

## **Fear of Scandalous Knowledge: Arguing About Coherence in Scientific Theory and Practice**

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Sokal, Alan 2008. *Beyond the Hoax: Science, Philosophy and Culture*. Oxford: Oxford University Press.

Boghossian, Paul 2006. *Fear of Knowledge: Against Relativism and Constructivism*. Oxford: Clarendon Press.

Smith, Barbara Herrnstein 2006. *Scandalous Knowledge: Science, Truth, and the Human*. Durham: Duke University Press.

*A decade after the “Sokal Hoax,” Alan Sokal and Paul Boghossian still claim that postmodern arguments are incoherent attacks on reason and truth. However, both also continue to mischaracterize “constructivist” epistemology, to engage in highly problematic logical gymnastics to defend their own views, and to ignore changes in philosophy of science and science studies since 1996. I offer a brief description of my own, rather different understanding of postmodern science criticism in order to contextualize my dissatisfaction with Sokal and Boghossian’s arguments, and to highlight the value of cross-subfield anthropological collaboration based on the constructivist perspective articulated by Barbara Herrnstein Smith.*

**KEYWORDS** *constructivism, postmodernism, relativism, science wars, social constructionism*

### INTRODUCTION: ANTHROPOLOGY AND THE SCIENCE WARS

In *Beyond the Hoax*, Alan Sokal describes how, in 1994, he read the book *Higher Superstition* by Paul Gross and Norman Levitt (Gross and Levitt

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1994) and discovered “the phenomenon of postmodernist literary intellectuals pontificating on science and its philosophy and making a complete bungle of both” (Sokal 2008:xiii). Sokal then “decided to write a parody of postmodern science criticism, to see whether it could get accepted as a serious scholarly article in a trendy academic journal” (2008:xiii). Gross, Levitt, and Sokal all identified with the political left. As Sokal reiterates, “I chose to critique postmodernist leftists not because I saw them as the principal threat to rationality and science—which they were not—but because I saw their ideas as undermining our shared commitment to the struggle for social justice” (2008:xv n13). A few months later Sokal had produced his “masterpiece,” a text entitled “Transgressing the Boundaries: Towards a Transformative Hermeneutics of Quantum Gravity” (1996). He submitted it to the cultural studies journal *Social Text*, and it was published early in 1996—in a special issue devoted to the so-called “science wars.” Shortly thereafter, Sokal revealed his hoax and, he tells us, “my life has not been the same ever since” (2008:xiv).

It was publicity about the Sokal affair that first drew my attention to the “science wars,” and from there, to science studies. But my response has been very different from that of Sokal, and of philosopher Paul Boghossian, the author of the second book under review here. Because the issues are complex and unlikely to be familiar to all readers of this review, I am taking the liberty of using the introduction to provide contextual information that I hope will illuminate my consideration of the three books in subsequent sections of this essay.

Both Sokal and Boghossian attempt to demonstrate that “postmodern relativism” is incoherent and self-contradictory. Their attempts are problematic and unpersuasive, however, for at least three reasons. First, they perpetuate all the weaknesses of their initial misreadings of “postmodern relativist” science criticism, in which they failed to grapple with actual texts by science critics. Second, they compound those misreadings by continuing to imagine that “postmodern relativist” critique can be demolished by ripping individual passages or sentences from the texts in which they appear and holding them up to ridicule. Third, they write as if “relativist” claims about the incommensurability of alternative scientific theories were still the focus of science studies. Science studies have moved on in interesting and important ways in the past 15 years, even if Sokal and Boghossian remain firmly rooted in the past. They seem not to have noticed that attention has shifted from “science” to particular sciences, from “theory” to material practices in laboratories and other research settings, and from physics to biology as the prototypical science. In my view, it is the developments they ignore that offer real promise to anthropologists in the 21st century who seek new ways of connecting the subfields of our discipline to one another. Barbara Herrnstein Smith’s book, by contrast, is an excellent survey of these exciting new directions, and has the additional advantage of being based on close reading of both “postmodern relativist” and traditional philosophy of science.

In taking this position, I do not pretend to speak for all anthropologists. My only defense is that I lived through the bitter dissention occasioned by the “postmodern turn” in anthropology, and arrived at my current views after much reading and serious reflection. It is important to stress, however, that the way had been prepared long before Sokal published his parody. Like other anthropologists working in the 1970s and 1980s, I had been grappling for some time with antagonisms in our discipline that predated the so-called “postmodern turn.” I was trained as a four-field anthropologist before the advent of sociobiology and evolutionary psychology. Like many of my colleagues, I struggled to reconcile my commitment to evolutionary biology first with E. O. Wilson’s (1975) threat to absorb the humanities and social sciences into his sociobiological “new synthesis,” and later, with evolutionary psychology’s harsh and (to me) misguided critique of what they called the “standard social science model” (Barkow, Cosmides, and Tooby 1992).

When I plunged into the science studies literature in the late 1990s, I still had reservations about the extreme claims attributed to so-called “post-modern relativist” science critics. But I resolved to read science studies texts as carefully as possible, for three principle reasons. First, I had recently explored the issue of “relativism” in the work of Benjamin Whorf, aided by the literary theory of Mikhail Bakhtin (Schultz 1990). Among other things, Bakhtin taught me to identify “double-voiced discourse”—that is, the dialogical elements present in ostensibly univocal texts. Whorf’s so-called “relativist” texts are full of double-voiced discourse, and so, I later discovered, were many of the so-called “relativist” texts that figure in the “science wars.” Sokal and his allies, such as Gross and Levitt, have seemed unable or unwilling to cope with double-voiced discourse and its implications; this was one reason why, so it seemed to me, that their objections so often missed the mark. Second, sociobiologists and evolutionary psychologists (who claim the label of “science” for their own work) were simultaneously engaging in their own polemics, often ignoring or distorting works of careful scholarship by cultural anthropologists to score points. If this activity was supposed to be “science,” then perhaps serious science criticism was long overdue. Finally, my work on Whorf taught me that anthropologists, of all people, should not need to fear any kind of “relativist” argument, but were, in fact, better equipped than most scholars to assess its value.

I have been working in the science studies literature for many years now, long enough to get past the name-calling on both sides, and I have come to the unfortunate conclusion that the biggest obstacle preventing people from sorting out the issues surrounding “postmodern relativism” is the fact that so many critics and interested bystanders have not had the opportunity (or the patience!) to wade through the sea of ink that has been spilled on the topic over the years. Perhaps this explains why critics like Sokal and Boghossian appear to have skipped the hard work of actually reading what people have written, choosing instead to lump together

heterogeneous “postmodern relativist” positions, paraphrase what they are supposed to have in common, and then attack their own paraphrase for incoherence or self-contradiction.

Thus are straw men created and destroyed—not excluding the “standard social science model” invented by evolutionary psychologists. Indeed, from my perspective, the “postmodern turn” (and the reactions it provoked) cannot be separated from the earlier “sociobiological turn” (and the reactions it provoked). Through both these turns, what it means to do science, and what it means to be an anthropologist doing science, have been at stake. Many anthropologists have seen the options the way Sokal and his allies see them: either cleave to a traditional positivist view of science, or abandon science for irrationalism and incoherence. But other anthropologists and “postmodern” science critics have refused to accept that these are the only options available. Moving forward requires expanding one’s expectations about what “proper” theoretical arguments are supposed to look like, opening one’s mind to alternative epistemologies, especially those built on foundations that challenge traditional assumptions—or eschewing foundations entirely. Learning to think in these new ways is rarely easy, but anthropologists ought to be especially well suited for such a task. And if members of our own discipline are unable to help us out, we may follow that venerable anthropological tradition of looking outside our own discipline for inspiration.

Like some colleagues (i.e., Goodman and Leatherman 1998), I found resources for responding to the sociobiological turn in the work of evolutionary biologist Stephen Jay Gould, geneticist Richard Lewontin, and ecologist Richard Levins. Not only did they offer cogent critiques of claims made by sociobiologists and evolutionary psychologists; they were also forging, by means of critique, alternative ways of thinking about genetics, ecology, and evolution (see, for example, Gould 1977, 1993; Gould and Lewontin 1979; Levins and Lewontin 1985; Lewontin and Levins 2007). These respected biologists all raised issues that some of their colleagues preferred to ignore, or to treat as settled, but their challenge to received views is hardly evidence that they had abandoned truth and reason.

Gould, for example, drew attention to so-called “developmental constraints” on adaptation. His discussion resonated with work by developmental biologists and psychologists who challenged the exclusion of development from the “modern evolutionary synthesis” of the 1930s and 1940s. Unconventional views of development were proposed by Humberto Maturana and Francisco Varela, who emphasized “autopoiesis,” or organismic self-organization (1980). Susan Oyama’s important *The Ontogeny of Development* (2000[1985]) demonstrated persuasively how developmental processes themselves (not genes alone) regularly draw on reliably present resources available in the organism’s environment to generate the “information” needed for further organismic development. From this and related

work emerged what came to be called “developmental systems theory,” or DST; an essential volume exploring the implications of DST for evolutionary theory is *Cycles of Contingency* (Oyama, Griffiths, and Gray 2001), to which anthropologist Tim Ingold contributed an essay. Another recent, indispensable volume exploring related issues is Mary Jane West-Eberhard’s *Developmental Plasticity and Evolution* (2003). None of these books rejects natural selection as a key evolutionary process, nor can the processes they describe be considered “Lamarckian” in any way. All of them, however, draw valuable attention to biological processes that an exclusive theoretical focus on gene selectionism tends to downplay or ignore. This is good news for all anthropologists seeking to escape sociobiological/postmodernist crossfire.

Equally valuable are a number of recent innovative attempts to make room for culture (or at least for material artifacts) in comprehensive accounts of human biological evolution. One approach involves arguing that genes are not the only inheritance system involved in evolution. Thus, Robert Boyd and Peter J. Richerson have written about “dual inheritance” of genes and culture, each of which is said to shape the other (e.g., Boyd and Richerson 1985; Richerson and Boyd 2005); Eva Jablonka and Marion Lamb speak of four different inheritance systems: genetic, epigenetic, behavioral, and symbolic (Jablonka and Lamb 2006). A related approach is niche construction, first coined by Richard Lewontin (e.g., Lewontin 2000; Odling-Smee, Laland, and Feldman 2003). Proponents of niche construction point to vast evidence that all kinds of organisms modify the environments in which they live, thereby altering the selection pressures to which they are subjected. Key examples are earthworms and beavers, but human beings are among the most intensive niche constructors on our planet. The ability of organisms to alter the selection pressures they experience (which is *not the same* as exempting them from *all* selection pressures!) allows culture to enter into the evolutionary process in multiple ways, via such factors as artifacts, features of the built environment, social arrangements, and language. Taking niche construction seriously, in my view, involves refusing to accept an eternal division between “nature” and “culture,” and recognizing that we all are part of “naturecultures” (the term was coined by Donna Haraway) in which living and nonliving, human and nonhuman elements assemble themselves over time into more or less stable networks. This, of course, is one of the reasons niche construction is opposed by defenders of nature/culture dualism and traditional Western epistemology.

All these “dissident” approaches draw attention to *processes, relations, and interactions* between parts of developing organisms and their environments. Many emphasize the *simultaneous* self-organizing emergence, co-construction, and stabilization of organisms *and* environments *and* cultural practices. Hence, terms like “constructivist” or “relational” have been proposed as adjectives describing the views of scholars who adopt these views, although no agreed-upon label has yet emerged. Because a

“constructivist” or “relational” approach focuses on a complex *network*, and on the contributions made to network stability by a variety of *heterogeneous components*, scholars adopting this approach regularly (and correctly) distinguish their views from the views of other theorists who explain human behaviors or beliefs in terms of “social” or “cultural construction” alone. Unlike a “constructivist” or “relational” approach, “social” or “cultural constructionism” is not concerned with heterogeneous networks or emergence. Rather, it presupposes the traditional nature/culture divide, but insists that it is “society” or “culture” (rather than nature) that exerts primary causal efficacy in all human affairs, including science. These positions are not unrelated, of course, as the confusion of labels indicates. But the theoretical distinction between “constructivist” and “social” or “cultural constructionist” views is often lost on outsiders to science studies debates, and may be further muddled by critics who themselves cannot tell the difference, because they have not read the relevant texts or followed the relevant arguments. In any case, systematically challenging some venerable presuppositions of traditional Western philosophy does not automatically render “constructivist” or “relational” views incoherent, irrational, or “anti-science.” Quite the contrary; this perspective takes “reality” very seriously, but conceives of it (and our understanding of what is “true” about it) in a different way, as I will show below. Well-known contributors to the development of the “constructivist” perspective in science studies include Donna Haraway and Bruno Latour (both long considered by many in our discipline to be “honorary” anthropologists).

New thinking promoted by the intersection of insights from “post-modern” science studies and alternative perspectives in evolutionary biology can be found in all traditional subfields of North American anthropology; in a recent article, I describe several prominent examples (Schultz 2009). Niche construction has been influential in recent work by biological anthropologists such as Agustín Fuentes and his colleagues Matthew Wyczalkowski and Katherine MacKinnon (e.g., Fuentes 2007, 2008; Fuentes, Wyczalkowski, and MacKinnon 2010). Archaeologist Bruce Smith argues that niche construction “provides an important evolutionary and behavioral context for understanding . . . the initial domestication of plants and animals,” because such a context removes “the proximate mystery . . . . Domestication was not the product of unusual ‘outside the envelope’ behavior patterns, but emerged out of coherent preexisting resource management systems” (2007:195–196). Niche construction also figures in the work of anthropologists attracted to theories of complex adaptive systems. Biological anthropologist Terrence Deacon has argued that the evolution of symbolic human language will not be understood until theorists address “the complex self-organizing and evolutionary dynamics that form the very essence of its design logic” (2003:81). Cultural anthropologist Stephen Lansing has described the water temple/irrigation/rice terrace network of the Balinese as giving rise to a constructed Balinese

niche that has emerged, complexified, and restabilized over centuries (Lansing 2002, 2006; Lansing, Kremer, and Smuts 1998). While not using the term “niche construction,” linguistic anthropologist William Foley describes much the same process in his studies of language change, drawing especially on “enactive evolutionary theories” developed by Oyama and by Maturana, Varela and their colleagues (e.g., Varela, Thompson, and Rosch 1992; Thompson 2007). Foley prefers enactive theories to “orthodox neo-Darwinism” because they make room for both “the traditional insights and discoveries of historical linguistics” and for current views in linguistic anthropology that downplay the role of grammar and conceive of meaning as distributed and embedded, on multiple levels, within a network of communicative practices (Foley 2007:1, 23).

In my view, these “dissident” approaches to traditional anthropological topics owe a great deal to collaborations between biologists and philosophers of biology. An classic example is the collaboration between philosopher David Depew and biochemist Bruce Weber, whose *Darwinism Evolving* (1996) considers in detail a number of significant challenges to hegemonic “neo-Darwinian” thinking. In my experience, philosophers of biology have offered consistently superior analyses of debates between gene selectionists and their critics (see, for example, Sterelny and Griffiths 1999; Wimsatt 2007). Similarly, so-called “postmodern” literary scholars with science backgrounds have helped me decipher the sometimes complex rhetoric in the texts of scientists and philosophers, just as Mikhail Bakhtin helped me better analyze the texts of Benjamin Whorf. Literary expertise is often needed in science studies, because key texts in the philosophy or social studies of science can be challenging to read. For example, British and American readers often have a lot of trouble with the “continental” style of writers like Claude Lévi-Strauss, Michel Foucault, or Bruno Latour. As I note below, Latour is a particular target of Sokal and Boghossian, who seem unable to cope with his ironic, dramatic, and often screamingly funny theoretical texts. Donna Haraway’s style is both verbally rich and self-consciously ironic, and this can also be off-putting. It helps to know, however, that she is committed to the view that no text is innocent and seeks to display that awareness when she writes. A reader unaware of these theoretically motivated rhetorical choices will surely be puzzled if asked to pronounce on the meaning of a sentence by either of these authors, yanked out of context and presented on its own.

Still, I insist that none of the philosophers of biology or science studies scholars whose work I admire—Latour and Haraway in particular—could in the least be described as “postmodernist literary intellectuals pontificating on science and its philosophy and making a complete bungle of both.” So when, in the first decade of the 21st century, Alan Sokal and Paul Boghossian mock or ignore the work of these scholars, this is a measure of just how little they grasp of the work they purport to critique. This is not to say that all the

scholars I admire always agree with one another; I don't agree with all of them myself all the time. But I value the serious attempts they are making to reassess what it means to do science (and scientific anthropology) in challenging times.

In my view, science studies has been good for science and for anthropology, especially for general anthropologists who value cross-field collaboration. Traditionally, we have used the term "holism" to index the anthropological orientation that encourages such collaboration. But holism remains tethered to other traditional concepts, like the nature/culture split, which encourages us to think of individual "cultures" (and individual human organisms) as self-contained, closed, and internally coherent systems dependent upon, but separate from, nature. For some time now, however, cultural anthropologists have learned to rethink "cultures" as heterogeneous hybrids that emerge and stabilize over time—and that may destabilize or change when conditions change. This loose-jointed but self-organizing view of cultural processes resonates well with "constructivist" and "relational" understandings of organismic and environmental evolution. For a rich, engrossing, maddening, and thought-provoking sample of what might be possible from such a perspective, I strongly recommend Shirley Strum and Linda Fedigan's edited collection *Primate Encounters* (2000).

In sum, I believe that the "constructivist"/"relational" perspective offers superior opportunities for productive "general" cross-field anthropological collaboration in the twenty-first century. The books by Alan Sokal and Paul Boghossian under review do not contribute to any such collaboration, but instead attempt to keep alive old controversies that many contemporary science studies scholars and anthropologists consider settled or view as marginal. By contrast, it is exactly these new directions in scholarship that Barbara Herrnstein Smith has been helping to define in recent years, not only in *Scandalous Knowledge* but also in her other books chronicling and explicating the "postmodern turn" (Smith 1991, 2001, 2010).

The skills of a literary scholar and philosopher are particularly useful when addressing issues associated with the "science wars." A decade ago, for example, physicist Peter Saulson commented that "[d]ifferent styles of language appear to be at the root of many of the disputes that have been loosely grouped together under the heading of the science wars," and he recounted an event he observed at a 1996 panel discussion of Sokal's parody article:

The key moment of the afternoon came when we invited questions from the floor. A graduate student of English stood up and, with obvious anger, asked Sokal, "What is your theory of language?" As she sat down, Sokal fumbled for an answer, clearly uncertain what the question meant. The matter was allowed to drop, and the event wound down to its conclusion.

It was only many weeks later that I realized what the question had meant, and why it was precisely the right question to ask. The key

rhetorical device in Sokal's hoax article is to analyze for its truth value a single sentence, taken out of context, written by a famous person whom Sokal wishes to ridicule. Now if he were critiquing a piece of writing in the natural sciences, this might be a perfectly reasonable way of assessing the validity of the work. Scientific writing often has the character of a mathematical proof; if a key sentence can be shown to be false, then the whole argument will fail. This kind of language use is much less common outside the sciences (with the exception of philosophy). Thus, Sokal's technique was seen, by those used to a different kind of reading, to flagrantly miss the point of the ridiculed works. [Saulson 2001a:79]

Saulson's charitable interpretation may help readers understand why Sokal and Boghossian used this technique in the late 1990s. However, the books under review demonstrate that both Sokal and Boghossian still cling to this technique, making it questionable how much charity readers ought to extend them today. As I show below, Sokal simultaneously acknowledges and rejects what context contributes to coherent argumentation, while the coherence of Boghossian's argument is purchased at the price of excluding everything but his own voice.

Barbara Herrnstein Smith, by contrast, has carefully studied the texts Sokal and Boghossian ridicule. She demonstrates the power of rich "constructivist" accounts of scientific knowledge production, in which truth, reality, and coherence emerge as the consequence of mutual adjustment among beliefs, practices, and other features of the social, cultural, and material contexts of scientific practice. Smith understands the classic philosophical arguments underlying the claims about coherence made by Sokal and Boghossian, but finds constructivist notions of coherence, illustrated in the work of Ludwik Fleck, more helpful for understanding current scientific work.

#### ALAN SOKAL'S STRUGGLE WITH COHERENCE AND CONTEXT

The essays in *Beyond the Hoax* were written by Alan Sokal (or co-written by Sokal and Jean Bricmont) between 1996 and 2008. As Sokal notes they were "all published previously (with the exception of chapters 4, 9 and 10), but they form . . . a coherent whole . . . the deeper theme is the importance, not so much of *science*, but of the *scientific worldview*" (2008:xi). The book has three parts. Part I, "The *Social Text* Affair," includes five chapters, the first of which is an annotated reprint of the original *Social Text* parody essay. Part II, "Science and Philosophy," includes two chapters by Sokal and Bricmont. Part III, "Science and Culture," includes three chapters on pseudoscience and postmodernism, religion and politics, and epistemology and ethics.

A good place to begin is chapter 5 (an essay begun in 1996 and completed for *Beyond the Hoax*), in which Sokal describes how he constructed his parody essay:

the article is structured around the silliest quotations I could find about mathematics and physics (and the philosophy of mathematics and physics) from some of the most prominent French and American intellectuals; my only contribution was to invent a nonsensical argument linking these quotations together and praising them . . . . In some cases, I took the liberty of parodying extreme or ambiguously stated version of views that I myself hold in a more moderate and precisely stated form. [Sokal 2008[1998]:153]

Unfortunately, many of Sokal's *non*-parody pieces are structured in exactly the same way. Consider, for example, the way he "critiques" the work of Donna Haraway in chapter 4, starting with a "silly quotation" and following it with a nasty putdown:

Here, for instance, is what Donna Haraway, professor of the history of consciousness (!) at the University of California-Santa Cruz and one of the most acclaimed feminist theorists of science, says about her research:

For the complex or boundary objects in which I am interested . . . dimensions implode . . . they collapse into each other . . . story telling . . . is a fraught practice . . . In no way is story telling opposed to materiality, But materiality itself is tropic; it makes us swerve, it trips us; it is a knot of the textual, technical, mythic/oneric, organic, political and economic.

As right-wing critic Roger Kimball acidly comments: "Remember that this woman is not some crank but a professor at a prestigious university and one of the leading lights of contemporary 'women's studies.'" The saddest thing, for us pinkos and feminists, is that Kimball is dead on target. [Sokal 2008:123]

Sokal provides no evidence that he has ever read, let alone studied, anything Donna Haraway has written. Sokal "apologizes" a few pages later for "this swift and selective refutation" (Sokal 2008:129). But since chapter 4 devotes no more than 25 lines to Donna Haraway, how could Sokal seriously imagine that he has "refuted" anything? First, the cited passage is riddled with ellipses. To be sure, Sokal links up subjects, verbs and objects, but the significance of their linkage is muddled because an unknown amount of potentially vital surrounding text has been excised. Second, deriving meaning even from this mutilated passage depends on knowing what "boundary objects" are. Science studies scholars Susan Leigh Star and Geoffrey Bowker define them as

objects that both inhabit several communities of practice and satisfy the informational requirements of each of them. Boundary objects are thus

both plastic enough to adapt to local needs and constraints of the several parties employing them, yet robust enough to maintain a common identity across sites.... Such objects have different meanings in different social worlds but their structure is common enough to more than one world to make them recognizable, a means of translation. The creation and management of boundary objects is a key process in developing and maintaining coherence across intersecting communities. [Bowker and Star 1999:297]

The concept of boundary objects is a key science studies innovation, illustrating how the shift from debates about theories to studies of practice helped resolve the incommensurability conundrum. With this definition in hand, it becomes possible to gauge the potential significance of other fragments of Haraway's text: for example, if boundary objects are plastic, they may meld if they meet; and if they meet under extreme conditions, they may implode—perhaps when the different communities of practice that make use of them are forcefully brought together? Similarly, once one knows something about constructivist thinking (i.e., in terms of heterogeneous assemblages of humans and non-humans, living and non-living phenomena—cyborgs, for example) Haraway's refusal to separate language from materiality makes sense, as when she warns readers that knotting words to objects impedes their free circulation; or, put another way, that the link between them stabilizes their relationship, for good or for ill. Sokal, however, knowing none of this, appears to conclude that refutation was achieved because Haraway's writing has been displayed as “meaningless” (Sokal 2008:129); because “her epistemological position is incoherent” (Sokal 2008:134). And although he urges the reader “to consult the original works and judge for herself their philosophical value” (Sokal 2008:129), Sokal himself seems never to have done anything other than page through them, seeking the “silliest” quotations he could find for his hit-and-run polemic.

Saulson's earlier observation helps explain this practice, and it may help explain another feature of *Beyond the Hoax*: the footnotes that weigh down each page, sometimes taking up more space than the main text. Every few sentences in the main text, new footnotes appear, sometimes two three at a time. This means that every few sentences, readers must move from the main text to the footnotes and back again. In addition, many footnotes are extremely lengthy—often much longer than the statements to which they are attached—and many provide significant commentary on those statements. It is in the footnotes, for example, where replies to Sokal's critics usually appear. Consequently, close readers cannot afford to ignore these footnotes, especially the long ones attached to texts written in the 1990s and only “slightly updated or revised” for *Beyond the Hoax*, like chapter 4. Struggling with this format for over 400 pages is not for the faint of heart. Readers who want to make sure they miss nothing Sokal has to say must be prepared for a choppy, somewhat dizzying journey.

Why configure a text in this way? Leaving original articles (mostly) untouched might signal to readers that the views an author originally expressed many years ago coincide with that author's views today. But if so, why overload the text with footnotes? Perhaps Sokal wanted to avoid the work of rewriting. Still, the care he put into the footnotes suggests to me that this unwieldy patchwork may accurately reflect Sokal's complex and somewhat contradictory orientation toward his own work. If it is true, however, as Sokal claims, that the essays in this volume form "a coherent whole," then the shape of his texts point to some serious tensions roiling beneath the surface of this "whole." In short, it appears that Sokal's stubborn unwillingness to give up his favored method of "analyz[ing] for its truth value a single sentence, taken out of context" is struggling with an equally powerful unwillingness to ignore commentaries and criticism of his texts written by others. The result is that each chapter threatens to come apart at the seams.

Dividing one's current views between text and footnotes, however, is also a handy way to evade criticism by readers. A good example is found in chapter 7, where Sokal and Bricmont seek "to defend a *modest realism*," because "relativism and radical social constructivism have become hegemonic in vast areas of the humanities, anthropology and sociology of science (among other fields)" (Sokal 2008:229; emphasis in original) [referring to *modest realism*]. In typical style, they ask readers to "consider the following assertions by prominent Science Studies practitioners," after which they list a series of statements taken from texts by five of their "usual suspects," including Harry Collins. The Collins assertion is classic: "The natural world has a small or non-existent role in the construction of scientific knowledge" (Collins 1981:3). The Collins *footnote*, however, unlike footnotes to the other assertions, is eleven lines long and contains "two qualifications:"

First, the statement is offered as part of Collins' introduction to a set of studies . . . . He does not *explicitly* endorse this view, though an endorsement seems implied by the context. Second, while Collins appears to intend this assertion as an empirical claim about the history of science, it is possible that he intends it neither as an empirical claim nor as a normative principle of epistemology, but rather as a methodological injunction to sociologists of science . . . . We have argued elsewhere . . . that this approach is seriously deficient *as methodology* for sociologists of science." [2008:230, n3]

Together, these qualifications suggest that Sokal's and Bricmont's understanding of the meaning of Collins's statement has become more nuanced over time. Yet they have neither *removed* the Collins quotation from the main text, nor *inserted* their qualifications into the main text. The qualifications (as well as the tensions they embody) remain in footnote limbo, and their significance to the authors remains ambiguous.

Chapter 7 is also interesting for the textual gymnastics in which Sokal and Bricmont indulge in order to refute the thesis of the under-determination of theory by evidence. They define “the general (Quinean) underdetermination thesis” as follows: “given any set of facts, just make up a story, no matter how *ad hoc*, to ‘account’ for the facts without running into contradictions,” adding in a footnote that Quine “even allows himself to change the meanings of words and the rules of logic, in order to show that any statement can be held true, ‘come what may’ (Quine 1980:43)” (Sokal 2008:236 n26). In the very next sentence of the main text, however (although in a subordinate clause!), Sokal and Bricmont concede that Quine’s thesis “played an important role in the refutation of the most extreme versions of logical positivism,” which was presumably good. If so, perhaps “radical relativism”—or, at least, hyperbolic statements that *appear* “radically relativistic”—may sometimes play roles in discourse *other than that of isolated true-or-false propositions*, which challenges their theory of language and meaning. But if this is the case, does it mean that Quine’s work (like that of “postmodern” or “postcolonial” intellectuals) is also dangerous, because it can play directly into the hands of ideologues?

Sokal and Bricmont struggle to finesse their way out of this dilemma. They admit that Quine’s thesis is “not very different from the observation that radical skepticism or even solipsism cannot be refuted,” which is presumably bad. Not to worry, however, because “it is clear that *in practice*, nobody ever takes seriously such ‘theories’ as those mentioned above, any more than they take seriously solipsism or radical skepticism.” But this statement suggests that *science in practice* may be able to overwhelm the epistemic power of scientific *theory*. In another anxious footnote they further contextualize Quine’s views: “Though Quine’s insistence that ‘any statement can be held true come what may’ (Quine 1980:43) can be read as an apologia for radical relativism, his discussion (pp. 43–44) suggests that this is *not* his intention” and that “Quine backtracked from his earlier assertion that ‘the unity of empirical significance is the whole of science (p. 42).’” (Sokal 2008:237, n28). Perhaps Quine is off the hook. It seems that with Quine, as with Harry Collins, the *context* in which a “relativistic” statement appears has the power to affect that statement’s meaning. But if this is true for Quine, and for Collins, why not for the authors of all the other “silly” statements that Sokal and Bricmont continually expose to ridicule?

The stakes rise in chapter 8, where Sokal classifies religious belief systems and scientific theories together as alternative epistemic “doctrines,” and undertakes to classify them along “a continuum from genuine science to pseudoscience, based on the strength of empirical evidence for or against a given theory and on the soundness of the methodology employed by the theory’s advocates” (Sokal 2008:268, figure 8.1). After discussing alternative therapies in nursing, Sokal turns his attention to “Hindu nationalist pseudoscience and postmodernism in India.” Summarizing Meera Nanda’s

*Prophets Facing Backward* (2003), Sokal says he will emphasize “the ideas put forth by the ‘postcolonial’ theorists on the one hand and the Hindu nationalists on the other, analyzing their similarities and differences” (Sokal 2008:297). Twenty pages of sound-bite analysis later, Sokal admits that “postcolonial intellectuals” such as Ashis Nandy, Claude Alvares, and Vandana Shiva

do not, of course, support the chauvinist and intolerant aspects of Hindu nationalism, and they cannot be held responsible for its rise. But . . . their denunciations of modern science and defenses of “local knowledges” played directly into the hands of the ideologues of Hindutva . . . undermining any possible ground for opposition to Vedic pseudoscience and, more generally, to the Vedic worldview. [Sokal 2008:319]

Many anthropologists are likely to object to Sokal’s view of religion because he lumps together into a single “Vedic worldview” an enormous range of ideas and practices, and because ethnography and science studies have both demonstrated that “beliefs” (i.e., epistemology) form only one part of the global cultural phenomena that Sokal calls “religion” and “science” (e.g., Bowen 2004; Latour 1987). Sokal also ignores the possibility, suggested by Ashis Nandy, that hybrid approaches to science may be possible; I will return to this matter below.

It is also worth reflecting on the consequences of evaluating the views of scholars and theorists *not* on their own merits, but according to the purported ease with which they give aid and comfort to the enemy. In his preface, for example, Sokal announces, with grim satisfaction, that “Bruno Latour, who spent several decades stressing ‘the social construction of scientific facts,’ now laments the ammunition he fears he and his colleagues have given to the Republican right” (Sokal 2008:xv), and he cites in support a passage from a recent Latour article (2004:227). Never mind that Latour’s actual views in this article require understanding why he *also* writes that “In spite of my tone, I am not trying to reverse course, to become reactionary, to regret what I have done, to swear that I will never be a constructivist any more” (2004:231). Never mind that Latour explicitly distances himself from theorists like Sokal, who he says, “looked backward, attempting to reenter the solid rock castle of modernism, and not forward to what I call, for lack of a better term, nonmodernism” (Latour 2004:227, n4). Never mind that Latour introduces new proposals in this article for moving from “deconstruction” to “constructivism” (2004:232).

But please do note that Sokal himself may also have something to lament. As chemist Jay Labinger noted years ago, using ridicule (as Sokal did in his parody) is “an attractive strategy . . . . But it is a corrosive weapon, which may as easily be brought to bear against science as for it” (2001:170). Right-wing lobbyists, as Labinger noted, can make use of ridicule just as

easily they can appropriate so-called “postmodern relativist constructionism.” Both are non-innocent (to borrow a term from Donna Haraway), both can be dangerous, and both need to be used with care.

#### PAUL BOGHOSSIAN’S FEAR OF CONTEXT

*Beyond the Hoax* is a weighty tome. By contrast, Paul Boghossian’s *Fear of Knowledge* is only 139 pages long, including bibliography and index. In his preface Boghossian explains that his book aims to “engage long-standing issues about the relation between mind and reality” prompted by the “remarkable consensus” that has formed over the past two decades concerning “the thesis that knowledge is socially constructed” (Boghossian 2006:vi). *Fear of Knowledge* has nine chapters. In chapter 1, Boghossian argues that the thesis that knowledge is socially constructed is connected to “relativistic” views based on a “radical” and “counterintuitive” doctrine of “equal validity,” which he defines as follows: “there are many radically different, yet ‘equally valid’ ways of knowing the world, with science being just one of them” (2006:2).

Boghossian then asserts that “[i]n vast stretches of the humanities and social sciences, this sort of ‘postmodern relativism’ about knowledge has achieved the status of orthodoxy” (2006:2), although he provides no evidence to back up this generalization. Instead, he supplies a couple of favorite sound-bites<sup>1</sup>: statements by two archaeologists extracted from a 1996 *New York Times* article on negotiations between scientists and members of Native American communities over the disposition of ancient human remains (Johnson 1996). First, archaeologist Roger Anyon is quoted as saying that “Science is just one of many ways of knowing the world. [The Zuni’s world view is] just as valid as the archaeological viewpoint of what prehistory is about.” Next, archaeologist Larry Zimmerman is quoted as calling for “a different kind of science, between the boundaries of Western ways of knowing and Indian ways of knowing,” and adding, “I personally do reject science as a privileged way of seeing the world.”

In the manner of Alan Sokal (and as Peter Saulson might have predicted) Boghossian regards the archaeologists’ statements as isolated propositions to be subjected to logical analysis that will determine their truth-value. After all, how else could an analytic philosopher proceed? Commenting on Boghossian’s use of the Anyon quotation in an earlier text, Gabriel Stolzenberg observes, “The obvious and surely the best answer is, ‘Ask Anyon.’ Remarkably, the idea seems not to have occurred to [Boghossian], perhaps because it would have been inconvenient to let Anyon have a say

<sup>1</sup>Boghossian first used this example in “What the Sokal Hoax ought to Teach Us,” a 1996 article published in the *Times Literary Supplement* (Boghossian 1996). Sokal cites this example in chapter 3, of *Beyond the Hoax*, making reference to Boghossian’s analysis (Sokal 2008:110).

about what he meant” (Stolzenberg 2001:50). As for the Zimmerman quotation, it might have helped if Boghossian had included the rest of Zimmerman’s words from the article: “That’s not to say [science] isn’t an important way that has brought benefit. But I understand that as a scientist I need to constantly learn.” But either move would interfere with Boghossian’s “blind entitlement” to use “his own epistemic system . . . without first having to supply an antecedent justification for the claim that it is the correct system” (Boghossian 2006:99).

In chapter 2, Boghossian declares: “Before proceeding any further, it will be useful to lay down some terminology for the systematic description of our cognitive activities” (2006:10). In other words, Boghossian is laying down the law: all that follows will be based on the traditional assumptions and practices of classic Western academic philosophy; and the terms, definitions, and statements Boghossian mobilizes will all be shaped to fit the needs and interests of formal logical analysis. Lockdown is secured as follows: “I have been talking about the Zuni believing this and our believing that. What is it for someone to believe something?” With this move, “the Zuni worldview,” already an empty abstraction, vanishes. Attention turns from beliefs to propositions, understood according to “the classical conception of knowledge” first defined by Plato (2006:15), and still endorsed by philosophers like Boghossian, who practice “within the mainstream of analytic philosophy departments within the English-speaking world” (2006:7).

Boghossian has now set the stage and provided the props for a rigged philosophical analysis in which, to use Gabriel Stolzenberg’s apt expression, he will be playing “the ventriloquist to a postmodernist dummy” (2001:51). Consider, for example, the way Boghossian discusses “fact-constructivism” in chapter 3. He begins by claiming that this “constructivist thesis” is “the most influential” and

also the most radical and the most counter-intuitive. Indeed, properly understood fact-constructivism is such a bizarre view that it is hard to believe that anyone actually endorses it. And yet, it seems that many do. [2006:25]

No evidence is provided to support his claims; in fact, the definition of “fact-constructivism” offered is Boghossian’s own: “it is a *necessary* truth about any fact that it obtains only because we humans have constructed it in a way that reflects our contingent needs and interests” (2006:25; emphasis in original). It is not clear why readers should accept either his claims or his definition. But if they do, the game is up: understanding fact-constructivism “properly” will entangle them the use of Boghossian’s own “proper” definition and Platonic reasoning.

Pausing for a brief swipe at Bruno Latour, Boghossian quickly turns his attention to “fact-constructivism” in the writings of philosophers Nelson Goodman, Hilary Putnam, and Richard Rorty. Unsurprisingly, his analysis

produces “decisive reasons against its ultimate coherence” (2006:38). In chapter 4, “Relativizing the Facts,” Boghossian directly addresses Richard Rorty’s “relativistic constructivism,” and concludes that Rorty’s “global relativism about facts” is also fundamentally incoherent. Chapter 5, “Epistemic Relativism Defended,” is the longest chapter in the book. Turning once again to Rorty’s work, Boghossian says he will make the best possible case for the “equal validity” version of relativism he crafted in chapter 1. He then cites Rorty’s discussion, in *Philosophy and the Mirror of Nature* (1981), of Cardinal Bellarmine, the Vatican lawyer who prosecuted Galileo for heresy.

Bellarmino is alleged to have refused to look through Galileo’s telescope, claiming that the Bible was a better source of evidence about the heavens. Rorty asks whether, in Galileo and Bellarmine’s day, there was some “antecedent way of determining . . . what sorts of evidence there could be for statements about the movements of planets?” and he concludes that there was not:

the “grid” which emerged in the 17th and 18th centuries was not there to be appealed to in the early 17th century, at the time that Galileo was on trial. No conceivable epistemology . . . could have “discovered” it before it was hammered out. The notion of what it was to be “scientific” was in the process of being formed. [Rorty 1981:328–329 cited in Boghossian 2006:61]

Remarkably, Boghossian does *not* interpret this passage as a description of the dilemma encountered when a stabilized epistemic system (that of the Vatican) *is being challenged by an unstable new epistemic-system-in-the-making* (“science”). Instead, he says Rorty is claiming that

Bellarmine and Galileo are operating with fundamentally different *epistemic systems*—fundamentally different “grids” for determining “what sorts of evidence there could be for statements about the movements of planets.” And there is no fact of the matter as to which of their systems is “correct.” [Boghossian 2006:62]

This interpretation allows Boghossian move into territory we have already visited with Alan Sokal. Boghossian wonders “how we should characterize the alternate epistemic system to which Bellarmine is said to adhere,” and he concludes that such a system would likely contain a principle concerning revelation (2006:69). Seeking a second candidate epistemic system that might claim “equal validity” with Western science, he turns to “the Azande,” and concludes that their epistemic system would likely contain a principle concerning oracles (2006:71). “The Azande” in Boghossian’s account are ciphers, just like “the Zuni.” Odd lapses in his text suggest that Boghossian thinks the poison oracle is a person, perhaps like the Delphic Oracle in ancient Greece, which further suggests that he has not actually read Evans-Pritchard’s ethnography.

Boghossian next proposes

1. that we “accept for now the claim that Azande and the Vatican circa 1640 represent the use of fundamentally different epistemic systems,” and
2. that we accept these two systems as “what I shall call *genuine* alternatives to ours;” that is: “on a given range of propositions and fixed evidential circumstances, they yield *conflicting* verdicts on what it is justified to believe” (2006:72; emphasis in original).

If these claims are granted, he asserts,

the relativist’s argument goes through. The most that any epistemic practice will be able to say, when confronted by a fundamentally different, genuine alternative, self-supporting epistemic practice, is that it is correct by its own lights, whereas the alternative isn’t. [2006:72]

But Boghossian leaves himself a loophole: how do “we” recognize “a fundamentally different, genuine alternative, self-supporting epistemic practice?” (He will make use of this loophole in chapter 7.)

In chapter 6, “Epistemic Relativism Rejected,” Boghossian insists that “it is crucial to the relativist’s view that thinkers *accept* one or another of these systems [i.e., “science,” “the 17th century Vatican world view,” “the Azande world view”], that they *endorse* one or another of them and then talk about what they do and do not permit” (2006:88; emphasis in original). But *why?* Such endorsement may be crucial in order for *the version of “the relativist’s view” that Boghossian has crafted* to succeed. But it is not clear why readers need accept Boghossian’s version of “the relativist’s view.” Why couldn’t so-called “relativists” endorse all or part of several *different* epistemic systems, depending on the questions they were investigating? Such “epistemic cosmopolitanism” would undermine both the argument that relativism is self-refuting, and Boghossian’s favored argument that epistemic relativism produces “a vicious regress in which we never succeed in specifying the conception of epistemic justification which is supposed to constitute a particular community’s epistemic system” (2006:88–89).

Of course, Boghossian would probably respond that “epistemic cosmopolitanism” is “incoherent.” In principle, he may be correct. In practice however, much work in the last 20 years in cultural and linguistic anthropology—and in science studies—has demonstrated that “essentialist” views of societies, cultures, languages, and scientific theories are *empirically inaccurate*. Social mixing, cultural hybridization, and linguistic pidginization are all well attested phenomena. As historian of science Peter Galison has shown, moreover, scientists belonging to different epistemic communities encountered one another in the course of the Manhattan Project, and when they had difficulty communicating, they invented a “trade language,”

a pidgin, that allowed them to get on with their work successfully (Galison 1997). There was no vicious regress (but there were boundary objects)!

Galison's position is also fully in accord with the Rorty passage Boghossian chose to misread. That is, no epistemic system springs into existence fully formed; it takes time and labor for any epistemic system to be hammered out. Moreover, the process of bricolage that gave birth to the hybridized system developed by Manhattan Project physicists may be standard for the emergence of *any* epistemic system whatsoever. In addition, Barbara Herrnstein Smith shows that so-called "relativists" like historian Carl Becker or microbiologist Ludwik Fleck held views that are more accurately described as "reflexive," a point that "needs emphasis in view of the routine charge that relativists, in (assumedly) claiming absolute truth for their own relativist views, are self-refuting" (Smith 2006:27). Boghossian might try to avoid this outcome by insisting that "science" is an epistemic system unto itself (e.g., 2006:85). Galison, however, would disagree, as would those contemporary science studies scholars who highlight "the disunity of science" (Galison and Stump 1997). As Galison recently observed, "the study of science (singular and universal) has begun to seem a bit like an all-out effort to make a theory of all the world's red objects. Possible, I suppose, but not the most illuminating task to undertake" (2004:379–380).

In chapter 7, Boghossian returns to the task of identifying the attributes that any "fundamentally different, genuine alternative, self-supporting epistemic practice" would have to possess in order for us (whoever "we" are) to concede that "we wouldn't be able to justify ours over it" (2006:96–97). But this project founders if we do not accept his assumption that competing epistemic systems are self-contained, come only one to a community, and do not overlap. Ironically, he refutes "the 17th century Vatican world view" by arguing that "*Pace* Rorty [sic], it is hard to understand the dispute between Galileo and Bellarmine as a dispute between epistemic systems . . . . It is rather a dispute, within a common epistemic system, as to the origins and nature of the Bible" (2006:104). Boghossian then hypothesizes that any apparent contradiction between "the Azande world view" and "ours" is a case of mistranslation, turning on whether or not the Azande use basic logical operators like *if*, *and*, and *or* in the same way we do (2006:106–109). Boghossian neglects, however, to tell us in which language(s) the Azande are using these logical operators, or whether translating logical operators from English to Azande or Azande to English might raise any difficulties of interpretation. What if the Azande are bi- or multilingual? Or is such a possibility "incoherent?" At this point one is tempted to ask Boghossian, "What is your theory of language?"

Boghossian concludes that relativistic arguments "which draw on the alleged existence of alternative epistemic systems . . . do not ultimately withstand critical scrutiny," which for him leaves only one option: "to think that there are absolute, practice-independent facts about what beliefs it would be

most reasonable to have under fixed evidential conditions” (2006:109). Maybe so. But the route he took to reach this conclusion seems, to borrow Peter Saulson’s words, “disappoint[ing]” and “misguided:” like Sokal, Bricmont, and Steven Weinberg, Boghossian has undertaken “an almost Scholastic exercise,” ignoring “the credo of science to respect how the world really is and instead arguing [his] case from first principles” (Saulson 2001b, 231). Boghossian’s attempts in chapter 8 to refute “strong constructivism” and “weak constructivism” in science studies founder in the same way. Once his assumptions and carefully crafted definitions and tortuous logical manipulations fail, so do the refutations of “relativism” that depend on them.

In chapter 9, his brief epilogue, Boghossian argues that “social constructivist thought” appeals to activists because it is “hugely empowering.” He insists, however, that using such discourse evinces a “fear of knowledge” and a “felt need to protect against its deliverances” (2006:129). The only polite way to describe this conclusion is as projection. Boghossian is the fearful one here, protecting himself from “relativism” and “constructivism” by retreating into what Latour calls “the solid rock castle of modernism” (2004:227 n4). Given his self-proclaimed “blind entitlement” to use “his own epistemic system,” perhaps Boghossian’s theory of language is just like Humpty Dumpty’s: “When I use a *word*,” *Humpty Dumpty* said, in rather a scornful tone, “it means just what I choose it to mean—neither more nor less.”

### COHERENCE AND CONTEXT WITHOUT FEAR

An old saying—“the problem of knowledge is the scandal of philosophy”—inspired the title for Barbara Herrnstein Smith’s *Scandalous Knowledge* (2006:1). The book has seven chapters; all or part of chapters 3, 4, 6, and 7 were published previously, sometimes in slightly different form. Every chapter is based on careful study and reflection on the texts that Sokal, Boghossian, and their fellow travelers fear, denounce, and dismiss. The first three chapters set out the constructivist point of view to which Smith is committed. Chapter 4 surveys some of the contradictions that appear in the texts of scholars in the 1990s who sought to pursue a narrow middle ground between traditional epistemology and so-called “relativist” critique. Smith argues, however, that the “fundamentally equivocating hybrids” she describes, “can do little theoretical work . . . . Conversely, what gives many of the ‘extreme’ proposals their conceptual power is, among other things, precisely their *extremity* . . . the unhedged explicitness of their questioning or rejection of various traditional ideas and the consistence of the alternative ideas they develop” (2006:90).

That constructivist proposals “have been misunderstood, misrepresented and greeted with ridicule, distress, and or outrage” (2006:3) does

not surprise Smith, because she views constructivism as “the major rival to the realist-rationalist-representationalist understandings of cognition that continue to dominate formal epistemology, analytic philosophy of mind and mainstream philosophy of science” (2006:25). This, of course, is the formal epistemology that underlies Sokal’s and Boghossian’s “theory of language,” in which meaning is understood to be encapsulated within individual propositions, i.e., sentences. As we saw, Sokal and Boghossian are aware of the chief problem associated with this approach to knowledge: David Hume’s skeptical argument that anyone committed to it can never be sure that he knows anything! Sokal and Bricmont try to banish this problem by arguing that “postmodern relativism” derives from “sophomoric” or “radical,” or “extreme” skepticism, which in turn is a form of solipsism (2008:176). They concede that solipsism cannot be defeated, but they insist that “*mere fact that an idea is irrefutable does not imply that there is any reason to believe it is true*” (2008:176, emphasis in original; see also Sokal 2008:99, 111). This principle allows them to reiterate classic philosophical responses to skepticism, based on the “intuitive view” (to use Boghossian’s expression [2006:130]) that “[t]he most natural way to explain the persistence of our sensations...caused by agents outside our consciousness” (Sokal 2008:176).

Smith is not impressed with this maneuver, and points out that it has been challenged by a long line of philosophers, historians, and social scientists since the end of the 19th century (2006:48). Fortunately, there is another option: to take the constructivist approach to the problem of knowledge by *radicalizing skepticism*: “to question, re-evaluate, and, as necessary, revise the system of ideas, definitions distinctions, principles, and undertakings in which the concept of knowledge or cognition has been classically situated in Western thought” (Smith 2006:2). The constructivist perspective cannot be encapsulated in an abstract proposition (the way Boghossian encapsulates “the social construction of knowledge,” for example). Smith defines constructivism in a long paragraph that assembles components gleaned from work by a variety of scholars and theorists, and which deserves to be quoted in full:

In contrast to...“realism”, constructivist accounts of cognition, truth, science, and related matters conceive the specific features of what we experience, think of and talk about as “the world” (objects, entity-boundaries, properties, categories, and so forth) not as prior to and independent of our sensory, perceptual, motor, manipulative and conceptual-discursive activities but rather, as emerging from or, as it is said, “constructed by” those activities. In contrast to the prevailing assumptions of rationalist philosophy of mind, constructivist accounts of cognitive processes see *beliefs* not as discrete, correct-or-incorrect propositions about or mental representations of the world but, rather, as linked perceptual dispositions and behavioral routines that are

continuously strengthened, weakened and reconfigured through our ongoing interactions with our environments. In contrast to referentialist views of language, constructivist accounts of *truth* conceive it not as a matter of a match between, on the one hand, statements of beliefs and, on the other, the autonomously determinate features of an altogether external world (Nature or Reality), but rather, as a situation of relatively stable and effective mutual coordination among statements, beliefs, experiences and practical activities. And, in contrast to logical positivist or logical empiricist views, constructivist accounts of specifically *scientific* truth and knowledge see them not as the duly epistemically privileged products of intrinsically orthotropic methods of reasoning or investigation (“logic” or “scientific method”) but, rather, as the more or less stable products of an especially tight mutual shaping of perceptual, conceptual, and behavioural (manipulative, discursive, inscriptional, and other) practices that have especially wide cultural, economic, and/or political importance. [2006:3–4; emphasis in original]

A key feature of this definition is the way it emphasizes constructivist respect for reality and truth. All the same, the “realist attitude” encouraged by constructivism is “at once, more stringent and completely different from the so-called realist philosophy of science,” as Bruno Latour observes (2004:234). It is worth emphasizing that, for constructivists, neither logical analysis from first principles nor the application of a single scientific method *by themselves* can guarantee secure access to reality and truth. Rather, our beliefs *emerge* and *stabilize* over time, as they are tested and justified *on an ongoing basis*, “contingently shaped and multiply constrained” by a range of heterogeneous factors (*not excluding* logic or scientific method!), in the course of our regular interactions with the world (2006:11).

Smith emphasizes that “constructivism is often conflated with “social constructionism,” and that “[d]istinguishing between the two is difficult because both terms have shifting contemporary usages and variants . . . and because the views and enterprises they name have complex intellectual-historical connections” (2006:5). However, three key features identify the constructivist perspective she defends:

1. “the ongoing questioning of standard understandings and treatments of such terms as *fact*, *discovery*, *evidence*, *proof*, *objectivity*, and of course, *knowledge* and *science* themselves,” based on the idea that meanings in language emerge and change in history (2006:7; emphasis in original);
2. “the idea that terms and concepts operate as elements of larger systems or *networks* (the term is recurrent) of assumptions, beliefs and conceptual-discursive practices that are both densely interconnected and, for that reason, and others, powerfully normative” (2006:7; emphasis in original); and
3. an emphasis on the *heterogeneous* components that link up and mutually coordinate in networks, including “*conceptual-discursive* elements (ideas,

definitions, distinctions, predications, and so forth) and both *perceptual-cognitive dispositions* (observations, classifications, interpretations and so forth) and *material practices*" (2006:8; emphasis in original).

Features 2 and 3, for example, distinguish the constructivism of Latour's actor network theory from the narrower social constructionism of Harry Collins.

Thus, constructivism has *nothing* in common with irrational, anti-scientific, "anything-goes," "equal-validity" "relativism." Nor is Smith persuaded that constructivism amounts to a totally new paradigm destined to replace traditional understandings of knowledge and science (2006:12). For one thing, as she shows in chapter 2,

one could claim as pre-"postmodern relativists" all those from Heraclitus and Montaigne to Alfred North Whitehead or Ludwig Wittgenstein who have questioned ideas of epistemic moral, or ontological fixity, unity, universality or transcendence and/or who have proposed correspondingly alternative ideas of variability, multiplicity, particularity or contingency. [Smith 2006:18]

The first third of the 20th century was full of "relativists" in many disciplines, including Franz Boas and Edward Sapir in anthropology. Of all these early "relativists," however, the least known and most important, in her view, was Ludwik Fleck, the Polish microbiologist and historian-sociologist of science, and author of *Genesis and Development of a Scientific Fact* (1979[1935]). Fleck is the hero of chapter 3, "Netting Truth," the longest chapter in Smith's book. Readers need to pay close attention to this chapter in order to understand not only what "constructivism" is, but also why Smith and other constructivists hold Fleck in such high esteem. Bruno Latour calls Fleck "the founder of science studies" (Latour 2004:234 n14), and Smith traces a tradition emerging from his work that includes "Thomas S. Kuhn, Michel Foucault, David Bloor, and Bruno Latour" (Smith 2006:3).

The "scientific fact" mentioned in the title of Fleck's book is the "Wasserman Reaction," a chemical test that indicates the presence in a blood sample of the syphilis microorganism, *Spirocheta pallida*. However, the emergence of this complex scientific fact is entangled with another phenomenon: the genesis and development of the concept of syphilis itself. As Fleck shows, both emerged historically "from the *reciprocally shaping and sustaining* activities" of physicians, scientists, technicians, changing tools and techniques, the varied interests of scientists, politicians, and ordinary citizens in finding the cause of a particular physical disorder, and popular and scientific beliefs about sex, sin, and the human body (Smith 2006:50). Fleck argued that the entire fluctuating network of which these elements were nodes "is called reality or truth." As Smith observes, "For [Karl] Popper, the net,

an individually conceived conjecture, may *catch* truth. For Fleck, the net . . . is truth” (2006:51; emphasis in original). Fleck also identified “truth” with “what we otherwise experience as stable, resistant and real,” a phenomenon he also described as a “harmony of illusions.” Fleck does not subscribe to a correspondence theory of truth, but his view hardly amounts to “a denial of external reality”:

It is, rather, that the specific features of what we interact with *as* reality are not prior to and independent of those interactions but emerge and acquire their specificity *through* them.” [Smith 2006:51; emphasis in original]

“Fleck’s reconceptualisations of *truth*, *facts*, *science*, and *reality* are not simple,” Smith cautions, but “there may be no better way to begin to appreciate the power of constructivist views of knowledge and their role in contemporary science studies than through an attentive reading of *Genesis and Development*” (2006:53; emphasis in original).

From my perspective, one of the most important (and systematically overlooked) features of Fleck’s constructivism is the *absence* of “radical discontinuities—‘revolutions’ or ‘ruptures’—in his accounts of intellectual history . . . . Fleck’s views . . . are attentive to subtler and more heterogeneous processes and effects” (Smith 2006:65). That is, the “thought collectives” of which Fleck writes were *misunderstood* by Kuhn and his followers to be separate, self-enclosed communities of scientists committed (blindly) to a single paradigm—or even a single *theory*. Fleck’s view, by contrast, is that scientists belong to many different thought collectives *simultaneously*. Unlike the views of Kuhn and his followers, therefore, Fleck’s views “do not imply either the imprisonment of thought as static, self-confirming circularity or the churning out of robotic individuals doomed to social conformity” (Smith 2006:67). On the contrary, in Fleck’s account, scientists themselves turn out to be boundary objects: “Intellectual innovation arises continuously from the ongoing communication of ideas by individuals moving between different collectives (for example, different scientific disciplines)” (Smith 2006:67). According to Fleck, “where the organization is democratic . . . thought develops responsively or, one might say, progressively. The best example of such conditions, Fleck observes, is ‘modern natural science’” (Smith 2006:68).

This feature of constructivism is central because, as we saw earlier, Sokal and Boghossian’s “anti-relativist” and “anti-constructivist” arguments *rely on* radical discontinuities among different epistemic systems. Indeed, in *Beyond the Hoax*, Sokal is suspicious of “critical traditionalism,” the hybrid scientific outlook suggested by Ashis Nandy that “refuses to give primacy to the needs of pure cognition at the expense of totality of consciousness” (Nandy 1987:125, 124 quoted in Sokal 2008:304). Sokal reads Nandy as advocating “syncretism,” which Sokal defines as “incorporating selected elements from modern science while rejecting its worldview,” and which he dismisses.

Sokal also confesses that he personally “lean[s] strongly toward caution in interdisciplinary endeavors . . . despite this, I have decided to stick my neck out an inch or two, because of the importance of the issues at stake” (2008:xi). But how would Sokal describe the result of his (partial) engagement outside his own field? He insists that “The only solution . . . is to pay less attention to credentials and more attention—*critical* attention—to the content of what is said” (2008:xii; emphasis in original). But if “content” means “propositional content;” and if the disciplinary insularity he wishes to protect is, in fact, *not* characterized by radical discontinuity from other disciplines, *then his own position becomes “incoherent”*—he is in fact engaging in pidginization without admitting it, or acknowledging the consequences. Perhaps he needs a constructivist theory of language.

Smith is unconvinced by arguments insisting that “moral responsibility in our time” requires “an untroubled faith in the simplicity and stability of truth;” that revealing complexities and ambiguities simply “makes life easy for liars and charlatans” (Smith 2006:46). On the contrary, she insists that

constructivist understandings of truth and knowledge . . . would not render one unable or unwilling to compare or judge divergent truth- or knowledge-claims . . . . To be sure, one would not . . . be inclined to proclaim the absolute, objective, or universal validity of any of these (or other) claims . . . . But one would certainly be inclined and equipped to affirm the superiority of one such claim over the other with regard to the various epistemic dimensions indicated above (congruence, connectibility, effectiveness, appropriability, extendibility, and so forth) and to argue the relevance of those dimensions to the purpose at hand. [Smith 2006:14]

Smith acknowledges that Fleck himself did not express any particular political views with respect to scientific practice, but she emphasizes that the portrait he provides of modern science is “neither idealized nor especially (self-) congratulatory” (Smith 2006:69). Nor are his views “‘cynical’ or even, for better or worse, ‘critical.’” Thus, he sees the typical ‘hero worship,’ self-effacement, ‘reverence for number and form,’ and arcane language of modern science as, again, emergent traits and effective norms, not delusions to be eradicated, bad habits to be foresworn, or stratagems to be exposed” (Smith 2006:69).

In the final three chapters, Smith uses her constructivist lens to examine three areas of contemporary scholarly concern: disputes between the sciences and the humanities, the claims of sociobiology and evolutionary psychology, and issues surrounding the rights of animals. Anthropologists should be particularly interested in chapter 6, where she offers incisive close readings of two evolutionary psychology texts, *The Adapted Mind* (Barkow, Cosmides, and Tooby 1992) and *How the Mind Works* (Pinker 1997). More important, however, is her subsequent discussion of several active research

traditions in biology, psychology, and cognitive science that currently *challenge* the claims of evolutionary psychologists (and which evolutionary psychologists regularly ignore), including work by Jean-Pierre Changeaux, Gerald Edelman, Antonio Damasio, Terrence Deacon, Philip Lieberman, Susan Oyama, and Rodney Brooks. Interestingly enough, Sokal and Bricmont seek support for classical epistemology in neo-Darwinian evolutionary theory, arguing that “the possession of sensory organs that reflect more or less *faithfully* the outside world (or, at least, some important aspects of it) confers an evolutionary advantage. Let us stress that this argument does not refute radical skepticism, but it does increase the coherence of the anti-skeptical worldview” (Sokal 2008, 177, n12, emphasis in original; see also 403 n60 and 424).

In Smith’s chapter 7, “Animal Relatives, Difficult Relations,” she seeks out as many points of view (and contradictions!) as she can find in current debates about animal rights, asking at the end: “Well...where does that leave us? Who is friend, who is enemy here...And are we sure, in all this, that we know—or agree—who ‘we’ are?” (2006:165). Her answer is that questions like this “cannot be answered at all,” for,

as the constructivist perspective makes us see, such questions restate the fundamental difficulties involved in any attempt to determine in a formally principled or univocal way...our relations to other creatures. This is not, in my view, a despairing observation. On the contrary, what it indicates is the necessary openness of these questions to ongoing address. [2006:165]

Or, as she put it in chapter 2, “I suggest that we accept the task of operating in the work in accord with our most profound convictions and values, and with all the consciousness we can muster of the limits of our knowledge, our sagacity and our righteousness” (2006:39). If this is “relativism,” then Melville Herskovits was right: it is indeed a tough-minded discipline.

## CONCLUSION

The traditional assumptions of Western philosophy are venerable, but they have become increasingly problematic to many scientists and scholars in a range of disciplines (including anthropology) over the course of the 20th century. The goal of the logical positivists to secure objective knowledge came to naught, undone by the nagging challenge of skepticism. So long as the goal of science or philosophy is taken to be absolute, unchanging, objective truth—and so long as it is believed that truth comes parceled out in individual sentence-length propositions—it seems that such failure is unavoidable. It also seems that the more fully one is committed to the traditional views of Western philosophy, the less possible it is to imagine that

any other philosophical approach could be rational or coherent, let alone capable of providing superior solutions to philosophical problems.

At the same time, one of the great attractions of Western science has always been the claim that it is open-ended and self-correcting—that its truths are the best we can come up with at the moment, but thanks to the means by which they were established, they deserve our respect, even though they are likely to change in the future. And, of course, if we understand scientific practices to be embedded in wider networks that reach outside the laboratory into government and industry and social relations of gender and race and class (among other things), we must also recognize that epistemic claims are never likely to be considered only on their own terms, however desirable that might seem.

Making do with truths that are not absolute is not necessarily problematic, of course, unless or until scientific truths so understood are challenged by proponents of alternative epistemic systems that *do* claim to offer absolute truth. Political, economic, or religious opponents of some kinds of scientific findings (e.g., global climate change) have learned that their interests may indeed be served if they can persuade the public that closed scientific controversies are still open.

Contemporary scholars must grapple with these challenges. Our situation can be made to seem hopeless, prompting nostalgia for the good old days of 50 years ago, when science was thought of as unified, when the scientific method was thought to be the same everywhere, and when the coherence or incoherence of scientific *theories* was the only thing that mattered. Except we know that this period did not end well. Science studies has developed over time in useful directions, insofar as its focus has turned from theory to practice; from arguments about propositional truth to ethnographies of scientists in action; and from idealized generalizations about the unity of “science” and a single “scientific method” to practical grappling with the diversity of concrete sciences and the specific methods that make them successful. Science studies borrowed a lot from traditional anthropology, including the relativity of perspectives engendered by encounters with differences that seem unprecedented and baffling. Anthropology in the 21st century stands to benefit reciprocally from the achievements of science studies.

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