DISCUSSION

Reply to Bruno Latour

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Bruno Latour’s response is very much of a piece with his other writings. It is, for instance, full of unresolved tensions. In the present case this contradictory quality resides in the unstable mixture of concession and defiant dismissal. Thus, I am ‘right in everything’ I say (p. 113), but then it turns out that I am only right from my own point of view. He says he accepts my definition of relativism, but in practice does not accept it (p. 120). There is a gulf between us, which renders me ‘wholly incompetent’ (p. 121) to criticise his work, but apparently leaves him wholly free to criticise mine. I am told throughout to give the objects of nature a vastly greater role, but then informed that I must not presuppose nature as an obvious background (p. 127). I am told that the transition from nature to verbal accounts of nature is merely an artefact of my assumptions, having just read that Latour himself has done more than anyone to make these transitions visible (p. 121). And so it goes on, all liberally spiced with ad hominem argument and other diversionary tactics. Methodological criticisms are transposed into quasi-political comments. Rather than confronting the specifics of an issue Latour will entangle it in the history of philosophy. Details are lost in a plethora of textbook categories: Kantianism, Lockean empiricism, Humean causality, Voltairian materialism, Durkheimianism and, of course, ‘modernism’.

In these circumstances it is especially important to keep the overall shape of the discussion clearly in view. The main points are as follows: Latour has attacked the Strong Program and, indeed, the whole sociology of knowledge approach. He claims to have detected deep flaws in the entire framework of thought it expresses and he has therefore proposed a radically new approach built on quite different principles. My argument, in ‘Anti-Latour’, is that when they are subject to detailed

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scrutiny both of these claims collapse. First, his understanding of the Strong Program is defective and its alleged flaws are really the products of his own misunderstanding. Second, the alternative he proposes, in so far as it actually differs from the Strong Program, is incoherent and indefensible. It is either completely obscure or it is inadequately motivated and justified. One of the main features of the allegedly new approach, which it shares with some of the allied movements mentioned by Latour, is that it represents an unprincipled retreat from the sociological program. It is essentially the sociology of knowledge in an emasculated form, with all its explanatory and causal aims discarded. At this point we must not allow ourselves to be sidetracked. We must address the question: has Latour really met these two criticisms? Has he been able to sustain his reading of the Strong Program and has he now clarified his own concepts and methods so as to demonstrate their viability? I think he has not.

To clear the ground I need to make two preliminary points. First, Latour assures the reader that he knows the sociology of knowledge literature ‘inside out’ (p. 114). He has, he says, spent many years actually defending it. Was it not he who arranged for the French translation of my *Knowledge and Social Imagery* (Bloor, 1976)? Indeed it was and I owe him a debt of gratitude for this. Unfortunately none of these claims meet the point. Long acquaintance with a doctrine is no good if it is, after all, based on a long-standing misapprehension. Whether Latour really knows the literature inside out is precisely the point at issue. My conclusion, and it has saddened me to reach it, is that for many years he has been unwittingly spreading misinformation about the sociology of knowledge. Given his considerable influence in the field, then goodness knows what damage has been done.

Second, after announcing his own qualifications Latour seeks to discredit mine. He has been out there, rubbing shoulders with real scientists, following them around in their laboratories. Where are my empirical, laboratory studies? I have ‘no business’ to criticise (p. 122). This man has never even been into a laboratory! (He means as a sociological observer, not as a researcher doing experiments, for that, in a modest way, I have done.) Had I seen what he has seen, the argument runs, I should have understood his novel perspective. But are fieldwork credentials necessary for the questions I am putting to Latour? The answer is no, because I am not challenging his facts but his interpretations. A statistician can raise questions about the analysis of data from an empirical study without setting foot outside the mathematics institute. Theoretical physicists can detect uncontrolled variables in an experiment without getting their hands dirty. I am doing something analogous. The simple division of labour does not preclude our talking rationally and critically, in both directions, across the divide.

Two quick illustrations will show that our differences do not lie in areas where a laboratory visit would make much difference. To begin with, I hardly think fieldwork has been responsible for, or would cure, the confusion in Latour’s mind
between the logic of zero-sum games and the principle of vector addition (p. 116, note 5). This is just a matter of thinking the thing through properly and keeping different processes and principles separate. Next, consider Latour’s emphasis on the way scientists work hard to give their own favoured theories a greater credibility than those of their rivals. This is a plausible empirical claim about scientific behaviour, albeit a rather obvious one. My point is that Latour is wrong to treat this as running counter to the relativism of the Strong Program. He thinks (Strong Program) symmetry means that all theories have the same credibility, but it doesn’t. To draw attention to this error does not involve challenging his facts.

Now to the all-important question of whether Latour has answered my charge of misrepresentation. I have already said I do not think he has, but Latour is confident that he has ‘entirely vindicated’ himself (p. 120). He reaches this conclusion by simply repeating the old misunderstandings in a slightly different form. Take the issue of the role of the material world in the construction of knowledge. What I called the charge of idealism and Latour the charge of Kantianism is still being repeated. This time the misrepresentation depends on a conflation that runs throughout his reply. Let me explain what that conflation is, where it comes from, and the mischief it does.

Latour thinks that the highly important and consequential underdetermination thesis is the same as the claim that experience of the world and the objects in it ‘makes no difference’. The Edinburgh School, as he put it, ‘forbids things to make a difference to our belief system’ (p. 120). This theme of ‘making no difference’ is repeated time after time, both in the text and captions to diagrams, and is even inserted into a quotation. As a gloss on the underdetermination thesis, however, this is wrong. Here is how the sorry process gets under way. Latour complains that, in the Strong Program, objects and our experience of them, ‘don’t do very much’ (p. 116). He accepts, as if it were a generous concession, that the Program’s adherents count themselves as materialists or certain kinds of realist, but he still poses the question, ‘are these objects allowed to make a difference in our thinking?’ (p. 117, his emphasis). Without textual justification it is alleged that in the past this question has always been answered with a resounding negative. After citing a passage from ‘Anti-Latour’, about the role of the electron in Millikan’s and Ehrenhaft’s work, he implies that I am mired in contradiction. He wearily asks me not to tell him that objects do and do not play a role, that something ‘makes a difference and that it makes no difference’ (p. 117).

Latour’s fallacious moves, and the resulting perversion of the underdetermination thesis, may be illustrated with a simple example. Imagine some prominent macro-object that is a salient feature of the environment of two observers. Call the object X and the observers O1 and O2. After inspection, O1 declares that X belongs to class C1, while after the same kind of inspection O2 puts it in class C2. They agree it cannot truly be both, so each thinks the other is wrong. Why do they
classify differently? The underdetermination thesis says that in these circumstances their encounters with X are insufficient to explain this difference. Something else, something about O1 and O2 themselves, is needed for the explanation.

Now, is this the thesis that the observation of X ‘makes no difference’? Is a defender of the underdetermination thesis committed to saying that things such as X are not to be thought of as making a difference to the situation described? Clearly not. The general difference made by the presence of X is that, in appropriate circumstances, it is capable of prompting acts of classification and, in this case, giving rise to the disagreement between O1 and O2. If the object were absent there would be no occasion for disagreement or, if there were a disagreement, it would be precipitated by other causes and would arise by another route. We can at least say that it would not be this dispute between them. So the object makes a difference even though, in the above scenario, it cannot explain the other difference about divergent classification. The attempt to tie me in knots is little more than a play on words. Latour has conflated a thesis of the form ‘makes no difference’ with a quite distinct one of the form ‘can’t explain this, specific, difference’. The first is the one he imputes to me; the second is the one I hold. He has simply confused different ‘differences’.

Notice that in my example the object X was not an invisible electron, it was a visible, macro-object. Underdetermination doesn’t depend on the object of reference being in any way elusive. Nor does it depend on the observer being passive rather than active and experimental. The argument goes through even if the ‘inspection’ had consisted of handling X, cutting it up, heating it, passing electric currents through it or, again, putting it in a cage with another X to see if they produce a baby X. It certainly doesn’t depend on, or push us towards, thinking of the object as a Kantian Ding an sich. Latour is wrong to inform the reader that things and objects in the Strong Program have ‘no other role than discriminating between philosophical schools’ (p. 118)—as if they are just an expedient to fend off accusations of idealism. Really, the richness of the natural world, and the complexity of the scientist’s engagement with it, is central to the thesis of underdetermination when properly understood, and hence to the Strong Program.

The person who is really in trouble over the agency of things is Latour himself rather than the naturalistic supporter of the Strong Program. He passes over this problem in silence, but just pause to think. If Latour dismisses the Kantian thing-in-itself as good for nothing but somewhat desperate moves in philosophical debates, what are we to make of his own ‘entelechies’, ‘monads’ and ‘actants’ which are neither in nature or society, nor in language (p. 124)? All requests for clarification of this extraordinary and ill conceived metaphysical exercise have been met by silence. If we ask ourselves whether we are one jot the wiser about Latour’s ‘actants’ after reading his response to ‘Anti-Latour’ we surely have to admit that we are not.

In a revealing passage, Latour takes issue with my recommendation that we keep
nature separate from verbalised accounts of nature. I spoke of driving a wedge
between them and Latour tries to have some fun at my expense on this theme.
Imagine driving a wedge into a tree trunk and rendering the connections of the parts
‘problematic’ (p. 120). Why, he asks with incredulity, should someone, ‘engage
in the strange contradictory task of severing what he wants to glue together?’
(p. 120). The general form of the question admits of a very simple answer and it
says a lot about Latour’s position that it does not occur to him. We take things
apart in order to see how they fit together and how they work. If we can’t literally
take them apart we see if nature obliges by producing natural variations and differ-
ent combinations of parts. How does this apply to knowledge? The answer is that
historical and cultural variation shows that the interface between language and the
non-linguistic world is a causal nexus of great theoretical interest. Latour seems
unable to appreciate this and appears to dismiss this interface as a mere ‘artefact’
(p. 122).

One ‘modernist’ feature of the position taken in ‘Anti-Latour’, and which Latour
affects to find ‘bizarre’, is what he calls my ‘extravagant distribution of forms of
agency’ (p. 119). Latour thinks he has found ‘three types of causality’ (p. 118) in
my work and seeks to present it as if it were the last gasp of some creaking and
complex cosmology. (At least, I think this is his point. There is little actual argu-
ment here.) Really the issue is not one of types of causality at all, so much as
different causal systems. But even if we speak loosely of physical causality and
social causality that only makes two. How does Latour get three? The answer is
that the very thing I explicitly say is not a causal relation, namely the relation of
‘correspondence’, Latour misreads as causal. It also transpires later that what he
calls this extravagant distribution of causality is taken over by him in an even more
extravagant form. He tells us that every entity actually manifests all three species
of causality (p. 124). But I am afraid that, by this stage, I do not understand what
he is trying to say.

While, by his own lights, Latour has been making rapid strides in his research
program, the Strong Program is, allegedly, where it was twenty years ago. Let me
focus on just one counter-example to this claim, Barnes’s important, theoretical
paper ‘Social Life as Bootstrapped Induction’ (1983). Here Barnes develops his
self-referring model of social institutions. It is elaborated in his book The Nature
of Power (1988) and has been applied and developed in my Wittgenstein: Rules
and Institutions (Bloor, 1997) and Martin Kusch’s Psychological Knowledge
(1998). Latour shows no appreciation of what Barnes has done and this may be
why he is able to convince himself that no progress has been made. He simply
assimilates Barnes’s work to Durkheim’s, seeing it as nothing but a version of
Durkheim’s famous thesis that social reality is sui generis. There are elements of
the self-referential in Durkheim, for instance in his picture of religion as society
worshipping itself, but a comparison on that level of generality fails to do justice
to the power and novelty of Barnes’s approach. It renders invisible the role played
by inductive learning, the valuable ideal types he introduces, called N-predicates and S-predicates, each with their precisely defined patterns of employment, and the revealing way he uses his analytical apparatus to build up certain very simple social structures. Instead of dismissing the self-referential model, taking for granted that it embodies ‘the worst defects of Durkheim’ (p. 118), Latour would be advised to address it properly and responsibly. Such labour may lack glamour, and will certainly seem pedantic to those in a hurry, but it is the only way to ensure that in our theoretical work we are not just building a house of cards.

If Latour had thought through the implications of Barnes’s work, or even reflected on what was said on the subject in ‘Anti-Latour’, he would not have needed to pose the question he does on p. 118. He wants to know how to relate self-referring social institutions to a world of external and independent objects. I shall repeat the answer given in ‘Anti-Latour’ because the issue is important. It reveals the mechanisms by which society is implicated at the most fundamental level in our understanding of the material world. Self-referential, social, processes give concepts their normative properties, that is, their standards of right and wrong application. This applies to all concepts, including those referring to the objects around us. Self-referential processes constitute the rules for the use of the concept, which is how this all connects with Wittgenstein’s account of rule-following as an institution. Without rules and normative standards defining their right and wrong application, concepts could not properly be said to have a content, and without a content they can have no genuine reference at all. In this way self-reference becomes integral to external reference. These are the connections which eluded Latour.

Finally, I must make some response to Latour’s interesting revelation (p. 127) that he is about to discard his generalised version of the symmetry principle. Given the picture of breathless progress painted in his reply, with new and ambitious enquiries being announced, it is all too easy to see this declaration as proof of how rapidly things are moving forward in the Latourian camp. What a contrast with the boring stasis in Edinburgh! But there is another construction that might be put on it, one more consonant with his failure to produce a convincing reply to ‘Anti-Latour’. This is not an advance at all, it is retreat. Latour has just abandoned his position.

References