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Replies to the Replies

Gabriel Stolzenberg

Reply to Labinger

> [T]he only thing an editor can do to promote productive discussion is to try to keep everyone engaged, talking *to*, rather than *past*, one another (Labinger, 2004: 92).

It is precisely because I know that this is Jay Labinger's view that I was surprised to find, in this conversation about science studies of which he is an editor, what seemed to be easily corrected misreadings of one participant by another. These misreadings made those who committed them talk past, rather than to, those whom they misread by causing them to respond to statements that had not been made instead of to ones that had. Nevertheless, contrary to what Labinger infers from a remark that he quotes from my review, I do not believe that, in a case of this kind, God wants the editor to suggest, much less request, that his authors correct what he believes to be misreadings. My thought was rather that if a response by one author to another seemed as if it *might* rest on a misreading, then God might want the editor to call this to the attention of the respondent.

In addition, as the following *mea culpa* should make clear, I too take very seriously the possibility that what I now believe is an author's misreading of a text may later be seen as a figment of my misreading of the author.[1] No matter how careful I may try to be in proving a charge of misreading, I may anyway have to eat my words. Therefore, I should be prepared to do so – if possible, with grace. As proof that I do not see this as merely an abstract possibility, consider my claim in the review that:

> . . . 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. (Stolzenberg, 2004: 78)

When I first read Collins' (2001: 187) criticism, its opening remark about Bricmont and Sokal 'embarking on the perilous ocean of counterfactuals'

Social Studies of Science **34/1(February 2004) 115–132**

ISSN 0306-3127 DOI: 10.1177/0306312704041343
www.sagepublications.com

made me think that he had failed to recognize that the use of counterfactual language is purely decorative and, because of this, had misunderstood their challenge. I persisted in this belief even when reading Collins' remark that Bricmont and Sokal 'try to imagine how one would ever explain our society's belief in the inverse-square law of planetary motion as opposed to, say, an inverse-cube law, if no information about planetary movements had been available'. However, although his use of 'had been' still gives me pause, after rereading the whole of Collins' criticism, I accept this remark as an accurate statement of the explanatory challenge that I thought he had not understood. Thus, what I represented in my review as Collins misunderstanding Bricmont and Sokal, I now see instead as Stolzenberg misreading Collins.[2] *Mea culpa*.

But Labinger is concerned with editors, not readers who think they have discovered errors in other people's readings. If I understand him correctly, it is his view that the same consideration that tells me to be prepared to eat my words should caution an editor to remain silent on matters of right and wrong.[3] Although I am not sure how well it works, Labinger chooses an extreme case, the 'dialogue of the deaf' between realism and relativism, to show why this is the right thing to do.[4] Thus he notes that the proponents of the realist argument that relativism is 'self-refuting' see no more merit in the 'counterargument'[5] than proponents of the latter see in the former. Maybe because of this stand-off or because, among the proponents of the realist argument, there are eminent philosophers like Thomas Nagel, or maybe just because the stand-off is several thousand years old and still going strong, Labinger is, to put it mildly, skeptical that anyone could convince him that the proponents of the 'self-refuting' argument suffer, as I say they do, from a kind of incompetence[6] that keeps them from 'getting it'.

I think he is mistaken about this. I don't see why it should be harder to convince Labinger of it than it was to convince myself. But perhaps his larger point is that, in a case of this kind, where conversation is impossible because each side is morally certain that the other side is terminally delusional, it would be both foolish and futile for an editor to try to arbitrate matters of right and wrong. It would indeed. Still, I can imagine benefiting greatly from an editor's criticism of my criticism of the 'self-refuting' argument. Indeed, when I don my editor's cap and rehearse my criticism, I see immediately that although I insist that relativism is not a claim but a mindset, I say nothing about how to acquire the ability to adopt it, if only temporarily. Nor do I explain why the particular mindset that I call 'relativism' is deserving of the name. Thus, here are two glaring gaps in my account that were practically begging for an editor to come along and call them to my attention. How nice that one finally did.

Reply to Lynch

Although Michael Lynch (2004) does not discuss my reason for calling Steven Weinberg's 'declaration of independence' an anti-intellectual conceit, it is apparent that he is not persuaded by it. I wish I knew why. Weinberg says,

> As is shown by our common use of words like 'real' and 'true', we all adopt a working philosophy in our everyday lives that can be called naive realism. As far as I know, no one has shown why we should abandon naive realism when talking of the history and sociology of science. 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, 2001: 240)

But I believe that it *has* been shown why science warriors should abandon naive realism when criticizing statements in the history and sociology of science. They should do so firstly because their naive use of loaded terms like 'real', 'true' and 'cause' tricks them into projecting their realist metaphysics, together with certain confusions about it, onto the authors of the statements they criticize, who are then blamed for the conceptual mess that results. These confusions, which arise from the same naive use of such terms, also infect the rest of their talk about these subjects, often rendering it nonsense. These claims are supported by many examples (Stolzenberg, 2001),[7] three of which I discuss in the review. In one, Weinberg convinces himself, on the basis of a novel mistake about the relationship between knowledge and belief, that he knows a case in which Strong Programme practitioners have to ignore a valuable clue to the past.[8] In a second, Bricmont and Sokal are deceived by their talk about 'the' explanation of an event into thinking that they have refuted the symmetry principle of the Strong Programme. In a third, the analytic philosopher, Paul Boghossian, projects confused realist notions of relativism onto a postmodernist-looking remark, which he then thinks he has debunked.

As I understand it, Weinberg's declaration of independence implies that nobody, not you or me or any of the targets of the attacks, has any business telling science warriors that they have no business behaving this way. It is not that he approves of such behavior – I am sure that he does not – but rather that he refuses to concede that the uncritical stance that he champions may severely limit his ability to recognize it. Lynch notes that Weinberg says that we can use words like 'real', 'true' and 'cause' 'sensibly and competently [in everyday life] without having to study philosophy'. However, the relevant question is not whether people *can* use such words competently but whether, in particular cases that matter, they do. Is Weinberg qualified to decide this? I think not. His talk of using such words 'sensibly and competently' seems to mean little more than using them the way he does. If to call his declaration an anti-intellectual conceit is philosophism, I say more power to it. But I don't think it is.[9]

Reply to Saulson

Peter Saulson and I agree that readers of Strong Programme literature need to understand that the notion of 'social' that the Strong Programme requires is not the ordinary one or, at least, not the ordinary one as it usually is conceived. We also agree that, whatever the cause, the failure of some readers to understand this has contributed significantly to the misreading of this literature. However, we seem to have very different ideas of what the Strong Programme notion of 'social' is and why it is misunderstood.[10] Saulson says that it must include 'all that scientists would think of as actual experimental evidence of the nature of the world, *as soon as that evidence has entered human minds*' (2004: 99). But much, if not all, of what scientists would think of this way is physical. It cannot literally enter a mind. Thoughts can. But a thought about evidence is not evidence. Also, the juxtaposition of 'actual' and '*mind*' makes me wonder whether he is trying to explain at least part of the Strong Programme's notion of 'social' within a realist framework. In my view, such an attempt is doomed from the start if only because, for a realist, the evidence of which Saulson speaks is evidence of the truth of a statement about reality 'independent of us', which is not a Strong Programme notion.

Saulson and I agree that much of the criticism by scientists of work in the sociology of science is marked by misreading[11] and sloppy reasoning. But we appear to differ about the role of the sociologists who are misread. Thus, in a statement that I continue to hope I misunderstand, Saulson says, '[w]ith the greatest respect for sociologists, I'd like to suggest that sometimes they enjoy being misread'.[12]

Although Saulson does not name names,[13] some statements made by Harry Collins strike me as possible manifestations of such an attitude. But, however outrageous or disturbing I find them, I try to bear in mind that, for all I know, it did not occur to him that these statements might be read the way I read them.[14] Moreover, a remark by Collins (2004: 105) in his reply to my review provides what I consider to be strong evidence of his good faith in this matter:

> The shock of what readers read into what you write must have been experienced since the early days of writing. Certainly, almost everything I write, and that includes the straightforward pieces, seems open to astonishing misinterpretation by at least a few people.

Of course, whether a statement is intentionally misleading or its author failed to anticipate an 'astonishing misinterpretation', the effect on the reader is the same. Still, a reader who finds a statement strange or unintelligible has a choice. He can assume that his reading of it is correct or he can withhold judgment, perhaps indefinitely. In the science wars, there has been precious little withholding of judgment.[15] Instead, by a suspension of critical thinking, scientists have deceived themselves into supposing that they understand statements that they do not – in fact, that they understand them well enough to debunk them, often by pronouncing them nonsense. Saulson says that I 'lament' the misreading and sloppy reasoning by scientists in their 'rather simplistic defenses' of science. I do, but I lament even more the lack of intellectual discipline that makes it possible.

Saulson seeks causes for these 'rather simplistic' defenses of science. Although features of the scientific mindset cannot suffice to explain why some scientists engage in this behavior but others do not, in his reply he is concerned only with contributions of this kind. He notes that, for most scientists, the shared intuitive understanding of science's connection to Nature 'is not something that it appears sensible to question' (2004: 000) and also that the scientific community's commitment to reason is itself based on faith, not reason. After arguing that these and other non-rational aspects of the scientific mindset bear 'all of the hallmarks of a religious faith', Saulson (2004: 000) suggests that the defenses of science by scientists are not truly rational arguments but 'testimonies of faith and denunciations of error'.[16] But even if this is right, it does not reveal how particular quasireligious aspects of the scientific mindset help cause particular defenses of science to be simplistic. This seems worth exploring. Also, why would a scientist who has written such a defense with the intention of making it public not first subject it to scrutiny, as he would of his own scientific work? If he did, would he not see the lack of evidence and rational argument? Maybe Saulson's (2003: 000) eloquent rendering of one such testimony of faith, the scientist's oath of allegiance to rationality everywhere, not just in science, can help answer this.

> We are proud of our commitment to reason. . . . [We] are brought up to believe that the scientific method . . . is the embodiment of intellectual virtue. . . . If only more people would apply its principles in their daily lives and in their own fields of endeavor, the world would be a better place, or so we hope at least.

If we take this at face value, then when a scientist fails to subject a defense of science to critical scrutiny, it seems that he is abandoning his commitment to rationality everywhere, not just in science. But if Saulson is right about the quasi-religious nature of the scientific mindset, then no matter how proud it may make scientists to think otherwise, there is no such commitment to abandon. Most of them are not committed to rationality even for thinking about science's connection to Nature and Reason, much less everywhere. The world would be a better place if they were. But they aren't and it isn't.

Reply to Collins

When I posed my two skeptical questions about alternation at the end of the review, it did not occur to me that they might elicit anything like Harry Collins' clinically objective report on the state of the art. I especially admire his forthright admission that, for an alternating sociologist, the problem of communicating with the folks back home is even more difficult than that of being able to tell when she has succeeded in 'going native' – that is, has acquired a desired mindset – which in many cases is difficult enough. Of course, Collins could have said this in order to try to impress us later on with the ability of sociologists to succeed at something so difficult. But instead he says that the sociologist who has acquired a new mindset can do no more than 'make a stab' at conveying what this means to the folks back home and that, even after she does, 'the paradox remains'.

To this I would add that, in some cases, communication of this kind is flat out impossible. If the new mindset is incommensurable with the old one, the folks back home may hear what seems to be a clear statement by the alternator reporting from her new mindset, but which is a radical distortion of what she is saying. In some cases, the distortion makes reasonable statements seem bizarre and even mad. In others, it leaves them sounding reasonable but radically distorts their content. Here I wish to consider only cases of the former kind. I also wish to assume that the alternator is so respected for her integrity and good sense that, when she thinks that she is onto something, however implausible, the rational response of her colleagues back home is to repeat the alternation and, from that perspective, assess her reasons for thinking what she does.

Because the alternator acquires access to the new mindset without losing access to the old one, she is fully aware of the distortion. She can hear it herself. But because it is caused by the incommensurability of the two mindsets, she may not be able to eliminate it by saying things differently. She may be able to explain, without distortion, that there is distortion but the substance of her explanation would be intelligible only to someone who was familiar with the phenomenon of alternation. For others, it would merely confirm their suspicion that she is delusional. But not Sherlock Holmes. At least, not my Sherlock Holmes. He would be bemused by the spectacle of someone, who is respected for her integrity and good sense, making statements that seem bizarre and even mad without it arousing suspicion that things may not be as they seem. My Sherlock's suspicion *would* be aroused. He would want to discover what is going on. And he would.

I'm no Sherlock Holmes but I once found myself in a similar situation. The alternator was my colleague, Errett Bishop, a plain-spoken, stubbornly independent thinker who, 50 years ago, produced a wealth of breathtakingly original, seminal mathematics. He was unassuming about his talent and generous in the intellectual and emotional support he gave his colleagues. But when he was 36 years old, he abandoned it all in favor of a 'constructivist' mathematics that, to the vast majority of mathematicians, including me, seemed tedious, pointless, and even perverse. It seemed to combine the worst features of pure and applied mathematics. Worse, he seemed to be saying that intellectual honesty requires every mathematician to do the same – basically, by abandoning the assumption that every mathematical statement is true or false independent of our knowledge of which.

This principle of bivalence, which licenses such reasoning as proof by contradiction, is the hallmark of ordinary or, as it also is called, 'classical' mathematics.[17] Most mathematicians who paid any attention to Bishop's constructivist assertions seemed to be content to assume that he had fallen under the sway of a pernicious philosophical dogma, for the sake of which he was willing to cripple mathematics. Others, who wished to preserve his place as a star in the mathematical firmament, chose to portray his constructivist work as a profound contribution to mathematics but in a way that meant that it could be ignored.

I had no opinion about the matter, mostly because I was not paying much attention. For no very good reason, I expected that, after a while, it would be sorted out by the 'wise men' of the mathematical community, one of whom was Bishop himself.[18] But when two years had gone by and Bishop's behavior made no better sense to me than when I first heard about it, I became very curious to find out whether, appearances notwithstanding, the problem was not with him but with how we were listening to him. The method that I employed was unimaginative but effective. Whenever I came upon a statement of his that seemed silly, I stopped to try to identify the beliefs and attitudes that certified this assessment and to reflect upon my reasons, if any, for holding them. As I did this, my unfavorable reading of these statements, which I later recognized to be an involuntary consequence of my mathematical training, gradually became voluntary. When it did, my mind became free to discern a different, far more interesting, reading of these statements and, in fact, of any statement of or about mathematics.

In this way, I soon mastered the constructivist mindset well enough to find, to my amazement, that there is a world of mathematical concepts and intuitions that have no place within the classical framework that I and my colleagues had been led to believe contains everything. But, although I found it fascinating to explore this new mathematical realm, for several years, I continued to take for granted that classical mathematics gives the big picture.[19] However, when, for an article about Bishop's work (Stolzenberg, 1970), I wished to balance my extremely positive view of the constructivist program with a 'reminder' of *why* classical mathematics gives the big picture, I was forced to recognize that this universally held view was based wholly on impressions and intuitions.

But I knew that, in the early part of the 20th century, there had been a controversy over how mathematics should be done, classically or constructively. So, because I, and everyone I knew, had been taught to do it strictly classically, I reasoned that this must have been the conclusion reached by the wise men of that time. But neither I, nor any of my colleagues, knew how they arrived at it. So, I decided to remedy our ignorance by reading the relevant literature and reporting on it in my paper. But when I read the wise men's rationale for their conclusion that mathematics should be done classically, I was shocked. It was an egregious case of what, I call 'wrong way' listening. Instead of comparing classical mathematics as it is seen in the classical mindset with constructivist mathematics as seen in the constructivist one – which is the relevant comparison to make – they relied upon the utterly irrelevant comparison of the two practices as they appear only in the *classical* mindset.

Disillusioned with wise men, I undertook to construct my own argument for why classical mathematics gives the big picture. I began by looking closely at some of the examples that are thought to illustrate it.[20] But when I did, not only did they fail to display the alleged advantages of working classically, they did the opposite. So, having previously discovered that classical mathematics is not all the mathematics there is, I now witnessed the shattering of the intuitions that had supported my belief that it nevertheless provides the big picture. But this too was a very good thing. It dramatically expanded my view of mathematics, revealing remarkable phenomena to which I previously had been blind. Nevertheless, the failure of the traditional rationales for why classical mathematics constitutes the big picture does not prove that constructivist mathematics does. It means only that the matter of which, if either of them, is superior for mathematical practice is an open question – one to which my own research has been devoted ever since I discovered, a long time ago, that it really is an open question.

Although both my skeptical questions and Collins' excellent response are about the general phenomenon of alternation, the one I have chosen to discuss here is of a special kind that deserves mention. It is a traditional Gestalt switch, like Jastrow's duck/rabbit drawing only vastly more complex. Instead of a line drawing that can be interpreted either as a duck or a rabbit, its material ground consists of all sentences that can be interpreted either classically or constructively as statements of or about mathematics.[21] This massive Gestalt switch, which I have always found uncanny, seems to be unknown to sociologists of science, even those like David Bloor, who have written about 'alternative' mathematics (1991: 107–30). True, I have no idea what sociologists of science could do with it – maybe nothing. Nevertheless, like the proverbial 800-pound gorilla, it is with us and we do better to know that it is than not.

Reply to Bricmont and Sokal

Bricmont & Sokal (2004: 107) say that I misrepresent them, and that I also misrepresent Paul Boghossian (1998), whereas I say that it is their misreading of me that causes them to think this. How, in any particular case, can a reader figure out who is right? I have no answer to propose except to urge close, skeptical, and unhurried consideration of what each of us says. Read and reread. In cases of this kind, the obvious is the enemy of the true.

Intellectual debate?

I am accused of using emotion-laden militaristic language to talk about intellectual debate. But, in the one case mentioned, which I discuss later, there is no militaristic language – only a tongue-in-cheek allusion to Bricmont and Sokal's seemingly hostile attitude toward the Strong Programme[22] and to the fatal consequences of accepting their criticism of it. When I do use militaristic language, like 'science warrior' or 'hatchet job', it is precisely to emphasize that what I am talking about is not intellectual debate but – I don't know a better way to put it – a hatchet job. But the focus here is on the Strong Programme, not French postmodernists or the editors of *Social Text* or even Bruno Latour, and I make it clear in my review that this conversation, in which Bricmont and Sokal are active participants, contains no hatchet jobs.

Boghossian's Ken Starr Imitation

Before rejecting my comparison of Paul Boghossian's treatment of Roger Anyon to that of a reckless prosecutor, Bricmont and Sokal might have asked themselves why I consider it apt. The relevant distinction is between wanting to discover the truth, wherever this may lead, and wanting to make a certain charge stick, as Starr apparently wanted to do with Clinton.[23] Instead of carefully weighing the evidence pro and con – as one might expect an analytic philosopher to do no matter how much he may desire a particular outcome, Boghossian went after Anyon with reckless zeal, the quality of his evidence and arguments be damned.[24]

Reading Jane Gregory

Bricmont and Sokal consider the dispute about Jane Gregory's remark a minor matter. I do not. Their criticism presumes, without a shred of evidence, that Gregory, co-author of 'The Public's Role in the Science Wars', was so ignorant about the science wars that she did not even know that the Sokal of Sokal's hoax is a card-carrying metaphysical realist and, therefore, would never say that a scientific statement can 'become true'. The implausibility of this presumption is one of my two reasons for believing that Gregory did not attribute the offending metaphysical view to them. The other is that she did not attribute *any* metaphysical view to them. Perhaps the simplest way to see this is to notice that, in the remark, the relevant part of which reads, 'the end product is usually well on its way to becoming what Bricmont and Sokal might call "reality" or "truth"' (Gregory, 2001: 201), the expression, 'Bricmont and Sokal', makes its appearance *after* 'becoming', not before. Therefore, unless it is written in Hebrew or Arabic, it would require an extremely clever reading on the part of Bricmont and Sokal for the remark to imply that they hold some view, no matter which, about anything *becoming* anything. But they don't have *any* reading of Gregory's remark, much less a clever one.[25] They merely believe that it implies that they are not metaphysical realists, an implication they seem to find too obvious to require rational justification. Here too, the obvious is the enemy of the true.

Death and the Strong Programme

Bricmont and Sokal complain that I do not give any evidence that they wish to see the Strong Programme dead.[26] This is true. I did not think it was necessary and I still do not. But I'm happy to oblige. The remark has less to do with 'emotion-laden' matters – like whether or not the Strong Programme irritates them the way they irritate me – than with their repeated statements about what would become of it were their criticism of it to be accepted. For example, in *The One Culture?* (2001a: 46), they say that 'science studies practitioners are not obliged to persist in a misguided epistemology; they can give it up and go on to the serious task of studying science'.[27] But, as they know, if the practitioners give up its distinctive epistemology, they give up the Strong Programme.[28] Therefore, if they would like to see their criticism of the Strong Programme accepted, then they would like to see it become what it is not. True, practitioners could then 'go on to the serious task of studying science', but not within the framework of the Strong Programme, which would no longer exist.

A Long and Sensible Chapter?

Bricmont and Sokal (2004: 107) note that Michael Lynch discusses Sokal's one-line invitation to social 'conventionalists' to jump from his window but not their 56-page chapter in *Fashionable Nonsense* about philosophy of science (Sokal & Bricmont, 1998: 50–105). As I explain in the review, in the same discussion Lynch characterizes the metaphysical discourse of the science wars, of which Sokal's quip is a notorious example, as 'sandlot' philosophy, in the sense that few of the participants are professional philosophers. Later, David Mermin (2001: 218) says that his physicist colleagues, Bricmont and Sokal, 'clearly delight in sandlot philosophy', adding that he too likes to play. Perhaps Bricmont and Sokal now wish to suggest, without coming right out and saying it, that the chapter in *Fashionable Nonsense* shows that they are more sophisticated philosophers of science than the label 'sandlot philosopher' might lead one to think. Moreover, in support of this assessment, they have the testimony of such luminaries as Noam Chomsky, who calls the chapter 'a thoughtful and constructive critical analysis of fundamental issues of empirical inquiry' and Thomas Nagel, who calls it 'long and sensible'.[29]

However, I see it differently. In my view, although Bricmont and Sokal are to be commended for their discussions of Popper and Kuhn, when they turn to skepticism, underdetermination, relativism, or any of the issues related to Bruno Latour or the Strong Programme, they misjudge the limits of their competence, with predictable consequences.[30] I am aware that to question someone's competence rather than merely criticize particular applications of it may, like a charge of irrationality, seem to verge on an *ad hominem*. But in this case, it means no more nor less than it says. If I have had anything distinctive to say about the science wars, it is in the importance that I attach to the failure of my professional colleagues, the science warriors, to recognize the limits of their competence to read and reason intelligently about certain texts that are outside their professional domains. Even when I criticize their most egregiously unjustified readings, my point is not that they should have been able to do better but that they should have known better than to think they could. Indeed, I am no more competent to read many of these texts than those whose readings I criticize.[31] But I recognize this and try to act accordingly. I also recognize that professional credentials do not guarantee success. In the science wars, some of the worst offenders are academic philosophers.

Incommensurable but not Incompatible

Bricmont and Sokal take issue with a claim of incommensurability that I make, reminding us that the descriptive and normative notions of reasoning are, or at least seem to be, compatible. But I talk about incommensurability, not incompatibility, which is a very different thing. For example, the two mindsets for mathematics that I talk about in my reply to Collins are incommensurable, but the bodies of mathematics produced in each are compatible – albeit in incommensurable ways.[32] Similarly, Bricmont and Sokal call attention to the compatibility of the descriptive and normative notions of reasoning in the teleological mindset, but they also are compatible in the naturalistic one.[33] What is treated in the former as a fact about correct reasoning is accommodated in the latter, without loss of information, as a belief about correct reasoning. If Bricmont and Sokal wish to object that the naturalistic mindset fails to do justice to the normative conception of reasoning, I agree. But the other half of this truth is that the teleological mindset fails just as badly to do justice to the descriptive one.

How Does Evidence Help to Make Us Believe that it is Evidence?

In the review, I say that, to use an explanatory scheme they seem to favor, Bricmont and Sokal must show that the *fact* that something 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. I also say that not only do they fail to do this, they write in a way that conflates the two states.[34] In reply, Bricmont and Sokal (2004: 110) point out that, 'far from conflating evidence with belief', they know very well that the relationship between the two can be very complicated. But this is not the conflation that I am talking about! I say that they conflate *the fact that something is evidence* for a belief with *the belief that it is evidence* for the belief and they reply that they do not conflate *the fact that something is evidence* for a belief with the belief for which it is evidence. They give the right answer to the wrong question.

An example may help. Sokal (1998: 16) claims that to plausibly explain the shift in scientific belief from creationism to Darwinism, we must refer to the fossil record. But does he mean to *evidence* provided by the fossil record or to the *belief* that it provides evidence? Reference to the belief does seem necessary but Sokal is talking about evidence. So, unless he knows a way that evidence can help cause belief other than by helping to cause the belief that it is evidence, to defend his claim about the role of the fossil record, he would have to do something that he does not seem to have tried elsewhere, with or without Bricmont. As I see it, they have an unhappy choice to make. They can either continue to ignore the crucial distinction between the fact that something is evidence and a belief that it is or they can face up to it and try to show, in at least one case, e.g., Sokal's claim about the role of the fossil record in the shift to Darwinism, that evidence did play a necessary causal role in helping to convince people that it is indeed evidence. But this, I think, is a fool's errand. It is, in effect, where they came in – promising to show us that, in some cases, we must appeal to the truth of a belief to help plausibly explain the belief that it is true. The only new twist here is that it is the truth of a belief that something is evidence for the truth of another belief. Is this progress?

A Serious Charge, But Also a Curious One

In the review, I discuss two cases in which Bricmont and Sokal (2001a) claim that to explain what makes somebody hold a certain belief, it is *necessary* to appeal to the fact that it is true. In my critique of these claims, I observe that Bricmont and Sokal offer no argument for either of them. They merely preface the first by 'it seems obvious that' and the second by 'certainly'.[35] But they dispute this (2004: 107), claiming that 'Stolzenberg carefully omits to mention the sentence immediately following the one he quoted, which is devoted *precisely* to giving an argument in support of the preceding assertion'. Even if we replace 'carefully' by the less paranoid-sounding 'carelessly', this is a serious charge. It also is a curious one because not only does the sentence they mention provide no such support, it doesn't even read as if it does. It doesn't even read as if *they* think it does.

The assertion in question is 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'. The sentence that allegedly contains an argument in support of it reads, 'If someone said that it is raining when it is not, one might think that he is joking or that he is mentally disturbed,[36] but the explanations would be very asymmetrical depending on whether it is raining or not'. But, as Bricmont and Sokal well know, a description of unreflective responses that, in ordinary discourse, might be called 'explanations' cannot provide support for their normative claim that there is such a thing as '*the*' explanation of why a person standing in the rain believes that it is raining, part of which is that the belief is true.

Nevertheless, it is true that, when I wrote the review, I did not fully grasp what Bricmont and Sokal take themselves to be doing in these two sentences. Nor did their reply help, except to reveal that they did not understand my criticism and I did not understand why. But, having reconsidered the matter, it seems to me that, in their minds, the first claim, together with the sentence that I 'carefully omit to mention', is an argument against Bloor's symmetry principle that true and false beliefs are to be explained by 'the same' kind of causes (Bloor, 1991: 7). However, I don't talk about the symmetry principle because I think that its wording invites misunderstanding.[37] I talk instead about the irrelevance of authenticity for explaining belief causation – in this case, of the fact that the belief is true. Because of this, the target of my criticism is not their argument, if that is what it is, against the symmetry principle but as I make clear their two claims about the necessity of referring to the truth of a belief in order to explain its causation. That Bricmont and Sokal think that I am mistaken when I observe, correctly, that these claims are supported only by 'it seems obvious that' and 'certainly' suggests that they do not understand that I am criticizing only the absence of support for *the claims themselves*, not for any role they are alleged to play in criticizing the symmetry principle.[38]

The Irrelevance of Authenticity for Explaining Belief Causation

Judging by their reply, Bricmont and Sokal agree that no reference to the whiteness of a sheep, one side of which I see, is needed to explain what makes me believe in it: at least in this case authenticity is irrelevant. Yet they show no curiosity, much less concern, about what, if anything, is different about the beliefs in their two examples that make it necessary to appeal to their truth to explain their causation. How can they justify saying that an appeal to the truth is necessary for local weather conditions but not for ewe hue? My impression is that, instead of continuing to insist upon the need for such appeals, Bricmont and Sokal are turning to other ways of trying to persuade us of the superiority of their 'truth-based' explanations. One sign of this is their acknowledgment of the interest-relative nature of explanation; there is no more talk of '*the*' explanation and causes that must be part of it, only of explanations that are deeper than other ones, or 'more adequate' or 'better' or 'based on simpler or more fundamental facts' or, my favorite, 'what one would quite reasonably want to know'.

But it is simplistic and even irresponsible to imply, as they do, that one explanation is better or more adequate than another one merely by virtue of being simpler or more fundamental, especially when the relevant senses, if there are any, of 'simpler', 'better', 'more adequate' and 'more fundamental' have not been explained. Worse, they ignore the crucial question of the *intrinsic* adequacy of an explanation – especially, of its causal mechanism. They do point out that, unavoidably, many aspects of the causal mechanism remain implicit 'until someone demands that they be made explicit and subjected to questioning'. Yet, one of the first things one notices about their alleged explanations of belief causation is that, at every point in a causal chain at which they go beyond the realm of Strong Programme explanations, there is a conspicuous absence of anything that deserves to be called 'a causal mechanism'.[39] I believe that, when such mechanisms are given in sufficient detail, far from strengthening Bricmont and Sokal's case, they will provide vivid proof of the irrelevance of authenticity by allowing us to see that they operate not only on the content of the belief but on any good enough 'imitation' of it.

A Belief and How I Came By It

I am inclined to see Bricmont and Sokal's rejection of my 'truth-based' explanation of my ewe hue belief (wherein the whiteness of a sheep 'explains' the whiteness of the side that I see) as evidence of their new, more flexible, approach. Although they still require that the truth of *some* belief plays a crucial explanatory role, it no longer has to be the one whose causation is to be explained. Thus, saying that my 'truth-based' explanation of my belief 'misses the main point', they propose to explain it instead in terms of how I acquired the more general belief that every sheep that is white on one side is white.[40] In the review, I was content to attribute this to 'my past experience', assuming that readers would fill this in much as I would. But I was mistaken. According to Bricmont and Sokal, an essential part of the story is that, a long time ago, I saw '*both* sides of a large number of sheep, and they were unfailingly monochrome'. But, even if something like this were true, which I strongly doubt,[41] the irrelevance of authenticity still rules. What insight into the process of belief causation is gained by knowing that the animals that I allegedly saw were not wolves impersonating sheep but authentic 'out there in reality independent of us' monochrome sheep?

A Truly Inadequate Explanation

In yet another attempt to persuade us of the rightness of their causes, Bricmont and Sokal compare their 'truly adequate' explanation of the acceptance of Newtonian mechanics with their ungenerous representation of a Strong Programme one. In keeping with the new paradigm, whereas, before, we were encouraged to assume, falsely, that we have an adequate understanding of what, if anything, is meant by '*the*' explanation of something, we are now encouraged to assume, falsely, that we have an adequate understanding of 'a truly adequate' explanation – so that when they tell us that *this one* is not truly adequate but *that one* is, we can look at the evidence and arguments they give us and make up our own minds.

The comparison, if it can be called that, is essentially a thumbnail sketch of the authors' ideas about how and how not 'to explain scientist X's belief in some theory'. Pausing at places where they imply, falsely, that a Strong Programme explanation must stop, they say that it is 'not natural' to stop there because one would 'quite reasonably' want to explain some of the things on which causal irrelevance of authenticity is ignored, requires an independent determination of whether the experiments that convinced X produced truth or falsehood. This creates an infinite regress but they do not notice.[42] Depending on how it is broken,[43] the punch line of their explanation, the part that, in their eyes, renders it 'truly adequate', is either that the experiment yielded E *because* E is approximately true or *because* of experimental error (including faking). However, if you find that it is 'not natural' to stop here because you 'quite reasonably' want to examine the causal *mechanism* in order to assess the causal relevance, if any, of E being approximately true, then, for you, this 'truly adequate' explanation is truly inadequate.

Notes

1. Or the text. Or both! Similarly, what I claim is faulty logic may later be seen as a figment of *my* faulty logic.
2. I wish to thank myself for calling this to my attention.

3. But evidently not on matters of meaning. At the request of Labinger and his co-editor (Labinger & Collins, 2001: 298), Bricmont & Sokal (2001b: 246) attempt to clarify a statement they made. But I don't see how to separate the question of what this 'clarification' means from that of its correctness. I think it is nonsense but maybe I misunderstand it.
4. Remarks like 'One reasoner's faulty logic is another's knockdown argument' suggest that Labinger (2004: 91) finds every case extreme, if not *as* extreme as this one. But one reasoner's faulty logic is *not always* another's knockdown argument. In mathematics, it is expected that disagreements will be resolved and they almost always are.
5. This is Labinger's (2004: 91) term. Actually, the only thing to counter is the claim that it is an argument against *relativism*.
6. Having to do with 'listening the wrong way'.
7. See also the essays at <http://math.bu.edu/people/nk/rr>.
8. Weinberg's argument seems to rest on the rogue realist intuition that, sometimes, a person will take a certain action if he possesses knowledge of a certain proposition but not if he merely believes that he does.
9. It is nothing like my favorite example, in which the philosopher John Searle (1994: 213, note 5) interrupts an argument to inform us that 6 added to itself 8 times is not 48, as is universally believed, but 54, adding, 'It is amazing how often this mistake is made'. The consideration that, if he is right, everyone else is wrong gives Searle no pause. It has been suggested that he is merely calling attention in a jokey way to an inconsistency between our well-established use (or avoidance) of locutions like '6 added to itself twice' and their literal meaning. But the obvious literal meaning of '6 added to itself twice' is $6 + 6 = 12$, $6 + 6 = 12$ and I know of no other. Searle says that it means 6 added to the result of adding 6 to itself, which is 6 added to 12, not to 6, which is 18. But I doubt very much that even he would dare claim that this is a literal rendering of '6 added to *itself* twice'.
10. I allude to my view of this notion in note 24 of the review, the subtext of which is 'Read *Philosophical Investigations* and then we'll talk'.
11. I have a bad habit of saying 'misreading' when I mean 'unjustified reading'. I assume this also is what Saulson means. The distinction is important. Often, we don't even need to know what a statement means to know that a reading of it is unjustified.
12. But he also says that they 'work in an environment where words mean what they say they mean, no more and no less'.
13. He accuses David Bloor (1991: 5–23) of writing in code but not of enjoying it.
14. Whether it 'should have' occurred to him is another matter.
15. To me, the relevant distinction is not between understanding and misunderstanding but, rather, between not misunderstanding and misunderstanding. I can misunderstand English but not Chinese.
16. He adds that to understand these testimonies and denunciations as scientists do, one must undergo 'the actual conversion of unbelievers, which comes through a mysterious personal journey, only partly mediated by written texts'.
17. Because it employs classical logic – what David Hilbert, writing in 1925, in 'On the Infinite' (1967), called 'the very laws that Aristotle taught'. His full remark is worth quoting. 'In any case, those logical laws that man has always used since he began to think, the very laws that Aristotle taught, do not hold. [But] we just do not want to renounce the simple laws of Aristotelian logic; and no one, though he speak with the tongues of angels, will keep people [from using] the principle of excluded middle. What, then, shall we do?' (Hilbert, 1967: 379).
18. I was young!
19. Precisely because it suppresses questions of constructivity.
20. The traditional proof of the infinitude of primes is often touted as an illustration of 'the power of the indirect method' (for example, Hardy, 1940: 92–94), even though it is a 'direct' proof pointlessly imbedded in an 'indirect' one.

21. Because every sentence that can be interpreted classically can also be interpreted constructively and vice versa, it would be a much better analogy if every drawing that could be seen as a duck could also be seen as a rabbit and vice versa. Not bloody likely.
22. An impression created in part by criticizing it in a book devoted to debunking 'fashionable nonsense'.
23. I used Starr, not because his behavior was especially bad as these things go, it was not, but because it seems that almost everyone has heard about it.
24. For example, even though he sees that his third reading of Anyon's remark gets him off the hook, instead of retracting his charge, he invents another, inconsistent with the first, that does apply to this reading.
25. Their 'charitable reinterpretation' 'along lines similar to Stolzenberg's by referring to "evidence"' (Bricmont & Sokal, 2004: 111, n. 3) is not a reading of it, but merely a thought evoked by the remark – one that, despite its reference to evidence bears no interesting similarity to my reading of it. Yes, evidence usually figures significantly in a process of this kind. But, for the point in dispute, it could just as well consist in waiting for a word from God. Furthermore, what is called for is not a logical qualifier like 'conclusive' but a cognitive one like 'convincing' or 'convincingly conclusive'.
26. I wrote, 'Three physicists, two of whom wish to see [the Strong Programme] dead and one who merely finds it boring, crippling and wrong-headed, attack it'. Lighten up, guys.
27. In addition, in *Fashionable Nonsense* (Sokal & Bricmont, 1998: 92), we are told that, depending on how one resolves an ambiguity in the intent of the Strong Programme, 'it becomes either a valid and mildly interesting corrective to the most naive psychological and sociological notions, reminding us that "true beliefs have causes, too" or else a gross and blatant error'. Thanks a lot! Finally, in support of my claim about 'repeated' statements, note that each of the two that are quoted here appears twice more elsewhere, (Sokal, 1998: 17–18, 2001: 24–25), for a total of six.
28. See their argument that methodological relativism makes no sense without philosophical relativism.
29. Chomsky's remark is on the book jacket. Nagel's is in his review of the book (Nagel, 1998: 33).
30. However, if the criticism of their reply that I give here is not convincing, I doubt that my critique of their chapter will be any more so.
31. In such cases, I almost always argue only that a reading is unjustified, for example, by giving a different one that is no less plausible in the argument, not that it is incorrect.
32. I do not know any way to confirm the incommensurability without learning to alternate between the mindsets. However, it is very easy to describe the two kinds of compatibility. In the classical mindset, constructivist mathematics is the part of classical mathematics devoted to seeing what can be proved without the law of excluded middle; whereas, in the constructivist mindset, classical mathematics is the part of constructivist mathematics devoted to seeing what follows from the law of excluded middle. So, in each case, the compatibility is that of part to whole. Each is a restricted subsystem of the other! However, because of the incommensurability of the mindsets, saying this does not do justice to either system.
33. I am using 'teleological' and 'naturalistic' as in Bloor (1991: 5–14).
34. Philosophers have made the same conflation. For example, Boghossian (2001: 8) writes, 'While it may be plausible to ignore the truth or falsity of what I believe in explaining why I came to believe it, it is not plausible to ignore whether I had any evidence for believing it'. Thus, whereas I say that it is not plausible to ignore *his belief* that he had evidence, he says that it is not plausible to ignore *whether this belief is true*, even while granting that it may be plausible to ignore whether his first belief is true! If evidence helps cause belief by helping to cause the belief that it is evidence, Boghessian's first clause *applied to his belief about evidence* contradicts the second one *in its original form*. See also Hacking (1999: 232, note 13).
35. An alert reader points out that I first say that there is no support for the weaker claim that, in each case, the truth of the belief is a partial cause of its acceptance, if not a

necessary one. However, although I then thought better of opening this can of worms in the review, I forgot to remove the remark!

36. I assume they mean to be giving only a sample of spontaneous reactions that people commonly have.
37. It could be taken, as Bricmont and Sokal seem to take it, to imply that one cannot give a causal explanation of a belief until one has determined its truth value. In addition, Bloor does not explain what he means by 'the same kind' even though any two things are of 'the same kind' in infinitely many respects.
38. But, of course, criticism based on an unsupported claim is itself unsupported.
39. For example, because Sokal conflates the existence of evidence for Darwinism with the belief that it is evidence, he is blind to the need for a mechanism by means of which the truth of Darwinism helps make us believe it by making there be evidence for it *that helps make us believe it is evidence for it*.
40. They also claim to explain why my belief that the sheep is white is true. But, if they do not wish to appeal to its truth to help explain why I hold it, this is pointless. Although they do not say why they reject this appeal instead of joining it with the explanation they prefer, it would not be too surprising if their devotion to such explanations was tempered by the consideration that treating 'the side is white *because* the sheep is white' as a causal relation runs counter to the intuitive idea that to cause something is to 'make' it happen. They do say that, in some cases, an explanation may take the form of a logical implication but I can easily imagine them declining to do so in this one.
41. Indeed, I doubt that it had much, if anything, to do with sheep, except in the negative sense of my never seeing what appeared to be a counterexample.
42. Unlike X, without and independent investigation, Bricmont and Sokal are unwilling to accept that the experimentalists got result E because E is approximately true. They say that this is an empirical question, which can be investigated by competent scientists in the usual ways. But this seems to be true only for the alleged cause, not for the causal claim. Also, unless they come up with a nonquestion-begging reason to trust the 'competent' scientists but not the 'honest and conscientious' experimentalists, their independent investigation will have to be independently investigated by other competent scientists and so on, ad infinitum. Bricmont and Sokal's (2001a: note 9) talk of independent assessment has the same problem.
43. For example, by Bricmont and Sokal deciding that they have no good reason not to trust the 'honest and conscientious' experimentalists.

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