THE 'SCIENCE WARS' AND THE DUHEM-QUINE ARGUMENT OF UNDERDETERMINATION

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Abstract. The Duhem-Quine thesis (DQT) of underdetermination of theories by facts and the consequences of this thesis play a central role in the debates between philosophy of science and sociology of scientific knowledge (SSK). Traditionally, the DQT has been taken as an argument for relativist SSK. In this paper I will demonstrate that such an unconditional acceptance of the DQT may turn against the entire sociological programme, since the application of the underdetermination argument gives rise to an inadequate dichotomy – 'rational' vs. 'social'. The dichotomy involves the 'arationality assumption': it is only the arational, which requires sociological explanation. This idea, however, contradicts the main claims of SSK.

The analysis of the argument of underdetermination in these discussions leads to the issue of the 'science wars' – the radical version of the debates between philosophers and sociologists. Much of the 'science wars' controversies is due to mutual misunderstanding based on the dichotomy of rational *vs.* social.

1. Introduction

The so-called 'Sokal-affair' in 1996[1] gave rise to a new wave of debates between the traditional philosophy of science and the relatively new sociology of scientific knowledge (SSK)[2]. Due to the radical nature of the arguments presented these debates have become widely known as the 'science wars'. Often enough, these debates have been depicted as if the status of scientific knowledge were at stake there. Do the sciences discover new facts of reality and objective laws of nature, or should we regard scientific truths as purely social conventions? In

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[1] For a brief survey, see Hacking (1999:2–5).

[2] For an overview presented from a traditional philosophical point of view, see a recent collection of papers edited by Noretta Koertge (1998); and for an SSK view see, e.g. Bloor and Edge (2000), Collins (1999).

principle, this is a continuation of old philosophical debates between realism and anti-realism (or objectivism and relativism) in a new territory. SSK has been engaged in these debates since the 1970s when the first theoretical SSK programme – the strong programme – emerged. As the theoretical core of several SSK programmes and schools involves epistemic relativism, this particular debate between philosophers and sociologists of knowledge is also known as the debate between (scientific) realism and (sociological) relativism.

One reason why this debate has continued for a long time is the wide range of mutual misinterpretations. Ian Hacking has described the opposition in these 'science wars' as follows: one side tends to combine irrelevant metaphysics with a rage against reason while the other side insists upon scientific metaphysics and an Enlightenment faith in reason (Hacking 1999:62). The conflict between these two parties seems irreconcilable. It is true that there have been several attempts of reconciliation, however, sooner or later, the controversy between philosophy of science and sociology of scientific knowledge has emerged again.

In the present paper, I aim to show, on the example of a particular theoretical argument often applied in the debate in question – the Duhem-Quine thesis of underdetermination of theories by data (DQT) – that the intended argument of the adherents of SSK may have rather unexpected consequences, and, therefore, it cannot be regarded as a compelling argument for one of the debating parties. I am going to question the correctness of the application of the argument, for I find that the argument of underdetermination *alone* neither justifies the relativist programme of SSK nor refutes scientific realism. It is important to note that I shall distinguish between the general underdetermination thesis (UDT) and the Duhem-Quine thesis (DQT). The latter allows to reconcile opponents in the 'science wars' debate, the former – not necessarily.[3]

On the other hand, it is not my main purpose to criticise SSK for the application of DQT. SSK has been extensively criticised by other philosophers of science. I see this particular case of the application of the underdetermination thesis in SSK as a unique opportunity for analysing the assumptions implicit in the 'science-wars' debates. For example, a more detailed analysis of both the SSK views and several versions of scientific realism results in the conclusion that these positions,

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[3]   André Kukla (1998) has demonstrated that no conclusive argument based on the radical thesis of underdetermination could be found to end the debates between realism and anti-realism. The underdetermination thesis as such is an ambiguous one: Michael Dietrich (1993) has, for instance, asked whether it is a theory or a theory *choice* that is underdetermined. Larry Laudan (1990) has pointed out that rational underdetermination cannot be limited to deductive logic only as is often assumed. Following Laudan, Dietrich distinguishes between logical and epistemic underdetermination. Another distinction to be made is the one between the holistic views of Duhem or Quine, and the generalised underdetermination thesis. According to the latter, there is always an infinite number of theories equally well supported by evidence. In such a form, as noted above, the thesis is certainly an anti-realist one. Anti-realism, at the same time, does not necessarily involve social explanation of theory choices. The thesis as construed by SSK rather assumes a moderate kind of underdetermination. See also sections 2 and 6.

previously considered as opposites, rather have certain similarities. As soon as one makes a further distinction between different levels of relativism, such as relativism in ontology and relativism in epistemology, it begins to appear that both SSK and most versions of scientific realism reject ontological relativism and accept epistemic relativism.[4] This distinction between different levels of relativism has received no attention from the 'science warriors'.

   Also, I am going to indicate another possible source of misunderstanding in the 'science wars'. These wars largely rest on an inadequate dichotomy – the dichotomy of the 'rational' and the 'social' (i.e. the rational reconstruction *vs.* social explanation of scientific beliefs).[5] The disjunctive either … or … -structure is unsuitable for reconstructing and interpreting the concepts of the 'rational' and the 'social' in explanations of theory choice. If this dichotomy were correct, it would mean, for example, that fully rational inferences lack entirely the social context, or that, on the contrary, only irrational actions need to be explained by social circumstances.[6] It would also mean that, in (cognitively) rational[7] reconstruction, it is only evidence and logic that matters: respectively, in social explanation, evidence and logic should not be seen as constraints. If the latter were true, scientific theories, and even scientific facts should be treated as purely social stipulations. The idea of a mutually exclusive opposition between the rational and the social (with the aforementioned consequences) has, for instance, served as an argument for Sokal in his attacks against "the postmodernist critics of science"[8]. These consequences have also led many other critics to regard SSK as an idealist programme, if not an anarchist one.

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[4]   For a survey of different kinds of relativism assumed in the SSK programmes, see Lõhkivi (1998). By and large, one may distinguish between radical and moderate versions of relativism. In radical programmes, e.g. as proposed by Woolgar, one assumes the objects under investigation in the sciences to be created by representations (Woolgar 1983). In moderate programmes – despite its name, I regard the strong programme as a moderate one – the following theses have been proposed: 1. beliefs on a topic vary by varying cultural and social settings; 2. the variation needs to be explained by reference to all kinds of causes for the adoption of the beliefs, regardless of their truth or falsity (Barnes & Bloor 1982).

[5]   When the manuscript of the present article was already submitted for publication, Helen Longino's new book "The Fate of Knowledge" was published. According to Elisabeth Anderson, the cover commentator for this book, Longino has given "the first compelling diagnosis of what has gone awry in the raging 'science wars'". This diagnosis consists in a detailed analysis of the dichotomy between the rational and the social. For similar attempts see also Kusch (2000) and Lõhkivi (2001).

[6]   The latter has been a view of mainstream philosophy of science for a long time, see Lakatos (1971), Laudan (1977), Niiniluoto (1999). Also, in early (Mertonian) sociology of science the organisational structure of scientific research was regarded as responsible only for mistakes, whereas true representations were seen as true and rational on the basis of correspondence to reality (for a survey see Woolgar 1988).

[7]   Longino makes the distinction between cognitive rationality and pragmatic rationality. As she sees it, the latter has always included the social, whereas the former has often been regarded as excluding the social (Longino 2002:2). In her social epistemology, she proposes the idea that cognitive rationality is social by its nature.

[8]   This is the tag he attached to the modern sociology of scientific knowledge. (Sokal 1996)

On the other hand, the dichotomy of rationality *vs.* sociality appears within the SSK theoretical framework as well. The central idea in some of the early manifestations of the strong programme (Barnes 1974, 1977) and other British SSK programmatic works seemed to be the abandonment of any rational account of scientific beliefs in favour of descriptions of the social circumstances of their emergence (Collins 1981c, Woolgar 1983). As critics immediately pointed out, such a social account of scientific knowledge, if taken literally, would be self-refuting.[9]

Although the dichotomy of rationality and sociality may certainly emerge in many different contexts, the case of the application of DQT in the realism-*vs.*-relativism debate is a most illuminating one. When the thesis was first applied in SSK, the structure of the argument was as follows:

1. If DQT is valid, theory choices are rationally underdetermined by data.
2. If rational criteria are not available, theory choices have to be based on other criteria, such as social circumstances for the acceptance of beliefs.
3. Thus, if DQT, then exclusively social explanation of knowledge.

In case of missing rational criteria for theory choice, it would be natural to look for other criteria, such as social circumstances. In this way the dichotomy of rational vs. social emerges within the SSK framework. To put it differently, social explanation will be justified only in case of missing rational criteria. The latter thesis is widely known as the *arationality assumption*.[10] This is exactly the opposite of the thrust of the original SSK attempts. Thus, if the DQT is given in the form in which it has often been applied by the relativist sociology of scientific knowledge, it turns out to be rather an argument against SSK.

On the other hand, as will be shown by the analysis of the views of Duhem and Quine who do not endorse the radical underdetermination, modest underdetermination of theories by data and a version of holistic interpretation of scientific knowledge could be fully acceptable both for realist and relativist parties.

What seems to be the key issue here is the interpretation of the underdetermination thesis. In section 2, I will examine the particular context of the SSK application of the underdetermination thesis. As a result of this, I shall pose some questions for further analysis:

1. What were the exact claims made by Duhem and Quine? How adequately have they been interpreted in the realism-relativism debate? (Section 3);
2. Is it possible to reconcile relativism inherent in SSK and scientific realism on the basis of holism acceptable for both of them? (Section 4);
3. Can theories be socially underdetermined? (Section 5);
4. On which conditions can the DQT still be applied within the framework of SSK without contradictions? What could be the benefit of this? (Section 6).

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[9] Self-refutation would be due to the fact that sociological research programmes are also located in a social context, and the validity of the aforementioned dichotomy would imply the non-rationality of these programmes.

[10] The concept was introduced by Laudan (1977) in his comments on Mannheim's sociology of knowledge.

2. The SSK interpretation of the underdetermination thesis

Traditionally, the underdetermination thesis generally, and the DQT in particular, has been discussed in the context of meaning holism, theory-ladenness of scientific observation, and other related issues of philosophy of science and philosophical logic. With the rise of a new theoretical discourse, the SSK, this familiar thesis has been planted into new surroundings. For several SSK authors, the acceptance of the thesis serves as a criterion for opposition between (philosophical) scientific realism and (sociological) relativism. Those who adopt the idea that scientific theories are underdetermined by empirical facts have been supposed to be relativists and anti-realists. According to the SSK authors, for example, a philosophical position like scientific realism should oppose, if not exclude, the underdetermination of theories by facts as well as meaning holism altogether. Furthermore, it is obvious that on such construal the DQT is taken to be a strong argument in favour of relativist sociology of scientific knowledge and against realist philosophy of science.[11]

This means that within the context of SSK the opposition between philosophical positions has been conceived somewhat differently than in the mainstream philosophy of science. Realism *vs.* relativism is hardly among the most discussed issues for the mainstream. Rather, realism is seen in opposition to anti-realism, instrumentalism, empiricism, etc. Relativism, in its turn, is contrasted with objectivism, universalism or absolutism. However, in this specific context of SSK argumentation we are considering, the dichotomy has been constructed as one of realism and relativism. Therefore, this particular dichotomisation justifies a more detailed analysis of the supposed opposites in the light of the underdetermination thesis.

Barry Barnes, an SSK author, and one of the founders of the strong programme, has given his interpretation of the DQT as follows:

> Almost everyone who accepts the Duhem-Quine hypothesis will recognize that any *theory can be maintained compatible with* any *findings by appropriate strategies of applications and interpretation, and that the strategies involved are just those which maintain our actual accepted theories as our accepted theories. ... I have never doubted the correctness of the Duhem-Quine hypothesis. This is why I am not realist, but an instrumentalist and a relativist. Since alternative systems of real universals can always be kept operative, and since the operation of all of them involves artifice, there is no way of knowing that the world is constituted of any set of real universals in particular, or even that it is constituted of universals at all.* (Barnes 1981:493)

Duhem stresses in his *The Aim and Structure of Physical Theory* (1906) that, in a scientific experiment, one cannot test a single theory, but rather a set of background theories together with the particular proposition intended to be tested:

> *The only thing the experiment teaches us is that among the propositions used to predict the phenomenon and to establish whether it would be produced, there is*

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[11] See, e.g. Knorr-Cetina & Mulkay (1983), Hesse (1980), Bloor(1973, 1976, 1981, 1998), Barnes (1974, 1977, 1981, 1992), Barnes, Bloor, Henry (1996), Collins (1981b, 1983, 1985), Woolgar (1983).

at least one error; but where this error lies is just what it does not tell us.
(Duhem 1906/1954:185)

Quine' s stand on the experimental testing of theoretical hypotheses as expressed in his *Two Dogmas of Empiricism* (1953) is more radical:

> [A] single theoretical hypothesis cannot be conclusively falsified, so any state-ment can be held to be true come what may if we make drastic enough adjustments elsewhere in the system.[12]

The basic idea is quite simple: in a holistic system, one can always invoke an auxiliary hypothesis, so that any kind of falsifying evidence becomes eliminated; therefore, it is possible, in principle, to construct an infinite number of theories conforming to the evidence available.[13]

Some sociologists of scientific knowledge have construed the underdetermina-tion thesis as if there were no empirical constraints, or even if they do exist, they will be so weak that the data can be flexibly interpreted (see Collins 1981a, b, c, & 1985). Thus, for example, Harry M. Collins suggests replacing empirical data in the explanation of theory choice with explanation by cultural diversities:

> *If cultures differ in their perceptions of the world, then their perceptions and usages cannot be fully explained by reference to what the world is really like. …We must treat our perceptions of the world … like 'pictures in the fire'. If the world must be introduced then it should play no more role than the fire in which the pictures are seen*. (Collins 1985:16)

This version of SSK seems to claim that philosophy of science has exhausted its resources in the pursuit of an explanation of scientific knowledge, and only sociology is capable of showing how particular contingent scientific theories are determined or underdetermined by certain social or cultural contexts.[14] The under-determination thesis, understood as justifying the sociological approach, also seems

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[12] Quine (1953:43). Lars Bergström has proposed a number of different forms of the Quinean thesis. The way Quine himself has formulated the thesis varies from one context to another. See Bergström (1993:331–358).

[13] Strange as it may seem, in such a form, the DQT might be taken not as an argument for holism but rather as an argument against it. Formulated like this, the DQT seems to allow scepticism or nihilism about scientific findings. The purpose Quine had in mind, however, was very different from, for example, Feyerabend's *anything-goes* scheme. Quine pointed out that it is impossible to prove a single theory on the basis of experimental data, because the experiment or observation is not set out independently of the theory. For the sake of clarity, another distinction should be made between Quine's concept of meaning indeterminacy and underdetermination of scientific theories by facts. Thus, Quinean holism in philosophy of science was, first of all, meant to replace the logical empiricist schemes of theory choice. At the same time, radical underdetermination and strong holism might turn out to be self-refuting on the ground of incommensurability: since in a consistently holistic framework, the criteria for the choice of the framework itself will be missing. Neither Duhem nor Quine assumed such a strong version of holism.

[14] Curiously, it was Carnap, Hempel and Neurath, logical empiricists at that time, who suggested in the 1930s – long before SSK emerged – that cultural and social criteria could be applied in the explanation of theory choice. See, e.g. Hempel (1935).

to involve the idea that a decisive role in the process of theory choice is played by non-epistemic factors. Since, if there were no empirical constraints to theory choice, and a theory could be made consistent with any data, no one theory could be rationally proved to be superior to any other, so that actual choice between them should be explained by reference to social circumstances. On this ground, the two central tenets of the strong programme of SSK – the symmetry claim and the requirement of methodological neutrality or impartiality of explanation – could be seen to follow directly from the DQT.[15] This is to say that as no single theory can be rationally proved to be superior to any other, all theories (accepted or rejected) must be explained symmetrically from a methodologically neutral position.

The social explanation was designed to be carried out as a causal explanation of beliefs. This is expressed by the causality tenet of the strong programme: the explanation of scientific knowledge must be causal, it must reveal the causal mechanisms in the acceptance of a theory. Within the SSK tradition, especially in social constructivism, the causal mechanisms of knowledge production are often seen as conclusively social or cultural.

Ingemar Bohlin (1995:231), in his excellent summary of the SSK interpretation of the DQT, explains the SSK position as follows: in the situation of experimental hypothesis testing, a scientist always faces a number of possible interpretations of phenomena, a package of hypotheses and background theories. This means that the choice of a theory depends crucially on decisions in favour of one or another interpretation[16]. In these decisions, according to the strong programme, one finds the interplay of epistemic and non-epistemic factors, social and other causes. However, the statement made by Barnes about the DQT (see above) is ambiguous: it can be read either as a radical social constructivist proposal, involving ontological relativism[17], or as expressing moderate constructivism, such as the strong programme, which involves only epistemic relativism. For radical social constructivism, purely social explanation appears to be an unconditional consequence of the DQT; this does not hold for the strong programme.[18] For the strong programme, the DQT only serves as an argument for denying the possibility of crucial experiments. The adherents of the strong programme claim that any instance of theory justification has to be seen in its particular context of decision-making where several factors, including those traditionally called 'rational' and 'social', play a role. In this sense, no one representation should be preferred over its possible alternatives beforehand.

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[15] This is a possibility for the theoretical validation of these principles, although the authors of the strong programme did not appeal to DQT when introducing the tenets.

[16] A thorough account of the role and the process of choice and decision in the sciences can be found in Knorr Cetina (1981).

[17] An example of radical social constructivism is Woolgar's postmodernist programme (1983).

[18] Also, the concept of the 'social' is interpreted ambiguously within the SSK. As it will be shown in section 6, the 'social' can be interpreted either 'traditionally' – as the area for non-rational, non-epistemic criteria, or 'non-traditionally – the epistemic or rational standards for theory choices are regarded as a subset of social causes.

Physicist N. David Mermin (1998) has criticised the SSK application of the underdetermination thesis, asserting that the problem in the sciences is not that any body of data is consistent with an infinite number of possible theories; rather, the problem consists in finding *any* theory at all properly conforming to data. In a reply to this, David Bloor, one of the leading advocates of the strong programme, insists that such claims cannot be taken as criticism, because this is exactly what the strong programme says: if nothing fits perfectly, the preference of one imperfection over another needs an explanation. The disagreement between Mermin and Bloor is in fact a difference between normative and descriptive approaches. Mermin compares the epistemologically normative (idealised) underdetermination thesis with the actual situation in the sciences and reaches the conclusion that real science is very different from the idealisations in epistemology, whereas Bloor's point of departure is in descriptive theory of science from the very beginning. For him, the DQT is a thesis claiming simply that no theory is a perfect map of reality, and neither are its possible alternatives. Thus Bloor gives his argument for the acceptance of the DQT:

> *There is no metric measure of shortfall, and we do not know in advance what further difficulties await us. Ultimately we can only rely on our practical judgement and on our sense of purpose. Given that our individual dispositions are inclined to vary, whatever we find stable and shared solutions to such problems of co-ordination, we will be dealing with conventions and institutions.* (Bloor 1998:633)

In addition to the attempts of sociologists to make use of the DQT, Mary B. Hesse, a philosopher sympathetic with both Quine's use of the network metaphor and his meaning relativism, on the one hand, and epistemic relativism of the SSK, on the other, has suggested a similar kind of interpretation of the underdetermination thesis. Her account of social explanation of scientific knowledge is valuable for its critical historical perspective as well. Hesse finds that the main historical change in the sociology of science is that: "social history of science is increasingly [...] taken to mean study of the social conditioning of the theoretical belief systems of science – in other words sociology of science has become a branch of sociology of knowledge". (Hesse 1980:29) This is relevant for the present discussion because some problems in the SSK interpretation of the DQT stem from its pre-history when sociology of science and sociology of knowledge were very different disciplines. According to Hesse, the early version of sociology of knowledge as initiated by Karl Mannheim suffered from a latent contradiction between 'real' and 'distorted' beliefs. If all beliefs are somehow distorted, as Mannheim held, and in need of removal of the distortion, how could one get to know what the 'real' part of a belief is, and how could one know that the 'real' is attainable at all? Presented in this form, Mannheim's theory seems to be self-refuting in its nature. To avoid the contradiction, he introduced a distinction which was later baptised the 'arationality assumption' by Larry Laudan (1977). According to Mannheim, only the 'arational' needed sociological explanation,

whereas 'rational' turned out to be rational due to its coherence with other rational ideas.[19]

Accordingly, the turn in the sociology of knowledge promoted by the strong programme consists in proposing the view that "true belief and rationality are just as much explananda – of the sociology of knowledge as error and non-rationality, and hence that science and logic are to be included in the total programme". (Hesse 1980:31–32) So, the arationality assumption and the asymmetry in explanation should be superseded by the strong programme's principle of symmetry.

In her analysis, Hesse also finds that Quinean holism and the doctrine of underdetermination support the strong programme:

> *Quine points out that scientific theories are never logically determined by data, and that there are consequently always in principle alternative theories that fit the data more or less adequately*. (Hesse 1980:32)

Therefore, she insists that it is rational to conclude that:

> *Thus it is only a short step from this philosophy of science to the suggestion that adoption of such criteria, which can be seen different for different groups and different periods, should be explicable by social rather than logical factors.* (Hesse 1980:33)

However, in an article from 1988, she sees no such direct connection between Quine's philosophy and social explanation of science. In *Socializing Epistemology* (1988), she emphasises that the individualist nature of Quine's epistemology should be clearly contrasted with the social approach. (Hesse 1988:98)

As for my own view, I suggest that the sociological interpretation and application of the DQT in sociology of knowledge gives rise to several serious problems. First, the idea of purely social determination of knowledge, endorsed by radical social constructivism, contradicts our common sense intuitions. As Hesse has argued, the variety of independent objects of reality may well be large, but it is not infinite, the world cannot be 'carved into pieces' arbitrarily. Relatively autonomous facts cannot be reduced to 'purely social stipulation' (See Hesse 1988:112–113). This is what many SSK theorists would entirely agree with. In his reply to the critics Bloor, e.g. has admitted that 'purely social stipulation' would involve idealism, and this is what most critics attack in SSK, although the strong programme has always explicitly endorsed materialism (Bloor 1991, 1996; Barnes, Bloor, Henry 1996:14–15). The strong programme does not pursue 'purely social explanation' even though the DQT is adopted into its theoretical framework. Actually, it could be asked whether there is any adherent of the SSK who would take the social explanation literally and exclusively (with the exception of Woolgar who obviously does). But even a radical constructive programme, the empirical programme of research (EPOR) which is also identified with the position named 'social realism', advocates relativism only in methodology, without any further ontological or epistemological consequences (e.g.

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[19] For Mannheim, see Ringer 1992 and Laudan 1977, as well as Hesse 1980:30–31.

Collins 1985/1992)[20]. Therefore, the meaning of the concept of the 'social', in SSK use, needs further exploration (see section 6).

Second, if it is assumed, in the spirit of early SSK manifestations (Barnes 1974 & 1977), that the 'social' should be understood as a reflection of group interests, it could also be asked whether it is justified to consider a world-wide consensus regarding some essentially scientific question only as an outcome of the interests of leading scientists in a research field or discourse. The interests as well as other social factors may (hypothetically) be radically different for different members of the community – or, to express it in terms introduced by Harry Collins, amongst members of a *core set* (Collins 1985:142, 154–155). To put it in another way, scientists with very different social and professional interests may still reach a consensus in some professional matter. The interplay between the interests and the contents of science must be more complicated than is held by many sociologists of scientific knowledge and by some critics of SSK.[21]

The third problem that arises, if the DQT is interpreted in this particular SSK manner, concerns the existence of empirical constraints to the social explanation itself. Typically, the sociological study of scientific knowledge involves empirical case studies. Does it mean that in the case of sociological study theories are not underdetermined by empirical facts? The old troublesome reflexivity problem emerges again.[22]

Fourth, on this interpretation, scientific realism which is taken to be a view radically opposed to sociological relativism is defined as necessarily rejecting the idea of the underdetermination of theories by data. Hence, scientific realism is seen as committed to a naive version of metaphysical realism: as if the world were truly and genuinely structured in ways depicted by current scientific theories, or structurable so in principle, so that we would be approaching the ultimate truth step by step. Such an account of scientific realism is certainly oversimplified, if not inadequate.[23]

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[20] A valuable distinction between the *literal* and the *charitable* reading of SSK has been suggested by Ilkka Niiniluoto (1999:Ch. 9). The charitable reading of the SSK views reveals its positive and innovative aspects often missed in literal reading.

[21] See Collins 1996, where he argues for the relative independence and value-neutrality of the sciences as well as science studies. This article makes one think that Collins distinguishes between two kinds of social relations: the relations within a scientific practice and wider social, political, etc. interests. He sees the latter to be quite independent from the former sphere of interests, as he finds it unpredictable in which political discourse a purely scientific issue may become involved. See also Hans Radder (1992) who is asking once again the old question of the social sciences: how are the boundaries between different social interests and respective groups to be defined. Longino (2002) insists, in her programme of social epistemology, that instead of group interests the interaction between scientists with very different group identities is essential for understanding the procedure of knowledge production.

[22] Reflexivity and the lack of it in SSK is a favourite topic for Woolgar (1992).

[23] This does not mean that such an account of science does not exist. On the contrary, both scientists and scientific administrators of science often assume such a nature of science. No matter how one labels these views – positivist, realist or objectivist – the central idea remains the same: the better

Taking these problematic issues into account, I find it reasonable to reconsider the application(s) of the DQT once again. In my opinion, the DQT cannot be seen as an argument in favour of relativism in sociology of scientific knowledge as long as the aforementioned problems remain unresolved. Neither can the DQT without further specification be used as the criterion of demarcation for distinguishing between realism and relativism. The following analysis will focus on four claims, each of them requiring a separate consideration:

1) The Duhem-Quine thesis is an inappropriate argument against scientific realism. Underdetermination of theory by data and scientific realism are entirely different issues. In certain contexts, scientific realism would rather be an ally to the sociology of scientific knowledge than its enemy;

2) Theories can be socially underdetermined;

3) Even if social determination could be inferred from the thesis of under-determination, it would entail a kind of asymmetry that was initially intended to be excluded by this very same thesis;

4) The Duhem-Quine thesis of underdetermination cannot serve as a logical or technical argument for a sociological explanation of knowledge: the deductive logical underdetermination does not necessarily imply that social determination is the only alternative; to claim the contrary would also mean that one has to face the dichotomy between the social and the rational.

3. Duhem and Quine between realism and relativism

In the previous sections I have claimed that the Duhem-Quine thesis cannot be used for drawing a clear-cut distinction between realism and relativism. Now it is pertinent to consider Duhem's and Quine's own self-reflections on their relevant views as well as other philosophers' views on Duhem and Quine. This is important since the statements and interpretations vary over a broad spectrum.

The general underdetermination thesis (UDT) could be represented in the following way:

1. Any theory may have an infinite number of empirically equivalent rivals.
2. Empirically equivalent hypotheses are equally plausible.
3. Commitment to a theory is, thus, arbitrary. (See Kukla 1998:58)

In this form, the underdetermination thesis is undoubtedly an anti-realist argument. Duhem's and Quine's versions of the thesis, although differing in certain respects, are both less radical than the UDT.

In his famous *Two Dogmas of Empiricism,* Quine (1953) argued, first of all, against the analytical – synthetical dichotomy in empiricism. The other essential dogma, reductionism, appeared to him as having the same consequences as the

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scientist, the more he or she *discovers* the more facts he or she collects, the more reports he or she writes, etc. The facts are just waiting outside there to be *discovered* and generalised into true theories about the world. For this view see, e.g. Chubin & Restivo (1983); Elzinga (1995).

first one. From his point of view, the dichotomy conflicted the very idea of empiricism because in a consistently empiricist programme analytical truths are unacceptable. I agree with the interpretation of A. J. Ayer, an opponent to Quine's views within logical empiricism, who says that it was Quine's intention

> *to deny the feasibility of Carnap's original project of translating every item of significant discourse into a language which, in addition to its logical apparatus, contains only references to sense-data, but also to take the much more radical and more questionable step of denying that any statement, taken in isolation, can be confirmed or discredited by the occurrence of sensory events that fall within some special range* (Ayer 1982:245).

The radical step in Quine's approach – the denial of any possible confirmation of an isolated statement – was inspired by Duhem.[24] Namely, the core idea in Duhem's conventionalism (as far as one can regard him as a conventionalist: according to McMullin (1990) he should rather be seen as attempting to find a middle way between conventionalism and scientific realism) was that no single proposition can be proven in the light of facts of reality, for the reason that there are always several background hypotheses mutually interacting in any experimental situation. There-fore, nothing like a 'crucial experiment' is possible in principle.[25]

Since Duhem devoted special attention to the conceptual problems surrounding symbolic abstractions involved in scientific theories, it would be correct to argue for the existence of conceptual constraints (as a sort of relativism) on the basis of his conventionalism.[26] Duhem insists that:

> *The symbolic terms connected by a law of physics are, on the other hand, not the sort of abstractions that emerge spontaneously from concrete reality; they are abstractions produced by slow, complicated, and conscious work, i.e. the secular labor which has elaborated physical theories. If we have not done this work or if we do not know physical theories, we cannot understand the law or apply it.* (Duhem: 167)

Here it should be noted – as Hesse has done – that our contemporary social constructivism, and Collins's in particular, "neglects the point of the Duhemian conception of holism of theory, which is not that *all* individual replications can be

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[24] According to Don Howard, Duhem's influence on Quine's holistic views was at the beginning indirect, exerting itself through Neurath's ship-metaphor. Only after Carl Hempel and Philipp Frank had referred to Duhem's work on the occasion of the publication of "Two Dogmas…" in 1951, did Quine become acquainted with Duhem's views and included a footnote citation in the second (1953) version of the "Two Dogmas…" (Howard 1990:376)

[25] All this applies to physics and to those other experimental sciences where the theoretical apparatus is as important as the material one. According to Duhem, the facts gathered in fields like physiology and chemistry can be seen as objective and independent, whereas in physics they depend on theoretical framework. (Duhem: 182) This still does not mean that holism is not valid for physiology and other similar sciences. Because of theoretical assumptions shared with physics-like sciences holism is relevant even for these sciences.

[26] Brenner (1990) finds some similarities between Duhem and the 'Natural Ontological Attitude' of Arthur Fine (1984).

reinterpreted at will but that *some* can, while being constrained by others, and by the coherence of the whole theoretical network" (Hesse 1988:113).
In fact, Duhem really claims that:

> *in order to compare his proposition with reality each (physicist) makes different calculations, so that it is possible for one to verify this law which the other finds contradicted by the same facts. This is plain proof of the following truth: A physical law is a symbolic relation whose application to concrete reality requires that a whole group of laws be known and accepted* (Duhem 168).

Obviously, Duhem must have favoured the idea that laws of nature should be seen as convergent. The convergence of laws is an argument often applied in realist philosophies of science (see E. McMullin 1990). So it seems that Duhem's holism is close to realism.

Quine's view, which says that our knowledge or beliefs are a "man-made fabric which impinges on experience only along the edges" (Quine 1953:42), is also acceptable for a naturalistically minded realist (see, e.g. Devitt 1984 and Wilburn 1992). The idea of a double dependence of science – on language and experience, fits perfectly well into current realist epistemology. Therefore, the following Quine-inspired claim made by Hesse that "no theory can exactly capture the 'facts of matter', even if it makes sense to speak at all of 'facts of the matter' outside the possibility of description by some theoretical conceptual framework or other" (Hesse 1980:33) is in full accordance with a version of critical or transcendental realism. It would be in accordance with an even stronger version of realism – the metaphysical realism – which assumes a causal theory of reference, the central idea of metaphysical realism being that the meaning of a theoretical statement is governed by social factors such as linguistic stereotypes, social linguistic division of labour, etc. Here, I think it is important to distinguish between 'metaphysical realism' and metaphysical realism. The former label is attached to an anecdotal assumption – sometimes also classified as positivism, objectivism, simply realism or monism – the account which posits a 'ready-made-world' capable of being viewed by the 'God's Eye'. Examples of the latter are the metaphysical realisms of Kripke and Putnam. The difference between these positions is that the first view can be characterised as involving an *epistemic fallacy* (see Bhaskar 1978:36). It is a metaphysical position which assumes that human classifications coincide with the real structure of the world. The second kind of metaphysical realism is a metaphysical view of natural kinds as rigid designators like proper nouns (e.g. Kripke 1972, Putnam 1975). The latter is also endorsed by the strong programme. The strong-programme analysts justify their adherence to Kripke's metaphysics by reference to the identification of natural kinds on the basis of spatio-temporal continuity, "a *collective* may come much closer to successful achievement of the task" (Barnes, Bloor, Henry 1996: 66, see also Bloor 1999).

Thus, it only remains to add that the 'realism' of Quine's 'relativism' belongs to one of the most frequently studied issues in Anglo-American philosophy. Duhem's and Quine's versions of holism can be interpreted both in a realist and relativist spirit, or as middle-way positions committed to neither of these views.

4. The possibility of holistic realism

David Papineau has pointed out that the DQT does not really do any harm to scientific realism (Papineau 1995). His argument, in general, is similar to the argument by Hesse referred to in the previous section – that neither Duhem nor Quine assumed total underdetermination of theories by data. Rather, they intended to demonstrate that some theory choices are underdetermined by data, therefore, the decision must be made on the basis of other background assumptions, which are certain to include theoretical assumptions, as well as other relevant experimental data and many other contingent issues. The central idea of this kind of holism is that in a scientific experiment, the scientists' perception of the objects under investigation is always mediated by background assumptions (Quine 1992), so that the interpretative aspect is involved in the very stage of facing evidence. There is no independent evidence, or to put it in terms of *empiricism without two dogmas*: there is no independent observation language[27]. If one compares this simple and clear idea with various views of those philosophers who claim to be scientific realists, one will have an answer to the question whether a holistic variant of realism is possible.[28] However, the question may be approached from many different perspectives. The relationship between the observable and unobservable is a central topic in several theoretical discourses: e.g. debates in the philosophy of mind over the content of mental states and other issues are, in some respect, pertinent to the present discussion (see, e.g. the analysis of interpretation in Barnes, Bloor & Henry (1996)), however, this will not be discussed here. Other relevant topics which I shall not consider here are: probabilistic inferences and inductivism in general, the opposition between realism and methodological anti-realism (Laudan, van Fraassen, and the 'internal realism' of Putnam).

My concern here is only certain versions of scientific realism which have been involved in one way or another, in discussions on sociological relativism. Some realist philosophers have explicitly claimed that there is no conflict between realism and knowledge relativism; many others believe tacitly that realism can be complemented by sociological relativism of knowledge; and still a few others prefer to keep realism and relativism in sharp opposition. Bhaskar, Papineau and Hesse seem to represent the first of these positions. Rom Harré could also be seen as an ally with knowledge relativism. Michael Devitt, Ian Hacking and Ronald N. Giere will here be regarded as representatives of the second kind of realism. There are surely realist philosophers and views opposing both holism and knowledge relativism. Since the concept of anti-relativism, however, is not unequivocal, I will not consider any specific example of this kind of realism in the present paper.

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[27] Duhem, however, made an exception here for non-physical sciences.

[28] For a scientific realist, such a question may seem redundant or naïve, because in contemporary scientific realism an account of theories as pictorial representations of reality could hardly be found. In this paper, the issue of holism in realism is only one landmark on the way to *reconciliation* of realism and relativism.

Hypothetically, it should be a variant of deductive 'double-barrelled' realism[29] or monism where ontological claims about the objects in reality are taken to coincide with epistemological ones, i.e. the world consists of the objects known already or knowable in principle. According to this view, knowing should be seen as individualistic learning by reasoning and experience.

Having this classification in mind, it seems to me that I have taken two steps at once: I have attempted to show the possibility of reconciling realism and relativism before having really shown the possibility of holistic realism. Therefore, let us consider the question of holism and realism first. Only after that can one draw any conclusions about the possible compatibility of realism and relativism because – as mentioned above – holism as such involves neither commitment to realism nor to relativism.

A theory of explicitly holistic realism has been proposed by Hesse in *The Structure of Scientific Inference* (1974). She describes scientific theories as models "in terms of a network of concepts related by laws, in which only pragmatic and relative distinctions can be made between the 'observable' and the 'theoretical'" (1974:4).

The networks consist in inter-related inductive inferences, so that the truth value of a statement may depend on its coherence with the whole theory as well as on its correspondence to the world:

> *It is this inductive construal of theoretical systems that has dictated realism, and realism is represented by the fact that all statements of a theoretical network have truth value and can therefore be assigned probability value as a measure of our belief* (Hesse 1974:293).

Harré has also endorsed the idea of inductive realism. Actually, he distinguishes between three levels or varieties of realism: policy realism, depth realism and convergent realism. The *Policy-Realism* claim is that

> *it is reasonable to read scientific theories as if the models upon which they are based resemble the aspects of the world they represent to some degree* (Harré 1996: 38).

The *Depth-Realism* claim is that:

> *models which stand in for unobservable aspects of the world resemble those aspects in relevant respects and in some degree, provided that the theories expressing them were empirically adequate, ontologically plausible and manipulatively efficacious* (op. cit.).

And the *Convergent-Realism* claim is that:

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[29] The term 'double-barrelled' realism has been introduced by Barnes (1992). In such an account, one runs realism in ontology and realism in epistemology together, so that the world is assumed to have permanent structure represented or capable of being represented by our concepts. Longino (2002) calls this position monism.

in the progress of science as measured by the improving empirical adequacy, ontological plausibility and manipulative efficacy of successive theories, the models for those theories are of greater verisimilitude (op.cit.).

Harré insists that establishing the first two varieties would be necessary for the possibility of the last one.[30] Realism for him is, first of all, a theory of ontological assumptions, or a theory of reference. Through the theoretical models constructed it will be possible to test the resemblance between the type-hierarchies and natural kinds (see Aronson, Harré & Way 1994). It is important to note that resemblance is assumed between certain structures – the type-hierarchies and the natural kinds. Nowhere in this theory can one find any attempt of one-to-one pictorial representation: it is structures, models and networks that are assumed and compared. It is a holistic model.

Similarly, Giere takes the central issue in his descriptive theory of natural science to be model(ling). His models are primarily constructions: "Scientists construct theoretical models that they intend to be at least partial representations of the systems in the real world" (Giere 1988:92). Models are like maps, internally coherent systems, resembling the territory mapped in certain respects and degrees.

Hacking focuses on laboratory science, describing it as a system-like structure with three groups of elements: ideas, things and marks. According to Hacking, Duhem's holism concerned only the sphere of ideas, it was too intellectual. Therefore, theories were seen as unstable or indeterminate:

> *Stable laboratory science arises when theories and laboratory equipment evolve in such a way that they match each other and are mutually self-vindicating. Such symbiosis is a contingent fact about people, our scientific organizations, and nature* (Hacking 1992: 6).

Hacking's argument is directed against underdetermination. But his criticism towards Duhem's holism can also be read as an accusation in setting limits to holism. The above-depicted structure certainly enables a wider holistic interpretation. Science is concerned with laboratory reality as a whole, therefore, Hacking emphasises that the reference to nature in the above-quoted passage should not be understood as if nature actively contributed to the success of science, or as if nature could be used in the explanation of the success. The concept of nature here is restricted to the empirical nature faced in laboratories. His entity realism is known by the slogan: 'If you can spray them, then they are real' (Hacking 1983: 2). But the 'reality' of entities is not seen as everlasting: 'we may live today in an environment in which all our apparatus ceases to work tomorrow.' *(op. cit.)* Theories come and go, the laboratory practice changes, new entities come into existence. However, according to Hacking, it "has nothing to do with 'meaning change' and other semantic notions that have been associated with incommensurability" (1992:57). This is so because theories should not be seen as simple conjectures about 'the

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[30] See Harré 1986, for an extensive treatment of the varieties of realism.

world', rather "[w]e invent devices that produce data and isolate or create pheno-mena, and a network of different levels of theory is true to these phenomena" (Hacking 1992:57–58). Hacking's version of entity realism seems to be holistic only conditionally, as long as incommensurability is excluded. That is a modest variant of holism. From a sociologist's point of view, Hacking's entity realism has a clear advantage over the DQT with regard to compatibility with knowledge relativism: here one is not required to introduce any special conditions for avoiding the dichotomy of the rational and the social. The 'rational' and the 'social' are not seen as standing in opposition to each other.

There could still be a problem because, quite in line with the 'double-barrelled realism', Hacking claims that: "I run knowledge and reality together because the whole issue would be idle if we did not now have some entities that some of us think really do exist." (1983:28) Nevertheless, not every claim of knowledge immediately assumes double-barrelled realism. Michael Devitt who defines his version of realism as an ontological one, finds that even such a realism must be partly epistemic, because knowledge is assumed also in the thesis "the world must be independent of our knowledge of it. So at least that much epistemology must be settled to settle realism." (Devitt 1984:4) When talking about the ontological assumptions and the metaphysical theory of reference, what is meant is the question "what would the world be like if our *knowledge* of it were true?" For this reason, it is easy to confuse a metaphysical theory of reference – 'residual' realism in Barnes's terms – with 'double-barrelled realism'. In my opinion, since the use of the concept of knowledge in scientific realism varies from one author to another, the only suitable criterion for the distinction must be a pragmatic one: we need to analyse what is actually being done or what is actually being claimed in a theory or a programme.[31]

The distinction between the 'residual' and 'double-barrelled' realism is also related to the issue of meaning finitism: knowledge relativism claims that "the established meaning of a word does not determine its future applications. … Meaning is created by acts of use." (Bloor 1983:25) Does this mean appealing to the radical underdetermination again: because of the lack of empirical constraints, knowledge claims should be explained only by social ones? Not necessarily. First, as both Duhem and Quine asserted, empirical underdetermination is valid only in some respect, in some specific part of a holistic system. And second, finitism as such does not exclude empirical constraints, it only insists that "we are to think of meaning extended as far as, but no further than, the finite range of circumstances in which a word is used" (Bloor 1983:25)[32]. Therefore, one could even claim that

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[31] Therefore, I do not agree with the view of Barnes (1992:144) on Bhaskar whom he regards as a representative of double-barrelled realism. Although Bhaskar has used the term 'metaphysical' for classifying his realism, it is in accordance with Barnes's residual rather than double-barrelled realism.

[32] See Barnes, Bloor & Henry 1996, and particularly Bloor (1999) for the importance of empirical constraints in explanation of scientific beliefs. See also Elzinga (1992:60–61).

empirical constraints involve social constraints, and vice versa. On this question Hacking and Bloor would agree: compare, for example, the aforementioned argument of Hacking with the following passage from a recent article by Bloor:

> *All knowledge always depends on society. This is because, as I have argued and as case-studies demonstrate, society is the necessary vehicle for sustaining a coherent cognitive relation to the world, especially a relation of the kind we take for granted in our science* (Bloor 1999:110).

For a strong programme relativist, there are no pure independent facts, no pure data, no meanings based on inherent properties – no more than there are any pure social stipulations in the sciences.

Both the transcendental form of realism, such as Bhaskar's or Niiniluoto's critical realism, and the version of empirical/inductive realism such as entity realism or Hesse's probabilistic realism, involve interpretation as a function in concept formation, and they both allow a wide range of interpretations of physical reality. One can say that such realism and the relativist SSK treat meaning in a similar, Wittgensteinian manner – as use.

Barnes (1992) finds that as soon as a realist comes to mention truth conditions, s/he must adhere to 'double-barrelled' realism. This I find to be a rash conclusion. Even if, in the realist approach, the meaning of a concept contains its truth conditions, i.e. if part of its meaning could be seen as reference, this is no loss for knowledge relativism yet. Reference could be understood as a hypothesis about the kinds of things there are in the world. Since we do not have direct access to the world, independently of our minds, we can only rely upon evidence which, indirectly, provides us with a positive or negative proof of the hypothesis. Therefore, the meaning of a concept remains interpretational and theoretical even in scientific realism, and it cannot be identified either with evidence (data, appearances) or with some 'inherent properties'. Meaning and reference both remain contingent and local issues in this approach.

Modest meaning holism has some other advantages in addition to those considered in this paper in connection with the realism – relativism debate[33]. For instance, it enables one to reach a better understanding of mistakes. According to this model, mistakes can be analysed as entirely natural and rational products of cognitive activity. This is to say that modest meaning holism involves fallibilist epistemology (see Papineau 1987).

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[33] There have been various attempts of reconciliation of realism and relativism which are not considered in the present paper, e.g. the 'two-tier-thinking' of Elkana (1978) and the 'double-reality of the sciences' (Elzinga 1993). The account involving the distinction between the residual and double-barrelled realism (Barnes 1992) could also be regarded as such an attempt. And, of course, there are the explicitly 'realist' views of Bloor (1991, 1996, 1999), etc.

5. Social underdetermination

Even those who admit the importance of social circumstances in the explanation of scientific knowledge, have to note that social underdetermination is as plausible as epistemic or logical underdetermination of theory choice. Perhaps it has been another misinterpretation of the underdetermination thesis that, in case of lacking empirical constraints, social constraints indispensably should be invoked. First of all, neither UDT nor DQT deny the existence of empirical constraints. Rather, the problem is – as pointed out by Longino (2002:63) – that sometimes the constraints are insufficient for imposing a decision in one or another direction. Even if social factors could be seen as playing a role in theory choice, this does not necessarily mean that the list of possible restrictions is limited to two mutually exclusive kinds of constraints.

Michael Dietrich (1993) finds it to be a common misinterpretation of DQT that the underdetermination of *theories* and the underdetermination of *choices* between theories are conflated with each other. If one focuses on the underdetermination of choices, one will find that underdetermination is a relation based on the principles of choice (Dietrich 1993:114). Thus, when the choice is taken to be logically underdetermined, and there are several theories mutually in contradiction but equally well supported by evidence, the decision could be made on the basis of some other principles.

According to Dietrich, however, more often the underdetermination is not seen as a logical relation, but rather as an epistemic one: some of the several epistemic criteria are underdetermined whereas others are not. The epistemic criteria, according to Laudan, include logical compatibility, explanation of evidence and empirical support by evidence. Respectively, three kinds of rules or principles for theory choice are implied.[34]

But the choice could also be made on the basis of the criterion of the lowest experimental costs. Dietrich points to the multiplicity of methodological rules for theory choice – the rules or standards applied may be epistemic, pragmatic, and social. And all of them involve respective kinds of underdetermination.

Dietrich notes that the Duhem-Quine thesis has had a special place in the sociology of science: it has allowed to break down the internal – external division. Still, he agrees with Laudan that the sociologists are over-optimistic when they regard the DQT as problem-free.

For example, the SSK authors seem not to have paid any attention to the possibility of variation in the strength of holistic claims constructed on the basis of the DQT. The strong version of holism involves the whole range of problems connected with incommensurability[35] whereas modest holism does not. Neither

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[34] The number of criteria for an adequate theory varies from one philosopher of science to another. Kuhn, for instance, gives five criteria – accuracy, consistency, broad scope, simplicity, and fruitfulness.

[35] A good overview of the problems of incommensurability can be found in Harris (1992).

have the adherents of the SSK considered the possibility of social under-
determination.[36]

Longino refers to a related difficulty of choice between different value systems
independently of the theories involved (Longino 1990:182). Such holism contains
a vicious circle because facts, theories and (social) values are all mutually related:

> *Thus, there would be no independent way of choosing between a theory that*
> *claims that some relationship is natural and one denying this, or between a*
> *theory prohibiting interference in natural relationships and one permitting it*
> (Longino 1990:182).

This may be interpreted as underdetermination as well. As it also follows from
Dietrich, underdetermination should be seen as a complicated scheme where
pragmatic, social and epistemic factors interplay. His conclusion is that theory
choice and underdetermination should be regarded as contingent problems. It is
always important to ask what exactly is underdetermined and to which degree. In
what way is something underdetermined or determined?

The moral for the sociology of scientific knowledge seems to be that since
theory choice can be socially underdetermined under certain conditions, it is
unreasonable, for a sociologist, to hold on to the strong underdetermination thesis.
A modest version, on the other hand, allows a sociologist to pursue a detailed
analysis of all the criteria operating in theory choice.

6. DQT and its consequences for SSK relativism: the dichotomy of social vs. rational

6.1. The arationality assumption revisited: a pragmatic argument

In his commentary on a critical article against the strong programme and social
constructivism (Roth & Barrett 1990), Steve Fuller (1990) has been concerned
mainly with the *arationality assumption*. For Roth and Barrett, the target of
criticism was the application of the DQT by sociologists. Fuller agrees with them
that in the sociologists' use of the Duhem-Quine thesis, the arationality assump-
tion often appears in its latent form. However, the end result of Roth's and
Barrett's criticism is the reappearance of the dichotomy between rationality and
sociality. Their taken-for-granted trust in DQT is presented in such a way that it
could easily be turned against the whole sociological enterprise. They regard
radical underdetermination as a dominant thesis in the SSK tradition. Fuller
disagrees with Roth and Barrett in this oversimplified account of SSK views and
critically reformulates their argument to indicate that it would lead to the absurd
conclusion that:

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[36] Indirectly, this possibility is involved in the strong programme's principle of causal explanation
which includes explanation of all causes of beliefs and the reflexivity principle. These principles
taken together should lead to the possibility of social underdetermination.

> *if a foolproof method for theory choice were possible, then there would be no need for a sociology of scientific knowledge* (Fuller:665).

The arationality assumption, according to which social explanation is required only for the arational, contradicts the principle of symmetry. The latter is a central theoretical thesis in the strong programme, and in the SSK in general. Thus, if a social explanation of theory choice is invoked only in case there is no rational explanation available, i.e. when rational criteria are not met, it would immediately imply asymmetry and a hierarchical structure of explanation. A proper, symmetric account explains both, the 'rational' and 'arational', by similar kinds of causes.[37]

Fuller, following the line of reasoning presented by Roth and Barrett, assumed in his argument that the appeal to DQT in SSK must be unconditional – as seemed to be the case in some specific SSK declarations of the 1980s – so that it would necessarily involve the validity of the implication 'if DQT, then social determination of knowledge', and even the other way round. If this implication were valid, it would certainly revive the logical positivist dichotomy of the rational and the social. Thus, Fuller is led to the conclusion that sociology would indispensably fall into the trap of logical positivism as soon as the DQT is adopted. This means that the SSK would return to historical division of labour between philosophy and sociology, where the task of philosophy was to give normative explanations and the task of sociology was to explain anomalies. For this reason, Fuller finds that it is not strategically reasonable for relativist sociology to rely upon the DQT. Furthermore, in Fuller's opinion, Quine must be seen as a philosopher within the internalist tradition whose views are quite similar to those of Laudan, a major critic of the SSK account of science. Laudan's criticism against DQT does nothing more than probe a few minor differences in his and Quine's views. Fuller's recommendation to the SSK is a pragmatic one: it is unreasonable to rely upon arguments deriving from a hostile tradition.

One may well agree that Duhem and Quine obviously are related to the internalist tradition[38], because Quine's naturalised epistemology was by and large an individualist theory, and so was Duhem's instrumentalism. At the same time, due to holism their epistemologies should be considered apart from general foundationalist tendencies in the internalist tradition. Anti-foundationalism is a feature of DQT that could be shared with the epistemology of the sociology of scientific knowledge. Still, anti-foundationalism alone is insufficient for accepting SSK relativism. The range of all anti-foundationalist epistemologies is rather wide.

The thesis of underdetermination of theories by facts has been over-exploited in philosophy, and the same applies to sociology of scientific knowledge. Some sociologists, viz. radical social constructivists, have reinterpreted the DQT as if claiming a total lack of empirical constraints. Such an interpretation necessarily

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[37] See either Bloor (1991) or Barnes and Bloor (1982).

[38] The opposition between internalism and externalism here is interpreted in the way sociologists and historians of science do. In epistemology, philosophy of mind and theory of action the distinctions are slightly different.

involves the above-mentioned difficulties for sociology of knowledge, plus the fact that neither Duhem nor Quine would have accepted such an interpretation. Criticism based on the arationality assumption, like the one presented by Roth and Barrett, leaves intact the modest SSK relativism of the strong programme. And modestly holistic claims seem to endure the SSK account of scientific knowledge. The latter issue will be considered in the following part of the essay.

6.2. *Laudan* vs. *strong programme and Hesse*

In this section, I shall first analyse a major criticism directed against both the DQT and the application of the thesis in sociology of scientific knowledge. In a number of critical articles, Larry Laudan has chosen either a strategy of refuting the whole thesis as long as it can be seen as an argument for holism in epistemology, or a strategy of refuting its possible consequences. Many other similar attempts have been made (see, e.g. Richard Boyd 1973, Lars Bergström 1993 and F. Weinert 1995). For the analysis of the current debate between realism and relativism such a radical approach is not really necessary. In a more traditional bipolar case, where one assumes realism to stand in unambiguous opposition to relativism, so that the latter would necessarily be in accordance with the underdetermination thesis, it would be indispensable for a realist to argue against underdetermination. As shown in previous sections, the DQT is less radical than the general underdetermination thesis; neither is the opposition between SSK relativism and scientific realism identical with the opposition between realism and antirealism. Therefore, it is not necessary either to refute or prove the DQT. For my analytical purposes, it is sufficient just to examine why it is incorrect to deduce social determination of knowledge from the DQT, what consequences such an inference might involve, and what would be the solution to the problem. As claimed above, direct inference of social explanation from the DQT would commit us to the misleading dichotomy of rational *vs.* social. The dichotomy is misleading because, as indicated, it involves the arationality assumption which appears to be implausible. However, this argument is not a compelling one. For the 'science warriors', the arationality assumption and the dichotomy of rationality *vs.* sociality can be sustained, and the war continued. A reconciling alternative may be found in social epistemology which offers a third-way solution without the notorious dichotomy.[39] To my mind, the strong programme variant of relativism and the views of Mary Hesse belong to the third way rather than to radical social constructivism.[40] Therefore, I am going to consider Laudan's criticism on Hesse and the strong programme and suggest a response.

In his article *Demystifying Underdetermination* (1990), Laudan demonstrates that deductive underdetermination is not necessarily valid for the whole area of

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[39] See Longino's recent study in social epistemology (Longino 2002).

[40] Those who endorse the dichotomy, tend to interpret the 'social' either as wishful, ideologically biased or interest-laden thinking. In the third-way approach, the 'social' is regarded as a concept applying to shared standards for the 'rational' as well as the patterns of human interactions in knowledge production.

possibly rational inferences[41]. Even if certain rules or standards strongly under-determine theory choice, the choice may still be a rational judgement based on other rational criteria. Laudan concludes: "what is wrong with QUD [Quine's under-determination] is that it has dropped out any reference to the rationality of theory choices, and specifically theory rejections" (Laudan 1990: 276). Accordingly, Laudan reformulates Quine's thesis to show that no reasonable argument for such a thesis can be given:

> *QUD2: any theory can be rationally retained in the face of any recalcitrant evidence* (Laudan 1990:277).

He finds this to be equal to the following assertion:

> *to say that a theory can be rationally retained is to say that reasons can be given for holding that theory, or the system of which it is a part, is true (or empirically adequate) that are (preferably stronger than but) at least as strong as the reasons that can be given for holding as true (or empirically adequate) any of its* known *rivals* (Laudan 1990:277).

Via several complicated reformulations of the DQT, Laudan actually tries to show that in no form can the underdetermination thesis entirely exclude rational criteria for theory choice. He stresses the fact that rational criteria do not end with deductive logic. Thus, for instance, Laudan restates the thesis in a form that looks quite Popperian: "any theory can be shown to be as well supported by any evidence as any of its known rivals" (Laudan 1990:277).

Laudan refers to Popper because the latter has shown that theories with the same positive instances *e* may be *differently* supported by the same evidence *e*. For example, the verisimilitude of the competing theories may vary, or the principal falsifiability may be different. Both these are purely rational or epistemic criteria for theory choice. And there are other rational criteria such as rational assertability or warrantability, reliability, plausibility, simplicity, problem solving ability, predictive power, etc.

Thus, Laudan succeeds in showing that the Quinean deductive underdetermina-tion does not extend over every possible epistemic criterion for theory choice. Even if deductive logic is abandoned, there remain certain possibilities for rational or epistemic determination.

On the other hand, since my brief analysis of Duhem's and Quine's views resulted in the conclusion that neither Duhem nor Quine advocated total under-determination, one may suppose that they might even have agreed with Laudan. Laudan's criticism is more adequate if directed against the general thesis of underdetermination. It is the UDT rather than the DQT that can be seen to involve irrational holism or radical perspectivism in epistemology. Laudan's main purpose is to defend epistemology from such holism. For him, the sociology of scientific knowledge in particular is an illustrative case of irrationality. Therefore, he has

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[41] Laudan (1990:267–297. See also Laudan & Leplin (1991:449–472, and Laudan (1981), and (1982).

chosen to attack the aforementioned argument by Mary Hesse (1980). In his criticism, he focused on an alleged mistake Hesse and the strong programme sociologists have made: "[It] is that of supposing that *any* of the normative forms of underdetermination […] entails anything whatever about what *causes* scientists to adopt the theories or the ampliative rules that they do" (Laudan 1990:288).

This criticism will be fair if the dichotomy of rational *vs.* social is taken for granted. This means that, according to Laudan, there are two strictly separated areas: the domain of normative, rational inferences and the domain of contingent causes of scientists' beliefs. This reminds one of Reichenbach's distinction between the 'context of justification' (normative, rational inferences) and the 'context of discovery' (subjective causes of beliefs). Within such a framework, the DQT of underdetermination certainly belongs to the first area and the social issues to the second. Laudan comes to recapitulate the same idea in a number of different ways, opposing 'good reasons' and 'causal production of belief':

> *The Duhem-Quine thesis is, in all of its many versions, a thesis about the logical relations between certain statements; it is not about, nor does it directly entail anything about, the causal interconnections going on in the heads of scientists who believe those statements [...] Whether theories are deductively determined by the data, or radically underdetermined by that data; in neither case does* anything *follow concerning the contingent processes whereby scientists are caused to utilize extralogical criteria for theory evaluation [...] The point is that normative matters of logic and methodology need to be sharply distinguished from empirical questions about the causes of scientific belief.* (Laudan 1990: 289)

Now that he has made a clear-cut distinction between the 'rational' and the 'social', he makes a sudden turn and ascribes this newly constructed dichotomy (rational *vs.* social) to Hesse and sociology of scientific knowledge, and starts to refute it as an incorrect one. Thus Laudan reads Hesse's argument as presupposing that: "everything is either deductive logic or sociology" (Laudan 1990:288). After that, he attempts to show that, for Hesse, the concept of the 'rational' is limited to deductive logic only. On the other hand, he points out – contrary to his own stand – that the laws of logic are formulated in a language made by humans (as social beings), and should therefore be considered as 'social'. He finds that Hesse has, in fact, neglected the social aspect of the laws of logic.

It is obvious that Laudan has misinterpreted Hesse's argument. I shall first consider the criticism concerning the minor issue of the laws of logic. What Hesse actually opposes is the traditional, individualistic, exclusively rational explanation of scientific knowledge that she calls 'the logic of science' (Hesse 1980:33). Instead, she suggests a network-image of scientific knowledge where the laws of logic cannot be seen as essentially different from the laws of science. Therefore, Hesse certainly regards the laws as having a social aspect.

As to the main point, which concerns the criticism on the limited scope of rational criteria, one should note that, in Hesse's use, 'rationality' is surely not restricted to deductive logic only. In the same section where Hesse gives her

reasons why there "is only a short step from (Quinean) philosophy of science to the suggestion that adoption of such criteria, which can be seen to be different for different groups and different periods, should be explicable by social rather than logical factors" (*op. cit.*), she explicitly enumerates the criteria (varying from one paradigm to another) of 'what counts as a good theory: criteria of simplicity and good approximation' (*op. cit.*), etc. Therefore, I think, Hesse cannot be accused of reducing rationality to deduction, even if rationality is being considered purely in terms of rationalist philosophy. Moreover, in the very same paragraph, she claims that both these criteria and "what it is to be an 'explanation' or a 'cause' or a 'good inference', and even what is the practical goal of scientific theorising" must be made intelligible by extra-scientific causation. This idea has not received any attention from Laudan.

Hesse certainly does not argue against the 'rational', and her ambition is not to abandon epistemology. On the contrary, she has argued that epistemology cannot be excluded from an account of science as a social institution, because such an attempt of exclusion would, sooner or later, result in the conclusion that: "An epistemology is needed to discover an epistemology." (Hesse 1988:107)

The main difference between Hesse's and Laudan's approaches is that Hesse does not dichotomise the 'rational' and the 'social'. For Hesse, the 'rational' is social/historical. For instance, she insists that: "Nothing is lost epistemologically if theories are taken relative to social context" (Hesse 1980:xxiv).[42] David Bloor has also presented arguments for the view that "epistemic factors are really social factors". (Bloor: 1984:297)

Laudan's ambition has been to prove that the inference of social explanation from deductive underdetermination is incorrect, since there are other epistemic criteria available for theory choice, ignored by Hesse and the strong programme.[43]

Even if seen from the third-way perspective, the implication 'DQT → social determination' is a false one – basically because it involves a sort of *category mistake*.[44] With this implication, one also assumes that the 'social' must be seen as a sort of negation of the 'rational': the 'social' may be understood only in the same terms as the 'rational' – as its negation, as non-rational (irrational, arational). Following this logic, many critics of SSK tend to emphasise the irrationality of the sociological account of scientific knowledge. The critics take it for granted that, when social factors are seen to be operating, no room is left for the rational factors, and vice versa. This opposition is essential in the 'science wars'.

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[42] In a recent study, Martin Kusch has argued for a similar position he calls 'sociologism'. This is a view that "all 'rational entities' (arguments, theories, reasons) are 'social entities." (Kush: 2000:ix)

[43] As shown by Dietrich's argument referred to in section 5, it is the variation of epistemic criteria, and accordingly, the epistemic underdetermination rather than the logical one that makes sociological explanation more plausible.

[44] For a historical analogy of category mistake, see *The Concept of Mind* by Gilbert Ryle (1949) which criticised the Cartesian understanding of mind via the negation of the concept of body.

A third-way solution would be to consider the context of justification together with the context of discovery, and to inquire why particular rational standards are taken to be valid in particular social contexts. This means, roughly, that the rational is social in its nature. Then the disjunctive structure disappears: the 'rational' and the 'social' are not seen as concepts of the same logical category any longer.

Still, the inadequacy of the above implication does not mean that the DQT and holism should be opposed by sociology of scientific knowledge. The acceptance of the DQT and adherence to holism in SSK seem rather to be empirical or contingent matters. Hesse has offered the following argument for accepting both:

> [I]t is generally accepted that in a complex scientific situation theories will be *underdetermined by logic and evidence and hence not explicable by purely scientific reasons in that sense. But the claim becomes substantial if we take it in the sense that internal reasons drawn from logic, evidence, normal inductive reasoning, and the local scientific tradition ("background knowledge") are not sufficient, and moreover that the remaining explanatory gap cannot be filled by reference to individual psychology ("great man" theories of explanation), and should not be filled by appeal to simple historical accident* (Hesse 1988:104).

Thus, she finds that a kind of contextualist approach is needed where both the empirical-theoretical/rational and the social issues are taken on board. Among realist philosophers, Rom Harré has also explicitly stated that the *rational* is *social* (Harré 1983). Harré as well as Bhaskar assume epistemic relativism within the framework of scientific realism. As they see it, epistemic relativism enables them to take into account culturally and historically varying standards of rationality.

There still remains a central question to be answered: are the strong programme and other SSK theories justified in their reliance upon the Duhem-Quine thesis, or are they not?

Many SSK theorists are aware of the possible consequences of the dichotomy of rational *vs.* social (see Barnes, Bloor, Henry 1996:28). Hesse has also noted that "Quinean epistemology is essentially *individualist*" (1988:98), i.e. it is not a collectivist social theory of knowledge[45]. Karin Knorr Cetina and Michael Mulkay (1983), two leading social constructivists, concede that some of Laudan's conclusions are correct, e.g. that social explanation cannot simply be logically inferred from the underdetermination of theories by data. However, they see the DQT to support social explanation of scientific knowledge anyway:

> *if the thesis that scientific theories are logically underdetermined by evidence is correct, it removes an important constraint on theory acceptance which opens the way for social science investigation* (Knorr Cetina & Mulkay 1983:3).

Thus they redefine the function of the DQT:

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[45] Social theories may also be individualist in their nature, e.g. those interpreting the 'social' via mutual intentions of the individuals (see Searle 1995).

while the Duhem-Quine thesis of underdetermination does not prove that social factors structure scientists' theory choices, it does make it more likely that some kinds of non-logical factors play a role (op. cit., p. 4).

With this conclusion they do not really solve the problem: at best, it is a commonsensical solution inspired by some specific interests of empirical research. Still, their argument is valuable, because it hints at an essential issue emphasised by Laudan. The point is that the DQT itself is not enough for deducing sociological relativism. As mentioned in the introduction, there are many different forms of the thesis, each of them claiming to yield different consequences. In one form, the thesis can be used in line with a positivist tradition of argumentation; in another form, the thesis can be applied by Popperians; in still another form, by rationalists like Laudan, etc. The thesis itself does not involve commitment to any specific position. Thus, the Duhem-Quine thesis could be used as an argument in SSK only under certain conditions:

1. The dichotomy of the rational and the social is not assumed;
2. The social determination of theory choice is not directly deduced from the thesis;
3. The thesis as such, without further specifications, will not be used for the explanation of one's commitment to SSK (or any other view).

7. Conclusion

The starting point for this paper was the Duhem-Quine thesis (DQT) of underdetermination of theories by facts as an argument often used in relativist sociology of scientific knowledge (SSK) for justifying social explanation of knowledge. My preliminary intention was to show that the argument may have unexpected consequences and it may involve contradiction. In addition to that, I tried to show that the DQT is irrelevant for the realism – relativism debate, at least insofar as scientific realism and relativism in SSK are concerned, on the one hand, and the DQT as a modest underdetermination thesis, on the other. The DQT can be adjusted to the requirements of realism, and so, the thesis in itself is incapable of directly imposing relativism or realism.

I come to the following conclusions:

1. The argument of deduction of social explanation from the underdetermination thesis contradicts the central principles of sociology of scientific knowledge: even if it were correct to deduce purely social stipulation from the DQT, it would directly involve accusations in idealism, which the SSK has actually sought to avoid. The strong programme, in particular, has emphasised materialism as its essential commitment.

2. A central thesis of the strong programme, the symmetry tenet, may be contradicted by consequences that follow from the DQT. The implication:

'DQT $\rightarrow$ exclusively social explanation of knowledge'

if presented without further specifications, involves a dichotomy between the rational and the social. This dichotomy, in its turn, involves an asymmetry of explanation, resulting in the notorious 'arationality assumption'.

3. However, on closer examination, it appears that it does not follow directly from the DQT that one could remove the empirical constraints for concept application and, respectively, restrict oneself to purely social explanation of knowledge. Underdetermination is not identical with indeterminacy: rather, it deals with a particular theory within a larger network of theories.

4. What is also worth noting is the possibility of underdetermination of social explanation. If it is natural to assume theory- and value-ladenness of observation, it will be as likely that sociological analysis is value-laden.

5. The idea of modest *theory-ladenness* of observation, essentially accompanying the DQT, is acceptable for both realism and relativism, because in both scientific realism and sociological relativism knowledge production in the sciences is conceived as a hypothetical, constructive and self-corrective process. Still, the basis for self-correction is seen differently: in the realist case, it may contain both empirical and social factors; in relativism, views regarding the grounds of cognitive action vary from one tradition to another. The strong programme seems to be relatively close in its approach to critical scientific realism, while radical social constructivism, tending towards a symbolic model of human social cognitive action, denies the empirical constraints of knowledge and sees it as a basically ritual activity.

6. Thus, one may also conclude that, in the light of the Duhem-Quine thesis of underdetermination, the previously obvious and indubitable opposition between scientific realism and relativist sociology of scientific knowledge seems to disappear. Therefore, the present analysis can be seen as a contribution to the wider project of reconciling scientific realism and SSK, as well as a contribution towards ending the 'science wars'.

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