

Risking Connection Across Difference: Reply to Sokal and Smith¹

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At the time I wrote my original review (Schultz 2010) of the books by Sokal (2008), Boghossian (2006), and Smith (2006), I did not know that I would have the opportunity to reply to their responses to my review. Nevertheless, I value the occasion this offers to correct errors and respond to their commentary. Let me say, first of all, that Alan Sokal is quite correct in pointing out that the citation from Donna Haraway which I attribute to him is incorrect. I realize now that I copied that version of the citation from an earlier working notes file for the review. I should have verified that the copied version was identical to the citation in the *Beyond the Hoax* text (Sokal 2008) before I sent off the review. That I did not do so is an error that I deeply regret, and for which I apologize. In line with this, I also apologize for, and retract, my observations in the review about the omitted passages in the citation, for which Sokal obviously was not responsible.

So, does consideration of the full quotation by Donna Haraway in any way affect the rest of my critique? Not at all, as it happens, because the focus of the rest of my remarks is on the way Sokal *treats* this passage by Haraway. That is, he presents the excerpt to readers in isolation, and *makes no effort to*

¹Editor's note: This is the review author's reply to two book author responses to Emily A. Schultz's "Fear of Scandalous Knowledge: Arguing about Coherence in Scientific Theory and Practice" (2010), published last year in *Reviews in Anthropology*. Schultz's essay reviewed three books, including Paul Boghossian's *Fear of Knowledge: Against Relativism and Constructivism* (2006), Alan Sokal's *Beyond the Hoax: Science, Philosophy, and Culture* (2008), and Barbara Herrnstein Smith's *Scandalous Knowledge: Science, Truth, and the Human* (2006). In this special section, the authors of all three books were invited to write a response with the understanding that Schultz would also be given the opportunity to reply. Boghossian was unavailable to prepare a response by our deadline, but Smith's and Sokal's responses appear here, with Schultz's reply following these.

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contextualize its content in terms of the author's own scholarly goals or disciplinary practices. Instead, readers are asked to consider the propositional statements contained in the excerpt, and then draw their own conclusions about the (apparently self-evident) incoherence of these statements. This is Sokal's approach not only to Haraway, but also to all the other writers whose work he criticizes (when, that is, he quotes from their texts at all).

Sokal says that I never mention his "detailed and respectful analyses of some key ideas from the work of Carolyn Merchant, Sandra Harding, and Evelyn Fox Keller (119–129), Barry Barnes and David Bloor (203–211) and Bruno Latour (211–216)." There is not room in this brief reply to consider each of these discussions, but let us consider his treatment of Merchant and Harding to see how "detailed and respectful" it is. Sokal observes that some feminist science critics look for sexist metaphors in the work of scholars like Francis Bacon: "Thus, Bacon stands accused of analogizing systematic experimentation to the rape (Harding) and torture (Merchant) of Nature, viewed as female" (2008:119). On the next two pages (2008:120–21), Sokal quotes "a now-famous (or should I say infamous?) passage" from Harding (1986:113, including two ellipses of Sokal's construction). In what follows, Sokal provides no substantive details concerning the broader argument from which the Harding excerpt was pulled (and none whatsoever about Merchant's argument). Sokal refers to the work of other scholars who he says *do* provide such details, but he cites only page numbers in their work where the details can be found, not the details themselves.

Although he provides no evidence for the claim, Sokal insists that Newton's *Philosophiae Naturalis Principia Mathematica* is "notoriously unreadable," (2008:121), thereby casting doubt on Harding's (or anyone else's) interpretation of this text. Readers are apparently expected to trust Sokal and Margarita Levin when they assure us that the machine metaphor is fruitful, but the rape metaphor is not (2008:121). And Sokal ignores historical analyses that trace the emergence and stabilization of modern scientific thought and practice, preferring to accuse their authors of committing the "genetic fallacy"—dismissing an "idea" on the basis of its origin rather than its content (2008, 122). Such maneuvers do not strike me as respectful.

Thus, if I take Sokal to task, it is precisely because he refuses to acknowledge here (and in his response to my critique) the point made by physicist Peter Saulson, which I quoted on pages 320–21 of my review: that "[d]ifferent styles of language appear to be at the root of many of the disputes" in which Sokal has engaged with writers like Merchant and Harding, with the result that his continued refusal to respect such differences "flagrantly misses the point" of works by scholars outside his field "who are used to a different kind of reading" (Saulson 2001:79).

Saulson is not the only physicist to object to Sokal's manner of dealing with the texts of writers with whom he disagrees. Physicist N. David Mermin, for example, has engaged in extended dialogue with sociologists of scientific

knowledge Trevor Pinch, Harry Collins, Barry Barnes, and David Bloor. In an essay he contributed to *The One Culture?*, Mermin reported that much of the original miscommunication between himself and Harry Collins was due to different unstated assumptions: “Harry Collins and I slowly realized in the course of several e-mail exchanges that by ‘our knowledge’ I always had in mind collective knowledge, but he was always thinking of the knowledge of any given individual. Realizing this eliminated much unnecessary misunderstanding” (Mermin 2001a:90, n7).

Mermin’s productive interactions with Collins and others prompted him to propose three rules to govern “how scientists and sociologists should converse, not in their roles as anthropologists and native informants, but as academic colleagues, reflecting on the nature of their two disciplines.

1. Rule 1: Focus on the substance of what is being said and not on alleged motives for saying it
2. Rule 2: Do not expect people from remote disciplines to speak clearly in or understand the nuances of your own disciplinary language
3. Rule 3: Do not assume that it is as easy as it may appear for you to penetrate the disciplinary language of others” (2001a:97–98).

As it happens, Mermin has also engaged in exchanges with Alan Sokal and Jean Bricmont, offering an alternative to their negative appraisal of the use made by Bruno Latour of Einstein’s theory of relativity (see Mermin 1997; Sokal and Bricmont 1998:131–133). In a second essay in *The One Culture?*, Mermin explicitly calls into question Bricmont’s and Sokal’s analytic style, citing a passage in their essay “Remarks on Methodological Relativism and Antiscience,” which appears as chapter 14 in the same volume (Bricmont and Sokal 2001:179–183). Mermin observes that Bricmont and Sokal

end their list of what underlies the “relativist Zeitgeist” with “a lot of concrete empirical work, on which we make no judgment.” But the quality of that empirical work is central to evaluating the whole undertaking. If it is set aside, one loses the most stringent test of whether “knowledge” is more productively viewed as situated in people’s heads or the mirror of something objective. (2001b:277)

By 2001, then, Saulson and Mermin had both achieved more nuanced understandings of the claims of science studies scholars, had engaged in mutually productive exchanges with them, and had abandoned the position that Sokal continues to occupy in 2011. Indeed, Sokal continues to ignore Mermin’s three rules of cross-disciplinary engagement, with the result that his most recent attacks on “postmodernism” (including his response to my

review) read as if they were written in 1996—for all the world as if the insightful, groundbreaking conversations of Saulson and Mermin with science studies scholars had never occurred, as if the widespread institutionalization of science studies which Smith describes were not a fact of academic life.

If the insight and advice of two physicists cannot prompt Sokal to reconsider his position, perhaps nothing can. Unlike Sokal (but much like Ludwik Fleck), Saulson and Mermin allowed themselves to become boundary objects (see pp. 322–323 in my original review); they withstood the friction generated by their efforts to establish awkward connections across difference (see Tsing 2005), and were in some measure successful. By contrast, Sokal appears to be a reluctant and resistant boundary object, and his encounters with disciplinary differences, however awkward, do not seem intended to establish connections. Nevertheless, he appears to be too conscientious a scholar to avoid qualification and contextualization altogether. I remain convinced that the friction produced by Sokal's boundary work generates the uncomfortable line separating text and footnotes in *Beyond the Hoax*.

I stand by my original analysis of Sokal's footnoting practices. Sokal does not accept my interpretation of his footnotes, so he and I will simply have to disagree on this point. I urge readers to seek out the text of *Beyond the Hoax*, examine his use of text and footnotes, and make up their own minds.

Barbara Herrnstein Smith's response to my review of *Scandalous Knowledge* (Smith 2006) raises a different set of issues associated with the challenges of attempting to establish connection across disciplinary difference. First, she is concerned that my use of the expressions "science criticism" and "science critics" describe the projects and scholars I wish to defend in misleading ways. Because I intended neither to evaluate the natural sciences in negative terms, nor to suggest that Kuhn, Fleck, or Bloor evaluated them negatively, her point is well taken. I encountered these expressions in contexts where they appeared to be neutral alternatives to "science studies" and "science studies scholars," but I am quite willing to replace them with the latter expressions to avoid confusion.

Second, Smith is concerned that my defense of the value of literary expertise in science studies might be read as justification for dismissing the writings of challenging thinkers. That was the opposite of my intention. I had been asked to write about these matters with a specific target audience in mind: those disciplinary colleagues of mine who remain deeply skeptical about "cultural studies" and "science studies," and who continue to regard the "postmodern turn" as an attack both on "science" and on their professional integrity. Smith is, of course, quite right to draw attention to the level of literary sophistication which writers like Latour take for granted in their audiences. My goal was more limited. Perhaps naively, I hoped that

testimony about my own experiences might encourage suspicious colleagues to reconsider the writings of challenging thinkers whose texts they had previously found off-putting—to keep reading, despite their misgivings. I still hope they will.

Finally, I was intrigued by Smith's comment concerning relativism, reflexivity, and Thomas Kuhn. It is surely the case that anthropologists of different generations, trained in different subfields, have encountered Kuhn's work under different circumstances and might evaluate it differently. Among North American cultural anthropologists of my generation, Kuhn's relativist credentials were widely acknowledged; his notion of paradigm incommensurability seemed to resonate well with the relativist tradition inherited from Franz Boas, Edward Sapir, and Benjamin Whorf; and his work could be mobilized against universalists who viewed cultural (or linguistic) variation as superficial.

Clifford Geertz's influential "Anti Anti-Relativism" (1984) rallied many anthropologists challenged by rationalist and naturalist attacks on the achievements of our discipline. Rather than defending any particular relativist dogma, Geertz's essay was directed *against the critics* of "relativism." The scandal of anthropology, he argued, was its provision of overwhelming evidence that could only undermine the certainties of Western thought—and yet, as he wrote, "science, law, philosophy, art, political theory, religion, and the stubborn insistences of common sense have contrived nonetheless to continue. It has not proved necessary to revive the simplicities" (1984:275).

If the persuasive goal of relativists is to destabilize certainties in the face of stiff opposition, they may choose to de-emphasize the ways and means by which "science, law, philosophy, art, political theory, religion, and the stubborn insistences of common sense have contrived nonetheless to continue." As I argued in *Dialogue at the Margins* (Schultz 1990), this is what I believe Whorf was doing, especially in his most notorious texts. Mikhael Bakhtin and his associates, by contrast, explicitly drew attention to the alternative polyglot and heteroglot resources available to speakers in communities who wished to voice "the stubborn insistences of common sense" in the face of censorship, manipulating the rules of grammar and discourse for their own ends. Reading Whorf through Bakhtin allowed me to detect the less-than-obvious dialogical/reflexive elements in Whorf's texts, and show why Whorf's relativity could not be equated with linguistic determinism.

Interestingly, Geertz says much more about relativism's role in promoting anti-foundationalism than he says about why it is that the world, despite a lack of foundations, fails to come to an end. This is why Fleck's work is such a revelation: he both attends to the harmony of illusions engendered by separate thought collectives *and* he explicitly draws attention to the porous borders of thought collectives and the opportunities, especially in democratic societies, for scientists to move from one thought collective to another. This is why Fleck should be read by all anthropologists who worry that there are no alternatives to foundationalist "simplicities." Engaging in dialogue,

reading different sets of “illusions” through one another, and making the sometimes awkward connections across difference give rise to shared stability as well as intellectual innovation.

Most anthropologists today would probably agree that relativity and reflexivity are not contradictory, but I suspect that few of them would credit Kuhn for that insight. Still, Fleck did influence Kuhn. Science studies scholars did take Kuhn’s multifaceted characterization of scientific paradigms as instances of exemplary experimental practice, and did demonstrate why scientists were not locked inside the prison-house of theory (see Schultz 2010:314). Smith has persuaded me that rereading Kuhn through Fleck may allow Kuhn’s reflexivity to become more apparent. Finally, I thank Smith for noting and correcting errors in quotations from her book that were missed in copyediting.

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