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Constructivist and Relativist Conceptions of Knowledge in Contemporary (Anti-)Epistemology: A Reply to Barbara Herrnstein Smith

This ought to be an exciting time for academic philosophy, for we are witnessing today a virtually unprecedented level of interest in philosophical issues across a virtually unprecedented range of academic disciplines. Why, then, instead of a sense of intellectual euphoria do we find academic philosophy experiencing an increasingly uneasy—some might even say, hostile—relationship vis-à-vis the rest of the humanities and social sciences?

When I was a graduate student in the early 1980s, the explanation that was most commonly offered was that academic philosophy, being primarily analytic in orientation, did not concern itself sufficiently with issues that really mattered, that it was overly preoccupied with the analysis of science, language, and knowledge and not concerned enough with literature, culture, and the life that most people lived.

It is rare to hear this complaint these days. As the philosophical interests of humanist scholars have turned increasingly to science itself and to the authority that it is accorded in contemporary society, the complaint most often heard is not that analytic philosophy is not interested in the

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right things, but that it is not interested in them in the right way. In particular, the most influential charge is the one that forms the backdrop to Barbara Herrnstein Smith's discussion in her essay—that analytic philosophy is in the grip of an inadequate conception of the nature of human knowledge, a conception that it cleaves to in the face of compelling objections that it resolutely ignores. As Smith puts it,

> Clearly there are significant contemporary challenges to classical epistemology and mainstream philosophy of science: new ways of answering classic questions concerning the formation and validation (or is it contingent stabilization?) of belief, new questions about the nature and operations of scientific knowledge, and new assessments of the role of academic philosophy both in posing such questions and in grounding or adjudicating their answers. These challenges are by no means recent in origin. Some have been part of the philosophical tradition since Protagoras; . . . yet others have emerged during the course of the twentieth century from research and analysis in the scientific disciplines themselves, for example, in quantum theory and, more recently, in developmental biology and cognitive science. Work in all these fields has indicated the need to review and, to some degree, revise traditional ideas and conventional wisdom . . . about knowledge, science, and cognitive processes. At the same time theorists and scholars in various relatively new fields, including feminist epistemology and constructivist history and sociology of science, have pressed these challenges with especially aggressive energy and in quarters quite close to home—that is, in academic philosophy itself.

There can be little doubt that vast numbers of scholars in the humanities and social sciences feel that important discoveries about human knowledge have been made by recent thinkers, discoveries that mainstream analytic philosophy ignores at its peril. The putative discovery goes by a number of different labels. Sometimes it is described as the view that all knowledge is "situated"; sometimes, as the view that all knowledge is "socially constructed." I will refer to the family of views gestured at here as "constructivist" conceptions of knowledge. The core idea is that, in contrast with the traditional theory of knowledge—what Smith calls "classical epistemology"—knowledge is not thought of as detached from the contingent social, cultural, and political context in which it is produced.

Even the most cursory acquaintance with the literature to which Smith is alluding shows that, while the protest against classical epistemology has reached the level of a roar, analytic philosophy has, for the most part, turned it a deaf ear. Why?

The analytic version, if I may be permitted to generalize freely, is that we often do not understand what alternative to classical epistemology is being proposed, and that when we do understand what is being proposed, it seems to amount to some species of relativism, a view about knowledge that we believe to have been discredited some time ago.

According to our critics, on the other hand, the case for revising traditional conceptions of knowledge has been overwhelmingly clear for quite a while, and nothing but the usual intransigence of established orthodoxy can explain the resistance with which these new ideas have been greeted.

What about the charge that constructivist views appear to entail an unpalatable relativism? There are two possible replies to the charge, both of which are represented in the literature. The first maintains that a constructivism about knowledge, properly understood, simply does not entail relativism. Thus Sandra Harding says (and is quoted by Smith as saying) that it is possible to maintain that "all knowledge is socially situated versus the conventional idea that beliefs count as knowledge only when they break free [of] . . . local, historical interests, values, and agendas [yet still do not] slide [into] relativism."[1]

The other possible answer is that while constructivism does entail some form of relativism, it is not of the objectionable variety. This is the tack taken by Lorraine Code: "Yet the relativism that my argument generates is sufficiently nuanced and sophisticated to escape the scorn—and the anxiety—that 'relativism, after all' usually occasions."[2] Which of these responses does Smith favor? It is hard to figure out. On one hand, one of the central themes of her essay is that constructivist epistemology is characterized by "equivocation": "For if one endorses a constructivist understanding of 'human involvement in the world' as *constitutive*, then one cannot consistently retain the 'epistemic deference' to a presumptively *autonomous* reality that generally defines realism. It is this sort of elaborated affirmation of mutually incompatible doctrines or, in the name of middle-road moderation, the simultaneous or rapidly oscillating avowal and disavowal of both traditional and more or less radically revisionist positions that I shall discuss here as 'equivocation.'" This suggests that Smith thinks that there is something rela-

tivistic about constructivism and, hence, that constructivists who deny that are engaged in the "elaborated affirmation of mutually incompatible doctrines." On the other hand, she chastises Lorraine Code for being overly concerned about the charge of relativism, on the grounds that "there is reason to think that the foolish or dangerous ideas commonly scorned *as* 'relativism'—the idea, for example, that all beliefs or accounts are equally valid (under all conditions, from all perspectives) or that the world can be constructed just as we choose—constitute a phantom heresy, without visible, palpable, or citable adherents."

How can this seeming inconsistency be resolved? It seems likely that Smith is best read as claiming that there is *one* sense in which constructivist views *are* relativistic and *another* sense in which they are not. In the sense in which constructivist views are relativistic, there is no point denying that they are, for to do so would result in the affirmation of "mutually incompatible doctrines"; however, there is also no *need* to deny that they are relativistic in that sense, for there is nothing objectionable about the implicated brand of relativism. As for the sense in which constructivist views are *not* relativistic, there is also no need to expend much effort denying that they are, for "there is reason to think that the foolish or dangerous ideas commonly scorned as 'relativism' . . . constitute a phantom heresy, without visible, palpable, or citable adherents." Either way, then, according to Smith, constructivist epistemology's recent preoccupation with the charge of relativism is misplaced.

If this is even roughly what Smith has in mind—and if it is not, then I have no idea what she has in mind—it seems to me to be a very interesting claim. If true, it will have established an important result, for it will have taught us how to understand constructivism in such a way that the normal concerns about it are silenced.

However, I do not really see by what supporting argument she seeks to establish the interesting and important claim. A properly convincing argument for it, it seems to me, would have to contain the following elements: First, there would need to be a clear statement of exactly what a constructivist view of knowledge is. Second, the two distinct senses of relativism, the objectionable and the nonobjectionable, would have to be defined. Third, it would have to be shown that while the first of these is objectionable, the second is not. Finally, there would need to be some demonstration that while the second thesis is entailed by constructivism, the first is not.

Of the three theses that are in play, the only one that is actually defined is

the "bad" relativism that is allegedly not entailed by constructivism. This is formulated as the view that "all beliefs or accounts are equally valid (under all conditions, from all perspectives)." But that is *not* the thesis that concerns philosophers when they worry about the relativistic consequences of constructivism. Rather, the relativism that concerns them is the view that beliefs or accounts can be said to be valid only relative to *particular* perspectives and under *particular* conditions, not with respect to *all* perspectives and under *all* conditions. So showing that *Smith's* version of bad relativism is not entailed by constructivism would do nothing to meet the concerns about constructivism that people actually have.

What I think I can most usefully do in these comments is try to explain, as clearly as I can, why constructivist views of knowledge have been generally considered problematic by the community of academic philosophers. I will aim to achieve some clarity about the substantive issues that divide most analytic philosophers today from their philosophically minded colleagues in the humanities and social sciences.

As I mentioned above, I take the core idea of a constructivism about knowledge to be the view that knowledge must not be thought of as detached from its social, cultural, and political context. As feminist philosopher Kathleen Lennon puts it, "Feminist epistemologists, in common with many others strands of contemporary epistemology, no longer regard knowledge as a neutral transparent reflection of an independently existing reality, with truth and falsity established by transcendent procedures of rational assessment. Rather, most accept that all knowledge is situated knowledge, reflecting the position of the knowledge producer at a certain historical moment in a given material and cultural context."[3]

The question is, What aspect of classical epistemology does this assertion contradict? Well, what conception does classical epistemology have of the relation between knowledge and the political and social context in which it is produced?

Does classical epistemology deny that knowledge is often produced collaboratively, by members of a social group? No. Does it deny that scientists have political and social values and that those values *may* influence what questions they ask and what they end up believing? No. Does classical epistemology have a view about how often in the course of history beliefs have

been shaped by political and social considerations as opposed to other types of considerations? No.

What matters to classical epistemology are three things: first, the claim that only some considerations can genuinely justify a belief, namely, those that bear on its *truth*; second, a substantive conception of the sorts of considerations that qualify for this normative status—observational evidence and logic, for example, but not a person's political commitments; and finally, the claim that we do sometimes believe something *because* there are considerations that justify it and not as a result of some other cause, such as because it would serve our interests to do so.

If we let "political values" stand as shorthand for Harding's "local, historical interests, values, and agendas," and if we legislate some terminology and say that a "reason for a belief" is any consideration that may *correctly* be invoked to recommend the belief, we may encapsulate the classical conception of knowledge as follows: (1) The only considerations that are *reasons* for believing something are those that bear on the *truth* of the belief. (2) A group's political values do not bear on the truth of an arbitrary belief about the world. So political values cannot be reasons. (3) Sometimes the correct causal explanation for why we believe something is that we have a reason for believing it. If constructivism is to dispute something that matters to classical epistemology, then it must be one of these propositions. Which one? There are three possible constructivist options: (A) It is not correct to say that the only considerations that can genuinely recommend a belief are those that bear on its truth. (B) Political values do bear on whether beliefs are true. (C) Reasons for belief cannot fully explain why we believe what we believe; political values must be appealed to as well.

I think we can be very short with option (A). To believe that *p* is to believe that *p* is true: these two phrases are platitudinously equivalent. To hold that considerations that are manifestly irrelevant to the truth of a belief may nevertheless be correctly invoked to recommend it is simply to misunderstand the sort of state that belief is. It is as if I were to say, "Look, I know that the fact that it will advance my career if everyone were to believe Maxwell's equations for electromagnetism is irrelevant to whether the equations are true, but I nevertheless think that it's a reason for believing them." As far as I can tell, Smith would agree with me about this (though a remark disapproving of the analysis of knowledge as justified true belief gave me pause).

It is tempting to be short with option (B) as well. How it is possible for anyone's political values to be relevant to the question of whether, say, Maxwell's equations are true? Perhaps if I am heavily invested in their being true, my interests will cause me to *find it plausible* that they are, but that is not the same thing as their giving me a *reason* to find them plausible.

Tempting as it may be, there is a somewhat subtler version of this view that needs to be addressed separately, if only because it has had such a long history. The thought behind the subtler version is not that political values justify beliefs directly, but that a particular consideration can have the status of a reason for a belief only against the backdrop of a particular set of political values. Nothing can *be* a reason for a belief, in this view, except relative to a particular background politics; vary that background and you vary what is and is not a reason for believing something. This is the view that has traditionally gone by the name of relativism—about justification rather than truth, in this instance—for it relativizes the existence of a reason to the existence of an appropriate political perspective.[4]

As a view about the nature of reasons, relativism has been around for a long time. It has failed to find many supporters within the mainstream of epistemology, however, because most philosophers became convinced long ago, by classic arguments to be found in Plato, among others, that the view is inherently self-undermining, that we cannot coherently think of ourselves as believing and asserting anything if all reasons for belief and assertion are held to be tied to background perspective in the way that the view envisions.

There are many ways to show this, but the simplest is also the most well known: Is a relativism not about reasons violated by the relativist's own stance toward his or her own view? Surely the relativist does not think that relativism is justified only relative to his or her own perspective? If he or she did, why is he or she recommending it to us, who do not share his or her perspective? Consider the claim central to Smith's outlook, quoted above: "Work in all these fields has indicated the need to review and, to some degree, revise traditional ideas and conventional wisdom . . . about knowledge, science, and cognitive processes."

Clearly, this is not the assertion of someone who is prepared to say, "Relative to my political perspective, it is justified to say that recent work has indicated the need to review and revise traditional views about knowledge; however, if you happen not to share that perspective, you may ignore everything I say, for I am not claiming that it is justified relative to *your* perspective." On

the contrary, the intended force of the assertion is, rather, this: Recent work has shown that conventional ideas about knowledge are flawed, so anyone, regardless of his or her prior commitments, who wants to believe what is true had better change his or her mind about the nature of knowledge. If this were not the intended force of the assertion, why the tone of rebuke?

I conjecture that one of the reasons why a relativism about reasons can sometimes seem compelling to those who hear it is that they hear it as making the rather different claim that what people *think* of as reasons for believing something can vary with their politics. This latter view, which is really an empirical conjecture about mental dispositions rather than a philosophical view about the nature of good reasons for belief, may or may not be true. It is certainly no part of classical epistemology to deny that it is true. But whether or not it is true has nothing to do with the philosophically controversial claim that lies at the heart of constructivism, namely, that what *is* a good reason—not merely what is *thought* to be a good reason—can vary with one's background political values.

At various points in her essay Smith suggests that there is something question-begging about the classical self-refutation arguments that are directed against relativism, that they depend in some unspecified way on the very system—classical epistemology—that relativism is supposed to place in question.

But what do these arguments presuppose? They presuppose nothing more than the assumption that when we believe something, we believe it because we think there are reasons to think it is true—reasons that we perceive as general enough to be obvious even to people who do not share our perspective, so that we feel entitled to recommend it to them. In other words, they presuppose nothing more than the very attitude that Smith adopts toward her own claim that there is something seriously wrong with classical epistemology. Can we really imagine a way of thinking about belief that will leave no room for that attitude?

It is time we turned to the third possible way of prosecuting a constructivist view of knowledge, the option I labeled (C) above. According to this family of views, political values play an indispensable role in explaining why we believe what we believe, not because reasons are *constituted* by political values but, rather, because we cannot be moved to belief purely by reasons. On this account of the matter, we preserve the distinction between factual evidence and political interests, but we insist that one cannot fully explain

why something is believed without adverting to political factors. Once more, the view comes in two flavors, one stronger than the other.

The stronger, more extreme view would have it that factual evidence plays no role whatsoever in the explanation of belief, that although values do not justify a belief, we are not in any way moved to belief by considerations that justify; we are only moved by our political interests.

Bizarre as this view may seem, it is practically orthodoxy among sociologists of science. On the most charitable reading, it stems from an innocent confusion about what is required by the enterprise of treating knowledge sociologically.

Some twenty years ago, when the idea of a sociology of science was first beginning to take root, David Bloor argued as follows: If we wish to explain why certain beliefs come to be accepted as knowledge at a given time, we must not bring to bear our views about which beliefs are true and which false. If we are trying to explain why *they* came to hold that some belief is true, it cannot be relevant that we know it not to be true. This is one of the so-called Symmetry Principles of the sociology of knowledge: treat true and false beliefs symmetrically in explaining why they came to be believed.

One can debate the merits of this principle, but for present purposes I propose to grant it. However, it is one thing to say that true and false beliefs should be treated symmetrically and quite another to say that justified and unjustified ones should be so treated as well. While it may be plausible to ignore the truth or falsity of what I believe in explaining why I came to believe it, it is not plausible to ignore whether I had any *evidence* for believing it. For some reason that is never explained, however, Bloor and his colleagues seem to think that the two principles are on a par and that both are equally required by the enterprise of treating scientific belief sociologically. Bloor builds *both* principles into the very foundation of the subject: The sociology of knowledge "would be impartial with respect to truth and falsity, rationality or irrationality, success or failure."[5]

The intuitive view, however, is that I sometimes believe something precisely *because* I have a good reason for believing it—as, for example, when I come to believe that there is someone at my door because I hear a knock. To insist that for every belief that I have there must be some *alternative* way of explaining why I came to have it, one that does not mention my evidence for it, is to insist that reasons can never constitute the real explanation for why I came to believe what I believe.

But what is the argument for this astonishing claim? And how could there be an argument for it that did not immediately undermine itself? Surely anyone promoting the view that reasons cannot move people to belief would presumably have to represent themselves as having come to *that* view precisely *because* it is justified.

None of this is to say, of course, that all belief—or even all scientific belief—must be explained in terms of the compelling evidence assembled for it. The history of science is replete with theories—phrenology, for example—for which there never was any good evidence. It is simply to insist that scientific belief is *sometimes* to be explained in terms of compelling evidence and that the history and sociology of science, appropriately conceived, need have no stake in denying that.

Smith has a number of strange things to say about this nexus of issues. First, there is her account of the symmetry postulate itself. She rebukes Lorraine Code for misunderstanding

> the frequently mentioned but evidently rarely read "symmetry postulate" of Edinburgh-based . . . sociology of science. The postulate, which has nothing to do with tolerance in any of the usual senses of the term, maintains not that all constructions of reality are equally *worthy* but, rather, that the *credibility* of all constructions of reality, including those now commonly accepted as true or reasonable, should be regarded as equally *needful of explanation* and as explicable, in principle, by *the same general types of causes*. A key methodological point of departure—not epistemic judgment—in contemporary science studies, the symmetry postulate is routinely transformed into a fatuous egalitarianism (everything is equally true, good, worthy, valid, and so forth) by those who encounter it primarily through hearsay.

However, the formulation of the symmetry postulate quoted above, which I am happy to say is taken from Bloor's actual text and is not based on mere hearsay, commits the sociology of knowledge to the view that it must always be possible to explain any belief without having to mention either its truth *or* its reasonableness. It is not the much milder view that Smith seems to think it amounts to, that no belief can be presumed *intrinsically* credible or self-evident. This latter claim insists only that no belief be adjudged reasonable without some supporting evidence; it has nothing to do with the radical proposal, clearly endorsed by Bloor and other supporters of the "Strong Pro-

gramme," that it should always be possible to explain any belief in terms that do not mention its reasonableness.

Another criticism of the symmetry postulate is forcefully expressed by philosopher of science John Dupré:

> By asserting that all scientific belief should be explained in terms of the goals, interests, and prejudices of the scientist, and denying any role whatsoever for the recalcitrance of nature, it leaves no space for the criticism of specific scientific beliefs on the ground that they do reflect such prejudices rather than being plausibly grounded in fact. The uncongeniality of the sociology of science program to thinkers genuinely concerned with political influences on scientific belief is nicely stated by the feminist philosopher of science Alison Wylie . . . : "Only the most powerful, the most successful in achieving control over the world, could imagine that the world can be constructed as they choose."[6]

Smith takes Dupré to task for trying to "validate" his "intellectual traditionalism" by appealing to gestures of "solidarity with political radicalism":

> Notable in that connection is Dupré's curious allusion to "the uncongeniality of the sociology of science program to thinkers genuinely concerned with political influences on scientific belief"—curious because the work of that program's most eminent practitioners, for example, David Bloor, Barry Barnes, Andrew Pickering, or Steven Woolgar, would certainly seem to be *extensively* concerned with "political influences on scientific belief." (Indeed, in the outraged view of various recent detractors, it is quite menacingly concerned with nothing else.) Has Dupré failed to notice this concern? Or is he suggesting that the sociologists are just faking it . . . ?

Smith seems to have missed the important but fairly obvious point that Dupré is making, which is that if you say that the correct explanation for all scientific belief is in terms of the political goals, interests, and prejudices of the scientist, you make it impossible to criticize a specific scientific belief as merely reflective of such prejudice. I take it that all of us would want to be able to distinguish between the conclusions of *The Bell Curve*, for example, and Maxwell's equations for electromagnetism by saying precisely that the first merely or largely reflects the prejudices of its authors whereas the second does not, that it is grounded in solid, persuasive evidence. How

are we to make this distinction, however, if we simultaneously insist that *all* scientific belief be explicable in terms of political goals, interests, and prejudices?

As I mentioned above, this third constructivist option has a more interesting, milder version, one that Smith does not discuss but which it would be good to have on the table for the sake of completeness. On this milder view, although evidence *does* enter the explanation for why a particular view is believed, it can never be enough to explain it. Any evidence we might possess always *underdetermines* the specific belief that we arrive at on its basis. Something else must close the gap between what we have evidence for and what we actually believe, and that something else is provided by the thinker's background values and interests.

This idea, that the evidence in science always underdetermines the theories that we believe on its basis, has exerted considerable influence in the philosophy of science, even in nonconstructionist circles. In its modern form it originated in the thought of turn-of-the-century French physicist and philosopher Pierre Duhem.[7] Suppose that an experimental observation is inconsistent with a theory that you believe: the theory predicts that the needle will reach 10, but the needle does not budge from 0. What Duhem pointed out is that this does not necessarily refute the theory. For the observational prediction is generated not merely on the basis of the theory; in addition one must assume auxiliary hypotheses about how the experimental apparatus works. In light of the recalcitrant observational result, *something* has to be revised, but so far we do not yet know exactly what. Perhaps it is the theory; perhaps it is the auxiliary hypotheses. Perhaps, indeed, it is the very claim that we recorded a genuinely recalcitrant result, as opposed to merely suffering some visual illusion.

Duhem argued that evidence alone could never decide which revisions are called for and, hence, that belief fixation in science could not be a purely rational matter. Something else had to be at work as well. What the constructivist adds is that this extra element is something political.

There are, however, several well-known problems with this ingenious argument. One problem is that it is subtly begging the question. In trying to show that we can never have reason to modify one belief as opposed to another, it must assume that we can never have such reason. Consider Duhem's example of an astronomer peering through his telescope at the heavens and being surprised at what he finds there, perhaps a hitherto unde-

tected star in a galaxy he has been charting. Upon this discovery, according to Duhem, the astronomer may revise his theory of the heavens or he may revise his theory of how the telescope works. Rational principles of belief fixation do not tell him which to do.

The idea, however, that in peering at the heavens through a telescope we are always testing our theory of the telescope just as much as we are testing our astronomical views seems absurd. The theory of the telescope has been established by numerous terrestrial experiments and fits in with an enormous number of other things that we know about lenses, light, and mirrors. It is better confirmed than our views about the contents of a given galaxy. As a result, it is simply not plausible that, in coming across an unexpected observation of the heavens, a rational response might be to revise what we know about telescopes. In the first instance it would be more rational to revise our beliefs about the galaxy.

The point, of course, is not that we might never have occasion to revise our theory of telescopes; one can certainly imagine circumstances under which that is precisely what would be called for. The point is that not *every* circumstance in which something about telescopes is presupposed is a circumstance in which our theory of telescopes is being tested, and so the conclusion that rational considerations alone cannot determine how we respond to recalcitrant experience is blocked.

A second difficulty with the general thesis of the underdetermination of belief by evidence is of a familiarly reflexive nature: it must exempt itself from the general claim that it is putting forward. To say that the evidence underdetermines what we tend to believe on its basis is to say that the evidence alone does not take us all the way to the belief that we actually form; something else must close the gap, and that something else is political according to the constructionist.

But let us ask this: How is this established? How do we know that our beliefs always outstrip the evidence and that it is something political that fills the gap? The answer, we are invited to believe, is that this is the lesson of the close study of the history of science engaged in by our colleagues in science studies. Careful, painstaking investigation of the actual course of the development of science reveals that what we believe must always in part be explained by appeal not only to the evidence at our disposal but by our underlying political values as well.

But if the evidence always underdetermines what we believe on its basis,

then presumably the historical evidence assembled by science scholars also underdetermines the claim that evidence always underdetermines what we believe on its basis. If the constructionist is being consistent, he or she must admit that the evidence alone does not settle whether belief is always underdetermined by the evidence, that something about his or her own political agenda is making the constructionist say that it is. But if that is the case, why do I have to accept what he or she says, if I do not happen to share that agenda?

A number of difficult questions are left dangling of which the most important is this: Suppose someone raises an entirely global skeptical challenge to the norms of correct reasoning that we employ. How could we go about adjudicating which of us is correct? It is hard to see how the debate would be conducted. We will expect a reasoned argument demonstrating the incorrectness of our norms. But a reasoned argument that will impress us would have to conform to our norms, which is precisely what the skeptic is challenging. On the other hand, if we are not given a reasoned argument that conforms to our norms, we will remain unmoved. In trying to get us to change our minds, the skeptic would be better off threatening us with physical harm than with giving us reasons that we refuse to recognize as reasons. There is a deep question about how our specific conception of what counts as a good reason is to be justified in response to a hypothetical global challenge. If the challenge is piecemeal, to some specific part of our conception, then it is entirely conceivable that we should be able to use one part of our conception to defend or attack another. But it is hard to see how the argument is to be conducted if the challenge is global. What is crucial, it seems to me, is to see that the varieties of constructivism that we have been exploring are totally inadequate responses to the possibility of such skepticism. They are not coherent attitudes by which a belief can be maintained or an assertion put forward, as is shown by the fact that they will be violated even by the skeptic's own attitudes toward his or her own assertions.

I do not see how we are to think about belief and assertion without the idea that some beliefs are backed by reasons for taking them to be true and others are not. I do not see how to hang on to that idea without drawing some sort of principled distinction between those things that count as reasons and those that do not. And I do not see how, in the most general case, I

can think of these reasons as mere reflections of my own perspective and not ones that may equally have a grip on someone else. In saying all this, I am, of course, showing myself to be one of those "scholars . . . whose conviction of the adequacy of traditional views remains unshaken and who, accordingly, will continue to reaffirm the traditional positions as such, staunchly rehearsing the classic justifications and refutations, unruffled by what colleagues experience as the logical bite of current challenges." But as Martin Luther is reputed to have said, here I stand, I can do no other.

Notes

1 Sandra Harding, "Rethinking Standpoint Epistemology: What Is 'Strong Objectivity'?" in *Feminist Epistemologies*, ed. Linda Alcoff and Elizabeth Potter (New York: Routledge, 1993), 50.

2 Lorraine Code, "Taking Subjectivity into Account," in Alcoff and Potter, *Feminist Epistemologies*, 39.

3 Kathleen Lennon, "Feminist Epistemology As Local Epistemology," *Proceedings of the Aristotelian Society* 97 (1997): 37.

4 A more extreme view would be to relativize not merely what we have reason to believe to be true, but the truth itself. I do not discuss this view partly because, so far as I can tell, Smith has no inclination to accept it and partly because its problems are a lot more familiar.

5 David Bloor, *Knowledge and Social Imagery*, 2d ed. (Chicago: University of Chicago Press, 1991), 7.

6 John Dupré, *The Disorder of Things: Metaphysical Foundations of the Disunity of Science* (Cambridge: Harvard University Press, 1993), 12–13.

7 Pierre Duhem, *The Aim and Structure of Physical Theory* (Princeton, NJ: Princeton University Press, 1954).