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REVIEW

Kinder, Gentler Science Wars

Gabriel Stolzenberg

Jay A. Labinger and Harry Collins (eds), *The One Culture? A Conversation about Science* (Chicago, IL: University of Chicago Press, 2001), 329 pp., US$65.00 ISBN 0–226–46722–8 (hbk), US$18.00 ISBN 0–226–46723–6 (pbk).

If, like many intellectuals, some of them eminent, you cheered the hatchet jobs of the science wars, then you may be bored by this mostly civil, often thoughtful and sometimes thought-provoking conversation about the social studies of science and the science wars. Indeed, to hear the editors tell it, it could have been called 'Not the Science Wars'. In it, 'It makes me laugh' is not an argument. Nor, more generally, is 'There is only one reading of a text, so if I have one, it must be the author's'. Of course, nobody really believes that 'It makes me laugh' is an argument or that there always is only one reading of a text or that context does not matter. Yet the science wars are, in large part, a story of otherwise smart people, both participants and observers, behaving as if they did believe these and similar absurdities (Stolzenberg, 2001).[1]

The subject of misreading and its causes figures prominently in the conversation. Two essays by the physicist, David Mermin, present a series of instructive misreadings, some of him, others by him. For example, in 'Reading and Misreading', he describes in detail how a copy-editor at *Nature* transformed an article of his into one that, in his opinion, badly misrepresented what he had submitted. Despite his protests, it was published under his name and *Nature* refused to print a correction. Although Mermin calls this 'the dirty trick *Nature* played on me', he does not attribute the action of the copy-editor to malice or idiocy but, rather, to 'strongly ingrained ways of thinking' that resulted in an inability to allow that 'something more subtle than merely clumsiness or error' might conceivably have been behind his words. But Mermin also knows that if the science wars can teach us anything, it is that misreading caused by strongly ingrained ways of thinking is a risk for each of us, not just copy-

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editors. 'We should all be on guard against it', he says. Indeed we should, but how?

This is not a rhetorical question. Consulting with the author is often an obvious first step. However, science warriors usually prefer other ways of 'making sense' of a text. For example, the philosopher Paul Boghossian (1998: 27–29) asks how we are to make sense of a certain 'postmodern relativist' looking statement by the anthropologist, Roger Anyon. But, instead of asking the author what he means by it, Boghossian considers three different ways of understanding it,[2] which he implies are the only ones, and finds it wanting on each. Yet, given his philosophical sophistication, had he not been doing his Ken Starr imitation, even without consulting with the author, it would have been easy enough for him to invent a reading that, unlike any of these three, is plausible and not vulnerable to his criticism (Stolzenberg, 2001: 50–53).

Six of the participants in the conversation are physicists, two of them Nobel Laureates. There are also five sociologists of science, one of them a former planetary scientist, an ethnomethodologist who studies how conceptions of nature and society feature in the contents of science, a chemist who talks to humanists, a geochemist turned historian of science and a historian of science and mathematics. Most have interesting and original things to say and they say them well – in some cases, extremely well. The book has a novel format. It begins as an ordinary collection of essays with each participant, or two working together, contributing a chapter. But instead of ending there, it continues with everyone having a chance to comment on everyone else's chapter and finally to reply to criticism and offer concluding thoughts. It works. The result is a conversation or, at least, the beginning of one.

It too contains misreadings and faulty logic, more than I thought the editors would tolerate. But most of it seems to be due to incompetence, or at least carelessness, rather than to biasing habits of thought. Furthermore, many second-round misreadings of first-round statements are exposed in the third round, usually by the person misread. However, for third-round misreadings, there are only the traditional remedies. Thus, when, in the second round, the sociologist, Harry Collins, criticizes two physicists, Jean Bricmont and Alan Sokal, for posing a silly challenge in the first one, they point out in the third round that they do not pose that challenge, leaving Collins looking a bit foolish. However, when another sociologist, Jane Gregory, makes a remark about the same two physicists, based upon the fact that the things we would call true are precisely those we believe, and they misread it as a misrepresentation of their concept of truth, the misreading occurs in the last round and, hence, goes unpunished.[3]

Although its range is broad, much of the conversation is about the Strong Programme in the sociology of knowledge, a hallmark of which is that the truth of a belief plays no role in explaining what makes people believe it. By contrast, social factors play a major role. However, the Strong Programme use of the term 'social' courts misunderstanding. One cannot consistently use it in the ordinary way, as practitioners often seem to do, yet hold, as they must, that scientific belief, practice and discourse are social through and through. In this conversation, three sociologists and a historian defend, or at least explain, the Strong Programme, partly in ethical terms. Three physicists, two of whom wish to see it dead and one who merely finds it boring, crippling and wrong-headed, attack it. Two other physicists, a chemist and an ethnomethodologist, each of whom wishes to reform Strong Programme practice but not kill it, attack the attacks and offer strong but collegial criticism. Finally, a physicist and a historian of science challenge sociologists to describe and explain the steadily increasing accuracy of certain scientific measurements over decades and even centuries.[4] The attacking physicists are Jean Bricmont and Alan Sokal, writing together, and Steven Weinberg. The defender–explainers are the historian Peter Dear, and the sociologists Harry Collins, Trevor Pinch, and Steven Shapin. The collegial critics are the physicists David Mermin and Peter Saulson, the chemist Jay Labinger, and the ethnomethodologist Michael Lynch. The descriptive–explanatory challenge is issued by the physicist Kenneth O. Wilson and the historian of science Constance Barsky in their chapter, 'From Social Construction to Questions of Research', which they say is a sequel to Gottfried and Wilson's 'Science as a Cultural Construct' (1997).

But in what sense is Gottfried and Wilson's 'repent or die' polemic against the Strong Programme a precursor to Wilson and Barsky's scholarly proposal for a program of research in the sociology of science? The authors don't say but perhaps their thought is that Gottfried and Wilson debunk slogans like 'reality is a social construction' that seem to deny that science has any objective validity and Wilson and Barsky consider the extent to which particular scientific statements, especially certain measurements, do have objective validity. However, for me, a more significant connection is that both essays are informed by an unhelpful pseudo-theoretical use of expressions like 'culture-free' and 'social construction' that are currently popular among physicists and some philosophers, who mistakenly assume that it is also how they are used in the Strong Programme. When a normally plain-speaking physicist was quoted on the front page of the *New York Times* saying, about some exciting discovery, 'This definitely is not a social construction', was it just a pretentious way of saying 'There's no doubt about it' or did he mean something more? I don't think it was either but I do think that he *meant* to mean something more – just as Wilson and Barsky, Saulson, Weinberg, and others surely do when they talk about scientific statements being more or less 'culture-free' or 'culture-laden'. They doubtless have their intuitions, however ineffable, but how different are they likely to be from those that inform Roger Penrose's physicist musings about mathematics (Penrose, 1989: 94–98), according to which clunky-looking mathematics is a human invention (social construction), whereas elegant, definitive-looking mathematics is a discovery about a realm independent of us (culture-free)?

Besides the debates about the Strong Programme, there are discussions of topics ranging from cold fusion to the allegedly excessive respect of physicists for the general theory of relativity to Hume's advice about reports of miracles to mad cow disease. There is also a wise and sometimes hilarious report by sociologists, Jane Gregory and Steve Miller, on what the public and science think about each other, an essay by Steven Shapin arguing that antiscience is less a matter of *what* is said than *by* whom and *to* whom, a proposal by the ethnomethodologist, Michael Lynch, for treating the science wars as 'an educational opportunity', and much more.

Lynch (2001a) portrays the science wars as a metaphysical conflict. This may seem rather grand, but he is quick to explain that most participants are amateurs using pop metaphysics and sandlot philosophy. He does not mean this as a criticism. Indeed, he is neither for ending the conflict nor for seeking 'professional' help.[5] He is, however, for raising the level of argument and his thoughtful analyses of pop metaphysical one-liners by Sokal, Weinberg, and Richard Dawkins are elegant illustrations of how this can be done. Less thoughtfully, in my view, Lynch (2001b: 271–72) endorses Weinberg's anti-intellectual conceit that 'philosophers may be able to help us to sharpen the way we understand words like "real" and "true" and "cause", but they have no business telling us not to use them' (Weinberg, 2001b: 240).[6] It is understandable that Weinberg would prefer to be sheltered from criticism. But as long as science warriors feel no pressure to scrutinize their own metaphysical views carefully enough to recognize how the muddles that they contain are projected onto the relativist and constructivist views that they think they are debunking, the discussion is dead in the water. If Weinberg or anyone else uses pop metaphysics, wittingly or not, to misrepresent or ridicule views of others or to disguise his own metaphysical mess, he has no business telling us that we have no business telling him to cut it out.

I suspect that one reason Lynch and I disagree about Weinberg's statement is that I do but he does not see the conflict in terms of right and wrong. However, when I say that I see it *in terms of* right and wrong, I do not mean that one of the two mindsets that dominates it is right and the other is wrong.[7] I mean rather that there is a right way and a wrong way to listen to what is said from within each of them. The right way is obviously to be in the same mindset as the speaker and the wrong way is to be in the other one. What saves this from being banal is that it is possible to learn how to shift between the mindsets, more or less at will. But listening in the right way is only part of getting it right. One can listen in the right way to a metaphysical statement, even one's own, and still misunderstand it. I see the science wars as a metaphysical conflict dominated by people with a strongly ingrained habit of listening the wrong way (mostly the same wrong way), many of whom are also badly confused about their own position. The main reason I doubt that Lynch would agree with this picture of things is that none of his suggestions for improving the quality of discourse in the science wars addresses it.[8]

For a case of wrong-way listening that is 'as old as the hills', nothing beats the traditional realist argument that relativism is self-defeating.[9] It begins by assuming falsely that, far from being a different mindset, relativism is a claim, perhaps, 'Every claim is relative', that is made in the realist mindset. The argument that relativism is self-defeating then proceeds by noting that if the claim does not apply to itself, it is false and if it does apply to itself, it is relative. In either case, it is not an objective truth. Therefore, no argument can rationally compel its assent: it can be ignored.

Moreover, if the relativist were to object that she does not see how *any* argument could rationally compel anyone to adopt either a relativist or realist mindset, far from causing the realist to reconsider his understanding of relativism, it would probably reinforce his conviction that it can be ignored. Thus, one realist philosopher says, 'If [the relativist] invites us to join him, we need not offer any reason for declining, since he has offered us no reason to accept' (Nagel, 1997: 15). In sum, for the realist, the claim of relativism is defeated because no argument can compel us to accept it; whereas, for the relativist, who never claimed to have a claim, much less an argument, this is no more a defeat for relativism than is the consideration that no argument can compel us to look through a telescope a defeat for astronomy.

In their opening essay, Bricmont & Sokal (2001) contend that the Strong Programme cannot succeed because, in some cases, to explain what makes somebody believe something, it is necessary to appeal to the fact that the belief is true. By way of proof, they present two examples that allegedly are of this kind.[10] But in another opening essay, Pinch (2001: 19) assures us that explanations of 'the emergence of truth by reference to its truthfulness' are circular,[11] which would appear to contradict the claim that the examples of Bricmont and Sokal are supposed to illustrate. However, Pinch merely says that there is such circularity. He makes no attempt to prove it. Nor does he try, later on, to find any in the examples of Bricmont and Sokal. Nor, for that matter, does anyone point out that both examples are question begging. Here are the two beggars.

> No one today knows the details of the causal mechanisms but it seems obvious that part of the explanation [of why someone standing in the rain says, 'It is raining today'] involves the fact that it really is raining today. (Bricmont & Sokal, 2001: 40)

> Certainly *some* part of the explanation (and a rather important part at that) [of why the European scientific community became convinced of Newtonian mechanics sometime between 1700 and 1750] must be that the planets and comets *really* do move (to a very high degree of approximation, though not exactly) as predicted by Newtonian mechanics. (Bricmont & Sokal, 2001: 40)

In each of these statements, Bricmont and Sokal claim that the truth of a certain belief is a partial cause of its acceptance.[12] However, contrary to what their easy talk about 'the' explanation might seem to suggest, it does not follow, logically, from this alone that an adequate explanation of the acceptance of this belief must include its truth as a partial cause. To reach that conclusion, one must also assume that every adequate explanation of it must contain *all* of its partial causes. But it is implausible that a finite explanation of an event or phenomenon could contain all its partial causes and Bricmont and Sokal give us no reason to think otherwise. Indeed, in their first example, they omit the many partial causes of the fact that it is raining today even though, if the fact that it is raining today helps make someone believe it, then so does every partial cause of it.[13] So, even if the first claim is correct, this attack on the Strong Programme is not.

But is the first claim correct? Bricmont and Sokal believe it is but they have no argument. In their first example, they merely preface the statement to be proved by 'it seems obvious that' and, in their second one, by 'certainly'. Is this the kind of scholarship that they and their admirers favor?[14] In the next round, Bricmont and Sokal explain that they have in mind cases in which the truth of a belief is a partial cause of what is recognized to be evidence for it. On first sight, this may seem promising: the truth helps make there be evidence, and observation of the evidence helps make us believe. But to carry this out in any case, one must show that the *fact* that it is evidence, which is a state of the world, is a partial cause of the *belief* that it is evidence, which is a state of mind. Not only do Bricmont and Sokal fail to do this, they write in a way that conflates the two states, if not in their minds, at least in their words.

Although the examples of Bricmont and Sokal do not refute the Strong Programme, they are informed by a powerful intuition that seems to run counter to the commonplace that authenticity is irrelevant for belief causation. For example, because of my past experience, whenever one side of a sheep looks white to me, I believe that the sheep is white. So, when it *is* white, my belief is true. Yet the fact that it is true seems to play no role in making me believe it. Nevertheless, Bricmont and Sokal might say, 'Look, the sheep *need not* have been white but, in fact, *it was*. And because it was, the side that you saw was white. And although nobody knows all the details of the mechanism of color perception, the fact that the side you saw was white obviously played an important role in making it look white to you, which, because of your past experience, made you believe that the sheep was white'.

However, this clash of intuitions seems to disappear if unlike Bricmont and Sokal, who claim that we *must* use the truth of certain beliefs to help explain their acceptance, we read their examples as attempts to show only that sometimes the truth of a belief *can* help explain its acceptance. In addition, because the irrelevance of authenticity applies to any possible partial cause of a belief, not just its truth, the Strong Programme's social causes also are not necessary. But Strong Programme practitioners never said they are.

In his chapter, 'Physics and History', Steven Weinberg has three criticisms of the Strong Programme. His two 'minor' ones are that by refusing to use current knowledge and perspectives to illuminate the past, it cripples historical research and it is boring (not fun). His 'major' criticism is that, because the Strong Programme is not teleological, it misses what he believes to be the point of the history of science. To support his first charge, Weinberg attempts to use current knowledge to help explain why, in a celebrated set of experiments in 1897, J.J. Thomson favored the higher values among the measurements he made to estimate what we would now describe as the ratio of the mass of an electron to its charge. However, unless I badly misunderstand it, his reasoning is suspect from the start. Without offering any justification, Weinberg considers only two possible explanations, the first of which is maddeningly imprecise and seems to be introduced only so that it may be slain by the sword of current knowledge, leaving his favorite the winner by default.

> Why did Thomson quote the high values as his favorite values? It is possible that Thomson knew that on those days he was more careful, or hadn't bumped into the laboratory table, or had had a good night's sleep the night before. But there is another possibility: that his first values were at the high end of the range and he was determined to show that he had been right at the beginning. Now which explanation is correct? (Weinberg, 2001a: 121)

Weinberg believes that our knowledge of the true value of the ratio that Thomson was trying to measure can help to answer this. Using it, he finds that the values Thomson favored are not more accurate than his other ones. In fact, they are more than twice the true value whereas some of the others are fairly close to it. But Weinberg also notes that the favored values do support Thomson's first measurements. To him,

> [This] strongly suggests that the measurements that gave high values were not in fact more careful and that therefore it is more likely that Thomson quoted these values because he was trying to justify his first measurements. (Weinberg, 2001a: 121)

And yet, if Thomson favored his higher values because they support his first ones and he was 'determined to show that he had been right at the beginning', why did he not even mention this support in his famous article about these measurements (Thomson, 1897)? Doesn't this also need to be explained? Also, why is it such a good thing for a statement to be more likely than one that has been shown to be unlikely?

Note also that Weinberg's elimination of his first hypothesis requires that when he writes, 'in fact more careful', he means 'more accurate'. Yet the latter seems inconsistent with his talk of not bumping into the laboratory table or getting a good night's sleep. Also, if 'in fact more careful' does not mean 'more accurate', then Thomson's higher values could be more careful without being more accurate, merely because of a systematic error in the experiment that yielded them (and only them).

Did Thomson really favor any of his values? Weinberg presents no evidence of it. Worse, he does not even tell us in what the alleged favoritism consists. In his paper, Thomson does say (p. 310) that his second method of measuring, which yields the higher values, is 'much less laborious and probably more accurate'.[15] But whether or not this is favoritism, it has no bearing on what Thomson takes to be 'the value' that is yielded by his entire set of experiments. It is not really a value or even an average of values, each of which Thomson calculates to two places, but rather an order of magnitude estimate of the average value of all his measurements.[16]

Later in the conversation, apparently without knowing that Thomson had suspected that his higher values were more accurate, Collins (2001: 192) proposes an explanation of the alleged favoritism for which this suspicion provides support: Thomson 'might have thought he was being careful while he was actually making mistakes of which he was unaware'. Weinberg graciously accepts this as an 'interesting alternative', apparently unaware that, because Collins' proposed explanation of Thomson's favoritism (he thought he was more careful) subsumes the one that Weinberg eliminated by an appeal to current knowledge (he *knew* he was more careful) but cannot itself be ruled out in this way, he has given away the store.

Finally, Weinberg is not always careful about which questions he is or should be considering. Is it only the question of why Thomson favored his higher values? Or is it both this and the question of why the higher values were not more accurate? Initially, he poses only the first question, as he should. But when, in his parting shot, he points out that Collins could not have guessed his answer without knowledge of 'the modern value', he does not seem to recognize that this is true only for the uninteresting reason that Collins' answer is to both questions, the second of which is in terms of the modern value.

The collegial critics, Labinger and Mermin, agree with Weinberg that Strong Programme practitioners ignore clues to the past provided by current knowledge and perspectives. In 'Conversing Seriously with Sociologists', Mermin sees such a clue in the importance that modern expositions of electromagnetism attach to the coherence that special relativity brings to it. He suggests that if Collins and Pinch had been guided by this consideration when writing about the early history of relativity for *The Golem* (1993: 27–55), they would have been more likely to recognize that its success in electrodynamics provided a powerful rationale for ignoring Dayton Miller's claims to have refuted it.

In the same chapter, Mermin looks back at his exchanges with Collins and Pinch about the history of science and at the evolution of his understanding of their perspective. He reminds us that, in his two-part review of *The Golem*, he said that he did not see how a lay reader could fail to conclude that the theory of relativity is fraudulent (Mermin, 1996a, 1996b). It then seemed to him that Collins and Pinch had failed to grasp that 'even though many clues in a complex network of evidence will always be far from definitive, the probability of a conclusion supported by a multitude of interlocking mutually reinforcing clues can still be close to certainty' (Mermin, 1996a: 13). However, in their reply, Collins and Pinch contended that they made 'much the same point' in *The Golem* (1993: 53) when they wrote: 'No test viewed on its own was decisive or clear cut, but taken together they acted as an overwhelming movement'. Mermin now seems to accept this. Indeed, writing more generally about his initial disagreement with Collins and Pinch, he says:

> I'm struck by how much less we disagree than we appeared to at the time . . . . I now regard as a relatively minor matter what once struck me as the central question: whether the construction of scientific knowledge should be viewed as a process of discovering how nature works or as a process of consensus building among scientists. I'm increasingly persuaded that any issue one can formulate in one language has a parallel formulation in the other. (Mermin, 2001: 82–83)

Peter Saulson has a similar view. He sees the debate as a battle of straw men, pointing out, for example, that the idea of a crucial experiment, which Collins and Pinch debunk, is too easy a target for a critique of it to give much insight into scientific practice. To Saulson (2001: 79), the dispute 'appears to lie chiefly over whether the language used to describe [how scientific ideas are accepted] emphasizes the attempt to fit together many partial pieces of knowledge, or whether [it] emphasizes that people with different interests are trying to do the fitting'. He thinks 'the "interests" that Collins ascribes to scientists in the network as they "negotiate" the acceptance of a new idea are for the most part a human embodiment of exactly the same process that Mermin would describe as the actions of individual threads in the "tapestry" of science'. Finally, Saulson asserts that Strong Programme case studies would be enriched if 'the primarily *intellectual* nature of the interests involved were made plainer'. I agree.

But it is not only a matter of enrichment, however important this may be. Failure to do justice to the intellectual nature of 'the interests involved' encourages readers to assume, falsely, that the Strong Programme's notion of 'social' does not penetrate fully into the intellectual content of scientific research and even offer, on the basis of this assumption, seemingly knock-down refutations of it. When, in their opening chapter, Bricmont and Sokal think they give a *reductio ad absurdum* of the Strong Programme's 'sociological reductionism' by arguing (badly) that it is absurd to think that one could explain what made astronomers accept Newtonian mechanics without reference to the astronomical data, they commit a blunder of this form. They somehow convince themselves that to achieve their *reductio*, it suffices to note that it is absurd to think that one could explain the acceptance of Newtonian mechanics using any information 'that could in any way be called sociological or psychological' but no astronomical data. But do they have any doubt that social and psychological factors, even ordinary ones, influenced and were influenced by the gathering and interpretation of astronomical data in ways that affected the acceptance of Newtonian mechanics?[17] I am sure they do not. But, then, on pain of a logical inconsistency, if they allow the use of any sociological or psychological information, they must also allow access to any scientific data to which it pertains. Had Bricmont and Sokal done this right, they would have tried to find something in the intellectual content of the investigations that seems vital for explaining the acceptance of Newtonian mechanics and free of any *ordinary* social or psychological influences. It might not have been easy to do. But if they had succeeded, it would have posed a challenge to the Strong Programme that required a response from which something might have been learned.[18]

Although I agree with much of what Mermin and Saulson have to say, I do not share their relatively optimistic[19] views of the disagreement. To me, it is less a battle of straw men than a difference in mindsets. It is, after all, a hallmark of the Strong Programme to bar the traditional notion of 'objectively correct' reasoning from its explanations of belief acceptance and rely instead only on accounts of how people do, in fact, reason. But these are two radically different conceptions of reasoning – corresponding, in my view, to the two languages described by Mermin and Saulson. There is a purely descriptive one, about how people reason, to be used in studying the acquisition of scientific beliefs, and a normative one, about how to reason correctly, to be used in seeking scientific knowledge. But if this is correct, the two languages differ in far more than what is emphasized. They are more like incommensurable mindsets, which, on my reading, is, in fact, how David Bloor (1991: 10–14) portrays them, calling the Strong Programme mindset 'naturalistic' and the other 'teleological'.

But, as I have already claimed, incommensurability does not mean that there is your view and my view and never the twain shall meet. In some cases, with sufficient discipline, skepticism, and either empathy or curiosity, it is possible to develop a new mindset and shift back and forth more or less at will. Elsewhere, Collins and Yearley (1992: 301–03) say that they and their colleagues perform such 'alternations' on a regular basis. Good sociologists, they tell us, acquire this skill through their training. But I am suspicious. Not only do they make it seem too easy 'to take on the ways of being in the world that are characteristic of the groups they study', but they also fail to explain how they know when they have gotten it right. Nor do they say whether, when they 'return home', they have any better luck communicating their findings than did Square, upon his return to Flatland from Spaceland. However, these suspicions notwithstanding, I do not doubt that some social scientists – Collins and his colleagues among them – have gone some way toward being able to shift between mindsets in cases of interest to them. By contrast, with a few exceptions, those on the science side have yet to begin or even to understand that there *is* anything to begin.

Notes

1. See also the supporting essays at <http://math.bu.edu/people/nk/rr>.
2. They depend on whether 'is valid' is taken to mean 'is true', 'is justified' or 'serves a purpose'.
3. Gregory (2001: 201) says that, in science, after replications, peer review and publication in *Nature*, the end-product is usually well on its way to becoming *what Bricmont and Sokal might call 'reality' or 'truth'*, that is, to becoming *accepted by them as true*. But by, in effect, conflating 'accepted as true' with 'true', Bricmont and Sokal somehow end up misreading her remark as implying, falsely, that they think that a statement can become *true*. (Actually, some ordinary statements do become true. Things change.)

4. Meeting the explanatory challenge strikes me as a metaphysical impossibility, but maybe I misunderstand what kind of explanation is sought.
5. Lynch does not assess the help provided by those few participants who are professional philosophers.
6. Physicists also talk like this about mathematicians when we complain, rightly or not, about what we see as their abuse of mathematics. Gottfried and Wilson (1997) go as far as to assure their readers that 'real' mathematicians know better than to make such a charge!
7. Nevertheless, I strongly prefer one of them.
8. He lists five at the end of his essay.
9. The philosopher, Thomas Nagel, says about this argument, 'Objections of this kind are as old as the hills, but they seem to require constant repetition. Hilary Putnam once remarked perceptively on "the appeal that all incoherent ideas seem to have" ' (Nagel, 1997: 15). Evidently, it did not occur to Nagel that these objections might be a product of wrong way listening, in which case, no amount of repetition will help. Nor that it is no less reasonable for the relativist to appeal to Putnam's dictum to explain why realists like Nagel persist in misunderstanding her than it is for him to appeal to it to explain why these objections 'seem to require constant repetition'.
10. There is also a third example, which they call a *reductio ad absurdum* of the Strong Programme's 'sociological reductionism'. But it is about an alleged need to appeal, *not* to the truth of a claim, but to *the intellectual content of certain investigations into* its truth, in order to explain its eventual acceptance by the investigators. I say more about this below.
11. Bruno Latour talks a similar way in *Science in Action* (1987: 98), seeming to forget that Nature exists not only in the present and future but also in the past, so that a representation of it may represent how things were before the representation was made. However, in his discussion of 'the puzzle of backward causation' (Latour, 1999: 168–72), he thinks better of it and adjusts his talk accordingly.
12. Where I write 'a partial cause', they have 'part of the explanation', an expression I avoid because its use encourages the reader to accept unreflectively that there is such a thing as 'the' explanation.
13. A partial cause of a partial cause is again one.
14. I don't doubt their sincerity. But if they hope to convince grown-ups that the Strong Programme is in part an attempt to hide the truth about how good certain scientific truths are at making us believe them, they need to do better than one-liners that begin with 'it seems obvious that'.
15. Furthermore, about his first method of measuring, using three tubes, the first two of which yielded the low values that are close to the true one, he says (p. 307), 'It will be noticed that the value of m/e is considerably greater for Tube 3, where the opening is a small hole, than for Tubes 1 and 2, where the opening is a slit of much greater area. I am of opinion that the values of m/e got from Tubes 1 and 2 are too small, in consequence of the leakage from the inner cylinder to the outer by the gas being rendered a conductor by the passage of the cathode rays'.
16. This apparently is all the precision that Thomson sought at this stage of his enterprise. (This impression is based on my reading of Smith [2001].) It is indeed roughly twice the true value but this seems to be because Thomson did his arithmetic right, not because he favored any measurements.
17. Recall Weinberg's psychological explanation of Thomson's favoritism. 'His first values were at the high end of the range and he was determined to show that he had been right at the beginning'.
18. This would have been the case if, for example, they had found that they had to take into account the Wittgensteinian sense (1968) in which the meaning of the intellectual content of the investigations is social: a sense in which social factors help to determine the meanings that expressions express, but not once and for all, nor with enough precision for them to leave home and make it on their own. Not for balls and strikes, not even in principle; nor for mathematics, where something like 'mathematical'

precision has been achieved but only at the cost of abandoning the very idea that its sentences mean something. Nor, given its use of mathematics, for physics.

19. For example, Saulson thinks that when David Bloor says (1991: 7) that other causes apart from social ones 'will cooperate in bringing about belief', it shows that he accepts that scientific progress can happen for 'good scientific reasons'. As for what Bloor might mean, if not this, see Barnes, Bloor & Henry (1996: 76–78), in which 'unverbalized' nature is invoked to help explain belief causation.

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Gabriel Stolzenberg is Professor Emeritus of Mathematics at Northeastern University. For the past several decades, his mathematical work has been devoted primarily to a 'non-scavenger' constructivist development of mathematics and comparisons of it with the traditional one. 'The Weiner Lemma and Certain of Its Generalizations' (Coquand & Stolzenberg [1991] *Bulletin of the American Mathematical Society*, 24(1): 1–10) is an exemplar of the approach. An earlier essay, 'Can an Inquiry into the Foundations of Mathematics Tell Us Anything Interesting about Mind?' (George A. Miller & Elizabeth Lenneberg [eds] [1978] *Psychology and Biology of Language and Thought: Essays in Honor of Eric Lenneberg*: 221–69), focuses on false comparisons based on 'wrong-way listening', a phenomenon that also figures significantly in the science wars.

Address: 1 Richdale Ave, Unit 11, Cambridge, Massachusetts 02140, USA; fax: +1 617 576 2066; email: gstolzen@math.bu.edu