with individual decision-making. It is interesting to see that most economic approaches in philosophy of science focus on the 'lone decision maker'. This view, though, cannot capture the consequences of one characteristic problem of interactions: strategic interdependence. This problem is illustrated by the prisoners' dilemma, which will be dealt with in Section 4.

3. The purpose of economics according to the Buchanan research program is not just a theoretical, but a practical one: the design of institutions (e.g., a market order) as the framework for individual actors. It is only for this goal that individuals – including scientists – should be modelled as self-interested actors. *Homo economicus* is only to be taken as a construction for the purpose of institutional design.[11] There are, of course, other models of man that might be better suited for other purposes. In this way, critics calling for more realism in economic models can be answered.[12]
4. There are no external normative criteria. Instead, normativity has to be grounded in consensus. In particular, normativity can be based on a hierarchy of levels of consensus. This idea will be elaborated later on.

This program opens up an interactionist perspective on philosophy of science. I will spend the rest of this section outlining the basic ideas of this perspective before discussing its application to philosophy of science in the next section.

Many traditional textbooks in economics start with the problems of Robinson Crusoe on a deserted island. However, the Buchanan research program stresses that economics deals primarily with interactions, with problems that arise in the social relations of human beings. Game theory provides the main tools for this task.

This does not mean that there are no economic problems for an isolated actor like Robinson Crusoe. Certainly he has to cope with scarce resources. But these problems are problems of *action theory*. Action theory models individual actions and decisions using the analytical tool *homo economicus*.[13]

But there are no conflicts of interests and no problems of interdependence of actions on Robinson Crusoe's island. These questions only become crucial in multiple agent economies and other social subsystems (like scientific communities), social systems that can even be *defined* by this interdependence. Therefore, the economic tools for Crusoe's world are inadequate for the economic problems of social systems.

Thus, action theory has to be supplemented by interaction theory. This means that models of interactions are based on action-theoretical models: In order to explain social phenomena, first the actors' situation has to be analyzed (step 1). This leads to situational constraints. Second, the individual decisions and actions of the actors have to be modelled (step 2). Finally, these individual decisions are combined in game theoretical matrices (step 3). Step 2 is part of action theory, step 3 belongs to interaction theory. Coleman (1990, 10) has illustrated this method as follows:

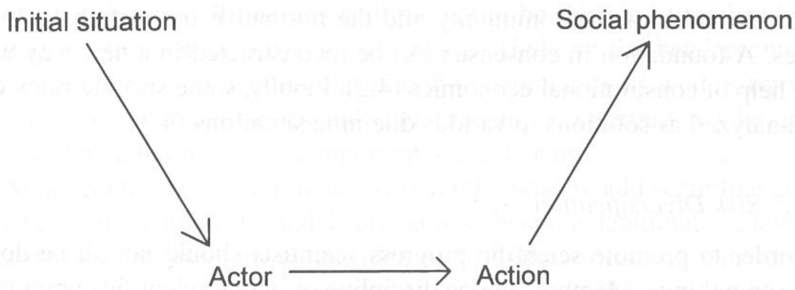

Figure 1. Coleman's 'bathtub'.

According to Brennan and Buchanan (1985, 3ff.) and Tullock (1985, 1079), a vast number of interactions can in principle be modelled as prisoners' dilemma situations, one of the most important types of models in economic interaction theory.[14] In this way, it is possible to represent many potential conflicts between individuals and point to solutions which improve the situation of every actor (Brennan and Buchanan 1985, 3).

Economic philosophy of science should also consist mainly in interaction theory based on action theory. On the level of action theory, the calculations of the individual scientists should be analyzed, in particular their decisions in matters of theory choice. On the level of interaction theory, these calculations can be combined in a matrix.

Thomas Kuhn seems to have been the first to have shown how 'external' social rules and 'internal' rules of science cooperate. The latter consist in the scientific values like internal and external consistency that – according to Kuhn – guarantee scientific progress even during and after a paradigm shift (Hoyningen-Huene 1990, 490). But social rules also play an important role, especially in the professional training of scientists. Without any standards upheld by examinations and the canonical literature, it would not be possible to work on the details of a research program. But this work is necessary, because it eventually leads to revealing anomalies and inconsistencies.[15]

4. PLURALISM IN SCIENCE

In this section, I focus on problems within the philosophy of science which concern interactions between scientists and which can be approached using tools from economics. First, I deal with risk diversification as a means to increase a scientific community's, or an entire society's, chances to accelerate scientific progress (4.1). Second, the necessary prerequisites for a working mechanism of risk diversification will be examined, which comprise the rules of the community and the normative foundation for such rules. A foundation in consensus can be reconstructed in a new way with the help of constitutional economics (4.2). Finally, some specific rules will be analyzed as solutions to various dilemma situations (4.3).

4.1. *Risk Diversification*

In order to promote scientific progress scientists should not all be doing the same things. Members of one discipline or of one scientific community should not all be working on the same theory. There are several arguments in favor of this "division of cognitive labor" (cf. Kitcher 1990):

First, theory choices are often highly risky, as they must be made in very early stages when information is scarce. The researchers within a scientific community are all in the same situation as a financial investor trying to invest money in different companies. As long as the investor does not have any secret information, he or she does not know which enterprises will perform best. The optimal strategy is to invest in different companies and not put all of the eggs in one basket. Thus, the investor composes a portfolio of assets. Likewise, every member of the scientific community should put together a portfolio of research projects. But not every scientist will choose the same portfolio.[16] This results in a balanced array of projects: Without any decisions made centrally, the scientific community hedges its bets.

Second, most decisions are found to be sub-optimal later on. The vast majority of our theories turn out to be wrong. If we all chose theory A – say, as A had fewer anomalies than B and C that were available at the same time, and finally arrived at falsifying A – it would take a lot of time to go back and explore the potential of B and C. We would have to invest again in working out the consequences of B and C, in inventing empirical tests, or even in simply understanding them. So if all members of a group make the same decision, no one is going to work on defeated alternatives which might eventually overcome their anomalies. It is much more efficient to have different researchers work simultaneously on different theories.

Third, no one is going to look for new alternatives to the accepted theory. Pluralism and competition in science avoid a 'dogmatic slumber' of theoretical monism, since there exist incentives for individuals to come up with new theories of their own. Scientific pioneers can be highly successful in receiving priority, e.g., they can look forward to eventually having their name attached to a particular effect or theory they came up with. In this way, they serve their own self-interest while at the same time preventing theoretical stagnation within the scientific community.

To sum up, the idea of risk diversification in science implies that it is efficient for several individuals or research groups to pursue different, competing theories. If one theory turns out to be false, no time has been wasted, on the overall progress of science. 'Hedging the bets' promotes scientific progress for a particular scientific community as well as for science in general. Of course, the individual researcher or group may be stuck in a dead end, but there is an important social benefit.

Many controversies in science, which otherwise would seem like quarrels between rationalists and irrationalists, become legitimate scientific controversies in the light of the idea of risk diversification. This idea – which has been used as an argument for pluralism of theories and against theoretical monism[17] – is a necessary prerequisite for rationally managing conflicts in any scientific community.

Feyerabend was well-known as a philosopher who was particularly eager to emphasize theoretical pluralism. He went beyond most other philosophers of science in calling any commitment to one method a way of restricting creativity. Such commitments – resulting in "ideology" (Feyerabend 1975, 307) and "totalitarian pretensions" (Feyerabend 1975, 308) – should not be used in education; they are just propaganda against which we should be warned. Pluralistic methodology and competition should rule in science. And Feyerabend leaves no doubt that in this competition of science all are admitted, "[e]xperts and laymen, professionals and dilettanti, truth-freaks and liars" (Feyerabend 1975, 30).

How can the idea of risk diversification be implemented at the level of the individual researcher or the individual group? The idea itself only states that there is *some* optimal mix of strategies for a community. Tools from game theory might be useful here: The argument for risk diversification could be reformulated and strengthened using the concept of 'portfolio'.[18] For a specific problem, it might be possible to even give quantitative advice for the assignment of resources to each individual strategy at least within a small group. In the case of theory choice, such advice, given to a research group, might look something like this: 50 per cent of its members should work on the new, promising program A, 30 per cent on the still successful program B, 10 per cent on the old program C, and 2 per cent should even hold on to the outdated programs D, E, F, G, and H, respectively. In order to put this proposal for a portfolio of research projects into practice, however, individual researchers would have to be given individual advice, and incentive-compatible schemes would need to be implemented to reinforce that advice.[19]

Philip Kitcher's work on the "Advancement of Science" (Kitcher 1993, cf. also Kitcher 1990) systematically links risk diversification to the self-interestedness of researchers.[20] Kitcher's main task is to defend philosophy of science against relativist sociologies of science. For this purpose, he employs, without explicitly using the term, the classic economic argument of the invisible hand. Basically, Kitcher's strategy consists in accepting the influence of social factors on scientific knowledge whilst at the same time maintaining that social aspects do not necessarily obstruct scientific progress, but might – under certain conditions – promote it. Kitcher employs formal models from economics to show that communities of "epistemically sullied" scientists can perform better than communities of "epistemically pure" ones:

> [T]he operation of social systems in ways that we might initially view as opposed to the growth of knowledge can be dependent on the use of complicated reasoning and can contribute to the community's attainment of its epistemic ends. (Kitcher 1993, 388)

This 'operation of social systems' is modelled in a way that is a standard procedure in economics: Given certain assumptions about (a) the actors' preferences, (b) the constraints of the situation and (c) the behavior of the agents according to the rationality principle, it is shown that there exists some equilibrium point to which the interaction process converges. This does not mean that this point will necessarily be reached in reality, but it is a *possibility*. As in Adam Smith's classical argument, Kitcher shows that the positive consequences of distribution of labor can be found in science as well (Kitcher 1990). Under specific constraints scientific progress is fostered, not hindered by self-interest.

It is these models that have been the target of a lot of critics (Hands 1995, 1996, 1997; Mirowski 1996). In my view, these criticisms are not lethal and can be answered. The invisible hand argument has to be embedded in a methodologically sound conception of economics that is apt to counter several standard objections against economics. The *Buchanan research program* seems to be designed for this task. I will show how this program can be applied in the following sections.

Brock and Durlauf (1999) have developed Kitcher's argument further. They show that external factors do not necessarily have consequences for scientific progress. These factors do not obstruct progress in case the empirical evidence for both theories differs sufficiently.[21] Brock and Durlauf also show that there are conditions under which external factors can obstruct the acceptance of a theory that is superior according to internal considerations. However, under relatively weak conditions, external factors can accelerate and strengthen the emergence of a consensus in a scientific community.

But neither Kitcher nor Brock and Durlauf consider making *active*, positive use of self-interest. There could be two ways to achieve this: to give direct orders or to set incentives. As in a planned economy, direct orders are inefficient and not feasible. But it is possible to set incentives in order to indirectly promote the desired allocation of resources on different research programs (like A to H in my example). Specific institutional frameworks could make productive use of scientists' different starting positions (e.g., in resources or career expectations) in order to achieve the desired distribution of risk. So it could make sense to set incentives for unemployed scientists that enable them to work on more exotic, rather neglected projects like C to H (in my example). But in any case, advice from game theory can only be implemented *after* analyzing in detail all relevant incentives, strategic interdependences and dilemma situations. Examples will be given in Section 4.3. If incentives are given or fixed, advice to individual scientists is like a management-consultant's advice to an enterprise: Find out which theory is – under the given constraints – optimal for you in the long run!

My observations concerning risk diversification can be summed up as follows: Communities benefit from risk diversification. However, it is the individual who decides. These decisions cannot be forced, but may be influenced by setting incentives.

4.2. *Consensus Practices*

An interactionist philosophy of science can also shed a new light on the normative foundation for methodogical rules. This is one of the current key problems in naturalized philosophy of science that has been identified both in Kitcher's and Larry Laudan's approaches. Both authors ground methodological criteria in some form of consensus:

Kitcher has been criticized for uncritically speaking of "consensus practices" in science (Hands 1995). Laudan (1987, 1990, 1996)[22] wants to reconstruct "our goals" in order to gain a criterion for evaluating the progress of research traditions. But in what sense can common practices or goals be identified? This is especially a problem for an economic approach as economics is usually rooted in methodological individualism.

The Buchanan research program of constitutional economics can be of use here. Buchanan takes the consensus of all citizens to be the only adequate normative basis for the political rules governing modern societies. As I have outlined in Section 3, if a solution for a political or economic dilemma is proposed, it must be possible to reconstruct this solution as being legitimated by consensus. However, trying to come to a consensus concerning every single problem would be too costly. Therefore, it is only required that the most abstract principles of a constitution are consented to. These principles provide rules for the process of deciding problems of lower levels. So there is a hierarchy of different levels of consensus (like: simple majority, two third majority, unanimity). For example, a society will usually not come to a consensus on detailed tax rules. But *constitutional* constraints on the fiscal process can more easily be agreed on, mainly because it is much more uncertain how each individual will be affected by these constraints (Buchanan 1975, 47).

This reconstruction can be applied to science as well.[23] As a subsystem of society, science is governed by rules legitimated by a hierarchy of more abstract rules and ultimately by consensus. On the lower levels of this hierarchy, unanimous consent to rules is not required. This applies to external rules (such as laws governing universities) as well as to internal rules (such as the classical criteria of philosophy of science like internal consistency etc.).

Moreover, on these lower levels, decision rules in science differ significantly from those in other social subsystems. Though *external* rules are subject to procedures of administration and legislation in the same way as traffic rules, internal rules are not subject to majority votes. Instead, internal rules evolve in the process of science, often with only a small fraction of a scientific community's members explicitly participating in this process. A realistic consensus model for scientific normativity has to take into account that a small elite takes the lead in methodological questions. The other members consent to these decisions only *ex post*.

Kitcher (1993, 217) and Rudwick (1985, 420ff.) hold this view. Kitcher builds on Rudwick's analysis of the Devonian Controversy. In this case, it is just ten leading geologists deciding the adoption of the new Devonian system for their entire discipline. Rudwick clearly shows that all less prominent geologists accepted the leading role of the elite. In 'exchange', these minor geologists – rationally – claimed to be regarded as competent in details of local geology. This serves as an example for the hierarchy of levels of consensus in science.

On the basis of this economic reconstruction of scientific normativity, Kitcher's term "consensus practices" and Laudan's term "our goals" can both be given a new meaning. According to Hands (1996, 145), legitimate collective goals cannot easily be grounded in a consistent naturalistic conception. I disagree. There is a naturalistic criterion of normativity: consensus, more precisely a consensus of several different levels. Drawing the analogy to Buchanan's work: The most abstract principle that the members of a society consent to is the existence of any state instead of no state. The anarchy of the "Hobbesian jungle" is always the worst case for anyone (this principle is illustrated by the prisoners' dilemma; cf. Buchanan (1975, ch. 1)).

This idea can be applied to science. There should be a very broad consensus that an anarchic situation in science is unacceptable. At least a minimum of universally accepted rules, which might even be selected randomly, is needed. In an economic perspective, this solution is still superior to one in which everyone observes only his own rules and in which no one is 'forced' (e.g., in academic training) to adopt some common rules.[24] Economics points to the fact that in a world of limited resources, only a few theories will be able to gain enough resources for systematic scientific investigation. An anarchic situation leads to an undesirable waste of resources, while pluralism allows for a dominant and several dissenting, alternative theories.[25] Feyerabend may have made it plausible that scientific progress can be obstructed by the existence of too many rules. A certain degree of violation of rules may even promote progress. But from an economics perspective, too few rules can also obstruct progress. Methodological anarchism does not necessarily lead to the desired consequences:

> And unless some modicum of agreement is enforced, even those areas within which anarchy might indeed suffice to generate tolerable order would be subject to gross violations. [...] [A]narchy works only to the extent that limits among persons are either implicitly accepted by all or are imposed and enforced by some authority. (Buchanan 1975, 8f.)

So in the case of science either scientists implicitly, perhaps due to a common ideology, accept certain rules, or the (seemingly) anarchic situation leads to the uncritical (and dogmatic) adoption of certain rules.

So far I have only argued for a broad consensus on the most abstract principles of science. Taking the analogy to the political area further, a hierarchy of descending levels of consensus could be reconstructed: First, the members of a society consent to leaving methodological decisions to scientists themselves (external rules, however, are still subject to political decision-making). Second, scientists consent to leaving these methodological decisions to a small elite. This has been convincingly shown in Rudwick's case study, which will be examined in the next section.

To sum up: Good science is characterized by processes that are legitimated by a hierarchy of levels of consensus.

This consensus model presents an answer to Hands' (1995, 616) problem of how to get from individual to social goals. Trying to find a social welfare function is futile.[26] Instead, a consensus of individuals should be reconstructed which does not extend, however, to common goals but to common rules or decision mechanisms. Kitcher's "consensus practices" can thus be reconstructed in an economic way: Consensus practices are grounded in a consensus of all people involved. However, these individuals consent only to some very abstract rules which guide the decisions concerning rules of lower levels and eventually particular consensus practices.

It is thus possible in an economic philosophy of science to talk about 'the social' without contradicting oneself. It is, however, not yet clear whether this equally answers Mirowski's (1996) criticism:

> Kitcher responds to this challenge [i.e., the thesis that success is culturally relative] by essentially denying that goals can be incommensurate once one takes a broader view and translates the goals of one culture into another. In other words, 'success' of science is a human universal, and can be demonstrated as such once one finds the right language in which to express universally held goals. This dream of a generic aggregate 'welfare function' is not novel; indeed, its Esperanto is mathematics; and it was the dream of the nineteenth century British utilitarians and their twentieth century cousins, the neoclassical economists (Mirowski 1996, 160).[27]

Mirowski hereby turns the welfare criticism against Kitcher himself. He compares Kitcher's thesis of non-relativist scientific goals to the welfare economists' search for a social welfare function. According to Mirowski, both dreams are futile.

This criticism cannot be immediately rebutted in the framework of the consensus model. Only detailed analysis would reveal whether scientists of *different cultures* consent to the same basic methodological principles. The corresponding problem in economics is the problem of how to reconstruct a social contract on a transnational level. Economists have argued that this is possible (Buchanan 1990). Some of their arguments could be used in economic philosophy of science as well. However, this opens up a new discussion and cannot be pursued further here.

In general, reconstructions of different levels of consensus seem very useful and should be extended to other areas of philosophy of science. They can be interpreted as compatible with the normative basis of the program I have sketched: There is a hierarchy of different levels of consensus among the members of a scientific community (in this case, the philosophers of science).

4.3. *Dilemma Situations in Science: The Case of the Devonian Controversy*

I am now going to use an example from history of science to illustrate the method of economic philosophy of science. I will show how dilemma situations can be reconstructed in one particular historical episode and what institutions have been developed to overcome these situations.

Martin Rudwick's "Great Devonian Controversy" (Rudwick 1985) is the perfect example, as it provides us with a very detailed narrative and gives deep insights into the factors influencing the actors' theoretical decisions. The Devonian Controversy took place during the 1830s and 1840s,

Carboniferous	Coal Measures
	Mountain Limestone
	Old Red Sandstone
Greywacke (replaced by Silurian and Cambrian in ca. 1835)	Upper Greywacke
	Lower Greywacke

Figure 2. Geological subdivisions of the Paleozoic-equivalent era in ca. 1834.

when the "Devonian" was accepted as a new geological period. The main storyline goes like this:

In 1834 the standard geological model of what is today the Paleozoic era was comprised of only two subdivisions, the Carboniferous and the Greywacke (Rudwick 1985, 402f.). The Carboniferous consisted of three subdivisions, the Coal Measures, the Mountain Limestone, and the Old Red Sandstone. The Greywacke had only two subdivisions, Upper and Lower Greywacke:

If one of these strata was missing in a local area, there had to be an unconformity instead. If no unconformity was observed, this meant that strata belonged to the same geological period. The Devonian Controversy started in 1834 when the traditional model (Figure 2) was confronted with an anomaly: In Devon, the geologist Henry De la Beche discovered some unusual fossils within strata he thought to be Greywacke due to their general structure. But experts identified the fossils as of the Coal Measures Period. Now two different criteria of the age of strata were in conflict, the structure criterion and the fossil criterion. Two different conclusions were now drawn by different members of the scientific community of geologists:

1. De la Beche himself and others saw no possibility to rank the strata in Devon as Coal Measures because (a) there was no unconformity between them and the lower strata, and (b) the Old Red Sandstone was missing, which was very characteristic due to its red colour and which was – at least in Great Britain – always found as the end of the Carboniferous Period. Therefore, De la Beche concluded, the strata he had discovered had to belong to the Greywacke. Consequently, he regarded the fossil criterium of rock age as unreliable.
2. Other geologists like Roderick Murchison and Charles Lyell drew a different conclusion. They were convinced that the fossil criterion could not be wrong and that De la Beche's strata had to be Coal Measures. Murchison's and Lyell's problem, however, was the missing unconformity which would have had to be present between the Coal Measures and the Greywacke, as the expected Old Red Sandstone was missing. Despite extended searches, this unconformity could never be found.

This problem could eventually be resolved by introducing a completely new geological period, the Devonian. Murchison was the first to systematically describe the Devonian as comprising parts of the strata where De la Beche found the fossils but also the Old Red Sandstone from other parts of the country. The decisive piece of evidence in favor of this solution was found in Russia in 1840: fossils similar to De la Beche's from Devon were discovered together with others characteristic of the Old Red Sandstone. Soon, the controversy was over, as the elite at first and the vast majority of the geologists shortly afterwards adopted the new system.

In this new system, the Old Red Sandstone was separated from the Carboniferous Period and – together with other strata like those found by De la Beche – extended to a separate system, the Devonian Period. The geological model of systems at the end of the Devonian Controversy – disregarding some minor modifications made later – was very similar to the present model which can be seen in Figure 3.

In view of the economic perspective on philosophy of science, what can we learn from the Devonian Controversy? There are several crucial situations in this story during which the interaction of the geologists is important for the resolution of scientific problems:

(1) As I have already mentioned, the Devonian Controversy might serve as an example for the application of Buchanan's consensus model to science, in which good science is characterized by processes that are legitimated by a hierarchy of levels of consensus. Here, the members of the (British) society consented to leaving methodological decisions to the geologists scientists themselves (though external rules of funding, e.g.,

Permian (introduced by Murchison in 1841, cf. Rudwick 1985, 379)
Carboniferous
Devonian
Silurian
Ordovician (introduced by Charles Lapworth in 1879, cf. Duff/Smith 1992, 63)
Cambrian

Figure 3. The geological model of the Paleozoic era today.

were still subject to political decision-making). Second, all (important and non-important) geologists consented to leaving methodological decisions to a small elite of only about ten people. Their authority in these questions was not in doubt during the controversy (Rudwick 1985, 420ff.).

(2) Rudwick's main protagonists compete fiercely over scientific reputation, government jobs (De la Beche), knighthood (Murchison) and financial rewards (De la Beche, cf. Rudwick 1985, 103ff.). But it is this interaction of competing individuals that leads to a solution: As Rudwick (1985, 420ff.) continues to stress, in the beginning none of the protagonists advocated or even considered the finally successful interpretation of the Devon. Instead, this interpretation comprises ideas of nearly all scientists involved:

Murchison and Sedgwick were the first to find the correct model of the Devon strata and their correct age (ibid. 1985, 160ff.). But they had to give up the unconformity they had postulated at first (ibid., 190f., 276f.). Moreover, another part of the Devon strata belonged to neither the Carboniferous nor the Greywacke. This had instead been recognized by some of their rivals, the geologists Robert Austen (ibid., 237f.) and William Buckland (ibid., 168). But Austen did not accept the term "Devon" (ibid., 317f.), and Buckland did not work out his proposal further. De la Beche was also partly right, as he had always been opposed to the existence of an unconformity in Devon (ibid., 103f., 166f., 180). But he was wrong to regard the strata in Devon as Greywacke (ibid., 93ff., 240, 267f., 276f.).

I will analyze this structure of interaction as the *priority dilemma* – with positive consequences for scientific progress – in Section 4.3.1.

(3) In 1837, De la Beche announces that he is going to give a paper at the meeting of the Geological Society on his interpretation of the strata in Devon. For this meeting, Murchison and his colleague Adam Sedgwick have already prepared a paper on the same subject (Rudwick 1985, 183–185). Murchison therefore writes to Sedgwick, "that we ought to place our *whole* view before the public, ere any of the *pirates* can rob us of our bark" (ibid., 184; his italics).

This case settles down for some time. But in 1839, De la Beche publishes a report which in Murchison's view makes use of his – Murchison's – own results without stating their source. Sedgwick can barely stop his colleague calling De la Beche a plagiarizer in public. I am going to deal with this problem situation as the *property rights dilemma* in Section 4.3.2.

(4) In 1837, De la Beche refuses to grant his rival Murchison access to some of the fossils he found in Devon. When Murchison threatens to make this public at the next meeting of the Geological Society, De la Beche gives in. He opens up his archive of fossils, and anxiously asks George Greenough, his mentor, to defend him at the meeting (Rudwick 1985, 211f.). It looks as if De la Beche at first saw an opportunity to hinder his rival.

The three types of dilemma situations in the Devonian Controversy – the priority dilemma, the property rights dilemma in science, and the case of free access to objects of study[28] – will now be dealt with in turn.[29]

4.4. *The Priority Dilemma*

The institutional structure of science sets certain incentives for scientists. Among these are jobs (professorships), money, and scientific reputation. These incentives force scientists into a *priority race* or *priority dilemma* that is similar to the situation of firms on ordinary markets. In this winner-take-all-situation competing research teams invest resources in solutions for the same problem (Dasgupta and David 1994). These teams would all be better off if either they worked on different problems or if they were not forced to be the first to find the solution, but (for example) only independently from other teams.

But science's structure of incentives leads to this priority race which is eventually superior to non-competitive solutions in providing society with the public good knowledge. This point has been made clear by von Hayek (1978) in his analysis of the benefits of competition:

1. Competition fosters innovation. This also applies to the priority dilemma.
2. The successful pioneer forces others to imitate the successful strategy. In the case of the priority dilemma, this means that though imitators do not benefit directly (e.g., in terms of reputation) from adopting the victorious theory, they cannot ignore this theory and must build on it in order to continue doing research in their field.

The Devonian Controversy illustrates the priority dilemma well, as I have outlined above. There is intense competition between the main actors. This competition, however, *promotes* scientific progress, as the solution of the controversy could not have been found by a single actor, but only by several people, not working together, but competing.

This dilemma shows that the priority race has positive as well as negative consequences. Reconstructing a situation as a dilemma does not automatically mean that this situation is undesirable and should be overcome.[30] Other arguments, e.g., the consensus of a scientific community's members, are required to give normative significance to a dilemma situation.

4.5. *The Property Rights Dilemma in Science*

The priority race does not only have positive, but also negative consequences: Scientists may be induced to steal from each other. This problem concerns the enforcement of property rights in science and can be reconstructed as a prisoners' dilemma:[31]

Suppose there are two actors X and Y, with utility functions comprising two arguments, direct utility and indirect utility[32] (note that, according to constitutional economics, utility comprises all advantages an actor can gain, like money, fame, status, power, and so on[33]). So:

$$U_{\text{total}} = f(U_{\text{direct}}, U_{\text{indirect due to security of property rights}}).^{34}$$

Also assume that X and Y can either work on their own or plagiarize. Their pay-off-matrix might look like this:

For both X and Y it is better to work on their own than to plagiarize. In this case, they gain indirect utility (U_{indirect}) due to the security of their property rights within their scientific community. At the same time, however, due to the incentives, both will commit theft, as the direct utility (U_{direct}) of stealing may be very high and may reduce the cost of one's own research significantly.

One single case of theft does not severely disturb the atmosphere in a scientific community. But if X commits theft, then Y is better off to steal as well in order to reduce his own costs and thus to compensate X's

X \ Y	work on his own	plagiarize
work on his own	I B , B	II D , A
plagiarize	III A , D	IV C , C

where A > B > C > D

Figure 4. The dilemma of property rights in science.

advantage. So both end up in the "social trap" of quadrant IV, although the situation in quadrant I would benefit them both. The reason for this is that they are afraid of being 'exploited' by free riders (quadrant II and III), or, in Murchison's quite similar terminology, of being 'robbed by pirates'.

There are several alternative institutions that help to overcome this trap.[35] The first one is the scientific public. If stealing is made public, this might deter possible thieves. In the Devonian example, when Murchison sees his property rights in danger, he threatens the president of the Geological Society, "if you do not interpolate our names, my honest opinion is that you will do yourself a disservice & be sorry for it hereafter" (Rudwick 1985, 346). In other words, Murchison threatens to make the controversy public and thereby damage the reputation of geology. In his day, this solution worked.[36] In later times, more formal institutions are required. This includes, for example, patents and specific mechanisms of submitting papers to scientific journals.

		Y	
		grant access	deny access
X	grant access	I B , B	II D , A
	deny access	III A , D	IV C , C

where A > B > C > D

Figure 5. The dilemma of free access to objects of research.

4.6. *Free Access to Objects of Research*

Knowledge is often characterized as a public good. If this were correct, then nobody could be excluded from the benefits of this good. But it is evident that many objects and data of research are controlled – initially, at least – by one or several researchers. In the long run, this practice is not tolerated, as it obstructs the possibility of independently testing these results. But in the short run, the situation can be characterized as a prisoners' dilemma.

Again suppose there are two actors X and Y. Their utility functions are given by:

$$U_{\text{total}} = f(U_{\text{direct}}, U_{\text{indirect due to free access to objects of research}}).$$

Assume further that X and Y can either grant access to their own work or deny access. Their pay-off-matrix might look like this:

For both X and Y it is better to grant access than to deny it. In the granting case, they are both gaining indirect utility (U_{indirect}) from free access. However, due to the incentives, both will deny access, as they may gain a very high direct (individual) utility (U_{direct}) from this act. But if X denies access, then it is better for Y to deny access as well in order to compensate X's advantage. Quadrant IV therefore represents a "social trap".

During the Devonian Controversy, Murchison's threat – to make De la Beche's reluctance to grant access to the fossils public – suffices. De la Beche sees his scientific reputation in danger and asks Greenough for support.

The institutionally organized scientific public can enforce rules like "grant free access to your objects of research". Learned societies were founded exactly for this purpose. One of their aims has always been to make results better known to the public. Evidence can be gathered, e.g., regarding the foundation of the Mathematical Society (Mathematische Gesellschaft) in Hamburg in 1690. This society was to promote the communication of results of research, according to Wettengel:

> Science was to be promoted by publishing mathematical writings. In the 17th and 18th century, this was still relatively difficult and expensive. It seems that the intention to publish has therefore been the main reason for the founding of the [Mathematical] society. (Wettengel 1990, 71; my translation)[37]

The "Deutsche Akademie der Naturforscher Leopoldina" in Halle was also founded for this purpose, according to Uschmann (1989). In 1651, one of the society's founders, Johann Laurentius Bausch, sent an invitation for the founding of an academy. According to this invitation, "the study of the manifold nature cannot – due to the short lifespan – be done by a single individual, but only through the cooperation of several people working together in a comradely manner" (Uschmann 1989, 22; my translation).[38]

And in 1670, the editor of the first scientific journal of this academy wrote that he "intended to promote a quick publishing of new discoveries so that doctors could make their results and their observations known" (Uschmann 1989, 27; my translation).[39]

The rigid structures of universities also seem to have obstructed the publishing of results and thereby scientific progress. The scientists involved in the foundation of these societies had recognized this and understood that only *institutional* changes could improve this situation.

5. CONCLUSION: A POSSIBLE LINE OF RESPONSE TO CRITICISMS OF ECONOMIC PHILOSOPHY OF SCIENCE

Economic philosophy of science, as outlined here, is a part of the general naturalistic research program. Just like philosophy of science has made use of methods from cognitive science and biology, it can gain new problems and a new perspective on old problems from economics. The interpretation of economics provided by the Buchanan research program makes this co-operation especially fruitful. From this view, the following consequences for philosophy of science can be drawn:

– Economic philosophy of science cannot be reduced to simply requiring parsimony in theories. Rather, the economic approach requires us to pay attention to *all* cost factors in theory choice. The criticisms raised by Hands and others seem to me due to their more traditional view of economics, one which would only be relevant for the external and not (also) for the internal factors in science.
– Instead of focusing on maximization of utility, economic philosophy of science builds on different levels of consensus as a normative foundation. Most criticisms against economic philosophy of science (cf. Hands 1995) can be answered in this way.
– Interaction theory poses new tasks for philosophy of science, in particular institutional design. This is the main part of an economic philosophy of science and should be explored further. For example, Kitcher's invisible hand argument against the sociology of scientific knowledge could be worked out in more detail by looking at particular dilemma situations and institutions which shape science.
– Finally it should be emphasized that this paper is not (mainly) a contribution to sociology or history of science, but to *philosophy* of science. The analysis presented here is, and is tended to be, relevant for the *normative* aspect of science. History and sociology of science provide the empirical basis which the philosopher of science can use for economic reconstructions, but they are not the whole story.

Thus it seems to me that Thomas Carlyle's 'dismal science' might have something to contribute to philosophy of science as well as to the domains of political science, sociology, and other disciplines which the economic approach has been recently applied to.

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NOTES

[1] Cf. recently (Hands 2001).

[2] The tradition of using economics for philosophical questions goes back well beyond the last decades. Ernst Mach's principle of the "economy of thought" is one of the best known examples of this tradition. Another ancestor – in (at least) two different ways – is C. S. Peirce. First, Peirce uses cost-benefit analysis in his work on economy of research (Peirce 1879, 1958). Second, there is an economic element in the mechanisms of research he sees as a guarantee for reaching truth in the infinite: As Rescher (1978a, 15) and Haskell (1984, 211) have pointed out, it is the *competition* of scientists that ensures the optimal result for science. Just like competition on ordinary markets makes use of the selfish motives of individuals for the betterment of all, competition in science generates true theories in spite of every single researcher pursuing his own interests.

This argument is not stated explicitly by Peirce, but has been employed by others. In 1962, Michael Polanyi examined the functioning of the "republic of science" (Polanyi 1962) and argued that this 'republic' works best without government regulation. More recently, this argument has become prominent in the work of Kitcher, which will be dealt with later, and of Goldman (Goldman and Shaked 1991). Goldman's approach has been criticized from an economic viewpoint by Sent (1997).

[3] This point has been stressed especially by Rescher (1989, 1996). According to Rescher, the economic approach can help to clarify the problem of induction, Hempel's raven paradox, Goodman's grue paradox, or Popper's problem of "generality preference" (Rescher 1989, ch. 6). Moreover, on a more abstract level, Rescher sees economic limitations to scientific progress: Due to the rising cost of required technology, scientific knowledge becomes more and more expensive. Its growth undergoes a "logarithmic retardation", eventually slowing down to "*the logarithm of a linear transform of the elapsed timespan*" (Rescher 1978b, 113; his italics). It follows that the "extent of our scientific knowledge is inexorably limited – not by imperfect intelligence but by the economic realities of the scientific enterprise" (Rescher 1996, 113). This thesis of cost escalation in science and the principle of diminishing returns is probably Rescher's most known contribution to an economic philosophy of science.

[4] For the difference, cf. Hands (1994b).

[5] Cf. Barnes (1974) or Pickering (1984). A more general difference is that economics of science seeks to give advice in the shape of proposals for institutional reform, whereas sociology of science is mainly a *descriptive* enterprise. Thus, they have different problem situations.

[6] This point has already been made by Popper : "[...] objectivity is closely bound up with the *social aspect of scientific method* [...]. Scientific objectivity can be described as the inter-subjectivity of scientific method. But this social aspect of science is almost entirely neglected by those who call themselves sociologists of knowledge" (Popper 1957, vol. 2, 217; his italics).

[7] Cf. for example Furubotn and Richter (1992).

[8] I acknowledge that there is as yet no unified treatment of knowledge in economics. There are other economic approaches to the problem of knowledge, e.g., in evolutionary economics (Hodgson 1993). And, of course, social and economic analyses of science have to be careful in view of possible pitfalls of reflexivity (Collins and Yearley 1992; Wible 1998).

[9] Of course, economics cannot settle *all* questions regarding the epistemic uniqueness of science. But it can rebut *one specific argument* directed against this epistemic uniqueness, namely the sociological argument (scientists are self-interested, not disinterested, ergo science is not rational), by stating that, depending on the institutional framework, the 'outcome' of a system *may* be rational despite all its members being 'irrational'. Consequently, a social system like voodoo practice (assuming that it exists as a system) probably will not deliver rational outcomes, as its institutional system is inadequate for this purpose. So I believe that economics can ultimately trace back the epistemic privilege of science to its institutional framework.

[10] A social welfare function would comprise all individual preferences within a society into a single function. According to this approach, the task of the economist is to find this function and ways to maximize it. The task of the politician is to carry out the maximization.

[11] Thus, Buchanan employs the standard economic model of rational optimizing behavior. He only departs from standard neoclassical economics with respect to the interpretation of this model: Buchanan insists that *homo economicus* is not a model of man 'as he really is', but only a tool made for certain purposes, namely *institutional design*. Neoclassical economics, according to Buchanan, has lost this purpose out of sight.

[12] In particular, this point concerns the status of assumptions (like, e.g., common knowledge) in economics. Quite generally, the status of assumptions depends on the problem that the approach in question tries to solve. In the case of the economic approach, this problem is the design and reform of institutions. Consequently, the assumptions of economic models are not primarily required to be empirically adequate or intuitive, but rather to be fruitful in the development of proposals for institutional reform.

[13] The connection between action theory and the economics of science has been made clear within pragmatism for a long time. Cf. Rescher (1978a, 1989). Rescher, e.g., is interested in induction as "optimal *systematization* of experience" (Rescher 1989, ch. 5) or in a preference for generality (Rescher 1989, part of ch. 6). His questions are: Should a particular scientist choose an inductive method, and, should she go for more general statements, respectively? By contrast, an *interactionist* approach would focus on the social situation of the scientist, for example, on the question whether there is a prisoners' dilemma and, if yes, how to solve it.

[14] There are, of course, other models, like the Chicken game, the Battle of the Sexes, or the principal-agent model. I refer to the Chicken game in footnote 36.

[15] This point has been made – employing different terminology – by Lakatos (1970) as well as by Kuhn (1970, 177f.). For a recent overview, cf. Hands (2001).

[16] Researchers may, e.g., choose different portfolios of projects due to differences in their amount of funding, in intellectual capacities, or in status within their scientific communities.

[17] According to some interpretations (Hoyningen-Huene 1993, 152), Kuhn seems to hold this view. Lakatos (1970, 155) also stresses that the existence of rival scientific theories is fruitful: "The sooner competition starts, the better for progress" (ibid.; originally in italics). The potential of a research program can only be realized if its initial anomalies do not result in abandoning this program immediately. This would be some sort of destructive "instant rationality" (ibid.).

[18] For similar arguments in evolutionary scenarios, cf. Binmore (1994, ch. 3).

[19] Basically, this implies that the scientific community as a whole hedges its bets if its research teams do not all choose the same research program.

[20] Of course, this applies only to approaches in philosophy of science. Economics and sociology of science have for a long time presupposed self-interestedness. Cf. for an overview of this literature Diamond (1988), Dasgupta and David (1994), Stephan (1996), and Wible (1998).

[21] This applies under some conditions which cannot be stated here in detail.

[22] For an economic critique, cf. Hands (1996).

[23] With a different focus, Hayek (1952) has also discussed how science is (and should be) governed by rules.

[24] Of course, there are extremist positions that are often taken to imply that the existence of science is itself a fundamental evil. But I believe that most of these positions only fight *certain* forms of science, like the western one.

[25] A similar point has been made by Wible (1998).

[26] The *locus classicus* is Arrow (1951).

[27] The core of Mirowski's argument can already be found in Boland (1971).

[28] These examples are discussed in order: The third one may seem the most convincing.

[29] Kitcher also makes extensive use of the Devonian Controversy. However, he does rarely relate this historical example to his formal models. Kitcher (1993, 354, fn. 35) seems to be the only exception.

[30] A more formal reconstruction can be found in Pies (1996).

[31] A similar problem has been treated – differently – by Wible (1992).

[32] This is, of course, a simplifying assumption. For the problem at hand, I concentrate on direct and indirect utility, but a more 'realistic' reconstruction would have to take more factors into account.

[33] The payoffs in the following matrix represent payoffs to the individual researchers. I disregard possible payoffs to collective entities like the scientific community or the public at large.

[34] This is to say that the actors profit from a common atmosphere in which they all respect each other's property rights. This reduces everyone's need to invest in security mechanisms (such as secrecy).

[35] Overcoming the trap by way of (re-)constructing institutions means to change the payoff matrix by making the option of defecting more expensive.

[36] This solution may itself be reconstructed in game-theoretic terms: Murchison's threat changed the structure of the game from prisoners' dilemma to Chicken game, which has two Nash equilibria. Thus, the trap of mutual defection in the PD was eliminated in the first step. In the second step, the president of the Geological Society used his power to select between the two equilibria: He resolved the game in favor of Murchison.

[37] As their founding fathers put it: "[...] wann einige Glieder / so am nähesten beysammen wohnen / sich unter einander aufmunterten / bey vorfallenden etwan ledigen Stunden zusammen kämen / unter sich ein und andere Frage erörterten / und / wann einigen etwas mangelte / hierüber von andern besserwissenden sich Rahts erhohleten / und alles wol annehmen; [...]" (Wettengel 1990, 70).

[38] "[...] daß die Erforschung der so mannigfaltigen Natur einem einzelnen wegen der kurzen Lebensdauer des Menschen nicht möglich sei, sondern nur durch das Zusammenwirken mehrerer 'in gemeinsamer und kameradschaftlicher Weise' geleistet werden könne" (Uschmann 1989, 22).

[39] "In seiner *Epistola invitatoria* bemerkt der Herausgeber, dass es ihm auf eine schnelle Veröffentlichung neuer Entdeckungen ankomme, wobei die Ärzte die Möglichkeit hätten,

ihre Forschungsergebnisse und Beobachtungen in Form kurzer Mitteilungen bekanntzumachen" (Uschmann 1989, 27; his italics).

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