GÁBOR KUTROVÁTZ

An Epistemological Cross-Section of Science Studies

In the Context of the Science Wars

– PhD dissertation –

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1. INTRODUCTION

1.1 Plan of the Work

In this work I attempt to outline and examine, in the context of the debates called 'the Science Wars', an epistemological cross-section of the field of science studies. The term 'epistemological cross-section' was chosen to suggest some features of my approach.

First, while science studies can be examined from a number of, partly interconnected, perspectives, my concern is mainly epistemological: I focus on accounts of scientific knowledge, i.e. for science studies what it means to know in science. Methodological, metaphysical, ontological, moral, political etc. questions will be addressed only insofar as they are primarily relevant to the ways epistemological questions are dealt with. While such problem areas will be shown to be intersected by the epistemological dimension, I will discuss them no further than it seems necessary from my epistemological point of view.

Second, what I try to sketch here is *an* epistemological cross-section, and not *the* epistemological survey of the field. I will proceed alongside certain problems: those that seem primarily responsible, although most often covertly, for the recent rage against science studies. Identifying these problems is more an empirical question than a philosophical one, and in order to find them I will set out from frequently formulated criticisms of the discipline. Since I agree with many analysts in that these criticisms are often based on misunderstanding of, and superficial acquaintance with, central claims of the field, my purpose is to discuss these claims at a more attentive level where substantial inquiry becomes possible.

Third, in drawing a map of an epistemological cross-section I do not expect to find homogeneous commitments universally characteristic to the whole of science studies.[1] Rather,

[1] The term 'epistemological cross-section' was inspired by Gaston Bachelard's concept of *profil épistémologique* (Bachelard 1962: 41-51). The term was used to capture, on empirical grounds, the diversity of philosophical commitments within the usage of one and the same scientific concept (such as mass, energy), as opposed to ascribing single philosophical positions to scientific conceptualisations. 'Cross-section' is a literal translation of what stands for '*profil*' in the Hungarian translation of Bachelard's work, and the reason I chose this term is that it seems to express better the way I want to conduct a partly empirical inquiry into the diversity of epistemological commitments of a discipline, rather than, as did Bachelard, examining the complex philosophical attitudes in the usage of certain concepts. While the term 'profile' seems to capture an external image of the field looking from a certain perspective, a 'cross-section' should reveal the very internal workings that may be hidden from the outsider's view.

it will be seen that disagreements emerge between even the most prominent representatives, and such theoretical contrasts often frame divergent trends in science studies. Again, I will primarily focus on those trends and figures that, intentionally or not, most often appear in the Science Wars, usually as targets of criticism. However, some disagreements, both essential and less important, will nonetheless be highlighted.

Fourth, my purpose is not to construct a continuous and linear philosophical narrative. I will rather visit and problematise a bunch of issues that the epistemological cross-section reveals. Coherence is intended to be imposed on the work by specifying a precise problem situation from which I proceed toward its several consequences in the epistemological dimension. As a result of my approach, no single and unique thesis will emerge from the investigation of these topics, and I am bound finally to conclude with drawing a general context in which the basic issues discussed can be reformulated. The originality of this work is meant to consist in the way in which the topics are selected, unfolded, connected and contextualised.

In order not to choose too broad a scope for this work, I will concentrate on only one side of the Wars, and the other side will not be discussed in its own right, but only as contrasted to science studies. In that, my account is irrecoverably asymmetrical and, I have to admit, it is rather biased. My excuse is that, instead of taking sides in the Wars against scientists and defenders of science, I want to show why a science studies approach to scientific knowledge is a legitimate one, and why its alleged refutations are misconceived. Hopefully this conscious bias will prove not to exclude the possibility of a careful and factually impartial analysis.

In the following three sections of this first chapter I will outline the context in which the main problems will be formulated, and the central problem situation will be introduced. After giving a summary of the Science Wars (1.2), I will be able to place science studies in a context where some preliminary distinctions and characteristics emerge (1.3). The initial problem, i.e. the meta-scientific nature of science studies, will be formulated on methodological terms, related to the overall undertaking of the discipline (1.4). This prepares the ground for the dissertation to set out to examine two main issues.

The second chapter is devoted to, as it is often called, the problem of idealism, i.e. the tendency in science studies accounts of scientific knowledge to avoid any reference to the natural or material world as the object of knowledge. After presenting the problem in the context of the Wars (2.1), I examine different strategies in science studies to deal with it or to dismiss it (2.2). Among those who take idealism justified, a methodological approach will be

illustrated by works of Harry Collins (2.2.1), and then contrasted to a philosophical solution, offered by Karin Knorr-Cetina, with some ontological underpinning (2.2.2). Then David Bloor's strategy will be shown to attempt to develop a non-idealistic position in a naturalistic framework (2.2.3), and finally I will discuss Bruno Latour's approach that seeks to invalidate the problem by deconstructing the whole epistemological setting where it can be formulated (2.2.4). Having discussed this variety of attitudes, I offer a way to fit the problem in a general philosophical background (2.3). After arguing for placing the position of science studies in a broad Kantian tradition, for dealing exclusively with the subject of cognition (2.3.1), I will examine some key differences between Kant's original philosophical approach and that of contemporary science studies (2.3.2), and conclude with some remarks concerning the scope of sociological explanations of scientific cognition (2.3.3).

Chapter 3 addresses the issue of naturalism, i.e. science studies' inclination to investigate scientific knowledge in non-philosophical, scientific terms. The problem will be introduced with reference to the descriptive, as opposed to normative or evaluative, prose of explanations, as related to the initial methodological position (3.1). Being non-evaluative with respect to scientific knowledge often prompts the charge of anti-science in the Wars, and this invokes the problem of the esteem of science in general (3.2). I will sketch two opposing answers to the question, on what grounds science should be evaluated (3.2.1), and this will prepare the ground for introducing the topic of public understanding of science (3.2.2). Robbing science of its usually granted positive esteem can serve different purposes, as will be shown with reference to classical sociological concepts such as 'desacralisation' (3.2.3) and 'unmasking' (3.2.4). The last broad topic discussed in this work will concern different aspects of sociological, understood as scientific, explanations of scientific cognition (3.3). The scientific character of explanations will be interpreted with respect to a preference of the causal idiom (3.3.1), and some broad types of sociological explanation will be distinguished along the local/general distinction (3.3.2). The question regarding the scope of these explanations will be found related to the problem of contingency (3.3.3), and sociological accounts of cognition will be contrasted to psychological naturalism and explanations preferred by cognitive science (3.3.4)

Chapter 4 concludes with some general remarks. After summarising the most important claims made throughout this work (4.1), I try to show, by drawing a context from the history of philosophy, why the most important charges made against science studies in the Wars are based on unwarranted expectations stemming from an unreflected external position in which criticisms are formed (4.2). In particular, I will use partial conclusions formulated

throughout this work to undermine some aspects of the initial problem situation, such as the implicit assumption of the external relation between science and society, or the applicability of the subject/object epistemological scheme, taken from traditional individualistic epistemology, to problems of collective cognitive processes. This work thus hopes to show the need for proper attention and comprehension before substantial dialogue can begin.

1.2 A Historical Summary of the Science Wars

Science Wars (henceforth SW) is a series of controversies and debates about science. Although the term has been used widely since it was coined (Ross 1995: 346, quoted in Sokal 1998: 18), both the participants and the analysts agree that it seems practically impossible to come to a public consensus as to between whom, and on which terms, the war is fought. Instead of trying to specify clear-cut definitions, I first give a list of events that are often recollected under the name of SW.[2]

SW was initiated in 1994 by a group of scientists who wanted to give voice to their dissatisfaction with the prevailing 'relativistic' and 'postmodernist' atmosphere in the American academia with respect to the evaluation of science. The target of their rage was a very diverse group, ranging from feminist epistemologists and cultural leftists to postmodernists and multiculturalists, including New Age authors and sociologists of scientific knowledge, etc. Besides publishing a very influential book (Gross and Levitt 1994), they organised a couple of conferences with the agenda of purging science from external misuses and abuses.[3] These 'science warriors' set out to identify and combat all kinds of 'anti-science' movements (Holton 1993) undermining the public respect for science, and thus jeopardising human rationality, Western civilisation and democratic society.[4]

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[2] A detailed source for the history of SW is Segerstråle (2000b).

[3] Two conferences: "Objectivity and Truth in the Natural Sciences, Social Sciences, and the Humanities", 1994 November, Boston; and "The Flight from Science and Reason", 1995 June, New York. The latter's materials were published as Gross, Levitt, and Lewis (1996). Some personal recollections are reported in Martin (1996).

[4] As E.O. Wilson, sociobiologist and science warrior famously put it, "Multiculturalism equals relativism equals no supercollider equals communism." (Wilson 1994, quoted in Franklin 1996: 152) The closedown, in 1993, of the Superconducting Supercollider in Texas, "the greatest project of postwar science", was seen by many scientists as a clear mark of "the end of Big Science" (Cholakov 2000: 131), and the beginning of a new cultural irrationalism.

At the same time, several other events indicated the culmination of anti-antiscience sentiment. A grandiose exhibition presenting science in cultural context, 'Science in American Life' at the Smithsonian Institute in 1994, enraged some scientists by supposedly conveying explicit anti-science bias, and the organisers were eventually convinced to 'cleanse' the show of 'misrepresentations'.[5] The case spurred a heated controversy between sociologist Thomas Gieryn (1996) and biologist Paul Gross (1996). Other debates were also triggered, such as the one, about historian of medicine Geison's (1995) book on Louis Pasteur (a piece of social constructivist history of science), between molecular biologist Max Perutz (1995, 1997) and supporters of Geison (Summers 1997, see Dear 2001a). The atmosphere was somewhat similar in Britain, where former embryologist and then prominent science populariser Lewis Wolpert, having published his pro-science defence (Wolpert 1992), became engaged in public debates with Harry Collins, a distinguished sociologist of scientific knowledge.[6] As a result of these clashes, camps were emerging on both sides.

In reply to the fierce and often hasty anti-antiscience activism, Andrew Ross, editor of the cultural studies journal *Social Text*, decided to dedicate a special issue to what he started to call the 'science wars'. With the purpose of 'bridging the Two Cultures', the 1996 Spring/Summer issue of *Social Text* was offered as a forum for clarification and defence to some of the authors attacked, or sympathetic to those attacked, by the science warriors. However, the gesture was spoiled by a cuckoo-egg paper by New York physicist Alan Sokal who, while pretending to offer a scientifically illustrated defence of 'cultural relativist' science criticism, actually submitted a hoax article of smartly assembled nonsense. As he soon revealed, his intention was to show that cultural studies scholars lack the intellectual standards to demarcate between meaningful and gibberish in their own field.[7]

The 'Sokal hoax' made an unexpected upheaval world-wide, reaching the cover of newspapers such as the *New York Times*, the *Observer*, *Le Mond*, etc.[8] Since both within and without the academia huge numbers of people became interested in the background of the

[5] Such as the aspects of commercial interests, environmental problems and social minorities. See some details in Winner (1996), Bauer (2000), and Gregory and Miller (2001).

[6] Collins, together with Pinch, had published a popular book (Collins and Pinch 1993) on the workings of science that offered an image diametrically opposite to the one presented by Wolpert (1992). For details of the debate, see Rose (1996) and Fuller (2000).

[7] The hoax paper is Sokal (1996a), and Sokal (1996b) is the revelation. The *Social Text* special issue was later published as Ross (1996), including a few additional papers, and with the emission of Sokal's contribution.

[8] Many important non-academic press reactions are collected in 'Editors of *Lingua Franca*' (2000).

hoax, it provided "the oxygen of publicity" (Rose 1996: 80) to SW. As a result, the stakes increased, and the war had its victims. When distinguished historian of science Norton Wise (1996) criticised a *New York Review of Books* article on the Sokal hoax written by Nobel laureate physicist Steven Weinberg (1996), it probably cost him a Research Professor position in the Princeton's Institute of Advanced Studies (McMillen 1997). The vehement debate in *Science* between historian of science Paul Forman (1997a, b) and defenders of Gross, Levitt, and Lewis (1996) (Herschbach 1997, Levitt 1997, Trefil 1997) is said to "have contributed to the resignation of the book editor" of the journal (Segerstråle 2000b: 21).

Having received a huge number of demands for further explanation of the hoax's background, Sokal, together with the Belgian physicist Jean Bricmont, published a book-length criticism of what they identified as the intellectual sources of anti-science relativism (Sokal and Bricmont 1997 in French, Sokal and Bricmont 1998 in English). Here the authors blame French postmodernist and post-structuralist philosophers, illustrated by numerous citations, for spreading an alluring but nonsensical intellectual mentality and jargon in general, and for bringing into academic fashion the abuse of scientific concepts and terms in particular. In a similar manner, a collection of pro-science papers edited by the philosopher of science Noretta Koertge (1998a), although keeping the eclectic tradition of Gross and Levitt (1994) of bringing all kinds of 'enemies of science' together, bore the subtitle 'Exposing Postmodernist Myths about Science".[9]

Gross, on the other hand, opened a new chapter in SW with an article (Gross 1997) in which he claimed to have found the roots of anti-science corruption in science and technology studies. From then on, the focus of interest shifted from alleged forms of postmodernism to different trends in contemporary sociology of science, such as the so-called sociology of scientific knowledge. The readers of *Physics Today* had already witnessed a controversy between physicist David Mermin (1996a, b, c, 1997) and sociologists Harry Collins and Trevor Pinch (1996, 1997), and soon Mermin (1998a) also attacked Barnes, Bloor and Henry (1996) – see replies in Barnes (1998), Bloor (1998) and Mermin (1998b). A peace-proposing volume co-edited by a scientist and a sociologist (Labinger and Collins 2001) lately portrayed SW as a clash between sociologists and scientists.

[9] On the other hand, many writings in Koertge (1998a) clearly represent a step forward in the debate by concentrating, beside the theoretical legitimacy of science studies, on the empirical adequacy of case studies. See some replies and further discussions in the 1999/2 issue of *Social Studies of Science*.

In the past few years, the initial fury of SW has either been intimidated and fuelled into more substantial academic discussion, or has simply died down as SW fell out of wider interest.[10] A number of analyses have come out, providing accounts of SW that inevitably vary with, and rely on, the authors' preconceptions.[11] In the next section, a similarly incomplete and partial account will be briefly offered.

1.3 Science Studies in Context

The above historical summary provides a rough basis for making preliminary distinctions and bringing some contexts into play. While disregarding a lot of important aspects, I will distinguish between two, both temporally and thematically divergent, faces of SW.

When SW broke out it was often characterised as a new front in the so-called 'Culture Wars'. Beginning already in the 1970s and flourishing in the 80s, a series of sporadically related debates spread over the American academia, where the fundamental values of Western civilisation and modernity were taken under fire. It was frequently claimed, with reference to the given historical situation, that the cultural values of the modern West tended to monopolise the whole world, and that previous cultural diversity was rapidly giving way to one uniform global civilisation. Furthermore, post-colonial 'oppression mentality' was shown as a constitutive feature of modernity in a number of respects: first, with regards to other cultures, second, within Western societies where privileged social groups strive to capitalise all values, and third, in the process of exploiting the natural environment and thus jeopardising our own future. These undesirable processes were contrasted with the promises of diversity, multiculturalism, political correctness, green peace, etc. Like in the case of SW, the backlash of pro-modernity 'universalists' was often harsher than the criticisms. It is also important to note that the intellectual and conceptual features of the anti-modernist discourse

[10] Another peace proposing volume in 2001, collecting the materials of the 1997 conference 'Science and Its Critics' in Kansas, was given the optimistic title *After the Science Wars* (Ashman and Baringer 2001) – expressing the editors' hope for a new era of "science peace talks" (2).

[11] For example: Herrnstein Smith (1997), Gieryn (1999), Brown (2001).

were essentially shaped by French intellectuals such as Lacan, Foucault, Derrida and Lyotard, being extremely popular in the humanities but seen by many as apostles of 'irrationalism'.[12]

'Cultural studies' is a loosely coherent inter- and multidisciplinary field with the purpose of critically re-examining the most fundamental theoretical commitments of contemporary culture. It represents the anti-modernist standpoint in the Culture Wars. Science studies in the first sense is the name for cultural criticism aimed especially at science as one of the most fundamental institutions of our culture. Scholars in this field are engaged in, on the one hand, revealing all kinds of social asymmetries (gender, race, social status, nationality, etc.) encoded by the inherent cultural structure of modern science, and thus, on the other hand, proclaiming a negative evaluation of certain basic aspects of science that should be radically transformed. Among the intellectual sources, we can find the postmodernist philosophers with their rejection of 'grand narratives' such as a universalistic conception of science (Lyotard 1979), the so-called post-Kuhnian (Kuhn 1962) philosophy of science with its relativist and anti-realist dispositions, different trends in the sociology of science and sociology of knowledge (see below), and all sorts of cultural movements such as feminism, neo-Marxism, multiculturalism, etc. SW, in the original sense of Gross and Levitt (1994) and Ross (1996), is best seen as an opposition between scientists and science studiers thus understood.

Science studies in the second sense is a field applying the toolkit of social sciences to empirical inquiries into the practice of science. On the one hand, it relies on traditional sociology of science (e.g. Merton 1973) when studying the institutionalised forms of science as social phenomena, but it also goes beyond it by extending the scope of research to the cognitive content of scientific theories as well. On the other hand, it improves on classical sociology of knowledge (e.g. Mannheim 1929) by, in opposition to it, regarding scientific knowledge socially contingent in the same way as all forms of knowledge are seen dependent on social conditions. In sum, the discipline of science studies examines scientific knowledge in practice as relative to social context.

It seems that the first explicit, and probably the most influential, science studies enterprise was the so-called 'Strong Programme' in the sociology of scientific knowledge (SSK), initiated by Bloor (1992 [1976]), Barnes (1977) and others. It was characterised by four planks: (1) causality: it is "concerned with the conditions which bring about belief, or

[12] Anti-modernist trends were reinforced at the beginning of 1990s when bipolarity in the political world-picture disappeared and the global capitalist world order began to rule. It was often claimed that humankind had entered its final state of cultural 'heat death', and the historical progress was coming to an end (see Fukuyama 1992).

states of knowledge", (2) impartiality: "it would be impartial with respect to truth and falsity, rationality or irrationality, success or failure", (3) symmetry: "the same types of cause would explain, say, true and false beliefs", and (4) reflexivity: "its patterns of explanation would have to be applicable to sociology itself" (Bloor 1992: 7). Of these four planks, the first three constitute the 'hard core' of SSK.[13] Versions of this programme were employed in studies of scientific laboratory work (Latour and Woolgar 1979, Knorr-Cetina 1981), analyses of scientific controversies (Collins and Pinch 1982), histories of older (Shapin and Schaffer 1985) and recent (Pickering 1984) science, theories of technology (Bijker, Hughes and Pinch 1987), etc. Because of their strong interest in, and emphasis on, the inevitably social character of scientific knowledge production, these authors were often labelled 'social constructivists'. Later events in SW were frequently clashes between scientists and science studiers in this second sense.[14]

The focus of this dissertation is limited to the second face of SW. Postmodernism or cultural (and science) criticism will not be considered, nor their possible connections with social studies of science. It will be assumed, and partly argued, that SW sheds new light on a now long-standing opposition between traditional philosophies of science, based on a positivistic and positively evaluative picture of science, and the post-Kuhnian attitude of neutrally describing science as an institutional form of culture. Hence I am mainly concerned with widely acknowledged problems in studies of science.

Let me add here that science warriors have indeed sound reasons to worry about the present 'hard times' for science. On the one hand, respect for science in contemporary societies seems to have lessened since the Cold War era, or, at least, science has lost much of its narrative function in providing a global frame of world-explanation and promising an optimistic 'science fiction' future (e.g. Fuller 2001). On the other hand, the organisational

[13] The reflexivity principle is claim that is often seen as heavily problematic and that has almost routinely been used, for its alleged implication of self-refutation, in sweeping criticisms of the Strong Programme. In this dissertation it will have a role less important than that of the other three planks. While, from an epistemological point of view, it is highly questionable whether this principle can be detached from the "hard core" of SSK, it seems that the central figures in the Strong Programme make less use of it than of the other three tenets. For further elaborations on the principle, see e.g. Woolgar (1988) and Ashmore (1989).

[14] This account inevitably oversimplifies both the historical process of the field's emergence and the variety of different approaches within it. While the Strong Programme played a major role in motivating these studies, it is surely not the only recent source. Also, while many authors within the field would, or actually do, object to the label 'social constructivist', critics of their approach often lump them together with others under this name.

conditions of doing science seem to be witnessing radical transformations, becoming explicitly governed by economic-commercial needs rather than the ideology of a pure desire to understand the world, and the 'post-academic' (Ziman 1994, 2000) or 'Mode 2' (Gibbons et al. 1994) science will lack the cultural dignity we customarily attribute to science as paragon of rationality. However, contrary to the claims of many science warriors, when only the relative social and cultural weight of science studies is compared to that of science, it seems almost absurd to put the blame for these processes on the social studies of science, or on criticisms of science in the cultural studies (see Nelkin 1996, Gregory and Miller 2001). It is the more interesting, then, to examine why science warriors found their scapegoat in science studies.

1.4 Perspectives and Levels of Study

The starting point of the following analysis is a very simplistic comparison between the perspectives taken by the two sides in SW. The assumption will be made that science studies is the 'science of science'.[15] In other words, an analogy will be pursued between science as a discourse about nature, and science studies as a similar meta-discourse about science.[16]

Science is a descriptive enterprise with regards to things (in nature). In other words, the ontological domain of causal explanations is the realm of nature.[17] Science studies, in a similar fashion, is a descriptive enterprise with regards to science as a social phenomenon, and particularly scientific beliefs about nature. The ontological domain of causal explanations is the realm of the social, including institutions, interests, 'forces' and 'fields', scientific and

[15] This term is used here only metaphorically. According to Cholakov (2000: 126-127), the term 'science of science' was spread, from 1959 on, by a movement with the same name developing on John Bernal (1939); see also Goldsmith and Mackay (1966). Although this movement is not a central source of SSK, and I have not met any explicit use of the term by its practitioners, it is often used informally to characterise it (e.g. Mermin 1998a), especially since Larry Laudan, in his "The Pseudo-Science of Science" (1984), cited a number of passages from Bloor (1976) emphasising the scientific nature of SSK. The 'scientific' character of science studies will be further examined in Chapter 3.

[16] A profound discussion of the reflexivity thesis may call for the need to consider my own position as meta-metalevel, i.e. a metalevel of science studies. My treatment of the issue is not reflexive in this sense.

[17] No relevant distinction is made here between description and explanation, both meaning, in this respect, account of how the subject of discourse is.

extra-scientific beliefs, etc. It should be noted that, while followers of different sociological trends tend to disagree about what types of entities constitute the realm of the social (see Chapter 3), this is not completely dissimilar to how the ontology of the natural sciences varies with disciplines and over time. However, this has no bearing on the overall methodological positions discussed here.

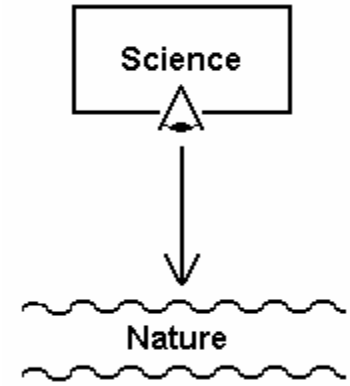

Figure 1: The perspective of science

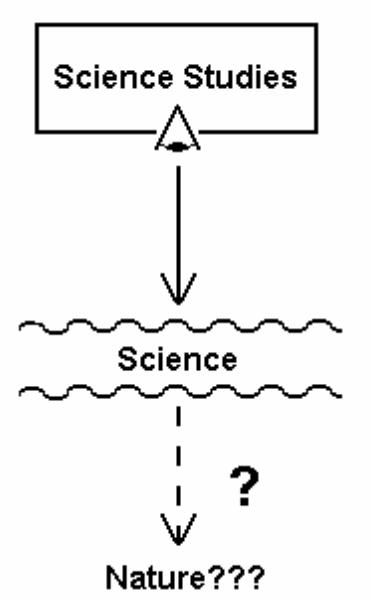

Figure 2: The perspective of science studies

Figures 1 and 2 address the issue of what theories of a certain discipline are expected to talk about.[18] By comparing them, we can also note some points concerning what these disciplines are not enabled to talk about. First, assuming that the 'inter-level accessibility relation' is not transitive (as seen in figure 2), science studies is incompetent with respect to

[18] Figures in this work have been criticised by numerous readers of earlier versions (or of Kutrovátz 2004a), mostly on the basis that they are too simplistic, especially to express the positions of specific authors in Chapter 2.2. It must be emphasised that, first, they are not meant to convey all subtleties of different stances: on the contrary, they are presented as oversimplified pictures that blur some distinctions in order to highlight others. Second, they do not show how I see the discussed positions in general, but how I see them appear in the context of the related controversies. Often in science studies, such a rigid separation of levels is not justified, as it will be suggested in Chapter 2.2 and reexamined in the concluding sections. However, I still find these figures useful as tools that help formulate initial problems, and I hope that they do not put unnoticed constraints on further discussion.

things in nature. Because sociological research has no access to nature independently of science as the very object of its study, 'nature' appears only in beliefs about it. Hence the reliance on any responsibility of things for beliefs about them is rendered highly problematic. In other words, since 'things' as objects of beliefs are not parts of the domain of explanation, nature is bracketed out when explaining beliefs about it. This is referred here as the problem of idealism, which will comprise the central issue of Chapter 2.

Besides, figures 1 and 2 suggest that what scientific discourse, as opposed to that of science studies, is not intended to talk about is science itself. This is by no means a grievous restriction, and it can be backed up, for example, by considering that any descriptive discourse taking on the task of describing itself invokes the problem of circularity. It follows that when scientists, quite inevitably, start talking about science, they abandon scientific discourse and the language they use becomes charged with normative and evaluative terms. Scientists hold an initial trust (or even belief) in science as an enterprise with privileged access to its subject,[19] and they employ value-laden terms such as 'objectivity', 'rationality', or 'truth' as tools to account for the (guaranteed) success of science.

For science studies, on the other hand, being descriptive concerning science means to be obliged to avoid being normative or evaluative with regards to it. This, besides realising an approach to science that seems unusually irreverent, looks to block the way to deal with questions central to most traditional inquires about science, such as the nature of rationality, objectivity, truth, since *prima facie* these concepts are based on normative distinctions of a 'good/bad' type. Science as a social phenomenon is rendered 'natural' in sociological explanations. In this dissertation, Chapter 3 will deal with the problem of naturalism.

The tension between scientific and metascientific perspectives is nicely illustrated by two teasing reflections on their relations. One is "Richard Feynman's famous (perhaps apocryphal) judgement that philosophy of science is about as useful to scientists as ornithology is to birds" (Kitcher 1998: 32).[20] The other is philosopher of science Imre Lakatos's 'pet' claim saying that "most scientists tend to understand little more about science than fish *about* hydrodynamics" (Lakatos 1970: 148n, emphasis in the original). That is, while scientists can do what they do without the need of theorising about it, analysts of science may know things about science that practitioners of science are ignorant of. But if the case is that

[19] Theoretical physicist Peter Saulson (2001) eloquently confesses this position. The opening sentence is: "I believe in the Church of Science." (227) Saulson (2004) further elaborates on the topic.

[20] Prominent physicist and science warrior Steven Weinberg (1992) quotes this opinion in a chapter entitled "Against Philosophy".

simple, why use such ironical analogies? And if we come to science studies, which suits these quotations even better than the philosophy of science, why is irony replaced with enmity and war?[21] This dissertation seeks to find partial answers to this question.

[21] The two sides in SW are taken by scientists and science studiers, and the place for traditional philosophy of science is quite problematic. Philosophy of science sides with science studies in being an enterprise contrived to examine science. However, traditional philosophers of science maintain an essential role for their positive evaluation of science, and they are inclined to become engaged, as a rule, in a normative discourse that relies on this presupposed evaluation. For this reason, they sometimes join science warriors in their rejection of science studies, like in the case of Boghossian (1998) and Koertge (1998b). In this dissertation philosophy of science will not play a significant role in its own right, but only as related to central problems.

2. IDEALISM IN SCIENCE STUDIES

2.1 The 'Epistemological Fallacy'

As suggested by figure 2, there is a problem with the 'lower levels'. Taken at face value, nature, as the object of science, is not directly accessible from the science studies perspective, i.e., science studies sees nature only through science, as given by it. This methodological position makes it illegitimate for science studiers to attribute agency to natural objects in their explanations[22]—unless either the nature of agency is left totally unspecified and transcendent, or they rely on an epistemic access to natural things other than, and independent of, that of science, or else they allow for a kind of circularity in using scientific knowledge as a source, and not only the object, of explanation. While these options will in part be examined, let us set out from the strict position where nature is taken invisible, and hence there is no possibility to make a systematic distinction between beliefs about nature and nature 'in itself'. This attitude is sometimes exemplified by what Roy Bashkar (1986: 23ff) calls 'the epistemic fallacy', which "conflates ontology and epistemology" (Sismondo 1993: 535) by not distinguishing between talk of things and talk of beliefs.

The epistemic fallacy is splendidly illustrated by Bruno Latour's famous 'third rule of method', a subscription for analysts of science:

> Since the settlement of a controversy is the *cause* of Nature's representations, not the consequence, we can never use the outcome—Nature—to explain how and why a controversy has been settled. (Latour 1987: 99, 258; emphasis in the original)

The example Latour examines is the solar neutrino debate of the 1980s, subject of a classical SSK case study written by Trevor Pinch (1986). Latour suggests, in an ironical tone, that the controversy can nicely be settled by seeing "for ourselves in which camp the sun is really to be found". He continues: "Somewhere the natural sun with its true number of neutrinos will close the mouths of dissenters and force them to accept the facts no matter how well written these papers were." (Latour 1987: 95) This is a caricature of the extreme realist position

[22] According to Norman Levitt (1998: 272), a leading anti-science warrior, it is the "central dogma" of science studies that "'nature' plays only an auxiliary role—possibly no role at all—in the determination of scientific truth".

characterised by the claim that 'we say "X is the way the world is" exactly because X is the way the world is'. Science studiers would have an easy job in explaining the emergence of a scientific consensus about, say, the flux of solar neutrinos simply by referring to the 'real' flux of solar neutrinos. The problem is, of course, that they have no knowledge of the matter independently of the very consensus that is about to be explained.

The third rule is a favourite target of Sokal's and Bricmont's criticism (e.g. Sokal 1998: 13, Sokal and Bricmont 1998: 93, Bricmont and Sokal 2001a: 28), who ask why Latour slips from 'Nature's representation' to 'Nature', apparently handling them as if they were the same. They come to the conclusion that the reason is simply "sloppy thinking", which consists in conflating different levels of analysis such as ontology, epistemology, sociology of science, etc. (Sokal 1998: 14-15). For them, the "correct answer to any scientific question, solved or not, depends on the state of Nature (for example, the number of neutrinos the Sun really emits)" (Sokal and Bricmont 1998: 97), and they hope that "the controversy will be settled […] thanks to an accumulation of evidence that, taken together, will indicate clearly the correct solution" (95).

They also attempt to give an acceptable (and "undoubtedly *not* Latour's own") interpretation of the third rule:

> Let us read it as a methodological principle for a sociologist of science who does not himself have the scientific competence to make an independent assessment of whether the experimental/observational data do in fact warrant the conclusions the scientific community has drawn from them. (Sokal and Bricmont 1998: 97-98)

This is perfectly in line with the implications of the science studies perspective, only with the qualification that this is a question not of competence but of methodological commitment. But the "sensible conclusion" for them to draw is that "sociologists of science ought not to study scientific controversies on which they lack the competence to make an independent assessment of the facts…" (98). However, this would render science studies completely impotent, since non-scientists have no such competence independently of science. Luckily they do not need it: what they are trying to make an independent assessment of is not scientific but sociological 'facts',[23] which means a different level of study.[24]

[23] I am using the term 'fact' in an analogical sense here, with no intention to specify any ontological commitment to an image of the social realm. However, the essential point to make is that in science studies, ontological

While Sokal and Bricmont's claim for the illegitimacy of the methodological attitude of science studies seems clearly unwarranted, they are partly right in asserting that Latour has different motivations (see Section 2.2.4). According to Steven Fuller (2000: 204), "[t]he tendency to conflate epistemology and ontology is not an intellectual failure but a philosophical standpoint nowadays called 'antirealism'".[25] Since I am neglecting technical issues specific to philosophy of science, instead of 'antirealism' the (more provocative) term 'idealism' will be used, meant to express the 'bracketing' of nature in explanations of beliefs. This is a highly unusual and counterintuitive position to account for science (as shown e.g. by Sokal's and Bricmont's reactions), and usually it is problematic to defend both the methodological and the philosophical motivations of an idealistic form of science studies. However, this work hopes to show that what seems an idealistic position from the scientists' perspective, is thoroughly realistic for science studiers who reproblematise the traditional distinctions between nature, science and society.

This chapter examines different solutions to the problem of idealism. The order of discussion is rather *ad hoc*, and it is not meant to represent any temporal or logical order of the examined positions. Also, the reconstructed positions are static, and temporal changes within each author's views are neglected. The point here is to trace some strategies how, based on existing views and arguments, the charge of idealism can be either accepted, with various qualifications, or rejected on different grounds. The term 'strategy' does obviously not apply to the programmes themselves, but to the ways how they would deal with the problem of idealism.

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commitments concern the realm of the social, and not any image of the world deprived of social dimension. When knowledge is assigned social reality, the study of knowledge is not epistemology *as opposed to* ontology, hence the charge of 'epistemological fallacy' is misconceived.

[24] This does not mean, however, that sociologists need not understand, to certain degree, the claims of science under study. Collins and Evans (2002) introduce the distinction between 'contributory expertise' and 'interactional expertise': while the former is required to make any regular contribution to a field, the latter is what one needs to understand the relevant ideas and principles, and to be able to engage in substantive communication with contributors, but without being able to contribute. While contributory expertise is found unnecessary to have for a sociologist of scientific practice, interactional expertise is taken as a requirement.

[25] Collins (1999a) makes a similar observation: "Sokal and his philosophical claque seem not to have understood that the abolition of the difference between ontology and epistemology is the very project of the philosophically inclined postmodernists". Then he immediately switches to his own methodological reading (see next section): "He [Sokal] has not noticed that ignoring the difference between ontology and epistemology is the core of the sociologists' method" (288). For more discussion see Koertge (1999) and Collins (1999b).

2.2 Strategies

2.2.1 Methodological Relativism: Collins

According to Harry Collins who seems to have invented the term,

> [m]ethodological relativism is an attitude of mind recommended to the social-scientist investigator: the sociologist or historian should act as though the beliefs about reality of any competing groups are not caused by the reality itself (Collins 2001b: 184).

He confronts it with 'epistemological relativism' ("one social group's way of justifying its knowledge is as good as another's…") and 'ontological relativism' ("within social groups […], reality itself is different"), and the two views combined as 'philosophical relativism' (184). With this careful separation he underlies the purely methodological motivation for the kind of relativism he adheres to—a position that, for our purposes, can be also named 'methodological idealism' (see figure 3).[26]

The methodological relativistic position was developed as a reading of the symmetry principle.[27] Bloor himself supports this reading by writing, e.g., that "[t]he point of the symmetry postulate is to enjoin sociologists to draw back from making first-order judgements" (Bloor 1999a: 102). The following example is to illustrate the case: Suppose that there exist two rival scientific theories, T_1 and T_2, both widely accepted at a given time, aimed to describe the same set of natural phenomena. After a series of controversies T_1 wins. If a scientist is asked to explain why T_1, and not T_2, won, she might either reply that it is because

[26] 'Relativism' is a very widespread term in, and associated with, post-Kuhnian studies of science, but it is sometimes misleading. It is used in numerous ways (see some thirteen versions in Feyerabend (1987)), very often understood to claim that 'everything is equally true'. Taken literally, however, it implies that all significant aspects of science are relative to the social and cultural context, especially viewed from a historical perspective. While it is generally contrasted with 'realism', the correct opposite is 'absolutism' (e.g. Bloor 1999a: 101), i.e. the view that in science there exist context-independent and absolute aspects. 'Methodological relativism' as formulated above has not much to do with context-dependence—it is closer to formulations of an idealistic position. Bloor also prefers to call Collins' position 'methodological idealism', see e.g. Bloor (1996: 844).

[27] An early statement of the position was: "The tenet of symmetry implies that we must treat the natural world as though it in no way constrains what is believed to be" (Collins 1981b: 218, quoted in Collins 2001b: 184n). Pinch (1984) also argues for "the methodological nature of symmetry" (quoted in Pinch 2001: 19n).

T_1 describes nature the way it is while T_2 does not, or, more cautiously, she may say that T_1 is supported by more empirical evidence than T_2. These are 'first order judgements' belonging to, as it is often called, the 'actor's category'. Now, when a sociologist is asked to explain the same case, she cannot give the same answer because it is the task of scientists, and not of hers, to decide the matter in question. She will make second-order judgements belonging to the 'analyst's category', and her explanation will rely on social factors such as power relations, interests, traditions, etc. What she must not do is to fall back on first order judgements and provide an explanation that is asymmetrical with respect to the causes of acceptance of T_1 and T_2, by saying that T_1 was accepted because it correctly described nature, while T_2 was accepted owing to some distorting social causes. This would be the type of 'sociology of error' Bloor has made so much effort to reject.

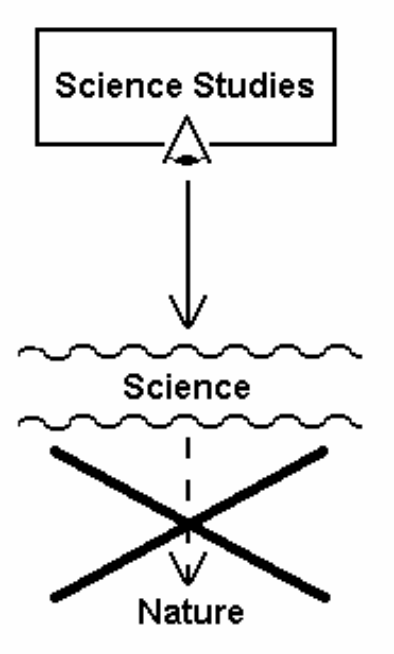

Figure 3
Collins' "solution" to the problem of idealism:
From the perspective of science studies, nature is completely ignored on methodological grounds.

Scientists such as Sokal and Bricmont are customarily tempted to make first order judgements. In opposition to the symmetry principle, they raise the question why the European scientific community accepted Newtonian mechanics somewhere between 1700 and 1750. They say that "a variety of historical, sociological, ideological, and political factors must play a part" in the explanation, but "certainly *some* part of the explanation (and a rather important part at that) must be that the planets and comets really do move […] as predicted by

Newtonian mechanics" (Sokal and Bricmont 1998: 90, emphasis in the original).[28] The last part of this statement is clearly first-order. If we are to explain, on the other hand, the belief in astrology around 1600 we cannot (they say) refer to natural causes because there are none, and this shows "a necessary and crucial asymmetry in the explanatory scheme" (Bricmont and Sokal 2001a: 41). Their main charge against methodological relativism is that "it makes sense only if one adheres to cognitive [epistemological] relativism" (38). The argument is, as seen above, that if there is a cause on nature's side in the formation of a belief, then you must refer to that cause in your explanation, otherwise you implicitly deny that nature supports our beliefs.

Collins admits that before about 1980 he made no distinction between philosophical relativism and methodological relativism, but then he had to realise that philosophical relativism (a kind of scepticism) cannot be justified by case studies and so he changed his philosophical position—having left the validity of case studies untouched (Collins 2001b: 184n). However, the issue does not seem so simple.[29] The answer to the question how much you can explain about science on a science studies level, depends on the degree of social contingency you attribute to science. You either allow for non-social causes but you "concentrate on only one kind of cause [social] even if others are operating" (Collins 2001b:

[28] Throughout their arguments they unreflectedly conflate the role of the natural world with that of empirical evidences. Clearly it is the evidences, and not the motion of the planets, that convinced the scientific community to accept Newtonian mechanics. The difference is essential since the construction of evidence, such as the selection and interpretation of data, is a social process, and, thus, sociological explanations can indeed consider 'evidences'. Note that here the notion of 'social' is taken in a sense different from the way Sokal and Bricmont seem to use it: see Stolzenberg (2004: 85-86). Bricmont and Sokal (2004), in reply to the above objection, acknowledge that, taken the term 'social' in this broad meaning, "*at least in principle*, one can provide a 'complete' explanation of the acceptance of Newtonian mechanics by 18th-century scientists without ever referring to the actual motion of the planets—it suffices to refer to the *observations*" (109, emphasis in the original). But they claim that it is 'unnatural' to stop the explanation here, because it is very reasonable to ask why the observational evidences were what they were—and, eventually, the answer must refer to the natural processes that make our claims true.

[29] Some formulations of the principle allow for not purely methodological motivations, see e.g. the following by Kitcher (1993: 162; quoted in Labinger 1995: 291): "The deep point of the sociological critique is that the social forces that operate in this modification of practice […] may be sufficiently powerful that the effects of nature are negligible." Although he makes it clear that 'relativism' "is a methodological heuristic designed to *counteract* the science studies practitioner's own standing prejudices" (155n, emphasis in the original), and he excludes that it is a "dogma", he makes less effort to deny that it is "an empirical conclusion that follows from the studies" (162).

188), or you hold that "by analysing the allies and resources that settle a controversy we understand *everything* that there is to understand in technoscience" (Latour 1987: 97, emphasis in the original)[30]. In the first case, your explanation will necessarily be partial to a possibly large extent—which is usually not made explicit in science studies works. In the second case, you do need to occupy a philosophical position that explains why everything in science can be explained from the science studies perspective.

The most frequent argument for neglecting social causes is that no matter how much role they played in early formations of beliefs, the workings of science guarantee that the effects of these causes will be "refined away, like slag from ore" (Weinberg 1996). Even if sociologists such as Collins come up with convincing examples of social influences, their claim is made harmless, for Weinberg, Sokal, Bricmont and others, by the almost exclusive scope of their interest in instances of 'science in the making'.[31] Underlining the social nature of scientific practice does not, in itself, ensure the social contingency of the results of this practice.[32] Rather, one seems to be in the need to provide a philosophical frame in which either the essential presence of the social in all results of science can be shown inevitable, or the scope of science studies be extended to non-social mechanisms as well. In any case, there is more to the background of science studies than methodology.[33]

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[30] Latour's claim, however, is used here as it is misunderstood by critics. When he mentions 'allies' and 'resources', these are not restricted to the realm of social, as opposed to nature. In fact, it is very hard to find someone who would fit the charge of denying any influence from nature on beliefs.

[31] Collins' reason to prefer "rough diamonds" to "crown jewels" is that the public is generally confronted with instances of the first kind, and this is where external decisions are required (2001c). Here the question arises whether methodological relativism is a useful position only in these 'hot' cases (which would sound reasonable but weak), or all over the field of science (which is clearly his view, but it is not justified by the above argument). Compare e.g. with Latour's treatment of a very similar distinction in Section 2.2.4. We return to the question in Section 3.2.2.

[32] On the other hand, this disagreement displays an ambiguity in the concept of 'science' on the two sides. For the scientist, 'science' often means the set of accepted knowledge that is taught as such, offered as a frame for understanding natural phenomena. Here the original social context is actually often 'refined away' (but other social contexts, like that of education, can still operate). For the science studier, science is a social activity where consensus is a goal which shifts out of focus when reached.

[33] The connection of idealism and constructivist historiography of science is examined (in Hungarian) in Kutrovátz (2006).

2.2.2 Constructivism: Knorr-Cetina

Rather than addressing the problem of idealism in purely methodological terms, some authors prefer to approach the question with an explicit ontological consciousness. For some of them, science studies gains an access to the natural world by analysing the content of scientific beliefs, exactly because the natural world is given in the form of these beliefs. In other words, certain aspects of knowledge are seen as constitutive of the world, and thus the world is constructed in the process of acquiring knowledge. Proponents of this belief are often referred to as 'constructivists' (or 'constructionists'), generally with the adjective 'social' in order to emphasise the decisive role of the social dimensions in knowledge production.[34]

One of the most explicitly constructivist authors about ontology is Karin Knorr-Cetina. For her, the term 'constructive' is contrasted with 'descriptive', emphasising that "[r]ather than considering scientific products [i.e. beliefs] as somehow capturing what is, we will consider them as selectively carved out, transformed and constructed from whatever is" (1981: 3). Elsewhere she summarises her views with the followings:

> The vision behind the constructivist programme as I conceive of it is that of a potentially increasing stock of problems created by science in the process of secreting an unending stream of entities and relations that make up 'the world'. The 'unknown world' as an intentional object of science is itself a function of a constantly changing scientific practice, of what at every moment of scientific work emerges as the known world. (1983: 135-136)

In sum, the world is not given to us in 'facts'[35] or 'entities', but is achieved as a result of the scientific cognitive process (see figure 4).

[34] 'Constructivism', however, is not a well-defined philosophical position but a family of views centred around the metaphor of knowledge construction. For different senses of the term, see e.g. Sismondo (1993) and Hacking (1999). For problems with the adjective 'social', see e.g. Latour (2003). Here I use the term in its strongest or ontological sense.

[35] As she points out (referring to Latour), the word 'fact' originally means "that which has been made", derived from the Latin *facere* (to make) (1981:3).

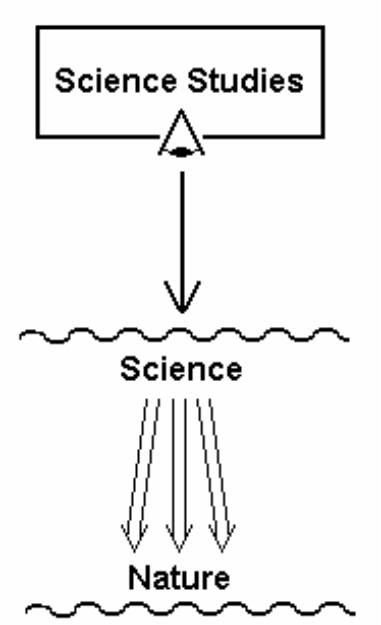

Figure 4
The constructivist "solution":
Nature is seen as constructed by scientific activity, and natural reality is inaccessible independently of the beliefs that provide the framework in which it can gain its shape.

Most generally, one is tempted to place these views in a Kantian paradigm. According to Ian Hacking (1999: 40-49), all epistemological positions relying on the construction metaphor, from logical positivism to mathematical or moral constructivism, were born in "Kant's House". The sense in which such a claim can be justified is that all kinds of constructivism seem to concentrate on the epistemic subject's, and not the object's, contribution to knowledge. For Knorr-Cetina, while 'objectivism' wrongly maintains that "the goal of knowledge is to provide a literal account of what the world is like" (1981: 1), science studies has shown that "within science, existence does seem to flow from beliefs which involve what we customarily call 'knowledge'" (1993: 558). The direction of explanation is the opposite of that of objectivism, since objects are explained from beliefs, and not the other way round. It is important to note here that such a philosophical set-up is in very good accordance with, and can actually support, the methodological position in science studies: the reason for focusing on beliefs without any heed to things is that things can be approached only from beliefs. Also, for the very same reason, constructivism is hardly reconcilable with the position of scientists who see scientific cognition as the revelation of the inherent structure of nature.

Of course, constructivists are faced with the charge of idealism in a similar, philosophical sense in which Kant had to deal with the problem.[36] One way to respond is to

[36] That is not to claim that the present form of the problem has the same philosophical depth and significance as displayed in the works of, and in the classical reflections on, Kant. For more details of the comparison and further elaboration on some of the differences, see Section 2.3.2.

point out that it is not the 'external world' which is denied its knowledge-independent existence, but objects or entities with knowledge-independent identity conditions (Knorr-Cetina 1993: 557).[37] In this respect, scientific cognition is essentially the same as everyday ways of knowing: "What pre-exists before scientifically delimited objects are culturally delimited objects, those humans pick out and encounter and deal with in everyday life". But this "existence is again a variable, one that flows from, and captured by, cultural designation" (558).

The key strategy in constructivism to avoid idealism, as suggested by the appeal to everyday knowledge, is the shift from pure beliefs to actions. Like in everyday life where the furniture of the world is delimited by 'encounters' and 'dealings' rather than deliberate observation, science furnishes the world through all kinds of active manipulations. The metaphor of construction already implies awareness, on the one hand, of the process of cognition beside its result (an ambiguity present in the very term 'construction'), and, on the other hand, of the essentially manipulative nature of 'science in action'. Thus idealism can be avoided by leaving the field of traditional epistemology, since "the active constitution of facticity through science" (1981: 2) is not simply the epistemic mind 'making up the world', but the emergence of an ontological framework through collective action. "In all this, there is no conflation of epistemology and ontology because existence can be established on grounds other than representations and 'knowledge'" (1993: 558).

As a result, inquiry into the nature of cognition is not any more a philosophical task of 'transcendental reflection' of the cognitive subject, but an empirical investigation of social actions producing knowledge. The place of *a priori* epistemology is taken over by "an empirical, constructivist epistemology" (1983: 136); and good-old philosophical ontology is replaced with a "potentially empirical investigation into the kinds of entities, the forms of being, or the structures of existence" (1999: 253).[38] And this is another respect in which the constructivist framework seems to be in a very good harmony with the science studies perspective.

[37] That is, she rejects the idea of "unmediated reality" or "existence without representation", as one may put it (Taylor 1995).

[38] In her later book, she deepens the constructivist programme by being "not interested in the construction of knowledge but in the construction of the machineries of knowledge construction" (Knorr-Cetina 1999: 3), and thus she leaves behind the neighbourhood of traditional epistemology for a genuine sociological enterprise.

2.2.3 Naturalistic Relativism: Bloor

The original programme of the sociology of scientific knowledge was formulated by Bloor as an enterprise searching for causal explanations of beliefs. In other words, SSK displays a naturalistic attitude with respect to beliefs. However, a naturalistic cognitive approach seems rather odd if it leaves no room for the objects of cognition to play causal role in the cognitive process. But any explanatory strategy attributing causal influences from nature on beliefs, and thus explaining beliefs from their objects, is plainly at odds with strict constructivism. In this section, Bloor's position is presented as an attempt, on the one hand, to avoid the radical implications of constructivism by making nature causally responsible for scientific beliefs, and, on the other hand, to justify methodological relativism in a widely acceptable philosophical framework.[39] In the followings, the central source of Bloor's presentation will be his paper entitled 'Anti-Latour' (1999a), an exceptionally motivated summary of his philosophical position provoked by Latour's criticisms (often cited here, offering a frame of polemical reconstruction of Bloor's views).[40]

Bloor's reconciling strategy is expressed, for instance, by the seemingly sober and plausible claim (implied by a "correct, naturalistic reading of the symmetry principle") that "both 'nature' (that is, non-social nature) and society will be implicated in the formation of belief" (Bloor 1999a: 88). In order to warrant such a claim, the possibilities of such a heterogeneous 'implication' need to be accounted for. The ontological background of Bloor's comprehensive naturalism is characterised by a physicalistic and scientistic view—"Voltairian materialism", in Latour's words (1999: 125)—according to which the final components of all nature are those described by (modern) science. Society, in this sense, is a part of nature since it is contained by "the all-encompassing, material system" (Bloor 1999a: 87), and sociology thus becomes a segment of the naturalistic enterprise called 'science' set out to explain natural processes. This ontological uniformity, however, does not in itself indicate how naturalistic

[39] As far as I know, the term 'naturalistic relativism' is, for lack of better, my awkward invention. It is meant to express at the same time two aspects of Bloor's position that are essential from an epistemological point of view.

[40] It is worth noting here that while in the 1970s and 80s the independence war of science studies seemed to unify most authors in one front line, important tensions and oppositions started to emerge from the end of 80s on, especially under the influence of pro-science attacks linking different approaches together. The Bloor-Latour debate (Bloor 1999a, b, Latour 1999) is a late example of such delimiting declarations. For an analysis of the debate in Hungarian, see Kutrovátz and Zemplén (2006).

explanations considering both types of cause are to be given. After all, causes invoked by natural sciences are often very different from causes summoned by sociology.[41]

The role of the (non-social) natural world in cognition is described as "prompting and sustaining belief" by providing "sensory input" (90)—intended in an empiricist context. This is not much, especially because any further role is left unspecified. Latour suggests that the natural world has basically the same function for Bloor as 'things in themselves' for Kant: they are "there to make sure that one is *not* an idealist" (1999: 117, emphasis in the original). For example, when Bloor discusses the history of the electron's discovery, he compares the case of Millikan (who believed in the existence of the particle) and that of Ehrenhaft (who didn't), and claims that we are not allowed to make the electron causally responsible for the differences between their beliefs. This is because in the two cases the electron, of which we today grant that it exists, "is a common factor behind two different responses", so in a sense "the electron 'itself' drops out of the story" (1999a: 93).

Social factors, on the other hand, play an active role in cognition. The reason is that reality (given as sensory inputs) is always "more complicated than we can assimilate into our current conceptual schemes and theoretical systems" (90). In other words,

> Nature will always have to be filtered, simplified, selectively sampled, and cleverly interpreted to bring it within our grasp. […] These processes, which are collective achievements, must ultimately be referred to properties of the knowing subject. This is where the sociologist comes into the picture. (90)

That is, while natural objects, at least in the cited case, make no difference it is social factors that make all the differences to be explained. This is again an explanatory strategy that eventually seems to make the subject, and not the object, causally responsible for all knowledge.

Note that the passage just cited suggests that 'properties of the knowing subject' play an almost exclusively restrictive role by 'filtering', 'simplifying', 'sampling' nature.[42] Bloor's

[41] Latour (1999: 119) claims that Bloor actually needs three different types of cause: one 'classic' type present in natural processes, one 'self-referential' type responsible for social processes, plus one 'mixed' type linking sensory inputs to beliefs. While his claim can be contested, it points to some problems with the concept of cause in explanations of science. Some of these problems will be addressed in Chapter 3.3.

[42] The only term implying a non-negative function is 'clever interpretation'. But that, on the other hand, might point to a hermeneutic context which is clearly not significant for Bloor.

version of SSK is interested solely in differences, and differences (in categories) are produced by social causes. This would be insufficient for any constructivist position committed to the image of an essentially constitutive subject.[43] For instance, Bloor seems to reject the subject's capacity of object constitution when he writes that, in a sense, objects of nature (like 'trees', 'rocks', 'electrons', 'bacilli') "have no 'history': they are just 'there', providing a stable backdrop for the more volatile happenings on the human stage, where ideas change and theories come and go" (86). Remember that for Knorr-Cetina, a natural individual thing with identity is already constructed—or, in a more familiar philosophical terminology, constituted—by the subject. But Bloor explicitly reproves Latour for not making a distinction between the objects of nature and the beliefs about these objects (87), and quotes Latour's third rule with condemn (101).

Indeed, Bloor does not hold that all kinds of knowledge and cognition could be rendered as proper object of sociological study. In an earlier paper (Barnes and Bloor 1981) he suggested a division of labour in naturalising epistemology, between cognitive psychology focusing on the causal background of some elementary and individualistic types of knowledge (those that, being entirely pre-linguistic, animals can also possess), and cognitive sociology concentrating on institutionalised forms of knowledge. Nevertheless, most forms of human cognition, including science, have linguistic bases and, therefore, are essentially social in their character. Bloor (1996) argues that concepts, meanings, and intentionality are all social institutions, in the sense that they are self-referential and are in need of ceaseless reinforcement by social action.[44] Thus the causal linkage between 'sensory inputs' and 'beliefs' becomes clearly relevant to the sociology of knowledge, since beliefs are social institutions.[45] For human forms of knowledge, non-social reality can be accessed only through the mediation of social processes, and we can "know reality" only "through society" (Bloor and Edge 2000). This is why, although allowing for non-social influence on cognition,

[43] Bloor is clearly not constructivist in a strong sense of any kind and, despite its widespread association with science studies, he very rarely relies on the construction metaphor. Comp. Bloor (1998: 624): "For us it is *knowledge* that is 'socially constructed', not *reality*." (emphasis in the original)

[44] This is why, according to Bloor (1996), the charge of idealism becomes most convincing when taking knowledge as a social institution, since "institutions exist because we believe in them" and so here "the knowing and the thing known are not ultimately separable" (842). For a comprehensive exposition of the self-referential nature of the social, see also Barnes (1983).

[45] While Bloor relies on many results of cognitive psychology, he also criticises cognitive scientists for working under an individualistic model of cognition that, in several respects, restricts the scope of their findings (1996).

sociology can explain practically everything in science without having to consider those natural phenomena subject to psychological research proper. In sum, Bloor offers a framework to justify why causal influence of things on beliefs, even if things exist and provide the rough material constituting the object of knowledge, can be disregarded when explaining scientific knowledge.

Of course, every naturalistic position has to confront the problem of circularity described e.g. in Quine (1969). That is, since our knowledge of nature is given as scientific knowledge, we are confined to rely on scientific knowledge when we want to account for the natural causes of it. In short, we have to presuppose the representational validity of the very thing we are to explain as a natural phenomenon. To face the problem, Bloor (1999a) resorts to a distinction between source and topic, saying that we have to use as source some parts of scientific knowledge in order to be able to make another part a topic of our naturalistic inquiry. This demarcation is the result of a methodological necessity to break the circle artificially, but this circularity "does not necessarily destroy the credentials of the enquiry: it is just part of the cyclical way in which all cultures must grow and understand themselves" (108). Circularity is not only accepted for him but it is made, in the reflexivity thesis, an explicitly constitutive principle of the Strong Programme.

In this alleged circle of naturalism, however, the term 'knowledge' receives two different meanings: one the one hand, it is 'collectively accepted belief' to be explained (topic), on the other, it is 'correct representation' to be used (source). In the first sense it is descriptively taken as it is, while in the second case it is laden with the evaluative charge that it is 'true'. If the non-coincidence of source and topic is granted, which seems a plausible stipulation, then we have a direct methodological reason for excluding, for example, any reference to electrons when explaining Millikan's and Ehrenhaft's beliefs about them. Hence the danger of 'short circuit' is avoided, and the analyst is bound to work with larger, non-vicious circles, where empiricism and relativism can be kept together in a philosophically supported methodological framework (see figure 5).

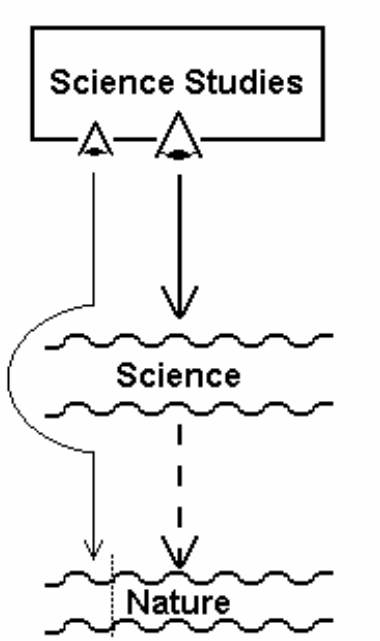

Figure 5

Bloor's "solution" to the problem of idealism: While science studies is interested primarily in social causes of beliefs, it also allows for natural causes to play their role. Since the most reliable access to nature is that of science, science studies sometimes has to rely on science when appealing to natural causes, and make scientific beliefs the source, rather than the topic, of inquiry.

2.2.4 Metaphysical Revisionism: Latour

Finally we examine Latour's own solution to the problem posed by his 'third rule of method'.[46] This rule is introduced in his *Science in Action* (1987), a book intended to be a summary of the methodological features of science and technology studies (in his terminology, studies of 'technoscience'), supplementing these 'rules of method' with 'principles' extracted from his own inquiries (16-17).[47] However, contrary to the main genre in STS where the authors (e.g. Bloor, Collins, and most followers of the strong programme) either apply already available social theories to the analyses of science or first develop the

[46] Latour's position, in contrast with e.g. that of Bloor, is admittedly elastic and has undergone a series of essential changes (Latour 1998: 115), which makes it very difficult to summarise it in one short section. The reconstruction offered here follows primarily narrative motivations, but to some extent it also goes parallel with Latour's intellectual development. This account, being by no means comprehensive in its intent, focuses only on the aspects relevant to the problem of idealism.

[47] Most of what is reconstructed here as preliminaries to the third rule already appear, albeit in a less systematic form, in Latour and Woolgar (1979)—including the statement later called the 'third rule' (180-182n).

theories they later apply, Latour—similarly to Knorr-Cetina—is engaged in a close inspection of scientific practice on a micro-level from which he tries to generalise.[48]

His inspection first reveals that science has two faces: one that offers an image of congregated positive knowledge with its focus on ready-made and highly consensual science, and the other that shows 'science in the making' by displaying a lively, uncertain, essentially controversial character (4). The 'first rule of method' stipulates that it is 'science in action' that STS is interested in (15, 258), since this is where the workings of science can be understood by looking at the steps taken and the decisions made. Latour describes how facts—and machines[49]—become established (or 'black-boxed') in a collective process, where the 'modality' of a statement depends entirely on what later scientists do with it (dispute, accept, reject, embed in explanations, etc) (see his 'first principle': 29, 259). His version of STS is interested in exactly these transformations, rather than attributing intrinsic qualities to statements ('second rule': 59, 258).

When looking behind the textual and the supportive technical (figures, graphs, numbers, etc) strata of science displayed in papers, Latour finds, instead of 'Nature', the laboratory—by etymology, the 'work site' of science. This is a labyrinth of materials, machines, and other 'allies' represented by the scientist as a spokesperson to back up her claims ('second principle': 90, 259). Here some of these 'allies' are exposed to 'trials of strength' in order to probe their stability and resistance, and thus to give shape to emerging 'new objects' on the 'windows of instruments'. At first these are "list[s] of written answers to trials" (87), named after exactly what they do, and later the 'essence' of a reified thing slowly unveils in series of different trials done in different labs by different scientists for different reasons. Thus not only facts but also things are constructed in collective processes. "Laboratories are now powerful enough to define reality" (93), where, by etymology again, 'defining' means delimiting and 'reality' means what resists.

As to nature 'herself', Latour concludes, there is no place for her to enter the picture, since she never appears in the controversies as an 'external judge' to decide. If you look at ongoing debates, nature is never referred to or appealed to by scientists, apart from being

[48] As expressed in the book's subtitle he 'follows scientists', starting his journey with the slogan: "Abandon knowledge about knowledge all ye who enter here" (7). It goes without saying that his claim to summarise the method of all STS (17) can surely be contested.

[49] He tries to render a symmetrical treatment of science and technology, and this symmetry later reduces to a kind of ontological homogeneity (see below). Here I focus only on science, being primarily relevant to our original epistemological problem.

"embroidered on all the banners" (96), rather, they are eager to collect the more and the stronger allies in order to win. Then all agree that what we will think about how nature is furnished will be the result of the settlement of the controversy. Hence the 'third rule': "Since the settlement of a controversy is the *cause* of Nature's representations, not the consequence, we can never use the outcome—Nature—to explain how and why a controversy has been settled." (99, 258) Interestingly enough, notes Latour, when a controversy is settled and there is a strong consensus about a certain 'fact' of nature then the direction of the explanation reverses and scientists appeal to 'Nature' as the cause of what they believe. Since we 'follow scientists' we are told to be "realists as much as the people we travel with" when we describe the 'ready-made' face of science, and "become as relativist as our informants" when looking at the other face 'in action' (100).[50]

So far, we seem to face a position very close to Knorr-Cetina's constructivism,[51] spiced with a small methodological restriction. However, all that come later in the book (and in succeeding writings) lead to a picture very different from, and mostly polemical with, many traditions in science studies. He develops a 'translation model' focusing on the network of 'associations' (interested and interpreted alliances), both by examining the partly referential, partly causal, partly semiotical, partly polemical, nature of these associations, and by following the chains of associations as far as they lead him. This model is contrasted with the traditional 'diffusion model' (132-144) which presupposes the autonomous existence of both nature and society, and examines how the latter reacts to the former. Again, the two models are associated with the two faces of science, and the 'diffusion model' is rejected on the basis of the first principle. This implies the refusal of SSK tradition which tries to explain an already existing nature in terms of an already existing society.[52] Latour rather wants to show,

[50] This methodological restriction is usually not noticed when citing the third rule (see e.g. the criticism by Sokal and Bricmont (1998)). Right before the statement of the rule, Latour writes: "For these parts of science our third rule of method will read:" (99), where "these" stands for those belonging to the controversial face. He is also explicit that for the other side, "Nature talks straight, facts are facts" (100).

[51] This is clear in passages resounding with quotes from Knorr-Cetina (see Section 2.2.2), such as the following: "What we have [instead of 'Nature'] is an array allowing new extreme constrains to be imposed on 'something'. This 'something' is progressively shaped by its re-actions to these conditions." (89) Note also their common preference to perform microsociological and ethnographic studies of small-scale laboratory activity, often leading to similar results.

[52] The main accusation made by him against SSK is that while 'realists' want to explain society in terms of nature, 'constructivists' or 'relativists' (such as Bloor or Collins) do no more than reverse the direction of

by neglecting most of what is customarily presupposed, how nature and society are co-produced (or, we could say, 'co-constructed').

From this position he sets out to tackle two interrelated questions. The first is this: Supposing the invalidity of our traditional commitments in epistemology, ontology, metaphysics etc., why do/did we think that they are valid? His strategy to answer this question is an amalgam of Derrida's deconstruction, Nietzsche's genealogy, and Mannheim's unmasking, and this leads him to put the blame on modernism and modernity. The 'modernist settlement' is based on separations between nature and society, mind and matter, subject and object, right and might etc, and those who introduced these asymmetries were followed by those who inherited the interest in enlarging separations, thus maintaining a certain arrangement (order) of people, things and forces. According to Latour, however, this ambition is shown vain by the proliferation of all kinds of 'hybrids' in our age, and in order to become able to understand the world we live in we have to get rid of the modernist separations because, strictly speaking, "we have never been modern" (Latour 1993).

The other question he deals with is the following: Given the methodological rules described earlier, to what kind of conceptual and philosophical position do we get instead of the traditional one if we follow, together with the scientists, the chains of associations? This position is sought with an explicitly revisionist metaphysical attitude, offering sweeping criticisms, radical innovations and brave generalisations, often rehabilitating marginalised or discriminated philosophical ideas.[53] As to the problem of 'epistemic fallacy', since he erases the distinction between subject and object and substitutes it with the ontological homogeneity proper to networks, he intends to disqualify epistemology at large[54]:

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explanation. This latter approach is as asymmetrical as the former, by offering a position that is idealistic with respect to nature but realistic with respect to society (e.g. 1999).

[53] The outlines of such a philosophical framework already appear in the second part of Latour (1984). For his inclination to unwarranted metaphysical revisionism (and especially 'realism') Latour has been very often criticised, even within science studies: see e.g. Shapin (1988), Amsterdamska (1990), Schaffer (1991), Collins and Yearly (1992), Gingras (1995), Elam (1999), and of course Bloor (1999a). On the other hand, his later attitude illustrates the difference between the two senses of 'science studies' separated in section 1.3, one with its intention to apply social science theories on science itself (Bloor and his followers), and one with the purpose of revising fundamental cultural commitments connected to, or based on, science (Latour).

[54] Latour (1999) offers a systematic attack on the subject/object scheme, and Latour (2000) contains numerous arguments against epistemology. His main argument against both the scheme and epistemology is that it is more difficult to maintain them than to get rid of them: "The claim is that the price of gaining analytical clarity—

> The easiest and quickest thing to replace will be the entire artefact of epistemology. The idea of an isolated and singular mind-in-a-vat looking at an outside world from which it is thoroughly cut off, and trying nonetheless to extract certainty from the fragile web of words spun across the perilous abyss separating things from discourse, is so implausible that it cannot hold up much longer… (2000: 296)

But if there is no distinction between subject and object, the levels of Figure 2 surely collapse, and thus the possibility of epistemic distinctions between nature, science, and science studies disappears. All these fields are certain parts of an overall web of 'actants' called the 'collective', parts that seem illusively separated from each other by the agonising modernist settlement (see figure 6). The question of knowledge is replaced with an 'epistemo-ontological' question, since the bifurcated world—what we know about the world and the world behind it—fuses together in the world as known to us, shown to us, acted upon by us and acting on us. Eventually, Latour's position is not an attempt to solve the problem of idealism but, quite ambitiously, it seeks to invalidate it together with its entire context.

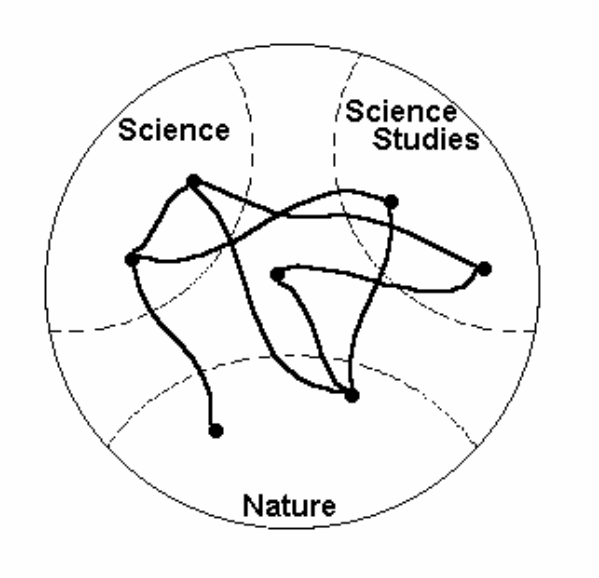

Figure 6
Latour's "solution":
The levels of figure 2 are replaced with an overall web of 'actants' that are connected in a way not to be described in epistemological terms. Usual distinctions such as those between disciplines, or the one between subject and object, are illusions produced by modernity.

words severed from world and then reconnected by reference and judgement—is greater and produces, in the end, more obscurity than granting entities the capacity to connect to one another through events." (2000: 309)

2.3 Apriorism in Science Studies

2.3.1 Idealism and Kant's legacy

So far we have been dealing with the following problem: If we examine science, the question arises how it is related to the natural reality it is intended to describe. By focusing on the set of linguistic, methodological, institutional etc. practices that scientists have developed over the past centuries of modern science, the natural world tends to become ignored. In other words, since it is the subject of scientific cognition that science studies concentrates on, the object is likely to slip through the fingers. To account for cognition only in terms of the subject is a position that, in the history of philosophy, is customarily labelled as idealism. Thus it is the problem of idealism that science studies has to face in the epistemological cross-section of its theoretical commitments.

In this chapter we have reconstructed different attempts to deal with the problem within science studies. One attitude, associated with Harry Collins, is to accept idealism solely on methodological terms, i.e. to make it a constitutive methodological feature of science studies to ignore nature in accounts of beliefs about it. However, many science studiers, while mostly agreeing with the descriptive and explanatory validity of Collins' analyses of science, would demand some kind of philosophical justification for the idealistic methodology. David Bloor and his Edinburgh fellows claim that all kinds of linguistically explicable knowledge are social institutions and thus, while having non-social causes as well, are open to sociological analysis—especially shared beliefs that comprise the body of scientific knowledge. Constructivists such as Karin Knorr-Cetina are less permissive regarding the relation between the world and knowledge, and warrant some kind of idealism in the framework of a philosophical tradition that, by enduing the subject with cognitive conditions constitutive of the object, denies any possibility to appeal to nature independently of the very set of culturally shared beliefs about it. Finally, one influential but more radical approach is offered by Bruno Latour who seeks to avoid the problems by rejecting the entire epistemological tradition centred on the subject/object scheme and, eventually, develops a new metaphysical framework for leaving behind the theoretical and practical traps of modernism.

Given this variety of attitudes towards the problem of idealism within science studies, the question arises whether there is a single epistemological feature common in all branches

we have examined. To this point, the unity of the field seems to rely on its methodological outset represented in Figure 1, rather than on shared epistemological commitments—while, indeed, a diversity of epistemological views was revealed along the cross-section we made. This already suggests that the subject/object dichotomy, as mapped onto the society/nature distinction, is not the chief philosophical problem of the discipline: in fact, despite the specific differences, all the examined approaches tend to undermine the often unreflected demarcations between nature and society on the one hand, and science and society on the other. The problem of idealism arises when looking at science studies from a position, such as the classical modernist epistemological framework, where these demarcations are implicitly imposed. Before we examine the normative character and role of these assumptions in Chapter 3, the epistemological problem will be reinterpreted in traditional philosophical terms.

Looking from the theoretical framework where the problem of idealism is formulated, the common point in all science studies epistemologies—with the possible exception of Latour[55]—is that they seek to analyse the subject of scientific cognition. In the history of philosophy the turn, often called 'Copernican revolution', that decided to make the subject, instead of the object, decisively responsible for cognition, is associated with the name of Kant, as we have already noted in Sections 2.2.2 and 2.2.3. Kant's inquiry focused exclusively on the (individualistic) subject's cognitive capacities, and he chose to leave the alleged 'noumenal' object totally unspecifiable: 'things-in-themselves' without any conditions of identity whatsoever. That is, he allowed for, and even found necessary, a mind-independent reality, but he denied to attribute any specifiable responsibility to it for the result of cognition because, according to him, every discussable specificity is determined by the subject's features existing prior to the actual process of cognition. In other words, he was looking for the mind's *a priori* conditions that make possible the constitution—in our terms, the construction—of the content of knowledge.

Kant's turn had an enormous impact on later philosophical traditions, and many authors, both within, for, or against science studies, refer to the Kantian roots of the field. Since Kant's time, however, many essential elements, as well as the philosophical contexts, of his 'subjectivism' have undergone fundamental transformations. Since I am focusing on

[55] He is inclined to distance himself from any problem situation formulated in traditional philosophical terms. In the following, although once he was a central, and remains a leading, figure in social studies of science, Latour will not be taken as a typical representative of science studies, since he escapes the scope of most of my general claims concerning the field.

epistemological strategies emphasising the *a priori* conditions of the cognitive subject of science, I am concerned with those aspects of the Kantian tradition that can be labelled apriorist. In the following I will underline some central differences between the concept of *a priori* in Kant and in science studies.

2.3.2 The 'new' a priori

1. The main difference between traditional epistemology and that of the sociology of scientific knowledge is that while, according to the former, the traditional epistemic subject is the individual mind, in science studies its role is taken over by (some parts of) the society. On the one hand, this enables e.g. Bloor to bracket, and others to ignore completely, some individualistic and psychologically specifiable forms of cognition. On the other hand, it replaces traditional problems concerning 'ideas' and their relations to each other with questions concerning the acceptability and credibility conditions of beliefs, the role of traditions, testimonies and authorities, the function of social and economic interests, power relations etc. All these factors contribute to the epistemic subject's *a priori* features preconditioning any cognitive act.

2. For Kant the realm of *a priori* is endowed with absolute necessity, providing the solid grounds for any cognitive capacity—from logic and spatio-temporal orientation to possibly ethics and religion—to any epistemic subject ('all possible rational beings'). In contrast, the social *a priori* is relativised in at least two senses:

a) Social *a priori* is relative in the historical dimension. A major impact of the history of science on the philosophical and other studies of science, from the 1960s on, is the recognition that the set of standards of scientific activity is a function of historical time. In other words, there are no universally valid criteria of what count as accepted scientific activities or results, and the entire realm of *a priori* is subject to social dynamics. The emblematic source of historical relativity is Kuhn, who expressed this in terms of apriorism: "I go around explaining my own position saying that I am a Kantian with movable categories" (Kuhn 2000: 264; quoted in Richardson 2002: 254). His apriorism is described in Kuhn (1991: 12) as "a sort of post-Darwinian Kantianism". What he calls 'lexicons'—the linguistic-

conceptual taxonomic schemes central to his later philosophy[56]—are said to supply "preconditions of possible experience" in a way that they "can and do change" in small scales but producing large-scale effects (*ibid.*). His Darwinianism consists in an image of lexical dynamics understood as analogous to biological evolution/speciation.[57]

b) Social *a priori* is relative to culture. One of the main sources of relativism in science studies is the so-called cultural (or anthropological) relativism, a dominant attitude in the social sciences in the 1970s and 80s. In studies of science, the prime theoretical tool to apply cultural relativism is the notion of incommensurability, launched by Kuhn (1962) and Feyerabend (1962), supported by an underlying holistic conception of language (e.g. Quine 1951). The cultural relativity of science, although proposed by SSK from the beginning (e.g. Bloor 1973, 1982), is especially emphasised in the cultural studies tradition of science studies, like in multiculturalist criticisms of science (e.g. Ross 1996).

3. In Kantian epistemology, the *a priori* features of the individual mind are accessible only by means of some kind of transcendental reflection. It is because his *a priori*, being universal preconditions for every cognitive act, can never be presented as the content, rather than the form, of empirical knowledge.[58] In science studies, however, the relative character of *a priori* makes it possible to subject it to empirical investigations. In this sense, science studies is a naturalistic enterprise where social science explanations take over the role of 'fundamental' philosophical inquiries (see next chapter). One consequence of this is that the epistemic subject of science studies, unlike the rational mind, is not supposed to be self-transparent, and thus the tacit dimensions of knowledge, hidden from reflection, become open to investigation on the same grounds as explicit theoretical commitments (e.g. Collins 1974). In a similar way, science studies can concentrate on actions and performances, instead of ideas of a reflective mind: its epistemological significance is shown, for example, in Knorr-Cetina's refutation of idealism in Section 2.2.2, or in Latour's attempt to outdo epistemology

[56] More precisely, a lexicon is "a taxonomically ordered network of kind-concepts and kind-terms" (Irzik and Grünberg 1998: 210). See below some more aspects of lexicons and their constitutive possibilities.

[57] For another influential way to employ the concept of a changing *a priori* in the history of science, see Foucault's 'historical *a priori*' (1969).

[58] Such a restriction is inherited by different figures such as the members of the Vienna Circle for whom philosophy is a non-empirical inquiry into the necessary logical forms of all possible meaningful expressions, or Wittgenstein who, despite all the radical changes in his philosophy, always sticks to the non-discursivity of the overall linguistic forms of communication. For a defence of the non-empirical transcendentality of a historically moving *a priori* in science, see e.g. Friedman (1998).

at large. The relative nature of the empirical *a priori* also creates the possibility for forms of reflexivity that are not bluntly circular, as in Bloor's conception of the mobile source/topic distinction. Still another consequence is that while Kant's philosophy sought for the possibility of (all) knowledge, an empiricist science studies enterprise has primary access to actual forms of knowledge and only an indirect grasp on the historical dynamics of possibilities.

2.3.3 A constitutive and *social* a priori*?*

Focus on the social dimension of cognition poses some novel problems for the constitutive possibilities of *a priori*, and this is an issue about which, as we have seen, different trends in science studies strongly disagree. For empiricists like Bloor, the *a priori* plays a role in 'socialising' individual experience, i.e. in restricting presently possible forms of explicable experience.[59] Here specific objects are supposed to exist prior to cognition and thus any usual concept of object constitution is excluded. The influence of society consists in making the differences in knowledge, expressed by negative terms, as we have seen, such as 'filtering', 'simplifying' and 'sampling' (1999a: 90). For constructivists, on the other hand, cognition is an essentially constitutive process—as implied by the metaphor of construction—and reality can appear only in forms shaped by the *a priori* features of the subject.

A second brief look at Kant's philosophy may shed more light on the role of constitution in apriorist studies of science. According to him, the mind needs two *a priori* cognitive capacities for connecting the undifferentiated plenum of sensory 'input' to knowledge: sensibility and understanding (Kant 1933 [1781]: 92-93). The first is through what the object is given, intuited in two forms, time and space, as frames of arrangement; the second is through what the object is thought, structured by the *a priori* principles of the forms and features of concepts and judgements. These two capacities are equally necessary to obtain any kind of empirical knowledge. For us, the point here is to introduce a distinction between two sides of 'construction': one is 'articulation' of the object by making it intelligible, i.e.

[59] His famous parallelogram of force (1976: 32), according to which beliefs are the resulting 'vectors' or prior beliefs and experience, suggests that the social dimension have a function in 'distorting' individual experience. However, as we have seen in section 2.2.3, those individual experiences without social dimension are of a very limited character common to human and animal cognition.

accessible to conceptual operations, while the other is 'constitution' in the sense of being given, to some degree, by the perceptual machinery. The first provides the frame of identity, or 'delimitation' as Knorr-Cetina called it, without which any epistemic means of accessing the object would be impossible, but it is the second that is responsible for anything 'being an object' even in a pre-conceptual sense.[60]

For a collectivist epistemology the question is that, in an apriorist framework, how much role can the social be attributed to in constructing the content of knowledge. What Kant called understanding can be mapped onto something like Kuhn's lexicon, a sort of conceptual frame (or conceptual scheme) that provides an interconnected network of quasi-linguistic forms preconditioning any articulated experience.[61] Unlike the transcendentally given structure of Kantian understanding, this lexicon is shaped by empirically discernible factors that can, in theory, be both biological ('innate') and social ('acquired'). The social part is open to inquiries, for example, into the cultural contingency of elementary classificatory forms, as the tradition from Durkheim and Mauss (1902) to Hesse (1974), Bloor (1982), and many present constructivists illustrates. Given the inevitably linguistic nature of the lexicon as presented by Kuhn, and dwelling on a fundamentally social conception of language such as the ones stemming from Wittgenstein, the science studies tradition has no difficulties in showing that the articulating preconditions of all knowledge are at least socially determined. This much is enough to ground the view that scientific knowledge is not only a collective achievement but also a social construction: any conceptual operation is rooted in—at least partly—social conditions.[62]

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[60] This is surely not to claim that the above distinction is precisely what Kant meant by his Intuition/Concept dichotomy since, as we saw in the preceding section, there are significant differences between the two forms of apriorism. What I would like to show here is that even a very rough comparison with Kant's system can illuminate some epistemological problems relevant to science studies.

[61] Let us note that Kuhn understood the lexicon to have perceptual, in addition to conceptual, functions. He writes that a lexicon is "a particular operating mode of a mental module prerequisite to having beliefs", and that "[s]ome such taxonomic module I take to be pre-linguistic and possessed by animals". He adds that "it evolved originally for the sensory, most obviously for the visual, system" (1991: 5). He also claims that it is "constitutive of the possible experience of the world" (1993: 331), but it is not easy to see how a taxonomic module can have such a function (see Irzik and Grünberg 1998).

[62] This comparison falls back on an individualistic epistemological view in that, although the lexicon is a collective property of the community, the subject of cognition in an individual conditioned by this lexicon. The question here is the possible role of the social in individual cognition, if one is inclined to be a constructivist. This is a convenient framework to deal with perception on the same grounds, as shown below.

The counterpart of Kantian sensibility, on the other hand, seems to be missing from most contemporary idealist epistemologies. This lack of attention is a significant obstacle to a complete epistemology of cognition since, as argued by Kant, something is needed to bridge the gap between the ocean of 'sensory data' and the forms that articulate experience: something that renders the object given to the conceptual strata of cognition. Of course, one can suppose that whatever the perceptive processes may be, they are 'hard-wired' so that they are entirely free of any social influence at all. For example, it is conceivable that Kant's space and time as forms of intuition may somehow be substituted with the (given) spatio-temporal distribution of neural stimuli, and that the perceptual output of these stimuli is constituted by innate mechanisms. A science studies position adopting such a theory of perception is only modestly constructivist in that the constitution, as opposed to the articulation, of the object is not a social process. In contrast, one can also find several frameworks in which it is possible to allow for acquired elements in perception such that they give ground for some degree of social contingency. On the naturalist side, the famous Gestalt-psychology experiments cited by Kuhn (1962) and recalled, for example, by Barnes, Bloor and Henry (1996) are often understood to support such a theory of perception.[63] Also there are anti-naturalist, philosophical theories—especially in the phenomenological and hermeneutic philosophies of science, see e.g. Ihde (1977), Heelan (1983)—where perception in science is argued to be a socially and culturally shaped capacity. In these cases, the very way in which objects are perceived as given before/to conceptual articulation is also exposed to social influences, and so constitution becomes a process with an inevitable social dimension.

The questions outlined here point to problems that are usually associated with psychological and biological approaches to cognition. The possible relations between these fields and social studies of science will be addressed in Section 3.3.4. Before that, the problem of idealism has to be put aside and the topic of naturalism needs to be introduced.

[63] In the light of relevant discussions in Barnes, Bloor and Henry (1996), it is interesting that Bloor sticks to a non-constitutive conception of cognition, while many authors claiming for the constitutive character miss to recognise the above problem. Naturally, arguing for some social contingency in perception does not imply that one is a constitutive constructivist.

3. NATURALISM IN SCIENCE STUDIES

3.1 Descriptive neutrality

In their criticism of the methodological position of science studies, Sokal and Bricmont (1998) draw criminal case analogies in order to show the dire consequences of adopting a relativist stance (99). At the practical level, according to them, science is essentially analogous to criminal investigations. The argument is designed to illustrate how absurd it is to refrain from considering the evidences and making judgements in situations where obviously "there *is* a truth of the matter" (100, emphasis in the original), and where the purpose is to find this truth.

The previous chapter attempted to offer different answers to the question why analysts of a descriptive practice may refrain from taking the result of that practice as valid, rather than simply given. The methodological point is to suspend making judgements at the same level on which actors under study make their judgements. Take criminal investigations as a handy illustration: it is institutionally regulated which people are appointed to make generally accepted (although contestable) judgements. No one else is in the position of making judgements, including those who, in parallel with practitioners of science studies, analyse criminal investigations or legal practice. In their discourse, the sentences "*X* is guilty" and "*E* is an evidence for *X* being guilty" are replaced with "*X* was found guilty by the judges" and "*E* was accepted as an evidence by the judges for *X* being guilty". Science is slightly different in that the whole community is responsible for making judgements such as "*L* is a law of nature" or "*E* is an evidence for *L* being a law of nature", but it is equally true here that those who do not participate in the practice of science are not authorised to make their own judgements of this sort.

Making judgements requires a normative background. We can hold someone guilty only if we acknowledge a system of norms on the base of which we consider an act a crime, and also, according to which we regard something as an evidence. (As to he former, norms are highly codified in modern societies, as to the latter, it is much less so.) The practice of making factual judgements is largely contingent on this normative base. Questioning the judgements poses factual problems, while questioning the norms implies regulative decisions. In other words, making factual judgements is a descriptive activity, but reflecting upon the conditions under which we make these judgements leads to the realm of normativity.

Let us consider the concept of 'knowledge'. According to the traditional philosophical reflection on the conditions of knowing, knowledge is justified true belief. While we may be able to describe the process through which someone comes to accept a belief as justified and true, we can agree to accepting the same belief as justified and true only if we share with the person under study the norms according to which we accept something as justified and true. Truth, as "correspondence to reality", is a normative postulate rather than a factually verifiable claim, and "justification" involves norms of rationality, empirical testimony, etc.[64] The normative character in this conception of knowledge is highlighted by the evaluative charge of related concepts in traditional epistemology: rational as opposed to irrational, objective versus subjective, or even true versus untrue (either error or lie)—all dual distinctions with the atmosphere of an opposition of the good/bad type.

In contrast, the tenet of impartiality in SSK enjoins analysts of science from relying on such evaluative distinctions (see Section 1.3). According to Bloor, "[i]nstead of defining it as true belief—or perhaps, justified true belief—knowledge for the sociologist is whatever people take to be knowledge" (1992: 5). In the sociological analysis, knowledge is not contrasted to a negative counterpart such as error or ignorance, and just as the natural scientist describes processes in nature without ascribing values or normative evaluations to them, so does the social scientist with respect to scientific cognition. Sokal and Bricmont severely criticise this "redefinition of knowledge" for being inapplicable to everyday ordinary knowledge, and hence implausible in the case of scientific knowledge (1998: 87, 195-196). They also recall that both philosophy and everyday language know the distinction between knowledge and mere belief, and that "the word "knowledge" has a positive connotation, while "belief" is neutral" (195)—which underlines their opposition to a level of analysis that is descriptively neutral with respect to locally accepted beliefs both in everyday practice and in science.

Historian of science Peter Dear coined the term "epistemography" (Dear 2001a) in order to capture "the indispensable disciplinary core" (141) of science studies. He finds that science studies inherited from traditional philosophy of science those parts which were not normative and evaluative, and hence became an entirely descriptive enterprise. While the term epistemology has strong normative connotations, the suffix "-ography" is meant to express the

[64] Ian Hacking calls epistemological (and other philosophical) terms "elevator words" (1999: 22-24), suggesting that the things they refer to are not "objects in the world" or "ideas", but they work at a "higher level". The use of these words "elevates" the discussion to a higher level, often unnoticed, where normative commitments become involved (see p. 88).

purely descriptive character. In science studies, knowledge becomes open to a scientific, instead of a philosophical, investigation.[65]

By using the term 'naturalism', I simply refer to that property of an approach which chooses to deal with its subject on purely descriptive, as opposed to normative or evaluative, terms. Naturalism with respect to knowledge is a widely discussed and debated issue, since it is at odds with traditional normative epistemology in philosophy (see e.g. Kornblith 1985). However, I am not going to touch on the classical arguments and positions put forward in that debate—although I return to the general problem in Section 3.3.4—since my purpose is not to take side in matters of philosophy of science. Rather, I want to expand on some of the consequences of the view that takes scientific knowledge as a natural phenomenon within the realm of the social. First I will examine what naturalisation means with regard to how we esteem science in general, and then I discuss several aspects of causal explanation of beliefs.

3.2 The esteem of science

3.2.1 Devaluating science

It is a question whether refraining from judging any specific scientific belief, by choosing not to evaluate them the same way as scientists do, implies neutrality with respect to the entire system of norms that could form the base of those judgements, and whether such an approach amounts to a non-evaluative attitude toward science as a whole. Nonetheless, sociological analysis is most often immune to the respect and generally positive opinion surrounding science, save as a research topic itself. Scholars in science studies, as opposed to many scientists, aim to investigate *how* science works instead of *why* it is a good thing.

[65] Naturally, science studies approaches have their own normative commitments, but these norms are the principles and standards of research into science, and not of science itself. The two spheres may nonetheless be connected by constructive uses of the reflexivity thesis. For example, Collins and Evans (2002) develop a normative theory of expertise by simultaneously addressing it "as an *analyst's category* as well as an *actor's category*" (240, emphasis in the original). However, such an approach is rather untypical of the main circles in social studies of science, and distinguishes itself from earlier trends by advocating a 'Third Wave' in science studies.

One of the most frequent charges made in SW by defenders of science is that science studies intentionally degrades science. For example, Henry H. Bauer, scientist and member of the science studies community since very early on, complains that it is precisely the neutral attitude to science that corrupts the discipline, and the attacks against the field in SW are well deserved (Bauer 2000). He claims that "[t]he 'strong programme' and its ilk are fundamentally and irretrievably wrongheaded" with their "primary 'principle,' that scientific activity should be investigated 'neutrally,' without prior judgement about the correctness or incorrectness of the science being studied" (46). For him, the problem with this attitude is that it does not deal with the most important question in—his conception of—science studies, namely, "what is *special* about science, what is *different* about science" (51, emphasis in the original). Instead, all "constructivists and relativists seem intent on showing how every aspect of scientific activity is significantly *the same* as what humans always do" (52, emphasis in the original). This approach embodies, as the title of his paper goes, 'Antiscience in Current Science and Technology Studies'.[66]

Bauer refuses to suspend, in his investigations, both the norms that participants in scientific practice accept and the positive image of science that follows from the evaluation of these norms or the match between them and the actual practice. Because of the lack of positive evaluation in current science studies, the discipline appears to him as hostile towards science. But *de*-valuation in the sense of suspending the evaluative dimension altogether does not entail, but rather excludes, devaluation in the usual sense of downgrading. Just as neutrality is at odds with praising, so it is with denigrating.[67] As the analogy goes, just as it is not the professional purpose of the natural scientist to show how magnificent or how vile nature is, nor does the social scientist seek for such things in science.

Harry Collins (2001a) offers a striking illustration of that point: Suppose that a sociologist wants to explain why members of a certain community believe that wine can turn into blood, while members of another community don't. She will be considering all kinds of social conditions, but she will estrange herself from the communities in that she will not want

[66] Gerald Holton, in his seminal book *Science and Anti-Science* (1993), identified the four main representatives of antiscience in the academia, and in the first place he named sociologists of science "who wish to abolish the distinction between science and fiction" (153). He continues with "alienated intellectuals" (e.g. Koestler), "Dionysians" with "New Age preferences", and radical feminists.

[67] At least in the logical sense. Taken pragmatically, being silent about something that is usually added to the topic often means implicit denial. However, this is not the case with science studies' (lack of) evaluation of science, since, as argued here, this approach has essential non-pragmatic grounds to proceed this way.

to decide, and hence take into account, whether or not the wine can 'indeed' turn into blood. Her analysis will be seen by the 'yes' community as downgrading their claim, and its entire cognitive background together with it, because they maintain a positive evaluation of the norms that justify this belief for them. (At the same time, the other group will have the same impression regarding their own position, for the very same reason.) On the other hand, if she wants to give causal explanation as to why participants in the Nazi regime behaved in the way they did, her account will be seen as making unwarranted excuses—this time because of the lack of the generally accepted negative, while in the previous case positive, evaluation attached to the matter. So "that whether a sociological explanation is seen as an attack on or a defense of a set of beliefs and actions is itself relative to the valuation of those beliefs and actions within the group considering the explanation" (Collins 2001a: 157).

Steven Shapin (2001) collects a list of statements expressing the main credos of science studies and then, offered as a hoax of his own, reveals that all the statements were made by prominent scientists. He notices that such claims revolt scientists when made by outsiders, while they are perfectly acceptable when made by their own colleagues. So "it cannot be the claims themselves that are at issue" but "[r]ather, it is *who has made* such claims, and what motives can be attributed" to them (Collins 2001: 101, emphasis in the original). For scientists, acknowledging that science proceeds by violating the alleged norms as often as not, and that it owns the uncertain and occasional character of any collective human practice, is a secret of the trade. By making such claims they do not question the norms themselves but express, as they all know, how difficult it is to follow these norms in actual practice. Moreover, scientists often refer to social causes in describing the workings of science when they explain *wrong* elements, those that are shown to be wrong by attributing to them causes other than rational and evidential support prescribed by the norms.[68] On the other hand, from the outsider's perspective where norms informing scientists may not be shared, such negative statements can gain a different status in that they can act as evaluations of science as a whole, and they put the public respect for science at stake. Thus it is not the intentionally neutral position of science studies where the question of evaluation bears serious impotance, but the field of public understanding of science (PUS).

[68] This is definitely one source of the kind of asymmetrical explanation that Bloor calls 'the sociology of error'.

3.2.2 Public Understanding of Science

For scientists, PUS consists in the question to what extent the public understands the content of science. PUS is mediated by science popularisation in books, the media, public lectures, etc. This view is based on what Gregory and Miller call 'the deficit model', according to which "scientists are the providers of all knowledge, and the arbiters of just what should be provided, to an empty-headed public" (Gregory and Miller 2001: 61). However appealing this image is to scientists, it breaks down when the public decisions informed by, or concerning, science are at issue. Obviously, the public will prove to have heads that are, rather than empty containers to be filled, "chock-full of intellectual strategies for dealing with the problems of everyday life, of which scientific strategies may not be the most important" (65). In the contextual model of PUS, the public's attitude toward science is embedded in a network of aims, norms, strategies, and interests that is tied to the social setting. Laypeople turn to science not for the promise of a body of disinterested, pure knowledge to receive and store, but because science is involved in the everyday environment in which they conduct their lives. They have to make decisions about what kind of medicine they should take, what products they ought to consume in order to preserve health, etc. Their "paradigmatic science" is medicine (Gregory 2001: 203), and they need scientists not as teachers but as experts.[69]

Collins and Pinch (1993) also deals with science, not as 'true representation of nature' or 'paragon of knowledge', but as a system of expertise. For them, the important point is that science, as laypeople are faced with it, is not the hardly contestable but quite boring body of theories found in textbooks but, quite the contrary, a highly controversial field about which, or in connection which, they have to make decisions.[70] For this reason, it is in the responsibility of science studies to offer an 'ideology-free' picture of the real workings of science to the public, since the image spread by scientists portraying science as certain and uncontroversial

[69] The above distinction between models of science communication, widely advocated by Jane Gregory and Steve Miller, derives from a classification by John Ziman (1992). He distinguishes between the 'deficiency model', which asks how public ignorance of science should be reduced, the 'rational choice model', asking "What do people *need* to know in order to be good citizens—even to survive—in a culture largely shaped by science?", and the 'context model', asking "What do people want to know in their particular circumstances?" (16-18). For a comprehensive survey of aspects of science communication in the US, see Weigold (2001).

[70] That is why, as we saw in Section 2.2.1, Collins (2001c) chooses 'rough diamonds' instead of 'crown jewels' as objects of study, i.e. controversial cases rather than exceedingly consensual ones—in contrast with scientists popularising science.

may result, when found misleading again and again, in "disillusionment" and decline of public trust in science (Pinch 2001: 22).[71]

On the whole, scientists have very different purposes with offering images of science from that of practitioners of science studies. While the latter often examine science in order to gain a picture of it that is accurate enough to inform public decisions concerning its functioning within society, the former are likely to endow it with a cognitive status of authority that is seen to contribute to its distinguished social role. Social scientists tend to portray science on a par with any other cultural institution and view it as, so to say, 'natural' in the sense of being understandable by means of non-evaluative, causal explanations, while scientists insist on emphasising the highly valued exceptional and 'unnatural nature of science' (Wolpert 1992). The contrast is made sharper by the difference as to in which context the two sides place PUS: while scientists view PUS as a problem of scientific erudition, science studies authors see it as a problem of social education and judgement—deciding whom to believe and on what grounds (Collins 2001c). On the science studies perspective, Bauer's question 'what's so special about science' is not a philosophical matter, but an empirical issue that can be addressed by examining it and comparing it with other expertise cultures.[72]

3.2.3 Desacralising science

Bricmont and Sokal find that they are unable to understand Collins' (2001a) point in his example of the sociological explanation of beliefs concerning the turning, or not turning, of wine into blood. They say that if one group have a "solid empirical evidence" for their belief,

[71] Gregory (2001) finds this worry unwarranted, and claims that people precisely know how uncertain science is, and what they can expect from it—the same as from any other culture of expertise, where controversy and uncertainty are unavoidable.

[72] Philosophy of science is the discipline that set out to give answer to this question, and its different answers were couched in normative terms. Since science studies' field of interest largely overlaps with that of philosophy of science, although here a markedly different, descriptive methodological strategy is at play, the two disciplines are rivals in certain respects. Kusch (1999) describes SSK as a "challenging discipline" to traditional philosophical epistemology of science, while Collins (1999b: 786) understands the opposition in SW between philosophers and SSK as a "turf war". However, since science is not professionally aimed at dealing with its own functioning, no such disciplinary conflict emerges between scientists and sociologists of scientific knowledge.

then the sociologist must take it into account (Bricmont and Sokal 2001b: 180). According to them, this problem bears importance not only for purely intellectual matters:

> an intellectual movement that claims to give a causal explanation of the content of scientific theories by treating them, for methodological purposes, just as if they were a religion or a myth cannot be expected to be of much help in fighting the irrationalism prevalent in our societies (to put it mildly). (183)

The connection between methodological idealism and irrationalism, however surprising it may or may not seem, is not explicated further. Still we may venture that the implicit connection is something like this: if we suspend the norms that serve as bases on which to find reasons for our beliefs, then we offer a ground for irrational beliefs and actions. It is because norms of rationality as such, in our societies, are desired to coincide with, or at least converge to, norms of rationality employed by scientists. After all science is customarily taken, in the Enlightenment tradition, as the paragon of human rationality.[73]

Bloor (1992) offers a scenario in which the moral implications of toying with the norms of science can be explained. He recalls the Durkheimian distinction between the realms of sacred and profane, as developed in Durkheim (1912), and claims that sociologists, when dealing with science, make it profane what is essentially sacred for scientists. Although Durkheim introduced the distinction in order to characterise religious behaviour, Bloor maintains that, analogically, he can also discuss science: in terms of social mechanisms, the principles of science are "fortified, elevated and protected" just as religious principles are (Bloor 1992: 48). Moreover, similarly to religious principles, "the sacred aspects of science can be thought of as informing or guiding the more mundane, less inspired, less vital parts" (49)—because the sacred is a source of strength. In this picture, the problem with SSK is not only that while being profane it deals with the sacred, but that it turns the natural flow of strength by explaining the sacred science from the profane society.

A similar analogy is pursued in Bloor (1988) where he compares objections to SSK to 19th century reactions to the so-called Tübingen school of church historiography. This school offered explanations of the development of early Christian ideas that relied on the social

[73] Such a commitment is a central source of science defence in the SW. As the physicist Peter Saulson puts it, Sokal's main motivation for his hoax was his insistence "that only by returning to a belief in objective truth, revealed by the methods of science and reason, can we clear the path for progressive social change" (Saulson 2004: 101).

environment and interest relations of those church parties that negotiated these ideas. Critics rejected these analyses by maintaining the distinction between authentic doctrines and heresy, emphasising that while the latter are indeed to be explained as deviations from true doctrine due to forces external to religious matter (such as social factors), the former are in no need of explanation since they are the revelation of God's truth. In a similar way, when rationalists object to SSK on the grounds that only irrational beliefs require sociological explanation, while rational ones are self-explanatory in that they are the 'revelation' of Reason's working, the essential structure of this objection is the same as that of the anti-Tübingenists. In both cases, norms that create the background of right/wrong distinctions are seen as so fundamental that they are immune to suspension at a less basic level, i.e. that of sociological explanation.

Why is science seen so fundamental that the validity of its norms seems to emanate from scientific practice to the whole sphere of social existence? Bloor continues to answer the question in a Durkheimian manner. He assumes that religion, taken in the broad sense as the realm of sacred, "is essentially a way of perceiving, and making intelligible our experience of, the society in which we live" (Bloor 1992: 51). In other words, the sacred is to "symbolise the principles on which the society is organised" (*ibid.*). Since knowledge, in the normative sense as embodied in science, belongs to the sphere of sacred, reflecting on the conditions of knowledge tends to amount to "tacitly manipulating images of society" (52). As an illustration, he gives a detailed comparison of Karl Popper's and Thomas Kuhn's images of knowledge. He argues that while Popper's conception is an implicit symbolisation of the Enlightenment vision of society, Kuhn's theory encodes the Romantic ideal. What is common in them is that they both mystify, rather than naturalise, knowledge by refraining from analysing its final, most fundamental conditions.[74] This is a warning that knowledge should be entirely naturalised, "otherwise the grasp of [its] nature will be no more than a projection of our ideological concerns" (80). That is why any philosophical, normative account is insufficient for understanding science, and that is why it is necessary to subsume its norms under sociological analysis, rather than keeping the analysis subsumed under them.

[74] Kuhn's conception, however, is often improved upon in a naturalistic framework by science studies authors. The reason according to Bloor is that there's a difference in the degree of threat that the two conceptions are seen to exert on society. Since the prevailing atmosphere is heir to the Enlightenment, and our present society is somehow modelled upon this image, Popper's account is closer to the generally accepted realm of the sacred, and it is menaced by Kuhn's 'heretical' view. For further details, see Bloor (1992: 55-83).

3.2.4 Unmasking science

Ian Hacking (1999) relies on another classic source of the sociology of knowledge when dealing with the functions of sociological analysis, namely Karl Mannheim's concept of 'unmasking' [Enthüllung]. Unmasking, he quotes Mannheim (1952 [1925]: 140, quoted in Hacking 1999: 53), is

> a turn of mind which does not seek to refute, negate, or call in doubt certain ideas, but rather to *disintegrate* them, and that in such a way that the whole world outlook of a social stratum becomes disintegrated at the same time. We must pay attention, at this point, to the phenomenological distinction between "denying the truth" of an idea, and "determining the function" it exercises.

In other words, unmasking a statement is achieved at a level where the aim is not to evaluate its claim to truth, but rather to show its "extra-theoretical function" (54) by presenting it as embedded in the context of its presuppositions and consequences, and showing what it implies to be committed to it. It is stressed that when a sociological analysis employs an unmasking attitude, it does not mean that it is antagonistic with respect to its subject, e.g. science.

Unmasking can nonetheless have moral implications, since it can be used as a tool for criticism on the grounds of what we think about the extra-theoretical functions science is shown to have. According to Hacking, in a vast body of science studies literature the point is "to liberate the oppressed, to show how categories of knowledge are used in power relationships" (58). If knowledge is taken to encode power relations, then by 'unmasking' or 'deconstructing' it we can subject it to criticisms with political intent. Indeed, much that is going on under the name of science studies is *against* science in that it is shown to embody and reinforce power inequalities within modern societies. Feminist epistemologists are concerned with gender inequalities inherent in actual scientific practice (e.g. Hubbard 1996), neo-Marxists and new liberals aim to show how science contributes to widening class inequalities (e.g. Levins 1996)[75], multiculturalists argue that modern science has a role in the

[75] The most important source of poitical criticisms of modern science is a variety of approaches within different Marxist traditions formulating ideological critiques of modern "capitalist" forms of science. One striking

oppression of the Third World by developed countries (following the vein of the Cultural Wars). In this respect SW, as seen by Hacking, "combine irreverent metaphysics and the rage against reason, on one side, and scientific metaphysics, and an Enlightenment faith in reason, on the other" (1999: 62).[76]

The political intent in science studies is often justified with reference to some understanding of the reflexivity thesis. If 'desacralising' or 'unmasking' results in depriving science of the false atmosphere of being detached from ideological, political and moral implications, then why should a scientific understanding of science strive to be politically and morally neutral with respect to its subject? In this vein, Scott, Richards and Martin (1990) argue for "a high level of political commitment by researchers in the sociology of scientific knowledge" (229) since commitment is shown inevitable. The answer for many is to take neutrality as a methodological stipulation, even if neutrality, like other methodological principles in general, can shown to be violated in the actual practice. If science is accepted as it is, i.e. trying to be as 'objective' as possible within the unavoidable limits, then science studies can also take this attitude—the main difference being that while science is not expected to reflect upon its own limits to disinterestedness, science studies ought to, as long as it is reflexive.[77] Methodological neutrality is thus a dividing principle between the two traditions in science studies, introduced in Section 1.3, that is, scientific analyses of the workings of science on the one hand, and cultural criticisms on the other.[78] In what follows, I continue to focus on the non-political version of science studies.

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example is a series of early papers by Imre Lakatos, later prominent defender of rationalism and asymmetrical explanations, see Kutrovátz (2002a).

[76] Here the epithet 'postmodernist', often used to characterise the enemy by defenders of science, can also play a legitimate role. The postmodern's antagonism with modernist institutions, e.g. science, provides a source, as well as form, of criticisms of science.

[77] Replying to the critics of the tenet of disinterested research in science studies—see the debate in *Social Studies of Science* **26**/2, an issue devoted to this problem—Harry Collins argues that "a 'scientific' approach is a good one, even in the face of our understanding that science is not what we once thought it to be" (Collins 1996: 241).

[78] Steve Fuller often refers to the two traditions as the 'high and low churches' in science studies: "the former concerned with anti-foundationalism and other recherché epistemological doctrines and the latter with a participatory politics that harkens back to more traditional forms of democratic populism" (Fuller 2001: 183). This metaphor, however, does not seem to express that while the latter, as a rule, relies on theoretical considerations of the former, the two are at odds with respect to their basic methodological commitments.

3.3 Aspects of explanation by the social

3.3.1 Causes and reasons

Descriptive neutrality is a desired feature of any scientific enterprise, and in the case of SSK, it is a sign of its own scientific character. According to Bloor (1992: 5), "[t]he sociologist is concerned with knowledge, including scientific knowledge, purely as a natural phenomenon". The four tenets of the Strong Programme "embody the same values which are taken for granted in other scientific disciplines" (7), and sociologists' ideas "will be in the same causal idiom as those of any other scientist" (5)—suggesting that the truly scientific style consists, first of all, in the description in terms of causal mechanisms.

In an influential paper, philosopher of science Larry Laudan (1984) examines the relation between the causal tenet and the alleged scientific character of the Strong Programme. He claims that it is the tenet of symmetry that amounts to the betrayal of scientific virtue, since no scientific discipline determines *a priori* the types of causes used in explanations of phenomena, while SSK postulates relying on "the same types of cause" in case of "say, true and false beliefs" (Bloor 1992: 7). He admits that in case of the true/false distinction, such a restriction is justified, while for the rational/irrational distinction it is certainly at fault. It is because we can never know whether a belief is true (*a la* Popper), but it is an empirical question whether it is rational or not. Note that he endorses a non-normative concept of rationality that can be utilised in a purely descriptive discourse.

A rational belief, according to Laudan (1984: 58), is one "that the agent can give reasons for antecedent to the adoption of the belief". Hence the difference between rational and irrational beliefs lies in that the former are based on reasons (or arguments), while the latter not. This is where the principle of symmetry seems to break down. However, this is not to say that only irrational beliefs can be explained by causes, and rational ones can be attributed non-causal reasons since, he insists, reasons are just a type of cause, such that arguments are causally efficient in adopting rational beliefs.[79] Laudan also admits that

[79] Philosopher Adrian Haddock (2004) gives a detailed analysis of the question whether reasons can be causes or not. His point is a refutation of Michael Friedman (1998), who states that SSK and traditional epistemology can actually go hand in hand since, where epistemologists offer a normative account, sociologists give descriptive answers to the very same questions. Haddock claims that reasons can be causes, and SSK be reconciled with philosophy, only in an internalist epistemological framework—meaning that objects of explanation must belong

rationality can be contingent upon society, i.e. the norms of rationality are changing with time. He claims that it is precisely this contingency that sociologists ought to set out to explain, perfectly in causal terms.[80]

While Laudan's criticism is often taken as a refutation of the Strong Programme, I find that eventually it boils down to rather minor points. He does not deny that all beliefs, including scientific ones, can be explained causally. He also admits that any belief, even rational belief, has social causes, since rationality has social bases (although he emphasises that not only social causes are at play, which is perfectly in line with Bloor's position, as we saw in Section 2.2.3). Moreover, he acknowledges that sociologists must not rely upon the very norms they invoke in their explanations, and that they must be impartial and neutral with respect to them. But he insists that rationality as a socially contingent system of norms provides a causal background to some beliefs which is different from the causal background of other beliefs. However, contrary to his understanding of the symmetry principle, sociologists will not decide *a priori* which types of causes they use in their explanations since, as a rule, they do not know which belief is rational and which is not prior to the empirical investigation of the social conditions of beliefs.[81] In my reading, the symmetry principle proposes a position that is meant to exclude precisely the *a priori* decisions suggested by antecedent evaluations.[82]

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solely to the mental realm—while externalists are at odds with SSK. Since my comparison is not between science studies and modern philosophy, but between science studies and their opponents in the Science Wars, I am not following this argument any further.

[80] On the face of it, this approach is somewhat similar to that of Kuhn in his later years, also the two have very different motivations. Kuhn wishes "to restore some badly needed bite to the whole notion of cognitive evaluation. It is needed, that is, to defend notions like truth and knowledge from, for example, the excesses of post-modernist movements like the strong program." (Kuhn 1991: 3-4) His view is that evaluations, especially those concerning the rationality of claims, can be employed at the level of analysis, bearing in mind that the standards of evaluation must be relative to the historical environment under study. However, Kuhn does not want to give causal explanations of norms, and in that his stance remains at the non-normative, philosophical level.

[81] The relation between norms and symmetry in SSK is summarised as the following: "For the relativist there is no sense attached to the idea that some standards or beliefs are really rational as distinct from merely locally accepted as such. Because he thinks that there are no context-free or super-cultural norms of rationality he does not see rationally or irrationally held beliefs as making up two distinct and qualitatively different classes of things." (Barnes and Bloor 1981: 27-28n)

[82] Karyn Freedman has recently improved on Laudan's criticism (Freedman 2005). Her target is the symmetry principle when she claims that the difference between rational and irrational beliefs is that the former were accepted with reference to evidencing reasons, even if these are contingent norms to be explained, while the

Now the question remains whether one can appeal to rationality and reasons in a non-normative fashion. Philosopher James Robert Brown, in his 'opinionated guide' to the Science Wars (Brown 2001), tries to disqualify SSK by giving a negative answer to this question. In his reading, Bloor renders science irrational, since he

> cannot allow that a single belief has been caused by reason and evidence (in the strongly normative sense). For the serious naturalist like Bloor, there simply aren't such things as reason and evidence (as normally conceived), since they are not part of the natural world. (Brown 2001: 151)

The two qualifications in parentheses show that for Brown, reason (and evidence) can only be understood in the 'strongly' normative sense, at least this is how we 'normally' conceive them. Stephen Turner, in his review of Brown, rather sarcastically replies that "the idea that SSK might be an attempt to explain *why* something was taken as a reason […] seems to be beyond Brown's intellectual horizon" (Turner 2003: 590; emphasis in the original). In other words, one can discuss norms in a non-normative fashion simply by treating them as objects of explanation, i.e. by naturalising them.[83]

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latter not. However, she illustrates the difference with individually held beliefs, forgetting about the fact that SSK set out to explain collectively held beliefs. Since any collectively held belief is consensual, it is based on norms as institutions (see below). Now Freedman's task would be to show the difference between epistemic norms resulting in rationally held beliefs on the one hand, and other epistemic norms resulting in irrational beliefs on the other. If she maintains that this is a difference of 'natural kinds', then she may be suspected to introduce *a priori* normative distinctions, and thus to beg the question.

[83] This difference in treating norms features in a controversy between Trevor Pinch and philosopher William McKinney. In reply to McKinney (1998), a criticism of Collins and Pinch (1993), Pinch (1999: 236-37) writes: "In our case studies, we look at the way epistemological criteria and methodological canons are used in practice. In other words, we are concerned with how scientific actors themselves employ notions like replication and calibration. On the other hand, philosophers like McKinney seem to want to use epistemological factors and methodological canons to assess how each side performed." McKinney (1999: 242) replies that, however praiseworthy purely descriptive accounts are, he "simply present[s] another narrative, but one which includes normative claims". His reason is that he wants to "discover demarcations between good science and bad science, and between science and non-science." (*ibid.*)

Before leaving this topic, let me outline as an illustration one strategy to naturalise norms, namely that of Bloor. According to him, "normativity is grounded in consensus" (Bloor 1996: 848), and consensus must be taken as an institution understood in the self-referential model developed by Barnes (1983). That is, similarly to (all) other institutions like money or leadership, which function as money or leadership exactly because all members of the community accept them and treat them as such, norms ought to be seen in the same framework. By accepting consensual standards—because everyone else accepts them as such, being a requirement for coordination—we create the possibility to apply the concepts 'right' and 'wrong': "I will call something 'right' if and because other people do so" (Bloor 1996: 848). Thus, the evaluative charge characteristic of the realm of norms stems from the consensually coordinated nature of social behaviour, and should be explained in terms of social interactions. This approach offers one manner in which to give causal explanations for the reasons and cognitive values working within a certain society.

3.3.2 Types of social explanation

We have so far relied on the supposition that society has a causal role in the formation of beliefs, but yet we have not said anything about what this role consists in. In other words, a comprehensive theoretical account is needed with respect to the effect of society on knowledge, and the mechanisms manifesting this effect. However, such an account is very rare in the literature, and as far as theoretical models are explicated, it often has a polemical purpose: implicit views seem to vary broadly within science studies.

The lack of such an account can be seen as inherited from classical sociology of knowledge. Bloor, for example, reproves Durkheim and Mauss (1902) for failing to give us "an adequate theoretical underpinning" for their claim, or providing "a general picture or model" of the processes they study (Bloor 1982: 268-269). Robert K. Merton (1941) offers a detailed analysis of the ambiguity present in Karl Mannheim's work with respect to the nature of social determination of knowledge, and he identifies five different types of connections invoked by Mannheim: (1) directly causal (in that a particular instance of knowledge follows from a general social law), (2) based on interests (either deliberate or unconscious), (3) social restriction of perspective (where the problem situation orients the inquiry), (4) social formations as prerequisite for forms of cognition, and (5) indefinite parallelism between

knowledge, culture and society. This list may suffice to illustrate how wide types of explanation can range, at least partly unintended, even within one author's work.

This situation has not much improved recently. By today, there is a huge amount of case studies in the relativist and constructivist tradition, but a widely accepted theoretical framework is still missing (see e.g. Seguin 2000). Collins (1981) called for a "third stage" in the "empirical programme of relativism", that is, after having had shown the "interpretative flexibility of empirical data", and the mechanisms delimiting debates over interpretations, he found the time ripe for relating the work, in terms of mechanisms, to "a wider social and political structure" (6-7)—which has not been accomplished in the quarter of a century since then. Indeed, the proliferation and divergence of different, and most often implicit, explanatory strategies has become so immense that it seems a hopeless task to offer a detailed and overarching classification. Instead, I will outline a simple and rude framework, based on the general/particular dichotomy, with the minimal purpose of highlighting some elementary distinctions and characteristics.

I. The first type proceeds by explaining the general from the general, i.e. it explains larger formations of knowledge (cognitive styles, conceptual schemes, paradigmatic theories, etc.) with reference to broad social characteristics. While such an explanatory strategy might be seen as suggested by the Marxian base/superstructure distinction, an important source of the sociology of knowledge tradition, explanations of this type are very rare in science studies. One classical example is Boris Hessen's paper on how Newton's physics was determined by the social and economic structure of his age (Hessen 1931). A more recent but still classical piece is Paul Forman's analysis (1971) of the development of quantum physics in Germany between the two World Wars, and the effect of cultural environment on the notion of causality and its crisis. Even Bloor's (1982) reconstruction of the Durkheimian thesis, "the classification of things reproduces the classification of men" (267), leads to a theory according to which "the social message comprises one of the coherence conditions" of the network of concepts (293)—as shown in the case of 17th century corpuscularianism which took its model, according to Bloor, from the favoured picture of social organisation.[84]

[84] Bloor's claim, presented in Section 3.2.3, that conceptions of knowledge encode images of society shows an inclination towards explanations aimed at general parallelisms. In that case, however, the direct message is that it is our view of knowledge, rather than knowledge itself, that is modelled on social formations or, again, views of these formations.

II. Since most science studies authors prefer to focus on small scale elements of the scientific landscape, many of them explain particular knowledge claims with reference to general social settings. Particular cases to be explained can range from specific beliefs to small scale theories, advocated by a particular scientist, and the general basis of explanation can comprise in norms within the wide community, or general interests of cognitive, economic or political kind, or technological possibilities available at a wider socio-historical level—where the source of explanation expands far wider than the local topic and its immediate environment. When Bloor (1973) raises the fundamental question, "How do beliefs relate to the institutionalized ways of behaving in a society?" (174), and he claims that the answer can be given, improving on Wittgenstein, by referring to norms, then both the theoretical approach and the cited historical cases imply that his emphasis here is on certain beliefs and their small environment, rather than belief systems, but the explanation draws on the large scale social. Many studies of controversies, such as the classical one by Shapin and Schaffer (1985), also contribute to the genre of work carried out to explain specific claims from the wide realm of the social.[85] This approach tends to be more empirical and less theorising than the previous one.

III. Probably the largest bulk of recent work in science studies is committed to explain specific instances from specific sources, i.e. where the range of explanatory principles does not extend beyond the immediate environment of the local phenomena under study. This is where the wide-spread notion of 'constructivism' is most native, according to which scientific knowledge is "made with locally situated cultural and material resources" (Golinski 1998: *ix*), and it is a part of its Kuhnian legacy that "some of the most important values governing scientific practice are quite local, frequently being specific to subcultures considerably smaller than all of the practitioners of a discipline" (22), not to mention society at large. While Collins (1985) argues that it is impossible to exclude either the microlevel or the macrolevel from the explanation, since "[m]an proposes, but society disposes" (149), constructivists such as Knorr-Cetina, and in some works Latour, created a paradigm of

[85] According to Golinski (1998: 37), Shapin and Schaffer shows that "[t]he local setting where scientific knowledge originates is crucial, but the wider realm beyond its walls can also be viewed as an arena in which knowledge is constructed", as a result of "large-scale extensions of local forms of life". However, Golinski's perspective is that of point *III* below, and to him, being interested in the non-local environment is a peculiar feature of that work.

entirely local explanations.[86] These studies shift the emphasis from historical cases, where large scale social formations often seem to show up better than local settings, to contemporary science where local factors are more visible than sweeping generalities. Also they most clearly leave behind the original framework provided by the sociology of knowledge, and they focus on actions and performances at least as much as on beliefs, guided by local values, local interests and local materials.

The shift from global to local types of explanation has several consequences for our investigation here. One concerns the relation between science and society: while global explanations, encouraged by the base/superstructure scheme, tend to view science and society as two distinct realms connected by some general mechanisms, explanations of types *II* and *III* see scientific activity as basically embedded in social context at every local point. Morover, while type *II* explanations presuppose an antecedent social world that frames local scientific performances, constructivist explanations of type *III* do not introduce any preliminary separation of science from society, and the two are intimately, often inseparably, interwoven in their case studies. So when the question of the influence of society on science is addressed, it is interpreted essentially differently by the two sides of SW, i.e. by scientists who normatively separate society as external to science, and by science studies practicioners who take science as one part of the social world. What is a serious question for scientists, whether scientific beliefs are determined by the natural world *or* society, is a false dichotomy for those who do not rely on clear demarcations either between science and nature (remember figure 2) or between science and society. Again, the position of science studies seems to escape the grasp of epistemological questions it is confronted with in SW.

Another consequence of the local orientation of science studies is that 'determination' by the social is not what the term usually implies in the natural sciences, where universal natural laws are taken to govern all fields of phenomena. The local perspective of science studies redirects the attention, from general laws and principles as determinants of knowledge, to the historically and culturally situated nature of scientific knowledge with all its opportunistic and contingent features. This brings us to the problem of contingency.

[86] Mary Hesse (1980) uses the label 'intensive' to explanations that seek to find correlations of specific cases, with no reference to general laws, as contrasted with 'extensive' explanations searching for general correlations between sciences and their historical settings.

3.3.3 Contingency

Bricmont and Sokal (2001a) repeat the same objection, explicated in Sokal and Bricmont (1998: 90), that was presented in Section 2.2.1 as an argument against methodological relativism: the acceptance of Newtonian mechanics in the 18th century cannot be accounted for without considering the evidences for it. To reinforce the point, here they supplement the argument with the following thought experiment (2001a: 41): Imagine that "a Laplacian demon were to give us all conceivable information about seventeenth-century England that could in any way be called sociological or psychological", and we could even process all the information. From these data alone, if we did not consider 'evidences' such as Kepler's observations, we would never be able to "predict" that scientists will accept a theory of a universal force of inverse-square law attraction. On the other hand, if we are to give a causal explanation of the acceptance of astrology, "it is at least conceivable that one could obtain a purely sociological or psychological account of the incidence of such beliefs, without ever invoking the good evidence supporting those beliefs—simply because there is no such evidence" (*ibid.*).

Here I will not return to all the objections that could be repeated from earlier sections considering the relation between reasons and causes, or regarding unsuspended *a priori* evaluations. What I want to point to is the authors' presupposition that having a satisfactory causal theory means being able to predict, from present parameters, any future state of the system under description.[87] This is the classical deterministic model of causality to which Laudan (1984) implicitly sticks to when he, criticising SSK's claim to be scientific by being perfectly causal in description, invokes that some parts of quantum mechanics, statistical mechanics and classical kinematics are not entirely causal altogether. While Laudan does not explain what he means by that—although such a claim, using a deeply metaphysical concept as that of cause, can easily be contested—the fields he refers to (or at least the first two) seem to hint that he identifies causality with its deterministic conception inherited from the Newtonian ideal of scientific explanation. However, the argument could be reversed: if such inevitably scientific and causal disciplines as mentioned above are non-deterministic, why should anyone expect causal determinism from the sociology of knowledge? Since the unfolding of the Strong Programme, sociologists have often argued that causal explanation,

[87] The impossibility of prediction in social sciences is a classical argument against comprehensive sociological explanations; see e.g. Popper (1957).

even if it allows for general laws, does not have to imply causal determinism: for instance, Mary Hesse (1980) makes this point, and she emphasises that not even the 'hardest' scientific disciplines are, or have been, able to achieve complete causal determinism.

Indeterminism is supported by the emphasis on the contingency of social processes, not in the sense that these processes are 'contingent upon' something (which is another matter), but simply expressing that "It could easily have been otherwise." (Sismondo 1993: 536) According to Hacking, contingency is one of the most important metaphysical 'sticking points' that play their role in SW. Natural scientists are 'inevitabilists' (Hacking 1999: 79) with respect to science, in that they suppose that there is some form of necessity in the way that science developed. I suppose that Sokal and Bricmont would grant that, given the evidences offered by Kepler, plus given some rational and clever enough scientists, physics was bound to evolve into a theory of a universal force of inverse-square law attraction. Contingentists, on the other hand, think that the dynamics of science is "[n]ot determined by how the world is, not determined by technology now in existence, not determined by the social practices of scientists, not determined by interests and networks, not determined by genius, not determined by anything" (73).[88] A useful analogy here is that of biological evolution: it is governed by laws of nature, but no set of conditions determines its future course (74).[89]

One way to introduce contingency, lately endorsed by the Edinburgh school of the Strong Programme, is through the concept of finitism. It is related to the topic of taxonomic classifications or 'lexicons', discussed in Section 2.3.3, and comes from a theory of meaning that emphasises the open-endedness of concept applications. This view claims that since any case of concept application is open-ended, i.e. not entirely determined by previous applications, all prevailing forms of classification are revisable and contingent (e.g. Barnes, Bloor and Henry 1996, Chapter 3). As beliefs are interwoven with, and dependent on,

[88] The emphasis on being 'not determined' might seem to invoke the famous Duhem-Quine underdetermination thesis, as formulated e.g. in Quine (1951). However, that is a claim about the relation of theories and experience, both understood in the positivistic framework, i.e. the relation between the network of propositions of a quasi-formal language on the one hand, and those propositions that directly anchor in sense experience on the other—to the effect that the latter do not determine the former. Non-determinism in science studies is a much broader claim, as this quote from Hacking suggests.

[89] In science studies, the most important source of this analogy is the last chapter of Kuhn (1962). There it is used to express that "scientific development must be seen as a process driven from behind, not pulled from ahead" (Kuhn 1991: 7). However, when dwelling on the implications of this analogy, Kuhn does not emphasise contingency explicitly.

classificatory practice, they are also subject to arguments for finitism. The essential indeterminacy characteristic to the whole conceptual realm is supported by Wittgensteinian arguments, where the "deep point" is that "rules do not contain the rules of their applications" (Collins 1999b: 785), and this applies not only to conceptual operations but, as for Collins, the methodological side of science as well. But it is not only 'intellectual science', i.e. science as dynamics of beliefs, that is open-ended in this sense: if we turn to science as practice, "we are confronted with the somewhat annoying picture of an indeterminacy inherent in social action" (Knorr-Cetina 1983: 134). Knorr-Cetina presents scientists as 'practical reasoners' embedded in highly 'opportunistic' and 'contextually contingent' situations, where the emphasis is on not purely cognitive, but morally charged features such the 'decision-ladenness' of all stages of research (1981).

Peter Dear (2001b) also connects contingency to moral issues. He contrasts the contingency postulate of science studies to scientists' overdeterministic meta-scientific claims: overdeterminism means that even if things could have turned out otherwise owing to some degree of social contingency, sooner or later the very same theory would have appeared in a different way—Newton's theory would have been developed by someone else—because truth will come out somehow. Then he draws an inspiring parallel between overdetermination versus contingency on the one hand, and religious beliefs about predestination versus free will on the other. The parallel illuminates the problem from a moral, rather than a metaphysical, perspective: if the development of science is preordained in any way, the ground for moral judgements concerning science and scientists (praising or condemning, making or not making responsible, etc.) is feebler than under the contingency assumption. Quite paradoxically, contingency is a prerequisite for the positive evaluation of scientific activity that scientists demand.

To sum up, having a comprehensive theory about the causal background of scientific beliefs, even if an essential role is attributed to the social sphere, does not imply that the dynamics of science is socially determined, or indeed determined anyhow, in the classical sense. But the extent to which social factors contribute to human cognition still remains a problem, especially in light of the enormous progress made by cognitive science recently.

3.3.4 Sociological versus psychological naturalism

According to Barry Stroud (1985: 71),

> Naturalized epistemology is the scientific study of perception, learning, thought, language-acquisition, and the transmission and historical development of human knowledge—everything we can find out scientifically about how we come to know what we know.

On this brief list we can recognise topics that bear primary importance for the sociology of knowledge approach to scientific cognition. Nevertheless, the term 'naturalised epistemology' is most often used to signify an intellectual tradition that has hardly anything to do with social studies of science, and in its mainstream form is often antagonistic to social explanations.

Naturalised epistemology emerged as a critique of traditional empiricist epistemology in philosophy of science (see e.g. Quine 1969). In reaction to the crisis of logical empiricism, it was suggested that the normative enterprise of traditional epistemology should be abandoned, or at least essentially revised, and the question "how we ought to" is to be replaced with, or subsumed to, the question "how we actually do". The call invoked extensive discussions and controversies, such as the one concerning whether normative concepts are really to be dispensed with or rather to be reduced to actual cognitive processes, but the whole discourse remained within the realm of questions related to traditional epistemology.[90] It was also implicitly agreed that naturalisation means reduction, in part or in total, of epistemology to psychology: the question "how we actually know" must be answered by psychologists. It is because traditional empiricist epistemology sought to ground knowledge in immediate perceptual experience, I think, that those advocating a scientific understanding of knowledge

[90] In reply to Hilary Putnam's argument as to 'Why reason can't be naturalized' (1983), which invokes that dispensing with the normative implies the elimination of the concept of truth and thus undermines its own claim, a number of philosophers tried either to replace the truth concept or to rescue normativity on naturalistic terms, and this lead to nuanced philosophical debates. In contrast, science studies is *ab ovo* immune to Putnam's argument inasmuch as they stick to the distinction of levels introduced in Section 1.4—according to which, suspending the commitment to truth claims at the object level is nowhere near to doing the same at the level of analysis. On the other hand, considering the reflexivity thesis of SSK would render the situation more problematic.

at first focused primarily on cognitive conditions of perception and their relation to belief formation.

However, earlier sections from this work may suggest that 'thought', 'learning', etc. have thorough social roots and, therefore, sociology is a likely source of our understanding of important aspects of cognition. Naturalism in science studies draws on sociology or social sciences in general, rather than psychology, and this preference widens the gap between traditional epistemology and the naturalistic approach in several respects, presenting sociological naturalism as more radical than the psychological version. Some of the most important contrasts are the following:

1. While psychological naturalism remains within the framework of individualistic epistemology, and thus sides with traditional epistemology regarding the overall epistemic situation, in social studies of cognition the epistemic subject is of a social character. In other words, knowledge for the sociologist is seen as something fundamentally social, and our understanding of it requires going well beyond the individual mind. Some important consequences of this turn were outlined in Section 2.3.2.

2. Naturalisation implies a realist commitment to the ontological domain of its source discipline. When psychology plays that role, we take it for granted that neural stimuli and neurophysiological states exist, in perfect accordance with the familiar physicalistic ontology of the natural sciences. But when explanations are grounded in social science theories, the ontological commitments are directed to entities of the social realm, such as institutions, interests, norms and all, and these are often seen as more problematic in terms of existence than customarily postulated physical entities. This becomes dubious in the light of idealism in science studies: as we have seen in Chapter 2, most social students of science take an idealistic attitude toward the objects of scientific discourse on either methodological or metaphysical grounds, and any position granting social entities and denying physical ones may seem especially suspicious.[91] This also explains why the charge of being anti-science, often repeated in connection with science studies, is practically absent from the literature

[91] It is often asked by defenders of science in SW why anyone would want to explain the quite well-established knowledge of natural sciences on the grounds of a much less stable knowledge of social sciences. This question, if viewed as involving more than the worry about reversing the alleged hierarchy of certainties, reflects the general resistance to approaches with such contraintuitive ontological commitments. On the other hand, critical views arise even within science studies: Latour often makes the objection to SSK that it is asymmetrical in that it attributes more existence to society than to nature (e.g. Latour 1999).

surrounding classical naturalistic epistemology: trying to explain science on 'real' scientific terms is less problematic than subjecting it to the sport of 'soft' sciences.[92]

3. As we have seen in Section 2.3.3, sociological explanation seems immediately suitable for the conceptual aspects of cognition, while psychological explanation often starts off from the perceptual aspects. The difference in perspectives results in a significant difference in focus and interests, and this can party explain the lack of mutual communication between the two fields. When sociologists of knowledge refer to cognitive science with its psychological framework, however, they often seem uncertain how to think about the relation between the two fields. Recall that some participants in science studies, such as Bloor, suggest some kind of cooperation since "cognitive science and the sociology of knowledge are really on the same side. They are both naturalistic and their approaches are complementary." (Bloor 1992: 170) In line with his view outlined in Section 2.2.3, he also admits that "[c]ultural variation is plausibly thought of as imposed on a stratum of biologically stable sensory capacities" (31). Collins (1985: 172-173) cites similar passages from Barnes (1981) and Hesse (1974), but he argues that the "physics and physiology of situations" (Hesse 1974: 13) is a needless assumption for sociologists since they work with cases which are wholly institutionalised and beyond the range of any (methodologically) considerable limit from perception. He concludes that "physics and physiology are taken to play no part in the maintenance of conceptual order" (Collins 1985: 174).[93]

4. While reduction of epistemology to physiology highlights those aspects of cognition that are universally human, i.e. stem from the biological side of 'human nature', reduction to

[92] Ullica Segerståle (2000c) argues that in the nature/nurture debates of the 1980s, it is those people who resisted the naturalistic calls of biologists and psychologists, coming mainly from the humanities but some from natural sciences as well, that were labelled as anti-science. It suggests that, in some respect, the proscience/antiscience divide seems to run along, instead of the antinaturalistic/naturalistic opposition, the natural/social sciences dichotomy.

[93] One way to get beyond the traditional problem situation, inherited from logical positivists, where all knowledge is expected to be founded in perceptual experience, is shifting the focus from clrearly empirical sciences to mathematics, where perceptual experience clearly plays a less essential role. While Bloor puts a great emphasis on mathematical knowledge, and his books extensively discuss the topic on the same grounds as problems concerning empirical sciences, most authors in science studies tend to disregard mathematical sciences because they focus on scientific activity as material manipulation of the physical environment. Thus philosophy of mathematics is mostly unable to profit from recent proliferation of science studies approaches, although the ground for such a change is prepared by works such as that of Lakatos (see Kutrovátz 2002b), or historians of mathematics like Árpád Szabó (Kutrovátz 2004b).

sociology is interested in the variability brought about by cultural differences. Steven Pinker, in his popular book *The Language Instinct* (1994), develops his model of the biological nature of language (chapter 3) in opposition to the Sapir-Whorf thesis in linguistics, according to which language determines thought in a way that allows for radical cultural differences in thinking about the world. (Note that he supports his view with examples that are directly tied to perception, such as colour terms, and he even describes the biological background of the cultural stability of colour term arrangements, which links back to point 3 above.) He devotes his last chapter to a refutation of what he calls the 'Standard Social Science Model', according to which human behaviour, in contrast with animal behaviour, is not determined by biology, but it is culture that shapes all aspects of cognition. After citing Jerry Fodor, who admits to hate nothing more than relativism—which he attributes to philosophy of science, anthropology, sociology and linguistics—Pinker claims that relativists' mistake consists in the denial of universal human nature.[94] But while he is eager to find what is common to all cultures, as if he were searching for the 'universal laws' of human cognition, Bloor insists that "variation forms the starting point of the sociology of knowledge and constitutes its main problem" (1991: 5), and is looking for the causes of differences.

The linguistic tradition Pinker works in emerged under the assumption that all human languages have the same biological bases to the effect that they are, to certain degree, similarly structured and preformed by these capacities; and cognitive science has achieved enormous successes in extending this assumption from language to many aspects of human behaviour. However, just as it is completely implausible to claim today that no aspect of language is biologically determined (therefore, every aspect is culturally shaped), the same is true to the claim that all aspects are determined by biological built-up, and none by the social—and it is true for human behaviour in general. Cultural variation and invariance are equally present in human cognitive behaviour, and both are legitimate subjects of scientific inquiry. Both Pinker's polemical tone and Collins' strategy of hedging are admissible as tools of legitimating disciplinary autonomy for the biology/sociology of cognition, but they are not to be used as arguments against the possibility of fruitful communication. Again, just as it was questioned in Section 2.3.3 that sociology should have nothing to do with perception, so it

[94] He finds this view politically faulty, since it is favoured by dictators who want to see human beings as perfectly pliable by ideologies. His argumentation mostly follows a manner of popularisation and PUS, rather than remaining at a scientific level, and his attack on social sciences is in line with that of SW, just like the passage he quotes from Fodor. Noam Chomsky shows a similar attitude to relativism in social sciences, and he is often cited by the scientist side in SW (e.g. Sokal and Bricmont 1998: 201-202).

seems unreasonable to deny that psychology has important things to say about our capacities concerning concept formation and usage. Also, for many working in cognitive science it seems unjustified that the perceptual and the conceptual are two totally distinct spheres—which suggests that however we specify the subject matter of the two disciplines, they are bound to overlap to some degree. While this bears the promise of a future cooperation, some disciplinary tensions are also likely to appear, as seen in Pinker's case.

Perhaps it is worth recalling, however, that 'cognitive sociology' is only a part of the project pursued by science studies. While Bloor's Strong Programme is placed in a sociology of knowledge tradition, and aims at providing causal explanations of the social processes forming, sustaining and changing scientific beliefs, much of what is done in case studies in the discipline is less oriented toward beliefs and more toward practice.[95] While this might shed different light, regarding the context of relevance and significance, on some of the issues I have discussed above, my epistemological focus has kept me with the cognitive aspects of science studies.

[95] Of course, the difference is by no means sharp. Polanyi's (1958) concept of tactit knowledge bridges the seeming gap between beliefs, understood as propositional and being modelled upon theoretical knowledge (as in the classical trends in philosophy of science), and the realm of practice and actions. The concept has widely been utilised in science studies: e.g. Collins (1974).

4. CONCLUSIONS

4.1 The Curiosities of a Position

The main aim of this dissertation has been to show that a number of those features in science studies that are often seen disquieting or even threatening compared to more traditional approaches to understanding science are consequence of the position taken by science studies in analyses of science. In contrast with both the metascientific claims made by scientists and the reconstructionist attitude of classical philosophy of science, the aim in science studies is to give explanations of the workings of science from an external point of view, rather than express or explicate the scientific enterprise in and from itself.

One part of this contrast, as was emphasised, is that the source of explanation is different from that within science: instead of the 'physical'/'material' world or its immediate experience, participants in science studies ground their explanations on either social theories about, or the phenomenological description of, scientific practice. The resulting accounts display a practically exclusive interest in the subject of scientific cognition, understood as the research community and its social environment, and the neglect of the object of knowledge is often seen as a sign of idealistic philosophy. While individual authors and schools in science studies differ in their strategy as to what degree, and on what grounds, they justify this idealistic setup, there is a widespread commitment, with notable exceptions, to its methodological form throughout the discipline. In Chapter 2 I attempted to describe some of the basic strategies adopted by prominent science studies authors in reply to the problem of idealism that stems from the methodological outset of the discipline. The choices as presented above can be summarised according to the following scenario.

If we are to explain the dynamics of scientific knowledge in a broadly scientific manner, i.e. on non-normative base, we have to take a position at a metalevel distinct from the level of the scientific discourse which we are to study. With science as our object of analysis, the object of the studied scientific inquiry, nature, is removed from immediate access. On the face of it, this suggests that our analysis is unable to talk about nature independently of beliefs about it. Such a position can be adopted on purely methodological grounds (Collins), or within an aptly constructivist metaphysical framework (Knorr-Cetina). But it can also be refused, either with a claim to resort to science as a partial source, and not only the topic, of investigation (Bloor), thus restricting the scope of our enterprise, or by denying the validity of

any strict distinction of levels based on the subject/object dichotomy (Latour). While these options certainly do not exhaust the range of possibilities, they are characteristic of the main trends in traditional science studies. In all these options, one major remaining question concerns the scope of validity offered by a framework where scientific knowledge is explained by the properties and conditions of the collective cognitive subject.

Another aspect of the contrast between traditional and recent sociological analyses of science concerns the object of explanation: instead of focusing on the results and the conditions of success in scientific activity, science studies research concentrates on the conditions and mechanisms of the everyday practice of science. The naturalistic attitude refrains the discipline from reasoning from the norms and commitments underlying scientific activity and, by taking the 'alien's' perspective, it brings these very norms and commitments into the domain of explanation. In other words, invoking the classical dichotomy, the approach taken by science studies aims at providing explanation, e.g. by offering causal or otherwise naturalistic narratives, rather that obtaining understanding as an ability to reproduce the perspective of the actors under study.

As I tried to show, suspending the assumption of the unproblematic validity of science is a consequence of the intention to see it as natural, rather than 'enchanted', and this 'agnosticism' in no way reveals opposition to or hostility towards science. Nevertheless, the images of science are functional within society, as pointed out in Bloor's recall to Durkheimian arguments, and the interaction between scientists' understanding of their own activity and the wider public's concept of science is constitutive of a social dynamics where the stakes are far from being purely intellectual. This theme is central to a variety of trends in science studies in which cultural-political criticism and participatory activism are at issue—trends that this work has chosen, for sake of coherence and integrity, to dismiss almost entirely. Instead, I concentrated on some of the basic features of explanations with naturalistic intent, such as giving causes rather than reasons, or the preference of micro and/or macro levels, or the issue of contingency in sociological accounts. Again, the question was raised how much sociological reductionism can grasp of the workings of science.

In what follows, I try to outline a philosophical framework where some of the discussed problems are seen to be interconnected. My aim, however, is not to identify the 'true' epistemological position of science studies that was anyway found diverse enough, but to reconstruct a very general perspective from which criticisms of the field are seen in a new light.

4.2 Asking the Wrong Questions

Throughout this work I have made claims, at several points and from several perspectives, regarding the epistemological stance of science studies. Now I attempt to sketch a rough picture based on the most general lessons drawn from the two substantial chapters, which I take to be the following: 1) In science studies, there is an intrinsic problem regarding the possibilities of considering the object of scientific cognition. 2) Scientific knowledge is seen by science studies as determined, whatever it might mean, by social conditions. Bringing these two claims together calls for further specification and qualification of the overall epistemological position of the discipline.

In order to reconsider the charge of idealism, I place the concept in a general, albeit gross and oversimplified, context of the history of modern philosophy. Paraphrasing a classical formulation given by Marx (1977 [1857]), idealism can be taken to be the position claiming that 'consciousness determines existence', rather than the other way round. In the traditional epistemological framework, it means that cognition is an epistemic process acting from the subject on the object, while in the opposite of an idealistic position, whatever it is labelled, the object of cognition acts upon the subject and shapes it. It seems clear that the 'folk epistemological intuition' underlying much of modern epistemology sides with the non-idealistic direction of the epistemic relation, and even when cognition is seen as partly active on the subject's side, rather than a passive reception of influences, the fundamental epistemic situation is taken to concern how the object (nature or the world) determines the subject (representing of the former). On the other hand, as we have seen in Sections 2.2.2 and 2.3, there is a tendency in science studies to see *a priori* conditions of the subject as determinative and even constitutive of the object of cognition. And here is a problem: while Marx is a classical source of science studies, holding that knowledge is largely determined by the conditions of the subject, he was against idealism in that the emphasised that it is existence that determines consciousness and not the other way round. The above presentation of his thesis is faulty in that by 'existence' he means something different, the social existence of men, from what I have suggested, the existence of the object of cognition. My aim with this small cheat is to show why the charge of idealism is misconceived: not because it is entirely false, but because it is related to a general epistemic problem that is *not* the main problem dealt with in science studies.

The traditional epistemological problem concerns the relation between the object and the subject. In modern epistemology, from Descartes on, accounts for this relation have had to face the difficulty that there is a radical, often ontological, gap between subject and object, the mental and the material, that is supposed to be bridged. According to the familiar scenario offered by 19th century historiography of philosophy, the two main strategies to bridge the gap were that of empiricism, starting out from the object and proceeding toward the subject, and that of rationalism, proceeding in the opposite direction. While empiricism was immediately faced with the problem of the contribution of the subject to cognition, from Locke's 'substance' to Hume's 'necessity' and 'causality' to the logical empiricists' 'logical must', rationalists became soon idealists by removing the object as such out of access (notably Kant). While both traditions were primarily concerned with the subject, having taken the quest of accounting for knowledge, they disagreed over what they make primarily responsible for the possibility of knowing: the givenness of the object or the constitutiveness of the subject.

It is in this, perhaps too simplified, sense in which the position of science studies was earlier related to Kant's legacy, with the purpose of contextualising the alleged idealism of the discipline. One of the important differences from a Kantian approach, now becoming essential, was found in the nature of the constitutive epistemic conditions of the subject: while for Kant these conditions are transcendental, for science studies, just like for Marx, they follow from the social setting of the subject, now open to naturalistic discussion. What determines 'consciousness' here is the social reality of the cognitive subject. In this sense the position of science studies is not idealistic: it is neither necessarily realist nor anti-realist with regard to the existence of the object, but it is realist regarding the social existence. Ontological commitments of the discipline primarily concern the realm of the social, and the object of cognition becomes an optional problem. It is important to note that, as argued in Section 3.2.2, social determination of knowledge does not mean external determination because society, in an important sense, is the subject itself. While traditional epistemology studies relations of determination between two separate realms, of the subject and of the object, in science studies the problem concerns the self-determination of a complex (although not necessarily homogeneous) system—just like in the case of the science of material nature.

The question in science studies is not how to bridge the classical epistemic gap between subject and object, and the fundamental epistemological situation of the discipline is different from that of traditional epistemology. Recalling Figures 1 and 2, the role of 'nature' in the science studies position (Fig. 2) is practically as unimportant as the role of 'science studies' would be for the position of science (were it there in Fig. 1), and we expect the level

of ‘nature’ to be somehow there in Fig. 2 only because good-old familiar questions are related to Fig. 1. So, when seen from the classical situation, like in criticisms in SW, the picture is different from the one we get by looking from within science studies: while norms render society seemingly external to science, and thus the danger of external determination appears, the most important question is taken to concern the role of the object in scientific cognition, and the problem of idealism emerges. But if my description of science studies is correct then these problems are not internal to the discipline and are, in their acute form, native to an external view of the field. The problem with external criticisms is that they, by relying on the critic’s own presuppositions and commitments, often fail to consider those of the criticised position, or even take into account that they are different from the ones used. Before such fundamental differences in interest are realised, criticisms such as cited here are likely to remain similar to the monologue of the deaf.

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