James Robert Brown

Boundaries, Reasons, and Ideology: Reply to Sismondo

Sergio Sismondo's "Boundary Work and the Science Wars" (this journal, xxx) nicely exemplifies a hotly debated central issue. One side, let me call them the rationalists, tries to explain episodes in the history of science in terms of reason. They claim that scientists, past and present, believe what they do because of the evidence that they have at the time. The other side, following Sismondo, let me call them STSers (STS = science and technology studies), claim that social and other non-cognitive factors are the frequent causes of belief. This disagreement naturally leads to a meta-level debate. Rationalists, such as myself, try to give reasons for believing the STSers are wrong-headed in their approach. And Sismondo replies with the claim that whether I realize it or not I am really doing boundary work – an explanation of my activity in terms of social factors.

No doubt, each of us feels somewhat frustrated and thinks the other is begging the question. But our respective responses are probably inevitable. After all, I think it's reasonable to cite reason and evidence. And he, being equally consistent, will cite social factors in accounting for the presence of social factors and for my failure to appreciate them. The upshot is that it is difficult – though not impossible – to have a real head-to-head debate. All that either of us can do is lay out our respective positions and hope for the occasional glancing blow. This, I fear, is what will happen as I take up the topic of "boundary work." But I think we can come close to a real debate over the viability of naturalism and the role of reason.

There are many claims in Sismond's essay, some of which I might concede and some I would certainly dispute. But I will pass over most of the minor points and focus on the main two, boundary work and naturalism. At the end I'll add some remarks on ideology.

But before the main task, a personal remark. Sergio kindly recalls his student days. I, too, fondly recall those sparkling seminars in which Sergio shone brightly. Then we quarrelled over the foundations of physics. Now the topic is different, but the friendly disagreement continues and I'm pleased to say it also continues to stimulate.

Boundary Work

Sismondo's principle claim is that I am doing boundary work in my book *Who Rules in Science?: An Opinionated Guide to the Wars*. I deny this. But before saying why, let me make two short comments. First, the claim comes across as a criticism, but this seems somewhat at odds with the STS attitude toward *prescribing* behaviour. The widely-shared view (though Sismondo may not share it himself) is to tell us how things are, not to tell us how things ought to be. Since I reject this anti-normative attitude, I'm not complaining in principle, merely pointing out that making normative claims is hard for anyone, even self-conscious naturalists, to avoid. Second, there seem to be two things Sismondo is running together under the heading of "boundary work." One of these is the claim that I am trying to defend philosophy's turf; the other is that I am trying to defend philosophy's authority. Since I am not doing either of these things, I won't pursue the issue. But those who think I am doing boundary work might want to disambiguate the charge.

Let's turn to a dispute where the idea of boundary work has real substance – sociobiology. Various kinds of behaviour, such as altruism, homosexuality, male dominance and female passivity, have been traditionally studied by sociologists, but in the late 20th century biologists offered explanations for these behaviours in terms of evolutionary adaptiveness. Some of the response to sociobiology was clearly boundary work. Sociologists worried that some of the grant money that traditionally had gone to them would

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be shifted to biologists. The STS investigation of this aspect of science is tremendously valuable.

But it is very far from the whole story. There was also a political fight that had nothing to do with the boundary between sociology and biology. Many people worried that sociobiology was just right-wing ideology disguised as science. If female passivity or xenophobia is adaptive and encoded in one's genes, then it is "natural" and it would be folly to try to change it with affirmative action programmes. Women, according to many sociobiologists, were meant to remain at home raising children. People who feared the political consequences of sociobiology were not fighting a boundary war; from a professional point of view, philosophers, political scientists, and so on couldn't have cared less where the line between sociology and biology is. They cared about what they took to be sociobiology's pernicious social effects. STS can make great contributions by studying this aspect of science, too.

Still this is not the end of it. Anyone who thought that it was only a boundary fight and a political battle would be missing the serious intellectual debate that also took place and that had a great effect on many of the participants. The ingredients in the debate included considerations such as this. Physical traits are clearly adaptive and genetically encoded. Behaviour matters to reproductive success, so it is very reasonable to think that behaviour is biologically adaptive, too, and is controlled by our genes. Against this, opponents claimed that it might be more adaptive to give humans plastic minds that could learn appropriate behaviours in various different environments; hard-wired behaviour would be maladaptive. They also noticed that many of the arguments for biological naturalness were circular. For instance, different ant species are described as being in a master-slave relation, then the inference is made that slavery is natural and so we should expect it among humans. But how do we really know that the ants are slaves? We don't. We are quite possibly projecting human concepts illegitimately onto the non-human realm; we're anthropomorphizing nature.

What's the moral? When participants in an intellectual debate line up along disciplinary lines, this does not mean that they are fighting a boundary war. A turf battle may be part of the dispute, but the participants are also debating which of the rival theories is right. The moral can be reinforced by considering all the battles within any discipline. There is no boundary to be defended when utilitarians fight with deontologists. They are fellow philosophers who are simply arguing over who has the right ethical theory. When the Popperians fought with the Carnapians, it was not over philosophy's boundary with any other discipline. Many philosophers would like to see set theory used more extensively in philosophy; many others oppose this. Mathematicians are indifferent. Many historians would like to see economics used more extensively in the study of history; many others oppose this. Economists are indifferent. Philosophers often pronounce on the relative merits of astronomy and astrology, both of which are on the other side of the philosophy boundary, but it would be silly to see those philosophers as doing boundary work – they are doing epistemology. Even within the sociobiology debate, several sociologists were keen to use evolutionary biology to explain behaviour and many biologists were utterly opposed. Are such people disciplinary traitors, lacking any sense of patriotism? Or are they instead genuinely interested in how things work and advocating the theory they think best supported by the meagre evidence available?

One of my main interests is in the philosophy of mathematics, so I can speak with first-hand knowledge of what happens in this field. Mathematicians and philosophers alike contribute to many of the same journals. There are debates over issues such as Platonism, but one would be hard pressed to tell from the view advocated whether a particular author is a philosopher or a mathematician. There are, of course, some differences having to do with training. Mathematicians are more likely to base an argument on the details of some exotic example. Philosophers are more likely to introduce something like the causal theory of knowledge. Obviously, they are calling on their different backgrounds, but neither is defending a discipline, since there are pro- and anti-Platonists in both camps.

One of the most common things we hear philosophers say is "That's an empirical issue."

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They are forever passing off things as not philosophy. Quine, famously, has declared that epistemology is just a branch of psychology. Many philosophers are happy to declare ethical norms to be the product of a Darwinian process. Clearly, they are not defending philosophy's traditional boundary. These are naturalist views and they are exceedingly popular with philosophers today. They are not popular with me, but that is neither here nor there.

The upshot of all this is that the idea of "boundary work" is not particularly helpful in explaining a great deal of intellectual activity. And it would be wrong to see philosophers and STSers as mainly engaged in a turf war. The truth is, we have rival accounts of how science works. The idea of a boundary dispute comes naturally to those who see science in purely social and political terms. Since I believe that there are social factors at play in science, I do not reject the idea of boundary work out of hand. But I would expect it to play only a minor role.

I'll leave this topic with a challenge: Suppose X and Y are having an intellectual debate. In one case X and Y are in the same discipline; in the other case they are in different disciplines. What is the essential difference? Of course, there will be some noticeable differences between the two situations. When X and Y come from different disciplines they will have had different educations, they will have different colleagues, they will be funded by different granting agencies, and so on, and these things will matter to their approaches to any problem. This I readily acknowledge. What I would like to know and pose as a challenge is this: What is it that makes X and Y do boundary work in one case but not in the other?

Naturalism

Rather than doing boundary work, I see myself as proposing a way to understand much of science and as criticizing rival accounts, especially those typically stemming from STS. (Following Sismondo I will use STS generically for a cluster of overlapping views that go by such names as social constructivism, SSK (sociology of scientific knowledge), anthropologists in the lab, the Strong Programme, etc.) David Bloor, for instance, I take to be a fellow philosopher with a rival theory. The thing that animates him and many others is naturalism.

Sismondo rightly notes that I take this cluster of views much more seriously than I take the postmodern critics of science. The latter were the main target of the Sokal hoax, made easy by their often comical understanding of the sciences. By contrast, the naturalists are a much more formidable opponent, since they take science seriously and often use the standard methods of the sciences in their analyses. For this reason Sismondo is wrong to lump me with Philip Kitcher who calls them "would-be-debunkers" of science. (Sismondo 2005, 240) I understand Bloor and the typical STSer to be very pro-science (unlike those I referred to as "the nihilist wing" of social constructivism). Bloor's views may have the consequence of debunking science, but that was certainly not his initial intention which is avowedly scientistic. Sismondo and I agree on this point.

In taking up this cluster of issues, let me begin with Sismond's comments on Forman's famous study of the rise of a-causal quantum theory during the Weimar Republic. I described this example at length in *Who Rules in Science?*, because it was a seminal paper in the rise of STS. Forman's claim was that various social factors (Germany's defeat in WWI, the loss of social prestige of German scientists, Spenglarism and a sense of cultural crisis, and so on), made an anti-deterministic physics seem attractive to the general public, so the Weimar scientists created quantum theory, reflecting the spirit of the times. Forman's account of the acceptance of quantum theory is in sharp contrast to standard accounts (such as Jammer's) that cite the anomalous Zeeman effect and other such standard pieces of evidence.

Interestingly, Sismondo notes that Jammer makes comments along a similar line to Forman, who cites Jammer approvingly and says he's going to fill in the details. Far from helping Sismondo's case, this hurts it. Jammer, though he does not use the words, is appealing to the well known distinction: discovery vs justification. Jammer's speculations that Forman takes up are on the discovery side. If this is all that Forman was doing, then there would be very little to debate. The issue has always been what caused the scientists to *accept* quantum theory, not what caused them to

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think it up in the first place. Forman claims that various social factors, not evidence, are what caused the scientists to believe it.

Sismondo says the view that I attribute to Forman and those who endorse him "is not held by anybody whose work Brown cites, and perhaps is not among the very diverse positions held by the major figures in S&TS or allied fields." (Sismondo 2005, 238) This is a stunning claim. If Sismondo or other STSers actually believe that then it's one of the biggest cases of *mauvaise foi* or colossal conceptual confusion that I've ever seen. Consider Bloor's remarks in *Knowledge and Social Imagery*. This is arguably the single most important work in STS. The Forman study is described just before he states the four tenets of the famous strong programme. He quotes Forman with approval as showing that the acceptance of quantum mechanics "was primarily an effort by German physicists to adapt the content of their science to the values of their intellectual environment." (Bloor 1976/91, 7)

Still not convinced? Then consider this question: Why is the strong programme called "strong"? The answer is very simple. Bloor rejected the "weak" programme of Robert Merton and other sociologists of science who said that social factors should only be introduced to explain scientific belief when that belief could not be explained in rational terms, i.e., by means of reason and evidence. Sociology (and psychology, etc.) was only to be used to explain irrational belief. Thus, Soviet biologists accepted Lysenkoism because of political pressure, while those who accept Darwin do so because of the evidence. Sociology plays a role in explaining the former but not the latter. This approach to understanding science is "weak," according to Bloor, because it only employs sociology in cases of irrationality. The strong programme insists on using sociology in a thoroughgoing way. And that means citing social factors in *every* case to explain the beliefs in question. This is what Forman does and it is why Bloor cites him with such approval. If Sismondo wants to claim that STSers are moving away for this view, then I would be very happy to cheer them on to a saner outlook. But it would be ludicrous to deny that anyone has held the view. It is absolutely central to the strong programme and its founder, David Bloor.

This is just one instance. There are many others, but I'll pass over them all except to respond quickly to Sismondo's observation that Latour and Woolgar (in the second edition of their classic book) dropped the term "social" in their original title, *Laboratory Life: The Social Construction of Scientific Facts*. He says it shows they "reject the label 'social constructivism'." (Sismondo 2005, 239) Not so. Here is what they say: "…'social' was primarily a term of antagonism, one part of a binary opposition. But how useful is it once we accept that *all* interactions are social?" (1979/86, 281)

Latour in his later work seems to be rejecting this emphasis on the social. He claims that nature just as society contributes to science. Thus, when, microbes and scientists interact, the microbes make the facts as much as scientists do. Well, that's a start in the right direction – nature itself plays a role in the formation of our beliefs. But how do nature and the scientist interact? He is quite explicit that they interact in a political way, by "negotiation," etc. In other words, Latour has not banished the social, but rather expanded it so that even microbes have become part of the social realm.

I turn now to the main point: reasons and causes. My claim was (and is) that Bloor and many other STSers deny that reasons are causes; in fact they deny the very existence of reasons all together. This claim is often greeted with a howl. Bloor and others have explicitly denied it and Sismondo denies it, too. So let me explain what I mean, since there is much confusion on this point.

I'm talking about the standard, traditional concept of reason. The crucial thing about it is that it has normative content. And this normative content can, I claim, cause someone to believe something. Here is a trivial example. Person A believes that it is raining. A also believes that if it is raining then the grass will be wet. Like any normal person, A thinks in accord with the logical principle *modus ponens*. When A is simultaneously conscious of all this, then A is compelled to believe that the grass is wet. The facts about grass that A believes and the logical principle are reasons to believe the additional fact that the grass is wet; they *cause* A to adopt this belief.

Reasons are not invariably a cause, since people are not logically omniscient. They don't notice all sorts of things, they make mistakes in

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inferences, and so on. In interesting cases in the history of science, the evidence and inference patters at work will be subtle and complex. It's easy to overlook things and to make blunders. In spite of the complexity, it is still the case that reasons are often the causes of belief. The normative aspect is in the form: If you believe X and Y, then you *ought* to believe Z, as well. This is the cause of the belief Z.

The moral is simple. In order to explain why a scientist believes Z, we point out that the scientist believed X and Y and noticed that Z was a consequence. That caused the scientist to belief Z.

Bloor and typical naturalists deny that anything like this happens. They deny the existence of objective norms that guide and influence belief. It is certainly true, as Sismondo asserts, that Bloor says he hasn't eliminated reasons and that reasons can indeed be causes. But what Bloor and Sismondo calls reasons are not genuine reasons, as normally understood. I think they should not be called "reasons" at all, but I don't want to fight over a word. They are reasons naturalized, robbed of their normative character. In his own words, Bloor wants "to stop the intrusion of a non-naturalistic notion of reason into the causal story." (1976/91, 177) Sismondo and other STSers fail to appreciate the fundamental significance of this point. Bloor's reasons are reasons in name only. When naturalized an arrangement of facts and inference patters will not cause one to believe some theory T in virtue of the fact that one *ought* to believe T, given that evidence. Rather the arrangement of facts and inference patters will lead to a belief in T because of various social factors.

This is a consequence of naturalism. The chief aim of any sort of naturalism is to eliminate norms or somehow explain them away. It's a common aim shared by Bloor and by naturalistic philosophers. And I am as critical of the latter as I am of the former. (Notice another reason to deny that this is boundary work.) I have always thought of Bloor as mainly a philosopher. His philosophical aims come through in his writings and especially in conversations I have had with him. He's a staunch naturalist, more or less a physicalist, and thinks that things such as "reason" and "evidence" (as understood normatively) are just mythical entities on a par with the Easter bunny. He wants only natural causes of belief. Though he is best known for offering social factors as causes, in conversation he expresses sympathy for philosophers who are naturalists and would welcome other types of natural causes of belief such as neural structures which had resulted from a Darwinian process. Of course, he thinks there are norms at work, but these are social entities such as the rule to drive on the right hand side of the road. He categorically denies the existence of objective norms, which is the very thing I categorically assert. Thus, we have a real philosophical debate. And the import of this debate is missed if we glibly say that Bloor believes in reasons, too.

Sismondo has some idea of what is at issue, even if he does not appreciate its full depth. He remarks "Put in terms of the history of science, Barnes and Bloor believe that scientists are pushed by reasons as they occur in the ordinary world, but Brown that they are pulled by a reason-in-the-sky." (Sismondo 2005, 240) Though he put it in a prejudicial way, his finger is on the issue. By way of criticism of my anti-naturalism he says "At the current philosophical moment such a Platonism is an extremely controversial view." (Sismondo 2005, 240) Sismond is right, my view is a type of Platonism and any sort of Platonism is indeed controversial. (Others will protest that one can be an anti-naturalist and hold to objective norms without being a Platonist, but I will let them make their own cases.) The reason Platonism is generally controversial is because of the popularity of naturalism inside philosophy as well as outside. I stress, however, that naturalism is a research programme; so far there is no obviously successful naturalistic account of reason, just a few hopeful suggestions. I won't rehearse my reasons for believing in objective norms here, but rather direct those who are interested to chapter seven of *Who Rules in Science?*

Ideology

Sismondo repeatedly says my book is a work of ideology. He's right. But it is not ideology in defence of the boundaries of philosophy. It is a book by a socialist written for other socialists in defence of socialism. Of course, I hope readers of any political outlook will consider the

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arguments at face value and judge them on their merits. But if anyone wants to know the motivation behind it all, it is political.

I see an intimate connection between social justice and our attitudes to science. My firm belief is that science can help bring about a larger measure of social justice, but only if science is viewed the right way. My aim was to persuade other progressive people to see science as tool for social progress by seeing it as producing (or at least as being capable of producing) relevant facts for social change. In this regard I take postmodern relativism to be a disaster. Though many postmoderns are firmly on the political left, I think their views on science make them hopelessly ineffectual or even worse. (This, by the way, was Sokal's reason for perpetrating the hoax. He was worried that left-wing students were being lead into an intellectual dead-end that would undermine useful social action.)

The outlook of Bloor and other naturalist-minded sociologists of science is, in my view, much too aloof. He would not intervene in the workings of science, but rather, like a biologist, merely observes the flora and fauna without moral comment. I see things very differently. Sismondo repeatedly calls me a defender of science. True, but it needs qualification. What I am really defending is the *possibility* of objective science. While I think much of contemporary science is indeed admirably objective, I do not for a moment think all of it is. There is a great deal of what passes for objective science that is anything but. Medical research, for instance, has been grossly corrupted by the commercial interests of pharmaceutical companies (see Brown 2004). Racist and sexist research continues. The difference between Bloor-type naturalists and those of us who think science can be objective, is that the possibility of objectivity gives us the grounds to act to improve science, for its own sake and for the sake of society. I still recall a conversation I had with Bloor many years ago. I was grumbling about something or other that I saw as bad science. He replied: "The trouble with you is that you are not sufficiently detached." It was all very good natured, of course, but it revealed a fundamental difference in our outlooks. Bloor's naturalism leads almost inevitably to a detached view of science and he gladly embraces it. There is no genuine objective difference between good science and bad, according to this hands-off anthropological attitude. Science is just a natural phenomenon; observe it, describe it; that's all.

I couldn't disagree more with this outlook. And in that sense I am glad to say that *Who Rules in Science?* is a work of ideology. To paraphrase Marx: The sociologists have only interpreted science; the point is to change it. Only those who believe in the possibility of objective science can shun detachment, become engaged, and reasonably expect to make the world a better place. Others are just getting in the way.

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James Robert Brown is Professor of Philosophy at the University of Toronto. His research interests include philosophy of science, philosophy of mathematics, foundations of physics, and the social relations of science. His most recent book is *Who Rules in Science? An Opinionated Guide to the Wars* (2001).

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