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Boundary Work and the Science Wars: James Robert Brown's *Who Rules in Science?*

Are the "Science Wars" Really Wars?

The Science Wars have not involved any violence, nor even threats of violence. Thus the label "wars" for this series of discussions, mostly one-sided and mostly located within the academy, is something of an overblown metaphor. Nonetheless, I will suggest that there are some respects in which the metaphor is appropriate. The Science Wars involve territory, albeit a metaphorical kind of territory. They inspire work that can be best interpreted as ideological, a result of disciplinary interests. Moreover, fellow participants in the wars and others reward that ideological work.

My goal in this is to display efforts to maintain a discipline's epistemic authority, the recognition that members of that discipline have legitimate claims to knowledge on a subject. The central section of the paper takes the form of a discussion of one recent contribution to the Science Wars, James Robert Brown's *Who Rules in Science?* My argument is at least somewhat generalizable beyond this book, and it therefore points to interesting phenomena related to epistemic authority.

Disciplinary Territory

Let me start by motivating the territory metaphor. Epistemic authority is a good that disciplines represent themselves as having, as well as a good that is distributed more or less unevenly among members of a discipline. "Boundary work," is the label that Thomas Gieryn (e.g. 1999) gives for the work of developing, maintaining, and attacking disciplinary epistemic authority. For Gieryn, boundary work occurs in the context of conflicts over claims, approaches, resources, or external issues. In particular, when broad disputes over epistemic authority arise, people attempt to draw boundaries: for members of a discipline to have authority on any contentious issue requires that at least some other people do not have it, or have less of it. Maintenance of a currency of epistemic authority requires maintenance of a boundary inside of which other currencies have only limited value, or whose value depends on a conversion. Moreover, a challenge to the distribution of authority should be as important as a challenge to particular limits, if not more so. That is, holders of authority may have more to lose from a threat to revalue their social capital (Bourdieu 1975; Collins 1998), for example in a challenge to an important doctrine or method, than from another epistemic field's appropriation of subject matter. Thus we can expect boundary work to be one response to doctrinal disputes.

For example, invocations of norms of behavior sometimes become quite prominent in intellectual disputes. Epistemic authority requires not only the right training, but also the right behaviour. When the prominent British psychologist Sir Cyril Burt was accused of fraud, psychologists responded with a variety of inconsistent attempts to maintain the scientific status of the discipline (Gieryn & Figert 1986). Until the full extent of the fraud was appreciated, science was portrayed as a flexible activity, and Burt's dubious practices as examples of practical necessity or even humour. Later, science was portrayed as more rigid, and Burt's dubious practices as deviant. Norms of scientific behavior are subject to flexible interpretation, interpretation that serves specific goals in specific contexts.

Recent authors have pointed to routine boundary work, occurring when there are no immediate conflicts on the horizon (Kleinman & Kinchy 2003; Mellor 2003). Routine boundary work highlights the either ideological or corporatist nature of disciplines, as it more obviously raises questions about motivation for boundary work that are difficult to answer in purely individualistic terms. What motivates a person to perform boundary *work* when there is

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no immediate threat to his or her status or resources? Routine boundary work, more than its conflict-bound counterpart, might seem to present an acute version of the classic free-rider problem, because an individual performing that work appears to have little to gain by it. In the case at hand, conflict is present, but the conflict is very general and the problem of motivation remains. I will argue that in philosophy of science's relation to the Science Wars we can see both disciplinary ideology and corporatism, bound together.

Boundary work is a concept with broad applicability. Norms are not the only resources used to stabilize or destabilize boundaries. Examples, people, methods, and qualifications are all used in the practical and never-ending charting of boundaries. Textbooks, courses, and museum exhibits, for example, can establish maps of fields simply through the topics and examples that they represent. In fact, there is little that does not participate in some sort of boundary-making, since every particular statement contributes to a picture of the space of allowable statements – though clearly some statements and actions are more important as boundary work than are others.

Philosophy of Science and the Science Wars

One of the concerns of philosophy of science has been the problem of characterizing the success of science, usually in terms of a scientific rationality, colloquially known as "the scientific method." Although many philosophers would reject strong versions of this project for one or another reason, almost all have looked to science as an exemplar of rationality, the lessons of which should be extended to other realms. Because of this, it would no surprise if the emerging field of Science and Technology Studies (S&TS) were seen as a threat to philosophy of science. Although the field is hardly monolithic, S&TS has an analogous central concern: among its more or less "philosophical" problems is the problem of understanding how scientific reasoning derives from particular and often local social and material contexts.

S&TS does not stem from another discipline's attempt to do philosophy of science. Though it is often talked about in terms of a contrast between philosophy and sociology of science, few people with sociological training were prominent until the field had some success in revising understandings of how to do sociology of science. Rather, the field stems more from work by philosophers, theoretically minded historians, and scientists turned humanists. Nonetheless, because of the perceived distance between the projects and styles of philosophy and S&TS the standard contrast is maintained. Researchers in S&TS routinely dismiss philosophy of science as concerned with an idealized science, in all standard senses of the term. And philosophers routinely dismiss S&TS, apparently placing "philosophical" work done under that heading outside the bounds of real philosophy of science; this often manifests itself as a kind of unconditional support for science against perceived threats.

As a result, philosophers have made substantial contributions to the Science Wars. In addition to many articles and uncountable glib asides, there have been a number of books that can be read as conservative – in an intellectual or cultural, not a political, sense – boundary work by philosophy of science against a threat from S&TS, including: Philip Kitcher's *Science, Truth, and Democracy* (2001), *The Science Wars*, edited by Keith Parsons, Rebecca Long, and Michael Sofka (2002), Parsons's (2001) own *Drawing out Leviathan* (2001), and Noretta Koertge's (1998) edited collection *A House Built on Sand*. And there are a few non-conservative books of boundary-work as well, of which Helen Longino's (2001) *The Fate of Knowledge* is probably the clearest recent example.

James Robert Brown's (2001) *Who Rules in Science? An Opinionated Guide to the Wars*, is a recent philosophical contribution to the science wars written for a general audience. I choose Brown's book as my example here because it is convenient, and representative in a number of respects. Had I chosen any one of a number of other conservative philosophical contributions to the science wars, my comments would be different, but the lesson about philosophical boundary work would be similar.

Who Rules in Science?

More than the other books mentioned above, Brown's *Who Rules in Science?* appears to be

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written primarily for a general audience, and as a result its faults and its aims may be made more obvious. The book should be taken seriously, though, because it is published by Harvard University Press, it has been widely, and generally very positively reviewed. For example, it is treated very generously by a review in *Philosophy of Science* (Parsons 2002), in *Philosophy in Review* (Wray 2003), and *First Things* (Behe 2004). Prominent Science Warrior Alan Sokal (2003) says that the book is a "superb introduction" and that Brown is "indisputably a trustworthy guide." Indeed, Brown has also been writing on these issues since the early 1980s, and Brown's professions of charity help him to present himself as an even-handed and moderate commentator – some of the other philosopher contributors to the Science Wars are transparently angry and as such might not be taken as writing genuine philosophical works.

In what follows I will focus on the main, primarily negative, part of Brown's book, and ignore the final short section, in which Brown pens some general thoughts about the politics of science. As Steve Fuller (2003) points out, the fact of Brown's discussion of a progressive politics of science is unusual and laudable, even if that discussion is relatively brief, and even if, as Stephen Turner (2003) argues, Brown ends up recommending that we simply defer to scientific experts. My objective here, though, is to give an example of the ideological philosophical boundary work driven by a disciplinary war. Unfortunately, that is easiest to do by drawing out inconsistencies between claims Brown makes about work inside and outside of good philosophy of science, or inconsistencies between the line-drawing Brown does and what we might think of as ordinary good philosophical or academic practice. The display of ideology is most straightforwardly done by exhibiting inconsistencies, and by relating those inconsistencies to interests. It is to this that I turn.

As a matter of full disclosure, my own philosophical work draws heavily on S&TS, even to the point of my recently publishing a comprehensive overview of the field (Sismondo 2004) though I have (until this point) attempted to be a non-participant in the Science Wars. In addition, Brown was a teacher of mine when I was an undergraduate and MA student, and that I have been a fan of his attempts to defend a kind of Platonism in the philosophy of science and mathematics.

The nihilist wing of social constructivism

The core of *Who Rules in Science?* is structured by a contrast between two wings of "social constructivism." The first is a "nihilist" wing represented by Stanley Aronowitz and Paul Feyerabend, with mention of such people as Andrew Ross, Jean-François Lyotard, Jacques Derrida, Katherine Hayles, and a few others. Within this group, Paul Feyerabend is singled out for special treatment, deserving of a sympathetic intellectual trajectory – I would suggest that this is because of his high status within disciplinary philosophy of science. Brown distinguishes the radical but sound early Feyerabend from the radical but unsound late Feyerabend. The early Feyerabend wrote "brilliant" essays, including one of the "masterpieces of philosophy of science." The later Feyerabend took the morals of the early Feyerabend too far. Here is Brown's central case against the later version:

> *Feyerabend's case for anarchy is based on a number of considerations – all rather weak. He takes common examples of methodological pronouncements, then shows that there are numerous examples in the history of science where those rules were clearly violated. Rules such as "Don't accept logically inconsistent theories" or "Don't accept theories that are in conflict with observation" have been breached by all sorts of great scientists. Looking at the course of history, these methodological infractions are happy events. … One of the problems with Feyerabend's case is that the rules he chooses to subvert are very likely not the sort of rules that any serious methodologist would propose, anyway. (Brown 2001, 90)*

Brown goes on to give an example (Quantum electrodynamics) of an important theory that was, and may still be internally inconsistent. Given that this theory seems promising, says Brown, Feyerabend's proposed methodological rule should not be applied to it: "A simple distinction

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between *theories with evidence for their truth* and *theories that seem promising* makes all the difference" (Brown 2001, 91).

I note three things on this. First, there is a Feyerabendian slyness to Brown's response. He implies that QED is a promising theory, but not a theory with evidence for its truth; this is because there may be serious inconsistencies in the theory. Yet QED is not *merely* a theory with promise, as it is a great success story. Second, the sample methodological rules that Brown takes from Feyerabend are blunt, but they are rules proposed by serious methodologists. Feyerabend takes both of these from Karl Popper, his usual target (Lakatos & Feyerabend 1999). Almost all other methodologists treat formal and empirical consistency as virtues in theories, though not exceptionless virtues; Brown himself places considerable weight on them in other parts of the book, and indeed it is difficult to imagine not employing them. Third, Brown's reasoning is essentially the same as Feyerabend's: history is inconsistent with the rule, so the rule is (at least) not universally applicable. The difference is in their conclusions: Whereas Feyerabend challenges anybody to come up with a rule that is *not* broken in exemplary episodes in the history of science, Brown simply asserts that better rules are available. Argumentative moves like these only serve to place Feyerabend on the other side of a boundary of acceptability.

Let me turn to Brown's treatment of Aronowitz. Stanley Aronowitz is not a nihilist in any obvious sense, but it is nonetheless understandable why Brown would not want to put him in the *naturalist* wing of social constructivism, reserved for members of the field of S&TS (under one or another acronym). Brown makes a number of charges against Aronowitz, many of them centering around the misinterpretation of either quantum theory or Thomas Kuhn. In the end, these charges are lessened by the observation that other people, including Nobel Prize-winning physicists, have said similarly silly things about quantum theory. Following Brown's lead, I will set these issues aside.

A separate charge, though, is instructive. Under the heading "An Irritating Style," Brown says that Aronowitz makes controversial claims without any evidence other than a citation. Brown says of Aronowitz:

> *He says that science is not "a steady march toward the truth (Mulkey; Barnes)," then he says that science is "conditioned by these [social] circumstances (Bloor)," "science itself is 'gendered,'" and "The key players and their institutions are the recognized gatekeepers of what counts as science and, more broadly, what counts as truth (Birrer)." On and on he goes in this vein. (Brown 2001, 79)*

This passage is interestingly misleading, and the charge revealing. Besides Brown's removal of dates from the citations, the first of the four short passages above is misquoted slightly. Here is the full sentence from which Brown takes his fragment:

> *The import of the new social studies of science is to have shown that none of these discoveries amounts to a steady march toward Truth (Mulkey [sic] 1990; Barnes 1974). (Aronowitz 1996, 179)*

The discoveries in question are a number of "impressive results" (Aronowitz, 179) mentioned in a previous paragraph. Whatever Aronowitz has in mind here, it should be clear from the capitalized "Truth" that he is not making some trivially incorrect claim.

Brown also neglects to put the citations in for the "science itself is 'gendered'" claim; in Aronowitz's text the claim that Brown appears to be quoting is followed by citations to two feminist works. But let me quote Aronowitz's sentence in full:

> *The claim of science to social neutrality is subject to increasing incredulity since the veritable subsumption, after 1938, of much of U.S. natural science (physics, chemistry, and biology) under the military; since the despoilment of the environment by science-based technologies such as plastics and genetic engineering; in the shameless use of the social sciences in the service of pacification programs in Vietnam; in the dismantling of the welfare state in the name of "public policy;" in controlling workers through industrial psychology; and since the feminist charge that not only have women been occluded when not entirely excluded by science and technology institutions, but that scientific*

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> *knowledge is itself "gendered" (Harding 1986; Keller 1985). (Aronowitz 1996, 182)*

This long sentence contains an understandable argument, if not an entirely convincing one, for questioning science's claim to social neutrality. The first five of his points are commonplaces, some of them perhaps exaggerated in importance. Aronowitz provides citations and marks the controversiality of the sixth point. The statements are certainly sweeping ones, but they are considered and qualified, and supported when most obviously necessary. And it should be mentioned that if Aronowitz does go on and on, it is not in the vein that Brown indicates. Statements of the sort that Brown dislikes are often separated by examples, to which Aronowitz gives space between a sentence and a paragraph.

In the context of a short article ostensibly commenting on the Science Wars, Aronowitz has little choice but to refer to other authors for backing on these points. If we wanted support for these claims, what would we have wanted other than citations? Certainly we can be sure that Brown, and probably most of the rest of us, would have been unsatisfied with a short argument on each of these points; that would almost certainly have been less convincing.

Brown's charge amounts to an assertion of disagreement couched in a less than convincing accusation of poor scholarly practice. Once the claims are contextualized and qualified as they were in Aronowitz's own text, and the misquotes corrected, only the one about the steady march toward Truth is unclear – it deserves explanation, though quite possibly not more evidence were it explained – and all the others so obvious as to hardly need citation at all. Science *is* conditioned by its circumstances. Whether or not it is gendered, prominent feminist scholars *have* charged that scientific knowledge is gendered. Key players and institutions are recognized gatekeepers of what counts as science and what counts as truth. There is abundant support for all of these claims – a perusal of the major journals in the history or social studies of science could confirm them. Interestingly, then, Brown is right that Aronowitz's style of argument is "name-dropping," though because in these cases Aronowitz does not need any real support at all.

In the end, though, Brown dismisses the nihilists much more casually: "Postmoderns who are hopelessly confused by quantum mechanics or chaos may be foolish, but do little harm" (Brown 2001, 95). Note that Brown's point is not put in epistemic terms, but in a political one: harm is the issue. The potential target of harm is unstated; presumably it is the status of the sciences. But perhaps, in comparison to the other wing of social constructivism, postmoderns do little harm to the amount and distribution of authority *of philosophy of science*. That is, the casual dismissal of the supposed nihilists comes because in the policing of philosophical boundaries the nihilists pose far less threat than do the naturalists. I will argue below that this is the most obvious interpretation of their respective treatment. The real targets for Brown, allotted more than twice the space and attention in the book, are not nihilist social constructivists but the naturalist ones.

The Naturalist Wing of Social Constructivism: Externalism and Internalism

The naturalist wing of social constructivism requires less dismissive and more careful treatment. Boundary work there is more difficult. Brown focuses on the work of David Bloor, and Bruno Latour and Steve Woolgar. There is also a short argument about the methodological relativism of Harry Collins and Steve Yearley, references to work by a small handful of other S&TS authors, and three-page descriptions of two additional empirical studies, Paul Forman's 1971 article "Weimar Culture, Causality and Quantum Theory," and John Farley and Gerald Geison's 1974 article "Science Politics, and Spontaneous Generation in 19th Century France." These two empirical studies are important, because Brown uses them to define the naturalist social constructivist program.

Both of these historical studies contain arguments that what we now consider important scientific advances were facilitated by external political events. In the first case, Forman argues that Weimar physicists rejected causality in the sub-atomic realm as a response to strong cultural pressures, before there was any good theoretical or empirical reason to do so. In the second case, Farley and Geison argue that the prize that

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Pasteur won for his paper on spontaneous generation, which effectively ended debate on the issue, was essentially guaranteed by the association of spontaneous generation with dangerous, evolutionist, anti-clerical positions. Brown does not take direct issue with either of these cases, though he strongly suggests that they are flawed by referring to one critical assessment of each of these papers – one might compare this to Aronowitz's making "major claims that are not explained or justified beyond citing some other author" (Brown 2001, 79). And with respect to Forman, he implies that this is lightweight history:

> *One can read about the events, study the experimental data, and laboriously work through the calculations that lead up to the revolution in quantum mechanics in, say, Jammer's history of the period (Jammer 1966). Of its kind it's a fine work, but it's also hard going. By contrast, Forman's account is a real page-turner, with descriptions of the social atmosphere of postwar Germany, Weimar politics, and so on. Social history is often more fun – but that, of course, doesn't mean that it's right. (Brown 2001, 118)*

Forman's article weighs in at 115 pages, was published in a scholarly journal in the history of science, and has hundreds of references. Among those references the first two are to none other than Max Jammer. We can see why from the first paragraph of Forman's paper:

> *In perhaps the most original and suggestive section of his book on* The Conceptual Development of Quantum Mechanics *Max Jammer contended "that certain philosophical ideas of the late nineteenth century not only prepared the intellectual climate for, but contributed decisively to, the formation of the new conceptions of the modern quantum theory" …. United in rejecting causality though on different grounds, these currents of thought prepared, so to speak, the philosophical background for modern quantum mechanics. They contributed with suggestions to the formative stage of the new conceptual scheme and subsequently promoted its acceptance." (Forman 1971, 2)*

Forman goes on to say that Jammer does not substantiate his claims that these philosophical ideas had any effects. His own paper is a refinement, narrowing, and demonstration of one of Jammer's claims. For Forman, "the historian cannot rest content with vague and equivocal expressions like 'prepared the intellectual climate for,' or 'prepared, so to speak, the philosophical background for,' but must insist on a causal analysis" (Forman 1971, 3). Forman, then, positions himself in quite a different relation to Jammer.

The important reason, though, for mentioning these case studies is to describe the use Brown makes of them. These two case studies are used to define social constructivism as the claim that scientific knowledge is fully and directly caused by external social and political forces. On such a view, scientific data and arguments are mere window-dressing for the determination of scientific knowledge by external political and social forces. This bears repeating: Brown characterizes the naturalist wing of social constructivism as claiming that evidence and arguments are irrelevant to the constitution of scientific knowledge. This is because the content of that knowledge is supposedly directly determined by factors external to science. The bulk of Brown's arguments are directed at the inadequacy of this position. That position, though, 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.

In the part of the book devoted to the naturalist wing of social constructivism, Brown describes only three empirical studies, all published before 1980 (a few more are mentioned very briefly). Of these three, the two mentioned above roughly fit his externalist picture of social constructivism, applied to single cases – though both Forman and Farley & Geison are aware of how evidence and arguments were marshaled in their respective cases. The third, Bruno Latour and Steve Woolgar's *Laboratory Life: The Social Construction of Scientific Facts* (1979), is explicitly *internalist*, which Brown does not mention. This is relevant because Brown's main argument against the naturalist social constructivist position is that reasons are causes. Thus, to pay attention only to the external social and political causes of

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knowledge is to ignore an important class of causes.

It may also be relevant to note that Latour and Woolgar explicitly rejected the label "social construction" by changing the subtitle of the second edition of their co-authored book, which became *Laboratory Life: The Construction of Scientific Facts* (1986). Latour has also rejected the substance of what Brown calls social constructivism in a number of publications (e.g. Latour 1990), rejecting even what might be called *methodological* constructivism (Callon & Latour 1992). Latour would be better described as focusing on how interactions and associations of actors, including material actors, create the social world. This is not to say that his position is philosophically straightforward (e.g. Law and Hassard 1999). Woolgar (e.g. 1981) has also rejected the substance of what Brown calls social constructivism, though for very different, perhaps more constructivist reasons.

The Naturalist Wing's Representative: Brown's Bloor

David Bloor famously defines his "Strong Programme in the Sociology of Knowledge" in terms of four presuppositions: it should be causal, impartial with respect to truth and falsity, symmetric in its style of explanation, and reflexive in the sense that in principle its patterns of explanation would be applicable to itself. Of these, Brown says that impartiality and reflexivity are uncontroversial. But causality and symmetry are wrong if we do not accept reasons as causes.

However, neither Bloor nor anybody else denies that reasons are causes, and mid-way through his second chapter on the topic Brown acknowledges this fact. Bloor explicitly assumes reasons are causes, by describing reason as a naturalistic phenomenon. The Strong Programme sets as a goal understanding the social and historical constitution of reason, understanding how groups of people come to accept some things and not others as good reasons in particular contexts. Reason and evidence are crucial to understanding science (and much else), and this is why they are central topics for the Strong Programme and other work in S&TS. Bloor and others hope to bring reason into the causal picture, by understanding it in naturalistic terms: Brown's label, "the naturalistic wing," is apt, though he ignores it.

Here is Brown's Bloor: "[R]eason explanations are taken to be noncausal, hence not natural or scientific. Bloor does not say this in so many words, but it is clearly implicit in all that he does" (Brown 2001, 150). "Bloor was educated at Cambridge University at a time when many of the so-called ordinary language philosophers he might be reading explicitly held the doctrine that reasons are not causes. But these same philosophers also held that reason-explanations of belief are quite legitimate. Bloor seems to have accepted half of this. Yes, reasons are a different kind of thing than causes, but no, reasons are not explanations–only causes can explain" (Brown 2001, 151-2).

Eventually Brown acknowledges his misrepresentation: "[M]any champions of SSK would deny that they are hostile to reason and evidence" (Brown 2001, 156). He follows this with a long quote from Bloor and Barry Barnes that ends with the following:

> *There is no question of the sociology of knowledge being confined to causes* rather *than "evidencing reasons." Its concern is precisely with causes as "evidencing reasons." (Barnes & Bloor 1982, 29)*

But Brown goes on:

> *At first glance this passage makes it seem that reasons are playing an acknowledged role. Yet this illusion quickly fades when we make a distinction between* reasons as causes *and* "reasons" as rhetoric. *… they do not accept a causal role for reason; rather they assert a causal role for reason-talk. (Brown 2001, 157)*

I must confess that I fail to understand how Barnes and Bloor could believe that rhetoric, in this case reason-talk, could have causal power if they do not believe that reasons have causal power; there would have to be some special power of rhetoric that did not depend at all on the content of that rhetoric.

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On the other hand, we have finally reached an important philosophical issue! Barnes and Bloor appear to believe that reasons, seen in naturalistic terms, can have causal power. Brown, however, believes that the important causal power of reason in science is held by a non-natural normative reason. 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. Brown's defense of the borders of philosophy depends not just on the view that reasons are causes, but on his Platonist view that non-naturalized reasons are causes. In the current philosophical climate such a Platonism is a controversial view. It is a view with which most philosophers and most, if not all, of the other philosopher contributors to the science wars would strongly disagree in principle.

I could address some of Brown's subsidiary arguments, but there is little to be gained. The pattern is set by the fact that Brown's reading of S&TS as a field is based on a reading of one 1976 book, *Knowledge and Social Imagery*, by one author, David Bloor; his reading is not responsive to much of the content of that one book, is not responsive to any of Bloor's own objections to this reading, is not responsive to any other theoretical positions within S&TS, and is not responsive to any explicit discussions of the meaning of "social construction." Without providing reasons he dismisses two empirical studies from 1971 and 1974, which he implausibly claims characterize the field; and his discussion is not responsive to a single one of the thousands of empirical studies done in S&TS since 1980. What is more, his central arguments depend on a non-naturalism that is contentious within philosophy, and is not identified as such. Surely all of this, especially in a book that advertises itself as a guide, even an opinionated guide, invites some sort of explanation!

But Is It Representative?

Who Rules in Science? is not the only example of such philosophical boundary work. Philip Kitcher's *Science, Truth, and Democracy* (2001) is a good reference point. Kitcher's book is more tightly argued than is Brown's, and appears to have been written with a goal of contributing a philosophical perspective (on the politics of science) as well as protecting territory. However, Kitcher's targets in S&TS are almost exactly the same people as Brown's: Sandra Harding, Stephen Shapin, David Bloor, Harry Collins, and Bruno Latour. Kitcher never describes the positions that any of these people take, simply grouping them together as "would-be debunkers" of science, a position he characterizes with a small amount of hand-waving. He does not demonstrate that any of the writings of these people should be read as attempting to debunk science, a curious fact given that all of these people have been quite explicit that they are pro-science; of these only Sandra Harding (1991) has a developed criticism of science as it is currently practiced.

For Kitcher, S&TS sets out to understand how scientists socially resolve the problem of under-determination. Data does not uniquely determine theories, so something else supplements the data so that scientists can agree upon theories. Though it captures only one of the S&TS's problematics – ignoring questions about the material cultures of research, the social structures of science and technology, the nature of expertise, forms of communication, etc. – Kitcher's characterization is not unfair. But he moves immediately to seeing this in terms of a debunking of science. Rationality is supposedly *replaced* by "values" or by the debunkers' "own psychosocial explanations" (Kitcher 2001, 31). Kitcher takes the project of the would-be debunkers to be one of showing the irrationality of science, rather than to show the social grounding of science's rationality; as we have seen, Brown does the same thing, overriding the explicit and obvious claims of his opponents.

Consistent with his strong commitment to naturalism, Kitcher insists that he understands science to be both rational and social (see especially his 2002). Nonetheless, he appears to mean by this that there are both social and rational ingredients in science, not that rationality can be understood as a social phenomenon. For example, he says such things as:

> *A little history is good. We come to appreciate that Galileo, Lavoisier, and Darwin were not*

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> *opposed only by ignoramuses, bigots, and fuzzy minds, but by intelligent defenders of alternative views who supplied challenging arguments. More history is better. For when we look more closely at the course of historical controversies, especially if we undertake the currently unfashionable work of analyzing the lines of reasoning, we discover that the disputes evolve, that positions are modified and new options emerge, that some problems are solved and others are generated. We find, in fact, that what was at one stage an impasse that made the tentative adoption of either of the rivals reasonable turns into a situation in which the balance of evidence is clear. (Kitcher 2001, 39)*

"Lines of reasoning" are here sharply distinguished from the stuff that would-be debunkers discuss. What is more, Kitcher here asserts, without citation or other evidence, that these lines of reasoning resolve controversies. Lines of reasoning in this context appear to have been purified of any contact with the material or social world. Curiously, this supposedly naturalist position is almost exactly the same as Brown's non-naturalist one: Kitcher's "lines of reasoning" arrive in the material world unheralded in the same way that Brown's "evidential reasons" do. Again, we have what appears to be a controversial philosophical thesis employed to police the boundaries of (good) philosophy.

But Is It Philosophy?

My claim is that in the light of scholarly oddities such as those summarized above, it is hard to read *Who Rules in Science?* as other than an ideological and territorial work. It is an attempt to place philosophy with respect to the interests of the natural sciences, to place or reinforce bounds on good philosophy of science, and to stipulate some types of work as outside those bounds.

The evidence that *philosophy* is at stake is a circumstantial one. No case needs to be made that philosophy of science and S&TS are competing for some similar terrain. Not only is there competition, but a number of members of both fields have acted as though it is worth attacking the other field as a whole.

Internal to the book, when Brown identifies from where the potential for "harm" comes (Brown 2001, 95, see above), he dismisses the so-called "nihilist" cultural critics, and focuses on the more analytical naturalists. The latter's studies of science as a socially and materially situated activity threatens the purity of dominant images of science, including philosophical images. But while the nihilist wing is often focused on normative issues, the naturalist wing engages primarily in analytic activities, only sometimes using that analysis as a platform for normative claims. Within S&TS, there have been debates over the place of normatively engaged work, but all of the authors Brown mentions stand firmly on the analytic side of the field, not one of them having made any substantial normative claims about science, and some of them having explicitly argued against politically engaged studies (e.g. Collins 1996). Brown's primary target, David Bloor, has consistently argued that rather than being anti-science, his program is explicitly scientistic (Bloor 1991, 160). Thus the balance of importance and attention that Brown gives to the naturalist wing over the nihilist wing is at least superficially better explained by the naturalist wing's challenge to philosophy of science than by its challenge to either progressive politics or to the sciences – though it might be argued that despite its non-normative stance the naturalist wing's challenge to the "positivist" (in a colloquial sense) image of science is ultimate more threatening to science than is the nihilist's more engaged position.

Philosophical views of science are without doubt the focus of Brown's attention, though these are distinct from philosophy as a discipline. He is willing to imply (i.e. without evidence) that various historical or sociological treatments of science are impoverished, but it is only those positions that are articulated as more or less philosophical against which he argues. Finally, that philosophy is partially at stake is borne out by some specific comments that Brown makes. For example, in one of the few places in the book where he explicitly criticizes a clear ally in the Science Wars, it is because of that ally's anti-philosophical stance. About physicist Steven Weinberg Brown says:

> *In his …* Dreams of a Final Theory, *Weinberg attacks all philosophers of science, not just*

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> *social constructivists. "The insights of philosophers have occasionally benefited physicists, but generally in a negative fashion—by protecting them from the preconceptions of other philosophers" (1992, 166). Saving us from bad philosophy of science is the one area in which Weinberg allows philosophers to excel. When they read Weinberg's philosophical remarks on truth, most philosophers of science, I dare say, would be delighted to exercise this skill for his benefit. (Brown 2001, 21)*

Brown jumps to the rescue of philosophy here, rather than science.

As of this writing, *Who Rules in Science?* has been reviewed at least four times in philosophical journals, and thrice more by identified professional philosophers: Keith Parsons (2002) reviews the book in *Philosophy of Science*, Joseph Rouse (2003) in *International Studies in the Philosophy of Science*, K. Brad Wray (2003) in *Philosophy in Review*, Miriam Solomon in the history of science journal *Isis* (2002), Steve Fuller in the *British Journal for the History of Science*, Steve Turner (2003) in *Social Studies of Science*, and Michael Ruse for Harvard University Press. Ruse gives a glowing positive endorsement for the publisher; any other content of that review is not publicly available. Fuller does not engage with any aspects of the book discussed here; instead he applauds and reflects on Brown's questions about the politics of science, and uses the review as an occasion to criticize his colleagues in S&TS for their apolitical stances and their neglect of progressive politics. Solomon criticizes Brown for his primarily conceptual arguments, and describes the book as a useful indication of what a logical empiricist would think of the Science Wars. Rouse's review is sharply critical, citing scholarly lapses and misinterpretations, including many not mentioned here. Turner's review – which I unfortunately became aware of late in the process of writing this article – is also sharply critical and makes some similar observations as are made here, though its main focus is Brown's discussion of the politics of science. Solomon, Rouse, and Turner have engaged positively with S&TS, and thus we might guess that boundary work here is a two-sided phenomenon.

Parsons's review is noteworthy for its prominent location and its strong endorsement of the book. Parsons has written his own book on social constructivism, *Drawing out Leviathan* (2001), in the context of a discussion of controversies over dinosaurs. For Parsons, "Brown's criticisms of both wings of social constructivism are clear, cogent, and concise" (Parsons 2002, 647). He commends Brown's "admirable commitment to fairness and evenhandedness, even when dealing with extreme positions," and says that this "should gain a hearing for his arguments even in the opposing camp. Preaching to the choir has been all too common in the science wars." Along these lines, Parsons contrasts Brown's book with a recent book by mathematician Norman Levitt (1999): "Whereas Brown seeks the high ground … Norman Levitt is an unabashed science warrior" (Parsons 2002, 648). Parson's only substantial criticism of *Who Rules in Science?* is that Brown is too positive about feminist science studies.

Who Rules in Science? is not only about philosophy, but is a sally in the Science Wars. It might be unsurprising, then, that its main concern appears to be territorial. Wars, after all, almost always concern one or another form of territory. The book is thus not only doing philosophical boundary-work in the disciplinary sense, but boundary work about philosophical views of science, in the context of a broad dispute over such views, a dispute that often appears to turn on the authority of different groups of people to comment on science. The book is a representation of philosophy to a large group of people who stand outside it but are keenly interested in who has what to say about science. Brown might be seen as reaffirming philosophy's unconditional pro-science stance. He might be seen as defending the purity of philosophy, against the possibility of taint by the more philosophical sides of S&TS.

Who Rules in Science? has been reviewed widely in serious popular venues such as the Toronto *Globe and Mail*, the Manchester *Guardian*, *Commentary*, *The Times Literary Supplement*, and *Scientific American*. Perhaps because of such attention, the book has sold well, at least as suggested by Amazon's sales statistics. Almost all of these reviews have included extremely positive assessments of Brown's

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positions *vis a vis* constructivism, though some criticize Brown's progressive politics. They are written by apparent non-specialists, though in some cases by other participants in the Science Wars – Alan Sokal's (2003) enthusiastic endorsement in *Science & Society* is a case in point. Sokal's only criticism of the book is on the issue of naturalism, though Sokal does not indicate the importance of this issue to Brown's main argument.

Defending Disciplinary Terrain

I offer a few claims drawn from this episode.

1. In the context of large disputes, such as the Science Wars, the boundary work done by members of a discipline can fail to meet the basic standards and norms of that discipline. When people discuss the work of competing fields, misrepresentations on a scale that would never be acceptable within their own field are perfectly ordinary.

2. Such boundary work is easily interpreted as ideological. It is unlikely that a well-regarded philosopher of science would set out to misquote and misrepresent opponents. This is even more sharply seen in the fact that a reviewer like Parsons completely fails to mention any of the conceptual or scholarly problems with which the book is afflicted. The generally positive reception for books like *Who Rules in Science?* in philosophy strongly suggests that philosophers are willing to take negative portrayals of competing fields at face value, and are uninterested in interrogating them closely. Those who do interrogate them, such as Rouse, Solomon, Turner, and myself for that matter, already have constructive engagement with the competing field. Reactions in print to Science Wars tracts seem to follow pre-existing patterns, patterns that might be called ideological.

3. These patterns are explicable in terms of disciplinary ideology rather than individual interests. Boundary work is not merely motivated by the development or maintenance of individual epistemic authority. I return to the point that boundary work is work. None of his targets poses a threat to Brown's status as a philosopher of science, or to the status of any philosophical positions in which he has invested effort (that is, other than ones connected with his participation in the Science Wars); these are insulated from general disagreements between mainstream philosophy of science and S&TS by differences in style and topic. So Brown's work, and the boundary work of other philosophical contributors to the Science Wars, is better seen as part of a general dispute than as produced by narrow interests.

4. On the other hand, there is a niche for boundary work, and it can be well rewarded. This is meant in a very general sense: we can assume that for (at least established) academics the primary currency of reward is recognition, or in other words that the monetary economy is subsidiary to the recognitional economy. If we accept this, and the metaphor on which it depends, then the niche is simultaneously an intellectual one and also a kind of economic niche.

Space in major journals is given over for reviewing contributions to the Science Wars. Such works are recognized as important intellectual work in philosophy of science, because they are about the proper edges of the field. In fact, because they are about the field they can, and usually do, receive a tremendous amount of attention. For example, the journal *Philosophy of Science* gave over a substantial portion of one issue to allow Philip Kitcher (2002) and Helen Longino (2002) to each review the other's recent book, and to comment on each other's review. (I would strongly encourage people who question my analysis here, or would want to take it further, to read these telling reviews.) This is unusual in a journal that normally contains focused research articles.

There is even more attention given to such books outside of the field. As mentioned above, *Who Rules in Science?* has been reviewed in *Isis*, *Science & Society*, *Metapsychology*, *The Times Literary Supplement*, *Commentary*, *Scientific American*, the Manchester *Guardian*, the Toronto *Globe and Mail*, and other venues. While old-fashioned understandings of the moral economy of the university would heavily discount such recognition, I would argue that for many of today's academics recognition outside of their home discipline counts for as much as or more than recognition within it.

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There is, moreover, a substantial and diverse market for sales of books like *Who Rules in Science?* From the locations and identities of the reviewers and commentators on the book, it appears that it is being read by psychologists, natural scientists, historians, and avid readers outside the academy. Even while they appear to be waning, the Science Wars sell well. Many people enjoy reading a good fight, and enjoy reading apparent refutations of views of which they are suspicious. Publishers thus have good reason to publish them.

There is an intellectual danger for philosophy here. To the extent that philosophy of science defines itself in blunt alignment with one camp in the Science Wars, and denies alternative views the status of philosophy, it deprives itself of intellectual diversity. If my observation of the place of ideology in such disputes is right, then the discipline's judgments of the reasonableness of positions will tend to become increasingly unresponsive to the kinds of evidence that outsiders value.

I close with a short passage from Bruno Latour. This passage, an aphorism in a colourful argument, is quoted a full *three* times – and crucially misquoted all three times, an observation I owe to Turner (2003) – in the course of *Who Rules in Science?*, the first time as an example of something bound to irritate scientists, the second and third times as evidence of social constructivism's cheerful dismissal of reason. It is a surprising aphorism, particularly because it appears internally inconsistent. Taken out of context it is not only surprising but an apparent indication of naturalist social constructivism's descent into nihilism. However, it is easy to read this passage as part of an argument for the naturalization of reason. That task is made especially easy given the cross-reference to another aphorism, which Brown deleted when he quoted it, explicating reason and work in materialistic terms. What we have is an insistence that actual reasoning is a part of the material world, not a free-standing Platonic world. That is undoubtedly right, both in general and in terms of boundary work in the Science Wars. About this case, though, its more colourful aspects, are perhaps more apt than Latour originally intended.

> *2.1.8.4 "Reason" is applied to the work (2.5.4) of allocating agreement and disagreement between words. It is a matter of taste and feeling, know-how and connoisseurship, class and status. We insult, pout, clench our fists, enthuse, spit, sigh, and dream. Who reasons? (Latour 1988, 179-80)*

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