

A Physicist Experiments with Scholarly Discourse¹

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We intend to provide a place where reason and good sense can be brought to bear on a field that has lost its mechanism of scholarly self-control... Our only target is shoddy scholarship. (Noretta Koertge, *A House Built on Sand*: 5)

Contents

“It makes me laugh” is not an argument: 2

Deleuze and Guattari: Chaos theory?
Lacan: Differential topology?
Irigaray: Borders of fuzzy sets
Latour: Frames are not enough
Reading Latour reading Einstein

Sloppy thinking about science studies: 6

Latour’s third rule of method
Eliding distinctions
Redefining “knowledge”?
Taking it back?

The irrelevance of authenticity for belief causation: 10

A bright idea
Explaining belief causation
Newtonian mechanics
Now you believe it, take two
From creationism to Darwinism
The man who screamed “Elephants!”

Appendix: Fashionable nonsense: 15

Nature as the external referee?
The reality of the past
We can all be iconoclasts.

Here I complete my defense of my claim in “Reading and relativism” that, in the opening chapter of *A House Built on Sand*, Alan Sokal’s “mechanism of scholarly self-control” goes haywire. To this end, I first debunk four more of his claims (11-13) about the “silliness” of certain statements about science or mathematics by French intellectuals.² I then turn to his accusations of sloppy thinking and rhetorical deception on the part of constructivist sociologists of science (13-17) and

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² I debunked two about Bruno Latour in “I am not a reference frame.”

debunk them too. In the third section, I do the same for Sokal's claims about the role of truth in belief causation and his novel proposal for how to avoid conflating ontology with epistemology. Finally, in an appendix, I discuss three blunders made elsewhere that are closely related to ones of Sokal that I consider here. Two are from *Fashionable Nonsense* by Sokal and Jean Bricmont and one is from Meera Nanda's chapter in *A House Built on Sand*.

“It Makes Me Laugh” Is Not an Argument

[The *Social Text*] article is structured around the silliest quotations I could find about mathematics and physics (and the philosophy of mathematics and physics) from some of the most prominent French and American intellectuals. (Sokal, *A House Built on Sand*: 11)

But is it the quotations that are silly or Sokal's readings of them? How can we tell? Many reasonable statements admit ludicrous misreadings. We need arguments. But Sokal doesn't offer any. Indeed, he seems oblivious to the need for them.³ He continues:

Now, what precisely do I mean by “silliness”? ...First of all, one has meaningless or absurd statements, name-dropping, and the display of false erudition. (11)

This is a good definition or at least the beginning of one. But having given it, Sokal promptly launches into his own display of name-dropping, false erudition and absurd statements! This runs from the top of page 12 through “OK, enough for examples of nonsense” on page 13. I quote some fragments of it below. The name-dropping is evident.⁴ That there is also false erudition and absurd statements will be demonstrated below.⁵ Sokal writes (12):

Here, for instance, are Gilles Deleuze and Félix Guattari holding forth on chaos theory....⁶

And there's much more—Jacques Lacan and Luce Irigaray on differential topology...⁷

—but don't let me not spoil the fun.⁸

...[Latour] claims that relativity cannot deal with the transformation laws between two frames of reference but needs at least three.⁹

I will spoil the fun. Sokal commits at least four significant mistakes.

³ Recall Socrates' admonition to Polus, quoted in “What's this, Polus?”

⁴ Sokal boasts that his targets are “some of the most prominent French and American intellectuals” and that “we're dealing with important intellectuals, at least as measured by shelf space in the cultural-studies section of university bookstores.”

⁵ I have already demonstrated this in “I am not a reference frame” (“Reading and relativism” (39-40)) but much more evidence will be presented here.

⁶ This is followed by a quote by Deleuze and Guattari that also is quoted in the hoax article.

⁷ Sokal also mentions Jean-François Lyotard on cosmology and Michel Serres on nonlinear time.

⁸ Did anyone proof read this? I see one ‘not’ too many.

⁹ This is followed by a quote by Latour that also is quoted in the hoax article.

Deleuze and Guattari: Chaos theory? As Sokal now knows, contrary to his jeering, “Here, for instance, are Gilles Deleuze and Felix Guattari holding forth on chaos theory,” the authors are *not* talking about chaos theory. Talk about false erudition.¹⁰ So far as I know, Sokal has not publicly acknowledged this blunder. However, in 1997, in a lecture following one by Sokal, Arkady Plotnitsky pointed out that, not only that the passage in question not about chaos theory, it doesn’t even look as if it is. Moreover, in 1998, in *Fashionable Nonsense* (156), Sokal and Jean Bricmont themselves took pains to make clear that it is *not* about chaos theory.¹¹ But they did not tell their readers that this contradicts Sokal’s jeering remark in *A House Built on Sand*.

Lacan: Differential topology? Differential topology is sophisticated mathematics. There is nothing in the passage that Sokal describes as “Lacan on differential topology” that requires any knowledge of it. Nor does Lacan pretend otherwise. But by describing the passage this way, Sokal encourages his readers to suppose falsely that, in it, Lacan pretends to know sophisticated mathematics. Here is the passage in question.

This diagram [the Möbius strip] can be considered the basis of a sort of essential inscription at the origin, in the knot which constitutes the subject. This goes much further than you may think at first, because you can search for the sort of surface able to receive such inscriptions. You can perhaps see that the sphere, that old symbol for totality, is unsuitable. A torus, a Klein bottle, a cross-cut surface, are able to receive such a cut. And this diversity is very important as it explains many things about the structure of mental disease. If one can symbolize the subject by this fundamental cut, in the same way one can show that a cut on a torus corresponds to the neurotic subject, and on a cross-cut surface to another sort of mental disease.

Although this *can* be formulated in terms of differential topology, it requires nothing nearly so fancy. It is mathematics for an eager amateur, the sort of thing one can find in a popular book. Furthermore, on Sokal’s view of Lacan, if he was pretending to use something as fancy as differential topology, he would have been sure to let us know. As for the quote itself, although I do not understand it well enough to judge it, I can say this much. Besides the Möbius strip, the surfaces that interest Lacan arise from the three different ways of ‘gluing’ the opposite edges of a rectangle, depending on whether orientations are preserved or reversed. His evident familiarity with this and the distinction he notes between such surfaces and a sphere (one cannot draw a knot on a sphere) suggests a better command of the mathematics than I expected him to possess.

Irigaray: Borders of fuzzy sets. Here is the remark of Irigaray to which Sokal is referring when says, “And there’s much more—Jacques Lacan and Luce Irigaray on differential topology—but don’t let me not spoil the fun.”

The mathematical sciences, in the theory of sets, concern themselves with closed and open spaces... They concern themselves very little with the question of the partially

¹⁰ The quote does contain mathematical language—simple stuff about limits and variables, awkwardly presented but otherwise innocuous. But Sokal has nothing to say about it. His sole *aperçu*, possibly evoked by the noticing the expression ‘a limit in chaos,’ is that the authors are “holding forth on chaos theory.” Hurray for scholarship.

¹¹ They say, “the word ‘chaos’ is *not* being used here in its usual scientific sense (see Chapter 7 above),” where its “usual scientific sense” is that of chaos theory.

open, with sets that are not clearly delineated [*ensembles flous*], with any analysis of the problem of borders [*bords*]...¹² (Irigaray)

There is nothing here about differential topology. Indeed, on my reading, Irigaray is not talking about *any* kind of topology except to note correctly that little if any of it promises to be of much use in studying the problem of borders for sets that are not clearly delineated, i.e., for fuzzy sets or vague predicates. If Sokal finds this a hoot, he probably missed the obvious connection between “the problem of borders” and “sets that are not clearly delineated.” The following statement in *Fashionable Nonsense* (120-121) strongly suggests that he did.

For what it’s worth, the “problem” of boundaries [*bords*], far from being neglected, has been at the center of algebraic topology since its inception a century ago, and “manifolds with boundary” [*variétés a bord*] have been actively studied in differential geometry for at least fifty years.¹³ (Sokal and Bricmont)

As a reply to Irigaray, this is risible. What do the boundaries [*bords*] of algebraic topology have to do with the problem of borders [*bords*] for vague predicates, e.g., for color names? In a novel I just read, Milan Kundera says about one of the characters:

He knew there existed a border beyond which murder is no longer murder but heroism, and that he would never be able to recognize just where that border lay. (*Immortality*, 105)¹⁴

Is Sokal suggesting that with the aid of algebraic topology, with its great theorems about cycles and boundaries, Kundera’s character might after all be able to recognize just where that border lay?¹⁵ I am sure he is not and that the reason it seems otherwise is that he has failed to consider the relevance of Irigaray’s mention of fuzzy sets for understanding *what kind* of borders she is talking about.¹⁶ Indeed, it is precisely the fuzziness of a fuzzy set—the vagueness of a vague predicate—that makes its border problematic.

Latour: Frames are not enough. Finally, contrary to what Sokal would have us believe, in the quote below, Latour does not say that relativity “cannot deal with” *the transformation laws between* two frames of reference but “needs at least three.”

If there are only one, or even two, frames of reference, no solution can be found.... Einstein’s solution is to consider three actors: one in the train, one on the embankment and a third one, the author [enunciator] or one of its representants, who tries to superimpose the coded observations sent back by the two others.¹⁷

¹² For proof that this is indeed the quotation, see p. 231 of *Fashionable Nonsense* and then pp. 228-32.

¹³ This is a response to a remark by Irigaray from which the quote above is taken. There is nothing in it about manifolds with or without boundary. Also, “geometry” should be “topology” but let’s pretend we didn’t see this.

¹⁴ Here is a more conventional example. When the letter ‘O’ is morphed into ‘D,’ which is then morphed back into ‘O,’ in each direction, one observes a break point or border. But where it occurs depends upon the direction.

¹⁵ And psycho-physicists might better understand hysteresis?

¹⁶ How likely is it that she would mention the ‘problem of borders’ right after mentioning fuzzy sets and not mean *that* problem of borders? At the very least, Sokal has no warrant to dismiss this reading.

¹⁷ This is the text on which Sokal bases his accusation in *A House Built on Sand*. For a longer passage that contains the full quote given in Sokal’s hoax article, see “Reading Latour reading Einstein,” below.

The first sentence mentions two frames but says *nothing* about transformation laws or a need for a third frame. The second sentence has nothing about relativity theory not being able to deal with the transformation laws between two frames or needing a third *frame*. How then did Sokal ‘find’ these two assertions in Latour’s remark? For the one about the transformation laws, see “Reading Latour reading Einstein” just below. As for the claim that Latour says we need a third frame, he does say that we need a third *actor*. Determined to make it be a frame, Sokal assumes that Latour doesn’t understand the difference between an actor and a frame of reference! For a more thorough discussion, see “Reading Latour reading Einstein” below and “I am not a reference frame” in “Reading and relativism.”

Reading Latour reading Einstein: Here is a longer passage from which the one above is taken. I will use it to explain what I understand Latour to be saying.

How can one decide whether an observation made in a train about the behaviour of a falling stone can be made to coincide with the observation made of the same falling stone from the embankment? If there are only one, or even two, frames of reference, no solution can be found since the man in the train claims he observes a straight line and the man on the embankment a parabola. Thus nothing tells us if it is the same stone acting according to the same law of physics. Each observer has ‘its’...own irreducible vision of the world. Einstein’s solution is to consider three actors: one in the train, one on the embankment and a third one, the author [enunciator] or one of its representants, who tries to superimpose the coded observations sent back by the two others.... [W]ithout the enunciator’s position (hidden in Einstein’s account), and without the notion of centres of calculation, Einstein’s own technical argument is ununderstandable...

On my reading, Latour first says that *without* the transformation laws specified by relativity theory, one cannot compare observations made in different frames. I get this by taking the “are only” in “if there are only one, or even two, frames” to mean “is nothing but.”¹⁸ E.g., imagine two observers in uniform relative motion and one, or even both, equipped with a frame of reference but ignorant of the Lorentz transformations between them.¹⁹ In such circumstances, Latour is evidently right. No comparisons can be made. And he also is right when he says that *if we do have* the Lorentz transformations, then we *can* make such comparisons—by transforming all the observations into a common frame, which may but need not be one of those in which one of the observations was made, and doing the book-keeping there.

As for the third actor, Latour’s observers can make observations (and maybe even transmit data) but it is not part of their job description to make comparisons. The third actor is a person or device that uses the Lorentz transformations to do this. Yes, it does need a frame in which it translates and compares the data received from the two observers. *But this frame need not be different from the first two frames* nor does Latour ever suggest such a thing. On the contrary, elsewhere in his essay, he emphasizes that there are no “privileged” frames. In principle, any frame, in particular, one of the first two frames, will do. Latour takes explicit note of this in his essay.

¹⁸ Think about the French counterparts.

¹⁹ Perhaps they are even unaware of each other’s existence.

If...the man in the train describes scenes according to instruments, which, after a few transformations, are made equivalent to the ones seen by the man on the embankment, this means that the latter will gain something. Without being on the train, the man on the embankment will have 'its' point of view *plus* another one compatible and addable to the first.²⁰

As a mathematician, when I talk about 'three things,' for example, the three roots of a cubic equation, I don't necessarily mean three *distinct* things. It depends on the context. So I am comfortable saying that the third actor comes with a third frame, which *may but need not be* one of the first two frames. For me, the virtue of Latour's way of looking at this becomes evident when there are more than two frames in which observations are made. His answer for ten frames is the same as for two: Transmit the observations to an actor who transforms them into a common frame and then tries to make comparisons.²¹

Sloppy Thinking about Science Studies

The remaining part of Sokal's chapter that I will criticize has trappings of scholarship. We are back in the realms of "Relativism and social constructivism" and "Reality is hard to talk about."

OK, enough for examples of nonsense (although a lot more are available). More interesting intellectually, I think, are the sloppy thinking and glib relativism that have become prevalent in many parts of Science Studies (albeit not, by and large, among serious philosophers of science). When one analyzes these writings, one often finds radical-sounding assertions whose meaning is ambiguous and that can be given two alternative readings: one as interesting, radical, and grossly false; the other as boring and trivially true. (Sokal, 13)

Latour's third rule of method: In defense of this claim, Sokal gives four alternative readings of Latour's third rule of method (*Science in Action*, 99)—two of which he says are "boring and trivially true" and the other two "interesting, radical, and grossly false." He begins by quoting Latour.

Here is [Latour's] Third Rule of Method: "Since the settlement of a controversy is the *cause* of Nature's representation, not the consequence, we can never use the outcome—Nature—to explain how and why a controversy has been settled." (Sokal, 13)

However, Sokal pays no attention to how well or badly any of his four readings of this sentence fits with the rest of Latour's seven-page discussion in *Science and Action* about the appeal of

²⁰ On my reading, Latour is saying roughly: Suppose the man on the train can plot his observations of a falling stone (or any other event) and transmit the graph to the man on the embankment, who transforms it into his 'embankment' coordinates. If he finds that it agrees with his own graph of a falling stone (or any other event he observed), he knows they observed the same event.

²¹ Or each observer transforms its observations into the common frame and transmits them in this form to the actor, who compares them.

appealing to Nature and the difficulty in so doing.²² As scholarship this is risible. Even worse, as I show below, Sokal misunderstands some of his own readings. Here is the first.

[Latour] slips, without comment or argument, from “Nature’s representation” in the first half of this sentence to “Nature” tout court in the second half. If we were to read “Nature’s representation” in both halves, then we’d have the truism that scientists’ representations of Nature (i.e., their theories) are arrived at by a social process and that the course and outcome of that social process can’t be explained *simply* by its outcome. (Sokal, in *A House Built on Sand*, 12, italics added)

But Latour is not asserting the truism that neither the course nor outcome of the process can be explained *simply* by its outcome. He is making the far stronger claim that, even in conjunction with other considerations, the outcome of a scientific controversy cannot be used to *help* explain how it was reached. This is a very different proposition. Indeed, for Sokal, far from being a truism, it is blatantly false.

On Sokal’s next reading, Latour’s rule is not only false but crazy. It begins:

If, however, we take seriously “Nature” in the second half, linked as it is to the word *outcome*, then we would have the claim that the external world is created by scientists’ negotiations: a claim that is bizarre, to say the least, given that the external world has been around for about 10 billion years longer than the human race.

The claim may be bizarre but not for the reason that Sokal thinks. The external world is four dimensional, not three. E.g., the atomic number of gold is a four-dimensional fact. Therefore, even if it is “created” *by* scientists’ negotiations, it is “created” *as* something that exists not only in space but also in time—not only in the present but also in the future and the past. This is an especially bad blunder for a physicist to make.²³

Here, finally, are Sokal’s remaining two alternative readings.

Finally, if we take seriously “Nature” in the second half but expunge the word *outcome* preceding it, then we would have either (1) the weak (and trivially true) claim that the course and outcome of a scientific controversy cannot be explained solely by the nature of the external world (obviously some social factors play a role, if only in determining which experiments are technologically feasible at a given time, not to mention other, more subtle social influences) or (2) the strong (and manifestly false) claim that the nature of the external world plays no role in constraining the course and outcome of a scientific controversy. (Sokal, 13)

Note that in both (1) and (2), the first clause of Latour’s third rule of method is ignored, even though the second one is supposed to be a consequence of it! Yes, in one case, the second clause seems to be a truism and, in the other, it seems false. But in both cases, by interpreting ‘Nature’ to include the whole external world, Sokal renders nonsensical Latour’s claim that the second clause is a consequence of the first. However, if instead we take ‘Nature’ to refer only to that bit

²² “Appealing (to) Nature” (94-100).

²³ See “The reality of the past” in the Appendix below.

of the external world that the controversy was about, then, with or without ‘outcome,’ it is not nonsensical. Moreover, even with ‘outcome’ deleted, the claim is not the “boring and trivially true” remark that one cannot explain the settlement of a scientific controversy *solely* by reference to the nature of the external world. Nor is it the factual claim that Nature *plays no role* in constraining the course and outcome of a scientific controversy. It says rather that Nature *cannot be used to help explain* how such a controversy was settled.²⁴ Given how concerned Sokal is not to elide the distinction between fact and knowledge, this is a very bad blunder.

Eliding distinctions: Sokal soon goes on to say:

It seems to me that much sloppy thinking in Science Studies, like that in Latour’s Third Rule of Method, involves conflating conceptions that need to be distinguished. Most frequently this conflation is accomplished by terminological fiat: the author intentionally uses an old word or phrase in a radically new sense, thereby undermining any attempt to distinguish between the two meanings. The goal is to achieve by definition what one could not achieve by logic. For example, we often find phrases like “the social construction of facts” that intentionally elide the distinction between facts and our knowledge of them.

In the first sentence above, Sokal seems to forget that he does not claim—much less attempt to show—that any of his four alternative readings of Latour’s rule is faithful to Latour’s meaning. He seems to have deluded himself into thinking that, by showing, as he believes he has, that all four of *his* Latours conflate “conceptions that need to be distinguished,” he has demonstrated that the real Latour does the same. Furthermore, his charge that Latour aims to “achieve by definition what he could not achieve by logic” stands the truth on its head. Latour tries, perhaps more than anyone else who writes about these matters, to achieve his goal by logic. Whether he succeeds is another question, one that Sokal does not address. For example, although one would never guess from listening to Sokal, in *Laboratory Life: The Social Construction of Scientific Facts*, Latour and Steve Woolgar devote more than eighty pages to laying out their reasons for talking about the construction of a fact rather than about a discovery of one. Accusing Latour of attempting to “achieve by definition what he could not achieve by logic” without refuting the apparent counter-evidence of this well-known book is an example of what I mean by Sokal’s mechanism of scholarly self-control going haywire.²⁵

In the last sentence above, Sokal says that in constructivist science studies, we often find phrases that “intentionally elide the distinction between facts and our knowledge of them.” He takes for granted that ‘the social construction of facts’ is an example of this. But good scholarship requires an argument. Maybe one reason he thinks this is that a fact often is about something that pre-dates our knowledge of it but to say that it is ‘socially constructed’ suggests otherwise. However, I have already explained why this reasoning is fallacious.²⁶ Consider also this. For Sokal, a fact is a perspective-independent truth. But unless social constructivists are metaphysical realists in drag, it is hard to see how they would ever have occasion to talk about perspective-independent truth, much less elide the distinction between such a truth and our knowledge of it. I suspect that

²⁴ Latour says that we cannot *know* what Nature is doing in a certain situation but Sokal takes him to say that Nature cannot *be* doing it. Thus, he mistakes an epistemological claim for an ontological one.

²⁵ If Sokal were making his accusation now, the best counter-evidence would be Latour’s *Pandora’s Hope* (1999).

²⁶ See my critique of Sokal’s second reading of Latour’s third rule of method.

what disturbs Sokal about the phrase “the social construction of facts” is not any conflation of facts with our knowledge of them—there is none—but that substituting “social construction” for “discovery” seems to get in the way of affirming metaphysical realism.

Sokal does seem to believe that constructivists are metaphysical realists in drag.²⁷ His position seems to be that there is only one position, realism, and that much, if not all, constructivist talk is merely jibber jabber that people who have nothing interesting to say sometimes use to attract attention.²⁸ In support of this, Sokal presents several examples in which social constructivists appear to invent exciting-looking nonstandard meanings for words like “true” and “know” but soon retreat to the standard ones. I consider some of these examples below.

Redefining ‘knowledge’? In “Reading and relativism” (47-48), I debunked Sokal and Bricmont’s claim that Barry Barnes and David Bloor make “a radical redefinition of the concept of truth.” Here I debunk a similar claim by Sokal that they also “redefine *knowledge*.”

Or to take another example, philosophers usually understand the word *knowledge* to mean something like “justified true belief” or some similar concept; but Barnes and Bloor redefine *knowledge* to mean any collectively accepted system of belief. Now, perhaps Barnes and Bloor are uninterested in inquiring whether a given belief is true or rationally justified; but if they think these properties of beliefs are irrelevant to their purposes, then they should say so and explain why, without confusing the issue by redefining words. (Sokal, 14)

They should say so and explain why? Sokal knows very well that the irrelevance of truth and justification for explaining belief acquisition is a central tenet of the Strong Program and that Barnes and Bloor, separately and together, have explained this project many times.²⁹ Moreover, in *Lingua Franca*, Sokal appears to have committed the same sin of which he accuses Barnes and Bloor: “redefining” the word *knowledge*. There, in the course of lecturing us on the correct way to think about our relationship to reality, Sokal says that human beings can and do obtain “reliable, albeit imperfect and tentative knowledge of [physical] laws’.” And on the same page on which he accuses Barnes and Bloor of “redefining *knowledge*,” he writes, “*Epistemology*. How can human beings obtain *knowledge* of truths about the world? How can they assess the *reliability* of that knowledge?” Did Sokal then think that justified true belief can be unreliable? Did he think that it can be imperfect and tentative? Did he confuse knowledge with evidence?³⁰

Taking it back? Sokal claims that elsewhere Bloor reverts to “the standard definition.

Note that Bloor, only nine pages after giving his nonstandard definition of “knowledge,” reverts without comment to the standard definition of “knowledge,” which he contrasts with “error”: “It would be wrong to assume that the natural working

²⁷ I say this with confidence about Sokal because it is the standard metaphysical realist view of constructivists and, more generally, anti-realists. See, for example, the two attempts by Paul Boghossian to prove this about relativists, both of which I debunk in “Reasoning about relativism.”

²⁸ See “Ambiguity as subterfuge” in *Fashionable Nonsense* (189).

²⁹ See, for example, Bloor’s *Knowledge and Social Imagery*.

³⁰ Although I think he did, for Sokal’s use of the first locution in his *Social Text* article, “evidence” doesn’t seem to be an option. There he is pretending to endorse a view held by others that is expressed in terms of knowledge, not evidence, the intimate relationship between the two notwithstanding.

of our animal resources always produces knowledge. They produce a mixture of knowledge and error with equal naturalness.” (Sokal’s footnote 28)

To see that this is mistaken, we need only substitute ‘beliefs that are collectively shared’ for ‘knowledge,’ ‘beliefs that are collectively rejected’ for ‘error’ and take into account how Bloor proposes to distinguish knowledge from ‘mere’ belief.

Of course knowledge must be distinguished from mere belief. This can be done by reserving the word ‘knowledge’ for what is collectively endorsed, leaving the individual and idiosyncratic to count as mere belief. (Bloor)

With this understanding, Bloor’s offending remark says only that the working of our animal resources (which differs from person to person, if only because people have significantly different experiences and thresholds) produces, with equal naturalness, beliefs *in individuals* that are collectively endorsed and others that are collectively rejected.

The Irrelevance of Authenticity for Belief Causation

The extreme versions of social constructivism and relativism—such as the Edinburgh “Strong Programme”—are, I think, largely based on the same failure to distinguish clearly among ontology, epistemology and the sociology of knowledge. (Sokal, 15-16)

A bright idea: In support of this charge, Sokal presents three cases, which I will consider below, in which he finds it obvious that if a certain statement is true, its truth is needed to explain what caused people to believe it. I think he believes, falsely, that the Strong Program’s objection to such explanations is that, because we lack “direct unmediated access to external reality” (17), we can never know that a statement is true. I think that he also believes that to regard this as an objection is to confuse the question of whether a statement is true (ontology) with that of getting evidence for it (epistemology). My evidence for this suspicion is Sokal’s almost comical way of trying to avoid such conflation in his own discourse. His bright idea is to stipulate that every statement of fact that he appears to make is to be reinterpreted as a statement about evidence for it. I am not kidding. In a footnote to his second reading of Latour’s third rule of method, Sokal writes:

[My] phrase “the external world has been around” should, if one wants to be super-precise, be amended to read: “there is a vast body of extremely convincing (and diverse) evidence in support of the belief that the external world has been around....” Indeed, all my assertions of fact—including ‘today in New York it’s raining’—should be glossed in this way. Since I shall claim later that much contemporary work in Science Studies elides the distinction between ontology and epistemology, I don’t want to leave myself open to the same accusation. (13)

But if the amended assertions are again statements of fact, they must be glossed in the same way. And so on *ad infinitum*—in which case no statements result. Moreover, Sokal’s idea for how to

be super-precise is just as crazy if the amended assertions are not statements of fact because in that case he is proposing to stop making any statements of fact.³¹

Explaining belief causation: By contrast, Sokal's view of belief causation, as I understand it from his discussion (16-17), initially seems to be well supported by our intuitions. Every waking moment, we take ourselves to be recognizing things and recognition entails the truth of what is recognized. A friend can recognize me across the street from her only if I *am* across the street from her. This is not an explanation of why my friend *believes* that I am across the street from her but it may seem to be the beginning of one. For the rest of it, if I am naïve, I may think of my friend as a camera: I was there and her brain recorded it. Click. Or if I am sophisticated, I may say something about optics and vision but caution that we have little understanding of the biology involved. But even with this caveat, the form of the explanation remains cause and effect: something is so and, by the operation of the laws of physics, it helps cause my friend to recognize it.

This *is* compelling—if we lose sight of what a *causal* explanation is supposed to be. But even a metaphysical realist acknowledges that a cup on the table has no more '*belief that I see a cup on the table*' evoking powers than a good fake. The fact, if it is one, that the belief is true—that what I believe is a cup is not a fake but a cup—does no causal work in evoking the belief. This is what I mean by the irrelevance of authenticity for belief causation.³²

Newtonian mechanics: Here is Sokal's first example in which he finds it obvious that the truth of a certain statement played an essential role in causing it to be believed.

Why did the European scientific community become convinced of the truth of Newtonian mechanics sometime between 1700 and 1750? Undoubtedly a variety of historical, sociological, ideological, and political factors must play a role in this explanation—one must explain, for example, why Newtonian mechanics was accepted quickly in England but more slowly in France—but certainly *some* part of the explanation (and a rather important one at that) 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. (*A House Built on Sand*, 16)

Why *must* this be part of the explanation? Sokal doesn't say, neither here nor in the following footnote to the statement.

³¹ Why does a believer in the epistemically privileged status of science propose to assess evidence by how strongly certain people are convinced by it? I suspect that if Sokal wanted to be super-duper-precise, he would make the standard move of shifting to something like likelihood theory. Indeed, he makes a nod in this direction in a footnote to his third example (17). However this has its own problems. Furthermore, it would not save him from the crazy consequences of his bright idea.

³² See also "Science and the study of science" ("Reading and relativism" (57-59)). This is reminiscent of "underdetermination of theory by data." But I am talking about a biological claim, not a logical one. I offer no proof of it here. Readers may find it intuitively plausible, as do I, but this is not an argument. However, for this critique, I don't need to appeal to anything so strong. I need only demonstrate the inadequacy of Sokal's reasons for holding that *nothing less than* the truth of certain statements, if they are true, was required to make people believe them. And this is trivial to do because, for his first two examples, Sokal doesn't even pretend to give a reason and, for the third, which is a fantasy, he merely expresses his confidence that reasonable people will agree with him.

Or more precisely: There is a vast body of extremely convincing astronomical evidence in support of the belief that the planets and comets do move (to a very high degree approximation, though not exactly) as predicted by Newtonian mechanics; and if this belief is correct, then it is the fact of this motion (and not merely our belief in it) that forms part of the explanation of why the eighteenth-century scientific community came to believe in the truth of Newtonian mechanics.

Evidently, in this case, instead of providing evidence and an argument that this is an example of the required kind, Sokal acts as if it suffices for him to express his opinion that it is one.

Now you believe it, take two: Note the formal similarity between this footnote and the passage below from the footnote to Sokal's second reading of Latour's third rule, a slightly different part of which I criticize above. In each, a statement that begins "there is a vast body of extremely convincing...evidence in support of the belief that" is followed by an inference that begins "and if this belief is correct."

[My] phrase "the external world has been around" should...be amended to read: "there is a vast body of extremely convincing (and diverse) evidence in support of the belief that the external world has been around... and if this belief is correct, then the claim that the external world is created by scientists' negotiations is bizarre to say the least." Indeed, all my assertions of fact---including 'today in New York it's raining'---should be glossed in this way. (13)

Evidently, Sokal created his footnote about Newtonian mechanics by following this rule for how to amend "all [his] assertions of fact."³³ Here I will pretend that the rule does not also apply to the footnote and try to read it as an ordinary statement, which undoubtedly is how Sokal meant it to be taken. But when I do this, something very strange comes into view. In the first part of the footnote, Sokal finds the evidence that the planets and comets move as predicted by Newtonian mechanics (to a high degree of approximation) extremely convincing. However, the hedging "if this belief is correct" in the second part strongly suggests that Sokal is *not* convinced that it is correct! So is he convinced or isn't he?

Exactly the same question may be asked about every one of Sokal's glossed statements of fact. How can he find evidence for *anything* extremely convincing if he isn't convinced by it?³⁴ Also, if he is unwilling to make statements of fact, he is unwilling to say that Newtonian mechanics is true (to a high degree of approximation). I think that when Sokal writes about this, he fixates on the consideration that we cannot know whether Newtonian mechanics, or anything else, is true because we have no "direct, unmediated access to external reality." Yet he also believes that, our lack of "direct, unmediated access to external reality" notwithstanding, we can and he does know that "out there in external reality" is a vast and diverse body of extremely convincing evidence for Newtonian mechanics. I discuss this blunder in "Reading and relativism" in "The infinite regress for evidence." See also "Now you believe it, take one."

³³ Except that he failed to notice that the "if this belief is correct, then" claim is itself an assertion of fact and therefore must be amended according to his rule. But this is the least of his problems here.

³⁴ Concerning the standard shift to something like likelihood, see note 31 and the third paragraph of "Science and the study of science."

The mess is of Sokal's own making. Had he not become obsessed about separating ontology and epistemology, he could simply have referred to the evidence that convinces him of the truth or likelihood of the ontological assumptions on which the explanations he favors are based. This is no answer to the biological truth that authenticity is irrelevant for belief causation but it would have kept him from foolishly promising to replace each assertion of fact by an infinite regress of glosses about evidence.

From creationism to Darwinism: I am unsure whether, in the quote below, Sokal means to say that if Darwinism is true, its truth as reflected in the fossil record helped make people believe it or that truth of the fossil record *simpliciter* helped make people believe it.³⁵ Here I will assume the latter.

Or to take another example: Why did the majority view in the European and North American scientific communities shift from creationism to Darwinism over the course of the nineteenth century? Again, numerous historical, sociological, ideological, and political factors will play a role in this explanation; but can one plausibly explain this shift without any reference to the fossil record or to the Galápagos fauna?

It may be that this shift in belief cannot be plausibly explained without reference to nineteenth century *beliefs* about the fossil record but Sokal's position is that it cannot be plausibly explained without reference to *the truth* about the fossil record. Yet in this passage, which is the only place where he makes this ambitious claim, he makes no attempt to justify it. Two examples down, one to go.

The man who screamed 'Elephants!' For his last example, Sokal uses a fantasy to highlight the common-sense intuitions that he believes he is employing.

Suppose we encounter a man running out of a lecture hall screaming at the top of his lungs that there's a stampeding herd of elephants in the room. What we are to make of this assertion and, in particular, how we are to evaluate its "causes" should, I think, depend heavily on whether or not there *is* in fact a stampeding herd of elephants in the room--or, more precisely, since I admit that we have no direct, unmediated access to external reality--whether when I and other people peek (cautiously!) into the room, *we* see or hear a stampeding herd of elephants (or the destruction that such a herd might have caused before exiting the room). If we do see such evidence of elephants, then the most plausible explanation of this set of observations is that there is (or was) in fact a stampeding herd of elephants in the lecture hall, that the man saw and/or heard it, and that his subsequent fright (which we might well share under the circumstances) led him to exit the room in a hurry and to scream the assertion that we overheard. And our reaction would be to call the police and the zookeepers. If, on the other hand, our own observations reveal no evidence of elephants in the lecture hall, then the most plausible explanation is that there was not in fact a stampeding herd of elephants in the room, that the man imagined the elephants as a result of some psychosis (whether internally or chemically induced), and that this led him to exit the room in a hurry and to scream the assertion that we overheard. And we'd call the police and the psychiatrists. And I daresay that Barnes and Bloor, whatever they might write in journal articles for sociologists and philosophers, would do the same in real life.

³⁵ Believe in the truth of Darwinism.

Why is a stampeding herd of elephants in a lecture room more plausible than a hoax, with the screamer and some of the peekers in on it, as a possible cause of “the destruction that such a herd might have caused before exiting the room”?³⁶ To justify calling the zookeeper, this need not matter. Cause for concern is enough. But to explain what caused the scream, it does matter and not only because Sokal might be mistaken. Even if the screamer did see elephants, this is irrelevant for a causal explanation of what made him believe that he did. E.g., if he saw only their fronts, then their 100% elephant behinds probably played no causal role in making him believe that he saw elephants. Thus, in the first scenario, Sokal’s candidate for a belief-evoking cause—elephants—is too specific. By contrast, in the second one, it is too general. Sokal’s task is to explain what made the man acquire the particular belief, or scream the particular scream, that he did. Even if he did “[imagine] the elephants as a result of some psychosis,” this does not explain why it was elephants that he imagined rather than Marilyn Monroe.³⁷

Summing up: In his attack on the Strong Program, Sokal makes several major blunders. First, he fails to grasp the relevance of the irrelevance of authenticity for belief causation. That he does not even raise it as an issue suggests that he has not thought seriously about what it means to be a *causal* explanation of a belief. Contrary to what he seems to think, it is not an everyday notion.³⁸ A second major blunder is his “bright idea” of stipulating that, whenever he seems to be making a factual statement, he means only that there is extremely convincing evidence for it. He apparently fooled himself into believing that such “super-precision,” the crazy consequences of which he failed to notice, would keep him from conflating ontology with epistemology. Imagine the hoots of “ideological straightjacket” that would have been heard had it been a constructivist sociologist, not Sokal, whose desire for “super-precision” had left her unable to straightforwardly assert a belief in Newtonian mechanics! These and the many other blunders that I describe in this essay are bad enough. Publishing them in a collection that advertises its devotion to exposing shoddy scholarship adds insult to injury.

Appendix: Fashionable Nonsense

Nature as the external referee? The allegation that Latour conflates facts with our knowledge of them figures even more prominently in *Fashionable Nonsense*. His claim that scientists “do not *use* Nature as the external referee” to settle a controversy is offered as one blatant example of this. But is it? Latour goes to considerable lengths to explain what he means but, on my reading, the heart of it is just this: Nature cannot be *used* as an *external referee* to settle a scientific controversy because a scientific controversy is a disagreement *about what Nature is saying*. This does not mean that Nature cannot play a role. It cannot help but play a role. It means only that it cannot play the role of an external referee.³⁹ But Sokal and Bricmont will have none of this.

³⁶ I like the image of the herd of elephants exiting the room. Did someone hold the door open for them? If the doors opened out, how did they get in? Why did Sokal choose such a goofy scenario?

³⁷ Also, Sokal doesn’t explain why a psychosis is more plausible than a prank, maybe one directed at him. A prankster could have spotted Sokal in the lobby, recognizing him by his physicist haircut, then slipped into the lecture room and reappeared a few moments later as the screamer.

³⁸ For example, the murderer doesn’t figure in a causal explanation of a murder. But it obviously does in a criminal investigation, which seeks both more and less than a causal explanation of the event.

³⁹ Latour also notes that, once agreement is reached, the process of agreement is seen as a shared discovery.

They offer instead the following preposterous rendering of what Latour means by “using Nature as the external referee.”

[Scientists] *do* “use Nature as the external referee”: that is, they seek to know what is really happening in Nature, and they design experiments for that purpose.

As an attempt to “achieve by definition what one could not achieve by logic,” this is hard to beat. If designing and running an experiment used Nature as external referee in the sense that Latour means, there could be no disagreement about how to *interpret* it.

The reality of the past: In *Fashionable Nonsense* (96), the authors quote Latour saying, “How could [the pharaoh, Ramses II] pass away due to a bacillus discovered by Robert Koch in 1882?” as an “even more extreme” case of conflating a fact with our knowledge of it. This is another bad goof on their part. If Latour means that a cause of death cannot pre-date its discovery, he is not conflating a fact with our knowledge of it; he is forgetting that the world is four-dimensional, not three—just as Sokal does in his second reading of Latour’s third rule of method. But *is* Latour forgetting this? In “The puzzle of backward causation” (*Pandora’s Hope* 1999), his answer to the question of whether there were microbes before Pasteur is “After 1864 airborne germs were there all along.”

We can all be iconoclasts: After convincing themselves that Barnes and Bloor make, or pretend to make, a “radical redefinition of the concept of truth,” Sokal and Bricmont undertake to show that they sometimes “fall back, without comment, on the traditional sense of the word.” In “Reading and relativism” (47-48), I showed, for the example in which they thought they saw a radical redefinition of the notion of truth, that there is no such radical redefinition and therefore nothing from which to fall back. Nevertheless, their ‘sighting’ is instructive purely as a case of careless reading. They claim,

Moreover, Barnes and Bloor fail to use systematically their new notion of “truth”; from time to time they fall back, without comment, on the traditional sense of the word. For example, at the beginning of their article, they admit that “to say that all beliefs are equally true encounters the problem of how to handle beliefs which contradict one another”, and that “to say that all beliefs are equally false poses the problem of the status of the relativist’s own claims.” But if “a true belief” meant only “a belief that one shares with other people in one’s locality,” the problem of the contradiction between beliefs held in different places would no longer pose any problem.

Barnes and Bloor are not talking about a contradiction between beliefs held *in different places*. They are talking about a contradiction between beliefs that are held *in the same place*. Also, contrary to what Sokal and Bricmont would have us believe, Barnes and Bloor do not make the absurd assumption that *all* of one’s beliefs are shared with one’s community. Meera Nanda makes a similar blunder in the final chapter of *A House Built on Sand*.

Barnes and Bloor...teach us that “faced with a choice between the beliefs of his own tribe and those of the other, each individual would typically prefer those of this own culture” (Barnes and Bloor (1982, 27)... Scientific “truths” are true only in the web of belief spun by the tribe of Western scientists. But, then, what about the iconoclasts, the

reformers, and the rebels who, in all societies at all times, insist on putting the good above the ways of the tribe? (Nanda, 300)

Yes, what about us? Within the tribe of scientists, I am an iconoclast about mathematics, certainly a reformer and maybe even a rebel. Faced with a choice between conventional and constructivist mathematics, I always choose the latter. But, *typically*, when I am faced with a choice between conventional science and a competitor, I choose the former. Furthermore, I rarely give the matter any serious thought. *Typically*, I trust my tribe. On the picture of things that Barnes and Bloor present, everyone can be an iconoclast about some things.