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PREFACE AND ACKNOWLEDGEMENTS

This PhD dissertation is a result of my decade-long studies both in philosophy of science and in the sociology of scientific knowledge (SSK). Having started from the investigation of the vast field of scientific realism, I came, step by step, to comprehend the significance of a relatively ill-examined territory of epistemological views — the sociology of scientific knowledge. The shift in my research interests that led to the change of the topic for the PhD dissertation was mainly due to the opportunity for guest studies at a leading centre for science studies — the Department of Theory of Science and Research, University of Göteborg, Sweden. At the beginning, the plurality of perspectives and approaches, which I found at the famous *Vetenskapsteori,* seemed scaring. My prior training was in natural sciences, basically chemistry, and in analytically oriented philosophy of science. Both of these fields are quite different from science studies. It was the friendly atmosphere and openness to various ideas that encouraged me to learn more about the sociology of scientific knowledge which is the main theoretical tradition in science studies. What also promoted my curiosity was a particular feature of the early SSK manifestations: it was the fact that the SSK adherents tended to contrast their views with scientific realism. However, their descriptions did not quite coincide with what I regarded as scientific realism.

By 1990s several philosophers of science had already paid attention to the SSK, most of them expressing their disapproval of this 'post-modernist fashion'. Thus, the sociologists and the philosophers seemed to ridicule each other's views. Still, a few outstanding British philosophers within the realist school (Mary B. Hesse and Rom Harré) appeared to favour the new sociology, and their arguments seemed to be convincing. Sociologists in their turn, saw Roy Bhaskar's version of realism as a tenable position. In addition to that, philosophers who advocate social epistemology — an epistemology sensitive to the social and cultural contexts of knowledge (Aant Elzinga, Steve Fuller and Helen Longino) — had proposed a compromise between traditional epistemology and the relativist sociology of scientific knowledge. Influenced by these two traditions in philosophy — scientific realism and social epistemology — I decided to continue my studies on the perspectives for reconciling the SSK and the philosophy of science.

It was exactly then, in 1996, that the 'Sokal-affair' restarted the 'science wars'. The debates between philosophy of science and the SSK obtained the character of a serious opposition. Suddenly, the realism-relativism issue was a hot topic. Accordingly, the amount of publications increased very fast, so that it has become somewhat hard to follow the course of the debate.

In this PhD thesis, however, I will not even attempt to offer an overview of the entire range of these 'science-wars' debates. Instead, I am going to focus on

three particular discussions of the SSK issues, which enable to shed light on the epistemological, ontological and methodological assumptions of this account of science; via these case studies I also intend to reveal the nature of the 'science-wars' debate. After the general introductory considerations in chapter 1, I will, in chapter 2, analyse the philosophical debate on the consistency of relativism that has been going on mainly within the SSK community. In chapter 3, I am going to consider a case of an 'imported' philosophical argument — the Duhem-Quine underdetermination thesis and its consequences for the SSK. In the final chapter, I will consider the SSK as a methodological programme for the study of history of science in the light of the debate between externalism and internalism.

Slightly different versions of the chapters 2 and 3 have been previously published in *Trames: Journal of the Humanities and Social Sciences*, respectively no 4, 1998, 299–330, and no 2, 2002, 141–172. Chapter 4 is based on an article published in *Estonian Studies in History and Philosophy of Science*, Kluwer Academic Publishers, 2001, 139–150. All these articles are reprinted here by permission of the the publishers.

One of the main theoretical points in my thesis is that, in the 'science-wars' arguments, the dichotomy of the 'rational' and the 'social' is assumed: the rational account of scientific knowledge is regarded as excluding any reference to anything social and vice versa (see, e.g., Laudan 1981, 1982, 1990, 1996, Koertge 1998, 1999, Newton-Smith, Hollis, and Barnes & Bloor 1982, Collins 1981c).

The adherents of the 'rationalist' tradition tend to present the opponent's position as irrational and even as endorsing politically dangerous views like treating the scientific evolutionary biology and creationist's ideas alike. The adherents of the 'sociological' tradition, in their turn, tend to ignore the normative questions of philosophy of science.

For the reconciliation purposes, the dichotomy needs to be overcome. One way for achieving this is via the reinterpretation of the 'rational' so that it will be regarded as social by its nature. This is how some advocates of the SSK, the strong programme sociologists Barry Barnes, David Bloor, Steven Shapin and philosopher Martin Kusch, actually have construed the concept of the 'rational'. Thus they should rather be seen as sharing a third-way position. On the other hand, the social practices may be reinterpreted as rational (Longino 2002).

Due to the third-way interpretation, the rational standards for justification will be regarded as a local, context-dependent matter. In my licentiate thesis defended in 1999 at the Department of Theory of Science, University of Göteborg, published in the series of reports of the department in 2001, I proposed a third-way view on the underdetermination thesis and social explanation. However, when the respective part of my work — newly elaborated and ready to be included in the present thesis — was already submitted for publication in

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1. RECONCILING THE SSK AND THE PHILOSOPHY OF SCIENCE

1.1. A survey of the philosophical problems in the SSK

Until the 1970s when the first programmatic works in sociology of scientific knowledge (SSK) appeared, philosophy of science, sociology of science and sociology of knowledge were all separate disciplines with no connections to each other. From the very beginning, the SSK had philosophical ambitions: most of its theoretical views have been developed in opposition to some dominant conception in philosophy of science. The SSK account of scientific knowledge was intended to substitute the philosophical mainstream view. The strong programme, one of the pioneers in this new approach, claimed, for example, that instead of the traditional concept of knowledge as true and justified belief, one should treat knowledge as a 'natural phenomenon' (Bloor 1991: 5). For a sociologist, knowledge is whatever people take to be knowledge. This does not mean that the strong programme is engaged with the study of idiosyncratic beliefs: rather, it focuses on scientific knowledge as institutionalised knowledge that enjoys a special authority in society. There are many specifically sociological questions to be asked in connection with scientific knowledge:

> Our ideas about the workings of the world have varied greatly. This has been true within science just as much as in other areas of culture. Such variation forms the starting point for the sociology of knowledge and constitutes its main problem. What are the causes of this variation, and how and why does it change? The sociology of knowledge focuses on the distribution of belief and the various factors which influence it. For example: how is knowledge transmitted; how stable is it; what processes go into its creation and maintenance; how is it organised and categorised into different disciplines or spheres? (Bloor 1991: 5)

Accordingly, for the strong programme, knowing reality is mediated by social circumstances, and this mediation requires empirical investigation. Bloor proposes four methodological principles for such an investigation. First, the sociological explanation of belief adoption should be causal: all causes, background beliefs, empirical evidence, technologies available have to be taken into account. Second, the explanation should be impartial. In the analysis, a sociologist of scientific knowledge is not allowed to favour one view under investigation over another, even though a particular theory might appear rationally more justified than its rival. Third, the sociologist has to consider the alternative views under investigation symmetrically: what has been regarded as rational or

irrational according to some specific standards, both have to be explained by similar kinds of causes. Finally, these principles have to be reflexive: if required they must obtain in sociology as well (Bloor 1991: 7).

This new methodology was designed both for the historical studies of science and for the analysis of contemporary scientific controversies. It has proven particularly successful as a meta-historiographical theory, since it has suggested a way for overcoming the notorious 'Whig history' — a presentist methodology for studying the history of science which imposes modern evaluative standards upon past science. On the other hand, the fact that controversy studies have become an independent sub-discipline within the SSK speaks for itself.

The main difference between the SSK and the mainstream philosophy of science in the early seventies was that the SSK approach is descriptive whereas the philosophy of science was mainly normative at that time.[1] However, the strong-programme authors were certainly inspired by the philosophy of Thomas Kuhn and late-period Ludwig Wittgenstein.

There are several other traditions in the SSK which also came into existence in the 1970s and 1980s. Methodologically, there are remarkable differences between them: some schools apply anthropological, ethnographic and ethnological research methods in their 'laboratory studies', others focus on the deconstruction of written texts, still others are engaged with the semiotic analysis of sign systems applied in the sciences. Most of the SSK schools and programmes, however, have been influenced by the theoretical core of the strong programme, although the four central tenets have been widely discussed among them. One might even construe the division into seeparate traditions of SSK via their respective stands on the four tenets, as will be seen in chapter 2.

Also, the critics of the SSK have made great efforts in order to show that the theses are either controversial or unachievable. So, e.g., Larry Laudan attempted to show that, on the one hand, it is an ambition of the strong programme to give a scientific analysis of science: Bloor, for example, refers to the natural sciences and thinks that the four tenets have always been applied there. On the other hand, it appears to be a pseudo-science, because the theoretical principles lack the empirical support (Laudan 1981)[2]. Martin Hollis and William Newton-Smith, e.g., have tried to show that relativism which is assumed by the strong programme, makes it self-refuting. Hence, the four principles with all their possible consequences could be seen as one of the central issues in the philosophical debates on the SSK.

The ambiguity of the concept of the social construction of knowledge has given rise to another kind of philosophical discussions on the SSK. Those pro-

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[1] In contemporary philosophy of science there are several schools which prefer a descriptive approach — naturalised epistemologies, naturalised philosophies of mind, etc.
[2] The methodological principles are seldom empirically proven in the sciences: rather, they must be theoretically justified.

voked by the bold rhetoric of the early laboratory studies, such as the *Laboratory Life. The Construction of Scientific Facts* by Bruno Latour and Steve Woolgar (1979), or *Constructing Quarks: A Sociological History of Particle Physics* by Andy Pickering (1984), seem to interpret the social construction as construction of *facts*, not as construction of *knowledge* (claims). Thus, e.g., Noretta Koertge, the editor of the journal *Philosophy of Science*, has collected a number of articles by outstanding mainstream philosophers of science into a volume entitled *A House Built on Sand: Explaining Postmodernist Myths about Science* (1998). With a few exceptions, most of the authors in this volume seem to regard the SSK as a philosophically idealist programme which attempts to discredit science. One of the main theses put forward in this book is that the SSK takes the scientists to be creating arbitrarily both the accounts and the reality.

This criticism is unfair because, with the exception of Woolgar's reflexivist programme, the SSK assumes only a social construction of the accounts, not one of the reality. Bloor explains the SSK position in his reply to a criticism by Gerald Holton, an outstanding historian of science. The strong programme has been asked: what about Mme Curie and the radium? Surely, Mme Curie discovered this element in nature and extracted it from there; she did not *construct* it. However, a social constructivist may explain the discovery in the following way. There were actually two competing schools which were both close to the discovery — the French and the British one. The difference between them consisted mainly in different styles of work. Basically this means that different *conceptual currencies* were in use: the Curie' tradition relied upon the French thermodynamics, whereas Rutherford and Soddie relied upon the theory of particles. What Mme Curie (re)constructed, was the concept of radioactivity and the concept of the element radium (Ra), which, in its turn, changed the chemists' views on the atom. This theoretical construction enabled her to discover this element in nature. So, nature is approached through a theoretical construction, located in a particular context of social interactions. (See Bloor 1997).

Another critical thesis often presented, and repeated by several authors in the aforementioned collection of essays, concerns the balance between the empirical evidence and the social circumstances. Some of the critics seem to regard the aspect of tradition in science either as a secondary topic (this has been, historically, the dominant view among philosophers of science), or as an aspect which gives birth do biased, irrational accounts of reality. In case of the most radical criticism, the SSK stance is construed as involving a total rejection of empirical constraints. Koertge, for instance, in one of her articles, identifies the 'social' aspect in science with wishful thinking (Koertge 1999), and then easily comes to conclude that the 'social' needs to be eliminated from science because this is the (social) norm accepted by scientists. Even if some scientists occasionally invoke metaphors or speculate hypothetically without proper reference to evidence, they are eager and willing to replace the metaphors with literal

terms, and the speculations with theories confirmed by evidence (Koertge 1999: 783).

Again, this criticism misses the point. To make the roots of the mistake explicit, the adherents of the strong programme have invoked the term 'zero-sum view' to label the position claiming that scientists' beliefs are caused *either* by cognitive *or* social facts (Bloor & Edge 2000: 158). This kind of dichotomization is generally characteristic to the recently re-opened debates between the traditional philosophy of science and the SSK. As noted above, this time the debates are being called the 'science wars'. It is Alan Sokal, an American physicist, who claims to have proven that the SSK endorses irrational accounts, because he succeeded in publishing an article which contained nothing but nonsense in an academic peer-reviewed SSK journal (Sokal 1996, for an overview see also Hacking 1999). Nevertheless, according to the strong programme diagnosis, arguments of this kind erroneously assume that the explanation of the scientists' belief adoptions must be either rational or social in its nature. Accordingly, the 'science-wars' argument claims that any rational belief adoption excludes any influence by factors that could be regarded as social.

Thus, many critics of the SSK accounts of science find that, if one succeeds in showing that empirical evidence is necessary for theory choice, there will be no room left for the social explanation. Other critics are ready to admit that, sometimes, this is social values rather than evidence which influences particular theory choices, but in most cases it is not so. Both these criticisms may be diagnosed as suffering from the 'zero-sum assumption'.

However, for the strong programme, culture and society operate like spectacles — through them people collectively perceive and grasp the world. Even the methodological norms for the science community are socially mediated and accepted. In addition to norms, skills and practical patterns of action are shared within the community, and transferred from one member of the community to another. At this point, the Kuhnian and late-Wittgensteinian influences on the strong programme could be recognised — the following argument sounds quite like the 'private-language argument':

> The point is that something can only become 'evidence' within the framework of an agreed theoretical understanding of nature. For the sociologist, the question is how that agreement was reached and how it is sustained. These (social) processes must be presupposed before talk of 'evidence' makes sense. […] It is the context of co-operation, interaction and coordination around these experiences that converts them into meaningful evidence and thus makes them available as an explanatory resource. This is a social achievement. (Bloor & Edge 2000: 159)

Accordingly, the strong programme does not appeal to 'purely social explanation', in the sense the critics tend to stress. The critics often attack a straw-man image.

Nevertheless, as to other traditions within the SSK, the criticism may be partially adequate, Harry M. Collins, e.g., has often emphasised what he calls relativism in methodology: in social explanation, nature has to be treated as non-existing (Collins 1981c). Nature is supposed to be explained through society, and reduced to society. Even if this view might still be interpreted as a 'spectacles-view', the way society is depicted by Collins will provide critics with what they have been expecting. Collins assumes society to be intelligible via commonsensical reflection. This somewhat naive attitude towards the knowability of society makes one to draw parallels with the early modern ideas of privileged access to one's own mental states. Due to the reflexivity thesis, this criticism can be rejected by the strong programme. Since Collins abandoned the tenets of impartiality and reflexivity, he has to face the criticism. To some extent, the concepts of alternation and meta-alternation enable Collins to parry the accusations in infallibilism: it is only for methodological reasons that one treats nature as non-existing, and society as entirely knowable via empirical studies (see section 2.4.3 in this essay). In a recent article, he has proposed a special meta-sociological theory of socialness where the socialness is defined as a basic human capacity, like the capacity of language (Collins 1998). Possible studies of socialness could resemble the studies of consciousness. Nevertheless, the concept of the 'social' still belongs to the issues which continue to be discussed within the SSK community.

The French school in the SSK — Bruno Latour and Michel Callon — argues against the (essentially) social explanation because, for them, it would be tantamount to a new asymmetry. In their opinion, the British social explanation exaggerates the inter-subjective relations at the cost of the research instruments, technologies and natural agents like laboratory mice or bacteria (see section 2.4.2 in this paper).

Also, the true post-modernists such as Steve Woolgar regard the strong programme approach, as well as the 'social realism' of Harry Collins, as naively positivist stands because their interpretations of science appear to be essentialist: both British schools seem to assume the independent existence of science as a research object, whereas actually it is created as such only through a representation process (see below, section 2.4.1).

In a slightly different form, the aforementioned 'zero-sum assumption' claiming the dichotomy of the 'rational' and the 'social', appears in connection with the application of the Duhem-Quine thesis in the SSK context. In the early stage of modern sociology of knowledge, many adherents of the SSK endorsed the following argument: as shown by Duhem and Quine, theory choices are underdetermined by data; therefore, some other than purely rational — or purely deductive-logical — criteria for choosing between competing theories have to be operating; hence, it is the social circumstances that determine the choices. The underdetermination was regarded by the sociologists of scientific knowledge as a simple matter of fact, so it was quite often used with the purpose of

proving the relevance of social explanation. At that time, this was also a topic in the debate between scientific realism and sociological relativism. Barry Barnes, for example — a strong-programme sociologist — declared that, since the Duhem-Quine thesis is correct, he is certainly a relativist, not a realist.

What the eager supporters of the underdetermination argument left unnoticed, is the hidden dichotomy of the 'rational' and the 'social' which is involved in this argument, if construed as the direct inference of social explanation from the lack of rational criteria. As will be analysed in more detail in section 3, the inference would rather imply that social explanation is relevant only in case there are no rational criteria available. Such a position is known as the 'arationality assumption'. This is a classical view held, for instance, by Imre Lakatos and Larry Laudan, that sociology is required to explain only those anomalies which philosophy is unable to explain by its methods of rational reconstruction. The arationality assumption contradicts some central ideas of the SSK, such as the symmetry and the causality of explanation.

In this case of application of the underdetermination thesis, the SSK itself has introduced the dichotomy of rational *vs.* social, and as far as the dichotomy is sustained, those who endorse it must be regarded as responsible for the 'science wars'. This would obviously minimise the perspectives for reconciling philosophy and sociology of science. However, the problem is still not insoluble, because again a third-way solution could be suggested. The third-way proposal is quite similar to the above-considered solution suggested by the strong programme in connection with the problem of constructivism: what counts as 'rational' justification should be seen as a result of cultural and social settings. Respectively, for introducing social explanation, one is not required to reject the 'rational' criteria for theory choice, rather, these criteria must be seen in the social context. From the third-way perspective, it does not make sense to deduce social explanation from rational underdetermination, because rational determination — or underdetermination — is seen as an issue for social explanation by its very nature. As Longino suggests in her version of social epistemology, rational justifications should be seen as local and epistemology should be seen as contextual:

> the epistemological problem is not determining which of a set of alternatives is always the superior one, but rather specifying the conditions under which it is appropriate to rely on a given set of assumptions." (Longino 2002: 206)

However, Longino also stresses that merely showing that there is a social dimension in cognitive processes is not sufficient for the conclusion that knowledge is social. In social epistemology, one should also be able to show how the social dimensions of cognition have resources for the correction of possible inappropriate exercise of authority, or shared biases invisible to a community otherwise critical (Longino 2002: 205). Therefore, as the strong programme account of science is a descriptive one, Longino does not regard it as a middle-

way approach between the two caricature extremes. In her analysis, however, she seems to rely upon the early studies by Barnes, Bloor and Shapin which endorsed the idea of explanation by social interests. If one had literally considered the social interests as the only reason for the scientists' adherence to one or another theory, the scientific practice would have become only a question of loyalty. As shown by the historical case studies conducted by Steven Shapin & Simon Schaffer — as well as by studies of Jan Golinski, a historian of science who has applied the methodology suggested by the strong programme — it is not a social reduction of this sort which is sought (see also section 1.2.3. and chapter 4 in this volume). Their purpose is rather to give a causal explanation: why, in a particular research arena, a scientist has accepted one theory and rejected another, why he or she has chosen a certain methodology, what kind of evidence and why was preferred, etc. Therefore, the explanation by interests does not necessarily mean an appeal to macro-sociological group interests; the interests should rather be taken as related to the micro-level of the research process.

When seen in the light of the above-considered criticism, (the accusations in idealism, irrationality, etc.), the strong programme sociology may certainly be characterised as a middle-way position. When seen in the light of social epistemology, the strong programme remains a sociological account of science, although very close to social epistemology.

Surely the strong programme cannot be criticised in the way Laudan (1990) does in his paper on the Duhem-Quine thesis of underdetermination. Laudan ascribes the strong programme and Mary Hesse the view that 'everything is either deductive logic or sociology' (see below, section 3.6.2).

Although the SSK authors may not approve the idea, it seems that their views have changed remarkably, if one compares their current position with their first programmatic works. When Barnes, in his paper from 1981, sincerely trusts that the Duhem-Quine thesis makes the distinction between realism and relativism, at present, I think, nobody would take such ideas seriously. First, the Duhem-Quine thesis should be distinguished from the radical underdetermination thesis: it is the latter rather than the former that makes the distinction in question. Second, scientific realism which was twenty years ago depicted as the main possible opponent to the SSK views, has by now rather become an ally. In *Scientific Knowledge: A Sociological Analysis* (1996), Barnes, Bloor and Henry come to favour a very strong version of metaphysical realism such as the one proposed by Kripke and Putnam. The authors point out that a relativist theory of knowledge requires a strong metaphysical thesis saying that the reality remains the same in spite of the varying accounts used for describing it. At the same time, the acceptance of the metaphysical realism by the strong programme, contradicts its central view of meaning finitism. The discussions between the SSK and the several versions of scientific realism continue over the issues finitism and rule-following. There are certainly other philosophical

problems in connection with the SSK which have not been taken up in the present essay. What I hope to have proven thus far is that the sociology of scientific knowledge deserves on-going philosophical scrutiny.

1.2. The plan of the argument

1.2.1. Relativism in the SSK and the problem of self-refutation

According to the classical, absolutist definition of the term, relativism involves a theoretical problem known either as the liar's paradox or the problem of self-refutation, or the problem of (in)consistency. In chapter 2, I shall first consider a case of self-refuting relativism, as discussed by Newton-Smith. After that I shall analyse some moderate versions of relativism which manage to avoid the aforementioned problems. Most of the philosophical criticism against relativism in the SSK is directed towards some supposed hidden assumptions — the implicit absolutist notions of truth and relativism. This criticism could be rejected, since such absolutist notions are not assumed in most versions of the SSK, and in those versions which do endorse such ideas, the issue of truth has been considered separately from the issue of the relative social context of knowledge claims.

Within the SSK community, the issue of relativism has given rise to debates about consistency. Those who stress the consistent relativisation of all beliefs, the position-consistency relativists, accuse others, who restrict relativism with some specific level(s) of belief, in inconsistency. On further consideration, one may find that the position-consistency relativism, in its turn — besides the problematic relativist regress it involves — is inconsistent in quite another sense: the empirical studies in this tradition are based on the hidden assumption that the claim for position-consistency is not really valid.

In this chapter, in addition to the analysis and classification of different SSK views, I shall also reach the conclusion that a version of moderate relativism in SSK, although inconsistent, can be reconciled with some versions of scientific realism.

1.2.2. The Duhem-Quine thesis and
the debates between the SSK and the philosophy of science

In chapter 3, the Duhem-Quine thesis (DQT) of the underdetermination of theories by facts together with its consequences which play a central role in the debates between the philosophy of science and the sociology of scientific knowledge will be analysed. Traditionally, the DQT has been taken as an argument

for relativist SSK. In this chapter, 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 the SSK.

The analysis of the argument of underdetermination 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.

1.2.3. The SSK as a meta-historiographical position

In chapter 4, I consider a significant case in the history of chemistry, the activities of an outstanding scholar of the period very close to those major changes in chemistry which are widely known as the Scientific Revolution. This is Herman Boerhaave, the great teacher from Leiden who lived and worked in the first half of the 18th century. According to internalist meta-historiography, Boerhaave deserves attention only as an early Newtonian scientist who applied mechanistic principles in chemistry. From this meta-historiographical viewpoint, his account of science would be regarded as progressive because it suits well into the logic of further development. He contributed to the progress of ideas. However, this meta-historiographical view is a thoroughly whiggish and presentist one, because it imposes today's perspectives of development upon past science. As an alternative meta-historiography, the externalism could be suggested. In this framework, one appeals to wider social, economic, political, cultural as well as metaphysical and religious factors in the explanation of past science. Curiously, even in this framework, Boerhaave may be regarded mainly as an adherent to Newtonian metaphysics. Does not this show that there is no unambiguous distinction between these two meta-historiographical positions? Being far from the naive belief that there is an entirely objective history, I rather tend to share the SSK perspective on historiography. I have chosen to follow the explanatory scheme proposed by Jan Golinski, a historian whose meta-historiographical position coincides with the view characteristic to the British sociology of scientific knowledge. Golinski together with J. R. R. Christie, suggests that the problematic classification be replaced with another. Respectively, he contrasts the *extrinsic* approach with the *intrinsic* one. In this latter account of science, the historian's task is to reconstruct the scientific practice so that, in addition to purely formal matters, didactic, instrumental, and communicative factors would be reported as well. Therefore, criticisms like the one advanced by Laudan (1996) which are directed against the social history — asserting that the latter ignores the cognitive aspect of science — are irrelevant. Even the wider context

of social and cultural settings would be important for the explanation, but only to the extent that is relevant for a particular problem. Thus the intrinsic meta-historiography consists in a local or micro-historiographical model. This would be opposed to the extrinsic perspective, according to which one considers particular cases only as the instances of general metaphysical, political, and social ideas. When considered in the light of the intrinsic stance, Boerhaave obtains a far more significant position in the history of chemistry than might have been expected within the more traditional explanatory scheme. The SSK historiography enables to reveal more of the instrumental, communicational, and didactic patterns than its traditional rivals, and it is certainly less whiggish[3]. Sometimes the critics have pointed to the problem that the SSK tends to entrench the existing myths by picking up the most outstanding scientists for the social study. Boerhaave's case proves that this is not true.[4] However, even this case study could serve as an example of the perspectives for reconciling philosophy of science and the SSK: the case shows that nothing is lost in the cognitive part whereas there is much to win through a more flexible explanatory scheme.

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[3] I am afraid that the author's or investigator's present-day-perspective will not disappear even via self-reflection, but it could be minimised.

[4] In most cases, the SSK rather brings forth relatively less known scientists who remain invisible in the whiggish history of winners.

2. THE PROBLEM OF CONSISTENCY IN RELATIVIST SOCIOLOGY OF SCIENTIFIC KNOWLEDGE

2.1. Introduction

With the acceptance of methodological relativism the sociology of scientific knowledge (SSK) has inherited a number of troublesome consequences that flow from relativism as such. The main and most inconvenient among them is the problem of consistency, known already from ancient philosophy. The contemporary applications of relativism, however, differ from the preliminary, purely logical version. Relativism in sociology of scientific knowledge can be seen as a tool, a method, invoked in certain cases, regarding particular purposes. Therefore one can distinguish between different variants of relativism in the sociology of knowledge, each one claiming relativism with particular strength and some area of application. This diversity of views involves endless discussions between the 'relativisms'.

My strategy in this chapter is first to consider the general, basically philosophical problem of inconsistency and self-refutation of relativism, and then to show how the problem of inconsistency applies to some variants of sociology of scientific knowledge. In this respect I shall devote special attention to the so-called strong programme. The last part of the chapter will be devoted to the debate on (in)consistency occurring within the community of the sociology of scientific knowledge. The debate between different branches of SSK concerns basically the radically relativist programmes of the so-called reflexivism, the programme initiated by Steve Woolgar, the so-called symmetrism as proposed by the Paris School, Bruno Latour and Michel Callon, and some modest varieties of relativism. Both the radical programmes claim consistency in relativism, relativisation of every single belief. The modest variants of relativism constrain consistency to certain dimensions or levels of relativist analysis. I shall consider what sorts of consequences follow from the seemingly consistent relativism. I shall question if the approach is really consistent and contrast the so-called position-consistency relativism[5] with modest relativism as it emerges, firstly, in the strong programme of the Edinburgh School (David Bloor, Barry Barnes and Steven Shapin), and secondly, in the empirical programme of relativism (EPOR) developed by Harry Collins and Steven Yearley.[6]

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[5] The terms 'position consistency' and 'inter-level consistency' come from Ingemar Bohlin (Bohlin 1995: 32).

[6] What counts as radical or modest relativism depends largely on the context of comparison. The empirical programme of relativism can be seen as a radical programme for its social reductionism. From a scientific realist's point of view social reductionism be-

One of my main purposes in this paper is to demonstrate how scientific realism can be reconciled with the modest relativism of the strong programme, and even more, how some form of realism[7] must be accepted as part of relativism to avoid the notorious paradox of self-refutation.

The main reason for the everlasting discussions on relativism consists in the fact that relativism as such involves a troublesome problem of self-refutation. The problem of self-refutation of relativism is known in the history of philosophy as the liar's paradox. It is the paradox of one and the same statement's being simultaneously both, true and false. The liar's paradox may have a number of troublesome consequences for relativism. Thus, relativism as such may turn out to be self-refutational in another sense — consistent relativisation of all beliefs, statements and judgements leads to the regress of relativism. A relativist cannot refute the statement '"relativism is wrong" is wrong' since this statement needs to be relativised too.

One way to abandon the paradoxical nature of relativism is to invoke certain constraints. But as soon as we restrict relativism in certain respects, or if we introduce some special conditions to a variant of relativism, we may become a target of severe criticism for inconsistency of relativism, i.e., such a restricted relativism is often taken to involve partial foundationalism, objectivism, representationalism, reductionism, etc. Thus, there seem to be two alternative inconsistencies to choose between, the first, which is partially non-relativist, restricted relativism and therefore inconsistent, and the second, seemingly consistent relativism which necessarily leads to self-refutation and regress.

Curiously, an unexpected kind of inconsistency appears in the actual studies of the position-consistency-relativism in SSK — their empirical research is often based on a controversial hidden assumption as if there were no regress, i.e., not all the beliefs need to be relativised, in spite of the theoretical claim of total relativisation.[8] Thus we may ask again, which kind of inconsistency to prefer,

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longs to radical skepticism. However, in comparison with symmetrism and reflexivism, EPOR belongs certainly to a modest, restricted kind of relativism for, as it will be shown below, EPOR does not require relativisation of all beliefs, whereas the other two kinds of relativism do.

[7]   It would be reasonable, however, to distinguish between scientific realism as a philosophical '-ism' with a number of different variants and realism as a position or a tendency through or over certain '-isms' which may apply to ontological, epistemological or, for example, moral issues claiming independent existence of something, either entities, ideas, attitudes, virtues or social relations. In most cases scientific realism combines realism in ontology with modest epistemological relativism and judgmental rationalism, to present it in Bhaskarian terms (Bhaskar 1978).

[8]   See, for example, Collins's and Yearley's critique on Latour's and Callon's application of symmetry in their empirical field work, which is in sharp contrast with the theoretical concept of generalised symmetry, or symmetrism. Collins and Yearley find a

either the inconsistency of restricted relativism, where the restriction is made by the rules of the game, by the framework conditions, as they are called, or the hidden inconsistency of the so-called consistent relativism.

2.2. Relativism in the sociology of scientific knowledge (SSK)

In an SSK interpretation, knowledge has to be relativised to knowers. Knowledge is seen as someone's knowledge in space-time location, in certain cultural, historical and social environment. According to such a view, knowledge is not made up purely of former knowledge by rational and logical inferences. Knowledge claims are erected in particular circumstances in the light of particular practical (research) tasks. This view on knowledge is radically different from the traditional rationalist methodology of scientific knowledge as it was developed in the methodology of research programmes of Imre Lakatos or in the critical rationalism of Karl Popper. In traditional methodologies of science, knowledge is delineated without the knower, science is outlined as research without researchers. Researcher as a subject becomes visible in the picture of science only when s/he acts irrationally, makes a mistake or ignores the internal "logic" of science. According to Lakatos, for example, a sociologist of science may explain only mistakes, deviations from the rational path, by reference to the external (social) factors that caused the scientist's error. Thus Lakatos prescribes internal rationality to sciences, that can be reflected, reconstructed, generalised and normatively criticised in philosophical methodology of science, and leaves sociology of science with anomalous cases in the history of science, events of less significance, with the so-called history of errors.[9]

Such a normativity is certainly not acceptable in the relativist sociology. The relativist sociology claims methodological neutrality, disinterestedness and epistemological finitism. This can be best illustrated by the example of the ideology of the strong programme.

In the strong programme, knowledge, and scientific knowledge in particular, is explained by its generative causal mechanisms. Whether rational or irrational, true or false beliefs, they all need to be considered in the context of their emergence.

The methodological principles for the strong programme have been formulated by Bloor in the shape of four tenets. Plausibly the most important tenet, the *symmetry* tenet, claims symmetry of explanation. Symmetry in explanation

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similar hidden inconsistency to emerge in Woolgar's application of reflexivity. Collins & Yearley (1992a & 1992b). In this essay see basically part 2.4.

[9]   See Lakatos (1971: 9), where he distinguishes between primary internal history of rational reconstruction of science with its internal 'logic', and secondary, external history that shows the deviations from mainstream history.

assumes that we treat both true and false beliefs equally. Both true and false beliefs are generated by their cognitive and cultural environments, they both have causes, therefore they need to be explained from the same *causal* basis, by reference to the same kind of causes.[10]

As one may notice, Bloor does not subscribe to the classical epistemological meaning of the term of knowledge. The classical concept of knowledge as defined in ancient philosophy involves the notorious problem of foundationalism — in classical accounts, knowledge has been defined as true and justified belief.[11] To avoid the possible consequences of foundationalism, Bloor constrains knowledge just to beliefs, collectively adopted beliefs: "[K]nowledge for the sociologist is whatever people take to be knowledge." (Bloor 1991: 5)

Such a definition, however, does not allow any arbitrary beliefs to count as knowledge:

> In particular the sociologist will be concerned with beliefs which are taken for granted or institutionalised or invested with authority by groups of people. Of course knowledge must be distinguished from mere belief. This can be done by reserving the word "knowledge" for what is collectively endorsed, leaving the individual and idiosyncratic to count as mere belief (Bloor 1991: 5).

In this way, interpreting knowledge as collectively endorsed beliefs, abandoning the troublesome normativity of classical epistemology, Bloor overcomes the asymmetry of traditional rationalist methodology of science, where rational and true statements, beliefs and theories were seen as self-explanatory, whereas errors and arational action were taken to be explicable by reference to their psychological, social, historical, cultural, etc. causes.

With the other two tenets — those of *impartiality* and *reflexivity* — the symmetrical approach becomes even stronger. The impartiality tenet emphasises the methodological neutrality of a researcher. The reflexivity claim means that if needed, all four tenets must be applicable on the strong programme itself.

Different SSK authors who make use of the tenets of the strong programme emphasise different particular tenets. One might say that David Bloor and also Harry M. Collins in his empirical programme of relativism (EPOR) claim symmetry to be a central principle. Similarly, Bruno Latour of the Paris School emphasises the symmetry tenet, however, the meaning of the concept has shifted in his use as will be considered in greater detail below. Collins points out that in contradistinction to the symmetry thesis, the causality and reflexivity theses can be seen as redundant, or perhaps even threatening to his empirical pro-

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[10] See Bloor (1991: 7). Originally the tenets were put in the following order: causality, impartiality, symmetry and reflexivity.

[11] About the problems of foundationalism see, e.g., Dancy (1985), or Everitt & Fischer (1995).

gramme (Collins 1981c: 215). Steve Woolgar in his turn puts a lot of effort into developing a 'really consistent' relativist programme, by playing up so-called reflexivism, revolving around its central thesis of reflexivity. According to Woolgar, neither the strong programme nor the Bath-relativism satisfies the conditions of reflexivity, and therefore they are all inconsistent in relativism. According to the Paris School, neither the strong programme nor Bath-relativism satisfies the symmetry tenet, and both approaches are therefore inconsistent.

2.3. The problem of self-refutation in relativism

Joseph Margolis has suggested a special term for an inconsistent, paradoxical, relativist point of view, according to which any statement, belief or sentence must be true and false at the same time, it is *protagoreanism* (Margolis 1986: xiii). Often, when the self-refutational character of relativism is under consideration, obviously protagoreanism is kept in mind. As an example, I consider here how William Newton-Smith deals with the problem of self-refutation of relativism.

   Newton-Smith poses the problem of self-refutation of relativism in a general philosophical manner. This implies quite a different approach to relativism than that of sociology of scientific knowledge. Newton-Smith finds that relativists are attracted by the variation of beliefs and opinions from one group to another, from one age to another, from one culture to another, etc. Different things are true for different social groups, they have different truths and so they live in different worlds. Despite the widely accepted belief that relativism has a particularly great explanatory power, Newton-Smith argues that in such explanations of varying beliefs relativism itself remains unexplained in quite many cases. Relativism of what? Relativism to whom? Newton-Smith finds the concept of relativism to be incoherent and the explanatory power of relativism dubious. Himself he defines relativism as follows:

> The central relativist idea is that what is true for one tribe, social group or age might not be true for another tribe, social group or age. If it were so, it would appear to license one to talk about the different tribes, social groups or ages as inhabiting different worlds, as relativists have been notoriously prone to do. Schematically expressed the relativist thesis is:
> something, *s*, is true for ψ and is false for ϕ (Newton-Smith 1982: 107).

Newton-Smith asks what exactly varies from one context to the other. What is that *something*? Is it a sentence, the truth of which varies? For example, it can be a sentence like 'grass is good to smoke', that can be true for a group of hippies and false for a farmer from Wales. Although the hippies and the Welsh farmer live in the same neighbourhood, their worlds are different, though not in

the substantial sense. In both cases the truth value of the sentence depends on its meaning and on how things in the world really are. The truth value of a sentence can change from a group to another when the meaning changes, but the state of real things in the world remains the same. At the same time, as soon as we take the varying *something* not to be a sentence but a proposition instead, the incoherence of relativism becomes obvious. One and the same proposition cannot be both true and false already by definition, be there different contexts or not (Newton-Smith 1982: 108).

Newton-Smith provides an example from the history of science which enables him to explain why and how relativism comes to incoherence. In Galileo's time, in the 17th century, it was widely believed that there are seven planets in the solar system. This belief was justified by the existence of seven 'windows' in the human head. According to a relativist, if seen from our point of view, such a justification will not obtain as rational justification, although, for them in the 17th century and earlier, it was a good and rational justification. *Them* and *us* are different. They did not make a mistake, they just applied other reasons for justification. Newton-Smith finds, however, that here the explanation should not end. Rather we should start the inquiry here. Then we would find that, according to the conceptual scheme valid in the 17th century, God created a harmonious universe. Harmony means that cosmos and the human being mirror each other, and therefore one can deduce the features of cosmos from the features of man. Thus there is really no difference between them and us:

> The difference between them and us is not a difference in what is a reason for something but a difference as to whether the conditions in question obtain. This fact which I will call *the conditionalization of reason* shows the reason is not relative and explains why it can appear to be so. We should not simply assume that different things are reasons for others. We should consider their web of belief. We are likely to find that difference is explicable in terms of difference in beliefs about what conditions actually obtain. This means that if we shared their beliefs about what conditions obtained we would tend to share their beliefs about which beliefs are reasons for which beliefs. (Newton-Smith 1982: 111).

Relativists, however, assume difference on two levels, according to Newton-Smith, difference in the beliefs about the world, and difference between beliefs. This diversity easily leads to incommensurability and skepticism. There is no further need to say that incommensurability involves self-refutation and turns relativism to incoherence.[12] Possibly this is the item Newton-Smith wants to point out — that relativism is incoherent for the incommensurability it involves.

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[12] See, for example, Harris (1993) about self-refutation of the incommensurability-relativism. Also Putnam has pointed out that incommensurability, despite the radical diversity of views, beliefs, opinions, statements, and worlds, always, presupposes a

Newton-Smith's argument can be read so that if we wish to save relativism we will have to avoid incommensurability. One way to escape incommensurability is to abandon relativism of truth in the traditional sense, and replace the sharp bipolarity of true and false with some more flexible, not mutually exclusive values, such as credibility and incredibility, for example, or those of plausibility and implausibility, as has been suggested by Margolis.

In a recent monograph Rom Harré and Michael Krausz indicate that the paradoxes of relativism may take a number of different forms. What is common to all these forms of paradox is that relativism needs a non-relativist, absolutist ground to make sense as a methodological programme. The general paradox of relativism has been conceived perhaps most dramatically by Richard J. Bernstein who offers a definition of relativism that he ascribes originally to Hans-Georg Gadamer: "For relativism, he thinks, is not only dialectical antithesis of objectivism; it is itself parasitic upon objectivism" (Bernstein 1982: 37).

As one could see above, Harré and Krausz define relativism as anti-absolutism too, but according to their view, this means that relativism may appear either as anti-objectivism, anti-foundationalism or anti-universalism. In addition to this classification, they distinguish between ontological and discursive variants of relativism (Harré & Krausz 1996: 4–7). Relativism may vary from one context to another, for instance, a variant of discursive relativism in a certain context may be anti-foundational but may allow universalism to some extent, etc. Thus, Harré and Krausz interpret relativism quite flexibly, relativism is capable of being combined with a number of epistemological and ontological positions. In addition, there are stronger and weaker forms of relativism.

I shall return to the 'weaker' variants of relativism after I have considered what kind of theoretical consequences may follow for the strong programme in SSK, from the paradox of relativism of truth which appears to be the strongest possible form of the self-refuting relativism.

It is namely the paradox of relativism of truth, the form of the paradox of relativism on which Newton-Smith and many other anti-relativists base their arguments against relativism. Harré and Krausz describe the form of the paradox as follows:

> (I)'"Truth is culture-bound" is true'
> Either 'I' is itself culture bound or it is not.
> (a) If 'I' is culture-bound, that is if it is true, there will be some cultural settings in which it is false, or in which it cannot be formulated at all.
> (b) If 'I' is not culture-bound, that is if it is false, then it will be true in all cultures.
> Therefore, if 'I' is true it is false, and if it is false it is true (Harré & Krausz 1996: 28).

God's Eye View — a universal point of view where from the diversity is defined — and is, therefore, inconsistent (Putnam 1982: 10–12).

It is obvious that relativism of truth, based on the traditional interpretation of truth and relativism, and truth relativism, involves self-refutation. However, if an epistemic, i.e., relativist concept of truth is assumed, the paradox will disappear, and the thesis (a) will be true[13]. Nevertheless, it does not follow from the classical paradox of relativism of truth, that relativism of knowledge in general would necessarily be self-refuting. The paradox works on the mutually exclusive concepts of truth and falsity. These mutually exclusive concepts are certainly not applied in the concept of knowledge of the strong programme. For this reason, the critics of the strong programme, endeavouring to show the self-refuting character of the programme, have been looking for a way of translating, i.e., transferring, the paradox into the conceptual scheme of Bloor's four tenets, to make the paradox bite also in the case of sociology of scientific knowledge.[14] For example, a possible result of such 'translation' might appear in the following form:

> (II) "Scientific knowledge is causally generated by its context" is causally generated by its context.

Thus, not only are the claims of the object science to be explained by their causes but also the meta-scientific claims must be explained by causal mechanisms. Similarly, one can construe three other forms of the paradox and assume self-refutation of the strong programme as a consequence, each of the paradoxes based on a particular tenet, either symmetry or impartiality or reflexivity. For example:

> (III) "Scientific knowledge needs to be explained symmetrically" needs to be explained symmetrically.

The argument against relativism in such 'quasi-translations' is built on the assumption that social causation of beliefs or the need of symmetric explanations themselves entail falsity of the beliefs. Bloor has summed up such attempts of criticism in the following passage:

> If someone's beliefs are totally caused and if there is necessarily within them a component provided by society then it has seemed to many critics that these beliefs are bound to be false or unjustified. Any thorough-going sociological theory of belief then appears to be caught in a trap. For are not sociologists bound to admit that their own thoughts are determined, and in part even socially determined? Must they not therefore admit that their own claims are false in proportion to the strength of this determination? (Bloor 1991: 17)

It is easy to see that the concepts of 'causally generated by its context' (II) or 'symmetrically explained' (III) cannot be identified with the concept of truth as

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[13]   The thesis (a) might, accordingly, appear rather as a definition for such relativism.
[14]   See, for example, Hollis 1982.

it was seen to be relativised in (I); neither is relatedness to a context equal to falsity. The 'translation' as such has failed. Even if one were to start from the other end and try to find an opposite term to 'context-bound', it could be 'universal', and this is certainly not an exclusive opposite to 'context-bound', at least not in the language game of Margolis, Harré and Krausz'. As Harré and Krausz (1996: 210) put it, the opposition does not need to be one of contradiction. 'Universal' and 'context-bound' may be incompatible, but the truth of some belief being context-bound does not make its negation necessarily false. So, according to Harré and Krausz, it is important to note that relativism is paradoxical only in the case of supposing the opposition between universal, or absolute truth, and relativism of truth, which indeed would involve self-refutation of relativism. In most of the actual discussions on scientific beliefs, this opposition is not involved. Instead of the absolutist concept of truth, often either pragmatic or epistemic concepts are assumed.

And even if one still assumes the absolutist concept of truth, it does not necessarily contradict knowledge relativism when a fallibilist theory of knowledge is favoured. In such a case, truth must be seen as a semantic relation between the epistemic entities like beliefs, statements, theories, models, etc., and reality. Independently of the truth value of the statements — which depends purely on the state of affairs in reality — the construction of knowledge may been regarded as relative to their social and cultural settings (see Niiniluoto 1999: Ch. 7).

Thus, in the 'translations', it seems that, contrary to the expectations of the anti-relativists, one may actually succeed in abandoning the paradox of relativism of truth because, in the statements (II) and (III), no bipolar truth values are assumed. Those who wish to use the argument of the paradox of relativism of truth against the strong programme, draw an extensive but still inadequate conclusion from (II) as if the causal nature of beliefs automatically made them false. Bloor has diagnosed the flaw in the arguments of his critics as follows: "This premise may be in the extreme form that any causation destroys credibility or in the weaker form that only social causation has this effect" (Bloor 1991: 17).

This conclusion would be fully adequate only if 'radical translation' from (I) to (II), or to (III) were possible. Since it is not, i.e., the three theses are not identical, the strong programme cannot be accused of self-refutation.

Mary Hesse suggests an even stronger argument against the critics of the strong programme. She finds in connection with the paradox of relativism of truth that, relativism of truth, nevertheless, involves neither incoherence nor self-refutation of sociological relativism. According to Hesse, the usual argument against the strong programme may run as follows:

> Let P be the proposition 'All criteria of truth are relative to a local culture; hence nothing can be known to be true except in senses of "knowledge" and

"truth" that are also relative to that culture.' Now if P is asserted as true, it must itself be true only in the sense of 'true' relative to a local culture (in this case ours). Hence there are no grounds for asserting P (or incidentally, for asserting its contrary) (Hesse 1980: 42).

Hesse finds that such an attempt of refutation of relativism is obviously fallacious "for it depends on an equivocation in the cognitive terminology 'knowledge', 'truth', and 'grounds'" (Hesse 1980: 42). Certainly it is incorrect to ask for absolute grounds for asserting either P or (I), or (II), or any other form of the paradox, or whatever statement, belief or opinion. Consequently, there is no conclusive argument for accepting the strong programme, according to Hesse too.

Hesse describes the conceptual change in epistemology proposed by the strong programme, as follows:

> [W]hat the argument from sociology has done is to suggest that we *shift* our concept of 'knowledge' so that the alleged refutation becomes an equivocation. This shift is the essence of the strong thesis: knowledge is now taken to be what is accepted as such in our culture (Hesse 1980: 42).

Thus the relatedness of knowledge to culture leads, instead of self-refutation, to self-reflection.

Whereas Hesse, a realist philosopher coming from a probabilistic tradition, endorses the idea of contextualisation of the concepts of truth and knowledge, the pragmatic realist Joseph Margolis suggests one should get rid of the strict bipolarity of truth and falsity in the explanation of scientific knowledge to save relativism in that way. The strategy of Margolis is to treat the argument that relativism necessarily leads to inconsistency as a purely technical one. He finds that:

> All we need do is *restrict* the admissible values — values such as "plausible" and "implausible", disallowing "truth" and "falsity" — so that the offending contradictions and self-refutations are precluded (without, of course, needing to disallow contradiction or self-refutation in other ways); then we should be home free (Margolis 1986: 66).

In principle, the views of Margolis and Hesse are quite close, for they both argue for anti-absolutism, and they both suggest constraints be put on the concepts of truth and falsity relative to some restricted framework, open for further re-estimations in varying contexts, i.e., they reject universal truth. Thus, Bloor's relativisation of scientific knowledge can derive support not only from Hesse but also from Margolis's form of restricted relativism.

In addition to the recommendation of eliminating the bipolar truth values, Margolis suggests another practically important move for restricted relativism. To be able to avoid radical incommensurability, and therewith self-refutation and skepticism, one needs to separate relativism of purely linguistic structures,

such as sentences and propositions, from the relativism of scientific beliefs, models and theories — in the former case we are bound to bipolarity of the truth value, but not in the latter case (Margolis 1986: 112–16).

Nevertheless, Harré and Krausz point out a problematic item in Margolis's strategy. They find that

> However, the abandonment of bipolarity alone does not lead to relativism. One must add a commitment to the contextual dependence of decisions as to where on some continuum of degrees the relevant properties of an object lie. Only then do we have genuine relativism (Harré & Krausz 1996: 147).

This is certainly so, but for our purpose which is to demonstrate that relativism as such does not necessarily need to be self-refuting when constrained, it would be even enough to accept only Margolis's minimal idea of the abandonment of the bipolar values of truth and falsity. It is so because our point of departure already is in relativism.

Hence it seems that, as soon as we have abandoned the relativism of truth paradox —as we have in a number of ways in the restricted relativism of the strong programme in SSK — the problem of self-refutation is resolved. However, as I referred to Harré and Krausz above, they mention some other forms of more or less internally problematic issues in relativism. Let us consider the two theses of relativism, one of which is called the thesis of ontological independence: "Entities, states, experiences and so on exist independently of culture for the fact of cultural diversity vis-à-vis these entities to show up" (Harré & Krausz 1996: 26). And, the other thesis is known as that of transcultural intelligibility: "Descriptions of some entity, state, experience etc. must be universally intelligible, if it is to be possible to realise that the entity, state or experience being described is being treated differently in different cultures" (Harré & Krausz 1996: 26). In Harré's and Krausz's vision, these two assumptions or theses actually make relativism tenable. The theses enable relativism to escape regress and self-refutation.

Is Harré's and Krausz's first thesis of relativism still not paradoxical? [15] According to this thesis relativism needs some certain ground, independent existence of entities that makes the diversity of views on these entities, experiences and states of affairs possible.

For instance, to be able to compare symmetrically two different paradigms in chemistry we need to assume the existence of a common ground for both of them in reality. For a comparison of the phlogiston theory of combustion with the atomistic oxygen-theory of burning, one needs to assume a really existing

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[15] Woolgar finds this to be the irony within the strong programme account that divergent views on a scientific object are taken to be about the same real object. Latour, for his part, finds that we can assume the access neither to the objects of relativised views nor to the subjects (social actors) whose views are relativised.

entity to correspond to both, the concept of phlogiston, and respectively to some concept in the atomistic theory, electron, or whatever it may be dubbed. Not even a one-to-one translation from one conceptual scheme to another is required. It would be enough if we could learn and translate from one scheme to another, in principle. But does the assumption for such interpretation of the grounds not involve the acceptance of foundationalism and objectivism in relativism? Is it not a variant of self-refutation of relativism, or at least a sort of inconsistency anyway? As Bernstein has demonstrated (1983: 8), the content of relativism is anti-objectivism, and this is what Harré and Krausz claim about a variant of relativism. If we now come to admit that relativism needs to accept some objectivism, what else may it be than inconsistency?

Similarly, we may see a problem in the hypothetical paradox of transcultural intelligibility, since, on the one hand, we have — according to relativism — plurality of cultures and contexts as opposed to universalism, on the other hand, relativism assumes universalism to the extent making the existence of different cultures knowable.

It is quite a common praxis to interpret the theses as paradoxical, and in this connection to delineate relativism as a variant of skepticism:

> The interesting charge advanced (by the would-be opponents of relativism), therefore, is that, although it opposes objectivism, untenable in any case, relativism is committed to the thesis that *only* objectivism could preclude skepticism. Hence, relativism is committed to skepticism, in particular to a version of the (radical) incommensurability thesis (to the effect that claims drawn from different "paradigms" cannot be treated as cognitively competing claims) (Margolis 1986: 70).

Therefore, the first task, according to Margolis, is to reject the mutually exclusive definitions of relativism and objectivism, the definitions like the one suggested by Bernstein. Margolis proposes that:

> The counterstrategy is at once clear: construe relativism as (indeed) opposed to objectivism, disallow the skeptical reading (which the sanguine opponent of both objectivism and relativism — so styled — already insists is a viable option), and reinterpret relativism as a thesis about science and rational inquiry viewed in terms of just those conditions. The importance of the relativistic alternative (thus interpreted) lies in this: theories of science and rational inquiry may (viably) oppose both objectivism (or foundationalism) and skepticism (or incommensurability or the like); and *yet they may still be usefully sorted as favoring and opposing a refurbished relativism* (Margolis 1986: 70–71).

The moderate version of relativism as Margolis describes it, must be separated from all forms of irrationalism, cynicism, nihilism, anarchism, skepticism, and incommensurability. At the same time, it is a version of relativism that rejects

objectivism, universalism, foundationalism, essentialism and logocentrism. Naturally, Margolis (1986: 72) recommends to avoid the exclusive option between objectivism and skepticism, or the like. For such relativism, the paradoxes could be resolved, even more, the theses of independent existents and intercultural intelligibility are taken to be necessary for relativism.

Margolis's moderate relativism suits well with the multi-level relativism of Harré and Krausz. Hence relativism can be combined with both general historicism and scientific realism for the reason that we may find subtly presented in the following passage by Harré and Krausz:

> Different aspects of the world are available to different kinds of creatures, in so far as their sensory systems differ, and to different groups of human beings in so far as they are differently placed and differently equipped. In this sense knowledge of the world tends to the relative. But all such aspects are aspects of one and the same world, and in that sense knowledge of the world tends to the absolute (Harré and Krauzs: 224).

Consequently, relativism assumes a modest variant of realism, such as 'residual' realism or 'single-barrelled' realism.[16] Modest realism in the shape of the thesis of independent existents saves relativism from the vicious circle of bipolar opposites of objectivism and skepticism. The relativist regress can be stopped only when some absolutism in the shape of ontological realism and cultural universalism is taken to be acceptable. Obviously this conclusion entails important consequences for the strong programme and other variants of restricted relativism.

Although Margolis's modest relativism seems to suit Bloor's strong programme, Margolis himself regards the epistemologies of Bloor, Collins, and SSK in general, as social reductionist and skepticist ones. There seems to be a frequently supported but still erroneous view among philosophers and natural scientists that the attempts of sociological interpretation of knowledge necessarily lead to social reductionism, or to a kind of idealism, or to skepticism.[17] Without developing the argument at greater length here, I just refer to David Papineau, a philosopher who has examined the issue and reached an atypical conclusion for a philosopher of science in the 1980s asserting that the sociology of scientific knowledge, relativist or not, does not necessarily involve skepticism:

> the new sociology of science does nothing to show that scientific practice is not generally reliable for generating true theories. It may well show that scientists are often swayed by prejudice, ambition and other ulterior motives. It may well show that the internal mental motivations of scientists are no dif-

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[16] Both terms are often used by Barnes, see especially Barnes 1992: 137, and both the terms are also applied by Harré and Krausz.

[17] See, for instance, a recent review article of N. David Mermin 1998.

ferent from those of the general public. But it by no means follows that the overall structure of scientific practice is not reliable for truth (Papineau 1988: 51).

The possibility for reconciliation of scientific realism as basic ontological position and sociological knowledge relativism appears explicitly in the variant of scientific realism suggested by Ilkka Niiniluoto (1987: 137) who makes a clear distinction between the semantic concept of truth and the procedure of truth(making)[18]. The semantic concept of truth concerns the field or area of entities postulated by a conceptual scheme (scientific theory, model, etc.). The procedure of truth (knowledge) concerns the process of gathering credibility. Although the procedure could be interpreted in narrowly epistemic terms, it may allow sociological approach as well. Scientific realism in philosophy has been mainly engaged with the ontology of the postulated entities and theories. Obviously there is no controversy, no conflict between the two dimensions — ontology and the study of the context of knowledge-claims. I think, they could be seen as complementary. In a recent essay on critical scientific realism, Niiniluoto, when referring to Peirce, admits that: "Knowledge is a *social* product of a community of investigators." (Niiniluoto 1999: 94)

Considering Margolis's criticism on Bloor, we must admit that it is not fully relevant, for there is no reason to accuse Bloor of skepticism — without further explication what is meant by skepticism[19] — when Bloor (1991: 37) admits even the correspondence theory of truth: "There is little doubt about what we mean when we talk of truth. We mean that some belief, judgement or affirmation corresponds to reality and that it captures and portrays how things stand in the world". At the same time it is quite clear that Bloor is not a metaphysical realist[20] (or objectivist), he adheres to fallibilism in epistemology:

> We never have independent access to reality that would be necessary if it
> were to be matched up against theories. All that we have, and all that we
> need, are our theories and our experience of the world; our experimental re-

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[18]   Elsewhere Niiniluoto has indicated that in the case of *truthlikeness*-realism, i.e., critical scientific realism, the cultural, political, social and other contextual determinants of knowledge can be taken into account. See Niiniluoto 1991, 1995, and especially 1999, Ch.-s 7 & 9.

[19]   If Margolis means by skepticism, e.g., fallibilist epistemology, his claim is correct. Nevertheless, it seems that it rather is radical skepticism he keeps in mind.

[20]   By 'metaphysical realism' I mean a view, similar to the so-called *double-barrelled realism* (See Barnes 1992) in its strongest possible form, the view that assumes both the independent existence of objects in reality and our ability to obtain true belief of these objects. This assumption, in its turn, involves necessity of the 'ready-made-world' with the ultimate number and structure of the existing entities. The position can be formulated only as a theoretical extreme, for there are no adherents precisely to this view.

sults and our sensory-motor interactions with manipulatable objects (Bloor 1991: 40).

In a manner quite similar to Margolis, Bloor finds that the universal concept of truth as defined in the correspondence theory is not suitable for practical explanations. As a scientist, one rather has a point of view, which is given preference with regard to a research issue because one does not have independent access to reality, and one does not know the (absolute) truth. It is natural to sustain one's own fallible beliefs rather than to have no stand at all. Anyhow, a choice between possible varieties of views must be made, since one cannot hold more than one view of the item at a time — to make sense, one has to speak in only one language at a time.[21] Sometimes the view preferred on the basis of the evidence at hand is being called truth. This shows that the term 'truth' may be applied in different senses. Therefore, Bloor suggests putting the question about truth in another way, focusing on the functions of truth. He distinguishes between three functions of truth — discriminative, rhetorical and materialist functions. The first, the discriminative function of truth, refers to the need of sorting our beliefs into plausible and implausible ones. Often the truth values, 'true' and 'false' play such a pragmatic role of distinction. The rhetorical function of truth serves as a means of criticism and convincing or persuading others. Bloor characterises the third, materialist function as follows:

> All our thinking instinctively assumes that we exist within a common external environment that has a determinate structure. The precise degree of its stability is not known, but it is stable enough for many practical purposes. The details of its working are obscure, but despite this, much about it is taken for granted. Opinions vary about its responsiveness to our thoughts and actions, but in practice the existence of an external world-order is never doubted. It is assumed to be the cause of our experience, and the common reference of our discourse. I shall lump all this under the name of 'materialism'. Often when we use the word 'truth' we mean just this: how the world stands (Bloor 1991: 41).

Such an attitude coincides with what is called *objectivity* by Margolis (1986: 112–113)[22]. Objectivity can be seen as a weak form of objectivism. The thesis of independent existents, objectivity, and materialism seem to be the sides of

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[21]   It is, nevertheless, possible to switch between different views or languages, it is possible to interpret a single sentence or symbol from different perspectives, but it is not possible to claim or state different things at one and the same moment and remain consistent at the same time.

[22]   Margolis makes a clear-cut distinction between objectivity and objectivism. The former does not involve the latter. Objectivity is permissible for relativism whereas objectivism is not.

the same coin, and on this basis, it seems possible to reconcile realism and relativism.

Thus I can assert here in concluding this section that, the paradoxes of relativism and respective kinds of the problem of consistency can be resolved by introducing certain constraints to relativism. One way for that, is in stating at the outset of an account, that in this particular application of the concept of truth, the framework of truth relativism, or epistemic theory of truth is assumed, either of them being opposed to the classical concepts of truth and relativism. Another more general prescription may be simply avoiding the bipolar structures, which involve the liar's paradox. For Harré and Krausz, as well as for Margolis, and also for Bloor and Collins, relativism can be made to work in this way. In this way they seek the explanation of varying, sometimes controversial scientific views.

2.4. Relativist regress, normativity and the problem of consistency

Hilary Putnam has referred to a form of the paradox of relativism not yet considered at greater length here. According to him, a relativist cannot assume a normative stance with regard to any belief, statement, action or whatsoever. A relativist cannot assert:

"Relativism is wrong" is wrong.[23]

Although any relativist wants to save relativism and assert relativism's being right and valid, an adherent of hypothetically consistent relativism in SSK should stay neutral, disinterested, symmetrical and reflexive also about her/his own claims. It is easy to see how another variant of the paradox of relativism may follow from that. However, it is also easy to see that Putnam is talking about the abstract variant of relativism considered above quite in the same manner as Newton-Smith, focusing on relativism with exclusive bi-polarities. Therefore, there can be a similar solution to this possible form of paradox of relativism, a solution comparable with the form considered above in connection with Margolis's attempt to reconcile realism and relativism. To abandon the paradox, Margolis (1986: 68) introduced another restriction to relativism: "Relativism should not be construed as precluding comparative judgements of the usual sort and range (for instance, of better or worse, or of more or less adequate) conceded within theories that do subscribe to bipolar truth values." Thus, in Margolis's modest relativism, normativity, i.e., universality, is permis-

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[23] See Putnam 1982, where the argument comes from, here presented in my reformulation.

sible to some extent, or in some respects. Thus the paradox is supposed to be overcome. In sociology of scientific knowledge this variant of the paradox of relativism is neutralised by introduction of separate levels of relativism. Karin Knorr Cetina and Michael Mulkay distinguish between epistemic and judgmental (levels of) relativism:

> Epistemic relativism asserts that knowledge is rooted in a particular time and culture. It holds that knowledge does not just mimic nature, and insofar as scientific realism wishes to make such a claim, epistemic relativism is anti-realist. On the other hand, judgmental relativism appears to make the additional claims that all forms of knowledge are 'equally valid', and that we cannot compare different forms of knowledge and discriminate among them (Knorr-Cetina and Mulkay 1983: 5).

Naturally, this last possible consequence of relativism would not find support among SSK analysts. Following Knorr-Cetina and Mulkay, many others have attempted to abandon judgmental relativism by separating normative/judgmental stratum from the other dimensions of analysis, thus saving relativism from a variant of self-refutation.

The issue of normativity in sociology of scientific knowledge has recently been taken up again by a group of Australian sociologists, Pam Scott, Brian Martin and Evelleen Richards, who criticise relativist methodology for its illusory neutrality and hidden normativity.[24] They do not demand an equal position for any kind of knowledge, but rather they try to point out that the principles of symmetry and neutrality are not really valid in SSK analyses, for, on the one hand, analysts certainly have their preferences, and on the other hand, even if they do not, symmetrical analysis gives advantage to one of the analysed parties, to which one, depends on the social context. In short, the argument consists in reference to inconsistency of relativist SSK.

Nevertheless, a relativist may argue that the claim of consistent relativism concerns only one certain level, the object level. In science studies it is usually the epistemic or epistemological level which is relativised, i.e., epistemic symmetry is claimed. The judgements, evaluations, and questions about social significance of a cognitive activity belong to another, meta level or levels, where it is permissible to take sides and be normative, e.g., to admit that one's own approach is more justified and better than its alternatives for certain research purposes. The picture becomes even more complicated when one considers the procedure of symmetrical analysis in detail. The symmetry principle assumes that an analyst should be able to switch between two radically different episte-

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[24] Scott, Richards and Martin 1990 find that "an epistemologically symmetrical analysis of a controversy is almost always more useful to the side with less scientific credibility or cognitive authority. In other words, epistemological symmetry often leads to social asymmetry or non-neutrality" (1990: 490).

mological systems (conceptual schemes, models, theories, beliefs, etc.) under investigation. For this epistemic switch Collins and Yearley have introduced a term 'alternation' into SSK as a loan from Peter Berger's *Invitation to Sociology*. Alternation means that sociologists exchange different frames of reference, move between different 'worlds', for example, between two different models in physics, where gravity waves are supposed to exist in one of them and not to exist (or not to be detectable by the given method) in the other (Collins 1981a). Thus alternation can be seen as the method or reification of symmetry.

Another kind of alternation occurs when a sociologist needs to switch between the conceptual scheme of the 'world' under investigation and of the other, her/his own taken-for-granted-world, either her/his own professional or common sense beliefs. It may be called 'meta-alternation' after Collins and Yearley. For instance, a description of a scientific laboratory may be given purely in terms of the natural sciences — there are columns, detectors, sample collectors, amplifiers and recorders with a chromatogram in a lab where gasliquid chromatographic analysis is being done. The same laboratory may be described in terms of social science as a place where scientific authority gathers support, or, e.g., in common sense language — a room full of computers, tables with different tubes, pipes, boxes, altogether smelling badly. Normally scientists would subscribe to the first description, some SSK analysts would subscribe to the second, and more radically minded social constructivist or position consistency sociologists of scientific knowledge would agree with the third description of the same laboratory.

When considered purely epistemologically even these 'worlds', one on the ground level and the others on different meta levels, could be seen symmetrically, if reflexivity is invoked. This is what a branch of SSK, the so-called reflexivism claims to be the necessary and central part of investigation. According to reflexivism, alternation in symmetrical analysis is often accompanied by 'meta-alternation' taken to be at least a problematic issue if not a failure. The general argument against the strong programme and other methodologies of symmetrical analysis in this context consists again in accusations of partial objectivity and foundationalism. Collins and Yearley (1992a: 302) point out that if inadequately understood, the over-emphasised problem of meta-alternation may entail a new way of knowing nothing in sociology:

> In spite of this achievement, all of us, however sophisticated, can switch to modes of knowing that allow us to catch buses and hold mortgages. We all engage as a matter of fact in what we might call "meta-alternation". Our argument here is that social studies of science ought to erect meta-alternation as a principle, not treat it as a failing. To treat it as a failing is to invite par-

ticipation in an escalation of skepticism which we liken to the game of chicken; in this case the game is epistemological chicken.[25]

The idea of Collins and Yearley is quite simple, their aim is to explain something by something else, i.e., to explain scientific knowledge by its cultural and social conditions. This idea involves acceptance of social reality in the shape it is given by current social theories and our common sense.[26] According to position-consistent relativism of reflexivism and symmetrism, Collins and Yearley should, for the sake of consistency, reflect also on their own conceptual scheme in relativist manner, but they do not, though they may agree that it would be possible in principle. Thus their social realism is seen as inconsistent.

In the following three sub-sections I shall consider the arguments from reflexivism and symmetrism, and contrast these with social realism. I make use of a sort of *reductio ad absurdum* argument. When it is impossible to give a conclusive argument for any of the considered variant of relativism in sociology of scientific knowledge, for all they seem to be somehow inconsistent, be it inconsistency in the sense of partial absolutism (realism, materialism, etc.), or inconsistency in the sense of relativist regress, following from the requirement of relativisation of every belief in the so-called position-consistency relativism, then it is still possible to show that the latter variant of relativism is inconsistent also in its empirical applications, i.e., its method does not really work, and therefore the alternative view, that of constrained relativism, is better endorsed and preferable.

For this analysis it is less important exactly which variant of constrained relativism to consider, but since the concepts of alternation and meta-alternation have been elaborated in social realism, I, therefore, devote more attention to social realism of the Bath School here.

One may still wonder, how can social realism be a variant of constrained relativism? As seen from a natural scientist's point of view both the variants of position consistency relativism and social realism are more or less socially reductionist, for they take empirical (natural) scientific factors to play minimal role in the sciences. Nevertheless, when considered with respect to the consistency problem, social realism can be regarded as a variant of restricted relativism, and thus taken to be compatible with the strong programme, and respectively incompatible with symmetrism and reflexivism.

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[25] The metaphor of *epistemological chicken* refers to a game 'chicken' which consists in dashing across the street in front of cars. 'Chicken' is the person who crosses the street first, the winner is the one who succeeds to cross the street as the last person. In the game of 'epistemological chicken' the winner would be the one who succeeds reflecting on one's own views longest.
[26] See Collins 1983: 87–95.

2.4.1. Reflexivism

According to Steve Woolgar, inter-level inconsistency, i.e., the alternation between different levels and positions, entails a difficulty he calls the 'Problem of Representation'. This is closely related to a variant of objectivism. Woolgar (1983: 243) distinguishes between three different views on the problem of representation:
1. a reflexive[27], naive realist position, which assumes scientific representations truly to picture independent reality 'out there' as it is in itself;
2. a mediative position, which takes social environment to mediate reality in representations, thus endorsing the idea of the parallel existence of different representations of a piece of reality;
3. a constitutive position, where reality is seen as created by representation.

Relativist sociology of scientific knowledge, or social constructivism, as Woolgar applies the term to both, the strong programme and Bath-relativism, is related to the mediative position. Woolgar notes that sociologists are well aware of the fact that selection between theories cannot be made on the ground of facts of reality because of the underdetermination of theories by data. Rather theory choice is based on a social convention. At the same time, Woolgar points critically to a shortcoming of the mediative position — the mediative position assumes that there are ready-made theoretical alternatives, ready-made representations waiting to be picked up by the scientists of different communities. Woolgar comes to find ironically that, according to such a view, there must also exist a ready-made image of science, a ready-made image of what it is to be a scientist, and respectively the images of cultures, communities, etc. which *de facto* mediate reality for the sciences. Woolgar (1983: 251) notes with sarcasm that the mediative position implicitly assumes one and the same reality to ground different representations of it.[28] This makes Woolgar to conclude that, although the strong programme has recognised the conventional basis of the sciences, it still holds on the methods known from the sciences, and therefore, there is no essential shift from a variant of earlier asymmetric Mertonian sociology of science to the strong programme (1988a: 50–51).

The constitutive position, initiated by Woolgar, involves (re-)inversion[29] of the object and representation — the research object must be seen as generated

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[27] It is essential to note that 'reflexive' here does not have any connection to 'reflexivity'.

[28] As we could see above, this is a basic thesis for relativism, the irony would be suitable only in the case if such an ontologically realistic view were exclusively opposed with relativism. Since it is not, there is no reason for irony.

[29] According to Woolgar, inversion of the object and its representation is a feature of scientific research. As result of scientific practice, hypothetical entities turn out to be objects which are believed to act as causes of representations. See Woolgar 1988a: 54.

by representation and not the other way round. As a result of the re-inversion, the social network obtains in addition to its mediative role also a role of generator of the object. The latter, generative role, is important to keep in mind in every analysis of knowledge, especially in the case of sociological analysis. No *a priori* distinctions can be made between accounts and reality, accounts are the reality. Thus, even social reality is being created by the sociological accounts. The only way to avoid inconsistency, according to Woolgar (1988a: 93), is to abandon unreflexive representationalism, since scientists never face nature as such in their research, and the same applies to the sociology of scientific knowledge — sociologists never face science as such but only theoretically constituted representations, etc. This is why more reflexivity is needed, according to Woolgar and Ashmore:

> The general issue of reflexivity emerges in the specific area of the social studies of science, once it is recognised that the same point can be made about the knowledge produced by SSK. Its determinants, results, insights, and so on are themselves the contingent product of various social processes (Woolgar & Ashmore 1988: 1).

Woolgar and Ashmore actually do not have an ambition to solve the problem of inconsistency of relativism, since they do not believe in the possibility of any ultimate solution of any problem. They just criticise the other branches of sociology of scientific knowledge for the controversy between their claims of relativism in the explanation of the scientists' activities and realism in their self-reflexive views.

From Woolgar's point of view, the real issue is the lack of relativism in science studies. In his reply to criticism from Collins and Yearley (1992a) where the two critics classify Woolgar's reflexivism as a post-relativist approach, he asks: "When did we finally get to relativism?" (1992: 330). According to Woolgar, one should only start with relativism. For this purpose he finds the *new literary forms* to serve as a suitable method. In the new literary forms, the author's *alter ego*, a second voice which is suppressed and ignored in ordinary cases, comes to serve the purposes of reflexivity (see Woolgar 1988b). The second voice is meant to point at the representations given by the first, author's basic voice, to ask what exactly is the generative ground for the given claim, etc.

The danger of relativist regress does not seem to threaten him because the problem of relativist regress belongs to another world — the world of formulae, regularities and logic, the world which is of no interest for a reflexivist. A reflexivist is more interested in dismantling myths, traditions, and certain grounds: "Reflexivity and actor-network theory offer ways of further challenging the preconceptions and assumptions of (what are now) current orthodoxies" (Woolgar 1992: 339).

However, there is a controversial precondition that makes reflexivity tenable as the empirical programme — reflexivity works on the condition of reflexivism being invalid. The concept of reflexivism cannot mean anything else but generalised reflexivity. Thus, a consistent reflexivist should be reflexive about every single belief, every single view, even about the view about reflexivism itself. Such a position is obviously regressive if not controversial. In the empirical studies of Woolgar, one can hardly find reflexivism at work. For instance, in a description of ethnomethodological fieldwork he declares that "The main rationale of this kind of work is that this process of collection and observation provides the basis for an authentic picture of what actually goes on in the laboratory" (Woolgar 1988a: 85). On the one hand, he notes that the truth about science cannot be heard in the interviews with scientists, but, on the other hand, it appears that an ethnologist can easily find it out:

> *in situ* monitoring of scientific activity gives us the benefit of the experiences of an observer undergoing prolonged immersion in the culture being studied. This kind of participant observation thus makes it possible to retrieve some of the craft character of science. This approach is designed to reveal the messy, idiosyncratic, stop-and-start character of the work in the laboratory (*op cit.*).

To me, both these passages seem essentialist, representational, and certainly unreflexive.[30] Thus reflexivism is not really applied in the fieldwork, and therefore such a seemingly radical relativism turns out to be still inconsistent.

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[30]   In an earlier ethnographic study Woolgar and Latour admit that the problem of representation emerges in their own descriptions of scientific laboratory, it "is both insoluble and unavoidable" (Latour & Woolgar 1979: 283). However, they see reflexivity as applied in the ethnographic study without necessary reflexivist regress: "We attempted to address the issue of reflexivity by placing the burden of observational experience on the shoulders of a mythical "observer". We attempted to alert the reader to the nature of his relationship with the text (and by implication to the nature of readers' relationship with all attempts to constitute objectivities through textual expression)." *op. cit*. The idea of reflexivity, according to Latour and Woolgar, is to remind the reader that all texts are some kind of stories.

2.4.2. Symmetrism

Latour and Callon, too, regard the variants of Edinburgh and Bath relativism as inconsistent: the strong programme is taken to reduce knowledge to its social environment, i.e., merely to shift the focus from nature to society. Such a shift is seen as retaining strong asymmetry. Latour notes that the explanation of scientific knowledge by its social conditions would be acceptable only when "we can impute interests to social groups given a general idea of what the groups are, what society is made of, and even what the nature of man is like" (1983: 144). But in the Anglo-American sociology of scientific knowledge the concepts of ideology, society, and interests are, according to Latour, quite ambiguous. Another problem with the strong programme, according to the Paris School, consists in its inability to overcome the classical dualism of content and context — content is explained by context as if it were possible to distinguish between them. Callon asserts that the distinction is impossible because: "context and content are simultaneously reconfigured" (Callon 1995: 51). Latour points out that in scientific practice the 'social outside' and 'scientific inside' appear to be in permanent displacement:

> There is no outside of science but there are long, narrow networks that make possible the circulation of scientific facts. [...] Once all these displacements and transformations are taken into account, the distinction between the macrosociological level and the level of laboratory science appears fuzzy or even non-existent (1983: 167).

Therefore, Latour finds that in addition to the social turn, science studies need another radical turn to establish real symmetry. Geometrically expressed, the turn consists in a 90-degree shift with the symmetry thesis of the strong programme. As result, we get the second principle of symmetry which claims equal explanation of both nature and society (Latour 1992: 279). Latour's argument for the new principle of symmetry is based on the ontological equability of nature and society: "We live in a Society we did not make, individually or collectively, and in a Nature which is not of our fabrication" (1992: 281). Neither one nor the other can be used for explanatory purposes: "Society cannot be used to explain the practice of science, and, of course, Nature cannot either, since both are the results of the practice of science- and technology-making" (*op. cit*.) Latour criticises the British sociologists for ignoring the research objects and technology[31], and for giving clear advantage to human actors, in their explana-

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[31] According to Latour, the main mistake of the British sociologists is taking the concepts of science and technology essentialistically. He finds that "'science' does not exist. It is the name that has been pasted onto certain sections of certain networks" (Latour 1988: 215). The same can be said about technology and society. So, from his point of view: "We are never confronted with science, technology and society, but with a gamut

tions of science.[32] In the Paris School vision, scientific activities must be seen as a chain of actions where both human and non-human actors are involved. Such chains belong to more extensive networks where all elements are considered equally, their identity is defined in their mutual enrolments and translations.[33] Hence one is not entitled to distinguish between the content of knowledge and its social context any more. The microbe (of anthrax) discovered by Louis Pasteur belongs to the same network with the French farmers, thousands of infected cows, with the laboratory of Pasteur and finally, with the interests of Pasteur. A French microbiologist Pasteur becomes "Pasteur"[34], a revolutionist of scientific medicine, through his ability to find allies, i.e., through his skill to translate between the interests of different actors of the network. Pasteur was able to translate farmers' concerns into the 'language' of his own scientific interests, and then back again to the 'language' of farmers' interests. He gave farmers a new social actor, the *microbe*, until then invisible reason of the terrible disease, anthrax, and thus he came to show also the ways of getting rid of the diseases and their economic consequences. The microbe as such is not less a social actor, according to Latour, than the whole French hygienists' move-

of weaker and stronger associations, thus understanding what facts and machines are is the same task as understanding who the people are" (Latour 1987: 259). *Science and technology* can be seen as only a subset of something called technoscience, a broader network where besides science and technology interested social forces are involved. See, e.g., Latour 1987: 175.

[32] Social context is not seen as suitable for the explanation of scientific content because, on the one hand, it leaves aside the real content, and on the other hand, such an explanation requires special language which is different from the 'tribe's' own language. Latour sees such a language choice as a problematic issue for the reason referred above — we do not and cannot have a complete and objective picture of society. (Latour 1988: 8–9).

[33] In an interview to Werner Callebaut (1993) Latour explains the difference between the British and French understanding of the notion of "actor": "What we did in the social studies of science, all things considered, is to reposition the notion of the actor. I would call "actor" the shifter, the redistributor, *the delegator of actions either to humans or to nonhumans*. In technology studies you can't start from a list of what humans are able to do as contrasted to what "mere things" will never be able to do, because the job of the engineer is to cross the boundary constantly and to reallocate skills and competencies among "actants"" (Callebaut 1993: 473). To illustrate the difference, I bring some examples from Latour's and Callon's translations between the conceptual framework of actant-network theory and Anglo-American sociology: 'actant' — actor, 'actant network' — social relations, 'translation' — proof, data (Latour & Callon 1992: 347).

[34] Latour refers to the distinction between the man, Pasteur, and the "Pasteur" — ideas of Pasteur (or perhaps the reception of his ideas by society?), the former is often identified with the latter. (Latour 1988: 13).

ment.[35] The *microbe* made possible the colonial wars without the dangerous infections, field surgery in the world-war, stormy development of food industry and wine production. Thus the discovery of the microbe is not just a cognitive issue, it is a social issue as well, and Pasteur formed and re-formed both the content and the context at the same time. It is important to note that the political/social consequences of the laboratory activities cannot be predictable. For this reason, they are inseparable from the purely cognitive processes, in principle: "Pasteur, representing the microbes and displacing everyone else, is making politics, but by other, unpredictable means that force everyone else out, including the traditional political forces" (Latour 1983: 168). This means, according to Latour, that Pasteur modified the society of his time — the interests, the society and science, all they are included in the changes and the reconfigurations prompted by events in his laboratory.

At the same time, Pasteur as the spokesman of the microbe has to bear an enormous burden of responsibility for all the 'translations'. Latour is often accused of letting scientists speak on behalf of nature — as the only representatives of nature, they obtain considerable power in society. Collins and Yearley find that:

> If nonhumans are actants, then we need a way of determining their power. This is the business of scientists and technologists; it takes us directly back to scientists' conventional and prosaic accounts of the world from which we escaped in the early 1970s. (Collins & Yearley: 1992a: 322)

Nevertheless, according to the principle of mutual 'translations' in the actor-network, also the scientists belong to the network and are somehow defined by the other parts of the network — they are *defined, enrolled, translated*, so they are not the independent representatives. A scientist is regarded as a scientist as long as she or he is taken to be a scientist by the network. Besides that, the statements, scientific texts, formula, etc. cannot be taken as representing reality because, according to the actor-network theory: "Statements do not talk of an outside reality; they are simply one location point in a long and teeming network" (Callon 1995: 53). The symmetrism of Latour and Callon can be interpreted so that things turn out to be research objects when they are taken up as

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[35]   Latour describes the displacement of content and context on the example of the hygienists movement and the role of Pasteur in the movement. The French hygienists were fighting for the improvement of general welfare, e.g., one of their goals was to improve public health. They saw the reasons of illness in the environment, and therefore the real actor, one single cause of the diseases, was missing. As soon as Pasteur gave them the *microbe*, the hygienists' ideas gained a theoretical foundation, and thus, hygienists survive as a social movement thanks to Pasteur. On the other hand, the "Pasteur" is made by the hygienists' social network. See for details the first part of Latour 1988: 3–146.

'objects'.[36] And similarly, science and technology appear to be objects of social study when they are thus considered, although, in the light of the idea of new symmetry, Latour and Callon cannot favour purely social explanation of science.[37] Both the natural and the social sides need to be analysed symmetrically.

In his *Science in Action* 1987 Latour introduced an image of science as *Janus bifrons* whose backward looking face — the one looking to the left on plane figures — corresponds to the so-called natural scientific realism. His forward — to the right — looking face, which stands for science in progress, may be seen equivalent to social realism.[38] One is usually natural realist about the past science, settled, certain and legitimated knowledge, in this sense one talks about scientific facts. Differently from this, in the science as process one sees controversies, debates resulting in decisions about what counts as the facts. Therefore sociological explanation is relevant here. The 'new symmetry'[39], symmetry between the two sides of *Janus bifrons*, however does not mean alternation between natural and social realism, rather it must be taken so that nature and society are twin results of the process of network building (Callon and Latour 1992: 348).[40]

This is the point where one may note the relativist regress threatening the entire actant-network approach. Collins and Yearley claim that if one considers actant-network theory in the light of its own methodology, the theory will require another consideration as *Janus bifrons* from another point of view. And, the new point of view, in its turn, would need to be considered symmetrically from the next point of view, etc. Collins and Yearley call such a rule of method active in the actant-network theory hypersymmetry (1992b: 379). If hypersymmetry is unavoidable, the attempts for consistent relativism necessarily will end in relativist regress.

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[36] The ordinary objects of scientific research are seen as quasi-objects — the term comes from Michael Serres, a French philosopher — the quasi-objects are seen as half natural, half social. Such quasi-objects are taken to build both nature and society. Again the identities are created in the mutual 'translations' accompanied by inscriptions which include graphic display, laboratory notebooks, tables of data, reports, etc. (Callon 1994: 50–51).
[37] Purely social explanation is, according to Latour, something characteristic of the English tradition: "Especially in England, the human actor is supposed not to be deconstructible" (See Latour's interview in Callebaut: 472).
[38] In this question Latour's use of the terms varies from one context to another, he sometimes calls the two sides respectively as realist and relativist ones (Latour 1989: 107), sometimes he contrasts natural realism with social realism (Latour 1992: 276)
[39] The term 'new symmetry' which is introduced by Latour, can be seen as a synonym for Collins' and Yearley's term of 'symmetrism' they apply in the characterisation of Latour's method.
[40] In this context the authors use the term of *general symmetry principle*. See also Latour 1987: 98–99.

Nevertheless, Latour and Callon appear to manage the regress in their case studies, i.e., their pursuit of symmetry is realised without the regressive symmetrism. This seems to be another kind of inconsistency — symmetry without the above claimed symmetrism, reflexivity without the above claimed reflexivism, and perhaps also relativity without relativism? In their empirical studies, Callon and Latour proceed quite in the manner of restricted relativism, while theoretically disagreeing with the foundationalist inconsistency of restricted relativism.

2.4.3. Social realism

Social realism is an epistemological and methodological view as proposed by Harry M. Collins in his empirical programme of relativism (EPOR). Collins takes relativism to be an important methodological rule, but since he is well aware of the theoretical problems related to relativism, he insists that it is a rule of methodology:

> I do not want to defend relativism. I do not want to talk about what exists in the natural world or how we ground our knowledge of it. Ontology and epistemology are not the subject of this paper, the subject is methodology of social science. I *will* try to show that the appropriate method for the *social* study of science entails that the natural world — as opposed to the social world — is approached … relativistically — even if a relativistic epistemology be resisted (1981c: 216).

According to Collins, the fact "that the natural world needs to be approached in a relativistic way ... does not imply that the social world be approached in this way" (1981c: 216). In the footnote explanation, he comes to the definition of social realism:

> I am coming to realize that this is an unusual view — some even find it shocking. Not only does it deny the importance of, currently fashionable, reflexivity, but it reverses the accepted wisdom about where certainty and reality are to be found. My prescription is to treat the social world as real, and as something about which we can have sound data, whereas we should treat the natural world as something problematic — a social construct rather than something real. This seems to me to be an entirely natural view for a social scientist (1981c: 216–17).

Thus the sociologists of scientific knowledge are supposed to study the social world of science in the same way the natural scientists study the natural world. In his later articles, Collins often emphasises the resemblance between the methods of SSK and the natural sciences: "Most practitioners of SSK, far from being against science, warrant their own work by reference to "scientific crite-

ria" — careful observation, repeatability, and so forth" (1996: 230). Elsewhere Collins, nevertheless, notes that the SSK can be seen as a *philosophical* school:

> One school, however, inspired in particular by Wittgenstein and more lately by phenomenologists and ethnomethodologists, embraces an explicit relativism in which natural world has a small or non-existent role in the construction of scientific knowledge. Relativist or not, the new philosophy leaves room for historical and sociological analysis of the processes which lead to the acceptance, or otherwise, of new scientific knowledge (Collins 1981b: 3).

Collins distinguishes between three stages in the sociological explanation of knowledge. The first stage concerns the "empirical documentation of the interpretative flexibility of experimental results" (Collins 1983: 95). One, and the main issue, under examination in the first stage, was experimental replication. Collins himself, for instance, has made a case study on the attempts of building a TEA-laser.[41] Through his personal experience in the British laboratories, he tried to explicate the role of so-called tacit knowledge, the role of skills in scientific practice. Theoretically, the problem concerns social negotiations on what exactly counts as experimental replication.

A research programme, such as laser-building or detecting the gravitational radiation,[42] involves a set of rules of interpretations taken for granted by the group of scientists. The taken-for-granted rules of interpretation make knowledge and skills a local phenomenon. Therefore, "the data are not meaningful outside of this interpretative context" (Collins 1983: 92). In order to reveal the hidden rules, a sociologist of scientific knowledge must 'go native', obtain native competence in a local scientific culture.[43]

The taken-for-granted rules become visible also for the 'native' participants in the case of scientific controversy. When there are at least two competing theories at hand, the way data should be interpreted will be seen as a questionable item. The interpretation itself turns out to be decisive. Collins notes that: "A comprehension of the scientists' interpretative competencies is a vital part of the enterprise, but whether a change comes about or not, is a consequence of more than what happens in any single location" (1983: 95). The scientific controversies resulting in the change of a whole set of interpretative rules are, basically, considered at the second stage of empirical programme of relativism.

At the second stage, the analysis:

> "is concerned with the way that the limitless debates made possible by the unlimited interpretative flexibility of data are closed down. The mechanisms of closure have been found to include various rhetorical, presentational and

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[41] For a detailed survey, see Collins 1985 (2nd ed. 1992)
[42] For Collins' case studies, see Collins 1981a, 1985, & 1996.
[43] Naturally, going native should be accompanied by the ability of alternation between 'cultures', for a sociologist cannot become a scientist.

institutional devices working within a context of 'plausibility' and other conservative forces" (Collins 1983: 95–96).

If one's aim is to understand the revolutionary changes in the science, the activities of the set of leading experts from different institutions, called the 'core-set', needs to be investigated. According to Collins, the outcome of an attempted change, closure of a problem, depends on the interaction between the core-set institutions. This is related to what he calls the sociological resolution of the problem of induction (1985: 6). The core-set model was designed in accordance to the so-called 'Hesse net' — a network-structure of joint entrenchment of interrelated concepts. In the 'Hesse net' the relations between concepts are probabilistic and logical ones.[44] Collins sees the relations to be "better described as the networks of social institutions that comprise forms of life" (1985: 17). In his case study on the attempts to detect gravitational radiation, he analyses the problem of experimental replicability in terms of the core-set model, respectively, as the social relations between leading institutions. He demonstrates how the replicability of the experiments first carried out by Joseph Weber, the initiator of detection of gravitational fluxes, became rejected, step by step, by the rest of the *core-set*. Eventually, the scientific consensus was that Weber's experiment could not be repeated and, therefore, the method of detection of gravity waves turned out to be inadequate. In this particular case, the decisive role in the closure, or in 'changing the order' and getting the 'ships into the bottles'[45] was played by rhetoric applied by Weber's opponents. However, this does not mean that Weber's opponents' arguments were inaccurate, quite the contrary — the rhetorical methods were reasonably combined with the rules of action taken-for-granted by the wider scientific community. Weber's opponents knew well that, since the experimental results, in general, possess more weight than simply theoretical accounts, therefore, they did experiments, though not as extensive as Weber's, they presented their arguments as experimentally grounded ones.

The third stage of EPOR concerns studies into wider social and political structures of scientific knowledge, for: "The core-set does not work in isolation of course" (Collins 1983: 95) In this respect — what concerns the wider social connections — social realism has quite often been criticised. The other themes often criticised are the principles of impartiality and neutrality. The above referred group of Australian sociologists finds that certain types of commitment are inevitable, therefore, anything an SSK analyst does, may be seen as bringing forth some political consequences. According to Scott, Richards and Martin

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[44] See Hesse 1974.
[45] Collins applies the metaphor of ships in the bottles to our stable every-day perceptions, to our taken-for-granted rules of interpretations of data. Ships are in the bottles in the stage of normal science in Kuhn's terms. Respectively, the paradigm shifts are characterised as *changes of order*. See Collins 1985.

(SRM), the political position should be declared openly at the outset of a study. In his turn Collins notes that it is hard to predict the precise context a study may happen to be connected to. He finds that 'commitment to commitment' which SRM argue for, itself needs causal explanation. The relation between a cognitive issue and its social context is not always a simple or direct one. As an illustration of the statement, he gives an example: "the bomb may have saved more lives than it cost, and likewise the pesticides; the environmental catastrophes revealed in the Eastern Block may cause us to welcome the victory of capitalism" (1996: 231). Furthermore, the neutrality tenet should be seen as a norm, a rule guiding the scientific practice called SSK.

On the other hand, and this is far more important to be noticed for my present argument, the empirical programme of relativism is accused of altering the balance of power between science and culture. Both the Paris School and the adherents of the reflexivist SSK, such as Woolgar, certainly insist that social realism is inconsistent, since it replaces one kind of absolutism (natural) with another (cultural).

On several occasions, Collins has shown via careful analysis that the actant-network theory appears to come to a similar praxis.[46] When Latour and Callon so-to-say *black-box*[47] the natural scientific entities, according to Collins:

> the black-boxedness is not a property of things nor does it transfer from context to context [...]; the object of analysis is the thing *in the context of use.* If it is only the thing in its moment-to-moment context to which actant status can be assigned, it must *always* be on our mind that the power of things is the power granted to them by the community. This is the position of *Changing Order* rather than that of actant-network theory (1992: 187).

The accusations of the lack of reflexivity can be refuted within the principle of alternation and meta-alternation, as already indicated above.

Another kind of criticism on EPOR comes from the natural scientists, who do not agree with the proposed lack of empirical constraints in the theory choices.[48] Nevertheless, even this argument can be paralysed or postponed, due to the framework of alternation. In social realism, such as claimed in EPOR,

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[46] Bloor (1999) too asserts that the actor-network theory is inconsistent in the above indicated sense.

[47] *Black-boxing* is a theoretical assumption that enables an analyst to 'bracket' the scientific taken-for-granted meanings and burdens of interpretations of the entities and objects of the natural sciences. The voltmeters, cromatographs, etc. are seen as black-boxes producing data.

[48] Also, the strong programme does not agree with the radical opposition between nature and culture, because in the explanations of scientific knowledge, the beliefs about nature are examined and explained, not nature as such (Bloor 1999). Bloor insists that, on the other hand, the members of society belong to nature as living organisms (Bloor 1999: 88).

one regards science as a human activity. Obviously influenced by John Searle's *Speech Acts*, (1969) Collins takes science and scientific knowledge to be institutional. Scientific beliefs must be regarded as institutions, i.e., every collectively endorsed belief, right or wrong, is an institution. Nature itself as a research object does not make any beliefs about it more or less certain. In reply to critics, Collins says:

> It is often thought that the sociology of scientific knowledge is an attack on the institution of science as a whole. It is not. The sociology of scientific knowledge has only one thing to say about the institution of science: it is much like other social institutions. The re-analysis of scientific method does not of itself make science into a bad institution (1992: 190).

Collins suggests that the precise relation between the empirical and cultural constraints, and their connection to a wider social and political context, as well as the multilevel structure of the institution of scientific knowledge, needs further inquiry in a new 'knowledge science'.

2.5. Conclusion: have we ever been consistent?[49]

Is consistency of relativism possible? Is consistency achievable in relativism? The problem of consistency in the SSK appears in three particular forms. The first of them concerns relativist regress, self-refutation and skepticism. The second form of the problem appears when relativism is once restricted by some special conditions, and therefore seen as partially anti-relativist in the sense of its partial 'absolutism'. In the third form, the problem can be observed on the example of the empirical studies carried out by the two relativist schools in the SSK, both pursuing consistency in relativism, one of them appealing to generalised reflexivity, and, respectively, the other to generalised symmetry. In their empirical studies, however, they seem to withdraw from their original principles, and proceed in quite the inconsistent way in the second sense of the term.

Considered philosophically, relativism appears self-refuting if it has been based on the opposition to the absolutist concept of truth. In any other case, relativism does not necessarily involve a contradiction. Thus, e.g., the pragmatic theories of truth as well as epistemic theories of truth do not pose the opposition.

And even in case of absolutist concept of truth, knowledge relativism would be acceptable, since truth is a semantic relation between the linguistic or epistemic entities — like statements, theories and beliefs, — and reality, whereas knowledge claims are social constructions, made in their particular contexts.

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[49] This is a paraphrase of the title of a recent book by Latour *We Have Never Been Modern* (Latour 1993).

As Margolis has shown, partial 'foundationalism', 'universalism', or 'objectivism' does not necessarily make a relativist programme inconsistent, viz. self-refutational. According to him, in the case of scientific beliefs, theories and models, the truth values do not obtain in the sense of universal, mutually exclusive bipolar values. Hence, in the sciences both relativism and absolutism appear as terms with degrees and respects. Therefore the relation between such extremes as absolutism and relativism can be seen in quite a flexible way. Relativism may be characterised as an anti-absolutist attitude, a rule of action, a tendency, which nevertheless does not exclude 'absolutist' tendencies in certain respects and to certain degrees. Thus, historically and culturally varying scientific beliefs can be analysed and explained relativistically without any particular fear of inconsistency. Moreover, relativist explanations are tenable only as far as relativism is constrained to some certain area, i.e., as far as it applies relatively absolutist terms, defining clearly what is taken to be relative, and relative to what. The claim of necessary relativisation of all beliefs, and respectively the version of seemingly consistent anti-foundationalism bring us necessarily to relativist regress, and skepticism. In practice, it is impossible to follow the prescription of relativisation of all beliefs, since it would require, metaphorically spoken, an extraordinary capability of speaking an unlimited number of languages in an endless number of voices all at the same time.

In this chapter, I considered how the consequences of the problem of consistency of relativism apply to sociology of scientific knowledge. I observed two opposite views, viz. restricted, moderate relativism and radical, the so-called position-consistency relativism. The strong programme of the Edinburgh School and the empirical programme of relativism of the Bath School, both appear to be versions of the modest, partially 'absolutist' relativism, the first admitting two kinds of constraints, material and social ones, the latter adopting only social ones.

Woolgar and Latour for their part, insist on the requirement of consistency in relativism. They see the main purpose of the SSK studies to be the removal of any kind of absolutism, and substitution of it for true, i.e., consistent relativism.

In conclusion we may admit, however, that the versions of relativism which pursue or pretend pursuing position consistency, inevitably end up in regress. At the same time, the *reductio ad absurdum* argument which I invoked in the section 2.4, shows that both programmes the semiotic analysis by Latour, and the ethnomethodological research by Woolgar, when applied empirically, involve some hidden assumptions. In empirical research neither of the authors seems to regard respective principles of position-consistent relativism as really valid. In Latour's actant-network-theoretical case studies, the symmetry principle works only on the hidden assumption of symmetrism being invalid. In Woolgar's case studies, reflexivity works only on the hidden assumption of reflexivism being inactive. If this is not a controversy, what is it then? As it ap-

pears that, the more consistent a variant of relativism, the more inconsistent it is at the same time.

In order to make relativism a tenable and useful programme in the explanation of scientific knowledge, it must become restricted in the way either Harré & Krausz, Margolis, Hesse or Niiniluoto recommend. A version of restricted relativism can be seen active in the strong programme and in the empirical programme of relativism of the Bath School. In the latter, in particular, the issue of acceptance of the alternation between different levels, as well as the acceptance of inter-level inconsistency has been elaborated. In spite of the somewhat problematic nature of the sociological reductionism of the Bath School, they have successfully shown that the regress of relativism is not inevitable if relativism is constrained by levels allowing epistemological and methodological relativism while disallowing ontological and judgmental relativism.

3. THE 'SCIENCE WARS' AND THE ARGUMENT OF UNDERDETERMINATION

3.1. Introduction

The so-called 'Sokal-affair' in 1996[50] gave rise to a new wave of debates between the traditional philosophy of science and the relatively new sociology of scientific knowledge (SSK)[51]. 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 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 chapter, 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 im-

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[50] For a brief survey, see Hacking 1999: 2–5.

[51] 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.

portant 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.[52]

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, 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.[53] 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).[54] The disjunctive either … or …

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[52] 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 3.2 and 3.6.

[53] 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).

[54] When the manuscript of the present chapter was already submitted for publication as an article in *Trames*, Helen Longino's new book "The Fate of Knowledge" was published. According to Elisabeth Anderson, the cover commentator for this book, Longino

-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.[55] It would also mean that, in (cognitively) rational 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'.[56] These consequences have also led many other critics to regard SSK as an idealist programme, if not an anarchist one.

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.[57]

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.

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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.

[55] 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).

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

[57] 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.

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*.[58] 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).

3.2. 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

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

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.[59]

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 dichotomization 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 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:

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[59] 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.

[A] single theoretical hypothesis cannot be conclusively falsified, so any statement can be held to be true come what may if we make drastic enough adjustments elsewhere in the system.[60]

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.[61]

Some sociologists of scientific knowledge have construed the underdetermination 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.[62] The

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[60]   Quine 1953, p.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, pp. 331–358.

[61]   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.

[62]   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.

underdetermination thesis, understood as justifying the sociological approach, also seems 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.[63] 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[64]. 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[65], 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.[66] For the strong programme, the DQT only serves as an argument

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

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

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

[66] 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.

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.

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.[67]

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: 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

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

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., Collins 1985/1992)[68]. 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.[69]

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

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[68] 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.

[69] 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.

study theories are not underdetermined by empirical facts? The old troublesome reflexivity problem emerges again.[70]

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.[71]

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 underdetermination, 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.

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

[71]   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 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.

3.3. 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 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.[72] 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

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[72] 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)

experimental situation. Therefore, nothing like a 'crucial experiment' is possible in principle.[73]

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.[74] 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 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

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[73] 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.

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

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.

3.4. 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[75]. 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.[76] 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 essay. Hypothetically, it should be a variant of deductive 'double-barrelled' realism[77] 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. Ac-

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

[76]   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 dissertation, the issue of holism in realism is only one landmark on the way to *reconciliation* of realism and relativism.

[77]   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.

cording 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: 138).

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:

> 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.[78] Realism for him is, first of all, a theory of ontologi-

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

cal 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. He defends a position called perspectival realism which claims the substitution of:

> the standard framework of objectivist reference and truth as a basis for developing an interpretation of the practice of modern science. Rather than thinking of science as producing sets of statements that are true or false […], we should think of it as a practice that produces models of the world that may fit the world more or less well in something like the way maps fit the world more or less well. In such a framework, it is sufficient that empirical evidence can sometimes help us decide that one type of model fits better than another type in some important respects. (Giere 1999: 240–41)

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: 56).

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: 22). 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 world', rather "[w]e invent devices that produce data and isolate or create phenomena, 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.[79]

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

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[79]   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.

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)[80]. Therefore, one could even claim that 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 essay in connection with the realism–relativism debate[81], for in-

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[80] See Barnes, Bloor & Henry 1996, and particularly Bloor 1999 for the importance of empirical constraints in explanation of scientific beliefs. See also Elzinga 1992, pp.60 - 61.

[81] There have been various attempts of reconciliation of realism and relativism which are not considered in the present chapter, e.g., the 'two-tier-thinking' of Elkana (1978) and the 'double-reality of the sciences' (Elzinga 1993). The account involving the dis-

stance, 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).

3.5. 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 the 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.[82]

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

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tinction 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.

[82] 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.

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[83] whereas modest holism does not. Neither have the adherents of the SSK considered the possibility of social underdetermination.[84]

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.

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[83] A good overview of the problems of incommensurability can be found in Harris (1992).
[84] 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.

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

3.6.1. 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 assumption 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:

> 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.[85]

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

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[85] See either Bloor 1991 or Barnes and Bloor 1982.

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[86], 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 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 section.

3.6.2. 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

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[86] 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.

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.[87] 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.[88] 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 possibly rational inferences[89]. Even if certain rules or standards strongly underdetermine theory choice, the choice may still be a rational judgement based on other rational criteria. Laudan concludes: "what is wrong with QUD [Quine's underdetermination] 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).

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[87] See Longino's recent study in social epistemology (Longino 2002).
[88] 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.
[89] Laudan 1990, pp. 267–297. See also Laudan & Leplin 1991, pp. 449–472, and Laudan 1981, and 1982.

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 underdetermination 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 underdetermination, 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 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).[90] 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.[91]

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.*[92] 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'.

A third-way solution would be to consider the context of justification together with the context of discovery, and to inquire why particular rational

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[90] 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)

[91] 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.

[92] 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.

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[93]. 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

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

opens the way for social science investigation (Knorr Cetina & Mulkay 1983: 3).

Thus they redefine the function of the DQT:

> 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 the 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).

3.7. Conclusion

The starting point for this chapter 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'.

4. A CASE STUDY: HERMAN BOERHAAVE — COMMUNIS EUROPAE PRAECEPTOR (EXTERNALISM VS. INTERNALISM AS EXPLANATORY SCHEMES FOR HISTORY OF SCIENCE)

4.1. Introduction

Hermann Boerhaave (1668–1738), according to the often-quoted expression of his pupil Albrecht von Haller, *Communis Europae Praeceptor*, the teacher of all Europe, has not received the attention from the historians of science that he should deserve. In the early 18th century, he attracted students from almost all over the world. To mention only a few, among his students were Linné from Uppsala, Haller from Göttingen, Cullen, Monro, and Sinclair from Edinburgh and he had contacts with Russian and Chinese scholars, etc.[94] What was the basis of his fame? One cannot connect any scientific discovery to his name. At the same time, we know that his pattern of scientific work, his *method* spread all over Europe. A number of historians of science find that it was Boerhaave who made the Newtonian turn, i.e., the Scientific Revolution, in chemistry, as well as in biology and medicine. If so, why is Boerhaave then almost unknown at the end of the 20th century?

In this chapter, I shall restrict myself to Boerhaave's chemistry, although his scientific pursuits also concerned biology and medicine. The main issue of my study, however, is not the history of chemistry, rather it concerns some meta-level historiographical questions.

As soon as the theoretical discussion on the methodology for the studies of history of science began, the meta-historiographical positions concerning the interpretation of the Scientific Revolution became divided into two major traditions, one called internalism and the other externalism. The meta-historiographical dichotomy of internalism *vs.* externalism certainly applies to a remarkably wider area of questions in the history of science than that of the Scientific Revolution. For a long time, the internalist model served as an 'officially' accepted historiographical position, whereas externalism was taken to be a position of Marxist, leftist and other radically minded marginal historians.

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[94] See for instance Brock 1992: 37, 77, 108, 133, Butterfield 1949/1980: 205. According to G A Lindeboom (1968), about one third of his students came from English-speaking countries, where, at that time, no good medical education could be obtained, large number of students came from German-speaking countries, so, relatively to the other national groups only minority were Dutch. See his *Herman Boerhaave: The Man and his Work* (1968): 363.

Nowadays, the general attitude has changed. Internalism has become a view of the past that almost no-one in the field of history of science accepts any more, whereas externalism has advanced and become a widely approved position, although it might seem odd to talk about *generality* or *universality* of externalism, because externalism in itself rejects any 'big pictures'.[95]

According to the *internalist* rational reconstructions, the Scientific Revolution must be seen as a logical consequence of the earlier, pre-scientific imaginations of alchemy, iatrochemistry, etc., even if the revolution itself consists in the break with old tradition, it is the break within the content — in the internalist approach only the content matters. The problem with internalism is that it assumes the sciences to be following a mysterious internal rationality of development. Thus, chemistry was seen to become a science only after the 'rational' and 'progressive' Newtonian metaphysics was accepted by its practitioners. It is quite obvious how arbitrary and present this idea is — it imposes contemporary standards of rationality as universal upon history. As result of this kind of meta-historiography, only the achievements, discoveries and inventions supporting the supposed line of progress, remain visible in the story of science. Newton and mechanism, of course, are seen as an important stage of the universalist history of science. Against this background, it even may seem surprising that Boerhaave has acquired such a marginal position. The reason for that consists in the causes *why* Boerhaave introduced mechanicism into chemistry. In his natural sciences, mechanism served just as an instrument for practical purposes. Seen in the light of the dichotomy internal vs. external, the causes of acceptance of a theoretical belief must be regarded as external. Even within internalism, it is obvious that some external factors cannot be neglected. However, it is not just a matter of including the external issues. The internalist approach cannot be improved just by adding the external influences, although many internalist philosophers, historians and sociologists of science have proposed such an idea.[96] The external issues will thereby still remain secondary, additional and unimportant for the whole rational reconstruction. For instance, Larry Laudan takes it for granted that the external issues are something secondary and separate from the real essence of science, so he comes to insist that:

> In sum, if it is true that science matters (both intellectually and institutionally) because of the manipulative and predictive skills which its ideas confer on their possessors, then a concern with science as a cognitive process must be primary, for until we have understood how science works cognitively, the largest question about science will remain unanswered. The theorists of scientific change recognise the centrality of the cognitive; that is why their theo-

[95] See an extensive study of Jan Golinski (1998) where he describes the main historiographical changes in the last few decades.
[96] See for example Lakatos (1971), Laudan (1977 and 1981).

ries focus primarily on the dynamics of scientific belief change. (Laudan 1996: 52)

For this reason, as in such an account the external factors are regarded as something to be kept apart from the content, those not satisfied with that, could suggest another theoretical point of departure, a one which enables us to consider the external circumstances seriously, and that could be called externalism. Nevertheless, such a conclusion in itself is not satisfactory either. The problem with externalism, constructed as a counterpart of internalism, is that it preserves the whig history scheme, or internalism as such. Radical externalism would involve explanation of scientific knowledge in terms of social, economic, political, ideological, etc. causes. That means a social, economic, or political, etc. reduction.[97] Since the externalist model will thus view just the social, ideological or political, i.e., contextual aspect of scientific knowledge, it may appear necessary to save also the internal history in parallel, relating to the real content of science. Robert Young, a proponent of a form of externalism, which he calls contextualism or relativism, admits that

> It is therefore very difficult indeed to refrain from treating the materials in terms of the model of 'internal' and 'external' factors, science and society (Young 1973: 376).

Therefore, the label of externalism turns out to cause a serious confusion. Those who regard themselves as the externalists are certainly interested in the contents of scientific knowledge as well as its context. The externalists stress the significance of the connection between content and context. Thus they do not just deal with the external aspect of science, as one could assume on the ground of the dichotomy: internalism *vs.* externalism, they deal with science in its context. Externalism tends to be opposed to any 'big pictures' of the progressive 'edge of objectivity'.[98] To be precise, as for the question of the Scientific Revolution, there are two rather different tendencies within the externalist tradition: one of them endorses the idea that, there was the revolutionary turn that requires an explanation by reference to the external reasons. The other tendency could be called as continuism, and according to this consistently externalist view, there is no Scientific Revolution as such (see Henry 1997). Those who adhere to the idea of revolutionary change, may still be criticised for the whiggish bias, because, as John Henry (1996) has put it: the revolution marks the change from something unknown to us to something like us, or our science. Our contemporary science is taken to be the measure.

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[97] Such an attempt has been made by some sociologists of scientific knowledge in the 1980s. See especially H. M. Collins (1981). The radical social constructivism prefers pure social explanation rather than rational.

[98] See Gillispie (1960), *The Edge of Objectivity: An Essay on the History of Scientific Ideas.*

The continuists argue that in the history of sciences, we only find particular scholars — researchers, engineers, surgeons, and teachers — involved in their particular practical activities, solving problems, constructing new devices, doing experiments. Scientific Revolutions as well as the progressive and degenerative research programmes, rational models of research traditions, etc. belong to the 'big pictures' of internalist history.

The difficulties, however, just begin here, because the distinction between the internal and external factors is practically a very complicated one, the content and the context of scientific knowledge appear to be inter-related. For instance, there has been a lot of discussion around the possible classification of scientists' metaphysical, religious and aesthetic views, whether these should be seen as internal or as external ones (McMullin 1987: 58–64)? A number of historians consider ideological and metaphysical views as internal ones, whereas the others refer to these as external to scientific knowledge. Since I prefer to leave this huge historical discussion aside in my study, I suggest to replace the traditional, and still problematic dichotomy of internal *vs.* external with another, that of intrinsic and extrinsic, originally proposed by John R. R. Christie and Jan Golinski (1982) The new dichotomy is constructed with the intention of historical analysis and explanation. For such an explanation, one should choose between the two alternative approaches: either to consider both the internal and the external issues closely related to a particular discipline at a particular time and in a particular location, e.g., chemistry in Leyden in the 1720s, — this is the *intrinsic* approach; or to consider the scientific ideas in the light of some non-scientific, i.e., general metaphysical, epistemological, ideological, etc. framework — this is the *extrinsic* approach.

When following the intrinsic approach after Golinski and Christie, the reasons, that caused Boerhaave to accept Newtonianism, can be easily explained by the practical needs of his scientific activities: knowledge needs to be organised for the purposes of the treatment of the patients, as well as for teaching and communication. For Boerhaave, mechanicism served as a conceptual tool for efficient knowledge transfer. Similarly the big changes in the sciences in general, related to mechanicism, the new worldview that caused the change of scientific language, can be explained by reference to practical purposes of communication and teaching sciences, rather than by internal metaphysical necessity. Steven Shapin, a strong-programme historian, points to the common deficiency of many traditional accounts of the Scientific Revolution as a tendency to overestimate the formal methodological considerations. Methodology, he finds, is, in part, a myth, and the myths have certain historical functions. Therefore, the methodologies need to be investigated in the context of respective practices:

> [W]e will still need a more vivid picture of what a range of modern natural philosophers actually *did* when they set about securing a piece of knowledge.

> Modern natural philosophers did not just *believe* things about the natural
> world; they *did* things to secure, to justify, and to distribute those beliefs.
> Doing natural philosphy, that is, was a kind of work. So we now need to turn
> from abstract methodological formulas to the practical work of *making* expe-
> rience fit for certain kinds of natural philosophical inquiry. (Shapin 1996:
> 95–96)

In the internalist tradition, Boerhaave has been seen as a Newtonian scholar
who was close to revolutionary turnabout in chemistry. It is thus important to
add to the internalist description, that he was not *only* a great Newtonian
scholar who prepared ground for Lavoisier's work in chemistry. He was a prac-
tising surgeon, chemist, biologist, and a physiologist who was occupied with
several practical questions: how to treat his patients, how to classify diseases,
how to prepare drugs, how to understand a human body and processes therein,
how to classify herbs which he needed for preparing drugs, and finally, how to
pass all this practical knowledge that he possessed to the next generation of
scholars.

In the following section, I will analyse the meta-historiographical questions
at somewhat greater length, in the third section, Boerhaave's chemical activities
will be considered in the light of the meta-historiographical conclusions. In the
fourth concluding section, I will return to the questions posed at the outset to
the chapter.

4.2. A few further meta-historiographical considerations

4.2.1. The internalist — externalist distinction *vs.*
the intrinsic — extrinsic

According to a widespread opinion among historians of science, the content of
17–18th century science was mainly determined by religious, economic and so-
cial factors. (Nilsson 1984: 107) According to another opinion, development of
science is supposed to be explained on the basis of the science itself, on that of
observations and experiments more than theories and hypotheses, without ref-
erence to the historical context. (Nilsson 1984: 110) The first is the called as
the *externalist* and the second as the *internalist* position. Since the distinction is
a problematical one, as we could see in the introduction — it often is namely
the internalist position that assumes religious or metaphysical factors to deter-
mine the content, etc., — I hereby invoke another dichotomy, as suggested by
J. R. R. Christie and J. V. Golinski, namely that of *intrinsic* and *extrinsic*.
They describe their intrinsic approach as follows:

> The analytical focus we urge is concerned with the question of the nature of
> chemistry as an historical practice. This focus is interested in the whole range

of social and cultural conditions governing both practical chemistry and chemical discourse, but it is the human activities of practising and talking about chemistry which are central, and around which broader themes are articulated. We would like to describe this perspective as an 'intrinsic' one. Against it we would set a class of approaches which could be described as 'extrinsic'. Such approaches shift the focus of analysis away from chemical practice to non-chemical fields of discourse. Assuming the nature of chemistry as fundamentally unproblematic, the extrinsic approach tends to construct chemistry in terms which give great emphasis to the influence thereon of activities such as speculative natural philosophy, matter-theory, epistemology, methodology and theology (Christie and Golinski 1982: 235-36).

Without rejecting the influence of metaphysics, theology, etc. on chemistry, they, in contrast to the extrinsic approach, do not presuppose unidirectional determination of chemistry by external attitudes. An intrinsic approach insists upon the problematic nature of the relationship between such factors and the practice of chemistry.

According to Christie and Golinski, internalism includes those factors that have been designated above as extrinsic to chemistry, whereas it denies the social, economic, cultural characteristics in the description of scientific process, focusing only on the 'pure products of intellect'. The intrinsic approach, by contrast, demands sensitivity towards the precise location of intellectual production, for the location might have certain effect on the discipline, thus, e.g., theology and epistemology are suitable for explanation of chemical activities in certain circumstances, relevance of these explanations is historically variable.[99] In their article, Christie and Golinski demonstrate how the intrinsic metahistoriography might be made to work on the example of the 17-18th century chemistry as a practical and a didactic discipline.

In a later writing, Golinski (1993) points out that the misleading dichotomy brings one to a theory of postponed revolution in chemistry and its consequences such as referred to in Herbert Butterfield's account.[100] According to Butterfield, most historians of chemistry tended to consider chemistry as a science that suffered remarkably from the postponed revolution. Historians of chemistry tended to underestimate the achievements of the 17th century. Only after Lavoisier's contribution in the late 18th century, they thought, can one

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[99] Also a philosophical approach as presented by Nicholas Jardine (1991) appears to have the local scientific practices with the package of theoretical assumptions, applied rules and methods, institutional, educational and technological factors in its focus. In his account — very close to the intrinsic one — Jardine claims that it is the 'scenes of inquiry' that need to be put in the centre of both historical and contemporary science studies.

[100] See H. Butterfield (1949/1980), Ch. XI, "The Postponed Scientific Revolution in Chemistry", pp. 191–209.

speak about the scientific conceptual structure in chemistry. For Golinski, the view of historians, who attach rationality to the 17th century chemistry only to the extent it has taken over mechanistic philosophy and language, is equally fallacious. In the opinion of those historians, the criterion of maturity of science was its mechanicism, and the first scientific chemists accordingly were Nicholas Lemery (1645–1715) and Robert Boyle (1627–1691), whose works contain mechanistic elements. Golinski finds that:

> Such a historiography simplifies the relations between the chemistry and natural philosophy of the seventeenth century by assigning to chemical practice a position of subservience and passivity with respect to theoretical developments in contemporary metaphysics (Golinski 1993: 368).

Some studies on the 17th century chemistry say that the mechanistic philosophy was accepted by chemists only because of psychological and epistemological considerations, the language mechanics offered was a privileged one, clear and easily applicable in chemistry and in other fields of natural science as well. However, such an attitude lacks historical specificity, as Golinski argues, the transition could happen in any place at any time.

According to Golinski both the French historian Hélène Metzger and Butterfield take the criterion of a mature science to be the existence of a logical structure of concepts, founded upon a metaphysical theory of matter. Such a structure is assumed to be a mental entity, psychologically connected with immediate experience. Also, such a structure would be separable from its historical and material manifestations, words, texts, and practices. Consequently, the mechanical language of chemistry was regarded simply as representing the metaphysical structure.

Golinski sees two connected assumptions to exist in this historiography, first, that chemistry is taken to be essentially dependent on a philosophy of matter, and second, that the language in which chemistry represents phenomena is seen to be unproblematic. Chemical texts just reflect the philosophical theory that rest on the background and correspond with reality. In Metzger's opinion, the mechanistic attitude enables us to abandon the allegorical and metaphorical style of earlier chemical discourse. An interesting question arises, how actually did the alchemical obscure languages function? To what extent and how did alchemists understand each other? And why did chemists suddenly come to accept the mechanic concepts instead of the old alchemical language? Did they just decide to start talking more clearly?

4.2.2. From alchemy to scientific chemistry

According Maurice Crosland (1963), a historian of chemistry, it is plausible that the need for reorganisation of the language appeared within the alchemical

symbolic tradition itself. Crosland refers to Lemery who had been rather critical about his contemporary chemists' language, thus in his days already (in the end of 17th century) the obscure language of alchemy was out of date.

Lemery's own mechanistic rationalisations of the texts had a direct practical purpose: his pharmaceutical prescriptions needed to be widely understandable. So, we are justified to ask together with Christie and Golinski (1982: 245): did the mechanistic rationalisations serve, in some sense, as legitimisation for his chemistry, or were they a result of the search for clear and unambiguous descriptions of chemical processes?

Golinski points to another example of the intrinsic approach to the history of chemistry. This is Owen Hannaway's work on the history of early modern chemistry. Hannaway proposed to investigate the origins of chemistry at the beginning of the seventeenth century as a didactic discipline. In his opinion, the formation of chemistry, as a textual tradition, predated the widespread acceptance of mechanical philosophy.[101] The main task was to distinguish between the activities called chemistry and that of alchemy. Hannaway characterises chemistry and alchemy in terms of different attitudes to modes of argument and communication, and for the very possibility of learned discourse. Chemistry could be seen as a didactically oriented discipline, committed to the values of open and clear communication, whereas alchemy was an object of conspiracy. Hannaway equates the birth of chemistry as a didactic art with the appearance of Andreas Libavius *Alchemia* (1597) and sees it as formed in opposition to the Paracelsian-Hermetic school, exemplified in Oswald Croll's *Basilica Chymica* (1609). Croll's (alchemical) epistemology was individualistic: all the knowledge is taken to be the result of the interaction between a man as microcosm, the centre and subject of creation, and macrocosm. The interaction or externalisation of knowledge is possible only via a sympathetic attraction between these two: micro- and macrocosm. Knowledge was conferred by divine grace, rather than by reason, it could be read neither from the nature nor from the books. For Libavius, chemistry was supposed to be open, co-operative, and hence cumulative. His famous *Alchemia* was an attempt to embody chemical doctrine in a form that made it as communicable as possible.

Golinski sees Hannaway's approach as opposing that of Metzger and Crosland, according to whom, one may still claim that the language of chemistry changed because chemists decided to start talking more clearly, using mechanistic philosophy and corresponding terms for the purpose. Chemists even could have made such a decision, but the reason of that certainly lay in the communicative needs. Knowledge can only be acquired from other people, e.g., from the texts written by other people, if one can read them, and at special institutions, arranged for the purpose of the spread of knowledge. The institutions

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[101] Referred via (Golinski, 1993: 372).

of learning and teaching, for example universities, can exist only as long as there is a demand for academic knowledge.

According to Golinski, the social basis for the new model of chemical communication requires to be construed by reference to its historical context. The 17–18th century chemistry might be characterised by an expanding market for printed chemical texts, whereas besides that there also was a kind of restricted communication between chemists: chemical and other technological secrets were exchanged between one another, to the advantage of both participants. The new kind of communication offered opportunities for power, and new careers, which concerned new institutions, e.g., progressing universities. Crosland (1963: 370) notes that there were close connections between chemical theory and the industries involving porcelain, dyes and gunpowder.

4.2.3. A parallel to the oxygen-revolution

The 'oxygen-phlogiston revolution' at the end of the 18th century which has often been considered as the revolution establishing a scientific paradigm in chemistry, a revolution which in the internalist sense of the term, appears to be problematic if considered against the background of different meta-historiographical perspectives. Christie and Golinski (1982: 259) find that there was nothing that Lavoisier said about oxygen or phlogiston that the Edinburgh chemists had not said earlier. Lavoisier was not 'the final chapter of the influence of Newtonian matter-theory' regarding eighteenth century chemistry (Christie and Golinski 1982: 258). Different chemical communities were occupied with different theoretical and empirical issues, thus, 'the revolution' was variable. For Hannaway, e.g., Lavoisier's revolution consists in the decision to embody the new chemistry (new nomenclature) in an elementary textbook which was a realisation of the power of the word for chemistry (Christie and Golinski 1982: 260–61). Consequently, Boerhaave and his students from Edinburgh, could be seen as revolutionary in chemistry as well, especially, in the light of Lindeboom's statement that Boerhaave was regarded as the first scientific chemist by his contemporary scientists.

4.3. Boerhaave as a chemist

4.3.1. Science and (or) Art

According to Lindeboom, Boerhaave was an iatrophysicist who was interested in chemistry. However, it was Boerhaave who had brought chemical practice into the university. Thus, he himself had introduced a certain criterion for the

distinction between science and art: since then, sciences belonged to institutions like universities, arts such as alchemy, iatrochemistry, and botany were developed outside of the academic institutions, in drug stores, etc. Boerhaave himself uses both the term of 'Science', and the term of 'Art' about (even university) chemistry. Some historians interpret the emphasis on 'Art' as a rhetorical intention of convincing his audience in the need to develop chemistry as a Science, the others, such as Lindeboom, point out that in Boerhaave's time, the terms of 'Art' and 'Science' were used as synonyms. Otherwise, Boerhaave would have preferred 'Science' because what he had in mind, was *science*. Christie and Golinski refer to the unauthorised publication of Boerhaave's lectures, from the year 1727, where Boerhaave compares chemistry with the art of sculpting. Both the *artists* intend particular effects in the material world, both require material tools, and a principle of effective knowledge. Effective knowledge for Boerhaave meant communicable knowledge. (Christie and Golinski 1982: 248)[102] For both the *artists* the concept of 'instrument' is a central one: all the Aristotelian elements were defined as instruments, having a similar function in relation to the concept of art. Instrument is needed to attain the intended aim of the artist, so 'fire' — or any other 'instrument' — all equally served for the didactic aims.

Not only chemistry, but also the other sciences, such as geometry, botany, etc. were taken to be the arts. It can be understood as a popular metaphorical account of the sciences by demonstration of their the practical connections. The turn from purely scholastic writing to experimental science capable of solving practical problems was, perhaps, the main revolutionary change in the academic tradition. Theory became related to practice. It is not then surprising that in the period of transition the terms of 'Science' and 'Arts' were confused and conflated. Even the greatest scientists were engaged with the alchemical experiments: Newton and Boyle are well-known examples. Also, Boerhaave boiled mercury during a period of almost sixteen years, until a careless student broke the vessel. Being himself critical about alchemy and iatrochemistry, he admitted that important facts could be found by alchemists' observations.

According to Boerhaave, the movement in chemistry from art to science was possible only because chemistry was capable of correcting her mistakes — of abandoning the magical and mystical basis of alchemy, the dream of gold-making, exaggeration with the idea of effervescence and the wrong interpretation of fire as an immaterial substance. Therefore he assured in his inaugural speech in 1718, that:

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[102] Didactic practice as an aim was emphasised only in the first unauthorised issue of Boerhaave's *New Method of Chemistry 1727* known through a translation of Peter Shaw, based on notes from Boerhaave's lectures. The didactic aspect was decreased in the *Elementa Chemiae* 1735 published by Boerhaave himself.

while acknowledging that Science is strewn with the chemists errors, I shall try to prove that these same errors have been most successfully wiped out, solely by the efforts of these same chemists (*Boerhaave's Orations* 1983: 194).[103]

The principal mistake, from his point of view, was the uncritical application of chemical ideas in medicine, and respectively, the consideration of physiological processes as similar to those of chemistry. In *Discourse on Chemistry Purging Itself of Its Own Errors,* he criticised van Helmont, Paracelsus and his teacher Sylvius for these kinds of errors.

4.3.2. Making chemistry a physical science

In his textbook *Elementa Chemiae,* Boerhaave defines chemistry as:

> an Art that teaches us how to perform certain physical operations, by which bodies that are discernible by the senses, or that may be rendered so, and that are capable of being contained in vessels, may be thence produced, and the causes of those effects understood by the effects themselves, to the manifold improvement of various Arts (Lindeboom 1968: 328).

Boerhaave's chemistry could be seen as a branch of Newton's physics. Its various phenomena could generally be explained simply in terms of motion. When water dissolved salts, this was simply by the interior motion of its particles. These particles were in fact conceived as atoms — the solid, massy, hard, impenetrable, movable particles, as the atoms were depicted by Newton. Boerhaave speculates on the different sizes of atoms, which would serve for the explanation of certain chemical reactions. He believed that mathematics could be usefully applied in chemistry, his approach was more quantitative than usual at that time, e.g. it was not every chemist who when referring to the solubility of a salt would mention temperature. (Crosland 1963: 393–97). It was also due to the fact that, according to Golinski, he was the first who brought the thermometer systematically into chemistry in the 1720s (Golinski 2000: 190).

Boerhaave's most characteristic quality was his exactness, he formulated seven rules for doing experiments, and he followed these rules strictly. His writings are systematic, clear and accurate. His main textbook in chemistry *Elementa Chemiae* covers systematically all the chemical knowledge in Europe

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[103] "Discourse on Chemistry Purging Itself of Its Own Errors", in *Boerhaave's Orations* (1983).

at that time, and was used in many universities as a textbook for many years[104]
And it was Boerhaave who introduced microscope into chemical research.

His practical interests and will to cure his patients, made Boerhaave to examine carefully the nature of substances like milk, eggs, cream and other organic products. However, his firm adherence to the physical method and critical attitude, caused by that, towards *iatrochemists'* studies on effervescence, did not allow him to realise the importance of the area we call biochemistry today.

Data from experiments, which he had completed, spread widely among other scientists, and were approved. Thus, he *did* everything to develop chemistry as a science, as he had promised to do in his *Discourse on Chemistry Purging itself of its Own Errors,* 1718. First of all, he taught the new generation to understand the importance of chemistry. With great pathos he turned to his students in the end of the *Discourse on…*(*Boerhaave's Orations* 1983: 212–13) and expressed his gratitude to them whose demand had forced him to resume teaching and working on chemistry year after year.

4.4. Conclusions

At the beginning of this chapter, I posed a question why was Boerhaave so famous all over the Europe in the 18th century? And why is he almost forgotten by the end of 20th century?

Aant Elzinga and Andrew Jamison (1984) find that his fame was based on the school he created, and which continued his tradition that might be called *practical utopia* — a pedagogical utopia, oriented to systematisation and organisation of scientific training at the university. Boerhaave was famous because of his didactically orientated research in the sciences with practical aims of medicine. He was even somewhat eclectic, joining together different theories and traditions, but joining only to the extent the theories enabling communication and co-operation to solve some practical problem. He had a catalytic influence on the creation of, e.g., the Edinburgh school of medicine and chemistry, and therefore, the impact on British 18th century medicine and chemistry generally.

He did not make any scientific discovery. His only invention was a greenhouse heater (Cunningham 1986: 41) But as Elzinga and Jamison point out, in the early 18th century science: "It is the social meaning, generalist ambitions, and external service orientation that dominate" (Elzinga & Jamison 1984: 162).

Boerhaave's *practical utopia* suited well the social environment of the early eighteenth century. Leyden in the Republic of Netherlands was a real citadel of

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[104] The University of Tartu was among the others, there are several copies of Boerhaave's textbook in French and Latin available in the *Bibliotheca Universitatis Tartuensis* in the collection of the scientific literature of the 18th and 19th centuries.

tolerance because the University of Leyden was open to all students, irrespective of nationality or creed. This was different from the Oxford and Cambridge Universities, which admitted only the members of the Anglican Church, whereas many other universities in Europe were under Catholic control.

State and university authorities gave support to his chemical and medical studies. There were plenty of donations to his laboratory. It is namely because in his case, "the emphasis lay more on social utility and meaning than on scientific growth in any narrow sense" (Elzinga & Jamison 1984: 161).

Crosland (1963: 370) indicates that Boerhaave's textbook and even notes from his lectures found easily readers, there was a market for the systematised representations of the chemical knowledge. It was certainly related to the increasing need for applicable knowledge, necessary, for example, for the manufacture of porcelain, gunpowder, dyes, etc. Chemistry become a popular science by the second half of the 18th century, and chemistry was firmly established as one of the physical sciences — namely with the accent on the physical sciences. Here the answer to the second question becomes transparent. Boerhaave applied Newtonian mechanics and its extension, the theory of affinity to a new area of research — chemistry — whereas chemistry itself remained secondary or an assistant science in comparison to physics. It was regarded as a science to the extent it contained physics. On the other hand, chemistry served medicine. Chemistry enabled one to describe a few phenomena, which were not accessible by the other sciences. Without any doubt, chemistry turned out to be a science in comparison to alchemy or iatrochemistry because chemistry had become independent from the main mistakes of the latter — magic and mystic explanations — that Boerhaave referred to in his famous Oration, *Discourse on Chemistry* in 1718. This, again, may be regarded as a credit to Boerhaave. Also, his influence on the Scottish and French schools in chemistry is remarkable. But he did not complete the change in the paradigm, that was left for Lavoisier, who is regarded as the founder of the scientific chemistry — at least by the internalist historians — because he was able to link the French pneumo-chemistry together with the Newtonian tradition. It must be mentioned that Lavoisier could be seen as one of the pupils — as a reader of the textbook — of Hermann Boerhaave, *Communis Europae Praeceptor*.

Unfortunately, the textbooks in history of science often overlook the great teachers and the applicants of scientific findings whose influence on the discoveries and discoverers cannot be over-estimated. Perhaps the great teachers of the past will receive more attention when the intrinsic model of history of science gathers strength and comes more often to replace the internalist 'big pictures' of the great discoveries which still tend to be favoured, if not in research environment, still in popular publications and textbooks.

GENERAL CONCLUSIONS

In this essay, after the introductory considerations in **chapter 1**, I analysed the problems related to the concept of relativism. How this concept is applied in the sociology of scientific knowledge is discussed **in chapter 2**. After having summarised the critical arguments by philosophers of science who, in general, assume the classical absolutist concept of truth relativism and contrast their views of relativism with this absolutism, I showed that in the SSK, the concept of relativism is not construed as parasitic upon absolutism. In principle, there are two approaches: one endorsing the classical concept of truth as a semantic concept which is of no relevance for the relativist concept of knowledge construction; and the other appealing to a pragmatic concept of truth (and, respectively, to a pragmatic concept of relativism) without mutually exclusive truth values which would turn relativism self-refuting.

In addition to the discussions between philosophers of science and the adherents of the SSK, I also compared different SSK positions. It appears that those pursuing the consistency of relativism, i.e., relativisation of every single belief, fall into the trap of inconsistency even twice — first, because of the regress and self-refutation involved in generalised relativism, and second, because of ignoring the principles of generalised relativism (reflexivism, or respectively, symmetrism) in empirical studies. Thus it also appears that those who limit relativism to a particular level of their account of science, or, in general, set constraints to relativism, are even less inconsistent. The latter approach can be quite easily reconciled with the metaphysical position of scientific realism. In the particular case of the strong programme, realism in ontology is assumed explicitly.

In chapter 3, I once again considered the application of the Duhem-Quine thesis (DQT) of the underdetermination of theories by data in the SSK context. In the early stage, the SSK authors favoured the DQT as an argument for social explanation of theory choice. The SSK argument was as follows: if DQT, then social explanation is required. At closer examination, however, the thesis of underdetermination appears to involve the arationality assumption — the view that only the arational requires social explanation (from the lack of rational criteria one deduces the necessity of social explanation). This contradicts the core principles of the SSK. Therefore, as far as the dichotomy of the 'rational' and the 'social' is assumed, the SSK should rather avoid the underdetermination. Also, one should take into account the fact that the theory choice may be socially underdetermined as well. In addition to these counter-arguments, it is important to note that the Duhem-Quine thesis does not make the distinction between scientific realism and relativism in the SSK. The general underdetermination thesis (UDT) in its turn does not necessarily involve sociological relativism — rather it involves a version of anti-realism, which does not neces-

sarily mean relativism (e.g., the constructive empiricism of van Fraassen assumes the UDT).

On the other hand, part of the criticism directed against the SSK application of the DQT is irrelevant. Laudan, e.g., assumes that for the SSK everything is either deductive logic or sociology, i.e., in his opinion the adherents of the SSK reduce rationality to logic. Thus he points out that there are other rational criteria for theory choice besides deductive logical ones. However, it is exactly the plurality of epistemic criteria that makes social explanation more plausible. Besides that, in most contemporary versions of the SSK, no such dichotomy of the rational and the social is assumed. Rather, rationality is considered as social, since the criteria, or the standards of rationality, are related to the local contexts.

In a case study on the 18th century chemistry and the meta-historiographical programmes, **chapter 4**, I compared two classical theoretical schemes, internalism and externalism, which both appeared to be inadequate for the explanation of changes that occurred in chemistry in the early 18th century. As an alternative, I considered the intrinsic account as proposed by Golinski which could be regarded as a middle-way position between the internalist and externalist schemes. At the same time, the intrinsic account is contrasted to the extrinsic one. The advantage of the intrinsic approach appears in the explanation of the change of scientific knowledge. It enables to take into account the various aspects of scientific practice: the practical interests of experiment, treatment of patients, and the mechanisms for knowledge transfer, such as didactic and communicative practices. In contrast to the traditional internalist approach where particular scientists just mark the objective progress of ideas, the intrinsic approach brings forth the activities of the scientists, thus making history richer. When Herman Boerhaave, the great teacher from Leiden of the early 18th century, is regarded as just a Newtonian scholar in the internalist accounts; in the intrinsic account, he is regarded as a major figure, due to his textbooks and didactic practice.

In the light of the debate between the philosophy and sociology of science, the intrinsic meta-historiography could be seen as a middle-way position, where both the 'cognitive' and the 'social' matter, since, within this context also the 'cognitive' is depicted as social by its nature.

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SUMMARY IN ESTONIAN

Teadusliku teadmise sotsioloogia
filosoofi vaatekohalt nähtuna

Käesolev uurimus koosneb sissejuhatavast probleemiülevaatest ja kolmest varem eraldi ilmunud artiklist, mida on kohandatud siin avaldamiseks. **Sissejuhatavas** ülevaates käsitlesin teadusliku teadmise/tunnetuse sotsioloogia põhilise teoreetilise suuna, nn tugeva programmi tunnetusteoreetilisi seisukohti, tuues esile selle neli printsiipi (seletuse kausaalsus, sümmeetrilisus, erapooletus ja enesekohasus), mille alusel paljud kriitikud, nii teadusfilosoofid kui ka -sotsioloogid on püüdnud näidata tugeva programmi vastuolulisust. Nimetatud printsiipidega nõustumine või mittenõustumine võimaldab ka klassifitseerida erinevaid teadusliku tunnetuse sotsioloogia koolkondi. Samuti esitasin sissejuhatavas osas üldistavalt põhilised teadusfilosoofiliste argumentide tüübid teadusliku tunnetuse sotsioloogia vastu. Äärmuslikke neist argumentidest tuntakse teaduse sõja metafoori kaudu. "Teaduse sõja" argumentidele on ühine tendents luua kritiseeritavast vaatekohast karikatuur. Nii kujutatakse sotsioloogilist teadmise käsitust, otsekui viimane välistaks teadusliku teadmise seletusest ratsionaalsuse ja püüaks selle asendada meelevaldsete sotsiaalsete kokkulepetega. Samas kalduvad mõned tunnetussotsioloogid kujutama teadusfilosoofilist teadmise käsitust kui paratamatult individualistlikku ja logitsistlikku. Nii on "teaduse sõjale" tunnuslik eeldada dihhotoomiat: ratsionaalne sotsiaalse vastu.

Oma uurimuse kõigis osades püüdsin tunnetussotsioloogia seisukohtade üksikasjaliku filosoofilise analüüsi abil näidata, et mitte kõigi sotsioloogia traditsioonide puhul pole õigustatud "teaduse sõja" tüüpi argumendid, isegi kui mõne puhul on. Näiteks eelmainitud tugevat programmi võib iseloomustada pigem kui "kolmandat teed", mis võimaldab vähemalt dialoogi filosoofia ja sotsioloogia vahel.

Üks tüüpargumente teadusliku tunnetuse sotsioloogia vastu on sellega oletatavalt kaasnev idealism: kui teadmine on loomult sotsiaalne nähtus, siis paistab, nagu ei mängiks tegelikkus selle objektina enam mingit rolli ning selle võib kõrvale jätta — järelikult ei sõltu teadmine sotsiaalkonstruktivismi järgi materiaalsest tegelikkusest ja võib piiranguteta konstrueerida mida tahes. Sotsioloogia oponendid asuvad sellelt aluselt tõestama, et tegelikkus osutab vastupanu ja ei ole võimalik konstrueerida mida tahes. Paraku on see argument rajatud eksiarvamusele, et sotsioloogid räägivad *faktide* konstrueerimisest, kui tegelikult peetakse silmas *teadmise* konstrueerimist. (Tugev programm pooldab klassikalise teaduse määratluse asemel institutsionaalset.) Tugevas programmis tehakse selget vahet väidete ontoloogilise ja epistemoloogilise kihi vahel — kui räägitakse sotsiaalsest konstruktsioonist, siis peetakse silmas uskumuste tekke mehhanisme, mille juures eeldatakse kogukonnale ühiseid keelelisi, kultuurilisi,

praktilisi tavasid, mis mõjutavad iga tajuotsustust. Seega on empiirilised tõendid tõenditeks vaid sedavõrd, kui antud traditsioonis ja taustateadmisi arvestades saab neid tõenditeks pidada. Tõendid kuuluvad epistemoloogia ja mitte ontoloogia valdkonda. Viimases on tugev programm realismi pooldaja. Osalt on ontoloogiline realism samas seotud eeldatud epistemoloogilise relativismiga: kuna eeldatakse uskumuste varieerumist ühe ja sama objekti kohta, siis peab olema mingisugune alus arvamaks, et tegu on ikka sama objektiga. Ka sellele mõnevõrra problemaatilisele seisukohale rajavad mitmed teadussotsioloogia oponendid oma kriitika.

Idealismi argumendi kõrval kritiseerivad teadusfilosoofid sotsioloogiat, viidates sellele, et ehkki mõnel üksikul juhtumil teaduse ajaloos või tänapäeva teaduses toimivad teadlased oma sotsiaalsetest huvidest, võimuambitsioonidest või soovmõtlemisest tingitult, siis üldiselt see teaduses nii ei ole. Teaduses kehtivate üldtunnustatud normide hulka kuuluvad objektiivsuse ja erapooletuse nõue, mida kõik järgida püüavad.

Paraku on ka selle kriitika puhul tegu eksiarvamusega, nagu eeldaks sotsiaalne teadmise-/tunnetusteooria, et teadmised on sobingute ja võltsingute tulemus. Tunnetussotsioloogia seisukohalt tähendab teadmise sotsiaalne loomus seda, et teadmised luuakse inimeste ühise tegevuse ehk interaktsiooni tulemusena. Interaktsioon ise väärib sotsioloogide arvates uurimist, kuna sel moel loodetakse jõuda selgusele, miks mingil konkreetsel juhtumil, mingis kindlas kontekstis langetati just niisugused või teistsugused valikud ja otsused, sel moel loodetakse saada teada, miks mingis kontekstis peetakse õigeks ühe- või teistsuguseid ratsionaalsuse standardeid. Kriitikute seisukoha puhul tuleb tähele panna varjatud eeldust: ratsionaalne *vs.* sotsiaalne, mida vähemalt tugeva programmi sotsioloogias ei ole: ratsionaalset mõistetakse ratsionaalsena sotsiaalse konteksti kaudu.

Sarnasel vaatekohal on nn sotsiaalepistemoloogia, kus aga erinevalt sotsioloogiast, mis on oma teaduskäsituses deskriptiivne, on tegu normatiivse lähenemisega, ja seetõttu täiendatakse sotsioloogide väidet *ratsionaalne on sotsiaalne* väitega *sotsiaalne on ratsionaalne*. Viimane tähendab, et sotsiaalepistemoloogide arvates tuleb puhkudeks, kui tavaliselt kriitilise hoiakuga teadlasrühmas mingil üksikjuhtumil tekib n-ö soovmõtlemine või tendentslikkus, ette näha sotsiaalne korrektsiooni mehhanism (nt eelretsenseerimise nõudel on just see eesmärk).

Teadusfilosoofias leiavad teadusliku tunnetuse sotsioloogia uurimused ja argumendid järjest enam käsitlemist. Käesolev uurimus ei pretendeeri täieliku ülevaate andmisele neist käsitlusist, vaid keskendub kolmele enim ja kõige teravamalt käsitletud kitsale probleemile.

Teises peatükis keskendusin relativismist kui niisugusest johtuvatele probleemidele. Absolutistliku tõe relativismi määratlusega kaasnevad relativismi paradoksid, nagu valetaja paradoks. Sellele rajab oma kriitika Newton-Smith, kes püüab tõestada, et relativismi tõttu on sotsioloogilised teadusekäsitused en-

nastkummutavad. Samas näitavad Harré ja Krausz, et kui relativismi piirata, siis õnnestub pardaoksi vältida. Sarnasel seisukohal on Margolis, kes pakub pigem pragmaatilise lahenduse, mille põhisisu on, et relativismiga ei kaasne paradoksid, kui ei eeldata vastandlikke tõeväärtusi nagu lihtsate propositsioonide puhul. Isegi kui eeldada absolutistlikku tõerelativismi mõistet, ei ole selle abil võimalik kummutada tugeva programmi teese. Tugev programm ei eelda tõe relativismi, vaid uskumuste, teadmiste, väidete, teooriate relatiivsust. Millisele ka tugeva programmi teesidest relativismi paradoksi ei püütaks rajada, ebaõnnestuvad need katsed, sest teesides ei ole tegu vastandlike tõeväärtustega nagu valetaja paradoksis.

Tunnetussotsioloogia eri suundade vahel on relativism põhjustanud nn järjekindluse-vaidluse. Woolgar järjekindluse taotlejana leiab, et iga uskumus tuleb oma taustaga siduda, sel moel teeb ta relativismi regressist reegli ning kritiseerib tugevat programmi relativismi piiratuse pärast. Latour leiab, et looduse seletamine inimühiskonnas toimivate suhete kaudu on uue asümmeetria kehtestamine, mistõttu on vaja teist pööret tõelise sümmeetria kehtestamiseks (tugeva programmi sümmeetria tees nõuab erinevate uskumuste ühetaolist seletamist), tuleb leida uus vaatepunkt, millelt *loodust* ja *ühiskonda* saaks käsitleda ühetaoliselt. Üldistatult nõuab see printsiip aga järgmist vaatepunkti, millelt eelmised kolm oleksid sümmeetriliselt käsitletavad ja nõnda edasi. Jällegi on järjekindluse nõude tagajärjeks regress. Samas, empiirilistes uuringutes kumbki neist tugeva programmi kriitikutest oma teoreetilisest programmist kinni ei pea. Seega on relativismi järjekindluse taotlejad ebajärjekindlad kahes mõttes: nende programmilised seisukohad on ennastväravad ja nad ei pea empiirilistes uuringutes oma programmist kinni, kui nende kritiseeritud Briti tunnetussotsioloogia on ebajärjekindel vaid relativismi piiratuse mõttes.

Kolmandas peatükis uurisin Duhem-Quine'i alamääratuse teesi rakendamist teadusliku tunnetuse sotsioloogias. Ilmneb, et oodatud veenva teoreetilise põhjenduse asemel võib DQT hoopis sotsioloogia taotluste vastu toimida: kui eeldada, nagu osa sotsiolooge 1980ndate algul ja osa jätkuvalt, et kui on teooria valiku ratsionaalne alamääratus, siis paratamatult järgneb sellest sotsiaalne seletus, johtub sellest, et sotsiaalsed asjaolud tulevad arvesse vaid ratsionaalsete kriteeriumide puudumisel, st n-ö aratsionaalse valiku puhul. Aratsionaalsus sotsiaalse seletuse eeldusena toob kaasa asümmeetria, mis on teadusliku teadmise sotsioloogia programmiliste seisukohtadega vastuolus. Lisaks sellele tuleb arvestada ka sotsiaalse seletuse alamääratuse võimalusega. Kolmandaks, kui teadusliku tunnetuse sotsioloogia varasel perioodil peeti oluliseks teoreetilist vastandust teadusliku realismiga ja nähti DQT-s selget kriteeriumi vahetegemiseks realismi ja relativismi vahel ning argumenti relativismi poolt, siis Duhemi ja Quine'i vaadete lähemal uurimisel selgub, et need ei ole antirealistlikud. Üldistatud alamääratuse tees (UDT) võimaldab küll vastandada realismi ja antirealismi, kuid viimane ei tähenda antud kontekstis vältimatult relativismi, rää-

kimata sotsioloogilisest relativismist. Üldise alamääratusega on näiteks hästi kooskõlas van Fraasseni empirism.

Duhemi-Quine'i teesiga toovad sotsioloogid ise vaidlusse dihhotoomia ratsionaalne *vs.* sotsiaalne. Samas, kummalisel kombel, on see olemas ka kriitikute argumentides, kes sotsioloogide alamääratuse kasutust ründavad. Larry Laudan näiteks omistab tugevale programmile ja selle pooldajale, filosoof Mary Hessele, seisukoha: kõik on kas deduktiivne loogika või sotsioloogia. Seejärel asub Laudan seda omistatud seisukohta kritiseerima, väites, et

1) ratsionaalsete valikukriteeriumide ala ei piirdu deduktiivse loogikaga;
2) isegi kui kehtib täielik ratsionaalne alamääratus, ei järgne sellest midagi kontingentsete uskumuse tekke ja omaksvõtu põhjuste kohta.

Mida Laudan tähele ei pane, on see, et sotsioloogid ei taanda ratsionaalseid kriteeriume kitsalt loogikale ning just laiemalt mõistetud episteemilise (ala)määratuse (s.o mitte kitsalt loogilise) puhul kasvab sotsiaalse seletuse tõenäosus. Pealegi ei eelda vähemalt mõned tunnetussotsioloogia koolkonnad enam niisugust ratsionaalse ja sotsiaalse vastandust, nagu domineeris nende varastes uurimustes. Pigem käsitletakse ratsionaalsuse standardeid kui kohalikke, kontekstisidusaid standardeid. Epistemoloogia, mida pooldab tunnetussotsioloogia, on lokaalne.

Neljandas peatükis leiab käsitlemist üks teadusajaloo juhtum, mis illustreerib vaidlust vastandlike metahistoriograafiliste seisukohtade pooldajate vahel. Ma võrdlesin internalismi ja eksternalismi 18. sajandi nn teadusrevolutsiooni seisukohalt keemias. Ilmneb, et kumbki neist seletusskeemidest ei võimalda anda keemiarevolutsiooni vastuoludeta käsitlust. N-ö kolmanda-tee-lahenduse pakub Golinski, kes asendab klassikalise vastanduse ekstrinsilise-intrinsilise vastandusega. *Ekstrinsiline* on ajalooteooria, mis seletab üksikjuhtumit, uskumust või avastust ideoloogia, metafüüsika, religiooni või muu teadusvälise asjaolu kaudu; *intrinsiline* on teooria, kus seletus rajatakse teaduse praktikale, sisu seletatakse tegevuste kaudu, milles sisu sünnib, teadmise edasiandmise viiside, katsemeetodite ja -tehnika, uurimiseesmärkide ning probleemipüstituse kaudu. Sel moel muutuvad nähtavaks ja oluliseks teadusega seotud didaktika ja kommunikatsioon. Teise rolli omandavad õpetlased, kes internalistliku või ka eksternalistliku skeemi järgi vaid markeerisid mõne suure metafüüsilise või teadusliku suuna progressi. Traditsiooniliste metahistoriograafiate järgi seisnes 18. sajandi alguse Leideni suure õpetaja Herman Boerhaave tähtsus selles, et ta vahendas Newtoni mehaanika oma õpilaste kaudu Lavoisier'ni, kes n-ö teostas hiljaks jäänud revolutsiooni keemias. Intrinsilise metahistoriograafia järgi on Boerhaave puhul tegu silmapaistva praktiliste lahenduste pakkuja, õpetaja ja õpikute autoriga, kelle õpetus levis üle kogu maailma, kes oma süstemaatilisusega mõjutas Linnéd ja kes võttis mehhanistliku maailmavaate kõrval keemias kasutusele kvantitatiivsed meetodid. Ajalooteoreetilise pöördeta poleks see 21. sajandil meile teada.

Intrinsiline, n-ö lokaalne ajaloouurimine on leidnud toetust ka teadusfilosoo-fidelt nagu näiteks Nicholas Jardine. Kriitikutele nagu Laudan, kes väidab, et kui oluliseks peetakse teaduse manipuleerivat ja ettenägevat võimet, siis tuleks ka teadusajaloo uurimises tegelda esmalt kognitiivse ajaloo ja alles siis kõige muuga, saab intrinsilise positsiooni pooldaja vastata, et kognitiivne probleem ongi sellise uurimise keskmes, kuid seda käsitletakse seoses teadlaste tegevuse-ga, teadust ei mõisteta kitsalt kui loogilist järelduste jada, vaid kui tegevuste jada, sh ka loogilised järeldused. Jällegi eeldab Laudan "teaduse sõjale" omast vastandust: kognitiivne sotsiaalse vastu, mida intrinsilise metahistoriograafia pooldajad ei eelda.

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LIST OF PUBLICATIONS

I. Monographs (R2):
1. *Reconciling Realism and Relativism: A Study of Epistemological Assumptions in Relativist Sociology of Scientific Knowledge*, Insitutionen för idéhistoria och vetenskapsteori, Göteborgs Universitet, Rapport nr. 202, 2001, ISSN 1650-6499.

II. Academic papers published in international peer-reviewed journals (A1):
1. "Hermann Boerhaave — *Communis Europae Praeceptor*: Internal *vs*. External in the History of Science". In R. Vihalemm (ed.), *Estonian Studies in the History and Philosophy of* Science. In the series of Boston Studies in the Philosophy of Science, Vol. 219, Kluwer Academic Publishers 2001, 139–150.
2. "Pursuing Consistency in Relativist Sociology of Scientific Knowledge", *TRAMES*, *Journal of the Humanities and Social Sciences*, 1998, 2 (52/47), 4, 299–330.
3. "The 'Science Wars' and the Duhem-Quine Argument of Underdetermination", *TRAMES, Journal of the Humanities and Social Sciences*, 2002, 6 (56/51), 2, 141–172.

III. Other academic papers (A3):
1. "Changes in the Image of Science after the Social turn". In Anne Kull (ed.) *The Human Being at the Intersection of Science, Religion, and Medicine, Piirid ja kohtumised: teadus ja religioon tehnoteaduslikus maailmas* (Proceedings of the International Colloquium Tartu, 4–5 May 2001), Tartu University Press 2001, 86–93).
2. "Kas ülikoolis õpetab teadus?" = "Is It Science Taught at the Universities?" — K. Mits & L. Erilt (toimetajad) 2000, *Õppejõudu otsimas. Valik artikleid kõrgkoolis õpetamise metoodikast ja teooriast*, Collegium Humaniorum Estoniense, Tallinn, 29–39.
3. "'Postmodernistlik'" teaduse mõiste ehk veel üks katse kirjeldada elevanti" = "The Postmodernist Concept of Science: Another Attempt to Describe the Elephant" — K. Mits & L. Erilt (toimetajad) 2000 *Õppejõudu otsimas. Valik artikleid kõrgkoolis õpetamise metoodikast ja teooriast*, Collegium Humaniorum Estoniense, Tallinn, 40–53.
4. "Järjekindluse probleem teadusliku teadmise sotsioloogias" = "The Problem of Consistency in the Sociology of Scientific Knowledge" — *Studia Philosophica III,* Tartu Ülikooli filosoofia osakond, 1998, 91–123.
5. "Duhem-Quine'i tees ja realismi-relativismi tüli" = "The Duhem-Quine Thesis and the Realism-Relativism Debate" — 1996 *Akadeemia,* 8, 8, 1682–1698. Summary in English, p. 1762.

6. "Is Scientific Realism Too Optimistic?" *Studia Philosophica II*, Dept. of Philosophy, Tartu University 1995, 19–34.
7. "Realismi ja relativismi lepitamas" = "An Attempt of Reconciliation of Realism and Relativism" — *Mõistlike valikute õigustamise filosoofilised eeldused*, E. Loone (koostaja). Tartu Ülikooli filosoofia osakond 1995, 81–94.
8. "Kas teadlased mõtlevad realistlikult?" = "Do Scientists Think as Realists?" — *Teaduslugu ja nüüdisaeg IX*, Tallinn 1994, 131–143.

IV. Scientific translations
1. "Traces of Eurocentrism in Current Representations of Science" by Aant Elzinga, 1995, *VEST*, in Estonian published in *Akadeemia* 1998, no 9, 1920–1940;
2. "Meaning and reference" by Hilary Putnam 1973, in Estonian in J. Kangilaski & M. Laasberg (koostajad) *Tähendus, Tõde, Meetod: Tekste analüütilisest filosoofiast I*, Tartu Ülikooli Kirjastus, 1999, 310–331.
3. Lorraine Code (2001) "Rational Imaginings, Responsible Knowings: How Far Can You See From Here?" In Tuana, N., Morgan, S (eds.) *Engendering Rationalities* (2001) State University of New York Press: Albany, 261–282. In Estonian in *Ariadne Lõng, Estonian Journal of Gender Studies*, 2001, 1/2, 165–181.

V. Conference abstracts:
1. 2000 "Social Construction — Constructing Facts or Constructing Knowledge," *Historiae Scientarum Baltica. Abstracts of XX Baltic Conference on the History of Science* Tartu 30.–31. 01. 2001, 33–34;
2. 1998 "Teaduse ja pseudoteaduse eristusest teadusfilosoofi vaatenurgast" = "The Distinction between Science and Pseudo-Science from a Philosopher of Science Point of View" — in *Tiidüs ja tõdõ. Märgütüisi. Arutlusi tähendusest ja tõest. Võro Instituudi Toimetiseq 5 (Publications of Võro Instiute 5)*, 66–68;
3. 1996 Relativism and Science in the Small Countries. *Theses Historiae Scientarium Baltica I*, Associetas Historiae Scientarum Latvienses, 70.

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