Errata and Auto-Critique[1]

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Initially, I intended to compose only a list of errata that entered the texts of "Kinder, gentler science wars" and "Replies to the replies" (*Social Studies of Science*, February 2004: 77-90, 115-132) after I signed off on them. However, while doing this, I discovered more serious problems of my own making. Hence, the auto-critique.

The Errata

Gibberish: The second sentence in the last paragraph of 'A truly inadequate explanation' (128) should read:

> 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 the explanation currently rests. Their asymmetrical approach, on which causal irrelevance of authenticity is ignored, requires an independent determination of whether the experiments that convinced X produced truth or falsehood.

In the published version, the fragment, 'the explanation currently rests. Their asymmetrical approach, on which', is omitted. The result is gibberish. Speaking of which, in note 42, 'and independent investigation' should be 'an independent investigation'.

The new is born but the old is not yet gone. In note 31 (130), the bit, 'in the argument', is mistakenly retained from the previous version, which read:

> In such cases, I almost always argue only that a reading is unjustified, e.g., by revealing a logical flaw in the argument, not that it is incorrect.

The note is supposed to read:

> 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, not that it is correct.

Both statements are true and I no longer see any good reason for preferring the second.

Errata minores: On page 119, line 16, the word 'of' does not belong. And in note 34 (130), 'Boghessian' should be 'Boghossian'.

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[1] Updated July 2, 2004

Auto-Critique

Not a year but a decade: In the third paragraph of page 120, '50 years ago' refers only to the beginning of a decade of mathematical research that lasted from approximately 1955 to 1964. (See John Wermer's introduction to Errett Bishop's posthumously published, *Selected Papers*, World Scientific Publishing Co. 1986.)

Missing references: In note 30 (130), I forgot to supply references for my criticism of the philosophy of science chapter in *Fashionable Nonsense*. I will correct this here. In 'Reading and relativism', see 'The correct answer, take two' and 'The infinite regress for evidence' (56-57). In 'A physicist experiments with scholarly discourse' at my web site, see 'Sloppy thinking about science studies', 'The irrelevance of authenticity for belief causation' and, in the appendix, 'Nature as the external referee?' and 'The reality of the past'. Except for the appendix, this criticism is of views expressed by Sokal alone in 'What the *Social Text* affair does and does not prove'.[2] But they are repeated almost verbatim in *Fashionable Nonsense*. Finally, in 'Professor Nagel's *Fashionable Nonsense*' also at my web site, see 'A sensible chapter? Take two'. There I criticize views about philosophy of science that Nagel attributes approvingly to Sokal and Bricmont in his review of *Fashionable Nonsense*.

The Strong Programme notion of 'social': In my review, I say that the Strong Programme use of 'social' courts misunderstanding (78). But I do not say how it does it, not even that it does it, in part, by creating the impression that the Strong Programme notion of 'social' is the ordinary one. Nor do I say anything about how it *should* be understood.[3] Later (85), I add that it invites readers to assume, falsely, that the Strong Programme notion of 'social' does not 'penetrate fully' into the intellectual content of scientific research. But here, too, I should have said that this is, in part, because the Strong Programme literature often gives the impression that its notion of 'social' is the ordinary one.

Even later, on the same page, I say that if Bricmont and Sokal had discovered something in the intellectual content of the investigations of which they speak that seems necessary for explaining the acceptance of Newtonian mechanics and is free of any ordinary social or psychological influence, something of value might have been learned. But I failed to explain that I think this because, in my view, such a finding might have provided an occasion to call attention to certain social or psychological factors that are not ordinarily recognized as such. Finally, in note 18 (87), as an indication of the kind of factors that I have in mind, I mention the possibility of having to take into account the Wittgensteinian sense in which meaning is 'determined' socially. However, I give only the barest hint of what I mean by this,[4] pointing readers to the so-called 'rule-following' considerations in *Philosophical Investigations*.

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[2] See *A House Built on Sand*, N. Koertge, ed. or *After the Science Wars*, K. Ashman & P. Baringer, eds..

[3] Similarly, in my reply to Saulson, I say that we seem to have very different ideas of what the Strong Programme notion of 'social' is and why it is misunderstood. But I say nothing about what my idea of it is.

[4] I do slightly better in the section, 'Relativism and social constructivism', of 'Reading and relativism' (44-46).

In sum, although I claim that, contrary to a widely held view, the Strong Programme notion of 'social' is not the ordinary one, I say almost nothing about how it differs from it. But, however egregious this omission may be, I am not prepared to rectify it here.

Conflating conflations: In my review (82), I say that Bricmont and Sokal conflate the *fact* that something is evidence for a belief with the *belief* that it is evidence for it. In their reply to this charge, they say:

> Far from conflating evidence with belief, we explictly [observe 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….

In my response to this, I neglected to explain that the only way I see to make sense of this is to assume that, by 'conflating evidence with belief,' Bricmont and Sokal mean 'conflating evidence with the belief for which it is evidence' (125). But, if this reading is correct, they conflate *my* charge of conflation with a charge I do not make of a conflation *they* do not make, and then imply that I charge them, falsely, with making this other conflation![5]

My larger point (125-126), which may have been obliterated in the confusion over the three conflations, is this: Of course, we should seek evidence. However, if there is a question about how the truth of a belief can help to make us believe it, talk of evidence for it is not going to help. It merely raises the same question about a second belief—the question of how the truth of the belief *that something is evidence for the first belief* can help make us believe that it is.

An untenable distinction: Even I have difficulty following the reasoning in note 34 (130). Here is an attempt to do better.

My argument depends crucially on how Boghossian thinks evidence can help induce belief. More precisely, because he is silent on this vital point, it depends on how I think Boghossian thinks evidence can help induce belief. Recall that his view is that it is not plausible to ignore the role of evidence in explaining how a belief is acquired. Although he does not tell us what he thinks this role is, my impression is that, typically, in accounts in which evidence is portrayed as helping to induce a belief, those in whom the evidence is helping to induce it believe that it is evidence. For this reason, my best guess is that Boghossian thinks evidence can help to induce belief by helping to induce the (possibly unconscious) belief that it *is* evidence.[6]

But it is a consequence of this assumption that, if, as Boghossian says, it is not plausible to ignore the role of evidence in inducing the belief *for which* it is evidence, then it also is not

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[5] I don't do this for two reasons. Firstly, it is not the conflation I am talking about. Secondly, I know that they do not make it.
[6] After which the latter belief assumes the causal-explanatory burden.

plausible to ignore its role in inducing the belief *that* it is evidence. However, this is not consistent with Boghossian's claim that it may be plausible to ignore the role of the truth of *any* belief in explaining how it is acquired—in particular, the role of the truth of the belief that something is evidence for another belief.[7]

Finally, at the very end of note 34, I refer the reader to note 13 of Ian Hacking's *The Social Construction of What?* (232), but without saying why. I do so because there Hacking seems to make the same mistake as Boghossian. More precisely, he cites without criticism the view that "one should not use the truth of a true belief to explain why people hold or held it" and then, after saying that evidence is "quite another matter" from truth, he criticizes the view that "you cannot invoke the evidence available in a community for a belief in *p*, in order to explain why the people in the community believed *p*."

My 'truth-based' explanation: In speaking of *my* 'truth-based' explanation of my ewe hue belief (82; 127-128), I do not mean to give the impression that I accept it. I put it this way only to emphasize that it was I, not Bricmont and Sokal, who proposed it as an explanation that, in my opinion, is of the kind that they give in their two examples. Furthermore, although I do not say so, I did not expect it to find favor in their eyes because, despite its formal similarity to the explanations in these examples,[8] it seemed to me that the overtly logical character of the 'Because' in 'Because the sheep is white, so is the side I see' might give them pause.[9] Maybe it did because, not only do they reject this as an explanation, they seem offended by the suggestion that it is similar to ones of their own.

On the same subject: In the third sentence of note 40 (131), I note that Bricmont and Sokal choose not to join their explanation of why the sheep is white with 'mine' of why it looks white on the side that I see. But I fail to explain that the reason I mention this is that the proposed addition to their explanation of my belief (that the sheep I see is white) yields one that is 'more adequate' than theirs because it extends further back in a causal chain. To me, this is evidence of the inadequacy of their notion of 'more adequate'.[10]

Truly adequate, yet no causal mechanism? Even after the missing fragment is restored to 'A truly inadequate explanation'[11] (128), there remain flaws of exposition for which I alone am responsible. So, I will have another go at it here.

In shifting from talk about 'the' explanation to 'a truly adequate' one, Bricmont and Sokal drop their insistence upon the *need*, in some cases, to appeal to the truth of a belief (or any other truth) to explain its acceptance and instead attempt to demonstrate the superiority of

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[7] Note that this criticism is directed only at the logic of Bohgossian's claim, not at his general position. Even if he were to accept it, it would mean only that he should not have been so quick to allow the truth of a belief to be ignored in explaining its acceptance.

[8] E.g., compare 'Because the sheep is white, so is the side I see' with 'Because Newton's laws of motion hold everywhere, they hold where the astronomers looked' .

[9] See note 40 (131).

[10] See below and page 127.

[11] See the first item on the list of errata above.

such 'truth-based' explanations. For example, they claim that, because Strong Programme explanations (of the acceptance of a scientific belief) are not of this kind, they cannot extend as far back into an explanatory causal chain as theirs does—for which reason, theirs is 'more adequate'.[12] But, when they do go farther back, they provide no explanation worth talking about. There are no causal mechanisms! Newton at least had God pushing on the ether. But all Bricmont and Sokal offer us is whichever of the following two assertions we can prove to be correct:

> The approximate truth of the statement helped to make there be evidence for it, which in turn helped to convince scientists that it is true.

> The statement is false, but a mistake in assessing the evidence helped to convince scientists that it is true.

However, this is not the end of their account. They also imply that to decide which of these assertions is correct, it suffices to determine whether or not the statement in question is approximately true. But it does *not* suffice to determine this. E.g., in the first case, it must also be shown *how* the approximate truth of the statement helped induce the belief that it is true. Bricmont and Sokal have nothing to say about this. Nor do they seem to recognize that their prescription for how to decide whether the statement is or is not approximately true generates an infinite regress of assessments.

Finally, at the end of note 42 (131), I say that Bricmont and Sokal's talk about independent assessments in 'Reply to our critics' (*The One Culture?* note 9: 246) suffers from the same problem. I do not attempt to justify claim this there but I believe that the following excerpt suffices to make my point.

> [*If* sociologists] aspire to study the substantive content of scientific controversies…and *if* they want these studies to be logically sound, *then* they cannot avoid making an independent assessment of whether the experimental/observational data do in fact warrant the conclusions that various scientists drew from them…. Consequently, sociologists or historians who aspire to [do this] need to possess—or acquire—the scientific competence to make an independent assessment of the evidence, or else work together with scientist collaborators on whom they can rely for such an assessment.

But who are the 'scientist collaborators' on whom the sociologists or historians can rely for an assessment that is more reliable than that of the scientists who did the research and then scoured it for flaws or the critical scrutiny of their colleagues, who eventually accepted its conclusions? Also, in a case in which there are believed to be such 'scientist collaborators,' who assesses *their* assessments? Note, finally, how much of an explanatory burden the word 'independent' is asked to bear here.

Is talk of causal explanation unprincipled? I begin my criticism of Bricmont and Sokal's examples (81-82) by asking if they at least show that the truth of the beliefs considered in

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[12] In fact, the Strong Programme explanations can and do go further, but in different directions. Bricmont and Sokal seem unaware of this.

them *does* help to explain their acceptance—whether or not it is *needed* to do so.[13] And later (82), I say that a certain clash of intuitions would disappear if Bricmont and Sokal were content to find cases in which the truth of a belief (or, for that matter, *any* truth) plays *some* role in explaining its acceptance. But I did not explain that the only reason I allow that there might be such cases is that, however valuable a particular search for a causal explanation may be, I find talk of such explanations so unprincipled that, if someone were to say that she finds it useful to refer to the truth of a belief (or some other truth) to help produce a causal explanation of its acceptance, I probably would regard it as a purely subjective statement, not something to argue about. Of course, the author of such an explanation might well see it differently, in which case there might, after all, be something to argue about.

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[13] See note 35.