The Hoax According to Weinberg[1]

Gabriel Stolzenberg

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In "Reading and relativism," I said that, on a normal reading, Steven Weinberg's, "Sokal's hoax," (*New York Review*, August 8, 1996) is clear and convincing but, on a skeptical one, it can be seen to be riddled with confusion. I also gave three examples of this phenomenon.[2] Here I offer more—more than enough, I believe, to justify my use of the term "riddled."

A Change of Heart?

> After Sokal exposed his hoax, one of the editors of *Social Text* even speculated that "Sokal's parody was nothing of the sort, and that his admission represented a change of heart, or a folding of his intellectual resolve." I am reminded of the case of the American spiritualist Margaret Fox. When she confessed in 1888 that her career of seances and spirit rappings had all been a hoax, other spiritualists claimed that it was her confession that was dishonest.

Weinberg is quoting here from the *Social Text* editors' reply to Sokal's announcement of his hoax.[3] However, because the editors do not mention how long the alleged speculation was entertained, he has no warrant to assume, as he implicitly does by his allusion to followers of Margaret Fox, that it was long enough to justify mocking the editor for having entertained it. For all he knows, it was merely an initial, perhaps even momentary, reaction to news of

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[1]Updated March 15, 2004
[2] "The oracle of deconstruction" (42), "Now you believe it" (54), "The correct answer, take one" (55).
[3] "Mystery Science Theater" by *Social Text* editors, Bruce Robbins and Andrew Ross, *Lingua Franca* July/August, 1996.

Sokal's disturbing claim. Weinberg also shows no curiosity about why the editors of *Social Text* would appear to volunteer information that many would and did consider damning at the beginning of an article devoted to defending themselves against Sokal's attack. Isn't this a reason for him to pause and consider carefully whether his take on what they are saying is accurate? Finally, if, as I strongly suspect, Weinberg grants that people do sometimes recant their views under pressure[4], then he owes us an explanation of why it would have been unreasonable for the editor, upon hearing of Sokal's claim but not yet knowing any of the particulars, to consider the possibility that it was a case of this kind.

Physics and Mathematics

In his description of Sokal's parody, Weinberg mentions alleged howlers about physics and mathematics that were committed either intentionally by Sokal or unwittingly by others in statements he quoted and pretended to admire.[5] Below I consider three of the first kind and two of the second.

What is a noncommuting operator? In the following passage, Weinberg pays a price for failing to meticulously distinguish between singular and plural. It leads him to see a physics blooper that isn't there and miss one that is.

> In arguing for the cultural importance of the quantum theory of gravitation, Sokal refers to the gravitational field in this theory as "a noncommuting (and hence nonlinear) operator." Here "hence" is ridiculous; "noncommuting" does not imply "nonlinear," and in fact quantum mechanics deals with things that are both noncommuting and linear.

The term 'linear' applies to an individual operator but 'noncommuting' applies only to a set of two or more of them. Weinberg's seemingly innocuous use of the word 'things' blurs the distinction between singular and plural in a way that causes him both to talk nonsense and fail to notice the blooper that *is* present in Sokal's remark. Contrary to what he says, it is not "hence" but the expression, "a noncommuting operator." Because there is no such thing as *a* noncommuting operator.

Is the historicity of pi ineluctable? A little later, he writes:

> Sokal solemnly pronounces that "the pi of Euclid and the G of Newton, formerly thought to be constant and universal, are now perceived in their ineluctable historicity." This is absurd—the meaning of a mathematical quantity like pi cannot be affected by discoveries in physics, and in any case both pi and G continue to appear as universal constants in the equations of general relativity.

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[4] E.g., blackmail or a threat of being shunned.
[5] According to Weinberg, much of Sokal's account of developments in physics "was quite accurate but it was heavily adulterated with howlers, most of which would have been detected by any undergraduate physics major."

The *meaning* of a mathematical quantity? Mathematical quantities no more have meanings than do chairs or potatoes.[6] Maybe Weinberg meant to say "nature." I will assume that he did but this does not explain why he talks about *discoveries* in physics. There is nothing in Sokal's statement about discoveries in physics. It talks of an alleged change in our *perception* of pi and G but says nothing of how it came about. Does the fact that pi and G continue to appear in equations as universal constants *necessarily* conflict with their being perceived in their "ineluctable historicity"? I don't see why, *if only because this is so vague.* Although Weinberg was very quick to dismiss Derrida's "the Einsteinian constant is not a constant, not a center" as jibber jabber, he seems unfazed by "the pi of Euclid and the G of Newton…are now perceived in their ineluctable historicity." Yes, he finds it false and ridiculous but he does not find it meaningless or even ambiguous.

But how can he discern a *unique* meaning here?[7] In my dictionary, to perceive something in its "ineluctable historicity" is to perceive it in its "unavoidable historical actuality," which is vague enough to allow for different readings of Sokal's sentence, none of them especially compelling. For example, it might entail "recognizing" that each culture has to decide anew what if anything mathematics is about. Recall that on a Platonist conception of mathematics, pi and everything else is a set, whereas on a constructivist one, it is a rule.[8] For those who take their ontology seriously, these are very different things. (See, for example, *Realistic Rationalism* by the analytic philosopher, Jerrold Katz.) True, Sokal's sentence begins *as if* it is going to say that our belief in the constancy of pi is mistaken. But, perhaps out of fear that the editors might wake up or because he found the word "ineluctable" ineluctable, Sokal opted for "postmodernist" obfuscation. How then can Weinberg presume to understand the sentence well enough to fault others for not interpreting it as he does? Did he even look for a different interpretation?

Addendum: Perhaps because Weinberg knew that he was reading *a parody*, when he saw *the absurdity* to which the first part of the sentence seemed to be heading, it was natural for him to treat it as *the meaning* of the otherwise obscure second part. But the editors didn't know they were reading a parody. They believed they were considering a submission by a professor of physics at New York University, written in a self-consciously "postmodernist" style that they had reluctantly agreed to tolerate, containing nothing that he would not say to his own colleagues.[9] In such circumstances, the editors should *not* have read Sokal's sentence the way Weinberg felt free to do. For them, charity required that the reading on which it ends in "postmodernist" obfuscation trump that on which a professor of physics at a reputable university is saying something idiotic about high school science or mathematics.[10]

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[6] *Words* have meanings but I defy the reader to interpret Sokal's statement as being about words or other signs.

[7] If it is not unique, how can he fault the editors of *Social Text* for not adopting it?

[8] Furthermore, before mathematics was rigorized in the late 19th century, whatever mathematical objects were thought to be, they were not sets!

[9] "Sokal seemed resistant to any revisions, and indeed insisted on retaining almost all of his footnotes and bibliographic apparatus *on the grounds that his peers, in science, expected extensive documentation of this sort.*" (Italics added) Editors Bruce Robbins and Andrew Ross, "Mystery Science Theater," *Lingua Franca*, July/August 1996.)

[10] By ignoring the first reading, Weinberg missed the opportunity to criticize the editors for tolerating this choice example of "postmodernist" obfuscation.

'Complex number theory'? A little later, Weinberg says, about one of Sokal's footnotes:

> [Sokal] refers to complex number theory as a "new and still quite speculative branch of mathematical physics," while in fact it is nineteenth century mathematics and as well established as anything ever gets.

Contrary to this assertion, the expression 'complex number theory' does not name any nineteenth century or, for that matter, any other kind of mathematics. That Weinberg thinks it does shows that he is not a mathematician.[11] For the area of mathematics that he has in mind, mathematicians have several different names. The closest to 'complex number theory' is 'complex function theory.'[12] But saying that complex *function* theory is a "new and still quite speculative branch of mathematical physics" was not an option for Sokal. Although Weinberg does not seem to have noticed, Sokal was mimicking Robert Markley. In a statement quoted in the parody, Markley has 'complex *number* theory' where the context seems to call for either 'complex *systems* theory' or 'complexity theory,' both of which *do* name a new and still quite speculative branch of mathematical physics. None of this seems to have registered with Weinberg.

In the next two examples, Weinberg considers "physics and mathematics bloopers in remarks of others that Sokal quotes with sly mock approval."[13]

Weinberg criticizes Latour for agreeing with him. In "Reading and relativism," I pointed out two misreadings by Sokal of an essay by Bruno Latour about a book by Einstein. The book is an informal account of relativity theory for a lay reader. Here is a similar misreading by Weinberg of a passage from the same essay by Latour.

> How can one decide whether an observation made in a train about the behavior of a falling stone can be made to coincide with the observation of the same falling stone from the embankment? If there are only one, or even two, frames of reference, no solution can be found…Einstein's solution is to consider three actors… (Latour, as quoted by Sokal)
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> > This is wrong: in relativity theory there is no difficulty in comparing the results of two, three, or any number of observers. (Weinberg)

According to Weinberg, Latour is mistaken in saying that, in relativity theory, there is a difficulty in comparing observations made in different frames of reference. But Latour does not say that, *in relativity theory*, "no solution can be found." On the contrary, relativity theory *provides* the solution.[14] It tells us *how* to compare observations made in different

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[11] When my mathematician wife heard me utter the words, "complex number theory," she immediately called out, "There is no such thing."

[12] One colleague who heard me say "complex number theory" confessed later that he thought I had said, "complex function theory."

[13] The preceding example also is of this type. In it, Sokal gives his "sly mock approval" of Markley's mistake by mimicking it.

[14] "Einstein's solution" is a solution *within* relativity theory.

frames. What then is Latour's point? On my reading, he simply is noting that *the frames alone* are not enough. In a passage that Weinberg omits, he explains:

> If there are only one, or even two, frames of reference, no solution can be found since the man in the train claims he observes a straight line and the man on the embankment a parabola. Thus nothing tells us if it is the same stone acting according to the same law of physics. Each observer has 'its'…own irreducible vision of the world.

This is the sense in which the frames of reference are not enough. We also need the right transformations from each of them to a common frame. Relativity theory tells us that these are the Lorentz transformations. Thus, Latour and Weinberg agree that, *in relativity theory*, there is no difficulty in making such comparisons.

But if so, what is this third actor? Physicists never talk about a "third actor." On my reading, Latour's observers can make observations (and maybe transmit data) but it is not necessarily part of their "job description" to make comparisons. The third actor is a person or device that can use the Lorentz transformations to do this. But it needs a frame into which data received from the two observers is translated. Must this frame be different from the first two? No, nor does Latour ever suggest such a thing. On the contrary, elsewhere in his essay, he emphasizes that there are no "privileged" frames. In principle, any frame will do. In particular, either of the first two frames can be used.[15] Latour takes explicit note of this.

> If…the man in the train describes scenes according to instruments, which, after a few transformations, are made equivalent to the ones seen by the man on the embankment, this means that the latter will gain something. Without being on the train, the man on the embankment will have 'its' point of view *plus* another one compatible and addable to the first.[16]

When is a boundary not a boundary? Sokal's article includes the following quotation from a work by Luce Irigaray.

> The mathematical sciences, in the theory of sets, concern themselves with closed and open spaces… They concern themselves very little with the question of the partially open, with sets that are not clearly delineated [*ensembles flous*], with any analysis of the problem of borders [*bords*]…[17]   (Irigaray)

According to Weinberg, this merely reveals Irigaray's ignorance:

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[15] I suspect that one reason Latour's talk of a third actor was misunderstood is that, whereas physicists tend to fixate on the point that we *do not need* to use a third frame to make the comparison, Latour fixates instead on the point that we *are not forced* to use one of the first two.

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

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

> Luce Irigaray deplores mathematicians neglect of spaces with boundaries, though there is a huge literature on the subject. (Weinberg)

As a reply to Irigaray, this is risible. There is indeed an immense literature of the kind that Weinberg mentions but little if any of it promises to be of any use in studying "the problem of borders" for "sets that are not clearly delineated," i.e., for *fuzzy sets* or *vague predicates*. What do the boundaries of algebraic, differential or point-set topology have to do with boundaries of vague predicates, e.g., color names? It seems that Weinberg failed to grasp the relevance of Irigaray's mention of fuzzy sets for understanding *what kind* of borders she is most reasonably taken to be talking about.[18] Indeed, it is precisely the fuzziness of a fuzzy set—the vagueness of a vague predicate—that makes its border problematic.

Reality

On my reading, throughout his essay, Weinberg opposes the idea, which he thinks he sees expressed in the writings of others, that what we call scientific knowledge is "no better" than any other belief. I think he is seriously confused about this. I also that many of the particular confusions that I point out below are reflections of this general one. But I will not try to argue the general point here.

On a wing and a prayer

> I have come to think that the laws of physics are real because my experience with the laws of physics does not seem to me to be very different in any fundamental way from my experience with rocks. For those who have not lived with the laws of physics, I can offer the obvious argument that the laws of physics as we know them work, and there is no other known way of looking at nature that works in anything like the same sense.

If one believes that a statement is a law of physics, what does it add to say that it is "real"? To put it another way, what is the difference between saying "I believe the law of the lever" and "I believe that the law of the lever is real"? I don't see any, except that the second statement is not a normal way of talking. So, on one reading, Weinberg is merely saying, albeit in a queer way, that he believes the laws of physics. And, on this reading, he goes on to offer, in support of this belief, the "obvious" argument that these laws work, in the unique, extraordinary way that they do. But one can agree with him about this last part without buying into his metaphysics.

Or can one? Is this really all there is to Weinberg's queer talk of the laws being "real"? I am not so sure. It continues as follows:

> Sarah Franklin (in an article in the same issue of *Social Text* as Sokal's hoax) challenges an argument of Richard Dawkins, that in relying on the working of airplanes we show our acceptance of the working of the laws of nature, remarking that some airlines show prayer films during take-off to invoke the aid of Allah to

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[18] How likely is it that she would mention the 'problem of borders' right after mentioning fuzzy sets and not mean *that* problem of borders? At the very least, Weinberg has no warrant to dismiss this reading.

remain safely airborne. Does Franklin think that Dawkins' argument does not apply to her? If so, would she be willing to give up the use of the laws of physics in designing aircraft, and rely on prayers instead?

But even if Franklin should happen to believe that Dawkins' claim applies to her, this has no bearing on the question of whether there are people to whom it does not apply.[19] Why then does Weinberg respond to her suggestion that there are by challenging Franklin about *her* beliefs? Is this merely a logical lapse? I see no way of ruling it out. More important, in his challenge to Franklin, Weinberg seems to confuse ignorance with indifference. Take my own case. I would be lying if I said that Dawkins' wisecrack applies to me because I know nearly nothing about how airplanes are constructed. But it doesn't follow that I don't care or that I believe that any way of constructing them is as good as any other.

Werner's wanderings

Sokal quotes some dreadful examples of Werner Heisenberg's philosophical wanderings, as for instance: "Science no longer confronts nature as an objective observer, but sees itself as an actor in this interplay between man and nature." (Heisenberg was on the of the great physicists of the twentieth century, but he could not always be counted on to think carefully, as shown by his technical mistakes in the German nuclear weapons program.)

Weinberg finds Heisenberg's remark dreadful. But, because he doesn't give any reason for this opinion, for all we know, he has none. Actually, we don't even know what Weinberg thinks Heisenberg *means* by the remark—in particular, by saying that science "sees itself as an actor." I don't think I know what this means. Why should I assume that Weinberg does?

Then there is the parenthetical *ad hominum*,[20] in which the innocent-looking expression "not always" masks the looniness of linking Heisenberg's metaphysical remark to his allegedly careless (rather than deliberate) mistakes in certain wartime calculations in applied physics. Perhaps it is just carelessness, but Weinberg writes as if he would have us believe that some people can *always* be counted on to think carefully and, also, that if anybody (Heisenberg, Weinberg, Stolzenberg, it doesn't matter who) *once* in his life failed to think carefully, this is evidence that he is the kind of person who is likely to do it again.[21] This is what I mean by the looniness.

To put it simply

[Sokal's] targets often take positions that seem to me (and I gather to Sokal) to make no sense of there is an objective reality. To put it simply, if scientists are talking about something real, then what they say is true or false. If it is true, then how can it depend on the social context of the scientist? If it is false, how can it help to liberate us?

Even if there is an objective reality, it does not follow that scientists are talking about it, no matter how much they may hope they are. Secondly, one crucial way in which the truth of a scientific statement depends on the social context of the scientist is that it depends upon the

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[19] If the Pope were to remark that *not everyone* is Catholic, would it make sense to reply by asking whether the Pope believes that *he* is not Catholic?
[20] The bit after the 'but.'
[21] And, therefore, cannot be "counted on" to think carefully.

meaning of the statement, which in turn is a never finished product of that social context.[22] Finally, to see how a false statement can help to liberate us, it suffices to note that a false belief sometimes has a favorable outcome. (E.g., because you were mistaken about the time of departure, you missed a flight that crashed.)

Weinberg reads Harding

> I quoted [Sandra Harding] as calling modern science (and especially physics) "not only sexist but also racist, classist, and culturally coercive," and arguing that "Physics and chemistry, mathematics and logic, bear the fingerprints of their distinctive cultural creators no less than do anthropology and history." It seemed to me that this statement could make sense only to a relativist; what is the good of wishing that the conclusions of scientific work were friendlier to multicultural or feminist concerns if these conclusions are to be an accurate account of objective reality?[23]

What do Harding's remarks have to do with relativism? They are *empirical* claims, or would be if they were made more precise. How did Weinberg miss this? If he wishes to challenge these remarks, he should be looking at Harding's evidence and arguments. Also, where does he see her *wishing* for anything?

Lynch corrects Weinberg

> [If] we ever discover intelligent creatures on some distant planet and translate their scientific works, we will find that we and they have discovered the same laws.

Why should this be so? Weinberg seems to consider it obvious, which it surely is not. After repeating it in his opening statement in *The One Culture: A Conversation about Science*, he acknowledges in the third (238-239) that this is not what he should have said:

> In his essay in this book, Michael Lynch very properly caught me up on this and pointed out that the scientific works of intelligent creatures who inhabited Earth until a few centuries ago did not resemble the theories we believe in today.[24]

Clues to the past?

> Some historians do not deny the reality of the laws of nature, but nevertheless refuse to take present scientific knowledge into account in describing the science work of the past. This is partly to avoid anachronisms, like supposing that scientists of the past ought to have seen things in the way we do…. The problem is that if we try to avoid this sort of anachronism by ignoring present scientific knowledge, we give up clues to the past that cannot be obtained in any other way. ("Sokal's hoax")

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[22] See Wittgenstein on rule-following in his *Philosophical Investigations*.

[23] Weinberg goes on to say that Harding denied that she is a relativist and he did not press the matter.

[24] In what he goes on to say, Weinberg makes it fairly clear that he meant to talk only about aliens whose scientific work has reached what he calls "the asymptotic limit."

This is an intriguing claim. Clues to the past that cannot be obtained in any other way. I don't doubt that some version of this claim is true[25]. But here my concern is only with Weinberg's attempt to make good on it. Here is the example he offers.

> In the late 1890's, J. J. Thomson carried out a celebrated series of measurements of the ratio of the electron's mass and charge, and though the values he found were spread over a wide range, he persistently emphasized measurements that gave results at the high end of the range. The historical record alone would not allow us to decide whether this was because these results tended to confirm his first measurement, or because they were actually more careful measurements. Why not use the clue that the second alternative is unlikely because the large value that was favored by Thomson is almost twice what we know today as the correct value?

Weinberg seems to expect us to accept that he somehow knows that these two alternatives—the first of which is pulled out of a hat and the second is an explanation only in the syntactic sense that the word 'because' precedes the alleged explanation and follows a description of the phenomenon to be explained—are the only ones that we need to consider. But why should we take his word for this implausible claim? More important, even if the higher measurements had been 'actually more careful,' how could this have helped make Thomson favor them (whatever this means) unless he *believed* that they were more careful?[26] But he might have believed this even if these measurements were not 'actually' more careful.[27] And we can delete 'might' because Thomson makes it clear in his article that he suspected that the experiments that yielded the higher values *were* more careful.[28]

Rationality and witchcraft

> As I mentioned earlier, our civilization has been powerfully affected by the discovery that nature is strictly governed by impersonal laws. As an example, I like to quote the remark of Hugh Trevor-Roper, that one of the early effects of this discovery was to reduce the enthusiasm for burning witches. We will need to confirm and strengthen the vision of a rationally understandable world to guard us from the irrationalities that still beset humanity.

According to Stuart Clark, author of *Thinking with Demons: the idea of witchcraft in early modern Europe* (Oxford 1997), a more thorough investigation reveals a very different story. Enthusiasm for burning witches did wane during the seventeenth century but "the discovery that nature is strictly governed by impersonal laws" seems to have had little to do with it. By contrast, religious and legal considerations loomed large. Weinberg might have done better to claim that the acceptance of a mechanical theory of nature *eventually* contributed to the adoption of a new worldview, not only of nature, in which there was no longer a significant

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[25] But "cannot" seems too strong.

[26] The lower measurements might well have been 'actually more careful,' they are close to the true value, yet Thomson did not favor *them*. Why then does Weinberg take seriously the possibility (if only to rule it out) that Thomson would have done so if it had been the higher measurements?

[27] E.g., there might have been an undetected systematic error.

[28] For more about this alleged clue to the past, see "Kinder, gentler science wars" in *Social Studies of Science*, February, 2004.

role for witchcraft. But this took time and before such a worldview took hold, witchcraft was a respectable subject of scientific inquiry. For example, on page 297, Clark writes:

> But the most illustrious of Glanvill's collaborators in witchcraft research was Robert Boyle, likewise a leading founder of the Royal Society. He and Glanvill corresponded on the question of the reality of witchcraft, with Boyle encouraging Glanvill to think of it as a proper subject for science. As the one fully corroborated and verified account of demonic activity that would clinch the matter, Boyle chose the story of the devil of Mâcon in France, originally compiled by François Perrault.

And on page 306, we find:

> As late as 1737, William Whiston, Newton's disciple and his successor in the Lucasian chair of mathematics, wrote that the assaults of invisible demons, as long as they were well attested, were 'no more to be denied, because we cannot, at present, give a direct solution of them, than are Mr. Boyle's experiments about the elasticity of the air; or Sir Isaac Newton's demonstrations about the power of gravity, are to be denied, because neither of them are to be solved by mechanical causes.' [29]

Finally, the last sentence of Weinberg's remark,[30] together with the preceding one, suggests that he believes that, in the sixteenth and seventeenth centuries, witch burning was irrational. But because he makes no attempt to justify this, for all we know, it is merely an irrational prejudice. Nor does he attempt to explain *how* confirming and strengthening *his* vision of "a rationally understandable world" will "guard us from the irrationalities that still beset humanity." Indeed, except for his ill-considered choice of witch burning, Weinberg gives us no sense of what, in his view, *are* "the irrationalities that still beset humanity." But isn't one of them holding *so* fast to a vision—for example, to that of "a rationally understandable universe"—that one is unable to understand other points of view? If Weinberg disagrees, he needs to explain why. And if he agrees, he needs to explain how holding fast to his own vision of a rationally understandable universe guards him from holding *so* fast to it that he is unable to understand other points of view.[31]

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[29] There is a great deal more about these and related matters in Clark's opus, especially in "Science" (151-311), which includes such chapters as "Witchcraft and science," "The devil in nature," "The causes of witchcraft," "Believers and sceptics," "Natural magic," "Demonic magic" and "Witchcraft and the Scientific Revolution."
[30] Which is also the last sentence of his essay.
[31] I don't think it does.