

Reading and Relativism

An introduction to the science wars¹

Gabriel Stolzenberg²

When Larry was a kid his mother...sometimes, out of curiosity, stopped the dial at a place where foreign languages came curling out of the radio's plastic grillwork: Italian or Portuguese or Polish... "Jibber jabber," Larry's father called this talk, shaking his head, apparently convinced, despite all reason, that these "noises" meant nothing, that they were no more than a form of elaborate nonsense. Everything ran together; and there weren't any real words the way there were in English. These foreigners were just pretending to talk, trying to fool everyone. (Carol Shields, *Larry's Party*)

Late one afternoon in the spring of 1996, I heard part of a radio interview with a physicist named Alan Sokal. It was about something he had just done. He explained that he had been inspired to do it by reading *Higher Superstition*, a book by Paul Gross and Norman Levitt (1994) that also inspired Robert Bork (1996). Several years earlier, a colleague had tried to persuade me to read this book by shoving it under my nose open to a page on which an opaque remark by Jacques Derrida is alleged to reveal his "eagerness to claim familiarity with deep scientific matters." The remark begins, "The Einsteinian constant is not a constant, is not a center." My colleague expected me to find this ridiculous but I explained that I could not find it anything because I had no idea what it means. The authors talked as if they did but they gave me no reason to believe them.³ Later, after some investigation, I concluded that not only did they not know what Derrida meant, they probably did not care. They were engaged in an attempt at humiliation, not scholarship.

Therefore, imagine my astonishment when Sokal's one example of inspiration provided by *Higher Superstition* turned out to be Derrida's alleged "eagerness to claim familiarity with deep scientific matters." He offered no proof of the allegation—at least, nothing one would normally think of as a proof. He merely read the remark aloud, said that he had no idea what it means and paused for the interviewer to burst out laughing.⁴

For all I know, Derrida really did not know what he was talking about thirty-three years ago, when he made this remark as part of an answer to a question following a lecture about structuralism. I think he did. I even have an interpretation that I find plausible. But I have no proof that he did, so I don't say so. However, no such epistemic niceties deterred either Gross and Levitt or Sokal from pronouncing Derrida's remark nonsense. Nor were these isolated cases. In the literature spawned by Sokal's hoax, I often found high-minded talk about the importance of "doing it right" keeping company with readings and arguments as shabby or non-existent as in the attacks on Derrida. For example, in *A House Built on Sand*, the editor, Noretta Koertge, writes:

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. (Koertge 1998: 5)

Yet in the opening chapter, Sokal's "mechanism of scholarly self-control" goes haywire.⁵ Then, in the next one, the analytic philosopher, Paul Boghossian, touts his discipline's "subtle discussion of concepts in the philosophy of language and the theory of knowledge." Yet his scholarship is as shoddy as Sokal's.⁶ Also, at least two of the other chapters are disfigured by hostile misreadings⁷ that could easily have been eliminated.

Alas, my concerns were not widely shared. The attitude I usually encountered among scientists and old-fashioned humanists was "Why are you worrying about this? It's the other guys who are on trial." And *sotto voce*, "In a war, the good guys do dirty things too." Welcome to the science wars! I confess that I found it exciting. Sokal's unerring sense of what he could get away with never ceased to amaze me. Forget about the Derrida caper on the radio or even his success at getting *Social Text* to publish his hoax. I was far more impressed by his self-serving polemic that immediately became the received wisdom about how to read the hoax (where to laugh), why he did it and why the editors of *Social Text* published it—the last of which Sokal almost surely did not know when he wrote this. I was also very impressed by Steven Weinberg's influential essay, "Sokal's hoax" (1996a). On a non-skeptical reading, it is clear and convincing. Yet on a skeptical reading, as Weinberg would have seen had he made one, it is riddled with confusion.⁸ That he did not bother to do so suggests that he intuited correctly that few readers would look closely at what he said and that those who did could be ignored.⁹

Initially, I saw the science wars as the Derrida case writ large—otherwise respectable academics accusing others of spouting nonsense about science or mathematics on the basis of perverse and often preposterous interpretations of their remarks. And *other* otherwise respectable academics cheering them on.¹⁰ Critical thinking seemed to have taken a holiday. In "Hatchet jobs," below, I consider representative examples of these attacks. All of them have the appearance of self-fulfilling prophecies. People hunt for nonsense in statements of authors suspected of being partial to it and, when they find what looks like it, they consider themselves done. Yet had they hunted instead or in addition for more generous interpretations, they almost surely would have found them, as I did.¹¹ So much depends upon the reader's trust! As a cautionary tale, before turning to the hatchet jobs, I consider an authoritative-looking remark about physics by the nonsense hunter, Thomas Nagel (1998: 36). On a literal reading, he is spouting nonsense about the special theory of relativity. But the physics alone does not enable me to judge how well this reading reflects his grasp of the subject and, at the end of the day, it seems to be solely the extent of my trust in that grasp that determines how I make the call.

Although my attention was first limited to examples of this kind, I learned soon enough that the allegations of nonsense spouting were not limited to science and mathematics. Much of the ridicule was directed towards statements about our relationship to reality. I learned also that the alleged nonsense spouting was attributed to the pernicious influence of relativism and social constructivism, philosophies whose alleged denial of objective truth and justification were said to license disdain for logic, evidence, truth—even intelligibility. The reasoning went something like this.

If one does not admit the possibility of objective truth and justification, then anything goes. Reasoning is merely a form of rhetoric, science is just another belief system and there is no need to master a subject before holding forth on it because there is no such thing as “getting it right.”

The existence of people who fit this description may be a logical possibility. I can just barely imagine a world in which people are indoctrinated to think and behave this way. But it requires no act of imagination to reflect with awe upon the scholarship produced over centuries by scholars skeptical about simplistic conceptions of objectivity hardly different from those paraded in the science wars. It therefore is dismaying that so many people who should know better believe that skepticism of this kind licenses nihilism. Even worse, many believe that the sky has already fallen. Skepticism about objectivity, in the form of a ‘postmodern’ relativism in which ideology trumps truth, is believed to have taken over entire sectors of the academy and with ‘the expected’ consequences.¹²

Gradually, relativism and social constructivism and the evils attributed to them became the focus of my concerns. People were said to hold beliefs that imply that we can alter reality¹³ at our whim, that before there were humans, there were no dinosaurs and that what we call scientific knowledge is the result of negotiation not inquiry!¹⁴ What are relativism and social constructivism? Why are they thought to have such preposterous implications? And why do they generate such hostility, even fear? After “Hatchet jobs,” I begin to offer answers. In “Relativism and social constructivism,” I contest the conventional wisdom that these two isms deny context-independent truth and justification. In fact, they neither deny nor affirm them.¹⁵ The exemplar of this agnosticism, which is radically different from denial, is the philosophy of Ludwig Wittgenstein after the *Tractatus*.¹⁶ Although some philosophers worry that this alleged agnosticism collapses into denial, what seems to bother people most is Wittgenstein’s unwillingness to affirm that beliefs like the laws of physics are objectively true and that others like witchcraft and astrology are objectively false. That he affirms such things in the ordinary way does not alleviate their concern, which seems to have its root in Wittgenstein’s view that reasons go just so far, beyond which there is at most trust.

In “Reality is hard to talk about,” I use a version of the belief/knowledge distinction to show that seemingly innocuous realist statements about reality are, on realism’s own terms, badly confused. This confusion is one source of realist misrepresentations of relativism and social constructivism. The same method is then used to expose a more serious confusion that I believe is the main source of realist objections to constructivist science studies. Here, a powerful intuition that the truth of a statement often figures significantly in explaining what makes us believe it makes it extremely difficult for scientists to cotton on to an equally powerful counter-intuition that the growth of scientific knowledge should be explained purely as a process of belief formation.

This completes my introduction to my introduction to the science wars. The rest is a series of case studies, divided into the three sections described above. Although little that I argue for is difficult to prove, almost all of it stands the conventional wisdom on its head. This, I suppose, is its significance for the science wars. However, for me, personally, this chapter is primarily an attempt to persuade readers that trying to make sense of another human being is no less interesting a challenge and noble a pursuit than trying to make sense of the physical world. And that we are not yet very good at it.

Hatchet jobs

Read it as you would a love letter.¹⁷

The philosopher, Thomas Nagel, is a great admirer of the part of *Fashionable Nonsense* devoted to mocking people for spouting nonsense about science and mathematics. Indeed, in his review of this book (1998), he applauds the authors for their useful “hatchet job.”¹⁸ But how can we tell whether a statement about science or mathematics is nonsense? Nagel seems to believe that he knows it when he sees it.¹⁹ But it is trickier than he supposes. To see why, consider the following statement that Nagel makes in his review.

[T]he world of Einstein’s special theory of relativity, in which the interval between two widely separated events cannot be uniquely specified in terms of a spatial distance and a temporal distance, is not one that can be intuitively grasped, even roughly, by a layman.²⁰

I will offer two readings of this. The first is ungenerous and unforgiving but two physicists signed off on it, one of whom joked, “Let’s put Nagel in a remedial relativity course with Latour.”²¹ The second reading is generous and forgiving. Another physicist persuaded me of it. I greatly prefer it because I would much rather have a good opinion of Professor Nagel than a bad one.

An unforgiving reading

Contrary to what Nagel says, the interval between two events can be uniquely specified in terms of an associated spatial distance and a temporal one. Maybe he meant to say that the space-time interval between two different events does not uniquely specify associated spatial and temporal distances. But this is true even in a single reference frame and has nothing to do with the physics. The hypotenuse of a right triangle does not uniquely specify its opposite sides, yet nobody believes that the world of Euclid’s theory of plane geometry “cannot be intuitively grasped, even roughly, by a layman.”

This is not a malicious reading. It is not even a skeptical one. An unintelligible statement in *The New Republic* stands out. Moreover, everything I say is correct, though not necessarily as a reading of what Nagel meant. I take “interval” to mean “space-time interval.” For the special theory of relativity, I do not see what other kind of interval between two events he could have meant. When I say that the interval between two events can be uniquely specified in terms of a spatial distance and a temporal one, I am relying upon the standard mathematical interpretation of “uniquely specified in terms of.” I also rely on it in the second sentence, in which I, in effect, allow that Nagel may have meant to say instead “does not uniquely specify.”

A forgiving reading

For a forgiving reading, I assume that Nagel has a reasonably good layman’s grasp of special relativity but was muddled when he wrote this. I then try to guess what point he “really” meant to make. A natural guess is “In the special theory of relativity, neither the spatial nor temporal distance between two different events is the same in every reference frame.” Thus, we pretend that Nagel did not say “widely” and we assume that he was not

talking about the space-time interval. Indeed, we assume that when Nagel used the word “interval,” he wasn’t talking about anything. He was just having trouble finding a phrase like “for any two different events” and settled instead on the unhappy choice of “the interval between two widely separated events.” As in the unforgiving reading, we also assume that when he said, “cannot be uniquely specified in terms of,” he really meant to say, “does not uniquely specify.” This leaves us with something more awkward than the guess I made above but the content is essentially the same. There is one more point. The spatial distance between two events is relative even in classical physics.²² Therefore, to capture what is special about special relativity, Nagel should have contented himself with something like, “The temporal distance between events is not the same in all frames.” However, for most readers of *The New Republic*, the relativity of simultaneity would have been an even better choice.

To me, the unforgiving reading seems fair, the forgiving one wildly generous. But which if either of them is accurate depends on whether Nagel “really” knew what he was trying to say. If, as I prefer to believe, he did, then the wildly generous reading is far more accurate. If he did not, it is not. But this is not a question that a physicist can settle for us. Indeed, physicists endorsed both readings. Also, even if the generous reading is far more accurate, what ended up in *The New Republic* is best seen as gibberish.²³

What’s this, Polus?

Socrates: What’s this, Polus? You’re laughing? Is this yet another kind of refutation which has you laughing at ideas rather than proving them wrong? (Plato’s *Gorgias* 473e, Waterfield translation)²⁴

In the same review of *Fashionable Nonsense*, Nagel writes, “The chapters dealing in more detail with individual thinkers reveal that they are beyond parody. Sokal could not create anything as ridiculous as this, from Luce Irigaray:

Is $E = Mc^2$ a sexed equation? Perhaps it is. Let us make the hypothesis that it is insofar as it privileges the speed of light over other speeds that are vitally necessary to us. What seems to me to indicate the possibly sexed nature of the equation is not directly its uses by nuclear weapons, rather it is having privileged what goes the fastest....”

This may send Nagel into convulsions but how does he know that it is her problem not his? How can he possibly know unless he knows what Irigaray means by “sexed” and “privileges” and that her reference to speeds is not an ironic metaphor? If he does not know these things, he is kidding himself. But if he does know, why does he not tell us, so we can join in the fun of mocking Irigaray? Instead of fulfilling his obligation as a philosopher to give us a reason to believe what he says, Nagel encourages us to trust that whatever Irigaray means is refuted by the authors’ “comically patient” observation,

Whatever one may think about the “other speeds that are vitally necessary to us,” the fact remains that the relationship $E = Mc^2$ between energy (E) and mass (M) is experimentally verified to a high degree of precision, and it would obviously not be valid if the speed of light (c) were replaced by another speed.

This shows especially poor judgment. If Sokal and Bricmont think that something that is privileged can easily be replaced, there is little reason to suppose that they have any idea of

what Irigaray is talking about.²⁵ And by mocking her instead of giving us an argument, Nagel makes it appear that neither does he.

I am not a reference frame

“Incompetent individuals were less able to recognize competence in others,” the researchers concluded.²⁶

In the opening chapter of *A House Built on Sand*, Sokal presents what he considers to be some of the silliest statements that he could find “about mathematics and physics...from some of the most prominent French or American intellectuals.”

Now, what precisely do I mean by “silliness”? Here’s a very rough categorization: First of all, one has meaningless or absurd statements, name-dropping, and the display of false erudition. Second, one has sloppy thinking and poor philosophy, which come together notably (though not always) in the form of glib relativism.

Almost any statement that Sokal offers as an example of silliness serves as a caution that merely being a scientist does not make one competent to assess the use of scientific looking language in non-scientific texts. One also must know how to read. I think Sokal has no idea how difficult this can be. Consider his claim that Bruno Latour “doesn’t understand what the term *frame of reference* means in physics—he confuses it with *actor* in semiotics.” His evidence is that Latour says (1988: 10-11),

If there are only one, or even two, frames of reference, no solution can be found....²⁷ 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.

To misread this as Latour confusing the concept of a frame of reference in physics with that of an actor in semiotics requires a high degree of silliness.²⁸ Sokal knows that Latour is discussing relativity theory as expounded in a popular book by Einstein and that the two frames of reference are ones that Einstein introduces. One is stationary with respect to a train, the other with respect to an embankment. Each also is stationary with respect to an actor: one on the train, the other on the embankment. Hence, when we specify one of the actors, we specify one of the frames and conversely. All this is Einstein and it is why Latour can shift from talk about the two frames of reference to talk about the first two actors without changing the subject. Latour’s only contribution is the third actor, whose work requires not only a frame of reference but the Lorentz transformations between it and the first two frames.²⁹ However, bear in mind that although I am not a reference frame, all the ones with respect to which I do not move are uninterestingly different from the standpoint of the theory of relativity. In this sense, actors do come close to specifying reference frames. The converse is obviously false.

Places in space

Consider next Sokal’s claim that Latour “somehow got the idea that relativity concerns the problems raised by the relative *location* (rather than the relative *motion*) of different observers.” It is impossible to read Latour’s essay about relativity without noticing that it is dominated by a consideration of two reference frames in relative motion—the two

discussed above. But perhaps Sokal forgot this when he came upon the following passage from Latour's essay, which is the evidence he offers for his accusation.

Provided the two relativities [special and general] are accepted, more frames of reference with less privilege can be assessed, reduced, accumulated and combined, observers can be delegated to a few more places in the infinitely large (the cosmos) and the infinitely small (electrons), and the readings they send back will be understandable. His [Einstein's] book could well be titled: "New Instructions for Bringing Back Long-Distance Scientific Travellers." (Latour 1988: 22-23)

Is Sokal assuming that Latour is talking about places that are stationary with respect to us? Why would Latour do that? When we talk about a place outside the earth, we almost always have in mind a material body, like Mars or Alpha Centauri, and almost every material body in the universe is in motion with respect to us. This already is telling. But the best evidence I have against Sokal's reading of the passage is Latour's reference to electrons. If an electron is a place to which an observer can be delegated, how likely is it that Latour has in mind only places that are stationary with respect to us?³⁰

This argument notwithstanding, I don't pretend to know what Latour had in mind when he wrote this passage. But I don't need to know. Sokal does because he made the accusation. Finally, in criticizing Sokal's misreadings of Latour's essay, I do not mean to suggest that it does not merit criticism. On the contrary, the very passages that Sokal quotes make me wonder whether Latour mistook things that Einstein has his cartoon observers do in order to explain the theory of relativity for what real physicists do when they use that theory.

Masculine channels and feminine flows

In "Gender encoding in fluid mechanics: masculine channels and feminine flows," N. Katherine Hayles explores the thesis that fluid mechanics, or at least hydrology³¹, is gender encoded masculine (1992: 16-44). In his contribution to *A House Built on Sand*, the engineer, Phillip Sullivan finds fault with almost everything in the account of fluid mechanics that Hayles uses to support her thesis. But after six pages of authoritative criticism that apparently suffice to make his case, instead of declaring victory and going home, Sullivan presses on with a page of accusations that are false and unfair.³² Here I consider the least trivial of them.

The excision of the observer

Sullivan accuses Hayles of misconstruing the history of the calculus by saying that when the calculus was made rigorous, "rigor [was] equated with the excision of the observer—a fundamental premise in the ideology of objectivity." Hayles supports the claim with a quote from the introduction to Carl Boyer's history of the calculus. Sullivan counters with a longer quote from the conclusion of Boyer's book, his distillation of which is that "rigor in mathematics is associated with excision of 'irrelevant elements'." He then asks sarcastically, "But what meaning can be attached to Hayles's assertion that rigor requires 'excision of the observer': that mathematical arguments cannot be observed?" Ignoring Sullivan's attempt at humor, we can begin to answer this by noting that Hayles does not say that rigor requires the excision of the observer. She says that, as a matter of historical fact,

this is the view that informed the rigorization of the calculus (Sullivan 1998: 82-83). She is right, although Boyer's history of the calculus does not provide an adequate account of this. The importance attached to the idea of mathematics as "a reality independent of us" caused Gottlob Frege and others to insist that all traces of a subject/constructor/observer be removed.³³ So, for example, because a rule presupposes a subject/constructor/observer that executes it, in the rigorized mathematics, a function is no longer a rule but a set.³⁴ Sullivan is not a philosopher of mathematics, so his ignorance of this foundational matter is understandable. But it does not excuse the sarcasm that he directs at Hayles for failing to share it.

When a mathematical point moves

Sullivan also accuses Hayles of misunderstanding the concept of a particle of matter, or mass point, in Newtonian mechanics (Sullivan 1998: 82-83). Here is her offending remark as quoted by Sullivan.

Euler...wanted to develop a theory of hydraulics that would be based on the trajectories of particles as they moved through the flow.³⁵ The simplest way to deal with the particles was to treat them as mathematical points. But, by definition a point has no extension and consequently no mass. When a point moves, nothing in the material world changes.

Sullivan has no patience for this. He writes,

Every undergraduate physics student knows that Hayles' statement is nonsense. Newton's three laws of motion show that the motion of a body under the action of external forces can be calculated exactly by concentrating all the body's mass at a single point in its interior known as the *center of mass*.³⁶

Given the context, this is outrageous. The statement Sullivan trashes is part of Hayles' description of how Euler thought about fluid flow. Sullivan does not contest its accuracy. But if Hayles' reading of Euler is accurate, then, according to Sullivan, Euler too needed to learn freshman physics. This is a serious blunder. Sullivan should either have checked the accuracy of Hayles' account or taken a pass. Yes, for computation, it often helps to pretend that there are point masses,³⁷ ignoring the consideration that each would have to be infinitely dense. But for a continuum model of fluid flow like Euler's, it is the opposite of helpful. If any point in a body of fluid has positive mass, and mass varies continuously with position, then the body of fluid has infinite mass. This is not a useful idealization. According to Hayles, Euler worked instead with point densities, which in this context is a very useful idealization.

The oracle of deconstruction

Derrida's remark about the 'Einsteinian constant' occurred during an exchange with his colleague, Jean Hyppolite, following a lecture by Derrida on structuralism, much of which was devoted to the history of the idea of a center for a structure (Derrida 1970: 247-272). Although the lecture had nothing overtly to do with physics, Hyppolite asked a question about the theory of relativity in an attempt to clarify the notion of a center. In "Sokal's

hoax,” after ridiculing Derrida as “the oracle of deconstruction” and calling the ‘Einsteinian constant’ remark babble,³⁸ Steven Weinberg says, “I have no idea what it is intended to mean” (Weinberg 1996a: 11). So far, this is no different from Sokal on the radio. But Weinberg goes on to say that although there are fields like physics that require a technical language that cannot be understood without special training, “Derrida and other postmoderns do not seem to be saying anything that requires a special technical language.” This is silly. If Weinberg has no idea what the remark is intended to mean, how can he tell whether it requires a special technical language? Later, in a reply to George Levine’s (1996: 54) objection to the emptiness of this reasoning, Weinberg tries harder.³⁹ He now argues that ‘the Einsteinian constant’ and ‘center’ are bluff, fancy looking jibber jabber designed to create an impression of profundity (1996b: 56).

The Einsteinian constant

Weinberg remarks that ‘the Einsteinian constant’ is not an expression used by physicists. (Weinberg 1996b: 56). Indeed, a glance at the text (1970: 267) shows that Derrida introduced it merely as a way of referring, in the context of the exchange, to whatever is the constant that Hyppolite connects to Einstein. However, it is just this use of the expression to refer to whatever it is that Hyppolite is talking about, without having to say or ask what it is, that makes Weinberg think that Derrida is bluffing. He thinks Derrida “just started talking about the Einsteinian constant without letting on that (as seems evident) he had no idea of what Hyppolite was talking about” (Weinberg 1996b: 56). But “as seems evident” is hardly an argument. His accusation is only a suspicion.

Also, remember that Hyppolite started this by not bothering to tell Derrida explicitly what constant he had in mind. Why would he do that? One possibility is simple carelessness. But another is that this was not the first time that he and Derrida had talked about this subject. Furthermore, the back and forth that follows Derrida’s reply strongly suggests that Hyppolite, if not Weinberg, believed that Derrida knew what Hyppolite was talking about.⁴⁰ Was Derrida that good a bluffer? Note also that although Derrida doesn’t say what the Einsteinian constant is, he is willing to say things about it. If he is bluffing, this is a risky thing to do. For example, if Hyppolite’s constant is either the speed of light or the space-time interval, then calling it “the very concept of variability” does not seem like a good idea. So, contra Weinberg’s bluff hypothesis, we have support for the idea that both Derrida and Hyppolite thought that Derrida knew what Hyppolite was talking about and even for the possibility that he did. For Weinberg to show that he is not just peddling his prejudices here, he has to give an argument that is not vulnerable to such objections. This he has not done and that he pretends that he has is itself a bit of a bluff.

A center for a structure

For ‘center,’ Weinberg offers two pieces of evidence. The first consists in quoting one of the twenty-nine sentences in Derrida’s lecture about the idea of a center and reporting, “This is not of much help.” Indeed it is not. Perhaps neither are the other twenty-eight sentences, but how do we know? Weinberg’s other piece of evidence is that Hyppolite’s exchange with Derrida shows that, even after hearing both the lecture and Derrida’s

response to his question, Hyppolite still does not know what a center is. So it seems. However, the question that Hyppolite asks shows that he knows at least this much: that a center for a structure is supposed to be constant with respect to any variation that the structure can sustain without loss of identity. By contrast, Weinberg, who writes confidently about what Hyppolite does and does not know, gives us no reason to think that he knows even this. Like Weinberg, I doubt that Derrida is attempting to introduce a precise concept in his discussion of ‘a center for a structure.’ But I draw a different conclusion from this because I take seriously Derrida’s “reiterated warning that his texts are not a store of ready-made ‘concepts’ but an *activity* resistant to any such reductive ploy.” I am quoting Christopher Norris (1996: 24), author of *Against Relativism* (1997), a book that Weinberg would do well to read before beginning his next exegesis of the Derrida quote. In it, he will find the “physics babbler” enlisted as an important ally in a defense of the privileged status of science. Life is complicated.

Weinberg concludes his surmising with a flourish. “It seems to me that Derrida in context is even worse than Derrida out of context” (Weinberg 1996b: 56). I am sure it does. But why is Weinberg so sure that he read Derrida in the relevant context? I see no evidence of it. Derrida talks about the idea of a center in a historical context that extends back to antiquity.⁴¹ Does Weinberg think this is irrelevant? Michael Harris’s wickedly perceptive observation about a related conceit of Sokal and Bricmont applies to Weinberg’s without significant change.

In some cases, we have quoted rather long passages, at the risk of boring the reader, in order to show that we have not misrepresented the meaning of the text by pulling sentences out of context. (Sokal and Bricmont 1998: 17.)

This may satisfy those who imagine that the context of page 50 is pages 48-52, say, but if the context is an ongoing literary debate or an entire culture’s orientation to mathematics and science, then the length of the quotations is irrelevant. To paraphrase remarks made by David Bloor, Sokal and Bricmont are ‘as it were, coming into the middle of a conversation that has been going on for some time.’ (Michael Harris, “I know what you mean!”)⁴²

This completes the first series of case studies. In the next, the focus shifts from statements about science or mathematics to ones about our relationship to reality. In each case, I argue that charges of nonsense spouting are based on careless reading and a failure to distinguish between atheism and agnosticism about the possibility of context-independent truth and knowledge. This second series of case studies is prefaced by a thumbnail sketch of the post-Tractatus philosophy of Wittgenstein, which I offer as an exemplar of the mindset that informs both relativism and social constructivism.

Relativism and social constructivism

Deny that non-context-dependent assertions [exist and] can be true, and you...throw out the Nazi gas chambers.⁴³ (Sokal, *Afterword*.)

But to require that the truth about Nazi gas chambers not be context dependent suffers from as bad a problem. If, after presenting compelling evidence that there were gas chambers at Auschwitz-Birkenau, we still had to prove that the evidence is not context-dependent, we would be “throwing out” not only the gas chambers but also our sanity. A claim that the truth-value of a statement is not context-dependent is too strong for us to get evidence for it, much less proof. Yet many people believe otherwise. When they look around and see no context dependence, that is their evidence. Moreover, the idea of context independence is almost irresistible. When we formulate a statement, a relationship between it and the world is created that is itself in the world independent of us. Or so it seems. There are also counter-intuitions, for example, the constructivism of Kant’s *Critique of Pure Reason* (1998) and Wittgenstein’s discussion of rule following in *Philosophical Investigations* (1968).

The counter-intuition in *Philosophical Investigations* is that language never determines its applications. But language often does seem to determine its applications. Stipulations seem to stick and this impression probably is essential for our cognitive well being. To illustrate his ideas, Wittgenstein considers the rule for adding two, arguing that no matter what we say or do, we do not seem to determine its application. In support of this, note that we usually do not have a rule for following a rule and, if we do, we usually do not have a rule for following that rule. For Wittgenstein, the problem is not the possibility of multiple interpretations of a statement or rule or a change in its meaning. He does not think that there is even one way of ‘interpreting’ a statement or ‘following’ a rule, even though we seem to do it all the time. Nor is it a matter of vagueness. Some philosophers seem to think that, at least in principle, vagueness can be removed by arbitrary stipulation.⁴⁴ But Wittgenstein’s view is that we cannot stipulate anything, not even the rule for adding one, although it often seems that we can. If this is right, the idea that we can say things about a reality independent of us runs into trouble—not with the idea of such a reality but because we are unable to endow our statements with meanings that can leave home and make it on their own.

The mindset of the *Investigations* is the irreducible core of social constructivism and its alter ego, relativism. It therefore is not surprising that some readers of it or related writings like *On Certainty* (1972) think that, in them, Wittgenstein frequently comes disturbingly close to denying objectivity. Hilary Putnam (1992: 168–179) recalls his dismay on encountering, in *On Certainty*, questions like,

§609: ...Is it wrong for [people] to consult an oracle and be guided by it? If we call this “wrong,” aren’t we using our language game as a base from which to *combat* theirs?

§610: And are we right or wrong to combat it? ...

§612: I said I would “combat” [people who consult an oracle]—but wouldn’t I give them *reasons*? Certainly, but how far do they go?

Putnam first took this to be a clear denial of objectivity but he revised this view after a closer reading (1992:172). He now understands it to say⁴⁵ that, although we may be right to combat another mindset, argument is unlikely to be effective if it is too different from our own. Moreover, in deciding, from within our mindset, whether it is right to combat another one, we should not be too quick to treat it simply as a stupid or ignorant form of our own. If we resist this and allow ourselves to listen, we may discover that there are fewer mindsets that we want to combat.⁴⁶ I agree and invite the reader to consider whether my discussion of ‘hatchet jobs’ provides support for this view. Did Weinberg allow himself to listen to Derrida or did he assume from the start that Derrida was merely posturing? Did Nagel attempt to hear what Irigaray was saying before concluding that her mindset is merely a stupid or ignorant form of his own? And so on. If the hallmark of relativism is sensitivity to considerations of context, is it any wonder that hostility to it often keeps company with a resistance to looking for more generous readings of texts that initially put us off?

Transgressing those conventions

Anyone who believes that the laws of physics are merely social conventions is invited to try transgressing those conventions from the windows of my apartment. (I live on the twenty-first floor.)⁴⁷ (Sokal 1996)

Here Sokal is showing how much he can get away with. A social convention is one thing, a social construction another but “transgressing those constructions” is not funny. Neither Kant, whose view of reality is exactly the kind that Sokal detests, nor the other targets of Sokal’s ridicule think the laws of physics are social conventions.⁴⁸ Once this is understood, the joke is no longer funny. What is funny is the idea that knowledge of the relevant laws of physics has anything to do with why people and goats normally do not jump out of windows. It is dismaying that in an article allegedly devoted to assuring us of the seriousness of his purpose (Sokal 1996),⁴⁹ Sokal prefers being funny to getting it right. Instead of treating the absurdity of his idea of a social construction as evidence that it is not what social constructivists are talking about, he takes for granted that it is, from which it conveniently follows that social constructivists are merely posturing. If this is all that it takes to expose them, they clearly do not have good posture. Note also how little it takes for Sokal’s remark to stop being a joke and come within sight of addressing a genuine issue. He need only have asked how our normal fear of jumping from a height is consistent with the belief that ‘reality is a social construction.’

A good initial reply is that to say that reality is a social construction does not mean that we can do a damn thing to alter the reality that people normally are killed when they leap from too great a height. More precisely, we believe that this is the reality and our beliefs, together with our attitudes and desires, drive our behavior. Although this tells us only what ‘reality is a social construction’ does not mean, it serves to complicate the discussion in just the right way. If such talk now seems mysterious instead of ridiculous, great progress has been made. In the literature of science studies, talk about social construction is talk about scientific belief formation from a perspective from which rationality appears to have a less decisive role than classical epistemology says it should. To see rationality in this light, the strength of a belief must be rigorously differentiated from the strength of the support provided for it by logic and evidence, notwithstanding the influence of the latter on the former. For a simple illustration of what I mean, consider the following exchange between

a traditional realist and two social constructivists that appeared in *Physics Today* (January 1997).

Perhaps some sense of reality could be imparted to Collins and Pinch by putting them on jet planes that lack flaps or spoilers. (Belver Griffith.)

We agree that we prefer to fly on airplanes with flaps and spoilers, though we have never actually checked with the pilot and cabin crew before taking off. (Harry Collins and Trevor Pinch.)

Griffith thinks that Collins and Pinch wish to deny that facts matter but would admit that they do if their lives were at risk. Collins and Pinch counter that, even at 30,000 feet, all we have to go on are our beliefs about what the facts are and most of these are based on trust, not anything like rational inquiry. Even when our lives are at risk, we must and do rely on large doses of trust, including trust about what the relevant facts are.

I follow you follow me

On page 87 of *Fashionable Nonsense*, Sokal and Bricmont attribute to Barry Barnes and David Bloor a “radical redefinition of the concept of truth.” Here is the passage that they quote, followed by their misreading of it.

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.⁵⁰ 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)

For Sokal to regard the statement “I drank coffee this morning” as true is no more or less than for him to believe that he drank coffee this morning. So, the idea that Barnes and Bloor are offering “a radical redefinition of the concept of truth” must come from the talk about preferences. One does not have to be very clever to make such talk 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 that the relativist “will naturally have preferences,” they mean that his acceptance of a belief is not arbitrary⁵¹ and, when they say that these preferences “will typically coincide with those of others in their locality,” they mean that most cows agree about most things, especially those who read the *Daily Moos*, then it is not at all absurd. Sokal and Bricmont also 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.⁵² I follow you follow me.

A pledge of allegiance to truth

The mistake committed by Sokal and Bricmont is very common. A social constructivist statement that neither affirms nor denies that there is more to truth than consensus is mistaken for a denial. For another example, consider Thomas Nagel's attempt in *The Last Word* (1997: 28) to pin such a charge on Sabina Lovibond. He quotes her reference to

our lack of access to any distinction between those of our beliefs which are actually true, and those which are merely held true by us. No such distinction can survive our conscious recognition that some human authority has to decide the claim of any proposition to be regarded as true—and, accordingly, that the objective validity of an assertion or argument is always at the same time something of which human beings (those human beings who call it 'objectively valid') are subjectively persuaded.... Wittgenstein's conception of language incorporates a non-foundational epistemology which displays the notions of objectivity (sound judgement) and rationality (valid reasoning) as grounded in consensus—theoretical in the first instance, but ultimately practical.⁵³

Nagel's sole comment about this is

If one takes [such views] seriously, they turn out to be inconsistent with the very consensus on which they propose to "ground" objectivity. What human beings who form scientific or mathematical beliefs agree on is that these things are true, full stop, and would be true whether we agreed on them or not—and furthermore that what makes *that* true is not just that we agree to say it! (Nagel 1997: 29)

Apparently, Nagel expects us to see for ourselves how this queer remark, in which he barrels right through a full stop, refutes Lovibond. The trick, he tells us, is to take her seriously. I did and found that, even if we correct for the fact that Lovibond is talking about reason and judgment, not truth, her remark about objectivity "grounded in consensus" is consistent with his pledge of allegiance to what is true whether or not we agree that it is. "Grounded in consensus" is merely a convenient way of referring to a complex feature of Wittgenstein's conception of language. Where then is the alleged inconsistency? Maybe Nagel took Lovibond's first sentence to mean that there is nothing more to being true than being believed. That would do it but it would not be taking her views seriously. For a reading faithful to the rest of her remark, he should take it to say simply that we lack a God's eye view of our beliefs. Full stop. For the same reason, he should assume that when Lovibond wrote "No such distinction" she meant something like "No belief that there is access to such a distinction," in which case the remark up to the dash is a truism.

Nagel's last chance is Lovibond's carefully crafted remark about the objective validity of an assertion or argument: it is at the same time something of which those of us who call it 'objectively valid' are subjectively persuaded. Even without 'at the same time,' there is nothing here with which Nagel could disagree. Furthermore, if, as he believes, Lovibond denies that there is anything more to the objective validity of an assertion than our being subjectively persuaded of it, why does she add 'at the same time'? As I read her, she is being careful not to deny it but also not to affirm it. She also is reminding us of something that we are forever forgetting: that whenever we believe we have an objective justification,

we are still in the realm of unjustified belief.⁵⁴ So, yes, I do see an inconsistency. But it is between Nagel's reading of Lovibond and the one I get by taking her views seriously.

Reading Roger Anyon

Postmodernism, in seeking...to blur the distinction between [science] and "other ways of knowing"—myth and superstition, for example—needs to go much further than historicism, all the way to the denial that objective truth is a coherent aim that inquiry may have. Indeed, according to postmodernism, the very development and use of the rhetoric of objectivity...represents a mere play for power, a way of silencing these "other ways of knowing." (Boghossian 1998: 27)

This is part of Boghossian's reading of an account of postmodernism by the postmodernist scholar, Linda Nicholson (1990:3-4).⁵⁵ I believe that he has her badly wrong but I will not undertake to prove this here.⁵⁶ I wish instead to evaluate Boghossian's success in catching someone in the act of espousing the view he takes Nicholson to be describing—one whose ubiquity he tells us is "a distressingly familiar fact." He writes, "A front-page article in the *New York Times* of October 22, 1996, provided an illustration." The alleged espousal is attributed to Roger Anyon, "a British archaeologist who has worked for the Zuni people." It reads,

Science is just one of many ways of knowing the world...[The Zunis' worldview is] just as valid as the archaeological viewpoint of what prehistory is about.

Boghossian asks, "How are we to make sense of this?" (1998:27). The obvious and surely the best answer is, "Ask Anyon." Remarkably, the idea seems not to have occurred to him, perhaps because it would have been inconvenient to let Anyon have a say about what he meant. Boghossian contends that Anyon's 'just as valid' can be understood in three ways, none of which yields a remotely plausible assessment of the relative merits of the two views of prehistory.⁵⁷ He tries to justify this for 'valid' taken to mean either 'true' or 'justified.' But not only does he ignore the possibility that none of his interpretations is Anyon's,⁵⁸ when he comes to the third, which he explains in terms of serving symbolic, emotional and ritual purposes, he says,

The trouble with this as a reading of 'just as valid' is not so much that it's false but that it's irrelevant to the issue at hand: even if it were granted, it couldn't help advance the cause of postmodernism. For if the Zuni myth isn't taken to compete with the archaeological theory as a descriptively accurate account of prehistory, its existence has no prospect of casting any doubt on the objectivity of the account delivered by science. (1998: 29)

But the issue at hand is not the cause of postmodernism. It is whether Boghossian can fulfill his promise to show that this reading of 'just as valid' yields a postmodernist claim that is not remotely plausible. Not only does he not do this, he implies that it yields a claim that is both plausible and not interestingly postmodern. Perhaps Boghossian meant to say that, on two of his readings, the remark makes an implausible postmodernist claim and that, on the third, it is plausible but irrelevant to postmodernism. But if he has a way of making

sense of Anyon's remark on which it is plausible but irrelevant to postmodernism then if he has no evidence that it is not what Anyon meant, Anyon is off the hook. So Boghossian's attempt to show us one real-life espousal of a view that he says is ubiquitous fails. However, although it is fair to criticize him for this dreadful argument, I suspect that there is little that Anyon or many others in cultural studies could say that Boghossian would not hear as a denial of objectivity. So in this sense, he is right: for him, this view is ubiquitous.

Reasoning about relativism

A postmodernist dummy about truth

For his interpretation of 'just as valid' as 'just as true,' Boghossian, playing the ventriloquist to a postmodernist dummy, has the latter suggest that contradictory statements can both be true provided that each is true from a different perspective.

If I say that the earth is flat and you say that it's round, how could we both be right? Postmodernists like to respond to this sort of point by saying that both claims can be true because both are true relative to some perspective or other and there can be no question of truth outside of perspectives... But to say that some claim is true according to some perspective sounds simply like a fancy way of saying that someone, or some group, believes it. (1998: 28)

As a description of real postmodernists, this is fantasy. Boghossian offers no support for it.⁵⁹ But for the moment I will ignore this in order to focus on the logic of the argument he uses it to make. He begins by noting that what he says above implies that his dummy is committed to the view that anything is true provided only that someone believes it.⁶⁰ As if this were not bad enough, according to Boghossian, the dummy does not always honor her commitment to this view: she does not consider the view itself mistaken merely because somebody else thinks it is. Boghossian claims that this is not merely an inconsistency but that it reveals a commitment to truth "independent of particular perspectives." How it reveals this, he does not say—which I take to mean that he trusts the reader to see it.

Here is what I see. The dummy denies the claim, "If someone believes a statement, then it is true," by denying it for a particular statement. This lands her in a contradiction but I see no sign of a commitment to truth independent of particular perspectives (1998:28).⁶¹ However, the denial of a similar-looking claim, "If a statement is true, then someone believes it," does express such a commitment. So, my guess is that Boghossian mistook his dummy's denial of a claim of the form, 'If someone believes *S*, then it is true' for the denial of its converse.

However, Boghossian's discussion of this is maddeningly vague. I have relied on my own unconfident rendering of it. Here is the original.

If a claim and its opposite can be equally true, provided there is some perspective relative to which each is true, then since there is a perspective—realism—relative to which it's true that a claim and its opposite both cannot be true, postmodernism would have to admit that it itself is just as true as its opposite, realism. But postmodernism cannot afford to admit that: presumably, its whole point is that realism is false.⁶² Thus, we see that the very statement of postmodernism, construed as a view about truth, undermines itself: facts about truth independent of particular perspectives are presupposed by the view itself. (1998: 28)

I now am ready to explain my objection to “Postmodernists like to respond to this sort of point by saying that both claims can be true because both are true relative to some perspective.” According to postmodernism, a claim is not true because it is true relative to a perspective; it is true in the sense that it is true relative to a perspective. To see what a difference this makes, try to repeat Boghossian’s argument with ‘in the sense that’ in place of ‘because.’ You will not succeed. Also, if a postmodernist were to remark that “The earth is round” and “The earth is flat” can both be true, other postmodernists would probably understand it as a reminder that an apparent contradiction between statements may reflect a difference in the contexts in which their meanings have a home. Boghossian would do well to remember this too.

A postmodernist dummy about justification

For his interpretation of Anyon’s ‘valid’ as ‘justified,’ Boghossian again plays the ventriloquist to a postmodernist dummy. This one conflates the real postmodernist view that each justification we make is relative to a perspective with the nonsensical claim that a statement is justified if it is justified from some perspective. As in the case of postmodernist talk about truth, this conflation renders Boghossian’s argument worthless as a refutation against his real target. He also requires his dummy to hold that everyone is free to stipulate what counts as a rule of evidence, as if evidence need have nothing to do with inducing belief. This is used to show that the dummy is obliged to accept that the denial of her belief about justification is no less justified than the belief itself. But Boghossian thinks that she cannot accept this.

Presumably, however, the postmodernist needs to hold that his views are better than his opponents; otherwise, what’s to recommend them? By contrast, if some rules of evidence can be said to be better than others, then there must be some perspective-independent facts about what makes them better and a thoroughgoing relativism about justification is false. (1998: 29)

Thus in spite of her postmodernism about justification, the dummy is required to believe that some rules of evidence are better than others, from which Boghossian concludes that she is committed to the existence of perspective-independent facts. As in the case of truth, he offers no argument for this. Presumably he expects the reader not to need one. But this reader does. For one thing, he does not see how Boghossian can rule out the possibility that the dummy believes that there is a universal perspective from which some rules of evidence can be said to be better than others.⁶³ But even if Boghossian’s conclusion is unwarranted, he leaves us with a wonderful question. Forget about the dummy. Do you need to believe that you can give reasons for holding your positions that are good not only from these positions? What bad things will happen to you if you cannot?

This concludes my second series of case studies. In each of them, a believer in perspective-independence, that is, a realist, conflates agnosticism about it with atheism. In my final series of studies, I consider a confusion in realist thought that contributes to this conflation. Although it is not a necessary feature of realism, it is a predictable consequence of insensitivity to shifts in context—in this case, to shifts associated with the belief/knowledge distinction. So although it is not inevitable, neither is it a surprise.

Reality is hard to talk about

It appears to me that the ‘real’ is an intrinsically empty, meaningless category. (Albert Einstein)⁶⁴

In “Professor Sokal’s bad joke,” Stanley Fish (1996) described a sense in which both science and baseball are social constructions, yet no less real for it. The comparison was widely misunderstood. Soon Weinberg countered with his own idea of the sense in which the laws of physics are real. It too was misunderstood, even by Weinberg, as I show below.

The laws of physics are real...in pretty much the same sense (whatever that is) as the rocks in the fields, and not in the same sense (as implied by Fish 19) as the rules of baseball—we did not create the laws of physics or the rocks in the field, and we sometimes unhappily find that we have been wrong about them. (Weinberg 1996)

Contrary to what is suggested here, Fish is not comparing the laws of physics with the rules of baseball. His point is that the statements that we accept as laws of physics help govern its practice, just as the rules of baseball do for our national pastime. Fish is comparing baseball with physics as rule-governed activities. This is a ball. That is a strike. This experiment supports Weinberg’s theory, that one does not. As Weinberg notes, the statements that we accept as laws of physics are not stipulative of it.⁶⁵ But, as Fish explains in his essay, neither are the rules of baseball stipulative of baseball. If batters get too many hits, the strike zone might be shrunk or, to think the unthinkable, the rule might be changed to “Two strikes and you’re out.” And it would still be baseball. However, although there is nothing more to being a rule of baseball than being accepted as one by the relevant community, accepting a statement as a law of physics does not make it true. Nor does rejecting it make it false. To accept a statement as a law of physics is to believe that it is one. From inside the belief, there is no belief, only knowledge of how things are. By contrast, when we stand back and reflect on it, there is the belief but no believing it and, in a queer way, it may no longer seem to be our belief. Indeed, we usually allow that it may be false,⁶⁶ which, strictly speaking, is inconsistent with our believing that it is true.⁶⁷ However, when we stop reflecting on it, we no longer allow this. In the rest of the chapter, I will offer examples of how losing sight of how we oscillate between being inside a belief and standing back from it can lead us to make reasonable-looking statements about the nature of reality that on closer inspection turn out to be gibberish. I begin with Weinberg in the quote above.

Now you believe it

‘I always believe stories whilst they are being told,’ said the Cockroach. ‘You are a wise creature,’ said the Old Woman. ‘That is what stories are for. And after, we shall see what we shall see.’ (A. S. Byatt, *The Djinn in the Nightingale’s Eye*: 67)

For Weinberg, the laws of physics are real in the sense that sometimes we find that we have been mistaken about whether something is one. In each case, the words “we find” mark a report from inside our belief that we were mistaken: it is a discovery, not merely the adoption of a belief. Nevertheless, when we reflect upon it, we are likely to allow that it too may be mistaken. Indeed, we do sometimes find that we were mistaken about being

mistaken about whether something is a law of physics. However, although for Weinberg, it is human fallibility that makes the laws of physics real, this potentially infinite source of it should be no comfort to him. He notes that we believe that we cannot be mistaken about something if we believe that it holds by stipulation. But we also believe that we cannot be mistaken about something if we believe we have discovered it. True, even if reality bops us into a state of believing that we cannot be mistaken about something, it may then bop us out of it.⁶⁸ But so long as Weinberg remains inside a belief that he has discovered a law of physics, he believes also that he cannot be mistaken about it and hence that it is not, in his sense, a law of physics. I doubt that this is what he had in mind.

The correct answer, take one

The choice of scientific question and the method of approach may depend on all sorts of extrascientific influences, but the correct answer when we find it is what it is because that is the way the world is. (Weinberg 1996)

It seems that Weinberg is attempting to say that the correct answer to a scientific question does not depend on extrascientific factors. But this is a tautology. Anything that the correct answer to a scientific question depends on is, for that very reason, scientific even if it was not hitherto recognized as such. The second clause also is a tautology because to be the correct answer just means to state correctly the way the world is. And yet, so long as we do not look too closely, these tautologies seem to be saying something significant. Certainly, Weinberg seems to think that they are.⁶⁹ My guess is that this impression is created by the ideas that we associate with the expressions ‘extrascientific influences’ and ‘when we find it.’ But by the analysis of the preceding section, the dream of finding the correct answer to some scientific question has a damper. Whenever Weinberg dreams that he has found it, for that very reason, what he dreams he has found is unreal. If this is a disappointment, he can dream instead that he may be mistaken. But then he no longer is dreaming that his answer is correct. It’s a dilemma! But even worse, if to escape it, still dreaming or not, he steps back and reflects upon the whole of what from inside the relevant belief is his activity of scientific discovery, he will see only that sometimes he becomes convinced of something and at other times he becomes unconvinced. And contemplation of this barren view of his place in the world may drive him back to dreaming that he has discovered the correct answer to a scientific question.

Is this a fair reading of Weinberg’s remark? On the one hand, even if we ignore the loaded use of ‘when we find it,’ his reliance on fortune cookie philosophy⁷⁰ to lecture others on the correct way to talk about our relationship to reality merits scorn. On the other hand, if we wish to be forgiving, we can pretend that it never happened and take him to have merely meant to affirm the traditional realist credo: the truth-value of a scientific statement is independent of social factors.

The correct answer, take two

Weinberg is not alone. In *Fashionable Nonsense*, Sokal and Bricmont use the idea of ‘the correct answer to a scientific question’ to teach Latour how to talk properly about science and reality (1998: 92-99). Although Latour talks about settling scientific controversies

rather than finding correct answers (1987: 96-100), to say that a scientific controversy is settled means that there is an agreement that something is the correct answer to a scientific question. Seeing Latour seem to repeatedly insist that facts come into existence only by our recognizing them as such, Sokal and Bricmont accuse him of “playing constantly on the confusion between facts and our knowledge about them.” I disagree.⁷¹ However, here I wish only to consider what—apparently with straight faces—Sokal and Bricmont offer contra Latour as the correct answer to what is meant by ‘the correct answer to a scientific question.’

The correct answer to any scientific question, solved or not, depends on the state of Nature... 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). (1998: 97)

I believe that a careful reading will confirm that, as we pass from “we do know it” to “if the accepted solution is correct,” we shift from inside the belief that we know the solution to outside it, where we are able to allow that it may not be correct. Thus, the insight that Sokal and Bricmont seem to wish to share with us here—the one that they imply Latour, in his relativistic confusion, missed—is that, in science, when we believe we have solved a problem, we have, except that maybe we have not.

The infinite regress for evidence

In the previous cases, metaphysical nonsense was produced by failing to notice that one had moved outside a belief or back inside. Here, it is caused instead by failing to notice which relevant beliefs one has entered in the course of moving outside another one. On page 17 of *A House Built on Sand*, Sokal says that how he would evaluate the causes of a man’s claim should depend heavily on whether or not the claim is true.⁷² He then qualifies this, saying, “more precisely, since I admit that we have no direct, unmediated access to external reality,” it should depend on evidence for or against the claim. 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. The 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 there is for justification. In practice, for one reason or another, the regress stops. I doubt that it is often a matter of conscious choice. Where it does stop, for those like Sokal, who believe that talk about “direct unmediated access” to external reality means something, this *de facto* privileged level seems to become the practical equivalent of a realm to which we do have “direct unmediated access.” Maybe this is the source of the blunder.

Science and the study of science

There is a great and, to my mind, fascinating disagreement about the role that the truth of a scientific belief can, should or even must play in explaining why it is believed. In one

camp, which includes many scientists, there are those who find it obvious that in many cases both in science and ordinary life, we can and maybe even must appeal to the truth of a belief to help explain what causes someone to believe it.⁷³ Like Sokal in the preceding section, many in this camp consider it important to emphasize that to justify such an explanation, which should be thought of as a theory, one must appeal to the evidence for the belief, not to its truth. But they ignore the regress for evidence.

In another camp, in which constructivist sociologists of science figure prominently, there are those who say that such explanations are question begging, in part because of the regress. They try to position themselves outside of any belief whose acceptance or persistence they wish to explain. This is the ‘Strong Program’ for the sociology of science. For those who pursue it, the regress for evidence is a reminder that the power of evidence to induce belief varies with the state of mind of the jury. Evidence that convinces you may not convince me because a friend whom I trust warned me that it may be a con. The causal irrelevance of authenticity is another consideration. Because we learn to be good cup detectors, normally a belief that there is a cup on the table is pretty good evidence of its truth. Nevertheless, we understand, as a fact about our biology, that not only cups trigger our cup detectors. Any good enough imitation will do. Moreover, a good enough imitation need not be a particularly good approximation to the real thing. It need only be close enough in the realm of appearances. And the principle is perfectly general.

Those in the first camp think it is crazy to let such considerations dictate how we study knowledge acquisition. After rolling their eyes, they may explain, “Of course, we don’t really know that *S* is true. What the evidence really shows is that it is highly probable.” As if the claim that the evidence shows that *S* is highly probable were not subject to exactly the same concerns!⁷⁴ Yet it is they who insist upon the importance of distinguishing between what does convince people and what should convince them, which is precisely the distinction that grounds these concerns.⁷⁵ But they see no contradiction. Indeed, in “Science as a cultural construct” (*Nature*, 10 April 1997), physicists Kurt Gottfried and Kenneth O. Wilson couple severe criticism of the Strong Program with a defense of this apparent double standard. They contend that although the Strong Program cannot distinguish between objective knowledge and mere belief, they and other physicists can.⁷⁶ They consider it a scandal that the Strong Program ignores and, in their view, must ignore “the steadily improving predictions of twentieth-century science” (1997: 545). How do Gottfried and Wilson support these remarkable claims? On my reading, their whole justification consists of the following remark, supplemented by a reference to two illustrative examples.

Predictive power, the strongest evidence that the natural sciences have an objective grip on reality, is largely ignored by these commentators. For the question of whether scientific knowledge is contingent on culture, the discovery of phenomena that could not have been foreseen when a theory was invented but which are in accord with that theory are especially germane. (1997: 547)

So they would have us believe. But no matter what the Strong Program may or may not have been ignoring, there is nothing about even the most spectacular predictive success that puts it out of its reach. Not, at any rate, so long as considerations like the causal irrelevance of authenticity and the infinite regress for evidence retain their cogency. Furthermore, to say that a theory is unlikely to be “contingent on culture” if it holds for

phenomena that could not have been foreseen when it was invented is a *non-sequitor*. How a culture could have spawned scientists that invented such remarkable theories is indeed a mystery. But unless I am badly mistaken it is not one that need concern us here; it is a mystery about the origin of conjectures not the adoption of beliefs.

Elsewhere in their article, Gottfried and Wilson lecture their readers about the bad things that happen when accounts of scientific knowledge acquisition are written according to the Strong Program. These well-meaning admonishments are, in my view, vivid illustrations of what a world of difference it can make whether one remains inside a belief or stands back from it and takes a long look around before going back inside.

In 1925...and again in 1933, Dayton Miller announced a positive observation, which contradicts relativity, but was ignored. The authors of *The Golem* see this as proof that what matters is not the quality of an experiment but “what people are ready to believe” because “the culture of life in the physics community meant that Miller’s results were irrelevant.” When the word ‘culture’ is replaced by ‘facts,’ this sentence is no longer misleading, for in the decade following 1909 half a dozen independent laboratory experiments had confirmed the relativistic relation between velocity and momentum to better than 1 per cent.... (1997: 546)

To hear Gottfried and Wilson tell it, it goes without saying that everyone (except Dayton Miller) saw things as they do. Although they would never say, “It is a fact because we believe it,” they write as if “We believe it because it is a fact” is a satisfactory explanation of at least some cases of belief formation. The social constructivist authors of *The Golem* want to study what caused scientists to adopt the beliefs mentioned in the passage. If Gottfried and Wilson do not understand the relevance for this of what people are ready to believe, they understand nothing. Their factual claim about what half a dozen experiments confirmed may be mistaken. Thus their readiness to believe it is a fact about them, not about a world independent of them. In this respect, the authors of *The Golem* have it exactly right. However, readiness is one thing, believing another. It was reality, not a vote of the American Physical Society, that bopped a ready to be bopped physics community into believing the experiments.⁷⁷ How did reality do it? God only knows.

So. We may now perhaps to begin. Yes?⁷⁸

Bibliography

ADLER, MORTIMER (1940) *How to Read a Book*, New York: Simon and Schuster.

BARNES, BARRY and BLOOR, DAVID (1981) “Relativism, rationalism and the sociology of knowledge,” in Hollis, Martin and Lukes, Steven (eds.) *Rationality and Relativism*. Oxford: Blackwell.

- BELLER, MARA (1998) "The Sokal hoax: at whom are we laughing?" *Physics Today* September.
- BLACKBURN, SIMON "The sleep of reason produces monsters" (unpublished).
- BOGHOSSIAN, PAUL (1998) "What the Sokal hoax ought to teach us," in Koertge, Noretta (ed.) *A House Built on Sand: Exposing Postmodernist Myths about Science*, New York, Oxford University Press.
- BORK, ROBERT (1996) *Slouching Towards Gomorrah: Modern Liberalism and American Decline*, New York: Regan Books.
- DERRIDA, JACQUES (1970) "Structure, sign and play in the discourse of the human sciences," in Macksey, R. and Donato, E. (eds.) *The Structuralist Controversy*, Baltimore: Johns Hopkins University Press.
- FISH, STANLEY (1996) "Professor Sokal's bad joke," *New York Times*, May 21, A23.
- FREGE, GOTTLIEB (1982) *The Basic Laws of Arithmetic: Exposition of the System*, translated and edited with an introduction by Montgomery Furth, Berkeley, University of California Press.
- GALLO, ERNEST (1991) "Nature faking in the humanities," *Skeptical Inquirer* (Summer) 371-374.
- GOODE, ERICA (2000) "Among the inept, researchers discover ignorance is bliss," *New York Times* January 18.
- GOTTFRIED, KURT and WILSON, KENNETH O. (1997) "Science as a cultural construct," *Nature* 85(11), 545-547.
- GROSS, PAUL and LEVITT, NORMAN (1994) *Higher Superstition*, Baltimore: Johns Hopkins University Press.
- HARRIS, MICHAEL, "I know what you mean!" unpublished review of *Fashionable Nonsense*.
- HAYLES, N. KATHERINE (1992) "Gender coding in fluid mechanics: masculine channels and feminine flows," *differences: A Journal of Feminist Cultural Studies* 4(2), 16-44.
- KANT, IMMANUEL (1998) *Critique of Pure Reason*, translated and edited by Paul Guyer and Allen W. Wood, Cambridge, U.K. New York, Cambridge University Press.
- KOERTGE, NORETTA (ed.) (1998) *A House Built on Sand: Exposing Postmodernist Myths about Science*, New York: Oxford University Press.
- LATOUR, BRUNO (1987) *Science in Action*, Cambridge, Mass.: Harvard University Press.
- LATOUR, BRUNO (1988) "A relativistic account of Einstein's relativity," *Social Studies of Science*, 3-44.
- LEVINE, GEORGE (1996) "Sokal's hoax: an exchange," *New York Review of Books*, October 3, 54-56.

- MACKSEY, R. and DONATO, E. (eds.) (1970) *The Structuralist Controversy*, Baltimore, Johns Hopkins University Press.
- LOVIBOND, SABINA (1983) *Realism and Imagination in Ethics*, Oxford, UK: B. Blackwell.
- NAGEL, THOMAS (1997) *The Last Word*, New York: Oxford University Press.
- NAGEL, THOMAS (1998) review of *Fashionable Nonsense* in *The New Republic*, October 12, 32-38.
- NICHOLSON, LINDA J. (ed.) (1990) *Feminism/Postmodernism*, New York: Routledge.
- NICHOLSON, LINDA J. and SEIDMAN, STEVE (eds.) (1995) *Social Postmodernism*, Cambridge, New York: Cambridge University Press.
- NORRIS, CHRISTOPHER (1996) *Deconstruction: Theory and Practice*, revised edn, London, New York: Routledge.
- NORRIS, CHRISTOPHER (1997) *Against Relativism*, Oxford, Malden, Mass.: Blackwell.
- PLATO (1994) *Gorgias*, translated by Robin Waterfield, New York: Oxford University Press.
- Putnam, Hilary (1992) *Renewing Philosophy* Cambridge Mass.: Harvard University Press (paperback edition).
- SOKAL, ALAN (1996) "A physicist experiments with cultural studies," *Lingua Franca* 6(4) (May/June), 62-64.
- SOKAL, ALAN (1998) "What the Social Text affair does and does not prove," in Koertge, Noretta (ed.) *A House Built on Sand: Exposing Postmodernist Myths about Science*, New York: Oxford University Press.
- SOKAL, ALAN and BRICMONT, JEAN (1998) *Fashionable Nonsense*, New York: Picador USA.
- SULLIVAN, PHILIP A. (1998) "An engineer dissects two case studies: Hayles on fluid mechanics and MacKenzie on statistics," in Koertge, Noretta (ed.) *A House Built on Sand: Exposing Postmodernist Myths about Science*, New York: Oxford University Press.
- WEINBERG, STEVEN (1996a) "Sokal's hoax," *New York Review of Books*, August 8, 11-15.
- WEINBERG, STEVEN (1996b) "Sokal's hoax: an exchange," *New York Review of Books*, October 3, 54-56.
- WITTGENSTEIN, LUDWIG (1968) *Philosophical Investigations*, 3rd edn, translated by G. E. M. Anscombe, New York: Macmillan.
- WITTGENSTEIN, LUDWIG (1972) *On Certainty*, edited by G. E. M. Anscombe and G. H. von Wright, translated by Denis Paul and G. E. M. Anscombe, New York: Harper and Row.

¹ Supporting materials are posted at the web site <http://math.bu.edu/people/nk/rr>.

² I thank Phil Baringer, Susan Carey and Nancy Kopell for a wealth of constructive criticism that it was a pleasure to receive. And I thank Keith Ashman for his patience and understanding.

³ They say, “The Einsteinian constant is, of course, the speed of light in *vacuo*.” (Gross and Levitt 1994: 79). Of course? Also, the surface contradiction in “The Einsteinian constant is not a constant” is *prima facie* evidence that the two senses of “constant” are different. E.g., if one is numerical, the other is not. The authors need to prove that they are the same but they do not even attempt to do this.

⁴ See “What this, Polus?” below.

⁵ See “A physicist experiments with scholarly discourse” at the web site. Below see, “I am not a reference frame” and “The correct answer, take two.”

⁶ For a trivial but telling example, Boghossian is so confident that Sokal’s “But this framework is grossly insufficient for a liberatory mathematics, as was proven long ago by Cohen 1966” is not a pun that he bases one of his main arguments on it. Nevertheless, it is a pun on Cohen’s proof of the independence of the axiom of choice. Although the argument is easily repaired and Boghossian privately admitted the mistake after his essay first appeared, it appears again, unacknowledged and uncorrected, in Koertge’s volume. See note 12.

⁷ In one, on the evidence of a sentence stripped of its context and first clause, Paul Gross (p.104) concludes that Andrew Ross tried to deceive readers into thinking that Gerald Holton is the author of a famous apocalyptic remark by his *bête noir*, Oswald Spengler! See “Selective Quotation,” at the web site. A second example is discussed in “Masculine channels and feminine flows” below.

⁸ See “The hoax according to Weinberg” at the web site. Here see “The oracle of deconstruction,” “Now you believe it” and “The correct answer, take one” below.

⁹ A skeptical reading is a suspension of trust, a refusal to cut slack. We do not normally read this way.

¹⁰ There are many appalling examples but, to my mind, nothing beats the philosopher Thomas Nagel’s “The sleep of reason” (1998), a review of *Fashionable Nonsense* by Sokal and Bricmont (1998). The first part reads as if the reviewer was taking dictation. See “Professor Nagel’s *Fashionable Nonsense*” at the web site and “What’s this, Polus?” below.

¹¹ A generous reading is not necessarily a more accurate one. But in these cases, it is.

¹² This is the main brief of Paul Boghossian’s chapter in *A House Built on Sand*. Like Steven Weinberg’s “Sokal’s hoax,” on a non-skeptical reading, Boghossian’s chapter is clear and convincing. But, on a skeptical one, the reasoning is sloppy and the interpretation of the evidence lacks credibility. See “Reading Roger Anyon” and “Reasoning about relativism” below and, at the web site, “An unphilosophical argument.”

¹³ Or at least our beliefs about it.

¹⁴ For views of which these are parodies, see “Transgressing those conventions” and “Science and the study of science” below.

¹⁵ Almost always, those who call themselves or are called relativists or constructivists are agnostics not deniers, but the conventional wisdom is that they are deniers. I use the term ‘relativist’ here only for these agnostics—‘good’ relativists. But some agnostics (who are called relativists/deniers by others) use it only for deniers---‘bad’ relativists. *Caveat lector*.

¹⁶ See e.g., *Philosophical Investigations, On Certainty and Remarks on the Foundations of Mathematics*.

¹⁷ Victor Brudney’s exhortation to beginning law students about how to read a legal text. He is alluding to *How to Read a Book* (1940: 14), where the author, Mortimer Adler, writes, “When [men and women] are in love and are reading a love letter, they read for all they are worth. They read every word three ways; they read between the lines and in the margins; they grow sensitive to context and ambiguity... Then, if never before or after, they read.”

¹⁸ “Yet the effect of a hatchet job like this, if it succeeds [is a] shift in the climate of opinion... outsiders who have grumbled privately to one another for years have something concrete to which they can point” (1998: 34).

¹⁹ See “What’s this, Polus?” below.

²⁰ In my unscientific poll, three physicists and a physics teacher disagreed with Nagel’s claim that special relativity is beyond the intuitive grasp of a layman. One physicist agreed with it.

²¹ He and I read Latour differently. My Latour understands relativity much better than his does.

²² Trust me.

²³ In fact, it is gibberish offered by a layman to laymen as evidence that a layman cannot intuitively grasp Einstein’s theory, even roughly! Yet for a similar claim about quantum theory, which is far stranger than special relativity, Nagel uses an analogy to weather prediction that one physicist awarded an ‘A.’

²⁴ I thank David Mermin for calling this to my attention.

²⁵ For our purposes, it suffices to note that they offer no support for this implausible idea, a consideration that does not seem to trouble Nagel. Also, like Sokal mocking Derrida on the radio, Sokal and Bricmont are silent about what, if anything, their “comically patient” (I would say “hare-brained”) remark has to do with Irigaray’s speculation about gender coding. Whatever their intention, the effect is to treat Irigaray as an idiot and encourage their readers to do the same. Indeed, this is exactly the word Nagel uses to describe her elsewhere in his review. See “Professor Nagel’s *Fashionable Nonsense*” at the web site.

²⁶ “Among the inept, researchers discover ignorance is bliss,” Erica Goode, *New York Times*, January 18, 2000, D7.

²⁷ The ellipses are Sokal’s.

²⁸ Sokal seems oblivious to the implausibility of anyone confusing these two things.

²⁹ See “Reading Latour reading Einstein” in “A physicist experiments with scholarly discourse,” at the web site.

³⁰ Also, how can Sokal square talk about an observer being delegated to an electron with his unargued claim that, for Latour, an observer has to be human? In fact, for Latour, any recording device is an observer—even a stick that records a fire by being consumed.

³¹ Hayles seems unsure whether she wants to talk about fluids in general or only nearly incompressible ones, like water.

³² This is why God said, “Let there be editors.” What went wrong here?

³³ For Frege, see, for example, *The Basic Laws of Arithmetic*, (1893: xix). I thank Warren Goldfarb for this reference.

³⁴ Executing a mathematical rule requires both computation and observation.

³⁵ I assume she means “with the flow.”

³⁶ In its interior? Every undergraduate physics student should know that, in many cases, the center of mass of a body is not in its interior. Imagine if Hayles had made this slip, not Sullivan.

³⁷ Does he seriously think that Hayles doesn’t know this?

³⁸ So far as I know, the first attempt to brand Derrida a physics faker was made by Ernest Gallo (1991). Gallo was read by Gross and Levitt, who inspired Sokal, who made the ‘Einsteinian constant’ remark a centerpiece of his hoax, which brings us to Weinberg, whose efforts I discuss here. For a debunking of Gallo’s reading of the remark and also of more recent ridicule of it in *Fashionable Nonsense* and the second edition of *Higher Superstition*, see “The invention of Jacques Derrida, physics faker,” at the web site.

³⁹ Levine’s objection and Weinberg’s reply are in “Sokal’s hoax: An exchange,” *New York Review of Books*, October 3, 1996, pp.54-56.

-
- ⁴⁰ Consider, for example, Hyppolite's use of "It" in the exchange, "It is a constant in the game? It is *the* constant in the game." (Derrida 1970: 267)
- ⁴¹ For example, "Successively, and in a regulated fashion, the center receives different forms or names. The history of metaphysics, like the history of the West, is the history of these metaphors and metonymies." (Derrida 1970: 249)
- ⁴² This is an unpublished review of *Fashionable Nonsense*.
- ⁴³ This is part of a response to a remark by Andrew Ross that Sokal first correctly calls "epistemological agnosticism" but then misrepresents as a denial of objectivity. See (Sokal and Bricmont 1998: 274).
- ⁴⁴ See, for example, Paul Boghossian (Koertge 1998: 30) on stipulation in baseball. For my critique of this, see "An unphilosophical argument," at the web site.
- ⁴⁵ This is my reading of part of Putnam's discussion of *On Certainty* §§608-612 and Wittgenstein's notes on Frazer's *Golden Bough*. I use 'mindset' where Putnam, following Wittgenstein, says 'language game.'
- ⁴⁶ On my unscholarly reading, §612 also acknowledges that, even for us, our reasons go only so far.
- ⁴⁷ Merely social conventions? Tell it to the women of Afghanistan.
- ⁴⁸ In his unpublished essay, "The sleep of reason produces monsters," the philosopher, Simon Blackburn, notes that Kant was not in the habit of jumping out of windows.
- ⁴⁹ This is the article in which Sokal revealed his hoax.
- ⁵⁰ In a footnote, Sokal and Bricmont admit that Barnes and Bloor may merely be noting that what people believe they label 'true.' But they manage to make even this into a complaint!
- ⁵¹ It makes sense that they do. Their project is to explain, in cases of interest, what makes people believe one thing rather than another.
- ⁵² However, there is some truth to this. Even if Einstein did not follow any other scientist, which I doubt, plenty of them followed him.
- ⁵³ (Lovibond 1983: 37, 40)
- ⁵⁴ I am referring to the regress for justification. A belief that we have an objective justification of something itself stands in need of objective justification. If we provide what we believe is one, then that belief stands in need of objective justification. And so on. See also "The infinite regress for evidence," below.
- ⁵⁵ Boghossian seems unaware that Nicholson is a postmodernist. Why else would he say that she gives her description of postmodernism, on which he relies, "without necessarily endorsing" it?
- ⁵⁶ Nicholson does not say that postmodernism seeks to blur distinctions or to either affirm or deny that objective truth is a coherent aim of inquiry. She says that postmodernists think that the criteria for distinguishing true from false and science from myth are "internal to the traditions of modernity." See Nicholson's contributions to *Feminism/Postmodernism* (Nicholson 1990) and *Social Postmodernism* (Nicholson and Seidman 1995). See also note 59 and, at the web site, "An unphilosophical argument."
- ⁵⁷ However, the question is not only whether what Anyon said is implausible but whether it is postmodernist.
- ⁵⁸ My guess is that Anyon thinks in terms of coherence not correspondence—assessing a belief in terms of how well it serves within a particular worldview to help explain whatever those who hold it need explained, and that he does not find either belief about prehistory to be superior in this respect.
- ⁵⁹ I have already noted that it can be difficult to distinguish 'good' relativism (non-affirmation of objectivity) from 'bad' (denial of objectivity), offering as an example Putnam's difficulty doing this for Wittgenstein. Yet Boghossian does not even attempt to make the distinction. How then can he defend his reading of how postmodernists like to respond? Does he think Nicholson—a

postmodernist he seems to respect—likes to respond this way? I doubt it. Yet on his reading of her description of postmodernism, she does.

⁶⁰ The ‘because’ does all the work here. A claim is true because it is true from some perspective, that is, because some person or group believes it. Hence, everything anyone believes is true!

⁶¹ Indeed, it is compatible with the claim that a statement is true if and only if the dummy believes it.

⁶² A real postmodernist could have a good time deconstructing “presumably.” Also, note the crucial role played here by an alleged denial of objectivity: “Its whole point is that realism is false.”

⁶³ Such a belief does not seem to imply a commitment to perspective-independent justification, except in the sense that everything can be seen that way. This surely is not Boghossian’s claim.

⁶⁴ Quoted in Beller (1998: 31).

⁶⁵ Derrida puts this nicely when, on a reading I find plausible, Hyppolite calls Lorentz invariance “the rule of the game” and Derrida replies, “It is a rule of the game that does not govern the game... When the rule of the game is displaced by the game itself, we must find something other than the word rule.” (1970: 267)

⁶⁶ Such skepticism may be more difficult in some cases than in others.

⁶⁷ When people grant that a statement they believe may be false, they sometimes try to remove the appearance of a contradiction by saying that they really believe only that it is highly probable. However, when they reflect upon the claim that it is highly probable, they usually grant that it too may be false.

⁶⁸ Reality makes a rat expect a food pellet that it may or may not get and it makes people believe, rightly or not, that there is chaos in the solar system or a ghost in the closet. Reality is always bopping us into different states of mind and even into forming new states of mind into which we may then be bopped. But it operates by selection, not instruction. It cannot teach a chicken Chinese until the chicken is ready to learn it. Which state we go into depends not only on the bop but also on particulars of our cognitive structure and the state we currently are in.

⁶⁹ They are his rejoinder to a remark by Sandra Harding.

⁷⁰ “The correct answer is what it is because that is the way the world is.”

⁷¹ I say why at the web site in “A physicist experiments with scholarly discourse.”

⁷² The claim is that there is a stampeding herd of elephants in a nearby lecture room! Sokal’s discussion of it is repeated almost verbatim on page 91 of *Fashionable Nonsense*.

⁷³ E.g., I might explain why I believe there is a cup on the table by saying that there is one there and I see it. I might also say that part of the reason contemporary physicists believe that Newton’s laws of motion are true to within a relativistic correction is that, because they are, whenever competent scientists tried to confirm this, eventually they succeeded.

⁷⁴ See note 67 and the preceding section.

⁷⁵ The regress for evidence stops when we are convinced but, logically, its existence implies that we never should be convinced. Similarly, the causal irrelevance of authenticity implies that, whenever we are convinced, we should not be.

⁷⁶ This looks like a substantive claim. But if we insert “what Gottfried and Wilson believe is” before “objective knowledge” and “that something is objective knowledge” after “mere belief,” then Gottfried and Wilson seem to be saying that, although the Strong Program cannot distinguish between Bugs Bunny and a wabbit, they and other physicists can. Yet for everyone except them and maybe even them, the insertions do not alter the meaning.

⁷⁷ See note 68.

⁷⁸ Dr. Spielvogel to Portnoy in the last line of *Portnoy’s Complaint* by Philip Roth.
