

Reply to Gabriel Stolzenberg

Jean Bricmont and Alan Sokal

Gabriel Stolzenberg's (2004) review of *The One Culture* (Labinger & Collins, 2001) makes some incisive observations concerning the Strong Programme, which merit further discussion. It is a pity, therefore, that he mars his essay with snide comments about philosopher Paul Boghossian ('had he not been doing his Ken Starr imitation?') and – ironically in the light of his focus on 'misreading' – tendentious misrepresentations of our own views. Let us, therefore, deal quickly with the latter, before addressing more substantive questions.

Referring to our views on the Strong Programme, Stolzenberg asserts (p. 79) baldly that we 'wish to see it dead'. This is a pure invention on Stolzenberg's part, without the slightest basis in what we have written.¹ Indeed, we are perplexed as to why Stolzenberg would choose to describe an intellectual debate in such emotion-laden (not to say militaristic) terms.² Quite simply, we think that some aspects of the Strong Programme are epistemologically and methodologically misguided; our essay in *The One Culture* was devoted to explaining our objections and the reasons behind them.

Stolzenberg also accuses us of misreading a second-round comment of Jane Gregory's. We think that Stolzenberg's accusation is unjustified; but since the point at issue is relatively minor – a comment made in passing on the way to discussing the central question raised by Gregory – we leave the reader to judge for herself.³

Stolzenberg cites (p. 80) approvingly Michael Lynch's 'thoughtful analyses of pop metaphysical one-liners by Sokal, Weinberg and Richard Dawkins'. But he does not ask why Lynch chose to analyse a one-liner taken from a three-page polemical article, *rather than* the 56-page book chapter devoted to a detailed analysis of questions from the philosophy of science.⁴

Finally, Stolzenberg takes us to task for prefacing a statement with the words 'it seems obvious that'; he claims that we give 'no argument' in favour of the assertion in question, and he concludes by asking sarcastically: 'Is this the kind of scholarship that they and their admirers favor?' But

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. Is this the kind of scholarship that Stolzenberg favours?⁵

Let us now proceed to discuss Stolzenberg's very interesting comments concerning the Strong Programme.

The central aim of the Strong Programme, as set forth by David Bloor and others, is to give a causal account of the acceptance of scientific ideas, while remaining 'impartial' (or 'symmetrical') as to whether they are true or false, rational or irrational.⁶ The controversy over the Strong Programme, as we point out, arises in part from ambiguity about how the symmetry and impartiality theses are to be understood. We illustrate the problem with two examples (cited by Stolzenberg), one from everyday life and the other from the history of physics. Stolzenberg (pp. 81–82) comments that:

In each of these statements, Bricmont and Sokal claim that the truth of a certain belief is a partial cause of its acceptance. However . . . 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 . . . Indeed, in their first example, they omit the many partial causes of the fact that it is raining today [such as the antecedent atmospheric conditions (J.B./A.S.)] . . .

We agree with Stolzenberg's comment, but would like to amplify it slightly, by observing that much of the apparent controversy stems in fact from differing notions of what it means for an explanation to be 'adequate'.

Philosophers of science have extensively discussed the notion of 'explanation', and it is not our purpose to enter into that debate here. Suffice it to say that when one 'explains' a fact (or alleged fact) A, it is always with reference to some other set of facts (or alleged facts) B, which are temporarily taken for granted for the purposes of the argument.⁷ The idea, roughly speaking, is that we 'explain' surprising facts in terms of unsurprising (or less surprising) ones, complicated facts in terms of simpler ones, etc. Note also that many aspects of B, and of the claimed link between B and A, inevitably remain implicit, being taken for granted by both speaker and listener (at least until someone demands that they be made explicit and subjected to questioning). Thus, when someone asks, 'Why did the United States and Britain invade Iraq?' and we reply, 'In part to control Iraq's oil resources', we take for granted the knowledge that Iraq possesses vast oil resources, that the western world consumes far more oil than it produces, that countries sometimes invade other countries in order to exploit their economic resources, etc.

The key point, however, is that there is no natural end to the explanatory process. Having 'explained' A in terms of some simpler facts B, one can then ask to 'explain' B in terms of some yet simpler facts C, and so on.

Indeed, one characteristic of modern science is precisely to demand deeper and deeper levels of explanation, in which the facts taken for granted are both fewer and (in some sense) simpler or more fundamental.⁸

Furthermore, how far one wishes to push the chain of explanation – or, conversely, at what point one considers the explanation to be ‘adequate’ – depends strongly on the interests (in both senses of the word) of the inquirer. Thus, when an evolutionary biologist explains the rise in drug-resistant tuberculosis as an effect of natural selection, a molecular biologist will naturally want to know the details of the mutations that rendered the tuberculosis bacilli resistant to particular antibiotics. When a chemist explains a reaction in terms of oxidation and reduction, a physicist will naturally seek a more fundamental explanation in terms of quantum mechanics and electrodynamics.

Let us stress that these different levels of explanation are complementary, not contradictory. But they are not ‘equally adequate’; rather, they stand in a relation of hierarchy, in which the deeper explanation subsumes and extends the less deep one. Thus, it is perfectly legitimate to explain A in terms of some simpler facts B, *and stop there*; but if someone explains A in terms of B, and *then goes on* to explain B in terms of some yet simpler (or more fundamental) facts C, this is in an important sense a *better* explanation. (How much better depends on the details of the case at hand, as well as on the purposes for which the explanation is sought.)

With these considerations in mind, let us now turn to the Strong Programme, and in particular to one of the concrete examples raised in our article: why did the European scientific community become convinced of the truth of Newtonian mechanics sometime between 1700 and 1750? Advocates of the Strong Programme are correct to claim that, *at least in principle*, one can provide a ‘complete’ explanation of the acceptance of Newtonian mechanics by 18th-century scientists without ever referring to the actual motion of the planets – it suffices to refer to the *observations* concerning planetary motion that were available to scientists of the time. Furthermore, in order to explain the beliefs of those scientists who did not *personally* make observations of planetary motion, it is not even necessary to refer to those observations; it suffices to refer to the published articles and books, public lectures and private conversations, and so on, through which all their knowledge of planetary motions was acquired.⁹ *In this narrow sense*, therefore, Strong Programmers can validly contend that it is possible to provide a ‘complete’ explanation of the acceptance of Newtonian mechanics that makes reference only to ‘social’ factors.¹⁰

But the unsatisfying nature of such a purely social explanation should be manifest. If one aims to explain scientist X’s belief in some theory, one *can*, if one wishes, answer ‘because Y and Z published papers purporting to give strong experimental evidence in favor of that theory’, *and stop there*; but it is not *natural* to stop there. One would quite reasonably want to know, ‘*Why* did Y and Z publish papers purporting to give experimental evidence E?’ And the answer might be, ‘Because Y and Z performed experiments whose results were E, and they are honest and conscientious

scientists.’ (Or the answer might alternatively be, ‘Because Y and Z, though their experiments gave results quite different from E, believed that they could attain fame or fortune by publishing papers purporting to find E.’ The correct answer is, in each concrete case, an empirical question of history and sociology.) Nor is it natural to stop there: if Y and Z performed experiments whose results were E, one would quite reasonably want to know, ‘*Why* did Y and Z’s experiments yield result E?’ And the answer might be, ‘Because E is at least approximately the way the world is.’ (Or it might alternatively be, ‘Because Y and Z’s experiments were afflicted with a systematic error that led them to mistakenly observe E.’ This is once again an empirical question, which can be investigated by attempting to replicate the experiments, by carrying out related experiments, by investigating the internal consistency of Y and Z’s data, and so on.)

It is in this sense – a limited sense, to be sure, but a crucial one in our opinion – that we contend that any *truly adequate* explanation for the acceptance of Newtonian mechanics must include, as one element, the fact that the planets and comets really do move (to a very high degree of approximation, though not exactly) as predicted by Newtonian mechanics.¹¹

Stolzenberg (p. 82) makes one other criticism of our view. He says:

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.

So far so good; but Stolzenberg then goes on to assert that: ‘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.’ Not true! Far from conflating evidence with belief, we explicitly observe – even in the trivial example of a person standing in the rain who says ‘it is raining today’ – that ‘no one today knows the complete details of the causal mechanisms’ that lead a person standing in the rain to *believe* he is standing in the rain. And it goes without saying that, in the case of scientific theories, the relation between evidence and belief is yet more complicated; many issues in the history of science revolve around precisely this question.

Stolzenberg (p. 82) also points out that:

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.

Well, yes and no. The first question one should ask is, *why* is it that whenever one side of a sheep looks white to me, I believe that the sheep is white? And the answer is of course that in the past I have seen *both* sides of

a large number of sheep, and they were unfailingly monochrome; moreover, I possess a brain that is predisposed to make inductive inferences in certain circumstances – *these* facts together explain my propensity to believe that sheep observed to be white on one side are in fact fully white. Furthermore, the biology of sheep is stable over short periods of time, so if in my youth all (or nearly all) sheep were monochrome, the same is likely to be true today; the whiteness of the sheep today is thus *causally correlated* with the facts that led me to believe, decades ago, that when one side of a sheep is white, the other side is probably white too. The reply that Stolzenberg (p. 82) concocts on our behalf ('Bricmont and Sokal might say ...') misses the main point.

Stolzenberg concludes his review by noting that participants in this debate are divided by 'two radically different conceptions of reasoning':

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. (p. 86)

According to Stolzenberg, these are 'incommensurable mindsets'. But in our view, the divide is rather more banal. For there is no incompatibility whatsoever between descriptive and normative investigations of an issue, provided that one takes care to distinguish the two. Indeed, we all use both types of analysis in our everyday life.¹² Philosophers may be more interested in normative analyses, and sociologists in descriptive analyses; but this is no excuse for philosophers to make errors of sociological description, or for sociologists to make errors of logic.

Notes

1. It is telling that Stolzenberg does not cite even a single word from our text in support of this characterization of our alleged attitudes, even though he does quote extensively from our essay when discussing the content of the Strong Programme.
2. The strongest emotion mentioned in our own text is 'irritation' at the proliferation of sloppy relativist ideas (of which we cite five examples from prominent Science Studies practitioners).
3. Gregory writes (p. 202):

In science, replications, peer-review and publication in *Nature* are usually good enough: the end-product is usually well on its way to becoming what Bricmont and Sokal might call 'reality' or 'truth'.

We reply (p. 251):

To begin with, this grossly misunderstands what we mean by 'truth': as we explained at length in our essay, 'truth' signifies for us 'correspondence with reality'; it thus makes no sense to say that an assertion *becomes* true through replication, peer-review and publication. But more importantly, while 'replications, peer-review and publication in *Nature*' can constitute *evidence* (sometimes strong evidence) for the truth of a scientific claim, they are by no means conclusive ...

and we then proceed to discuss at length the specific case cited by Gregory, namely Jacques Benveniste's claims concerning the alleged 'memory of water'. Stolzenberg, in citing Gregory's sentence, appends to it the phrase 'i.e., to becoming accepted by them as true'; but this reformulation is Stolzenberg's, not Gregory's. Indeed, our own second

sentence provides a charitable reinterpretation of Gregory's sentence along lines similar to Stolzenberg's, by referring to 'evidence'.

4. The one-liner analysed by Lynch appears in the article in which Sokal reveals his *Social Text* parody (Sokal, 1996). The book chapter not mentioned by Lynch is Sokal & Bricmont (1998: chap. 4). For further elaboration of our philosophical views, see Bricmont & Sokal (2004).
5. We do think, nevertheless, that our discussion of the issue in question was too brief; we will amplify it later.
6. See for example, Bloor (1991: 7, quoted on p. 39 of our article). See also Barnes & Bloor (1981: 21–47).
7. We refrain from discussing in detail here the nature of the claimed relation between B and A. In some cases it might be 'B logically implies A'; but in other cases it might be, for example, 'the conditional probability of A given B is (vastly) higher than the conditional probability of A given not-B', or some other type of evidential relation.
8. For an excellent illustration of the demand for deeper levels of explanation, see Weinberg (1992: chapter II).
9. Note for mathematicians: we like to think of this as the 'Markov property' in the sociology of science – by analogy with the Markov property in probability theory, according to which (for a certain class of stochastic processes called Markov processes) 'the past affects the future only through the present', or 'the exterior of a region affects the interior only through the boundary'.
10. It is important to stress that we are here using the word 'social' in an extremely wide sense; indeed, the 'social' factors at issue here are merely the social *encodings* of the scientific evidence. By contrast, Strong Programmers, in *their practice*, often interpret 'social' in a much narrower sense, referring primarily to professional and class interests, struggles for power and prestige, and so on. We emphasize that analyses restricted to causal factors of this latter kind are *not* necessarily valid, as they may omit important causes (notably the scientific evidence).
11. See also footnote 31 of our article (pp. 40–41) in which we make this assertion more precise.
12. See, for example, Linda Feldmann (2003: 2), in which the journalist attempts to analyse why 45 % of US citizens believe that Saddam Hussein was personally involved in the 11 September attacks, even though there is no credible evidence to support this belief. For a more detailed recent study, see Steven Kull et al. (2003).

References

- Barnes, Barry & David Bloor (1981) 'Relativism, Rationalism and the Sociology of Knowledge', in Martin Hollis & Steven Lukes, eds, *Rationality and Relativism* (Oxford: Blackwell): 21–47.
- Bloor, David (1991) *Knowledge and Social Imagery*, 2nd edn (Chicago, IL: University of Chicago Press).
- Bricmont, J. & Alan Sokal (2004) 'Defense of a Modest Scientific Realism', in *Proceedings of the Bielefeld–ZiF Conference in Welt und Wissen – Monde et Savoir – World and Knowledge* (in the Press, Springer-Verlag).
- Feldmann, Linda (2003) 'The Impact of Bush Linking 9/11 and Iraq', *Christian Science Monitor*, 14 March: 2.
- Kull, Steven et al. (2003) 'Misperceptions, The Media and The Iraq War', The PIPA/ Knowledge Networks Poll, Program on International Policy Attitudes (PIPA), 2 October 2003. <<http://www.pipa.org/>> (accessed 27 November 2003)
- Labinger, Jay A. & Harry Collins (eds) (2001) *The One Culture? A Conversation about Science* (Chicago: University of Chicago Press).
- Sokal, Alan (1996) 'A Physicist Experiments with Cultural Studies', *Lingua Franca* 6(4): 62–64.

Sokal, Alan & J. Bricmont (1998) *Fashionable Nonsense: Postmodern Intellectuals' Abuse of Science* (New York: Picador): 50–105.

Stolzenberg, Gabriel (2004) 'Kinder, Gentler Science Wars', review of Labinger & Collins (2001), *Social Studies of Science* 34(1): 77–89.

Weinberg, Steven (1992) *Dreams of a Final Theory* (New York: Pantheon).

Jean Bricmont is Professor of Theoretical Physics at the University of Louvain, Belgium.

Address: Institut de Physique Théorique, Université Catholique de Louvain, 2 chemin du Cyclotron, B-1348 Louvain-la-Neuve, Belgium; fax: +32 10 472 414; email: bricmont@fyma.ucl.ac.be

Alan Sokal is Professor of Physics at New York University.

Address: Department of Physics, New York University, 4 Washington Place, New York, NY 10003, USA; fax: +1 212 995 4016; email: sokal@nyu.edu