

Professor Nagel's *Fashionable Nonsense*¹

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In “The sleep of reason” (*The New Republic* October 12 1998) the philosopher, Thomas Nagel, hails the publication of *Fashionable Nonsense* by Alan Sokal and Jean Bricmont as a much needed hatchet job. In “Reading and relativism” (note 10), I remarked that, in the first part of this review, Nagel accepts uncritically whatever the authors say, almost as if he were taking dictation. However, I gave only one example of this (38), Nagel mindlessly endorsing an ignorant wisecrack by the authors about Luce Irigaray’s use of the word, ‘privileged.’ Here I present more examples of this kind, as well as some that demonstrate in other ways Nagel’s failure to stop and think carefully before calling for the rope or praising the authors’ treatment of some topic and, finally, debunkings of some of Nagel’s own views about the philosophy of science.

The Conspicuous Presence of Derrida

When Sokal and Bricmont say (8),

Although the quotation from Derrida contained in Sokal’s parody is amusing, it is a one-shot abuse; since there is no systematic misuse of (or indeed attention to) science in Derrida’s work, there is no chapter on Derrida in this book,

Nagel turns it into,

Derrida, for example, is conspicuously absent from the book except for one quote in the original parody because that was an isolated instance, produced in response to a question at a conference.

However, although Nagel eschews the derisory terms ‘amusing’ and ‘abuse,’ his “that was an isolated instance” parrots the unsubstantiated claim that the Derrida quote is nonsense.² Nagel does deviate from the text when he asserts that Derrida is “conspicuously absent” from the book “except for one quote in the original parody”³ but this brings him only trouble. The quote, which appears only in Appendix A (in a reprinting of the parody), is indeed the only statement by Derrida in the book. It also is the only statement by Derrida that is discussed in it. Nevertheless, even though no more than half a dozen sentences are devoted to it, this does not justify Nagel’s use of the expression, “conspicuously absent.” One sufficiently hostile remark, like the one quoted above, refutes it. Recall that when Derrida was being portrayed as the Father of all Physics Fakers (roughly, 1991-1996), it was *solely* on the basis of this one quote. Moreover, very little was actually said about it, in some cases, virtually nothing. Yet it would be absurd to say that Derrida was then “conspicuously absent except for one quote.” On the contrary, he was conspicuously present—as he is in this book, when, for example, Sokal and Bricmont say that, although they haven’t the slightest idea what the Derrida quote means, they know that it is gibberish. Here are their exact words (263):

² Did Nagel even try to substantiate this claim? I know of only two people who did, Ernest Gallo and Steven Weinberg, both of whom made fools of themselves. For Gallo’s attempt in “Nature faking in the humanities” (*Skeptical Inquirer* Summer 1991), see “The invention of Jacques Derrida, physics faker,” at the web site. For Weinberg’s attempt in “Sokal’s hoax: an exchange” (*New York Review of Books* October 3 1996), see the “The oracle of deconstruction” in “Reading and relativism.”

³ The authors say only that there is no chapter devoted to Derrida.

The primary purpose of this section is to provide a gentle lead-in to the article's first major gibberish quote, namely Derrida's comment on relativity ("the Einsteinian constant is not a constant"). We haven't the slightest idea what this means—and apparently neither does Derrida—but as it is a one-shot abuse, committed orally at a conference, we shall not belabor the point.

Apparently, it does not occur to Nagel to wonder why Sokal and Bricmont are confident that the quote is nonsense if they "haven't the slightest idea what it means." Or why, right after saying, "we shall not belabor the point," they do just that, in the following gratuitously offensive, and also illogical, footnote.

For an amusing attempt, by a postmodernist author who does know some physics, to come up with something Derrida's words could conceivably have meant that might make sense, see Plotnitsky (1997).⁴ The trouble is that Plotnitsky comes up with at least *two* alternative technical interpretations of Derrida's phrase "the Einsteinian constant," without providing any convincing evidence that Derrida intended (or even understood) either of them.

This reasoning is at best irrelevant and at worst nonsense. Convincing evidence in support of a claim that a certain non-gibberish interpretation is what Derrida meant by the quote *suffices* to refute the charge that what he meant is gibberish but it is not *needed* to refute an alleged *justification* of such a charge. Anyway, this should not concern Sokal and Bricmont because they do not claim to have a justification. For them, it is an article of faith. What then is this talk about the trouble being that Plotnitsky presents two possible readings with no convincing evidence for either? In the eyes of Sokal and Bricmont, the trouble that trumps all other troubles is that, whatever Plotnitsky tries, he is wasting his time because the quote *just is* gibberish. Finally, as evidence that this '*just is*' view of their view is fair, note that Sokal and Bricmont are shameless about criticizing Plotnitsky, in the footnote, for failing to provide convincing evidence (which, for his purposes, he doesn't need) while, in the text, flaunting their complete lack of evidence for their view of the Derrida quote.

Sokal's Wordplay: Passing off a Sequential Relation as a Causal One.

Superficially, the following remark from the review does not read like dictation. However, I claim that when we take into account not only its literal meaning, but also the accompanying innuendo, it does.

⁴ Arkady Plotnitsky: "'But it is above all not true': Derrida, relativity, and the 'science wars'" (*Postmodern Culture* 7 no. 2 and http://muse.jhu.edu/journals/postmodern_culture/v007/7.2plotnitsky.html). It was Plotnitsky who saved Sokal and Bricmont from repeating the embarrassing blunder that Sokal made in *A House Built on Sand* when he mocked Deleuze and Guattari for "holding forth" on chaos theory. In a lecture at a science wars workshop at UCSC, in a session in which Sokal was the only other speaker, Plotnitsky explained that Deleuze and Guattari are talking about chaos, not chaos theory. As evidence that the explanation was accepted, in *Fashionable Nonsense*, the authors say, about the passage that Sokal described in *A House Built on Sand* as Deleuze and Guattari holding forth on chaos theory, "Let us note in passing that the word 'chaos' is not being used in its usual scientific sense." However, although they make it clear that "in its usual scientific sense" means "as in chaos theory," they do not acknowledge that this gives the lie to Sokal's mocking remark in *A House Built on Sand*. Nor do they credit Plotnitsky's help in getting this right.

Sokal's article expressed deep admiration for the view of two editors of *Social Text*, Stanley Aronowitz and Andrew Ross.

At the beginning of his announcement of the hoax, Sokal insinuates that this expression of admiration and other attempts to please the editors played a significant role in their decision to publish his article. This is essentially what I mean by the innuendo. And Nagel seems more than willing to take Sokal's word for it. Here is the relevant passage.

So, to test the prevailing intellectual standards, I decided to try a modest (though admittedly uncontrolled) experiment: Would a leading North American journal of cultural studies—whose editorial collective includes such luminaries as Fredric Jameson and Andrew Ross—publish an article liberally salted with nonsense if (a) it sounded good and (b) it flattered the editors' ideological preconceptions? The answer, unfortunately, is yes.

This statement is more of a con than the hoax and Nagel appears to be a victim of it, albeit a willing one. Apparently no warning signals went off when he encountered the word 'if' being used to pass off a sequential relation as a causal one. Nor does he seem to have noticed how Sokal exploits the ambiguities in "sounded good" and "flattered" by conflating his *attempts* to flatter the editors and to "sound good" to them with the assumption that he *succeeded*. Nor does Sokal's admission that he performed no controls seem to have alerted Nagel, who prides himself on his commitment to rationality, to the need to think about what difference this might have made and maybe even to consult an expert. Nor, for that matter, does he seem to have wondered how Sokal could have learned that his *attempts* to flatter the editors and make ideological statements that would "sound good" to them did indeed play a significant role in their acceptance of his article. Does Nagel think that the editors told Sokal this? If they did, would he not have mentioned it? In his announcement of the hoax, Sokal takes for granted that the fact that his article was accepted is proof that his attempts at flattery and "sounding good" were successful. If Nagel buys this reasoning, I have a bridge he might like to buy.

Confused Thinking about Confused Thinking

The hoax and the allegations in its announcement were not the only hostile actions that Sokal took against the editors of *Social Text*. He also cooperated with the editor of *Lingua Franca* to prevent them from presenting their side of things in the same issue in which he presented his.⁵ It was in this context that the editors of *Social Text* received a second submission from Sokal, an "afterword" to his hoax, about which Nagel remarks:

This second article was submitted to *Social Text*, but it was rejected 'on the grounds that it did not meet their intellectual standards'—an unintended compliment.⁶

⁵ Sokal's portrayal of the editors in his announcement, which included things he almost surely did not know to be true, such as the success of his attempt to flatter and "sound good," almost immediately acquired the status of received wisdom. Because of this, when the editors had their turn and disputed much of Sokal's account, they were harshly criticized. They allegedly had been "caught in the act" by the revelation of the hoax and their refusal to admit it sounded to many like, "Who do you believe? Me or your lying eyes?"

⁶ If Nagel thinks this also is Sokal's assessment of the journal, he is mistaken.

Did Nagel read Sokal's article? During my own initial reading of it, in a version downloaded from the web, I came upon a gaping hole where an argument belongs. I looked around on my desk and then on the floor for the pages containing it. But there were none because there was no argument, only a gaping hole. I will explain. After quoting Stanislaw Andreski about confused thinking,⁷ Sokal announces:

As an example of "confused thinking," I would like to consider a chapter from Harding (1991) entitled "Why 'Physics' Is a Bad Model for Physics."

When I first read this statement, I expected it to be followed, after some preparation, by a description of part of the chapter, including a quotation or at least a careful paraphrase of something that Harding says in it, and a proof that it is an example of confused thinking. Initially, I was not disappointed. Sokal writes,

Harding wishes to answer the question, "Are feminist criticisms of Western thought relevant to the natural sciences?" She does so by raising, and then rebutting, six "false beliefs" about the nature of science. Some of her rebuttals are perfectly well-taken; but they don't prove anything like what she claims they do. That is because she conflates five quite distinct issues:

So far, so good. Presumably this alleged conflation is the confused thinking that Sokal says he wants to consider. He next lists the five issues: ontology, epistemology, sociology of knowledge, individual ethics and social ethics, each accompanied by a question that seems to be an example of what Sokal has in mind by an issue of that type.

Ontology: What objects *exist* in the world? What statements about these objects are *true*?

Epistemology: How can human beings obtain *knowledge* of truths about the world? How can they assess the *reliability* of that knowledge?

Sociology of knowledge: To what extent are the truths *known* (or *knowable*) by humans in any given society influenced (or determined) by social, economic, political, cultural and ideological factors. Same question for the false statements erroneously believed to be true.

Individual ethics: What types of research *ought* a scientist (or technologist) to undertake (or refuse to undertake)?

Social ethics: What types of research *ought* society to encourage, subsidize, or publicly fund (or alternatively to discourage, tax or forbid)?

Sokal next points out that, although these issues are related, they are conceptually distinct. True enough, but how does Harding conflate them? This is what we are waiting to hear. One might suppose that he would now tell us. He does not. What follows instead is a page and a half long discussion in which Harding is mentioned in the first sentence, never to be heard from again. Still, the required argument might be buried in it. So let us see. It begins:

⁷ He's against it.

For example, Harding (citing Forman 1987) points out that American research in the 1940s and 50s on quantum electronics was motivated in large part by potential military applications. True enough.

This too is informative. It suggests that if Sokal thinks that Harding conflates at least two of the issues, the observation about research “motivated...by potential military applications” must be an instance of one of them. Furthermore, in the discussion that follows, he says:

Likewise, sociological questions arise, for example: To what extent is our (true) knowledge [likewise, erroneous theories, if any] of computer science, quantum electronics, solid-state physics, and quantum mechanics been the result (in whole or in part) of social, economic, political, cultural, and ideological factors, in particular the culture of militarism?

This makes it likely that Sokal classifies Harding’s remark under ‘sociology of knowledge.’ But he also says:

The militaristic orientation of American science has quite simply no bearing on the ontological question [whether atoms (and silicon crystals, transistors, and computers) really do behave according to the laws of quantum mechanics (and solid-state physics, quantum electronics, and computer science)], and only under a wildly implausible scenario could it have any bearing on the epistemological question. (E.g. if the worldwide community of solid-state physicists, following what they believe to be the conventional standards of scientific evidence, were to hastily accept an erroneous theory of semiconductor behavior because of their enthusiasm for the breakthrough in military technology that this theory would make possible.)

Thus, Sokal seems to be saying that Harding treats her sociology of knowledge observation about potential military applications motivating research in quantum electronics as if it says something about ontology and/or epistemology. But instead of trying to prove that Harding does indeed do this, Sokal suddenly changes the subject, never to return, leaving us with a gaping hole where an argument belongs. Thus, despite all his preparation, he does not come close to demonstrating that Harding makes the conflation that he says she does. To do this, he would have to show that she does treat her sociology of knowledge observation as if it were a point of ontology or epistemology. But he does not even *attempt* to do this. Bizarrely, he just walks away from it, leaving Harding tarred by an empty accusation passed off as fact to an audience of supporters eager to believe him.⁸ Did Nagel even read this?

Finally, I would be remiss in presenting Sokal’s list of issues without pointing out that the questions accompanying the second and third are almost comically muddled. See “Can a true belief be unreliable?” and “Can a statement be influenced by social factors?” in the appendix.

⁸ Although Nagel prides himself on close readings, he apparently had no problem with this. Nor did the editors of *Philosophy and Literature*, who not only treated their publication of Sokal’s Afterword as an event but were so impressed by it, or at least by him, that they invited him to join their editorial board!

Why Was It Wrong to Publish the Hoax?

Here I question the assumption, implicit in the remark that I quoted at the beginning of the previous section, that the publication of Sokal's article reveals *Social Text's* low intellectual standards.

Einstein yes, Weinberg maybe, Sokal no: Early on in this affair, I asked a few scientists and mathematicians, all of whom thought it was wrong for *Social Text* to publish Sokal's article, whether it would have made a difference if Einstein had been the author. Everyone said yes and for the same reason: that it would have been of great interest to know *whatever* was going on in Einstein's head about these matters even if it seemed crazy. However, when I posed the same question about Steven Weinberg, people were undecided. So it was Sokal, no, Weinberg, maybe and Einstein, yes, with *the same content* in each case. But if so, is it unreasonable that readers of a leftist journal of cultural studies should be curious about what is going on in the head of a 'politically committed' professor of physics at a distinguished university concerning allegedly promising consequences for "liberation politics" of recent scientific developments? Indeed, is it any more unreasonable than the eagerness of highly regarded intellectual journals to publish whatever certain scientists and philosophers wish to say, trusting them not to make blunders or perhaps not caring if they do. Can one imagine the editors of the *NYRB* asking a physicist to check the elementary physics in Steven Weinberg's criticism of what Bruno Latour says about the special theory of relativity? They wouldn't dare and it wouldn't help. Weinberg made a hash of this not because he doesn't understand relativity theory but because he doesn't understand Latour.

"Only a scientific ignoramus could have missed the joke." So far as I know, everyone who condemned the editors of *Social Text* for publishing Sokal's article pointed to what they took to be its execrable content. As Nagel puts it:

The nonsense made of the science was so extreme that only a scientific ignoramus could have missed the joke.

This is either bluff or delusion. Sokal did not perform any controls. He could have but he didn't and people like Nagel, who should have known better, didn't call him on it. Sokal worked this to his advantage by conning gullible readers into assuming that reading the article knowing that it is a hoax (and where to laugh and why) is uninterestingly different from reading it knowing only that he is a 'politically committed' professor of physics at NYU. Also, because it is a hoax, Sokal's intended meanings have no privileged status. E.g., if he tries to make a pun that is based on his own misunderstanding of a mathematical expression,⁹ it would be perverse to contend that, unless I misunderstand the expression the way he does, I have been tricked.

The existence of readings of nonsensical-looking passages, in the hoax article and elsewhere, on which they are not nonsensical¹⁰ is another reason why, in the absence of controls, Nagel has no business parroting, as he does above, Sokal's unwarranted assumption about what a

⁹ I give an example of this below.

¹⁰ See, for example, "An unphilosophical argument," at this website.

reader *would have* made of the article *before* it was revealed to be a hoax. Here is yet another reason for resisting this assumption.

“I assumed he was making a joke I didn’t get.” Some time ago, while thumbing through a friend’s copy of John Searle’s, *The Rediscovery of the Mind*, I encountered a blunder the likes of which I had never seen before. It begins:

Suppose that we have a computer that multiplies six times eight to get forty-eight. Now we ask, “How does it do it?” Well, the answer might be that it adds six to itself seven times. (*The Rediscovery of the Mind*: 213)

It was easy to see that if Searle had said “eight,” as the rest of humanity does, his argument would have proceeded without change. But he made it clear that he *wanted* his argument to be disrupted by this irrelevant deviant use of language. He did not just say “seven” instead of “eight” and go on as if nothing had happened. Instead, he tagged it with a note:

People sometimes say that it would have to add six to itself *eight* times. But that is bad arithmetic. Six added to itself eight times is fifty-four because six added to itself zero times is still six. It is amazing how often this mistake is made. (238)

I burst out laughing at this display of pretentious ignorance about an informal mathematical locution that is trickier than Searle supposed, capped by his conceit that he had discovered a mistake in arithmetic—or at least in the use of an arithmetical expression—that the rest of us dummies had missed.¹¹ Yet, my friend, who is both a great mathematician and a great reader, had read Searle’s book without noticing this. When I asked how he could have missed it, he explained, “I assumed he was making a joke that I didn’t get.” Unlike me, to whom the “Everyone’s out of step except John” conceit was all too recognizable as Searle, my friend knew of him mainly as the author of several books that he admired and therefore took for granted that he must be too much of a grownup to mean what he transparently seemed to mean.

For me, the relevance of this incident is that the position of the editors of *Social Text* with respect to Professor Sokal’s submission seems to have been very much like that of my friend with respect to Professor Searle’s bizarre digression. When one is in a sufficiently respectful mood, thoughts like “This must be a joke I don’t get” can excuse a multitude of sins—which shows how easy it can be, when one trusts the good sense of an author, to fail to see as such what one would otherwise take to be transparent nonsense.

Sokal and Bricmont see a joke that isn’t there and miss one that is. Finally, if Nagel’s “scientific ignoramus” remark is based in part on the many alleged examples of nonsense that were presented by ‘experts’ like Sokal, Boghossian and Weinberg, he may well have mistaken their confidence for competence.¹² Evidence of this comes in different sizes and shapes. Here is an example in which it is not a scientific ignoramus but Sokal and Bricmont who miss a joke about quantum mechanics in Sokal’s hoax article.

¹¹ For an explanation of how the locution works, see the appendix to “An unphilosophical argument” at the web site.

¹² See “Reading and relativism” and the essays at the web site.

In mathematics, two operations *commute* if reversing the order in which they are performed does not effect the result. Near the beginning of the third section of his parody, Sokal writes:

When even the gravitational field—geometry incarnate—becomes *a non-commuting (and hence nonlinear) operator*, how can the classical interpretation...be sustained? (Italics added.)

In *Fashionable Nonsense* (263), the authors say about this:

The first major blooper in this section concerns the expression ‘noncommuting (and hence nonlinear).’ In actual fact, quantum mechanics uses noncommuting operators that are perfectly *linear*.

But this alleged explanation of a blooper is itself one. When Sokal and Bricmont say “quantum mechanics uses noncommuting operators that are perfectly *linear*,” they mean that it uses linear operators that do not commute *with each other*, which is true but irrelevant because, in the sentence in the parody, there is only one operator. It is only a blurring of singular and plural that makes it seem otherwise. Nevertheless, the sentence in the parody does contain a blooper because there is no such thing as *a* noncommuting operator. This is the one the authors missed.

Are Your Purposes Being Served?

Yet there is no direct way to refute a fogbank, and so they have adopted the safer strategy of focusing on the occasions when these writers rashly try to invoke the authority of science by using a vocabulary that does have a clear meaning, and that could not serve their purposes, literal or metaphorical, unless it were being used more or less correctly. This also allows them to explain why the scientific material introduced, even if it were not completely garbled, would be irrelevant to the literary, psychological, or social topics being discussed.

This is more stenography on Nagel’s part. The first question that Sokal and Bricmont need to answer is not whether some vocabulary has *a* clear meaning but whether it is the one being used by those whom they say misuse it. It also is fair to ask whether the intended use serves the purposes of the user. But does Nagel believe that Sokal and Bricmont have the necessary expertise in these different areas to answer this? In their book, they simply assume that they do. Is this good enough for an analytic philosopher?

Allegations of Gibberish

Nearly half the book consists of extensive quotations of scientific gibberish from name-brand French intellectuals, together with eerily patient explanations of why it is gibberish. This is amusing at first but it becomes gradually sickening.

Evidently, Nagel trusts the authors’ ‘eerily patient explanations’ of why these quotations are gibberish. Yet the only such explanation in the review is the hare-brained remark about Irigaray’s use of the term “privileged.” (See “Reading and relativism” (38).) And it only gets worse.

The writers arraigned by Sokal and Bricmont use technical terms without knowing what they mean, refer to theories and formulas that they do not understand in the slightest, and invoke modern physics and mathematics in support of psychological, sociological, political and philosophical claims to which they have no relevance. It is not always easy to tell how much is due to invincible stupidity and how much to the desire to cow the audience with fraudulent displays of theoretical sophistication.

Nagel presents himself here as an authority. But even if he sincerely believes that he is one, why should we buy it? Surely he would want us to make up our own minds on the basis of the evidence he presents. But he doesn't provide any evidence! Also, it seems fair to ask whether Nagel's condemnation of "fraudulent displays of theoretical sophistication" is meant to apply to the passage in his review in which, in the same authoritative tone, *he* spouts gibberish about relativity theory. (See "Reading and relativism" (36-38).) If it isn't, why not?

Sokal and Bricmont misread Latour reading Einstein. In the rest of this section, I show that a fragment of the chapter in *Fashionable Nonsense* about Latour's essay on relativity theory refutes Nagel's extremely generous assumption of the authors' competence. Here first is a letter to the *Times Literary Supplement* in response to a review of the French edition of the book. In it, I contend that the authors misunderstand, seemingly perversely, both what Latour means by a 'privileged' frame and by an 'observer.'

Sir, As physicists, the authors of *Impostures Intellectuelles* (Science May 1) are well equipped to ask questions about the meaning and accuracy of scientific-looking assertions made in non-scientific texts. But what reason is there to think that their interpretations of the assertions are faithful to the meanings of the asserters? The reviewer doesn't say and the authors' own pleading about this amounts to nothing more than "Trust us. We can tell when these people don't know what they are talking about."

Their chapter on Bruno Latour's essay on relativity theory is an unhappy example of how this plays out. Latour has a peculiar mode of thought and way of expressing it. Failing to appreciate this, Sokal and Bricmont repeatedly "refute" Latour by a reading on which he is denying an elementary fact about relativity theory. They can do this because their default assumption is that Latour does not know the basic physics. If they believed that he does know it, it would be very easy for them to find another reading—for example, mine—on which what he says makes good sense.

Thus, seeing Latour talk repeatedly about a privileged frame, they charge him with ignorance of the truth that there are no privileged (inertial) frames. But Latour explains that it is precisely because no frame is privileged in the sense of Sokal and Bricmont that any frame may become privileged in the sense of Latour. He means that any frame may be one in which reports from other frames (probes) are received and compared. NASA central may be placed anywhere. Or to use Latour's own analogy, the National Weather Bureau is not located in Washington, D.C. because there is a better view of the clouds. "Privileged" is a sociological term.

Ironically, Sokal and Bricmont are as deaf to some of Latour's word play as the editors of *Social Text* were to Sokal's. They even misread "observers sent away might betray, might retain privileges" and "desire to discipline the delegated observers" as evidence

that Latour suffers from the misconception that, in relativity theory, observers have to be human! (For Latour, a recording device is an observer.) Every one of their examples of Latour failing to grasp a basic point about relativity theory is, in fact, an example of their own failure to understand a basic point about Bruno Latour. There are things in Latour's essay that merit criticism (inconsistencies, unsupported claims) but I have seen no proof that his command of the basic physics is one of them.

In “I am not a reference frame” and “Places in space” (“Reading and Relativism”: 39-40), I debunk two claims about Latour on relativity made by Sokal in *A House Built on Sand* that are almost identical to ones made in *Fashionable Nonsense*. One is the bizarre charge that Latour doesn't know the difference between a frame of reference and an “actor” in semiotics. The other is that he doesn't understand that relativity is about two frames in relative *motion*. In “Places in space,” I note that is that it is impossible to read Latour's essay on relativity without seeing that it is dominated by a consideration of two frames in relative motion. Nevertheless, like Sokal before them, Sokal and Bricmont ignore this, saying that Latour “appears to think that relativity is concerned with the relative *location* (rather than the relative motion) of different frames of reference, at least in the following excerpts.” They present four of them—two of which, removed from the context of Latour's essay, might well invite the kind of misreadings that Sokal and Bricmont make of them. Their verdict is that “this error” (of seeming to talk about relative location instead of relative motion), “can perhaps be attributed to a lack of precision in Latour's style.” But if they think this may be what it is, why are they talking about “this error”? The authors seem confused. Also, they miss an important clue: in two of the excerpts, Latour talks about ‘place’ not ‘position’ or ‘location.’ The authors assume that they mean the same thing but that they do not is shown in the fourth excerpt, in which Latour remarks that *electrons* are prominent among the *places* that he is talking about! That Bricmont and Sokal ignore this even while talking about about a frame of reference ‘attached’ to a proton is, I think, telling.

Latour reading Einstein: Frames are not enough: In “A physics experiments with scholarly discourse,” I criticize Sokal for taking a remark of Latour to mean something like “two reference frames are not enough, a third frame is needed.” I also criticize a similar reading of it by Sokal and Bricmont. On my reading, Latour is saying that “reference frames are not enough” because we also need to know the Lorentz transformations and “a third frame is needed” but it need not be different from one of the two in which observations are made.

A Sensible Chapter? Take One

There is also a long and sensible chapter on skepticism, relativism, and the history and philosophy of science.

Here, I point out some things in this “long and sensible” chapter that Nagel failed to mention, some of them absurd, others merely irresponsible. My guess, for what little it may be worth, is that he simply did not read this material carefully enough to notice them. As for what he would have done if he had, I have no idea.

“Now, it happens that, for the unsolved problems, nobody knows the right answer.”

Now, it happens that, for the unsolved problems, nobody knows the right answer, while for the solved ones, we do know it (at least if the accepted solution is correct, which can always be challenged).¹³ (*Fashionable Nonsense*: 96-97)

So, if a problem is unsolved, nobody knows the right answer but if it is solved, somebody does know it—unless she only thinks that she does! Doesn't this read like a hoax? Doesn't the folksy “Now, it happens that” read as if the hoaxer is almost asking to be unmasked?

Dr. Sokal and Mr. Hyde:¹⁴ Sokal and Bricmont say (91) that how they would evaluate the causes of a man's claim should depend heavily on whether or not the claim is true. They then qualify this, saying, “more precisely, since admittedly we have no direct, unmediated access to external reality,” it should depend on evidence for or against the claim.¹⁵ However, as an assertion about our relationship to external reality, this is crazy. The problem is not with the idea of looking at evidence. Of course we should. It is with the conceit that although we do not have direct, unmediated access to the truth-value of an assertion, we do have such access to the truth-value of a claim that *something is evidence for an assertion*. This is fantasy. A claim that a rabbit test is evidence of pregnancy is itself based on evidence, probably statistical. There is an infinite regress for evidence, just as for justification. In practice, we follow it just so far. I doubt that it is often a matter of conscious choice. Where we stop, for those like Sokal and Bricmont who believe that talk of “direct unmediated access to external reality” means something, this *de facto* privileged level seems to be the practical equivalent of a realm to which we do have “direct unmediated access.” Maybe this is the source of their blunder, which they proceed to compound in a most remarkable way.

Imagine the hoots if a social constructivist were to contend that, because we lack “direct unmediated access to external reality,” we must stop making factual assertions, no matter how convincing our evidence. Yet the authors say something (90) that amounts to this and faithfully follow it by refusing to say that Newtonian mechanics is true (to a high degree of approximation). Their discussion is based on an earlier one by Sokal (*A House Built on Sand* 13) that I debunk in “A physicist experiments with scholarly discourse.” There he 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... 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. Since I shall claim later that much

¹³ The sentence preceding this reads, “The correct answer to any scientific question, solved or not, depends on the state of Nature (for example, on the number of neutrinos that the Sun really emits).”

¹⁴ The first paragraph is adapted from “The infinite regress for evidence” in “Reading and relativism” (56-57).

¹⁵ “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, it seems clear, depend heavily on whether or not there *is* in fact a stampeding herd of elephants in the room—or, more precisely, since admittedly we have no direct, unmediated access to external reality—whether when we 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).”

contemporary work in Science Studies elides the distinction between ontology and epistemology, I don't want to leave myself open to the same accusation.

But if the amended assertions are again statements of fact, they must be glossed the same way and so on, *ad infinitum*, in which case, we get nothing. And if the amended assertions are not statements of fact, Sokal's bright idea amounts to a promise to stop making any!¹⁶ Yet he is serious about this and evidently Bricmont is too. In *Fashionable Nonsense* (90), they write:

Why did the European scientific community become convinced of the truth of Newtonian mechanics sometime between 1700 and 1750? ...[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.

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. Please note that *all* our assertions of fact—including “today in New York it's raining”—should be glossed in this way.

So, Drs. Sokal and Bricmont find the astronomical evidence that Newtonian mechanics rules the solar system (to a high degree of approximation) extremely convincing but Mr. Hyde is not convinced. All he is willing to say is “*if* this belief is correct.” How did the authors get into this mess and how were they able to go on in this way without someone—an editor, Nagel, *somebody*—noticing? Evidently, the emperor's new clothes come in many styles.

Redefining truth?¹⁷ Sokal and Bricmont present what they claim is a “radical redefinition of the concept of truth” by Barry Barnes and David Bloor. Here is their offending statement followed by their reading of it, followed by my debunking of their reading.

The relativist, like everyone else, is under the necessity to sort out beliefs, accepting some and rejecting others. He will naturally have preferences and these will typically coincide with those of others in his locality. The words ‘true’ and ‘false’ provide the idiom in which those evaluations are expressed, and the words ‘rational’ and ‘irrational’ will have a similar function. (Barnes and Bloor 1981, p.27)

But this is a strange notion of “truth,” which manifestly contradicts the notion used in everyday life. [*In a footnote, the authors allow that Barnes and Bloor may merely be noting that, what people believe, they call ‘true’ but even this is made into a complaint.*] If I regard the statement “I drank coffee this morning” as true, I do not mean simply that I prefer to believe that I drank coffee this morning, much less that “others in my locality” think that I drank coffee this morning! What we have here is a radical redefinition of the concept of truth, which nobody (starting with Barnes and Bloor themselves) would accept in practice for ordinary knowledge. Why, then, should it be accepted for scientific knowledge? Note also that, even in the latter context, this definition doesn't hold water: Galileo, Darwin, and Einstein did not sort out their beliefs by following those of others in their locality. (Sokal and Bricmont)

¹⁶ In one way, this makes sense. What better way to be super-precise than to say nothing?

¹⁷ This is adapted from “I follow you follow me” in “Reading and relativism” (47-48).

One doesn't have to be very clever to make such talk about preferences seem absurd and with "I prefer to believe that I drank coffee this morning" Sokal and Bricmont do just that. But if, when Barnes and Bloor say, "He will naturally have preferences," they mean merely that the acceptance of a belief is not arbitrary and, when they say that these preferences "will typically coincide with those of others in his locality," they mean only that most cows agree about most things, especially those who read the *Daily Moos*, then it is not at all absurd. Nor is it a redefinition of the concept of truth. Sokal and Bricmont turn the observation that our pattern of accepting and rejecting beliefs *typically coincides with* that of others in our locality into the bizarre claim that we sort out our beliefs *by following* those of others in our locality—who presumably sort out their beliefs by following ours.¹⁸

Taking it back?¹⁹ After Sokal and Bricmont convince themselves that Barnes and Bloor make, or pretend to make, a "radical redefinition of the concept of truth," they try to show that they sometimes "fall back, without comment, on the traditional sense of the word."

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.

Firstly, Barnes and Bloor note that the problem arises *if* one says that all beliefs are equally true. But *they* do not say this.²⁰ Secondly, they are not talking about a contradiction between beliefs held in different places but rather about one between beliefs that are held in the same place. Contrary to what Sokal and Bricmont imply, 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* (300).

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?

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

¹⁸ The claim is bizarre only because it is implicitly about *all* our beliefs. Everyone relies on the word of others for uncountably many things, some trivial, others matters of life and death. Scientists are no exception.

¹⁹ This is essentially the same as my critique of Sokal's discussion of these matters in *A House Built on Sand*, which is essentially the same as Sokal and Bricmont's discussion of them in *Fashionable Nonsense*.

²⁰ To say that two beliefs are *equally* true is to make an assessment.

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.

A Sensible Chapter? Take Two

In the last section, I considered some of the statements in the “long and sensible” chapter that Nagel failed to mention. In this one, I consider statements in the review that are presented as elaborations of ones in this chapter. But my criticism here is directed only at the statements in the review, not at the ones in the chapter.

Is there a fact of the matter about which theory is ‘most reasonable’ to accept?

[The] evidentiary relation pro or con between any experience and any theoretical claim always involves auxiliary hypotheses—things apart from the proposition and the evidence themselves that are being assumed true or false. There is nothing wrong with relying on many assumptions in the ordinary case, but it always is logically possible that some of them may be false, and sometimes that conclusion is forced on us with regard to an assumption that had seemed obvious. When that happens with a truly fundamental aspect of our world view, we speak of a scientific revolution.

Nagel says that it always is logically possible that something that is *assumed* to be true may be false. To see that this is a blunder, note that when what is assumed to be true *is* true, which, on Nagel’s view, is sometimes the case, he is saying that it is *logically* possible for something that is true to be false. He is right. It sometimes is, in the sense of possible world semantics. But in science, we are interested in what is true in *this* world, not in some other “possible” one. And in this world, a true statement is not false. Note that if Nagel had said “for all we know” instead of the pseudo-sophisticated “it always is logically possible that,” he would have avoided this problem. Note also that he neglects to consider the possibility that the assumption fails to refer to anything in the world and, hence, is neither true nor false. For those who associate scientific revolutions with incommensurability, conclusions of this kind play an important role. Nagel continues:

So far none of this implies that scientific reasoning is not objective, or that it cannot yield knowledge of reality.

So it may seem.²¹ However, Nagel seems indifferent here to the difference between denying that scientific reasoning is objective and, for whatever reason, neither accepting nor denying that it is. He seems to be responding to an unidentified opponent who has claimed, “Some of it *does* imply that scientific reasoning is not objective.” But, in my experience, believers like Nagel commonly mistake a denial of belief for a denial of the belief. I think it is much more likely that someone whom Nagel might think is denying that science is objective (in the metaphysical sense in which he means it) would actually be agnostic about it, if only because

²¹ But, unless he believes that, if there was such an implication, he would have spotted it—no matter how subtle it may be nor how many millions of words it may take to state it, he needs to insert “as I can see” after “So far.”

she would not think that she had a clear enough idea of what it is supposed to mean and would suspect that neither does Nagel.²² He continues:

All it means is that a scientific inference from evidence to the truth or falsity of any proposition involves in some degree our whole system of beliefs and experience; and that the method is not logical deduction alone, but a weighing of which elements of the system it is most reasonable to retain and which to abandon when an inconsistency among them appears. In normal inquiry, this is usually easy to determine; but at the cutting edge it is often difficult, and a clear answer may have to await the experimental production of further evidence, or the construction of new theoretical hypotheses. This means that most of our beliefs at any time must in some degree be regarded as provisional, since they may be replaced when a different balance of reasons is generated by new experience of theoretical ingenuity. It also means that an eternal set of rules of scientific method cannot be laid down in advance. But it does *not* mean that it cannot be true that a certain theory is the most reasonable to accept given the evidence available at a particular time, and it does *not* mean that the theory cannot be objectively true. Truth is not the same as certainty, or universal acceptance.

This, too, reads as if Nagel is replying to an unidentified opponent, one whom he takes to be saying in part, “It *does* mean that it cannot be true that a certain theory is the most reasonable to accept.” As before, I think that it is much more likely that someone whom he would take to be a denier would actually be agnostic.

A theory-laden diet of sundaes: Nagel next employs a thought experiment to argue that, in some cases, we are *rationally* required to accept (or to reject) some theory. My problem is not with his conclusion, about which I am neutral, but with the way that he argues for it.

Another point sometimes made against the claim of scientific objectivity is that experience is always “theory laden,” as if that meant that any experience which seemed to contradict a theory could be reinterpreted in terms of it, so that nothing could ever rationally require us to accept or to reject a theory. As Sokal and Bricmont point out, however, nothing of the kind follows.

Suppose I have the theory that a diet of hot fudge sundaes will enable me to lose a pound a day. If I eat only hot fudge sundaes and weigh myself every morning, my interpretation of the numbers on the scale is certainly dependent on a theory of mechanics that explains how the scale will respond when objects of different weights are placed on it. But it is not dependent on my dietary theories. If I concluded from the fact that the numbers keep getting higher that my intake of ice cream must be altering the laws of mechanics in my bathroom, it would be philosophical idiocy to defend the inference by appealing to Quine’s dictum that all our statements about the external world face experience as a corporate body, rather than one by one. Certain revisions in response to the evidence are reasonable; others are pathological.

Turning to the example, Nagel believes that he can show that the apparent weight gain, as evidenced by the bathroom scale, requires a rational dieter to abandon his theory. To this end, he supposes that the dieter tries instead to explain away the apparent failure of the

²² Similarly, someone whom Nagel thinks is denying that scientific reasoning can yield knowledge of reality is more likely to be saying “Who knows?”

theory by postulating that the diet caused a local change in the relevant laws of mechanics.²³ But instead of calling attention to the unending demand of special pleadings that would seem to be needed to maintain the causal claim, Nagel mistakenly supposes that it suffices for him to *say* that there is no such causal relation. He seems to forget that it is not enough to convince his readers, whom he has every reason to think are already firmly convinced. He owes them a rational argument. Worse, Nagel argues here against only *one* attempt to save the dietary theory in the face of appearances. But his claim is that *no* attempt can work.²⁴ So, even if his argument were valid, it would be of little help.

Also, unless I badly misunderstand, Nagel finds it preposterous that one could lose weight by a diet consisting only of hot fudge sundaes. But it is not. If, for example, each sundae is sufficiently small and there are sufficiently few of them, a weight loss is almost a sure thing. Perhaps Nagel did not have such a possibility in mind but he should have. Moreover, what counts here as sufficiently small and sufficiently few is relative to the size, metabolism and other particulars of the dieter. Thus, a dieter whose weight had been stable at around 400 pounds might well lose weight on the same hot fudge sundae diet that Nagel would expect to lead to a weight gain for *his* dieter, who is assumed to be Nagel himself. Thus, contrary to what he implies, it would be irrational for *my* dieter to immediately abandon his dietary theory on the evidence of his scale readings in favor of Nagel's apparent assumption that eating only hot fudge sundaes causes everyone to gain weight. Other, less implausible, possibilities would need to be explored. Maybe the scale is broken or the dieter has been reading it wrong. Or he sleepwalks and eats vast quantities of pasta when he does. Etc..

Finally, notice that, in the next to last sentence of Nagel's statement, his criticism is directed not at the dieter's *inference* from his apparent weight gain to the conclusion that eating only sundaes changes the laws of mechanics in his bathroom but, rather, at the idea of *defending this inference* by an appeal to the dictum that all statements face experience as a corporate body. But this has it exactly backwards. What makes it possible for the dieter to adopt his *ad hoc* explanation of the scale readings is precisely his *disregard* for this dictum. Instead of allowing *all* statements to face *experience* "as a corporate body," the dieter considers only *two* of them, the dietary theory and the laws of mechanics in the bathroom, together facing a *minute piece* of experience.²⁵

11, 12, 14, 15:

Feyerabend is more consistently outrageous than Kuhn, deriding the privileged position of modern science as a way of understanding the world. "All methodologies have their limitations," he says in *Against Method*, "and the only 'rule' that survives is 'anything

²³ Actually, Nagel considers two very different explanations without seeming to notice that they are different. One is that his intake of ice cream sundaes alters the laws of mechanics in his bathroom. The other is that his interpretation of the numbers on the scale is dependent on his dietary theories. But this must be a mistake because Nagel is not interested in explaining of *his interpretation* of these numbers, which, in fact, might well depend upon *his dietary theories*.

²⁴ To see this, recall that Nagel means to refute the view that "nothing could ever rationally require us to accept or to reject a theory." In the case he considers, the theory is the dietary theory and he means to show that it is contradicted by the experience of apparent weight gain as evidenced by the readings on the bathroom scale.

²⁵ Similarly, if, as it seems, Nagel believes that the scale readings rationally refute the diet theory, this too is a result of disregarding the dictum, in this case by allowing only one statement to face one bit of experience.

goes.” As Sokal and Bricmont point out, the first clause of this sentence maybe be true, but it does not in any way support the second.

This is Nagel supporting Sokal and Bricmont against Paul Feyerabend. But does Feyerabend say that the first clause supports the second? Not on the face of it. He joins the two clauses by “and” not “hence.” But if he is not making such a claim, it is fair to ask how he does support the second clause. It also is fair to assume that Nagel sees nothing, either in the first clause or the rest of the text from which the remark is taken, that supports it. However, it is not clear that what Nagel means by Feyerabend’s second clause is what Feyerabend meant by it—a consideration that does not seem to have captured Nagel’s interest. An obvious guess is that Feyerabend is noting, in a clever way, that, even in science, there are no constitutive rules—no law, theory, method or measurement that must be retained no matter what, on pain of it ceasing to be science. But is *this* what Nagel finds outrageous? He continues:

I was a colleague of Feyerabend’s at Berkeley in the 1960’s, and once it fell to the two of us to grade the German exam for the graduate students in philosophy. About twenty of them took it, and their papers were numbered to preserve anonymity. We discovered that the department secretary who assigned the numbers considerably left out of the number thirteen, and Feyerabend was appalled and outraged by this display of rank superstition. But his views developed; and both he and Kuhn have a lot to answer for.

Here too Nagel mistakes what it is like to be Nagel for what it was like to be Feyerabend. Just because, if the outrage had been Nagel’s, it would have been accompanied by certain interior metaphysical baggage, it doesn’t follow that Feyerabend’s outrage was accompanied by such baggage. Although Nagel may believe that, if he were to experience such outrage, he would be able to justify it—not merely to his own satisfaction but objectively, in the sense of metaphysical realism, it seems unlikely that Feyerabend was subject to *this* conceit.

Constructing the past:

Both [Feyerabend and Kuhn] are repeatedly cited in support of the claim that everything, including the physical world, is a social construction existing only from the perspective of this or that cognitive practice. ... Thus Sokal and Bricmont tell us that the sociologist of science Bruno Latour recently challenged as anachronistic the report of French scientists who examined the mummy of Ramses II that the pharaoh had died of tuberculosis, because the tuberculosis bacillus came into existence only when Robert Koch discovered it in 1882.

Contrary to what Nagel implies here, the view attributed to Latour is not a consequence of holding that the physical world is “a construction,” social or otherwise. Nor is it, as Sokal and Bricmont claim (96), a matter of his “playing constantly on the confusion between facts and our knowledge of them.” It is a consequence of forgetting that the world has a temporal component, which includes the past, as well as a spatial one—a blunder that Sokal also makes (*A House Built on Sand*: 13).

Kiss.²⁶

As Sokal and Bricmont point out, the denial of objective truth on the ground that all systems of belief are determined by social factors is self-refuting if we take it seriously, since it appeals to a sociological or historical claim that would not establish the conclusion unless it were objectively correct.

Why make it this complicated? It doesn't matter what the ground is for the denial. The denial of the existence of objective truth is not an objective truth. Full stop. More important, Nagel is following Sokal and Bricmont up the wrong tree. Again, no matter how those whom he attacks may seem to put it, non-acceptance, not denial, is very often all they mean. It also is all that is reasonable. In his careful account of Wittgenstein's agnosticism, in *The Last Word*, Nagel finds no self-refutations where other philosophers, who conflate this agnosticism with denial, do. Instead of making the same conflation himself elsewhere, Nagel would do better to seek cases that are framed in terms of denial and in which non-acceptance does not suffice for the 'critic of objectivity' to make her point. These would be the ones worth pointing out.

Last call: In this final section, I criticize a few more of Nagel's contributions to philosophy of science, some of them inspired by the authors "long and sensible" chapter about it, others not. He writes:

Relativism is kept alive by a simple fallacy, repeated again and again: the idea that if something is a form of discourse, the only standard to which it can answer is conformity to the practices of a linguistic community, and that any evaluation of its content or its justification must somehow be reduced to that. This is to ignore the differences between types of discourse, which can be understood only by studying them from inside.

This simplistic view of relativism is kept alive by Nagel's repeated refusal to study it from inside. Were he to abandon this anti-intellectual stance, he would soon discover not only that relativism is, almost by definition, committed to studying different discourses from inside but, also, that his statement about "conformity to the practices of a linguistic community" is more than a little simplistic. A little later, he informs us that:

Where agreement [in science] exists, it is produced by evidence and reasoning, and not vice versa.

This sounds nice but so does "sincerely yours." Nagel will not find evidence for "vice versa" until he gets serious about seeking it. He will then have to read the relevant case studies with much greater care than he so far seems to have done. It is dangerously easy to read them in a way that makes his claim seem obvious. This also is true for the following claim.

The constantly evolving practices of those engaged in scientific research aim beyond themselves at a correct account of the world, and are not logically guaranteed to achieve it.

Nagel ignores the standard alternative view that scientific research aims at coherence not at correctness. Yet how, without comparing the alternatives, can he legitimately conclude that it

²⁶ "It means, 'Keep it simple, stupid,'" my mechanic explained after responding, "Kiss," to my asking whether a problem with my windshield wipers might be due to manufacturing imperfections in the windshield.

is “more reasonable” to say, as he does, that these practices aim at a correct account of the world rather than at a coherent one?

Finally, further on, Nagel tells us that, in a criminal investigation, the evidence and relevant background assumptions of the investigators “do not entail an answer, but they often make one answer more reasonable than others.” Really? All *I* know is that they often make one answer *seem* more reasonable than others. Why does Nagel think that he doesn’t need the qualifier ‘seem’? I have no answer but, were I to seek one, I would first try to get a *general* sense of when and why Nagel does and does not eschew the use of qualifiers like ‘seem,’ ‘I think’ and ‘from my perspective.’ For this, the introduction to *The Last Word* (3-6) is a good place to begin. I think.

Appendix

How can a true belief be unreliable? In this section and the next, I defend a claim I make at the end of “Confused thinking about confused thinking.” The second item on Sokal’s list of five issues that Harding allegedly conflates is:

Epistemology: How can human beings obtain *knowledge* of truths about the world?²⁷ How can they assess the *reliability* of that knowledge? (Sokal’s “Afterword”)

In footnote 113 of *Fashionable Nonsense* (88), the authors write,

Philosophers usually understand the word *knowledge* to mean something like “justified true belief” or some similar concept; but Bloor begins by offering a radical definition of the term.

But if knowledge is something like justified true belief, then ‘the reliability of knowledge’ is an oxymoron. How can a true belief be unreliable? Elsewhere, referring to the same alleged radical redefinition of the word *knowledge*, Sokal says:

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, *A House Built on Sand*: 14)

Here Sokal commits the very sin of which he accuses Barnes and Bloor. He in effect redefines the word *knowledge* without acknowledging that this is what he is doing, much less explaining why he is doing it. Unlike justified true belief, Sokal’s kind of knowledge can be *more or less* reliable! This is not a one-shot abuse. In his announcement of the hoax, Sokal says that human beings can and do obtain “reliable, albeit imperfect and tentative knowledge of [physical] laws.”²⁸

²⁷ “Knowledge of truths about the world”? Why not just “knowledge”?

²⁸ Perhaps he is conflating knowledge with belief or even with evidence..

How can a statement be influenced by social factors? The third item on Sokal's list reads:

Sociology of knowledge: To what extent are the truths *known* (or *knowable*) by humans in any given society influenced (or determined) by social, economic, political, cultural and ideological factors. Same question for the false statements erroneously believed to be true. (Sokal's Afterword)

But how and in what ways can such factors *influence* or even *determine* a statement?²⁹ Sokal gives an example that seems to bear on what he means. He writes:

Likewise, sociological questions arise, for example: To what extent is our (true) knowledge [likewise, erroneous theories, if any] of computer science, quantum electronics, solid-state physics, and quantum mechanics been the result (in whole or in part) of social, economic, political, cultural, and ideological factors, in particular the culture of militarism? (Afterword)

This is still murky but at least it makes it plausible that Sokal has in mind whether and how social and other factors influence *the processes of* belief acceptance, which includes but of course is not identical with knowledge acquisition.

²⁹ I can answer this but not in a way that Sokal would like.