

PURSuing CONSISTENCY IN RELATIVIST SOCIOLOGY OF SCIENTIFIC KNOWLEDGE¹

Endla Lõhkivi

University of Tartu

Abstract. Relativism involves a theoretical problem known either as the liar's paradox or the problem of self-refutation or the problem of (in)consistency. Different traditions within sociology of scientific knowledge (SSK) suggest different ways of solution to the problem. The so-called position-consistency relativism insists upon consistent relativisation of all beliefs, whereas the so-called inter-level-inconsistency relativism restricts relativism to some level. Position-consistency relativism accuses restricted relativism of inconsistency – this can be seen as another kind of inconsistency. On further consideration of the relativist programmes in SSK, one can find that the position-consistency relativism in its turn is inconsistent in quite another sense – the empirical studies in this tradition are based on a hidden assumption that the claim of position-consistency is not really valid.

The article indicates a way how the problem of self-refutation of relativism can be avoided in SSK by abandoning the foundationalist concept of knowledge as true and justified belief.

1. Introduction

With the acceptance of methodological relativism the sociology of scientific knowledge (SSK) has inherited a number of troublesome consequences following

¹ The present article is based on my MA thesis, defended at the University of Tartu, on 17 April 1998. I wish to thank Associate Professor Rein Vihalemm for reviewing the paper. I am also grateful to Professor Aant Elzinga and Associate Professor Göran Wallén of the University of Göteborg, Sweden, for their comments on an earlier draft of the paper. In writing this paper I have greatly profited from the conversations with Professor Eero Loone of the University of Tartu, Emeritus Professor Håkan Törnebohm of the University of Göteborg, Professor Ilkka Niiniluoto of the University of Helsinki and Professor Rom Harré of the University of Oxford. I am grateful to Dr. Margit Sutrop, Mr. Jüri Eintalu and Mr. Petri Ylikoski for their critical remarks on the paper. My thanks for technical advice go to Mr. Toivo Tomson and Mr. Margo Laasberg.

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, in particular purposes. Therefore one can distinguish between different variants of relativism in the sociology of knowledge, each one claiming relativism with particular strength and area of application. This diversity of views involves endless discussions between the “relativisms”. My strategy in this article 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 paper will be devoted to the debate on (in)consistency occurring within the sociology of scientific knowledge community. 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 kind of consequences follow from the seemingly consistent relativism. I shall question if the approach is really consistent and contrast the so-called position-consistency relativism² 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.³ 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 realism⁴ must be accepted as part of relativism to avoid the notorious paradox of self-refutation.

² The terms ‘position consistency’ and ‘inter-level consistency’ come from Ingemar Bohlin (Bohlin 1995:32).

³ 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 belongs to radical skepticism. However, in comparison with symmetrism and reflexivism, EPOR belongs certainly to 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.

⁴ 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).

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, 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 can 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.⁵ Thus we may ask again, which kind of inconsistency to prefer, 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. Relativism in the sociology of scientific knowledge (SSK)

In 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

⁵ See, e.g., 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, the symmetrism. Collins and Yearley find a similar hidden inconsistency in Woolgar's application of reflexivity. Collins & Yearley (1992a & 1992b). In this essay see basically part 4.

rationalism of Karl Popper. In the 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 external (social) factors that caused the scientist's error. Thus Lakatos prescribes internal rationality to sciences, that can be reflected, reconstructed and 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.⁶

Such a normativity is certainly not acceptable in relativist sociology. 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 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 the same kind of causes.⁷

As one can notice, Bloor does not subscribe to the classical epistemological meaning of the term knowledge. The classical concept of knowledge as defined in ancient philosophy by Plato involves the notorious problem of foundationalism – after Plato, knowledge has been defined as true and justified belief.⁸ To get rid of 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

⁶ 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 the mainstream history.

⁷ See Bloor (1991:7). Originally the tenets are put in such an order: causality, impartiality, symmetry and reflexivity.

⁸ About the problems of foundationalism see, e.g., Dancy (1985), or Everitt & Fischer (1995).

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, when errors and arational action were taken to be explicable by 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 it will be considered in greater detail below. Collins points out that differently from the symmetry thesis, the causality and reflexivity theses can be seen as redundant, or perhaps even threatening to his empirical programme (Collins 1981c:215). Steve Woolgar in his turn puts a lot of effort into developing a “really consistent” relativist programme, the so-called reflexivism around its central thesis of reflexivity. According to Woolgar, neither the strong programme nor the Bath relativism hold actually on reflexivity, and therefore, are all inconsistent in relativism. According to the Paris School, neither the strong programme nor Bath relativism hold on symmetry, and are therefore inconsistent.

3. The problem of self-refutation of 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 means 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. He himself 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 such a justification cannot obtain as rational justification from our point of view, but 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 out 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.⁹ Possibly this is the item Newton-Smith wants to point out – that relativism is incoherent for incommensurability it involves.

Newton-Smith's argument can be read so that if we wish to save relativism we should try to avoid incommensurability. One way to escape incommensurability is to abandon relativism of truth 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 these of plausibility and implausibility, as it 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 paradoxes 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 we 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, it can be combined with a number of epistemological and ontological positions. There are stronger and weaker forms of relativism.

I shall return to the "weaker" variants of relativism after I shall have considered what kind of theoretical consequences could 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-refutational relativism.

⁹ See, e.g., Harris (1993) about self-refutation of the incommensurability-relativism. Also Putnam has pointed out how incommensurability, despite the radical diversity of views, beliefs, opinions, statements, and worlds, still presupposes a *God's Eye View*, a universal point of view from where the diversity is defined (Putnam 1982:10–12).

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).

It is obvious that relativism of truth involves self-refutation. However, it does not follow from the paradox of relativism of truth that relativism of knowledge in general would necessarily be self-refutational. The paradox works on the mutually exclusive concepts of truth and falsity, as we know. These mutually exclusive concepts are not applied in the concept of knowledge of the strong programme. For this reason, the critics of the strong programme, willing to show the self-refutation of the programme, have tried to find a way of getting the paradox “translated” into the conceptual scheme of Bloor’s four tenets, to make the paradox work also in the case of sociology of scientific knowledge. One possible translation may be given 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 another three forms of the paradox and assume self-refutation of the strong programme as 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 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 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 contradiction. 'Universal' and 'context-bound' may be incompatible, but the truth of some belief being context-bound does not make its negation necessarily wrong. So, according to Harré and Krausz, it is important to notice that relativism is paradoxical only in the case of claiming universal or absolute truth, which indeed would involve exclusive falsity as its negation. In most of the actual discourses about scientific beliefs it is not universal truth kept in mind, rather it is a sort of alethic truth or epistemic truth. Thus, it seems that contrarily to the expectations of the anti-relativists, we have succeeded to abandon the paradox of relativism of truth by this quasi-translation.

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 pointed to such views of his critics: "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, 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 to 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 go 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. But are there any conclusive arguments at all?

According to Hesse:

What 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.

When Hesse, a realist philosopher of probabilistic tradition, endorses the idea of contextualisation of the concepts of truth and knowledge¹⁰, the pragmatic realist Joseph Margolis has suggested to get rid of the strict bipolarity of truth and falsity in the explanation of scientific knowledge and save relativism in this 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 to constrain the concepts of truth and falsity 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 could find 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, we need to separate relativism from 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, not in the latter (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

¹⁰ The concept of truth in Hesse's use belongs to an epistemic (assertability) kind, whereas Margolis is talking about universal truth in this context. It is universal truth that involves exclusive bipolarity, which is seen as a source of the paradox of relativism.

enough to accept only Margolis's minimal idea of the abandonment of the bipolar values of truth and falsity.

It now 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 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? ¹¹ According to this thesis relativism needs 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 to 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 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 can ask about a hypothetical paradox of transcultural intelligibility, for, on the one hand, we have, according to relativism, plurality of

¹¹ Woolgar finds this to be the irony within the strong programme 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.

cultures and contexts as opposed to universalism, on the other hand, relativism assumes some universalism to make it possible to know the existence of different cultures.

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 (vially) 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, the version of relativism rejects objectivism, universalism, foundationalism, essentialism and logocentrism. Of course, Margolis (1986:72) recommends to deny the exclusive option between objectivism and either skepticism or the like. For such a 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 the multi-level relativism of Harré and Krausz very well. Hence relativism can be combined with both general historicism and scientific realism for the reason that we can find subtly presented in the following passage of 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 Krausz: 224).

Consequently, relativism assumes a modest variant of realism, be it called residual realism or single-barrelled realism.¹² The 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. This seems to be a frequently supported but still an erroneous view among philosophers and natural scientists that the attempts of sociological interpretation of knowledge necessarily lead to social reductionism, to a kind of idealism or to skepticism. Without developing the argument at greater length here, I just refer to David Papineau, a philosopher who has examined the issue and reached an unusual conclusion:

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 different 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 basically ontological position and sociological 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)¹³. The semantic concept of truth (knowledge) 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. Philosophical scientific realism has been more concerned with the ontology of the postulated entities and theories, whereas the procedures of knowledge have been left for only sociological study. Obviously there is no controversy, no conflict between the two dimensions, quite the other way round – they could be seen as complementary.

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 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

¹² Both terms are often used by Barnes, see especially Barnes 1992:137, and both the terms are also applied by Harré and Krausz.

¹³ 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.

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 realist¹⁴ or objectivist (if there are any metaphysical realists left by now at all):

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

In quite a similar manner with Margolis, Bloor finds that the universal concept of truth as defined in the correspondence theory is not suitable for practical explanations. As scientists we rather have a point of view which to prefer on a research issue, for we do not have independent access to reality and we do not know the (universal) truth. It is natural to prefer one's own point of view over the others. 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 – it is possible to speak in only one language at a time. Usually the view taken to be correspondent to the real state of affairs is called truth. So the term 'truth' and certain truth values can be applied in different senses. Therefore, Bloor suggests to focus the question about truth in another way, concentrating on the functions of truth. He distinguishes between three functions of truth – discriminative, rhetoric and materialist functions. The first, 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 rhetoric function of truth serves as a means of criticism and conviction purposes. 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).

¹⁴ By metaphysical realism I mean a view, similar to the *double-barrelled realism*, as it is known, (See Barnes 1992) in its strongest possible form, the view that assumes both the independent existence of real objects and our ability to obtain certain true knowledge of these objects. This assumption in its turn involves necessity of the 'ready-made-world' with the ultimate number of existing entities. This view can be formulated only as a theoretical extreme, for there are no real metaphysical realists.

Such an attitude is called *objectivity* by Margolis (1986:112–113)¹⁵. Objectivity can be seen as a weak form of objectivism. The thesis of independent existents, objectivity, and materialism seem to be the two sides of the same coin, and on this basis, we can conclude once again that it is possible to reconcile realism and relativism.

Thus I can assert here in conclusion to the section that the paradoxes of relativism and respective kinds of the problem of consistency can be resolved by introducing certain constraints to relativism. For Harré and Krausz, as well as for Margolis, and also for Bloor and Collins, relativism can be made to work in this way, for example, in the explanation of varying, sometimes controversial scientific views.

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 normative stand to any belief, statement, action or whatsoever. A relativist cannot assert:

*“Relativism is wrong” is wrong.*¹⁶

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 bipolarities. Therefore, there can be a similar solution to this possible form of paradox of relativism 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 permissible 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

¹⁵ 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.

¹⁶ See Putnam 1982, where the argument comes from, here presented in my logical reformulation.

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 the judgmental relativism by separating the 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.¹⁷ They do not demand equal position to 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 if to consider the procedure of symmetrical analysis in details. The symmetry principle assumes that an analyst should be able to switch between two radically different epistemological 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

¹⁷ 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).

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 chromatogram in a lab where gas-liquid 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 constructivists 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 participation in an escalation of skepticism which we liken to the game of chicken; in this case the game is epistemological chicken.¹⁸

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

¹⁸ The metaphor of *epistemological chicken* refers to the 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.

it is given by current social theories and our common sense.¹⁹ 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 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 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, the 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.

4.1 Reflexivism

According to Steve Woolgar, the interlevel 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. reflexive²⁰, naive realist position, which assumes scientific representations truly to picture independent reality 'out there' as it is in itself;

¹⁹ See Collins 1983:87–95.

²⁰ Please note that "reflexive" here does not have any connection with "reflexivity".

2. mediative position, which takes social environment to mediate reality in representations, thus enabling the parallel existence of different representations of a piece of reality;
3. 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 the 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 for 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 images of cultures, communities, etc. who *de facto* mediate reality for the sciences. Woolgar (1983:251) notes with sarcasm that the mediative position implicitly assumes the same reality to ground different representations.²¹ From this Woolgar (1988a:50–51) concludes 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 no shift can be seen from a variant of earlier asymmetric Mertonian sociology of science to the strong programme.

Constitutive position, initiated by Woolgar, involves (re-)inversion²² of the object and representation – the research object must be seen as generated 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, sociological accounts create social reality. The only way to avoid inconsistency, according to Woolgar (1988a:93), is to abandon unreflexive representationalism for scientists never face nature as such in their research, and the same is valid about 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:

²¹ As we could see above, this is a basic thesis for relativism, the irony would be suitable only when such an ontologically realistic view were exclusively opposed with relativism. Since it is not, there is no reason for irony.

²² According to Woolgar, inversion of the object and its representation is a feature of scientific research. As result of the scientific practice hypothetical entities turn out to be objects which are believed to act as causes of representations. See Woolgar 1988a: 54.

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 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, despite criticising the other branches of sociology of scientific knowledge, basically for the controversy between their relativism in the explanation of scientists' knowledge production and realism in the attitude to sociologists' own studies.

From Woolgar's point of view, the real issue is the lack of relativism in science studies. He asks in his reply (1992:330) to criticism from Harry Collins and Steven Yearley (1992a), where the two critics classify Woolgar's reflexivism as post-relativist approach: "When did we finally get to relativism?". 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, for the problem of relativist regress belongs to the world of formula, regularities and logic, the world which is of no interest to a reflexivist. A reflexivist is more interested in dismantling myths, traditions, 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 condition that makes reflexivity tenable in the empirical programme – reflexivity works on the condition of reflexivism's not being valid. The concept of reflexivism cannot mean anything else but generalised reflexivity. Thus, a consistent reflexivist should be reflexive about every belief, every view, even about the view about reflexivism, etc. Such a position is obviously regressive. In the empirical studies of Woolgar one can hardly find reflexivism at work. For instance, in a description of ethnomethodological fieldwork he finds 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). The truth about science cannot be heard in the interviews with scientists, according to Woolgar (*op cit.*), but 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.

To me, these passages seem to be essentialist, representational, and certainly unreflexive.²³ Thus reflexivity is not really applied in the fieldwork, and therefore such a seemingly radical relativism turns out to be still inconsistent.

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, 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 context and content – content is explained by context as if it were possible to distinguish between them. Callon asserts that it is not possible, for: “context and content are simultaneously reconfigured” (Callon 1994:51). 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 of laboratory science and the level of laboratory science appears fuzzy or even non-existent (1983:167).

Latour therefore 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 new 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

²³ In an earlier ethnographical 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 1986:283). 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.

not of our fabrication” (1992:281). Neither one nor the other can be used for explanations: “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 research objects and technology²⁴, and for giving clear advantage to human actors, in their explanations of science.²⁵ 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 units are even, their identity is defined in their mutual enrolments and translations.²⁶ Hence we do not need 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 became “Pasteur”²⁷, a revolutionary 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’ interest. He gave farmers a new social actor, the *microbe*, until then invisible reason of the terrible

²⁴ According to Latour the main mistake of the British sociologists is taking the concepts of science and technology essentialistically. He finds (1988:215) that “‘science’ does not exist. It is the name that has been pasted onto certain sections of certain networks”. 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 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.

²⁵ 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, 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).

²⁶ 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).

²⁷ Latour refers to the distinction between the man, Pasteur, and the ideas of Pasteur, the “Pasteur”, the former often reduced to the latter. (Latour 1988:13).

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 a less social actor, according to Latour, than the whole French hygienists' movement.²⁸ The *microbe* made possible the colonial wars without the dangerous infections, field surgery in the worldwar, 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.²⁹ It is important to notice that the political/social consequences of the laboratory activities cannot be predictable, and for this reason, not separable 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, society and science are all included in the changes, 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 (1992a:322) 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.

Nevertheless, according to the principle of mutual translations in the actor-network, also the scientist belongs to the network and is somehow defined by the other parts of the network, they are *defined, enrolled, translated*, so s/he is not an independent representative. S/he is a scientist as long as s/he is taken to be a scientist by the network. Besides that, the statements, scientific texts, etc. cannot be taken as representing reality, 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 1994:53). Latour's and Callon's symmetrism can be interpreted so that things turn out to be research objects when they are

²⁸ Latour describes the displacement of content and context on the example of the hygienists movement and Pasteur's role in the movement. The French hygienists were fighting for the improvement of welfare. For example, one of their goals was to improve public health. They saw the reasons of illness in the environment and therefore lacked the real actor, one single cause of the diseases. As soon as Pasteur gave them the *microbe*, their ideas had theoretical foundation, and thus, hygienists survive as a social movement thanks to Pasteur. On the other hand, the "Pasteur" is made by hygienists social network. See for details the first part of Latour 1988:3–146.

²⁹ See for further details Latour 1983 and 1988.

taken up as “objects”.³⁰ 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.³¹ 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, can be seen as an equivalent to social realism.³² We are usually natural realists about the past science, settled, certain and legitimated knowledge, in this sense we talk about scientific facts. Differently from this, in the science as process we see controversies, debates resulting in decisions about what counts as fact. Therefore sociological explanation is relevant here. The new symmetry³³, symmetry between the two sides, however does not mean alternation of natural and social realism, rather it must be taken so that nature and society are twin results of the network building (Callon and Latour 1992:348).³⁴

This is the point where one can notice relativist regress threatening the entire actant-network approach. Collins ja Yearley claim that if to consider actant-network theory in the light of its own methodology, the theory needs to be considered 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 (1992b:379) call such a rule of method that they find active in the actant-network theory, hypersymmetry. If hypersymmetry cannot be avoided, the attempts of consistent relativism necessarily end in relativist regress.

Nevertheless, Latour and Callon appear to manage the regress in their case studies, i.e., their pursuit of symmetry is realised without regressive symmetrism.

³⁰ The ordinary objects of scientific research are seen as quasi-objects – the term comes from a philosopher, Serres – 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).

³¹ 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” (In Latour’s interview to Callebaut: 472).

³² In this question Latour’s use of terms somewhat varies, he sometimes calls the two sides respectively realist and relativist (Latour 1989:107), sometimes he contrasts natural realism with social realism (Latour 1992:276)

³³ The term ‘new symmetry’ which is introduced by Latour can be seen as a synonym for Collins’ and Yearley’s term ‘symmetrism’ taken by them to characterise Latour’s method.

³⁴ In this context the authors talk about their *general symmetry principle*. See also Latour 1987:98–99.

This seems to be another kind of inconsistency – symmetry without the above claimed symmetrism, reflexivity without the above claimed reflexivism, and perhaps even relativity without relativism? In their empirical studies, Callon and Latour approach the restricted relativism, while theoretically disagreeing with the foundationalist inconsistency of restricted relativism.

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 claims:

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 a sociologist of scientific knowledge is supposed to study the social world of science in the same way natural scientists used to study the natural world. In his later articles, Collins often emphasises this close relation between SSK and scientific method as such: “Most practitioners of SSK, far from being against science, warrant their own work by reference to “scientific criteria” – careful observation, repeatability, and so forth” (1996:230). Elsewhere Collins notes that 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 “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, for instance, has made a case study on the attempts of building a TEA-laser.³⁵ Through his personal experience in the British laboratories, he tried to explicate the role of 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, be it laser-building or detecting the gravitational radiation,³⁶ involves a set of rules of interpretations taken for granted by the group of scientists. The unexplicated 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 uncover the hidden rules a sociologist of scientific knowledge must “go native”, obtain native competence in a local scientific culture.³⁷

The taken-for-granted rules become visible for even 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 a change of the whole set of interpretative rules are, basically, considered at the second stage of empirical programme of relativism.

At the second stage, an 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 institutional devices working within a context of ‘plausibility’ and other conservative forces” (Collins 1983:95–96). In a revolutionary stage, a set of leading experts from different institutions, called “core-set” by Collins, 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). Collins refers to a theoretical analogue of the core-set model, the “Hesse net”, a network structure of joint entrenchment of interrelated concepts. In “Hesse net” the relations between concepts are probabilistic and logical ones. Collins sees the relations to be “better described as

³⁵ For a detailed survey see Collins 1985 (2nd ed. 1992)

³⁶ About Collins’ case study concerning gravity waves see Collins 1981a, 1985, & 1996.

³⁷ Going native should be accompanied by an ability to alternate between “cultures”, because a sociologist should not become a scientist.

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 core-set and, respectively, the social relations between leading institutions. Collins demonstrates how the replicability of the experiments of Joseph Weber, the initiator of detection of gravitational fluxes, became rejected, step by step, by 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. Decisive role in the closure, in “changing the order” or getting the “ships into the bottles”³⁸ was played by rhetoric applied by Weber’s opponents. However, this does not mean that Weber’s opponents would have behaved incorrectly, quite the opposite, the rhetorical methods were reasonably combined with the taken-for-granted rules of action of wider scientific community – since the experimental results, in general, possess more weight than simply theoretical accounts, the opponents did experiments, though not as extensive as Weber’s experiment, 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, perhaps, what concerns wider social connections, social realism has been quite often criticised. On the one hand, critics point to the issue of impartiality, or neutrality. The above referred group of Australian sociologists of science finds that certain types of commitment are inevitable, therefore, anything an SSK analyst does can be seen to bring forth some political consequences. According to Scott, Richards and Martin (SRM), the political position should be declared openly at the outset of a study of science. However, 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. Collins (1996:231) illustrates the statement: “For 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”. Furthermore, the neutrality tenet should be seen as a norm, a rule guiding the scientific practice called SSK.³⁹

³⁸ 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.

³⁹ The interpretation of neutrality claim as a rule or norm refers indirectly to a possibility to accept the general reflexivity and generalised symmetry as similar norms. If this were so, the social realism would possibly lose in the strength of the argument against the two views of position-consistency-relativism.

On the other hand, and this is far more important to be noticed in the present work, the empirical programme of relativism is accused of altering the balance of power between science and culture. Both the Paris School and the reflexivist SSKers, such as Woolgar, certainly insist that social realism is inconsistent, for it replaces one kind of absolutism (natural) with another (cultural). Collins has shown in a number of cases that on closer consideration, the actant-network theory appears to come to a similar praxis. When Latour and Callon so-to-say black-box⁴⁰ 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 it was indicated above.

Another kind of criticism of EPOR comes from the natural scientists who do not agree with the proposed lack of empirical constraints in the theory choices. Nevertheless, even this argument can be paralysed in the context of alternation. The social realism of EPOR consists in considering science as a kind of act; inspired by the idea of institution of promising in John Searle's *Speech Acts*, Collins takes science and scientific knowledge to be institutional. In his reply to critics he 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).

However, according to Collins, the precise relation between the empirical and cultural constraints and their connection to wider social and political context, the multilevel structure of the institution of scientific knowledge needs further inquiry in a new "knowledge science" – there can hardly be a more precise term for such interdisciplinary study of knowledge?

⁴⁰ *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, chromatographs, etc. are seen as black-boxes producing data.

5. Conclusion: have we ever been consistent?⁴¹

Is consistency of relativism possible? Is consistency achievable in relativism? The problem of consistency in SSK appears in two particular forms. The first form of the problem concerns the relativist regress, self-refutation and skepticism of relativism. 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.

From a narrowly philosophical point of view, partial absolutism does not necessarily make relativism inconsistent, viz. self-refutational. Absolutism can be seen as an exclusive opposite to relativism only in the case of the relativisation of purely linguistic entities, such as statements and sentences with certain bipolar truth values. In the case of scientific beliefs, theories and models the values do not obtain in the sense of universal, mutually exclusive bipolar values. Here 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 holds on anti-absolutism as an attitude, or as a rule of action, which nevertheless does not exclude absolutism in certain respects and to certain degrees. Thus, historically and culturally varying scientific beliefs can be analysed and explained relativistically without the fear of inconsistency. Even more, relativistic explanation is tenable only as far as relativism is constrained to some levels, 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 variant 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, for it would assume, so to say, a capability to speak an unlimited number of languages at the same time, in an endless number of voices at the same time.

I considered in the paper how the consequences of the problem of consistency of relativism apply to sociology of scientific knowledge. I found there two opposite views, viz. restricted, modest 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 variants of modest, partially absolutist relativism, the first admitting two kinds of constraints, material and social constraints, the latter admitting only social constraints.

Woolgar and Latour on their part insist upon the requirement of consistency or relativism as such. They see the main purpose of the SSK studies in the removal

⁴¹ This is a paraphrase of the title of a recent book by Latour *We Have Never Been Modern* (Latour 1993).

of any kind of absolutism and in replacing the absolutism with true, i.e., consistent relativism.

In conclusion we may admit, however, that the variants or relativism which pursue or pretend the position consistency, inevitably end up in regress. Both the empirical programmes, either the semiotic analysis of Latour, or Woolgar's etnomethodological research are actually based on the hidden assumptions of respective position-consistent versions of relativism not being really valid. The symmetry principle works in Latour's actant-network-theoretical case studies only on the hidden assumption of symmetrism being invalid. Woolgar's reflexivity works only on the hidden assumption of reflexivism being inactive. If this is not a controversy, what is it then? It turns out that the more consistent a variant of relativism is, the more inconsistent it is at the same time is.

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

Address:

Endla Lõhkivi
Department of Philosophy
University of Tartu
3 Lossi St, TARTU, 50090

phone: 27/375 314

fax: 27/ 375 317

e-mail: endla@ut.ee

References

- Barnes, Barry (1992) "Realism, Relativism and Finitism". In *Cognitive Relativism and Social Science*. D. Raven, L. Van Vucht Tijssen & J. De Wolf eds. New Brunswick, N. J.: Transaction Books, 131–47.
- Barnes, Barry & Bloor, David (1982) "Relativism, Rationalism and the Sociology of Knowledge". In *Rationality and Relativism*. Martin Hollis & Steven Lukes eds. Oxford: Blackwell, 21–47.
- Bernstein, Richard J. (1983) *Beyond Objectivism and Relativism: Science, Hermeneutics, and Praxis*. Oxford: Blackwell
- Bhaskar, Roy (1978) *A Realist Theory of Science*. (2nd ed.) Sussex: The Harvester Press, N. J.: Humanities Press.

- Bloor, David (1991) *Knowledge and Social Imagery*. (2nd ed.) London, Chicago: Chicago University Press.
- Bohlin, Ingemar (1995) *Through Malthusian Specs? A Study in the Philosophy of Science Studies, with Special Reference to the Theory and Ideology of Darwin Historiography*. PhD Dissertation. The Department of Theory of Science, University of Göteborg
- Callebaut, Werner (1993) *Taking the naturalistic turn, or, How real philosophy of science is done: conversations with William Bechtel ...* [et al.] organized and moderated by Werner Callebaut. Chicago, London: The Un. of Chicago Press.
- Callon, Michel (1995) "Four Models of the Dynamics of Science". In *Handbook of Science and Technology Studies*. S. Jasanoff, G. E. Markle, et al. eds. London, New Delhi: Sage Publications, 29–63.
- Callon, Michel & Latour, Bruno (1992) "Don't Throw the Baby Out with the Bath School! A Reply to Collins and Yearley". In *Science as Practice and Culture*. A. Pickering, ed. Chicago: Chicago University Press, 343–68.
- Collins, H. M. (1981a) "Son of Seven Sexes: The Social Destruction of a Physical Phenomenon". *Social Studies of Science*, Vol. 11 (1981), 33–62.
- Collins, H. M. (1981b). "Stages in the Empirical Programme of Relativism". *Social Studies of Science*, Vol. 11, (1981) 3–10.
- Collins, H. M. (1981c) "What is TRASP? The Radical Programme as a Methodological Imperative". *Philosophy of the Social Sciences* 11, 215–24.
- Collins, H. M. (1982) "Special Relativism – The Natural Attitude". *Social Studies of Science* 12, 139–43.
- Collins, H. M. (1983) "An Empirical Relativist Programme in the Sociology of Scientific Knowledge". In *Science Observed: Perspectives in the Social Study of Science*. K. D. Knorr-Cetina & M. Mulkay, eds. London: Sage Publications, 85–114.
- Collins, H. M. (1985) *Changing Order. Replication and Induction in Scientific Practice*. London, Beverly Hills, New Delhi: Sage Publications
- Collins, H. M. (1992) "Afterword: Science Acts". In the 2nd ed. of *Changing Order*, 183–93.
- Collins, H. M. (1996) "In Praise of Futile Gestures: How Scientific is the Sociology of Scientific Knowledge?" *Social Studies of Science*, Vol. 26 (1996), 229–44.
- Collins, H. M. & Yearley, Steven (1992a) "Epistemological Chicken". In *Science as Practice and Culture* A. Pickering, ed. Chicago: The University of Chicago Press, 301–26.
- Collins, H. M. & Yearley, Steven (1992b) "Journey Into Space". In *Science as Practice and Culture*. A. Pickering, ed. Chicago: The University of Chicago Press, 369–89.
- Dancy, Jonathan (1985) *An Introduction to Contemporary Epistemology*. Oxford: B. Blackwell.
- Everitt, Nicholas & Fischer, Alec (1995) *Modern Epistemology. A New Introduction*. N. Y., St. Louis, etc.: McGraw-Hill, Inc.
- Harré, Rom & Krausz, Michael (1996) *Varieties of Relativism*. Oxford, UK & Cambridge USA: Blackwell.
- Harris, James F. (1993) *Against Relativism. A Philosophical Defence of Method*. LaSalle, Illinois: Open Court Publ. Co.
- Hesse, Mary (1974) *The Structure of Scientific Inference*, London: Macmillan.
- Hesse, Mary (1980) *Revolutions and Reconstructions in the Philosophy of Science*, Brighton, Sussex: The Harvester Press.
- Knorr Cetina, Karin & Mulkay, Michael (1983) "Introduction: Emerging Principles in Social Studies of Science". In *Science Observed: Perspectives in the Social Study of Science*. Knorr Cetina & Mulkay, eds. London: Sage Publications, 1–17.
- Lakatos, Imre (1971) "History of Science and Its Rational Reconstructions". In *Boston Studies in the Philosophy of Science* R. C. Buck & R. S. Cohen, eds. Vol. VII, Dordrecht: D. Reidel 91–136.
- Latour, Bruno (1983) "Give me a Laboratory and I will Raise the World". In *Science Observed: Perspectives in the Social Study of Science*. Knorr-Cetina & Mulkay, eds. London: Sage Publications, 141–70.

- Latour, Bruno (1987) *Science in Action: How to Follow Scientists and Engineers through Society*. Cambridge, Mass.: Harvard University Press.
- Latour, Bruno (1988) *Pasteurization of France*. Translated by A. Sheridan & J. Law. Cambridge, Mass., London, England: Harvard University Press.
- Latour, Bruno (1989) "Clothing the naked truth". In *Dismantling Truth Reality in the Post-modern World*. Lawson, H. & Appignanesi, L., eds. London: Weidenfeld and Nicolson, 101–26.
- Latour, Bruno (1992) "One more turn after the Social Turn...". In *The Social Dimension of Science*. E. McMullin, ed. Notre Dame, Ind.: University of Notre Dame Press, 274–94.
- Latour, Bruno & Woolgar, Steve (1979) *Laboratory Life. The Social Construction of Scientific Facts*. Beverly Hills, Cal.: Sage Publications.
- Laudan, Larry (1981) "The Pseudo-Science of Science?" *Philosophy of Social Science*, 11, 173–98.
- Margolis, Joseph (1986) *Pragmatism Without Foundations. Reconciling Realism and Relativism*, Oxford: Blackwell.
- Newton-Smith, W. (1982) "Relativism and the Possibility of Interpretation". In *Rationality and Relativism*. M. Hollis & S. Lukes, eds.. Oxford: Blackwell, 106–22.
- Niiniluoto, Ilkka (1987) *Truthlikeness*. Dordrecht: D. Reidel.
- Niiniluoto, Ilkka (1991) "Realism, Relativism, and Constructivism". *Synthese* 89, 135–62.
- Papineau, David (1987) *Reality and Representation*. Oxford: Blackwell.
- Papineau, David (1988) "Does the Sociology of Science Discredit Science?" In *Relativism and Realism in Science*. Robert Nola, ed. Dordrecht, Boston, London: Kluwer Academic Publishers, 37–57.
- Putnam, Hilary (1982) "Why reason can't be naturalized". *Synthese* 52, 1982, p. 3–23.
- Scott, Pam, Richards, Evelleen & Martin, Brian (1990) "Captives of the Controversy: The Myth of the Neutral Social Researcher in Contemporary Scientific Controversies". *Science, Technology & Human Values* 15, 474–94.
- Searle, J. R. (1969) *Speech Acts: An Essay in the Philosophy of Language*, Cambridge: Cambridge University Press.
- Taylor, Charles (1982) "Rationality". In *Rationality and Relativism* Hollis & Lukes, eds.. Oxford: Blackwell, 87–105.
- Woolgar, Steve (1983) "Irony in the Social Study of Science". In *Science Observed: Perspectives in the Social Study of Science*. K. Knorr-Cetina & M. Mulkay, eds. London: Sage Publications, 239–66.
- Woolgar, Steve (1988a) *Science: the Very Idea*. Chichester, Sussex & London: Ellis Horwood & Tavistock Publications.
- Woolgar, Steve, ed. (1988b) *Knowledge and Reflexivity*. London: Sage Publications
- Woolgar, Steve & Ashmore, Malcolm (1988) "The Next Step: an Introduction to the Reflexive Project". In *Knowledge and Reflexivity*. Woolgar, ed. London: Sage Publications, 1–14.
- Woolgar, Steve (1992) "Some Remarks about Positionism: A Reply to Collins and Yearley". In *Science as Practice and Culture*. A. Pickering, ed. Chicago: The Un. of Chicago Press, 327–42.