Anti-Latour

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1. Introduction

Bruno Latour is a vehement critic of the sociology of knowledge in general, and the Strong Program in particular.[1] For those who are familiar with his books *Science in Action* (Latour, 1987), *The Pasteurization of France* (Latour, 1988) and *We Have Never Been Modern* (Latour, 1993) the pivotal role played by these criticisms in Latour's writing will be evident. To those who only know his work by repute, or who have only read the first edition of Latour and Woolgar's *Laboratory Life* (Latour and Woolgar, 1979), presenting him as a critic of the sociology of knowledge may seem surprising. Latour's work and the Strong Program in the sociology of knowledge are frequently classed together under the label of 'social constructivism', and this creates the impression that the two enterprises must be fundamentally similar. This is reinforced by the fact that Latour wants to go further than sociologists of knowledge, whose work is said to represent something of a half-way house. He thinks sociologists are insufficiently radical in their critique of science (Latour, 1992, p. 273). Nevertheless, in reality, the two approaches are deeply opposed. In Latour's eyes the sociology of knowledge has been a failure, and it

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[1]The traditional stance towards the sociology of knowledge can be called the 'weak' programme. This involves the idea that socio-psychological causes need only be sought for error, irrationality and deviation from the proper norms and methodological precepts of science. Apart from this sociologists can, at best, illuminate the general conditions which encourage or inhibit science. Examples of this stance are to be found in the work of Lakatos (1971), Laudan (1977) and more recently Haack (1996). Followers of the 'strong' programme, by contrast, argue for the need to explain, in causal terms, all systems of belief regardless of how the analyst may evaluate them. It should perhaps be stated at the outset that the causes in question have never been confined to social causes. Such a limitation would be incoherent. Sensory stimulation by objects in the environment always plays a central role. An account of this approach can be found in Bloor (1976) and Barnes *et al.* (1996). The all important, but frequently misrepresented, 'symmetry' requirement will be discussed in the course of the present paper. For an account of the epistemological background and consequences of the programme, which includes a discussion of Haack's paper and other recent attacks, see Bloor, forthcoming.

will continue to fail unless it adopts an entirely new approach which will qualitat-
ively change its character.

In making these claims Latour knowingly aligns himself with a stance in the
sociology of science associated with the work of Robert Merton (see Latour, 1988,
p. 257). Like Karl Mannheim before him, and many others since, Merton felt that
sociological enquiry into the nature of knowledge was bound to be of a limited
character. It was confined to offering a description of the conditions encouraging
or inhibiting the growth of science. At most it could isolate the causes influencing
the direction of enquiry. The process of cognition itself, however, is governed by
methods and criteria which do not derive from, or vary with, our institutions and
conventions. Scientific knowledge answers to nature and reason, not society. Latour
shares this pessimism about the prospects of the sociology of knowledge, but
reaches the conclusion by a different route. He does not want to go back to asserting
the autonomy of reason and nature. Far from it: he believes sociologists are still
too much in thrall to such ideas. They must shake off their remaining influence if
they are to make any progress. We must, as he puts it, take 'one more turn after
the social turn' (Latour, 1992, p. 272).

I think Latour is wrong: there is no further 'turn' to be taken. I don't mean that
sociologists of knowledge have completed their task. Of course they have not.
Fundamental ideas still stand in need of refinement, and there is much work to be
done both empirically and theoretically. My point is that Latour's ideas do not
represent the way forward. If anything they are a step backwards. To make good
these claims I shall offer a defence of the Strong Program, first formulating Latour's
objections and then describing his own proposals for analysing knowledge. I shall
conclude that his criticisms are based on a systematic misrepresentation of the
position he rejects, and that his own approach, in so far as it is different, is unwork-
able.

In order that my aims should not seem too negative, let me indicate the points
of wider interest which will arise in dealing with these criticisms. Latour's errors
about the sociology of knowledge derive from his stance towards a very basic
principle which may be called 'the schema of subject and object'. This schema
implies that knowledge is to be understood in terms of an interaction between an
independent reality, the 'object' of knowledge, and a knowing 'subject', embodying
its own principles of receptivity. (Typically, though not necessarily, this subject
will be said to construct 'representations' of the object.) Remarkably, Latour wants
the sociologist to reject this schema. Of course, he is not the first to see the subject–
object schema as a source of problems, but it must be remembered that there are
many different ways of interpreting it, and many different levels on which it can
be applied. It would therefore be surprising if the subject–object schema had no
sphere of legitimate application. There is no doubt that it has some intriguing
limitations—I shall introduce one such in a moment—but, unlike Latour, I see no
advantage in adopting a wholesale opposition to it. On the contrary, under certain

interpretations, I think there is good reason to retain it. I shall therefore take the opportunity raised by this challenge to the subject–object schema to rehearse some of the fundamental, methodological questions in the field, and to address some of the confusions currently in circulation.[2]

2. Criticisms of the Strong Program

Latour argues that both sociologists of knowledge, and their previous critics, have all worked within the framework of the subject–object polarity. Everyone has been assuming that knowledge is to be analysed into two ingredients: one furnished by the object, the other by the knowing subject. Theories of knowledge are just the stories we tell about how these two supposed ingredients are to be identified, how they interact, and in what proportions. Some will lay great stress on the complexity of the knowing subject's contributions, others will see it as a passive receptacle, or like a blank sheet waiting to be written on. Some accounts of knowledge will treat the subject as an individual mind, others will identify it as a group or a culture. Obviously, for a committed sociologist, the ultimate knowing subject will be social in character, in short, 'society'. Whatever these differences in approach, whether nativist or empiricist, individualist or collectivist, the overall schema has always been the same, and the polarity of subject and object has been imprinted on the subsequent account. We can think of the schema, says Latour, as a line as shown in Fig. 1.

While most theories of knowledge of this kind apportion influence between subject and object, notice the two extreme positions on the scale. These end-points suggest the possibility of avoiding an eclectic or dualistic picture. We could aim at a purely objectivistic theory and try to explain everything (including society) in terms of nature. On the other hand, we could aim at a purely subjectivist theory and try to explain everything (including nature) in terms of society. Latour identifies the Strong Program in the sociology of knowledge as an approach of the latter type. It works within the subject–object polarity but occupies one of the extreme positions open to theories with this structure. As Latour puts it, for the Strong Program, 'Society was supposed to explain Nature!' (Latour, 1992, p. 278).
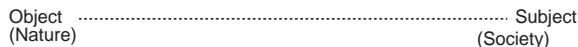
Object .... Subject
(Nature)   (Society)

Fig. 1. The subject–object schema.

[2]Valuable and detailed accounts of the stance toward the subject–object schema in the work of Husserl, Heidegger and Wittgenstein are to be found in Kusch (1989) and, with special reference to the analysis of self-consciousness, Tugendhat (1986). Kusch brings out the way in which, e.g., Heidegger's opposition to the schema was an expression of his rejection of the individualistic and transcendental tendencies in Husserl. The relevance of this for my argument is that rejecting an individualistic and transcendental version of the subject–object schema doesn't imply rejecting it in every sense. There could still be important work for it to do in the context of, say, an anti-individualistic and naturalistic analysis of knowledge.

To understand the significance of Latour's characterisation I need to explain the central idea of the Strong Program, and then relate it to his subject–object axis. The main feature of the Program is the so-called 'symmetry postulate'. Both true and false, and rational and irrational ideas, in as far as they are collectively held, should all equally be the object of sociological curiosity, and should all be explained by reference to the same kinds of cause. In all cases the analyst must identify the local, contingent, causes of belief. This requirement was formulated in opposition to an earlier prevailing assumption, still defended in many quarters, which has it that true (or rational) beliefs are to be explained by reference to reality, while false (or irrational) beliefs are explained by reference to the distorting influence of society. To take an example often used by critics of the sociology of knowledge, Mendel's discovery of the laws of inheritance are explained by his observations of the plants in his experimental garden. By contrast, the ideology of Marxist–Leninism, the workings of Stalin's dictatorship, and the political opportunism of certain Soviet agronomists suffices to explain the attractions of the anti-Mendelian claims of Lysenko (see Medvedev, 1969; Joravsky, 1970). The symmetry postulate signals the rejection of this approach, but what is the alternative? Given Latour's location of the Strong Program, at the extreme subjectivist end of the spectrum, the only alternative open to the sociologist seems to be that of 'even handedly forbidding both groups access to the real' (Latour, 1996, p.79). The claim would then be that *neither* Mendelism *nor* Lysenkoism had anything to do with 'nature'. Both would equally have to be seen as mere projections of some constellation of interests or institutionalised modes of thinking: 'the white screen on which society projects its cinema' (Latour, 1993, p. 53). Symmetry and subjectivism thus seem to go together.

Latour, rightly, rejects the symmetry principle understood in this way. Despite its name it is, he says, deeply *asymmetrical* because it puts all the explanatory weight on society and none on nature. It doesn't give proper weight to non-social things and processes, or acknowledge their contribution to our social arrangements. At first this may look as if Latour wants to mix together ingredients from society, and ingredients from 'nature', in the standard way, as if he merely wants to lure us away from the extreme ends of the subject–object spectrum. But this isn't his point. He explicitly rejects such eclecticism. His idea is that we must not try to explain nature in terms of society, or society in terms of nature, nor should we explain knowledge as a mixture: we must explain both society and nature, at once, in terms of a third thing or process. Society and nature are, as he puts it, 'co-produced' (Latour, 1992, p. 287).

In concrete terms, Latour says the attempt to account sociologically for the subtlety and richness of scientific results is a hopeless task. In *The Pasteurization of France* he rejects the possibility of accounting for Pasteur's discoveries about microbes by reference to social facts about Pasteur. Such facts are too sparse for the enterprise to be plausible:

> Conservatism, Catholicism, love of law and order, fidelity to the Empress, brashness, passion—those are approximately all we get of the 'social factors' acting on Pasteur. But they are not much if we put on the other side all the scientific work to be explained. (Latour, 1988, pp. 257–258)

There will have been many conservative, patriotic Catholics with character traits not unlike Pasteur's, but they didn't discover the anthrax bacillus. Social categories, it seems, are not discriminating enough for such an ambitious explanatory undertaking.

It is worth emphasising the logical connection between Latour's two main points, that is, between the idea that the Strong Program explains nature by reference to society, and the idea that the resources of sociology are too crude to account for the likes of Pasteur's work. The connecting link is that the subtleties of Pasteur's work come from the detailed character of the observations he makes. If the Strong Program denies any role to inputs of this kind, and treats Pasteur as responsive only to the social influences on him then, Latour concludes, it can't do justice to the detailed scientific findings. The dream of a sociological explanation of the content of science is an idle one—as its traditional critics have always said.

Latour's remedy for these defects is to propose a new symmetry principle. Calling the symmetry principle of the Strong Program the 'first' such principle, his second or generalised version is the idea mentioned above, that both nature and society should be seen as co-produced. Because, on Latour's reading, the Strong Program explained nature in terms of society, there was no way in which agency could be attributed to things. All agency resides with society. The second symmetry principle restores agency to things. It allows a truly symmetrical stance from which to understand the way in which both nature and society are constituted. Only in this way, implies Latour, can we acknowledge that to make a scientific discovery is, at the same time, to change society. For Latour, this represents an advance on what has gone before, allowing us to see changes in science as themselves changes in society.

The new principle of symmetry, in which the analyst is poised, as it were, above both nature and society, can be represented by another, vertical, axis on the diagram. This will be orthogonal to the original, subject–object axis and, of course, nature and society will be distributed symmetrically about it. Following Latour, we have something as shown in Fig. 2.

How does Latour understand the point I have labelled 'origin'? It has to be said that his account is obscure, but I shall come back to this. For the moment all we need to know is that the vertical axis is treated as a measure of 'stability' (Latour, 1992, p. 285). The stability in question is that of the distinction between nature and society. The idea is that low down on the axis, near the origin, there is no clear sense attached to the difference between things that are really in nature, and things which are merely matters of collective belief or opinion. Positions higher up represent situations where agents treat the demarcation between nature and
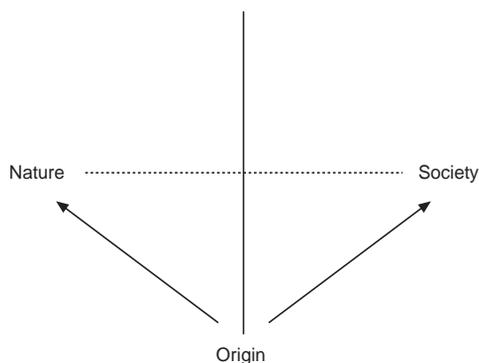
Fig. 2. Latour's second symmetry principle.

society as fixed and settled. The relations between nature and society cannot there-
fore be represented by a rigid polarity because that polarity itself is subject to
variation along the vertical, stability axis. Scientific innovation, for example, is to
be represented as a movement back and forth along the stability axis, as the inno-
vator modifies the shared sense of what 'things in themselves' really exist, out
there in nature. In modifying our ideas the innovator is, in Latour's terminology,
making and re-making both nature and society. As he put it:

> That there is a history of the 'things in themselves' seems absurd only to those who
> want to fix us forever into the boring confrontation between a subject (or a society)
> and an object (or a nature). Meanwhile, innovators are constantly crossing the bound-
> aries between nature and society, and turning our careful distinction between what
> has been revealed, what has been discovered… and what has been fabricated into a
> shambles. (Latour, 1988, p. 262)

It is worth reflecting on the implications of this passage. The reference to 'things
in themselves' is, of course, partly an allusion to Kantian ideas about the noumenal
basis of the subject–object schema. I think we may also assume it refers to more
common-sense ideas about the independence of the objects of nature from our
ideas about them. We take for granted that trees and rocks, as well as electrons
and bacilli, have long been stable items amongst the furniture of the universe.
Putting aside evolution, geological change and the nebular hypothesis, there is a
sense in which they 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. For Latour, however, there *is* a history for things in
themselves, precisely because he sees himself as having left behind the usual
assumptions of transcendence and independence. Belief in that transcendence and
independence is just an expression of the subject–object schema he wants to reject.

We have now seen the criticisms Latour directs at the Strong Program, the
assumptions he identifies as lying behind it, and the main lines along which he
thinks the enterprise should be reformed. His aim is to produce some manner of
non-sociological, non-reductionist analysis of knowledge, one that neither reduces

nature to society, nor society to nature. He calls this project 'anthropology' but it is not the anthropology of an Evans–Pritchard or a Mary Douglas. The entire Durkheimian tradition is dismissed on the grounds that it is scientistic and assumes a modernist division between science and society of the very kind that should be challenged (Latour, 1990, p. 167).

3. Assessing Latour's Criticism

Latour's attack depends on a specific characterisation of the aims of those who pursue the Strong Program. They are said to be trying to explain nature in terms of society. In reply it has to be said that this is a profound misrepresentation. The aim isn't to explain nature, but to explain shared beliefs about nature. The enquiry is into the character and causes of knowledge, or what passes as knowledge, and not (in general) into the objects which the knowledge is meant to be about. Obviously I will soon have to confront the question of whether, and how far, we can talk about the knowledge of an object without talking about the object itself, but we need to begin by getting the overall aims of the enquiry properly in view. This is something Latour fails to do. The idea that anybody should be trying to 'explain nature by society' is so peculiar that it is surprising that a critic should ever impute it, especially given that the formulations of the program are explicit in their materialism (see Bloor, 1976). Nevertheless, throughout the entire discussion, Latour makes no systematic distinction between nature and beliefs about, or accounts of, nature. He repeatedly casts the argument, his own as well as that of his opponents, in terms of nature itself rather than beliefs about it. (Remember that for Latour, it is society and nature, not society and accounts of nature, which are co-produced.) It is as if he has difficulty telling these two things apart. The quotation above, about the innovator crossing the boundary between nature and society, is a case in point. It is, however, easy to see why Latour proceeds in this fashion. He rejects the subject–object distinction, and drawing a boundary between nature, and beliefs about nature, is just a form of this distinction.

Latour's criticism, then, starts by ignoring the fact that the Strong Program is part of a naturalistic and causal enterprise. From the standpoint of the Strong Program, society itself is part of nature. The word 'nature' refers to the all-encompassing, material system in which human animals and the entire pattern of their interactions, and all the products and consequences of these interactions, have their allotted place. To talk about society explaining nature, when it is but one part of nature, is incoherent. Knowledge itself is just one more natural phenomenon. Rather than positioning the Strong Program at the subjectivist end of the subject–object axis, as Latour does, it would be closer to the spirit of the enterprise to put it at the opposite end.[3]

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[3]I am not suggesting that Latour would be any more sympathetic to the Strong Program if he had fully and properly realised that it was a naturalistic enterprise—he surely would not, given that he is anti-naturalistic and anti-causal. For an example of anti-causal thinking see Latour, 1996, p. 88.

Because Latour has picked up the wrong end of the stick it isn't surprising that his subsequent account of the symmetry postulate is confused. That postulate is not expressive of, or dependent on, an underlying asymmetry of attitude towards nature and society of the kind he alleges. A correct, naturalistic reading of the symmetry principle implies that both 'nature' (that is, non-social nature) and society will be implicated in the formation of belief. The 'symmetry' to be insisted upon is that both types of cause, both our experience of the world of things and the world of people, will be implicated in all bodies of collective belief. Systems of belief, that is, shared and institutionalised forms of knowledge, are the medium through which people co-ordinate their shared interactions with non-social nature. Having some causally structured relationship with the material world is unavoidable, and will be compellingly present in all cultures. Adopting a symmetrical stance means acknowledging both of these dimensions in all cases. Rather than believing, as Latour thinks, that nobody has access to the real, the position would be better expressed by the slogan: all cultures are equally near to nature. This means insisting that false systems of belief engage with nature according to the same general principles as do true ones. However uncomfortable it may be, adopting the symmetry principle means that Mendelism and Lysenkoism must be seen as two different ways of causally engaging with (non-social) nature. Both will involve sensory input from the world, interactions with things and people, the application of existing cultural resources, and widely shared, though disputed, standards and goals. Contrary to Latour's presentation, the position associated with the Strong Program points to *both* Mendelism and Lysenkoism, and *both* Newton's mechanics and Einstein's mechanics, and *both* Fresnel's wave theory of light and Brewster's particle theory, and *both* Koch's germ theory and Pettenkofer's miasmic theory of disease, being engaged with nature. They all, in their time, had the character of social institutions, but that does not mean their practitioners and believers were not causally interacting with nature. In their different ways, and with different degrees of success, they were. Somehow the naturalistic and materialistic emphasis on 'both' has become transformed, in the critic's mind, into an idealistic and subjectivist 'neither'.

This approach may seem very counter-intuitive. To sharpen the issue let me concentrate on what is, for the sociologist, the least favourable of the cases listed above. If we start from the assumption that, say, Mendel's theory is true or close to the truth, while Lysenko's neo-Lamarckian approach is profoundly wrong, and probably bolstered by false data about crop yields, then how can we sustain a 'symmetrical' approach? If one corresponds to reality while the other, for various reasons, fails to correspond with it, how can they both have the same kind of relationship to it? How can they both be, as the slogan had it, equally close to reality?

We need to examine the connection that holds when something stands as a true representation of something else. What does the Strong Program have to say about

the relation of correspondence involved? The point is that, on a naturalistic approach, 'correspondence' has to be seen as a relation which actors themselves assert or impute or accept, rather than something operating as a real cause. These assertions, imputations and acceptances are effects to be explained, rather than causes which can be cited in explanations. More specifically, 'corresponding', or not 'corresponding' to reality, are not causal relationships that bodies of belief bear to their referent. Beliefs do have causal connections to things in the world, but the words 'correspond' and 'do not correspond' do not capture those connections. Neither relationship is a genuine, or naturalistically specifiable connection existing in its own right. True and false theories do not represent two natural kinds of thing, any more than a piece of land that I own, and a piece of land I do not own, constitute two, different natural kinds of land. In both cases we are dealing with what might be called a 'moral' rather than a 'causal' discrimination.

There is, of course, a causal story to be told as to *why* discriminations of truth and falsity are made in the way they are, upgrading one theory and downgrading another. In the Mendel–Lysenko case, like the others, it would be the aim of a supporter of the Strong Program to tell that story. If it involves mentioning alter-ation of the data with the intent to deceive, whether by Mendel or Lysenko, then so be it: that becomes part of the story. In general, the account would deal with the pragmatics and contingencies of belief which would, for both theories, involve the generation and processing of data, its selection and evaluation, its perceived relation to existing bodies of theory, a distribution of expectations and power, and a set of goals and purposes. All of these judgements and decisions would have to be anchored in the practices and purposes of the relevant groups. In this case the groups were, roughly, provincial plant breeders and party bosses on the one hand and, on the other, metropolitan scientists—often a generation older—based in aca-demies and universities.[4]

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[4]Given the understandable odium surrounding Lysenko and his supporters, it may seem strange to talk of their engagement with reality in the same breath as that of the geneticists who suffered at their hands. To offset this strangeness it is worth examining the report on 'The New Genetics in the Soviet Union', published under the auspices of the Imperial Bureau of Plant Breeding and Genetics (Hudson and Richens, 1949). Of course, the writers of this report could not see everything that was going on at the time, but their matter-of-fact survey and analysis of the literature can help us recover the awareness that there were genuine intellectual issues at stake behind the brutality. For example, at the time, it was unclear whether certain empirical results called into question the current understanding of genetic processes, or whether the genetic theory could be rescued by blaming the effect on viruses. There was also room for argument about the appeal to polygenes, that is, explanations which concerned traits which seemed, to the geneticist, dependent on the operation of very large numbers of genes. This was an important issue because most of the economically significant traits of concern to practical plant breeders were beyond the powers of explanation of current genetics. To its critics the appeal to poly-genes was just like the introduction of new epicycles to save the old earth-centred astronomy. It is also worth remembering that the great success of wheat growers in the United States, standing in such stark contrast to the problems in the USSR, is itself a phenomenon that needs critical analysis. Their use of double-cross hybrids is often counted as a triumph of Mendelian genetics, but (a) according to Joravsky (1970, p. 283) they were developed empirically and, at the time of writing (1970) were not understood theoretically, and (b) it was not, in any case, possible to transfer this practice to the Soviet Union. It was tried and failed because it depended on the (traditionally scorned) need for annual seed purchases,

Talk about a theory 'corresponding' to reality is simply a vocabulary for expressing the upshot of these varied and complicated processes. It conveniently encodes their outcome, but it doesn't reveal or refer to what they really consist in. For everyday practical purposes such a convention works very well. It provides the practical discrimination we want, and draws together a diversity of processes in a convenient manner. The danger comes when such talk, of 'true' and 'false', etc., is taken out of its workaday context and treated as a given in reflective, analytical or philosophical enquiries into the working of science. Then it causes trouble by encouraging simple and misleading pictures, pictures that purport to refer to the causes of the judgements that we make, when really they are the effects of those judgements.[5]

Having now dispelled, I hope, the idea that the Strong Program is part of the project of explaining nature by society, and having re-asserted its naturalistic credentials and aspirations, I am in a position to answer Latour's main charge. This is his idea that social factors are too impoverished to explain the rich detail of scientific work. In the terms in which he understands these things he is, of course, right. Unfortunately he is working under the false assumption that, according to the Strong Program, a scientist is to be thought of as responding to society rather than to (non-social) nature. We have seen this is not the claim. The working assumption (unless there is specific evidence to the contrary) is that scientists are always responding to nature, but doing so collectively through their shared conventions and institutionalised concepts. There has never been any need, or any tendency, within the Strong Program to deny the subtle and detailed character of what scientists observe, or to deny that it plays a role in prompting and sustaining belief. Indeed, it serves the purposes of the sociology of knowledge very well to acknowledge the complexity, richness and causal efficacy of sensory input. We can assume that observation will always enable us to uncover a reality which is more complicated than we can assimilate into our current conceptual schemes and theoretical systems. Experience and practical involvement with the world will endlessly generate anomaly. Nature will always have to be filtered, simplified, selectively sampled, and cleverly interpreted to bring it within our grasp. It is because complexity must be reduced to relative simplicity that different ways of representing nature are always possible. How we simplify it, how we chose to make approximations and selections, is not dictated by (non-social) nature itself. 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.

Explaining the way in which agents negotiate the relation between a complex

as well as the extensive use of weed killers and chemical fertilisers. In modern day parlance what was needed was not this (relatively) high technology of the day but 'appropriate technology'.

[5]What is needed is a pragmatist's sense of the shortcomings of the idea of 'correspondence', combined with a sociological sense of the significance of the category of 'truth' as external, authoritative and compelling. See Durkheim's lectures on 'Pragmatism and Sociology', partially translated in Wolff (1964) and fully presented in Durkheim (1983).

(non-social) nature and a heritage of past achievements is not an exercise remotely resembling the one Latour describes. No one is trying to construct Pasteur's results out of his conservatism, his Catholicism, his loyalty to the Empress and a few personality traits. Of course *that* enterprise is hopeless. It would, in any case, be a misconceived piece of individualistic thinking rather than a genuine, sociological enquiry. The sorts of question that can be asked, and to whose answer the sociologist can contribute, concern the range of interpretations that might have been put on Pasteur's observations, the way his questions were framed, and his techniques for dealing with the uncertainties and unresolved problems in his data. Why did he bring these particular interpretive resources to bear, and why did he employ them in this precise way? After all, Robert Koch didn't respond to Pasteur's data in the precise way Pasteur did, even though they were well informed, expert and rational scientists who shared many presuppositions about the causation of disease.[6]

4. The Agency of Things

Latour's charge that the Strong Program denies agency to things, because it reserves all agency and power for social processes, is therefore wrong. The Strong Program does recognise agency in naturally occurring, non-social things and processes, namely causal agency. For example, things have the power to stimulate our sense organs. Thus Pasteur and Koch could see tiny objects with characteristic shapes when they used their microscopes to examine the tissues and fluids of dead animals. Equally obviously, things impinge on us in a mixture of subtle and unsubtle ways. For example, we would probably fall ill if we injected some of this fluid into our blood stream. I do not, however, think that such references to the causal agency of things in nature would impress Latour. He would see in it nothing more than an oscillation between the two poles, the nature pole and the society pole, of the framework he has already identified and rejected. The claim made about the role of objects would, at most, shift his perception of the Strong Program from being an extreme theory to being an eclectic one which represents knowledge as made out of social and non-social ingredients.

Why not have such a theory? Latour's argument goes like this. The attribution of causal agency to things is itself an exercise of knowledge. In fact it not only constitutes an employment of the very knowledge the sociologist typically aims to explain, it also presupposes a division of reality into the categories of 'the natural' and 'the social'. Now that division is itself the very thing Latour wants to render

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[6]First, Koch thought that Pasteur's work on anthrax was just a repeat of Tiegel's earlier experiments and added nothing new. See Carter, 1988, pp. 50–52. Second, for Koch, Pasteur confused anthrax bacilli with other organisms. This was connected with their divergent opinions about the variability to be expected within a given species of micro-organism, and this in turn was connected with the different relative importance they attached to morphological and physiological characteristics when identifying microbial species. Geison attributes this difference to their different scientific backgrounds and skills. See Geison, 1974, pp. 397–399.

problematic. Any taken-for-granted use of the distinction would thus, in his opinion, be question-begging and lead to a superficial, rather than a fundamental, analysis. So, for Latour, the appeal to 'natural' causes, of the kind I have just made, is the outcome of the very process we should be studying (see e.g. Callon and Latour, 1992, pp. 347–348).

There is something right about this objection and also something deeply wrong. What is clearly right is that no causal, naturalistic explanatory program, such as the Strong Program, can proceed without making some substantial assumptions about what the world is like. In so far as it does this, its practitioners will already be making some manner of claim to possess knowledge. The problematic feature of the objection is whether or not the point at which, and the manner in which, followers of the Strong Program make their claim to knowledge is any the less justified than the way others make it—where 'others' includes the critics, such as Latour. They also need, and utilise, some manner of knowledge claim in their work, so this is not a predicament confined to followers of the Strong Program. Of one thing we can be sure: nobody can develop any position in a wholly presuppositionless way. Nobody can turn every resource into a topic without finishing up with topics which they have no resources for tackling. The difficulty is to decide which things should be topicalised for investigation and which should be reserved as resources.

What are the standards for deciding what is legitimate and prudent here? We shall see in Section 5 that the line taken by Latour is to try to construct a fundamental ontology and a set of basic philosophical categories for describing events at the level 'below' that at which social and natural realities have crystallised, that is, at the origin point of Fig. 2. The description of these events, couched in his philosophical vocabulary, is Latour's basic resource: everything else is a topic for investigation. An alternative strategy, more in keeping with the Strong Program, would be to adopt an approach loosely derived from the empiricist tradition. The sociologist needs to have a grasp of what the agents under study are responding to, that is, what aspects of the world have been disclosed to them in their experience, and what predicament they take themselves to be in. If we can isolate the 'stimulus' then perhaps we can begin the task of explaining the 'response'. Of course, the real concern will not be with individual, psychological responses as such, but with those responses as mediated by a collective understanding, with its shared traditions and conventions.

There is no question here of trying to employ the empiricist's ideal of a pure data-language because, for good sociological reasons, such a thing is not available (see Hesse, 1974). Fortunately we don't need it for the task I am recommending. It doesn't matter greatly how we specify or describe what the agent experiences, as long as we manage, somehow, to capture that experience in a way that is sufficiently neutral for our purposes. Often scientists do this for themselves when they are unclear about the identity and causal role of the objects under study. For

example in 1850 the biologist Davaine reported seeing 'small filiform bodies about twice the length of a blood corpuscle' in the blood of sheep with anthrax. He didn't, at that point, say he was looking at the cause of anthrax; it could just as well have been a symptom or a by-product as a cause (Carter, 1988, p. 43). Davaine's description isn't cast in a pure data-language, but it is neutral amongst the range of likely theoretical alternatives (e.g. cause or symptom). Or, to take another example, we might say that both Priestley and Lavoisier were familiar with a certain 'reddish powdery substance' prepared from 'mercury'. Concentrating on the thing as 'reddish' and 'powdery' should encourage us to be puzzled by Lavoisier calling it 'mercury oxide', and not just with Priestley's calling it 'red calx'. Provided the (old) principle of symmetry has a clear hold on our thinking we could then, if we wished, dispense with the discipline of empiricism and adopt a less restrained, more realist sounding vocabulary. We could just say there is as much of a problem why someone should call mercury oxide 'mercury oxide' as to why they should call it anything else. All acts of naming place the item in a system of classification, and that system itself must be sustained as a system, as something shared, a collective achievement going beyond the thoughts within any individual's head.

The important point is to separate the world from the actor's description of the world. It is the description that is the topic of enquiry, and the proposed separation is one of our resources. This is all just another way of saying we must respect the distinction between the object of knowledge and the subject of knowledge. For example, it is often better for the historian of science, or the sociologist, to avoid saying Robert Millikan 'observed electrons', or 'observed the effects of electrons'. That talk should be left to Millikan himself. It is better to say that he observed something he attributed to, and explained by, a postulated entity he called 'an electron'. In this way we might be less tempted to think that nature has an automatic tendency to generate those particular verbal descriptions or responses. If we believe, as most of us do believe, that Millikan got it basically right, it will follow that we also believe that electrons, as part of the world Millikan described, did play a causal role in making him believe in, and talk about, electrons. But then we have to remember that (on such a scenario) electrons will *also* have played their part in making sure that Millikan's contemporary and opponent, Felix Ehrenhaft, *didn't* believe in electrons. Once we realise this, then there is a sense in which the electron 'itself' drops out of the story because it is a common factor behind two different responses, and it is the cause of the difference that interests us. For this reason, we are bound to pay special attention to the 'data' rather than the 'interpretation', that is, to what Millikan and Ehrenhaft actually saw, the readings they reported and the measurements they entered into their laboratory notebooks. Cultivating an empiricist sensibility can be a useful tool. Concentrating on what can be visually or otherwise sensed sustains our awareness of the gap between objects and their descriptions. These descriptions provide the main subject matter

and problem for the sociologist of knowledge. That is what the principle of symmetry is meant to keep before our minds.

I now want to make a conjecture. My suspicion is that the critics of the subject–object distinction, such as Latour, think that giving a causal role to nature *is* tantamount to the assumption that certain descriptions of nature are to be tacitly privileged. If this were to be anyone's ground for calling into question the subject–object schema, they would be guilty of confusion. They would be inverting the truth. Only by sustaining the distinction between subject and object, and by driving a wedge between nature itself and the descriptions of it provided by the knowing subject, can we highlight the problematic character of those descriptions. It is those who don't mark the different contributions of the subject and the object who pave the way to error. They tempt us to think of the transition from the object (under a given description) to the subject's response to it (in terms of that very same description) as if it were unproblematic—because, for them, there is no real transition to be made.

The idea that there is a direct correspondence between the terms of a theory and entities answering to them in the world might be called 'direct realism' or 'naive realism'. This is the assumption that if a theorist mentions some type of entity, and if their theory can be made to work effectively, then its terms must stand in a one-to-one link to the things mentioned. If the talk is about electrons or microbes, then there must be electrons or microbes; if the talk is about caloric or lines of magnetic force, then there must be such a stuff as caloric and such things as lines of magnetic force. Critics have pointed out that something remarkably like direct or naive realism turns up in Latour's methodology (Collins and Yearley, 1992). Before looking at that, however, I want to show how it is possible to be a realist (or materialist) about nature without assuming that any particular theoretical description of it is uniquely correct. We need to remember that we can be realists without being direct or naive realists.

Our connection with nature does not depend on each general name in our theories—even our successful theories—corresponding to a natural kind. A system of knowledge is used, employed and assessed as a whole. If it works as a whole we are, of course, inclined to project the parts of the theory onto nature. When it ceases to satisfy us we regain a sense of how varied, complicated, contrived and contingent those links really are. Obviously individual terms in the theory will have individual occasions of use. We talk about *these* electrons, *these* microbes, *these* lines of force, and so on. On those occasions particular experiential episodes will prompt the application of our terms, but that doesn't mean some uniquely direct or successful reference has been achieved. The entire system of classification is implicated and, before too long, this may change. It is best to think of theoretical systems as a whole having utility and embodying an overall adaptation to reality, where reality is rich enough to permit numerous possible adaptations and numerous possible descriptions and classifications (see Barnes *et al.*, 1996).

5. The Latourian Alternative

A number of writers have already responded in a thorough and critical way to Latour's proposals (see for example Amsterdamska, 1990; Collins and Yearley, 1992; Gingras, 1995; Knorr-Cetina, 1985; Schaffer, 1991; Shapin, 1988; Sturdy, 1991; Van den Belt, 1995). Though they begin from different starting points their evaluations show a remarkable convergence. Latour's style is seen as lively and engaging but his recommendations are treated as unconvincing and his thinking is judged to be confused. Taken collectively I find that the criticisms in this literature are devastating. Some of what I have to say in this and the next section unavoidably overlaps with, and has benefitted from, points made by these critics, but I shall couch my observations in a way that develops the line of my own, overall argument.

To begin with, it has to be said that Latour never succeeds in giving a clear account of the process he calls the co-production of science and society. Indeed, such accounts as he does give are deeply obscure. Nor is the situation helped by his appeal to dark sayings from Serres: 'J'imagine, à l'origine, un tourbillon rapide.' (Latour, 1990, p. 163) (On this basis Latour sometimes embellishes the origin point of Fig. 2 with a small spiral motif.) I shall illustrate these deficiencies by Latour's own words. First recall Latour's diagram, and the vertical axis which is meant to signal the stability with which social groups mark the distinction between nature and society. Let us ask whether, according to Latour, Pasteur's microbes were really there in nature all along, waiting to be discovered, or whether they are just an idea invented by Pasteur, which caught on (a pure 'social construct')? Latour says we should not ask this question. We must neither be 'realists' about microbes, nor try to 'reduce' Pasteur's science to social conditions. Properly understood this, or some version of this, may be the right answer, but let us see what Latour takes to be its appropriate, practical expression. He says:

> I want to stress again that I am not interested here in offering a social or political explanation of Pasteur or an alternative to other cognitive or technical interpretations. I am interested only in retracing our steps back to the moment when the very distinction between content and context has not yet been made. (Latour, 1988, p. 252)

The word 'context' here obviously refers to social context, so 'content', that is, the content of knowledge, is what Latour elsewhere calls the 'object' of knowledge. What, then, confronts the analyst at the moment before the distinction between content and context, or subject and object, has been made? At this moment, it seems, we are to think of ourselves as dealing neither with microbes (which would fall under the category of content, or the object of knowledge) nor with a society responding to the microbes it knows about (this would fall under the category of context, or the society as knowing subject). We are, rather, dealing with microbes in the making and with a society in the act of making them, and in the act of making itself. At this point:

it is crucial to treat nature and society symmetrically and to suspend our belief in a distinction between natural and social actors. (Latour, 1988, p. 260)

Whereas followers of the Strong Program would recommend treating nature and society symmetrically by saying that both have causal efficacy in bringing about belief, notice that Latour's generalised 'symmetry' refers not to two *causes*, but to two *effects*. Nature and society are two effects with a common cause or, since Latour is critical of causation, two processes with a common basis. The crucial phrase, and the one that is characteristic of Latour's recommended approach, is that of suspending belief in the 'distinction between natural and social actors'. What does this mean? Well, Pasteur was a social actor and his microbes were natural actors, so we have somehow to put aside all our usual assumptions about how different they are. Similarly, Millikan and Ehrenhaft were social actors and the (presumably real) electrons of the one, and the (presumably non-existent) sub-electrons of the other, would be natural actors. Again we must 'suspend our belief' in this distinction.

Astonishing though Latour's suggestion may seem we ought not to be too ready to scoff at such a goal. After all, it is deemed philosophically respectable to argue that minds are brains and that brains are computers. Such positions may be rejected by their critics as mistaken, but they are taken seriously. Formulations of the Strong Program have also been couched in terms of a background philosophical materialism, and, for the materialist, humans such as Pasteur and Millikan are just like microbes, or any other material object, in being collections of electrons and other basic particles. At some, ultimate, metaphysical level many of us are going to find ourselves in a posture that is not too dissimilar to Latour's. The important point is how these highly general themes find an expression in methodology, for instance, in the handling of empirical material drawn from the history of science. It is here that the real oddity of Latour's position becomes significant.

Latour wants to bring these ultimate issues right into the foreground. They are not distant goals, referring to some future synthesis. Unlike the identification of brains and computers they are to be given direct expression in the analysis of the day to day conduct of science. This is why he wants the analyst to operate, not with a sociological vocabulary, but at a level at which the ordinary categories of person and thing are held in suspense. Another, more abstract, vocabulary must be brought into play. Such a vocabulary, Latour is prepared to concede, doesn't yet fully exist, but it is under construction and further work of this kind should be one of the priorities of the field (Callon and Latour, 1992, p. 354). In the meantime we can appeal to the philosophical tradition for a range of neutral and monistic concepts. Thus, instead of people, like Pasteur and Millikan, and things like microbes and electrons, we are to deal with what Latour calls 'entelechies' or 'monads' or 'quasi-objects' or 'forces'. The processes which drive everything along are to be conceived abstractly as alignments of forces, or oppositions of forces, or 'trials of strength'. Nevertheless, Latour adds the significant qualification:

> No, we do not know what forces there are, nor their balance. We do not want to reduce anything to anything else. (Latour, 1988, p. 156)

and

> In place of 'force' we may talk of 'weaknesses', 'entelechies', 'monads', or more simply 'actants' (Latour, 1988, p. 159).

The reference to Leibnizian monads seems to be meant seriously. Recall that, for Leibniz, monads are said to be 'windowless'. Latour tells us in so many words that in his own system there is no question of an 'external referent' (Latour, 1988, p. 166). Reference is always internal. Thus:

> Every entelechy makes a whole world for itself. It locates itself and all the others; it decides which forces it is composed of; it generates its own time; it designates those who will be its principle of reality. (Latour, 1988, p. 166)

What are we to say to this? The main point to be made, and made emphatically, is that it still remains wholly unclear how to connect this metaphysical talk to historical and everyday reality. It exists merely as a fantastic gloss on a body of fact, such as Pasteur's work in microbiology, that exists quite independently of it. We are told to encourage the new perspective by deliberately inverting our usual conceptual conventions, using a purposive vocabulary for things which don't have purposes, and a mechanistic vocabulary for things that do. We must try to think of Pasteur as if he were a microbe, and microbes as if they were like Pasteur, or treat Millikan as if he were an electron and electrons as if they were like Millikan. But unless we are very confident indeed that the exercise is necessary and justified, this looks like a formula for imposing confusion on ourselves: it is obscurantism raised to the level of a general methodological principle.[7]

Here we need to tread carefully, because Latour runs together general metaphysical claims with specific issues thrown up by particular historical cases. He points out that Pasteurians frequently spoke of microbes as if they were like people, talking about them as one would an 'enemy', or as an unwelcome 'guest' or as a murderer with a deadly 'mission'. As actor's categories such talk must be taken as we find it. If it turns out to be more pervasive and literal in intent than we had expected, then so be it. Much of Latour's position is derived from the specific case of the Pasteurians, and in so far as it carries any general methodological message, corresponds to the widely accepted strategy that an investigation must start from a grasp of the actor's point of view. Here, however, 'actor' means human actor, and yet it is this very distinction Latour wants to break down. On the one hand Latour will speak of microbes as literally having interests (e.g. 'It uses your interests to carry out its own', Latour, 1988, p. 33). On the other hand, we are told that he is using the idea of agent in a broad, indeed all-inclusive, 'semiotic sense'

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[7]More recently Latour has appealed to Whitehead's metaphysics as a vehicle to convey his point (Latour, 1996). I do not believe this has advanced the argument in any way.

(Latour, 1988, p. 35), which seems to be a way of telling us not to take such talk literally.

Unresolved tensions of this kind are endemic in Latour's text as he struggles, unsuccessfully, to convey what he means by the 'co-production of collective things' (Latour, 1992, p. 287). Many of the specifications he gives to the enterprise are negative: he is not, he insists, merely saying that things are 'half natural, half social'. They are 'neither objects, nor subjects, nor a mixture of the two' (Latour, 1992, p. 282). But when it comes to a positive specification we find that the language, in so far as it conveys anything, begins to slip back into the more familiar language of the sociology of knowledge. After baffling talk about 'quasi-objects', which are 'produced' and 'circulate', we hear that they 'are a new social link that redefines at once what nature is made of and what society is made of' (Latour, 1992, p. 283). Here is something we can grasp: it is a 'social link' we are dealing with. Elsewhere it appears that co-production is a process that resides in 'common practice' (Latour, 1992, p. 281), and that 'objects and subjects are belated consequences of an experimental and historical activity' (Latour, 1992, p. 284). This, again, is something we can hang on to. No doubt these terms are meant to have all manner of subtle overtones which I am here passing over, but the aim is to salvage something concrete and usable from the obscurity. Monads and entelechies having been left behind, we are left with (something like) social links, social practices, historically situated activity, and even familiar sounding cultural categories such as the 'experimental'. But if we have social links, in anything like the usual meaning of the words, we must have a society. If we have practices, in anything like the usual sense, we must presuppose a form of social life. Latour's talk about making 'nature' and making 'society', it seems, can't be taken too seriously. Really it presupposes a nature and a society all along.

Latour's attempt to get to metaphysical bedrock doesn't work: he can't get away from a pre-existing nature and a pre-existing society. He is brought back to the same starting point as the sociologist of knowledge—that boring creature who Latour berates for not getting beyond Kant's *Critique*. It seems that, after all, we have to begin our investigations into the nature of knowledge from where we are standing. Our feet are on the ground of nature, and our position is in the midst of an existing culture, our own culture. This is, perhaps, not quite as limiting as it may seem, or as Latour paints it, because we don't have to take that culture entirely at face value, or respond to it uncritically. Nevertheless, such critical distance as we do achieve can only be got by using the resources of that culture itself, using one bit of it as the basis for looking at another bit. Given that our culture is complicated and pluralistic, and equipped with a sense of its own history, and divided into opposing traditions, these resources for achieving the necessary role-distance are, I suggest, as rich as we are ever likely to need.

6. The 'New' Symmetry in Practice

So much for Latour's attempt to spell out his methodology on the theoretical plane. What does it look like in practice? The answer is that it looks suspiciously like ordinary sociology of scientific knowledge, albeit of a rather limited and one-sided kind.[8] The only difference is the addition of some obscure terminological twists, and repeated assertions to the effect that the two enterprises are disjoint and opposed. Consider, for example, his main case-study dealing with the reception of Pasteur's work. We are explicitly advised against doing what any sociologist of knowledge would do, namely, identify different groups, locate their interests, and see if their differential response to a claim might be rendered intelligible in these terms. This old-style 'reductionist' sociology is, we are told, 'obsolete' (Latour, 1988, p. 256). Latour has 'asked sociology to abandon its 'social groups' and its 'interests' and allow the actors to define themselves' (Latour, 1988, p. 51). But as reviewers of *The Pasteurization of France* were quick to point out, if we look at what Latour actually does, we find him conforming exactly to the procedure he has just denounced. He identifies social groups and their interests and depends entirely on these to tell the story of the response to Pasteur's work. He identifies the hygienists, the army doctors, the surgeons, and the physicians. He shows how Pasteurian techniques suited the purposes of the first three of these groups but not, initially, those of the physicians, who feared disruption of the doctor–patient relationship. The emphasis on preventive rather than curative techniques posed a potential threat. Hygienists on the other hand, who had been frustrated by their lack of success in predicting and preventing outbreaks of disease, found in Pasteur's techniques an effective vehicle for the pursuit of their aims. This is how Latour makes sense of their enthusiastic endorsement of Pasteurian ideas. We are told:

> The immense trust in Pasteur derived partly from the work that he had done before 1871, which did not concern infectious diseases, and partly from the social movement that needed these discoveries but went well beyond them without waiting for them to be made. (Latour, 1988, p. 30)

The social movement in question is that of the hygienists. They went beyond Pasteur by immediately responding to his work on a limited range of diseases as if it held the key to all diseases. But the central point is the reference to an identifiable social group which 'needed' these discoveries. Further, Latour says of the Pasteurians:

> Working in few laboratories, they pronounced words that were immediately regarded as truthful and were integrated into evidence that at last allowed the hygienist movement to get on with its work. (Latour, 1988, p. 34)

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[8]As Schaffer points out, 'Latour is profoundly asymmetrical as between the Pasteurians and their opponents…' (Schaffer, 1991, p. 185).

So not only do we have a need, we have a group with a conception of its work that it can see how to further. This provides us with all the ingredients for the identification of interests, and their explanatory imputation to a social group.

Perhaps Latour senses the danger of this reading, because he tries to forestall it. As well as the passage already quoted, asking sociologists to abandon their appeal to social groups and interests, he says:

> Once again, whenever I use the words 'interest' and 'interested,' I am not referring to the 'interest theory' expounded by what is now called the Edinburgh School… I am rather referring to the notion of translation… 'Interest' means simply what is placed 'in between' some actor and its achievements. (Latour, 1988, p. 260)

Despite the denial, if we check this claim by trying to substitute the words 'in between', or the general idea of in-betweenness, in the passages in which Latour uses the word 'interest' we find it does not work. For example in *The Pasteurization of France* we read:

> Either the physicians could use what was taking place in the Institut Pasteur to advance their own interests, or they could not. (Latour, 1988, p. 120)

and:

> The Pasteurians added to society a new agent, which compromised the freedom of all other agents by displacing all their interests. (Latour, 1988, p. 122)

and again:

> Now the doctors, after many other groups, by giving Pasteurism a push would also advance their own interests. (Latour, 1988, p. 127)

All of these passages make perfect sense if the word 'interest' is read in the standard way to refer to a benefit the group would gain if some course of action were pursued. The idea that it refers to something quite other than this, something having the role of coming between an action and its achievement, is not readily intelligible. Instead of admitting openly that he is, after all, in the business of giving run-of-the-mill interest explanations, Latour simply makes the same points but transposes them into another vocabulary. For example:

> If hygienists had wanted to open up a dispute, they could have done so. The absence or presence of a controversy is a measure only of the angles of movement of the actors. (Latour, 1988, pp. 52–53)

This could just as well be expressed by saying that the hygienists might have found Pasteur's claims open to argument had they been inclined to do so, and they might have been so inclined if it had served their interests. Instead of being told about the perceived coincidence of the interests of the hygienists and the inner group of Pasteurians we hear about their 'angles of movement'. I do not want to quibble over terminology, but do these metaphors really enable us to say anything deeper, different, or better than standard talk about interests? I think not.

7. Relativism

In developing his criticisms of the Strong Program, Latour seeks to distance himself from a position he calls 'relativism'. 'Relativism', as he presents it, is said to be a direct consequence of the (first) symmetry principle. Given that there are a variety of different positions that might be called 'relativism' it is important to identify exactly what Latour is denouncing. I shall follow the discussion in *Science in Action*, though this is entirely representative of his treatment elsewhere. First, we must notice that Latour follows the widespread trend of treating 'relativism' as a contrast to 'realism' (rather than, as should be the case, as a contrast to absolutism). Thus realists are said to believe that scientific controversies about how best to represent nature are settled by nature itself, while relativists are said to believe that 'Nature will be the consequence of the settlement' (Latour, 1987, p. 99). (Notice that Latour says 'nature' not 'beliefs about nature'.) Second, 'relativism' is taken to be an evaluative position. Relativists are said to be committed to defending bodies of belief against various 'charges', such as the charge of irrationality. Their aim, allegedly, will be to convince us that such a negative evaluation is unfounded or impossible to sustain, and that the body of belief in question can be defended on the grounds that it is really rational after all. Latour draws a legal parallel. Relativists, he says, are like defence lawyers, arguing for the innocence of their client. Whenever the scientific community rejects a theory, as they rejected phlogiston or caloric or Newtonian mechanics with its absolute space and time, the relativist must make the case for the defence—in the teeth of the scientific consensus.

> Every time a charge of irrationality is filed, relativists argue that it is only an appearance that depends on the jury's relative *point of view*—hence their name—and they offer a new perspective from which the reasoning appears straightforward. Their position is called symmetric… (Latour, 1987, p. 195)

Latour finds this position indefensible because, by their commitment to the 'symmetrical' idea that all opinions are equally worthy of credit, 'relativists' ignore the obvious fact that scientists themselves work hard to establish asymmetry, that is, to make some theories more credible than others. Thus he complains:

> for four chapters we have followed scientists at work who strive to make their claims *more credible* than those of others. So if this enormous work makes no difference they have wasted their time, I have wasted my time, the readers have wasted their time. (Latour, 1987, p. 196)

The work that goes into achieving differential credibility takes the form of establishing alliances, connections, and the enrolment of others, that is, in the creation of what Latour calls 'networks'. These include all facets of the situation, material and social. Unfortunately, says Latour, these networks are neglected by relativists:

> But in the symmetric stand it is the very existence of the scientific network, of its resources, of its ability to sometimes tip the balance of forces, that is utterly ignored. (Latour, 1987, p. 196)

It would be difficult to imagine a more serious charge against the relativist sociology of knowledge of the Strong Program than that it 'utterly ignored' the phenomenon of interaction that goes into forming social networks, or that it ignores the possible role of sensory input in 'tipping the balance'. The charge is, however, wholly misconceived. The 'relativism' Latour rejects is quite distinct from the relativism of the program he takes himself to be attacking.

First, the relativism of the Strong Program is not to be counterpoised to realism. As I have emphasised, (non-social) nature plays a central role in the formation of belief, though how nature is experienced cannot provide a sufficient causal explanation of how it is subsequently described. Second, Strong Program relativists are not like lawyers trying to make out a case for innocence. If we go along with the legal comparison they would be better likened to philosophers of law who argue that there are no absolute standards of justice, or no absolute 'rights' against which legislation may be assessed. This is a quite different image.

The point of the symmetry postulate is to enjoin sociologists to draw back from making first-order judgements. The point is to make such judgements the objects of enquiry. It is precisely judgements of this kind which are to be explained. Such a position is 'relativist' because there are no absolute proofs to be had that one scientific theory is superior to another: there are only locally credible reasons. Of course the phenomenon of differential credibility is real. The aim of a relativist sociology of knowledge is not to ignore or deny such variation, but to explain it. Latour's idea that Strong Program symmetry means saying that all beliefs are equally credible is wrong. The claim is that all theories and beliefs equally face the problem of credibility, and hence that all differences in, and degrees of, credibility are equally in need of causal explanation.

Even if it is accepted that symmetry doesn't imply equal credibility, isn't there still something right about Latour's characterisation of relativism? Suppose a sociologist of knowledge were to examine, say, Robert Koch's early claim to have identified the bacillus which causes anthrax. At the time Koch's critics said he had not strictly proven that it was the bacillus alone, rather than some constant concomitant of the bacillus, which is the cause. Koch responded by saying it is impossible to provide complete and total proof, and therefore meaningless to ask for it. Later, in his work on tuberculosis, he endorsed, and claimed to have satisfied, what later came to be called 'Koch's postulates' for establishing causality. These seem to demand exactly what he earlier denied: the complete isolation of a bacillus so that it, and it alone, can be known to be the cause of a disease. Has Koch contradicted himself? Was his earlier claim logically at odds with his mature methodology? Now, a sociologist of knowledge may well be reluctant to make such judgements. This looks as if it fits Latour's stereotype that, for the relativist, everybody is innocent of any scientific crime of which they may be charged, in this case, the charge of logical inconsistency.

In fact something quite different is going on. The aim is not to substitute alterna-

tive evaluations (innocent rather than guilty, consistent rather than inconsistent) but to avoid evaluations being used as substitutes for more searching enquiries. The point a sociologist of knowledge would insist on is that Koch's postulates for identifying causes must always be applied within a context, that is, against some background which is being taken for granted. Like all rules and principles, they do not have any intrinsic basis for their application but always, and necessarily, depend on local contingencies. Without such a background they would yield no determinate answers. The later use of Koch's postulates, and the implicit admission that they did not make impossible demands, took place once the germ theory had become the effective paradigm for research in the area. The earlier claim, that they were impossible to satisfy with complete rigour, was made in the context of arguments with critics, some of whom did not yet fully accept the germ theory. In this context nothing need have counted as satisfying them. The critic could always say that it was *possible* that a concomitant cause had been overlooked. In such a context Koch's postulates always yield the result that no cause has been isolated. It is only in the context of the germ theory as an institution that the postulates discriminate between what counts as a rigorous proof and what, by comparison, must be dismissed as sloppy reasoning.[9]

It may well be reasonable to suspect that Koch was inconsistent, or that he changed his mind without admitting it. The important point for the relativist is that such an evaluation can only be made once we have established the background against which the content of his thoughts can be identified. The aim therefore is not, as Latour thinks, to make Koch out as 'innocent'. There is no such commitment or preconception. The point is to insist that the logical content of an argument, a claim, or a belief—that is, the preconditions of both consistency and inconsistency—cannot even be properly brought into view, let alone assessed, until they are relativised to their social context.[10]

8. Similarities and Differences

Much of Latour's 'new' perspective simply shadows things that have already been argued by sociologists of knowledge. For example, Latour insists that he wants to let actors 'define themselves'. On at least some of its possible interpret-

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[9]Significantly, once the germ theory of disease had become taken for granted the practical level of proof that was felt to be acceptable fell distinctly short of the satisfaction of Koch's postulates (see Evans, 1987). The extent to which they need to be satisfied, and what actually counts as their satisfaction, is still under dispute as can be seen from contemporary disputes over AIDS (e.g. Cohen, 1994). I should like to thank Henk van den Belt for drawing my attention to these references.

[10]I have based this example on two papers by K. Codell Carter, 'The Koch–Pasteur Debate on Establishing the Cause of Anthrax' (Carter, 1988) and 'Koch's Postulates in Relation to the Work of Jacob Henle and Edwin Klebs' (Carter, 1985). Since I am using the material to make a general point about methodology I have not, of course, tried to reproduce the historical detail. A reader of these fascinating papers will find that the more detail is brought in, the stronger the methodological point becomes.

ations sociologists of knowledge have long subscribed to this principle. The Strong Program can be said to have adopted this approach by discouraging the analyst from dividing agents into two, evaluative categories, namely those who subscribe to what we take to be true beliefs on some subject matter, and those who don't. Such a division would indeed be a case of *not* letting the actors 'define themselves', because we would be imposing our evaluation of their situation on them. But that, of course, is exactly what the symmetry principle forbids.

Again, Latour doesn't want to assume 'that interests are stable or that groups can be endowed with explicit goals' (Latour, 1988, p. 260). Fine, but no sociologists of knowledge are obliged to assume such a thing. Whether interests are stable or unstable, and whether or not goals are made explicit, are matters for empirical investigation—just as they are for Latour himself. Though he rightly doesn't *assume* that interests are stable, he certainly finds that some of them are, as we can see from what he says about the hygienists (see for example the comment about 'several generations', Latour, 1988, p. 62). Similarly, Latour doesn't want to say that Pasteur's interests 'fitted' those of the hygienists, 'but that there was room for a *negotiation* about the meaning of contagion' (Latour, 1988, p. 255). Is there any problem here for the sociologist? Are we to believe that sociologists are not at home with the idea of negotiation? Is the only available account of interests one in which the idea of negotiation can find no place? Clearly not (see Shapin, 1982). This is simply a hostile characterisation of the sociological enterprise designed to heighten the impression that it is distinct from Latour's approach.

Latour's recommendations don't always simply shadow those of sociologists. One respect in which they differ concerns the relationship between the analyst and established bodies of scientific knowledge. Here is the problem. The sociologist is committed to identifying the conventional aspects of knowledge. If something is conventional then, in principle, there must be a viable alternative. Driving on a certain side of the road is conventional because we could, in principle, drive on the other side. Given that we drive on one side, a change in convention might be costly and impractical, but that doesn't destroy the conventional character of the practice. Demonstrating a conventional component in knowledge means showing that understanding could have taken another route without overriding the normal, biological functioning of the human brain. At any given time such an alternative might be costly, but that is not the point. The point is that it must be rationally possible. This can present a difficulty when dealing with up-to-the-minute, and highly esoteric, knowledge of a kind which currently commands a consensus amongst the experts. Alternatives are not going to be easy to come by. To produce them would involve beating the experts at their own game. By its very nature this may be a practical impossibility. Any alternative that might be suggested is likely to be dismissed by the experts, on convincing grounds, as inadequate or erroneous. There will then be no known alternative, except error, so it will be impossible to exhibit the conventional character of the knowledge. Under these circumstances it

will be tempting to feel that the sociological approach has met its limits, not just its practical limits, but its theoretical and logical limits. The expert knowledge under study, we might feel, cannot be dependent in any significant way on convention. It must correspond directly to reality because the facts of the case give us no alternative.

Under these circumstances a supporter of the Strong Program should stand firm and go back to the basic principles of relativism. It could well be that a body of knowledge depends significantly on convention without our currently being in a position to demonstrate it directly in the particular case. The limitation could be entirely contingent, for example: lack of ingenuity and creative imagination in the relevant field. It is then necessary to depend on inductive arguments drawn from historical cases. Given historical distance, it is easier to show that there are alternative ways of understanding the data. Copernicus shows us how Ptolemy's data could have been understood otherwise, Lavoisier shows us the alternative to Priestley, Einstein the alternative to Newton, Cauchy and Weierstrass show us the alternative to the infinitesimals of Leibniz, and Robinson shows why there might be infinitesimals after all, and so on. The inductive generalisation from such cases to the consensus of today shows that it too will have alternatives—unless someone can produce remarkable and cogent reasons for thinking that qualitative changes have suddenly taken place in the nature of knowledge. It is no use a critic of the sociology of knowledge pointing to the success of current knowledge. That doesn't represent a qualitative change or a proof that conventionality has suddenly been transcended: accepted knowledge always works, it always has its successes, until we find something that seems to work better.

Latour's response to the problems of analysing authoritative and current bodies of knowledge is quite different from that sketched above. The sociological demonstration of conventionality requires that, in a certain sense, the analyst may need to know more than the social actors themselves—in the way in which historians need to know more than the historical actors they describe. And, of course, this may be unattainable in practice. Latour, by contrast, says the 'analyst does not need to know more' (Latour, 1988, p. 10). His view is that an analyst should only adopt a 'relativist' stance during periods of scientific conflict or indecision. During times of scientific consensus analysts should comport themselves as 'realists'. This is because Latour's aim is merely to travel along with the scientists, to follow them around and describe their opinions and attitudes. Thus:

> We do not try to undermine the solidity of the accepted parts of science. We are realists as much as the people we travel with… But as soon as a controversy starts we become as relativist as our informants. (Latour, 1987, p. 100)

When in the realist mode the analyst is enjoined to take nature as the cause of accurate descriptions of herself.

> We cannot be more relativist than scientists about these parts and keep on denying evidence where no one else does. Why? Because the cost of dispute is too high for

an average citizen, even if he or she is a historian and sociologist of science. If there is no controversy among scientists as to the status of facts, then it is useless to go on talking about interpretation, representation, a biased or distorted world-view… Nature talks straight, facts are facts. Full stop. There is nothing to add and nothing to subtract. (Latour, 1987, p. 100)

The issue is not however, as Latour presents it, one of trying to 'undermine' the solidity of science. Demonstrating, say, the underdetermination of theory by experience, and the negotiability of scientific concepts and conclusions is not, in any real way, to undermine science. It might undermine a range of theories about what its solidity consists in, but that is quite another matter. Nor is the issue one of 'denying the evidence'. While nature may appear to talk straight to the believer, that appearance is false, and it is just as false during periods of stability as during periods of instability. Direct realism may describe how things seem to the believer—the believer's phenomenology—but if we are to understand that phenomenology, and its variations, we cannot just endorse the agent's own perception of things.[11] Latour notwithstanding, there *is* always something to add to what scientists say in the conduct of their professional roles. That 'something' is a general model of knowledge. It is simply untrue to say that sociologists can't be more relativist than the scientists under study. They certainly can be, and not by denying evidence, but by taking into account more evidence, namely the evidence from the history of science, which points to the possibility of alternative understandings. What is true is that sociologists may not be able to exhibit these alternatives in certain particular cases, but that is a practical limitation, not the decisive factor in a discussion of methodological principle.

9. The Subject–Object Schema as Topic and Resource

Latour introduces the subject–object schema in its Kantian version and treats followers of the Strong Program as if they are little more then unreconstructed Kantians (Latour, 1992, p. 278). Durkheim's powerful naturalistic and sociological re-working of Kantian themes is simply brushed aside as offering nothing new (Latour, 1992, p. 277). The naturalistic reading of the subject–object schema, however, deserves better than this, as I now want to show. To begin the discussion, it is easiest to think (naturalistically) about the subject–object relation in individualistic terms. Consider an organism learning about its environment by causally interacting with it. This process involves both a degree of separation and disengagement from the environment, and a degree of connection and engagement with it. It is

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[11]Perhaps this is what Latour means by saying that he, unlike advocates of the Strong Program, allows social actors to 'define themselves': if the actors think something is true then the analyst is committed to agreeing. If this is the meaning, or part of the meaning of the slogan, then it does indeed conflict with the Strong Program. I should point out, however, that it seems to go far beyond allowing the actors to define themselves in that it allows them, in addition, to 'define the analyst', something for which I can see no good grounds.

an active process in which one part of nature (the subject) interacts with another part (the object). These fundamental, individual causal and biological processes do not, of course, derive from culture but are, rather, presupposed by culture. Again, the basis of the subject–object distinction may be approached through the model-building of cognitive science. Thinking about how a computer might learn to inter-act with a simple environment of moveable and shaped blocks would be one way of bringing the basic and pre-given relation of subject and object into view. In these ways the subject–object schema can become not only a resource, but also a topic of enquiry.

As we have seen, Latour also wants to address this very basic process of emerg-ence. This is the significance of the origin-point on his diagram. His approach, which stands in sharp contrast to those just sketched, rests on two errors. First, he rejects naturalism because he thinks it does not deal with fundamentals. We have seen the result in his recourse to the obscure terminology of monads, entelechies and the like. This should be sufficient to demonstrate the dire consequences of deserting naturalism. Second, Latour doesn't distinguish sufficiently between the historical emergence of certain (culturally determined) ways of understanding the subject–object schema and the biological and causal phenomenon that constitutes the natural ground of the schema. To support this charge I would cite his question-able use of Shapin and Schaffer's book *Leviathan and the Air-Pump* (1985). These authors give a detailed, historical analysis of certain crucial episodes in the emerg-ence of the modern scientific world-view. They describe the beginnings of a cos-mology and a social form in which a certain conception of the knowing subject, and a certain conception of the object and method of scientific knowledge, were forged. Latour, however, treats it as an account of the emergence of the subject–object schema itself, rather than an account of one of the historical and cultural forms that our understanding of it, as a biological given, has assumed. Shapin and Schaffer's work, Latour tells us, is a contribution to the new (Latour-style) 'anthropology' of knowledge, and illuminates the 'historical origin of this philo-sophical asymmetry between the two poles', that is, the historical origin of the subject–object schema itself (Latour, 1992, p. 279). In fact it does no such thing—but then, it was not meant to.[12]

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[12]In his fuller treatment of Shapin and Schaffer's book, Latour presents it as a contribution to (his conception of) 'anthropology', but sees it as a truncated and 'interrupted' attempt to do what others—he mentions Michel Serres—do better (Latour, 1990, p. 160). Their failure to 'dig much deeper' is said to derive from the authors' remaining attachment to 'the Edinburgh school's contention that there is a macro Society "out there" more sturdy and robust than Nature' (Latour, 1990, p. 158). No reference is given as to where this 'contention' is to be found. Presumably Latour sees it as implicit in everything that is said, or not said. Thus he claims that Shapin and Schaffer, like everyone else who adopts a sociological approach, 'happily use the words "power", "interest", "politics"' (p. 159) but treat them, uncritically, as given. Why don't they deconstruct power? No one, says Latour, 'has yet deconstructed his [i.e. Hobbes'] vocabulary of power, society, group, calculation of interests and sovereignty' (p. 159). Latour's claims here are false. It is simply untrue that there is any contention, claim, commitment to, or practice of, treating macro-society as more robust than Nature. Whilst some references to the social backdrop or starting point of an episode, say a scientific dispute, may have a taken-for-granted

I am making a distinction between, on the one hand, the underlying biology and mechanics of individual cognition and, on the other hand, the shared, cultural understanding of those processes, the kind of understanding described so vividly by Shapin and Schaffer. Is it legitimate to invoke such a distinction? Surely, a critic may say, the disciplines of biology and computer modelling, whose standpoint is being invoked, are themselves cultural products. If so, am I not begging all manner of questions by using them as resources when I should really be treating them as topics of enquiry? Despite the appearance of question begging and circularity I want to defend such a procedure as a legitimate part of a naturalistic enquiry into knowledge, and as a necessary background for more specifically sociological studies. The point is that 'circularity', of this kind, 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. Bringing cultural resources into play in order to understand culture is just as evident in Latour's own work as it is in that which he criticises. Leibniz's philosophy of monads, which is one of Latour's own resources, was itself a cultural product, and very much a child of its time.[13]

The theme of 'circularity', and the reflexive character of cultural self-understanding, brings us back to the subject–object schema. Earlier I referred to the limitations of the schema. If there is any single phenomenon, other than nature taken as a whole, which might instruct us in what it could mean to transcend the subject–object schema, it is the workings of society itself. By reflecting on social processes, that is, by making them an object of enquiry, we discover that there are circumstances in which the distinction between subject and object disappears. It is not those such as Latour, who evince a generalised suspicion towards the schema as such, who have identified and illuminated these processes, but a writer widely associated with the Strong Program. In his paper 'Social Life as Bootstrapped Induction' (1983) and his book *The Nature of Power* (1988) Barry Barnes has given an analysis of predicates which refer to social statuses, roles and institutions in terms of their self-referring character. The basic idea can be stated using a single, simplified example of a social institution, namely money. We can say, for example,

character, there is no methodological commitment of the kind Latour purports to identify. Indeed the work of the so-called Edinburgh school has exactly the opposite character to that imputed to it by Latour. Members of the 'school' have done exactly what Latour says they have not done, namely 'deconstructed' concepts like power. Barry Barnes' book *The Nature of Power* (Barnes, 1988) was available two years before Latour published this claim and has exactly the required character. He doesn't take the concept of power for granted, and then trace its various forms and manifestations, he probes into the very nature of power itself. Even earlier work by Barnes on the nature of institutions had been in print since 1983 and similarly proves the falsity of the claim that social categories are treated as unproblematic resources. In fact, if there is one feature of the 'school's' work that deserves notice, it is that it possesses the very characteristic that Latour wants to deny to it: it treats society and knowledge as having one and the same nature, of both being ultimately the same thing. I shall say more about the significance of these ideas towards the end of my discussion.

[13]The circularity would be destructive if justificationalist accounts of knowledge were correct. I am assuming they are not. In other words, anyone who accepted either a form of Popperian fallibilism, or a Wittgensteinian account of knowledge as grounded in unjustifiable 'language-games', or some form of pragmatism, could accept the procedure I am adopting.

that the predicate 'money' refers to something which is rightly called money because sufficient numbers of other people call it money. The reference of the predicate is a reality which is constituted by the social practice of making references to it. The example is grossly simplified because, of course, there is more at issue here than verbal behaviour. I am letting 'calling' and 'referring' stand in for all forms of intentional action. With this proviso we can say that the talk (about money) and the thing talked about (the money 'itself') are one and the same. The object of reference and the acts of making these references (i.e. the acts of these who know the object) are the same. When taken collectively, the subject and the object of discourse collapse into one. What applies to the institution of money applies to all social institutions: they are self-referring and self-constituting.

The interesting theoretical task is to combine this model of a social institution with the sociological insight that *all* knowledge has the character of a social institution. And this includes, of course, knowledge of an independent reality, with an independent object. The task is to combine two processes, one constituted through self-reference and the other involving external reference. Barnes has led the way by reminding us of numerous familiar examples of objects whose identity is given by the uses to which they are put. Tables and chairs and cups and saucers, as well as fertilisers, explosives, vaccines and dyes are all real and external things, but things whose identity is defined by their role in the life of a group who create that identity through their practices. Barnes' suggestion is that these cases can be used as a model for objects whose function in our lives is more subtle, like electrons and microbes, and he points us to Kuhn and Wittgenstein as thinkers whose work gives expression to this idea.[14] The link between self-reference and external reference is that the latter presupposes the former. It is only by collectively sustaining a set of concepts that genuine and coherent reference to an external reality becomes possible. To sustain conceptual content there must be normative principles governing the application of the concept. In other words, there must be rules, and rules, as Wittgenstein argued, are social institutions (see Bloor, 1997).

Latour is aware of Barnes' book on *The Nature of Power* but fails to see anything of potential interest in it (Callon and Latour, 1992, p. 360). He treats it as an analysis of social life which leaves out the role of things and objects in nature, as if it were another expression of the standpoint defined by the extreme subjectivist end of the subject–object schema. What I have defined as an interesting theoretical task—combining self-referential processes with external reference—would no doubt be seen by Latour as just another attempt to analyse knowledge by mixing the ingredients associated with the subject with those derived from the object. For Latour it would be proof that the argument is still circling around within the same old framework of assumptions.

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[14]For an exploration of these themes in connection with Wittgenstein's work, see Bloor, 1996. The central point is that, though this account of an institution looks as if it might lead to an idealist account of knowledge, properly developed, it is entirely consistent with a materialist approach.

The correct response is that adherents of the Strong Program do indeed still work within the framework of the subject and object schema. Their argument, however, is not, and never has been, confined within the framework set out by Latour, where so much influence from society is always purchased at the expense of so much influence from non-social reality. Latour has confused two targets: the subject–object schema and the game of assigning the proportion of influence exerted by nature and by society. He runs them together when, really, they are separable and typically separate. Latour's conception of the Strong Program as involving a zero-sum game between 'society' and 'nature' is wrong. There are, indeed, two such components or factors in knowledge, but they are not linked in the zero-sum fashion that Latour presents. They do not trade off against one another in the manner of Fig. 1. There are not different degrees of dependence of knowledge on society, with the quality of knowledge improving as the social component gets smaller. All knowledge always depends on society. This is because, as I have argued and as case-studies demonstrate, society is the necessary vehicle for sustaining a coherent cognitive relation to the world, especially a relation of the kind we take for granted in our science.[15]

10. Conclusion

Latour has endorsed, and encouraged, a wholly false stereotype of the Strong Program.[16] He has joined those who insist on reading it as a species of idealism rather than, as it really is, as a species of materialism. He belongs to the ranks of those, like the recent contributors to *The Flight from Science and Reason* (Gross *et al.*, 1996) who have only to see the label 'sociology of knowledge' to conclude that its doctrines must imply the absurdity that knowledge is 'purely social'. Thus we hear from Latour that, according to sociologists and Strong Programmers,

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[15]The only sociologist I know whose position arguably fits Latour's diagram is Stephen Cole in his book *Making Science* (Cole, 1992, see p. x). The sub-title is, appropriately, 'Between Nature and Society' which nicely captures the polarity pictured in Latour's horizontal axis. It is worth noting that Cole's conception of the Strong Program is identical to that of Latour's. They both subscribe to the same mistake. I have critically discussed Cole's methodological position in Bloor (1999).

[16]One way in which Latour casts doubt on the sociological enterprise is to take an example of work with clear limitations or weaknesses, treat it as representative or, indeed, exemplary, and blame the enterprise rather than the individual practitioner. For instance, he criticises Traweek's book *Beam Times and Life Times* heavily (Traweek, 1988). 'I have presented an account of how high energy physicists construct their world', says Traweek (p. 162) and claims to have illustrated the famous formula of Durkheim and Mauss that the classification of things reproduces the classification of persons (though her formulation of Durkheim's idea, on p. 157, looks in danger of getting the arrow of causation in the wrong direction). The latter, Durkheimian, claim makes it clear that the 'world' spoken of here is not merely the 'social world' of the scientists but the world of nature as represented in their theories. Latour says, harshly but correctly, 'in spite of her claim to 'thick description', Traweek is unable to relate the content of physics to the social organization' (Latour, 1990, p. 167). This is true, if only because there is no detailed description of the content of the physicist's knowledge in the book. Latour takes this failure to be damning to the sociology of knowledge or to anthropology as it is normally conceived. I fear the explanation is much simpler: the writer in question was, unfortunately, making somewhat inflated claims for her data and the significance of her findings.

'objects count for nothing' (Latour, 1993, p. 53) and that science merely generates 'arbitrary constructions determined by the interests and requirements of a *sui generis* society' (Latour, 1993, pp. 54–55). Despite everything that has been said to the contrary he also persists in associating the sociology of knowledge with 'muckraking' (Latour, 1983, p. 157) and the attempt to 'debunk' science (Latour, 1993, p. 54). For the past twenty years sociologists of knowledge have patiently tried to explain the misunderstanding behind such ideas and show their inapplicability to the enterprise as it is currently practised. Clearly their efforts have been wasted for here, once again, we find all the old mistakes back in circulation. Such errors might be expected from ill informed critics from outside the field; that they should be shared and sustained by the writings of one so central to it, is incomprehensible.

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