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DISCUSSION

For David Bloor… and Beyond: A Reply to David Bloor's 'Anti-Latour'

*Bruno Latour**

First, I want to thank David Bloor for his thorough and honest reading of my work. With his usual rigor, he has summarized in his crisp and analytical style many of the difficult points I have encountered in moving away from the tradition he has done so much to create.

The result is a devastating critique: when it is good, the sociology done at CSI is nothing more than traditional SSK couched in a fancy vocabulary; when we tried to amend the tradition, we succeeded only in adding obscurity and falling back into naive realism; finally, my positions are morally, ethically and—although David does not say it in so many words—politically reactionary, since it offers no way to protect the public against the claims of the winners, who pretend that their interpretation of nature goes beyond convention. To sum up, what I have done 'is obscurantism raised to the level of a general methodological principle' (p. 97). In addition, I am accused of not having understood SSK at all, and having provided fodder for the worst enemies of science studies by caricaturing the Edinburgh School position: '[Latour] belongs to the ranks of those, like the recent contributors to *The Flight from Science and Reason*' (p. 107). Apart from this last sentence which is uselessly polemical—even though it could help my standing in the Science Wars!—I left the article (and so did our doctoral students!) with the clear feeling that our brand of sociology of science was done for. The CSI might as well be closed down.

I think that David is right in everything he says, and that, from his point of view, there is no other way to see my work and that of my many colleagues who

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have, more or less quickly, abandoned the first attempts of the Edinburgh School at understanding science. The problem, of course, is that David's point of view is not the right one to evaluate our work from. What he sees as the main sources of obscurity, are the source, for all of us, of our main claim to analytical clarity.

It is because I believe that the difference in methodology is sharp enough to highlight what has been collectively achieved over the last quarter of the century, and what remains to be done during the next one, that I went to the effort of answering David's charges. After all, in this time of Science Wars, this clarification might be useful, since I have always warned my SSK colleagues that their obstinacy in sticking to traditional sociology would deliver us straight in the teeth of the Science Warriors—which is exactly what has happened. I am happy to seize the occasion given to me to illustrate how little consensus there is in this field, branded 'anti-science'. A scientist friend of mine, wary at first of science studies, sighed in relief when he realized that we were all disagreeing with one another: 'So, you are like the rest of us!', he said. Quite.

1. A Few Asymmetries between the Two Debaters

Before getting into the heart of the matter, I want to underline a few asymmetries between the two debaters.

The first one is that I feel entirely in David's debt. His symmetry principle, his Strong Program, allowed me and many colleagues in France to escape from the utter domination of the French epistemologists who had carried out, until then, a thoroughly whiggish history of science, that had made it impossible for them to profit from the new Anglo-American history of science, then in full bloom. As a tool to unlock the situation created by French epistemologists, who had made a career of treating true knowledge and false belief *differentially*, David's position is still, in my view, unmatched.[1]

The second asymmetry, very much linked to this one, is that I have translated, published, taught, discussed and defended for ten years the contributions of SSK. I know them inside out and three thick books have appeared in French out of this work through Michel Callon's and my mediation.[2] David, on the other hand, has only recently learned of our existence and had no time, it seems, to apply the strange principles that we have developed to any concrete and empirical situations—except second-hand examples from history concerning Pasteur, a topic about which I am not altogether ignorant either. If Michel Callon and I have been

[1]One look at the work of Gaston Bachelard is enough to see all the good that the Strong Program could do for the practice of history of science. See a recent paper by Pestre (1995) on the difficulties the French had with this principle. Although the situation is much better now, in part because of Pestre and Licoppe's works, it is still a very touchy issue; as the recent furore over the 'Sokal affair' in Paris has demonstrated vividly enough.

[2]We translated David's book in 1982 (Bloor, 1982), then published Callon and Bruno (1982); Callon and Latour (1985), in part republished as Callon and Latour (1991).

led to abandon SSK it is not on a whim, but for powerful reasons that quickly appeared once we had spent several years of studying the daily practice of scientists, engineers and politicians. When David uses the word 'empirical', I am tempted to think that it does not carry exactly the same meaning for both of us—more of this later.

The third asymmetry is more damning for me. I have continuously changed my topics, my field sites, my style, my concepts and my vocabulary—and indulged in too many Macintosh doodles! David has not moved an inch and his paper reiterates, word for word, what was already so forcefully set out in his first book. On the one hand, this simplifies my task, since I can take his article against me as perfectly representative of his thought, but, on the other hand, it renders my defense more difficult since I would be at great pains to say which paper, chapter or book is representative of my position. I would be tempted to say that the only sources to quote and to dispute are the articles or books I am *presently* working on, but that would be, I agree, a poor answer. Compared to David, I feel very much like a fly dancing ceaselessy on top of the Edinburgh Rock. That David has difficulty understanding what a gradient going from instability to stability might mean (p. 85) is no surprise to me. Empirical work has this unsettling quality of forcing you to move heaven and earth in order to try to follow what happens in practice. It makes me a moving target.

The fourth and last asymmetry is that if I am the only one attacked in David's article, I have always seen myself as speaking for many other lines of work. If one can read clearly in Bloor and Barnes's work a digest of the Edinburgh School, the work I am feeding on, and that I have now to defend, would involve visiting most of science and technology studies. Andrew Pickering for instance could be attacked very much for the same reason, and his 'mangle of practice'[3] is as far from SSK as Michel Callon's actor network or Isabelle Stenger's cosmopolitics, or Leigh Star's symbolic interactionism. And what should be done with ethnomethodology, so radically different from the Durkheimian framework that David identifies with the whole of sociology? I profited as much from Michael Lynch's principles of methods as from David's, not to mention the new waves of science studies developed by Anne-Marie Mol and John Law.[4] But David is not for waves nor for extending the philosophical tradition much beyond Wittgenstein, nor for changing sociology thoroughly.

This situation renders the discussion unequal. On the one hand there is a small body of canonical works feeding on a very tiny corpus of books—Durkheim and Mary Douglas—on the other there is a proliferation of works, a philosophical tra-

[3]David could have compared the changes from Pickering (1984) to Pickering (1995), with the moves I have had to make: he would have seen that they were parallel in many respects.

[4]I have always emphasized close connections between philosophy (Stengers, 1997), new changes in sociology such as those of Lynch (1985), and changes in the definition of the body, such as those initiated by Haraway (1991) and Mol and Law (1994).

dition that covers everything from Plato to Donna Haraway, and sociological traditions that mix up all of the schools from Auguste Comte to Anthony Giddens. When David writes that 'there is no further turn to be taken' and that 'Latour's ideas do not represent the way forward. If anything they are a step backwards', (p. 82) an image of a deep and entangled thicket of many varied works comes to my mind, and not that of an orthodox SSK that I have abandoned by taking the wrong turn. In any case, the situation is not the one complacently portrayed on p. 95 of a unanimous crowd of critics against one outsider—not to say a traitor! The truth is that, after so many turns and twists, 'science studies' has for ten years now escaped the narrow confines in which David wants to keep it. The Strong Program was useful and still is against the few remaining epistemologists. It has become an obstacle for the continuation of science studies.

My next task is to show why we had all good reason to put it safely to rest.

2. The Accusation of Having Distorted SSK

The most serious charge against me—I will speak in the first person since David chose me as his target, but of course none of the views I defend are mine only—is that I have distorted Edinburgh's tenets and treated them as unfairly as its worst critics. I can pass rather quickly over this point since David, with great honesty, reiterates even more forcefully the very points I have criticized in his position.

The first one is the role of the object in an Edinburgh type of explanation. I have never said that Bloor was an idealist but that his position was an elaboration on that of Kant with the only difference—due to Durkheim's emendation—being that the Ego had been replaced by a *sui generis* society—see the next point. Now to escape this charge, it is not enough to say that objects play the role of anchor in our beliefs about the world, as in the famous parallelogram of force that David used in his 1976 book and that, incredibly enough, he seems to still believe in.[5] Admirable consequence of stability! The question is to know *which role* objects play.

What sort of agency do objects have in making us modify our beliefs about them? I quote David:

> 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. (p. 90)
>
> For example things have the power to *stimulate* our sense organs… (p. 90) …things *impinge* on us in a mixture of subtle and unsubtle ways… (ibid.) (Italics mine)

[5]It is shown on p. 32 of the new edition (Bloor, 1991 [1976]). In his paper David criticizes me for not understanding that science is not a zero-sum game. My knowledge of physics might be small, but a parallelogram of force can be read in one fashion only: if *no* force was coming from the axis labelled 'prior belief', then the resultant would be *equal* to that of the axis labelled 'experience', and *vice versa*. This is what I call a zero-sum game.

They are also able to create 'anomalies' in our frames of interpretation, etc. If you make a list of all the roles that things or sensory inputs play in SSK's narratives, you will be struck by the fact that they don't do very much. Exactly as in Kant, and for the very same reason, things in themselves are there to make sure that one is *not* an idealist, to fill the phenomena—the meeting point of our categories and the noumens—with some sort of resistance, some sort of stuffing. They are like hosts at a party where all the food has been brought by the mind (alternatively, read society) to stand up as tokens, but they are not there to eat and certainly not to bring their own doggy bags.

If I wanted to make the most charitable reading of David's position, I would say: 'Okay let's suppose that this is enough to account for the 'coercions' the world exerts on us—to use William James' expression (James, 1907 [1975], p. 112); it's not much, but it's better than nothing, as with the card-carrying idealists.' The next question is this: are these objects allowed to *make a difference* in our thinking about them? The answer given by David and repeated over and over again by all the descendants of this tradition—even empirically minded ones such as Shapin, Schaffer[6] and Collins—is a resounding 'no'. Things are never enough: underdetermination is their only way of impinging on our beliefs. This is the position I have criticized and if this aligns me with the worst enemies of science studies, well so be it. There is no reason to stick out of loyalty to an absurd position just because it is attacked by even more stupid enemies.

When David gives the example of the electron, we clearly see where the problem resides:

> Once we realize this [that Millikan believes in the electron and that Ehrenhaft does not believe in it] 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. (p. 92) (Italics mine)

I agree: we are interested in differences. Now, I want someone to explain to me what it is for an object to play a role *if it makes no difference*. On a stage, when someone or something is said to play a role, and even an 'important', a 'crucial', a 'decisive' role—which would be necessary to counteract the charge of idealism—it has to produce differences. If it makes none during the whole play, I would say that it is a fixture of the backdrop or that it is, as we say in French, a mere 'potiche'. It counts for naught. Please, don't tell me that something plays a role and has no role, that it makes a difference and that it makes no difference, that it is there and then 'drops out of the story'.

This is the first technical point of divergence, and there is no way to hedge one's way out of that question. My diagram of David's position captured exactly the Kantian quality of this anchor: the two positions are at an *equal* distance from the

[6]The critiques made by Schaffer (1991) of my position are exactly the same as those of Bloor, and exactly as ill-conceived. Schaffer wants to share *unequally* the sources of uncertainties, so that all the uncertainties reside with humans, while the sensory inputs remain utterly neutral.

sensory data.[7] The 'electron in itself' is not allowed to make any other difference. This is exactly what I have said. It is not idealism, I agree and never said it was; it is exactly what Kant called 'transcendantal idealism' conjoined, as he explains at great length, with 'empirical realism'.[8] The things in themselves are there to make a difference, yes, but between 'empirical idealism' (the belief that reality is made out of thin air) and 'transcendantal idealism' (the belief that science grasps things-in-themselves). The only role of those things-in-themselves is to discriminate between philosophical schools—this is, by the way, the general problem with Kant: concepts have no substance and are there only to make distinctions between equally disreputable positions! David surely knows that Kant's solution was criticized as soon as it was uttered. It was part of the modernist settlement which held only as long as science was not examined. It fell when we started to subject science to the scrutiny of science studies. More of this in the last and most substantive section of my response.

Let us now examine the second charge according to which I have misunderstood the Edinburgh School. This will be even easier, since David, in the same page, mocks my contention that sociology of science of his sort 'generates "arbitrary constructions determined by the interests and requirements of a *sui generis* society"' (p. 110, quoting me), less than a page after he quietly reiterated, in an even more damning way, his claim that the causality proper to society is a self-referential one that explains the binding force of convention. This is what I call the 'straw man straw man fallacy': that is, portraying oneself as an unfairly attacked straw man, only to hide the simple fact that the real position is stuffed with even more straw than the one attacked! If he or Barry Barnes can explain to me what is the difference between 'bootstrapping' and 'sui generis', I would be glad to make amends. Meanwhile, I stick to my entirely accurate characterization of the Edinburgh School as using to define society the worst defect of Durkheim—taken straight from Kant and passed along straight to Mary Douglas—that is, the self-referential nature of society. I agree that one should not confuse 'convention' and 'arbitrariness'. But since things make no difference to our thinking about them (see the point above), I am allowed to make this little polemical shift. Kant had a solution: Reason. Bloor does not have this possibility. The belief system has to register the world without the world introducing any significant difference, apart from its mute presence and insistence. The only solution left is convention that will change only for arbitrary reasons. I agree it is not exactly necessary—Levi

[7]I presented the diagram on p. 95 of Latour (1993), and, if geometry is any guide, it is a perfect rendering of David's position. It says nothing about David being an idealist: things are there, exactly as in Kant, but they make no difference, each position (true or false) being at an *equal distance* from the neutral nature of things. Hard to be more precise, especially when you read things like: 'The processes [of filtering, sampling etc.] which are collective achievements, must *ultimately* be referred to properties of the knowing subject. That is where the sociologist comes into the picture.' (p. 9) Kant was in this picture two centuries before—no wonder: he drew it!

[8]See Kant (1950), chapter on the 'Refutation of idealism', pp. 244 *et seq.*

Strauss's structuralism provides another solution and so do Foucault's 'epistemes'—but they all travel in the same boat—and that boat, freighted by Kant, like the Titanic, has only the appearance of being insubmersible: the truth is that it sank on its first empirical voyage. Things make more difference than being summoned to bear witness that a philosopher (or a sociologist) has been unfairly charged with idealism.

It is only in reading David's critique that I realized what has always been so odd in the Edinburgh way of thinking: it puts to use three types of causality that are spread very unevenly and which are distributed according to an ontological divide between types of entities.

(1) For non-social nature (a strange way of speaking) there are causal linkages of the most classic sort—and wholly scientistic ones;
(2) for social nature there exist causal linkages too of the first sort and, in addition, self-reference of a Durkheimian sort (we create a society that strikes us with the complete realism of what has not been created, thus effacing the difference between object and subject);
(3) and, then, there exists a third type of causality, of a queerer sort, one that could be called Kantian, or better, Humean, and that ties neutral sensory data and human belief systems. Now, this one, *and this one only*, is not allowed to fully carry causality and will remain always underdeterminate. I quote:

> Corresponding, or not 'corresponding' to reality, are *not* causal relationships that bodies of belief bear to their referent. Beliefs do have cause connections to things in the world, but the words 'correspond' or 'do not correspond' do not capture those connections. Neither relationship is a *genuine*, or naturalistically specifiable connection existing in its own right. (p. 89) (Italics mine)

So we end up with the following distribution of types of causalities according to domains of reality (the presence of the wedge will be explained in the next section).

Everything in nature—since David is careful to make society a part of nature—can be causally explained, but only electrons 'themselves' are not allowed to cause our interpretations of them, no matter how much scientists engage in making them have a bearing, a causality, on what they (the scientists) say about them (the electrons). That this extravagant distribution of forms of agency could pass for reasonable, is in my view the most bizarre feature of the modernist settlement. It is of course thoroughly explainable historically and anthropologically,[9] but that one can stick to it for twenty years without realizing that there are a few problems with it is a good sign that belief systems can be immunized against dissident anom-

[9] I have begun to make an enquiry into the longer history—or mythology—of this strange configuration (Latour, 1997b).

alies—which, I agree, is in keeping with the Edinburgh way of seeing the role of data; at least they cannot be accused of incoherence!

I will leave aside the charge concerning my misunderstanding of relativism. It is not as important as the two others, and I have no difficulty in accepting David's definition of relativism as being the opposite of absolutism. My own definition is not connected to that issue, but to the more important one, in my view, that opposes absolute relativism—that engages in the sort of dispute David has been fond of—and relative relativism (or relationism) that sticks to the empirical task of tracing the establishment of relations. I will come back to the political use of relativism in the last section.

So when I say that the Edinburgh School forbids things to make a difference in our belief systems, and relies on a definition of causality for society that is *sui generis*—or self referential—that construes all questions of interpretation as so many conventions—more polemically 'arbitrariness'—I am entirely vindicated, and in case I doubted this, David's paper would have kindly provided all the resources for me to reiterate the point: 'The interesting theoretical task is to combine this model of social institution [bootstrapping] with the sociological insight that all knowledge has the character of a social institution' (p. 108). As David kindly predicts (p. 86), I call that combining the ills of sociologism with all the defects of Kantianism.

3. What Should be Used as a Resource for Enquiry and What as a Topic?

This, in my view, ends the agonistic aspect of the debate and allows me to begin the substantive part, which I will keep short enough, since so much work has been done on all of these other metaphysical aspects and since I want to concentrate only on the reasons that explain why David cannot see those limitations which are so glaring to all of us.

One remarkable sentence of his chapter explains it all, in my view:

> Only by sustaining a distinction between subject and object, and by *driving a wedge* between nature and the description of it provided by the knowing subject, can we highlight the *problematic* character of those descriptions. (p. 94) (Italics mine)

David does not realize the strange 'bootstrapping' of his sentence: it is as if he was saying that if you 'drive a wedge' into a tree trunk you would render 'problematic' afterwards the reconnection of the two parts of the trunk. The answer is definitively 'Yes'! But then the question is why someone would engage in the strange contradictory task of severing what he wants to glue together.[10] Only a modernist would do that, and we can measure the advance of science studies by the ease with which we can now see the oddity of such a position that, twenty years ago, would have seemed plain common sense.

[10] I have now begun an enquiry into this critical gesture itself, see Latour (1997a).

I have counted around twenty-five instances, in the text, of David driving a wedge between object and subject. No wonder the data are rendered problematic, they have been chopped into mincemeat. And he has the audacity to continue the sentence quoted above in the following way:

> It is those who *don't* mark the different contributions of the subject and the object [remember that the object is not allowed to make any difference, so this is just a word to avoid the charge of idealism] 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. (p. 94) (Italics mine)

William James, a century ago, made fun of all the epistemologists who, after having cut an abyss between words and world, imagined no other way to relate them than a 'salto mortale' above the yawning gap. He described his own position as a 'deambulatory' theory of truth, because it skipped no intermediary, no transition (James, 1907 [1975], p. 245). Now, what James was doing conceptually, we have done empirically and I am probably the one in the discipline who has proposed most terms to make this transition, this deambulation, observable, realistic and documentable: inscription, visualisation, translation, trials, mediation, names of action, black-boxing, historicity of things, etc., and I am of course not the only one: the whole field is about making the transition visible.[11] That David does not see it proves nothing about our work, but a lot about the sway that the modernist settlement can have over one's mind, even those of the first practitioners of science studies. He accepts only one single gap—in the middle of Fig. 1—while we multi-

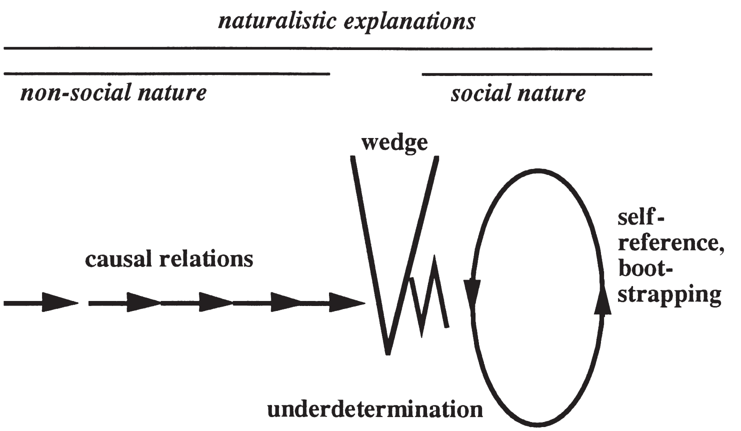

Fig. 1. Naturalistic explanations can be divided into two parts: one for non-social nature where causality reigns; one for social nature where an ad hoc *type of self-referential causality dominates; in between is a gap that neither causality nor self-reference is allowed to bridge, and that is defined as 'playing a role' without 'making a difference'.*

[11]Just one example of how problematic I can render the 'transition' is Latour (1995); another recent example is Goodwin (1995).

ply them all along the chain of associations, and he quietly comes to tell us that 'we pave the way to error'. Yes, we pave the way, but to mediations! David wants to keep the subject–object split in spite of its shortcomings, without realizing it is part of the problem and not part of the solution. He could say like one of James's imaginary objectors: 'Dualism is a fundamental datum: let no man join what God has put asunder!' (James, 1996 [1907], p. 36).

Bloor is of course perfectly right in saying that we cannot topicalize every concept and that some should be used as resources and some others as topics. The strategy in any research program is to distribute topics and resources in the most intelligent and fecund way—and, I would add, to move fast and to change tack often enough to maintain the strategic aim through many empirical moves. Bloor is even honest enough to recognize that I have chosen to topicalize the one—and all its descendants—that he takes as its most essential resource. But he does not draw the conclusion that this very choice is the source of all the obscurities he sees in my line of work and that his solution, although perfectly respectable, makes him wholly incompetent to judge mine. He even sees as a disreputable oddity my inability to distinguish 'nature' from 'beliefs about nature'. 'Latour,' he says, 'makes *no systematic* distinction between nature and beliefs about, or accounts of, nature (…) It is as if he has *difficulty* telling these two things apart.' (p. 87) (Italics mine)

Yes, I have great difficulties in convincing myself that it is useful to create an artefact to get at the facts. When insisting on the necessity of a difference I have undermined all along, Bloor aligns himself with the most reactionary philosophers of science who insist that science studies is all very well as long as it sticks to epistemological questions and leaves entirely aside—that is to the scientists!—the ontological ones. To write as he does that: 'The aim isn't to explain nature, but to explain shared beliefs about nature,' (p. 87) or that 'The important point is to separate the world from the actor's description of the world' (p. 93) is to grossly misrepresent the scientific enterprise, what all scientists (including us) strive for. As Isabelle Stengers is fond of saying: when a sociologist faces a scientist with this claim, 'this means war'—the Science War.[12]

Those who drive wedges to produce problematic connections have no business judging the work of those who follow the connections established by scientists and engineers along what they—and not philosophers—see as problematic. The former destroy the data that the latter keep intact for scrutiny. They cut it in the middle, we protect it against deterioration. If scientists insist over and over that they make no durable distinction between nature and beliefs about nature, if their whole work is directed to make sure that their beliefs are not representation, but deal also with ontology, we don't have, it's true, the courage to break what they say in two and

[12]See the title of Stengers (1996).

then look for a glue to bring their interviews back together. No, we drive no wedge, except between the modernist settlement and its practice (Latour, 1997a).

Every single one of the philosophical difficulties of the moderns comes from this double bind: driving a wedge and then, after having rendered problematic the connection, trying to patch it up. This is nowhere clearer than in the empiricist legacy—the one, remember, that forced Kant to build his palace of fallacies. David's paper makes very clear what is so wrong in the definition of empiricism as we inherited it from Locke. 'Cultivating an empiricist sensibility can be a useful tool,' David writes, 'concentrating on what can be *visually seen*, or otherwise sensed, sustains our awareness of the *gap* between objects and their descriptions.' (p. 93) (Italics mine) And earlier on, in alluding to Pasteur, he portrays him as an 'observer': 'The subtleties of Pasteur's work come from the detailed character of the *observation* he makes.' (p. 85) This is, I think, where all the differences between both of us hinge. In spite of twenty-five years of science studies, Bloor has not yet understood that scientists don't observe, nor see the world 'out there'. They are much more involved than that in the fate of non-humans. Scientific practice is the only place where the object/subject distinction does not work.

Using Pasteur as an example, David reiterates the distinction between sensory data—or at least 'a neutral vocabulary' (p. 92)—to describe inputs from the world and interpretations. He even uses an example very close to one I studied, in a paper he cites without understanding the first word of it (Latour, 1996a). In a perfectly Edinburghese style, he opposes a 'reddish powdery substance' on the one hand, and a belief system on the other (p. 92). Naturally, the reddish substance has been rendered neutral enough—or Humean enough—that it makes no difference among Pasteur, Davaine and Koch. So through what doors do the differences enter the scene? *From the back door of the self-referential social*: there is no other entry since he has shut off all the other forms of engagement and involvement with the world. David then claims that my solution is absurd or is tantamount to naive realism, because instead of this apartheid between neutral vocabulary and self-referential convention, I would let the microbes 'themselves' enter the discussion and *make a difference* between Koch and Pasteur.

The key difference between us can be stylized by one of my doodles (see Fig. 2).

What would I do, instead of this? I let Pasteur himself come to the 'greyish substance' and connect it to many other things as well: the experimental setting that reveals the presence of the ferment in the first place; but, as I showed at great length in commenting on his paper, Pasteur is also part of the very debate between empiricism and rationalism, and worries whether he is making up the facts or whether the facts carry weight by themselves. It is precisely this sort of question, that scientists themselves raise, that I can follow because I make no more definitive distinction between object and subject than they do, and that David wants us to abandon in order to replace them by his philosophical quandaries. The idea that there exists a neutral vocabulary which will be the same in Pasteur and (in my

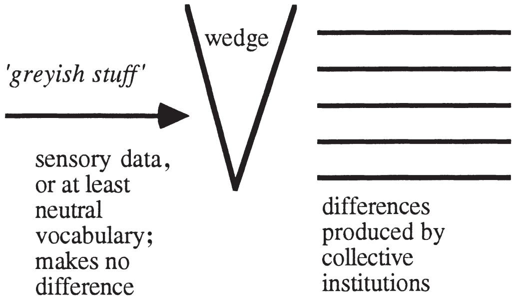

Fig. 2. Empiricism, Bloor style. Bloor defines empiricism as being made up of sensory data that provides a neutral description that makes no difference; the differences will begin when we look, on the other side of the gap created by the wedge between object and subject, at the institutions of society.

case) Liebig's laboratory and where the 'greyish substance' would be 'indifferent' or 'neutral', is entirely forced or cramped. It is not the *same* greyish substance in Pasteur's laboratory as in Liebig's for the very good reason that Pasteur built his whole experiment to make it visible and allow this 'sensory data' to *make a difference* and render it pertinent, to destroy the whole chemical theory of Liebig. The idea that one singled-out empirical feature (sensory data) could be neutral and make no difference is precisely what my enquiries have shown to be false. That the difference between primary and secondary qualities, which entered philosophy in the seventeenth and eighteenth centuries through Descartes, and then Locke and his descendants, for reasons entirely foreign to our contemporary moral, political, and technical situation, has always baffled me. Why we should stick to what Whitehead called 'the bifurcation of nature' is a complete puzzle to me.

My solution could be contrasted to David's in the following diagram (see Fig. 3).

Thus, there is indeed a difference between David's treatment of the case and mine: every single one of the entities aligned in Pasteur's laboratory, from the Emperor to the greyish substance, is allowed to make a difference. None of them is exactly causal. Each of them is allowed to make a difference. None of them is

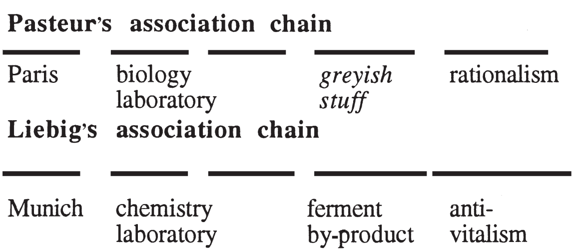

Fig. 3. Empiricism, Latour style: what is compared are not social differences with a neutral input from the senses, but long chains of associations including psychological, ideological, cognitive, social, and material entities, many of which are non-human agents. Along these chains, each element takes the meaning given to it by the adjoining elements in the series.

a mere intermediary. Each of them is a mediator. Each of them, human and non-human, is in part self-referential. The whole metaphysics that David finds so obscure comes from the necessity to follow those chains. In the central sections of his paper, David mocks my ridiculous attempts at using 'entelechies', 'actants', 'monads' to speak in a new way of this empirical world. He does not see that each of the entities I am dealing with possesses the three types of causality that he keeps so cleanly separated: each of them is self-referential—like society in his scheme—each of them is causal—as in what he calls non-social nature—and yet each of them underdetermines the next one in the series—as in his conception of the gap between sensory data and interpretation. Instead of distributing his three types of causality according to different ontological domains (one for non-social nature, one for social nature, and one for their connection), I attribute each of them to all of the entities. This is what has allowed me to tackle anew the question of what society and nature are made of. I claim that this question has become empirically studiable only since this methodological move has been taken.

I am convinced that one can do much better than I have done, but I am equally convinced that one should go through the whole of the philosophical tradition to avoid the absurdities of having some entities 'playing a role' and 'dropping out of the story', others that are self-referential and neither objective nor subjective, still others which are causal according to a non-examined kind of naturalism. If some-one compares the two types of 'obscurities' and puts into the balance the empirical fecundity of the two research programs, there is no question in my mind that David's definition of empiricism cannot obtain.

The extent of this misuderstanding over metaphysics is made even more glaring when Bloor believes he understands me (p. 96) in agreeing that, in the distant future, naturalism will have taken over even the social pole and that Pasteur and his microbes will all be made of electrons, photons, atoms and brain waves! This is metaphysics indeed, but at its most naive. If the Churchlands might believe such nonsense, nothing could be further from our definition of actants. They are not in nature, nor in society (nor in language). To imagine that one could solve the question by sticking to the poorest and most scientistic metaphysics of electrons, atoms, photons and brain waves, made clear again what I have suspected all along: the Edinburgh School has not even begun to understand the first thing about the philosophical originality of science studies, their metaphysics is that of Voltairian materialism. What they mean by a naturalistic enquiry takes its inspiration from the same type of nature as scientism. The idea that the very definition of the agency of matter could be one of the concepts to topicalize has not even crossed their mind, in spite of the mass of work published on empirical metaphysics in the journal *Social Studies of Science* published in Edinburgh by the same Unit.

The extent of the misunderstanding is made even clearer in footnote 10, where David ironizes on the fact that, according to me, actants 'would be allowed to define the analyst as well'. He is quite right. The whole philosophy of Isabelle

Stengers could be extracted from this little sentence, on which he believes he exerts irony by saying 'something for which I can see no good grounds' (Stengers, 1997)! Well, if he had entered a laboratory, David would have noticed the obvious: any scientist worth the name has been thoroughly redefined by the actors he or she has dealt with. This is also true of science students, of me, anyway. If he does not get that point then there is not much hope of bringing the Edinburgh School back into the mainstream of science studies.[13] The whole aim of science is to make non-humans, through the artifice of the laboratory, *relevant* to what we say about them. This implies obviously that the analyst cannot even think of a question that is not retranslated entirely by the experimental turmoil in which both the scientist and the thing to be analyzed—not to mention the rest of society—have been reformatted. This is what I have shown about Pasteur and I am pretty proud of it—if it looks like good old SSK, then all the better.

4. The Fight Against Absolutism

I don't have to go any further in this matter. Readers can go to the literature referred to in my footnotes and judge for themselves how different our papers seem when looked at through David's interpretation of empiricism and through mine. However, I have learned over the years that all methodological questions are based on metaphysics, and that every metaphysics is at heart a moral and political issue. Actually, a lot of good old Edinburgh School work has taught me this, as well as the earlier and similar debate Callon and I had with Collins and Yearley.[14] I want to finish with the question of politics.

David believes my position is reactionary—'a step backward', as he says. He thinks that only by keeping both sides of all scientific disputes *equally removed* from access to things in themselves, some sort of civility will be maintained, since we will all collectively avoid absolutism. Relativism of his sort, that is, the insistence on the conventional framing of a neutral impact of things on society, will protect us against an excess of power in some scientists who will not only say, if not opposed, that they have an interpretation of nature, but that nature herself is as they say she is. He compares himself to a philosopher of law (p. 101), who would insist that both sides of the dispute lack absolute grounds for their claims, which they should settle in a civil and modest manner. My position, as he sees it, is reactionary in so far as it breaks down the distinction between convention and nature herself, and then gives no power of appeal when defeated in a controversy

[13]One explanation might be that David Bloor has been trained in psychology, a discipline that is not known for its immense respect for the recalcitrance of the subject. In the psychological laboratory one acquires bad habits, such as that of never being forced to redefine one's questions, because of the tricky aptitude of the subjects for behaving like objects one can master. On all of this see Stengers (1996).

[14]See the debate in Pickering (1992). This first debate largely parallels the current one, except that David has engaged much more honestly with our enterprise and attempted to understand it much more fairly. A reading of Gerard de Vries' referee report would still be useful for this one (De Vries, 1995).

by a more powerful interpretation. I am in the camp of the absolutists and he quotes a passage of *Science in Action* that he construes as meaning that I abandon all efforts to help the weak to defend themselves against absolutism.

This is a serious charge, especially because in this passage I charge him with the same sin! I claim, that is, that the distinction between Nature and Society makes it impossible to *register* the different asymmetries that chains of associations produce when they encounter one another. It is precisely to register those many differences that we had to jeopardize the difference between Natural and Social (or conventional) explanations.

I don't have a definitive answer to this and probably need Bloor as much now as I needed him in the past to help me out. The question we both face is what sort of science studies will keep civil society more open, against the constant threat of absolutism, one of the absolutisms being that of nature herself, a question that the Science Wars has made even more urgent. Here is how I see things standing.

The appeal to the equally conventional character of all scientific knowledge is not only empirically false, it is politically vacuous. As Isabelle Stengers has so forcefully argued, one science student who enters a laboratory and quietly states, without taking any new risk, that epistemological and ontological questions will always be kept distinct and that things play no other role than neutral and silent partner in our dispute, will be shouted out—and rightly so (Stengers, 1993, 1996). If science studies has this agenda, I want no further part in it, and if this means a step backward, then it means a step out of prison towards freedom. So driving a wedge between conventionality on the one hand, and neutrality of sensory data one the other, is a pathetic weapon against absolutism. Actually it *encourages* absolutism, the new sort of postmodern assumptions of 'everything goes' that David abhors as much as I do and that he promotes nonetheless by refusing to enquire into its metaphysical roots. What David has the nerve to call the 'empirical sensibility' I will call the epitome of 'political insensibility'. The critical fight against naturalisation has failed and should be abandoned, together with the whole critical project (but this is another question) (Latour, 1996b).

The alternative I would prefer, is to engage in a complete reworking of the origin of the notion of 'nature'. Nature is the concept to topicalize. It is through nature that the whole history of absolutism has been developed.[15] This is a long history and not an easy task, although many elements have already been untangled—especially by anthropologists,[16] but also by many students of science (including Shapin and Schaffer, to whom I have done, contrary to what Bloor asserts, fuller justice than they themselves did!). I am not completely sure of where it leads us, but I am sure of one thing: no position that takes the very object/subject difference as a resource can even begin to fill the bill. We should be innovative precisely on

[15]For a first attempt, see Latour (1998).

[16]See Descola and Palsson (1996) to measure the quick new advances symmetric anthropology is making, now that it is freed from the Durkheimian sway of the notion of culture.

the point where David says we should sit snugly without interrogating further. No scrutiny of nature can be carried out if we first believe in nature as the obvious background of all our assumptions about it. It is because of the failure of his research program to move on that point, that we had to abandon the first principle of symmetry (and also now the generalized principle, I am happy to announce).[17]

This might seems incredible to some, but science studies evolve, move and thrive, and, although I would much prefer to benefit from David's continuing involvement in the discipline, I won't stick to reasonable absurdities just because it befits the tradition more. Our slogan 'follow the agents themselves' is not for the dogs. To it we sacrifice everything. They have priority over all disciplinary loyalties and all claims to stick to common sense. I have always described the failure of the Strong Program as a '*felix culpa*', that is, a welcome mistake that revealed what had been so wrong in the social scientists' notion of social construction and critical discourse generally. I am thankful to David for having shown me again the way to go: that is beyond the position he so clearly advocates.

Acknowledgements—I wrote this reply at the end of a writing workshop with the doctoral students at my centre. The class was divided in two: one half defended the CSI sociology, the other (including me!) defended Bloor's text. I thank everyone for their spirited defence of their position. I also thank Isabelle Stengers and Eric Francoeur for comments on this retort.

References

Bloor, D. (1982) *Sociologie de la logique ou les limites de l'épistémologie* (Paris: Editions Pandore).

Bloor, D. (1991) *Knowledge and Social Imagery* (second edition with a new foreword) (Chicago: University of Chicago Press).

Callon, M. and Bruno, L. (1982) *La Science telle qu'elle se fait. Anthologie de la sociologie des sciences de langue anglaise* (Paris: Editions Pandore).

Callon, M. and Latour, B. (1985) *Les Scientifiques et leurs alliés* (Paris: Editions Pandore).

Callon, M. and Latour, B. (eds) (1991) *La science telle qu'elle se fait. Anthologie de la sociologie des sciences de langue anglaise* (nouvelle édition amplifiée et remaniée) (Paris: La Découverte).

Descola, P. and Palsson, G. (eds) (1996) *Nature and Society. Anthropological Perspectives* (London: Routledge).

De Vries, G. (1995) 'Should we send Collins and Latour to Dayton Ohio?', *EASST Review* **14,** 3–10.

Goodwin, C. (1995) 'Seeing in depth', *Social Studies of Science* **25,** 237–284.

Haraway, D. J. (1991) *Simians, Cyborgs, and Women: The Reinvention of Nature* (New York: Chapman and Hall).

James, W. (1907) [1975] *Pragmatism: A New Name for Some Old Ways of Thinking followed by The Meaning of Truth* (Cambridge, MA: Harvard University Press).

[17]See Latour (forthcoming) where the notions of propositions and articulations render superfluous the notion of symmetry altogether. This new work, that makes much more vivid the differences between the subject/object dichotomy and the human/non-human relation will clarify, I hope, most of the technical difficulties that David (as well as I) found in my principle of generalized symmetry. (Added after reading Bloor's reply: Symmetry (limited or generalized) between two artefacts can only be a temporary scaffolding: once the artefacts are dissolved, symmetry is no longer necessary.)

James, W. (1996) [1907] *Essays in Radical Empiricism* (London: University of Nebraska Press).

Kant, E. (1950) *Critique of Pure Reason* (trans. Norman Kemp Smith) (London: Macmillan).

Latour, B. (1993) *We Have Never Been Modern* (Cambridge, MA: Harvard University Press).

Latour, B. (1995) 'The 'Pédofil' of Boa Vista: a photo-philosophical montage', *Common Knowledge* **4,** 144–187.

Latour, B. (1996a) 'Do scientific objects have a history? Pasteur and Whitehead in a bath of lactic acid', *Common Knowledge* **5,** 76–91.

Latour, B. (1996) *Petite réflexion sur le culte moderne des dieux Faitiches* (Paris: Les Empêcheurs de penser en rond).

Latour, B. (1997a) 'A few steps towards the anthropology of iconoclastic gestures', *Science in Context* **10,** 63–83.

Latour, B. (1997b) 'Socrates' and Callicles' settlement or the invention of the impossible body politic', *Configurations* Spring **(2),** 189–240.

Latour, B. (1998) 'To modernize or to ecologize, that is the question', in N. C. a. B. Willems–Braun (ed.), *Remaking Reality: Nature at the Millenium* (London: Routledge), pp. 225–246.

Latour, B. (forthcoming) *Pandora's Hope: Essays on the Reality of Science Studies* (Cambridge, MA: Harvard University Press).

Lynch, M. (1985) *Art and Artifact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory* (London: Routledge).

Mol, A. and Law, J. (1994) 'Regions, networks, and fluids: anaemia and social topology', *Social Studies of Science* **24,** 641–672.

Pestre, D. (1995) 'Pour une histoire sociale et culturelle des sciences. Nouvelles définitions, nouveaux objets, nouvelles pratiques', *Annales (Histoire, Science Sociales)* **3,** 487–522.

Pickering, A. (1984) *Constructing Quarks: A Sociological History of Particle Physics* (Chicago: University of Chicago Press).

Pickering, A. (Ed.) (1992) *Science as Practice and Culture* (Chicago: Chicago University Press).

Pickering, A. (1995) *The Mangle of Practice: Time, Agency and Science* (Chicago: The University of Chicago Press).

Schaffer, S. (1991) 'The eighteenth brumaire of Bruno Latour', *Studies in History and Philosophy of Science* **22,** 174–192.

Stengers, I. (1993) *L'Invention des Sciences Modernes* (Paris: La Découverte).

Stengers, I. (1996) *Cosmopolitiques—Tome 1: La Guerre des Sciences* (Paris: La Découverte and Les Empêcheurs de Penser en Rond).

Stengers, I. (1997) *Power and Invention. With a foreword by Bruno Latour, 'Stenger's Shibboleth'* (Minneapolis: University of Minnesota Press).