

# EPISTEMOLOGICAL REFLECTIONS OF AN OLD FEMINIST

Susan Haack  
*University of Miami*

The philosophy which is now in vogue . . . cherishes certain tenets . . . which tend to a deliberate and factitious despair, which . . . cuts the sinews and spur of industry. . . And all for . . . the miserable vainglory of having it believed that whatever has not yet been discovered and comprehended can never be discovered or comprehended hereafter. — Francis Bacon<sup>1</sup>

I have been a feminist since the age of twelve, when I got the top grade in my first chemistry exam, and the boy who got the next highest grade protested indignantly that it wasn't *fair*, "everyone knows girls can't do chemistry." And, since I have been working in epistemology for more than a decade now, I think I qualify as an epistemologist. So I must be a feminist epistemologist, right? Wrong; on the contrary, I think there is no such connection between feminism and epistemology as the rubric "feminist epistemology" requires.

Perhaps you think that only someone of extreme right-wing political views could possibly be less than enthusiastic about feminist epistemology. If so, you are mistaken; both because the only thing extreme about my political views is my dislike of extremes, and because my reasons for thinking feminist epistemology misconceived are, in any case, not political but epistemological.

The last fifteen years or so have seen a major shift within feminist philosophy: from a modest style which stressed the common humanity of women and men, focused on justice and opportunity, and was concerned primarily with issues in social and political theory; to an ambitious, imperialist feminism which stresses the "woman's point of view," and claims revolutionary significance for all areas of philosophy, epistemology included.

So, yes, the pun in my title is intentional; my feminism is of the older-fashioned, modest stripe.<sup>2</sup> But I am taking issue, here, only with the imperialist ambitions of the new feminism with respect to epistemology specifically.

The rubric "feminist epistemology" is incongruous on its face, in somewhat the way of, say, "Republican epistemology." And the puzzlement this prompts is rather aggravated than mitigated by the bewildering diversity of epistemological ideas described as "feminist." Among self-styled feminist epistemologists one finds quasi-foundationalists, coherentists, contextualists; those who stress connectedness, community, the social aspects of knowledge, and those who stress emotion, presumably subjective and personal; those who stress concepts of epistemic virtue, those who want the "androcentric" norms of the epistemological tradition to be replaced by "gynocentric" norms, and those who advocate a descriptivist approach. . . . Even apparent agreement, e.g., that feminist epistemology will stress the social aspects of knowledge, masks significant *disagreement* about what this means: that inquirers are pervasively dependent on one another; that cooperative inquiry is better than individual inquiry; that epistemic justification is community-relative; that only a social group, not an individual, can properly be said to inquire or to know; that reality is socially constructed. . . .<sup>3</sup>

The puzzlement is further aggravated by the reflection that neither all, nor only, females, or feminists, favor all, or indeed any, of the ideas offered under the rubric "feminist epistemology." Charles Peirce, for example, is critical of what he calls the "vicious individualism" of Descartes's criterion of truth, and has a subtle conception of the social aspects of inquiry; yet he was neither female nor (to judge by his use of "masculine intellect" to mean "tough, powerful mind") feminist. John Stuart Mill surely qualifies as feminist if any male philosopher does; yet one finds none of the supposedly feminist themes in his epistemology—any more than one does in Ayn Rand's.<sup>4</sup>

So, what *is* feminist about feminist epistemology? There seem to be two routes by which feminism and epistemology are taken to be connected, corresponding to two interpretations of the phrase "the woman's point of view": as "the way women see things," or as "serving the interests of women."<sup>5</sup>

Sometimes we are told that feminist epistemology represents women's "ways of knowing." This reversion to the notion of "thinking like a woman" is disquietingly reminiscent of old, sexist stereotypes.<sup>6</sup> Still, there

are disquieting truths, so this hardly settles the matter. But I am not convinced that there *are* any distinctively female "ways of knowing." All *any* human being has to go on, in figuring out how things are, is his or her sensory and introspective experience, and the explanatory theorizing he or she devises to accommodate it; and differences in cognitive style, like differences in handwriting, seem more individual than gender-determined.<sup>7</sup>

The profusion of incompatible themes proposed as "feminist epistemology" itself speaks against the idea of a distinctively female cognitive style. But even if there were such a thing, the case for feminist epistemology would require further argument to show that female "ways of knowing" (scare quotes because the term is tendentious, since "knows" is a success-word) represent better procedures of inquiry or subtler standards of justification than the male. And, sure enough, we are told that insights into the theory of knowledge are available to women which are not available, or not easily available, to men. In all honesty, I cannot see how the evidence to date could be thought to speak in favor of this bold claim; what my experience suggests is rather that the questions of the epistemological tradition are *hard*, very hard, for anyone, of either sex, to answer or even significantly to clarify.<sup>8</sup>

It is said that oppressed, disadvantaged, and marginalized people are epistemically privileged in virtue of their oppression and disadvantage.<sup>9</sup> If this were true, it would suggest that the *truly* epistemically privileged are not the affluent, well-educated, white, Western women who (mostly) rest their claim to special insight upon it, but the most oppressed, the most disadvantaged—some of whom are men. But, aside from appeals to the authority of Karl Marx on epistemological matters,<sup>10</sup> is there any reason to think it *is* true? Thomas Kuhn observed that revolutionary scientific innovations are often made by persons who are at the margin of a discipline;<sup>11</sup> but women, as a class, are not "marginal" in *this* sense. And one of the ways in which oppressed people *are* oppressed is, surely, that their oppressors control the information that reaches them. This argues, if anything, an epistemic *disadvantage* for "oppressed, disadvantaged, marginalized" people.

So no such connection between feminism and epistemology as the rubric "feminist epistemology" requires is to be found under the first interpretation of "the woman's point of view" as "the way women see things."

Under the second interpretation, "serving the interests of women," the connection is supposed to be made, rather, by way of feminist criticisms of sexism in scientific theorizing.<sup>12</sup> The two routes connecting feminism and epistemology would merge on the assumption—which, of course, I do not accept—that sexism in scientific theorizing is the result of the exclusion of female "ways of knowing." A very faint trace of the first route would be detectable along the second on the assumption—which, with the caveat that it would be naive to suppose that only men subscribe to sexist stereotypes,

I am inclined to grant—that women are a bit more likely than men to notice such sexism.

In the social sciences and biology, theories which are not well-supported by the evidence do seem sometimes to have come to be accepted by scientists, most often male scientists, who have taken stereotypical ideas of masculine and feminine behavior uncritically for granted.<sup>13</sup> Those who think that criticisms of sexism in scientific theorizing require a new, feminist epistemology insist that we are obliged, in the light of these criticisms, to acknowledge political considerations as legitimate ways to decide between theories.

But on the face of it these criticisms suggest exactly the opposite conclusion—that politics should be kept out of science.<sup>14</sup> I can make sense of how things get so startlingly *aufgehoben* only by looking at feminist epistemology, not just as part of a larger development in feminism, but also as part of a larger development in epistemology. Here the last thirty years or so have seen a major shift: from the old romantic view, which took science to deserve a kind of epistemic authority in virtue of its peculiarly objective method of inquiry; to a new cynicism, which sees science as a value-permeated social institution, stresses the importance of politics, prejudice, and propaganda, rather than the weight of the evidence, in determining what theories are accepted, and sometimes goes so far as to suggest that reality is constructed by us, and “truth” a word not to be used without the precaution of scare quotes.<sup>15</sup>

My diagnosis is that the new cynicism in philosophy of science has fed the ambition of the new feminism to colonize epistemology. The values with which science is permeated, it is argued, have been, up till now, androcentric, sexist, inhospitable to the interests of women. Feminist criticisms of sexism in scientific theorizing, the argument continues, cannot be seen merely as criticisms of bad science; the moral to be drawn is that we must abandon the quixotic quest for a science that is value-free, in favor of the achievable goal of a science informed by feminist values. There would be a genuinely feminist epistemology if the aspiration to *legitimate the idea that feminist values should determine what theories are accepted* could be achieved.

The arguments offered to motivate the shift from feminist criticisms of sexism in scientific theorizing to feminist epistemology are of precisely the kind this diagnosis would predict. I can consider here only the two most important lines of argument, each of which focuses on a notion dear to the hearts of the new cynics: underdetermination and value-ladenness.

The first appeals to “the underdetermination of theories by data,” claiming that, since there is unavoidable slack with respect to what theories are accepted, it is proper to allow political preferences to determine theory choice.<sup>16</sup> Suppose, first, that the appeal to underdetermination is intended only to point to the fact that sometimes the available evidence is not suffi-

cient to decide between rival theories, and that in some cases (e.g., with respect to theories about the remote past, “man the hunter” and all that) additional evidence may be, in practice, unobtainable. The proper response is that, unless and until more evidence is available, scientists had better suspend judgment—and that the lay public, philosophers included, should not be too uncritically deferential to scientists’ sometimes unwarrantedly confident claims about what they have discovered. Underdetermination, in this sense, has not the slightest tendency to show that we may legitimately choose to believe whatever theory suits our political purposes.

Suppose, next, that the appeal to underdetermination is intended, rather, to rest on the Quinean thesis that there can be incompatible theories with the same observational consequences—theories, therefore, between which not even all possible evidence could decide. Fortunately the issues at stake here do not depend on whether or not the thesis is proven. (Quine himself at one point suggests that what he formerly described as empirically equivalent but incompatible theories would really only be verbal variants of one theory.) For in any case, if the thesis were true, it would presumably be true only of the genuinely theoretical (in the sense of “unobservable”); it would be irrelevant, therefore, to such questions as whether men’s hunting or women’s gathering mainly sustained prehistoric communities. And if it *were* relevant to such questions, the feminists’ appeals to it would be self-defeating, since in that case it would undermine their presumption that we can know what theories conduce to the interests of women, or what those interests are.<sup>17</sup>

The second line of argument urges the necessity of “rubbing out the boundary between science and values,”<sup>18</sup> and hence, again, the appropriateness of allowing feminist values to determine theory choice. In one version, the argument seems to be that the idea that feminist values could not constitute evidence with respect to this or that theory rests on an untenable distinction of descriptive versus normative. This argument is only as good as the reasons for thinking the required distinction untenable. What is at issue is not whether moral or political criticisms of priorities within science, or of uses of the findings of science, are ever appropriate; not whether an evolutionary account of moral values is defensible; not whether simplicity, e.g., might have a more than pragmatic role; not whether some epistemic norms may turn out to be covertly of a descriptive, means-end character; but *whether it is possible to derive an “is” from an “ought.”*<sup>19</sup> I can find no argument in the literature that even purports to show this, and neither can I think of one. That it is false is manifest as soon as one expresses it plainly: that propositions about what states of affairs are *desirable* or *deplorable* could be evidence that things *are*, or *are not*, so.

In another version, the second line of argument seems to rest on the claim that it is impossible entirely to exclude “contextual” (i.e., external, social, and political) values from science. In this version, the argument is a

non sequitur. Perhaps it is true that scientists are never entirely without prejudice; perhaps it is impossible that they should entirely put their prejudices out of sight when judging the evidence for a theory; it doesn't follow that it is proper to allow prejudice to determine theory choice. Even if it is not possible to make science perfect, it doesn't follow that we shouldn't try to make it better.

The failure of these arguments is symptomatic of the false presupposition on which the second proposed route to connect feminism and epistemology depends: that, since the old romantic picture is not defensible, there is no option but the new cynicism. These are not the only options; the truth lies, as it so often does, between the extremes.<sup>20</sup> The old romanticism overstates the virtues, the new cynicism the vices, of science; the old romanticism focuses too exclusively on the logical, the new cynicism too exclusively on the sociological, factors that an adequate philosophy of science should combine. Science is neither sacred nor a confidence trick. It has been the most successful of human cognitive endeavors, but it is thoroughly fallible and imperfect—and, in particular, like all human cognitive endeavors, it is susceptible to fad and fashion, partiality and politics.

Implicit here is a conception of the epistemological role of the sociology of science which is worth making explicit, since it challenges an assumption which, it seems, both some old romantics and some new cynics take for granted—that the sociology of knowledge somehow constitutes a threat to traditional epistemological concerns. It is manifest as soon as it is stated plainly that no sociological investigation or theory could be sufficient by itself to show that the idea of theories being better or worse supported by evidence is untenable. But to say this is not to deny that the sociology of knowledge has any possible relevance to epistemology. Sometimes scientists are scrupulous in seeking out and assessing relevant evidence; sometimes not. Presumably, there is always some explanation of why they behave as they do, sometimes an explanation appealing to the individual psychology of the scientists concerned, sometimes an explanation appealing to considerations of a more sociological kind (e.g., that political pressures led these scientists to ignore or gloss over the relevance of such-and-such easily available evidence; that the knowledge that their work would come under the critical scrutiny of a rival team also aspiring to the Nobel prize ensured that those scientists left no stone unturned; etc.). The value of such sociological investigations to epistemology is that they may suggest what ways of organizing science are apt to encourage, and what to discourage, scrupulous attention to the evidence.<sup>21</sup>

If my diagnosis is correct, then though it is not inevitable that all the themes offered under the rubric "feminist epistemology" are false, it is inevitable that only those themes can be true which fail in their cynical intent. It is true, e.g., that inquirers are profoundly and pervasively dependent on each other; it is true that sometimes scientists may perceive relevant

evidence *as* relevant only when persuaded, perhaps by political pressure, out of previous prejudices. But such truths have no radical consequences; it does not follow, e.g., that reality is however some epistemic community determines it to be, or that what evidence *is* relevant is not an objective matter.

And the epistemological significance of feminist criticisms of sexism in scientific theorizing, though real enough, is undramatic and by no means revolutionary. One traditional project of epistemology is to give rules, or, better, guidelines, for the conduct of inquiry; another is to articulate criteria of evidence or justification.<sup>22</sup> One sub-task of the “conduct of inquiry” project is to figure out what environments are supportive of, and what hostile to, successful inquiry. One sub-task of this sub-task is to figure out how to minimize the effect of unquestioned and unjustifiable preconceptions in encouraging the acceptance of theories which are not well-supported by evidence. (Greater diversity within science may be one way to do this. If we cannot ensure that scientists leave all their prejudices at the laboratory door, it may nevertheless be possible to ensure that there is enough diversity within the laboratory for prejudices and counter-prejudices to cancel out.)<sup>23</sup> Feminist criticisms of sexist science, like studies of the disasters of Nazi or Soviet science, can be a useful resource in this sub-sub-task of the “conduct of inquiry” project. But this is a role that requires the conception of theories as better or worse supported by the evidence, and the distinction of evidential and non-evidential considerations, traditionally investigated in the “criteria of justification” project; it is not a role that allows us to abandon or requires us radically to revise the concepts of evidence or truth or reality.<sup>24</sup>

Still, you may ask, given that I have not denied that some themes presented under the rubric “feminist epistemology” are true, and that I grant that some feminist criticisms of sexist science seem well-founded and have a bona fide epistemological role, why do I make all this fuss about the label? Well, since the idea that there is an epistemology properly called “feminist” rests on false presuppositions, the label is at best sloppy. But there is more at stake than dislike of sloppiness; more than offense at the implication that those of us who don’t think it appropriate to describe our epistemological work as “feminist” don’t care about justice for women; more than unease at sweeping generalizations about women and embarrassment at the suggestion that women have special epistemological insight. What is most troubling is that the label is designed to convey the idea that *inquiry should be politicized*. And *that* is not only mistaken, but dangerously so.

It is dangerously mistaken from an epistemological point of view, because the presupposition on which it rests—that genuine, disinterested inquiry is impossible—is, in Bacon’s shrewd phrase, a “factitious despair” which will, indeed, “cut the sinews and spur of industry.” Serious intellec-

tual work is hard, painful, frustrating; suggesting that it is legitimate to succumb to the temptation to cut corners can only block the way of inquiry.<sup>25</sup>

I would say that inquiry really is best advanced by people with a genuine desire to find out how things are, who will be more persistent, less dogmatic, and more candid than sham reasoners seeking only to make a case for some foregone conclusion; except that, since it is a tautology that inquiry aims at the truth, the sham reasoner is not really engaged in inquiry at all.<sup>26</sup> This should remind us that those who despair of honest inquiry cannot be in the truth-seeking business (as they should say, “the ‘truth’ racket”); they are in the propaganda business.<sup>27</sup>

And this makes it apparent why the idea that inquiry should be politicized is dangerously mistaken, also, from a political point of view, because of the potential for tyranny of calls for “politically adequate research and scholarship.”<sup>28</sup> Think what “politically inadequate research” refers to: research informed by what some feminists deem “regressive” political ideas—and research not informed by political ideas at all, i.e., honest inquiry. Have we forgotten already that in *Nineteen Eighty-Four* it was *thoughtcrime* to believe that two plus two is four if the Party ruled otherwise?<sup>29</sup> This is no trivial verbal quibble, but a matter, epistemologically, of the integrity of inquiry and, politically, of freedom of thought. Needless sacrificing these ideals would not help women; it would hurt humanity.<sup>30</sup>

1. Francis Bacon, *The New Organon* (1620), Book 1, aphorism LXXXVIII.

2. The clash of “old” and “new” feminisms is nothing new; here is British novelist and feminist Winifred Holtby, writing in 1926:

The New Feminism emphasises the importance of the ‘woman’s point of view’, the Old Feminism believes in the primary importance of the human being. . . . Personally I am . . . an Old Feminist, because I dislike everything that feminism implies. I desire an end to the whole business, the demands for equality. . . . But while . . . opportunity [is] denied, I shall have to be a feminist. . . . (Cited in Rosalind Delmar, “Afterword,” to Vera Brittain, *Testament of Friendship* [1945; London: Virago, 1980], p. 450.)

It ought to be said that fewer opportunities are now denied, that the “end to the whole business” is, hopefully, closer than it was in 1926.

3. For example, Lorraine Code represents herself as an “empirico-realist,” acknowledging the affinity of this conception with foundationalism (*Epistemic Responsibility* [Hanover, NH: Brown University Press, 1987], p. 6); Lynn Hankinson Nelson follows Quine, whom she interprets as holding a coherentist theory of evidence (*Who Knows: From Quine to a Feminist Empiricism* [Philadelphia, PA: Temple University Press, 1990], pp. 25-27, 85-86, 91-94, 112-17); Jane Duran represents herself as a contextualist (*Toward a Feminist Epistemology* [Savage, MD: Rowman and Littlefield, 1991], pp. 119ff.). But matters are not really as straightforward as this suggests, since there are in each case apparent in-



consistencies: after aligning herself with "empirico-realism," Code bemoans the "aridity" of the whole issue of foundationalism versus coherentism (p. 7) and hints that it is somehow misconceived; Nelson acknowledges (pp. 22ff.) that Quine's conception of evidence allows an important role for experience; Duran describes the female point of view as instinctively coherentist (p. 14). Again, Duran appears to hold that a feminist epistemology should be "naturalistic" in the sense of descriptivist (pp. 204ff.); *and* that it should focus on other conceptions of justification than the epistemological (pp. 12-13); *and* that it should replace androcentric norms with gynocentric ones (pp. 73ff.).

It is all very confusing. Sandra Harding tells us that it is to be expected that feminist epistemology will "contain contradictions," that it is "multiple and contradictory knowledge" out of which we are "to learn and think" (*Whose Science? Whose Knowledge?* [Ithaca, NY: Cornell University Press, 1991], pp. 180, 285, 275). This is not very reassuring.

4. The critique of Descartes is to be found in Charles S. Peirce, *Collected Papers*, ed. Charles Hartshorne, Paul Weiss, and Arthur Burks (Cambridge, MA, and London: Harvard University Press, 1931-58), 5.213-310; Peirce's social conception of inquiry is already apparent in perhaps his best-known paper, "The Fixation of Belief," 5.358-77. See also Susan Haack, "Descartes, Peirce, and the Cognitive Community," *The Monist*, vol. 65, no. 2 (1982), pp. 156-81, and in Eugene Freeman, ed., *The Relevance of Charles Peirce* (La Salle, IL: Monist Library of Philosophy, 1983), pp. 238-63. For Peirce's use of "masculine intellect," see *Collected Papers*, 5.368, and his review of Lady Welby's *What is Meaning?*, 8.171: "Lady Victoria Welby's book . . . is a feminine work, and a too masculine mind might think it painfully weak." Other themes sometimes described as "feminist" are also to be found in Peirce (e.g., a penchant for replacing dichotomies by trichotomies); and different "feminist" themes are to be found in the other pragmatists (e.g., William James's Will to Believe doctrine, allowing a legitimate cognitive role to "our passional nature"). But, for obvious reasons, I think it inappropriate to attempt to trace "anticipations of feminist epistemology/metaphysics/philosophy of language/etc." in pragmatism (as in the symposium in *Transactions of the Charles S. Peirce Society*, vol. XXVII, no. 4 [1991]).

For John Stuart Mill, see *A System of Logic* (1843; London: Longman, 1970), and *The Subjection of Women* (1869; Chicago, IL: Phoenix Books, University of Chicago Press, 1970). I say Mill counts as a feminist "if any male philosopher does" to draw attention to Harding's discussion of the male feminist—"The Monster," as she calls him (*Whose Science? Whose Knowledge?*, p. 284)—and to note that some writers, though not Harding, suspect that the monster may be mythical, an impossible beast; see Scarlet Friedman and Elizabeth Sarah, eds., *On the Problem of Men* (London: Women's Press, 1982), and Alice Jardine and Paul Smith, eds., *Men in Feminism* (New York: Methuen, 1987).

For Ayn Rand, see *Introduction to Objectivist Epistemology* (New York: Mentor, 1966).

5. Of course, some of those who describe themselves as "feminist epistemologists" do so only because they are picking up some theme described elsewhere as "feminist"; and some, perhaps, for no better reason than that, since they are female and doing epistemology, what they are doing must be feminist epistemology.

6. Cf. this observation, from p. 1 of Nancy Holland, *Is Women's Philosophy Possible?* (Savage, MD: Rowman and Littlefield, 1990): "Women's philosophy seems to entail a healthy skepticism about universal generalizations." (My thanks to John Nuechterlein for drawing this gem to my attention.)

The tendency for feminists' generalizations to mirror old stereotypes can hardly escape attention; Andrea Nye's "feminist critique" of logic (*Words of Power* [New York and London: Routledge, 1990]), mirroring the old cliché that "women are so illogical," being a striking case in point. Ironically enough, where they are at all plausible Nye's criticisms of formal logic are familiar from the work of earlier (male) writers who stressed the inadequacy of symbolic logic to represent pragmatic aspects of reasoning. See Ferdinand C. S. Schiller, *Formal Logic: A Scientific and Social Problem* (London: MacMillan, 1912); Peter F. Strawson, *Introduction to Logical Theory* (London: Methuen, 1952); Stephen

Toulmin, *The Uses of Argument* (Cambridge: Cambridge University Press, 1958). And, of course, the notion of "reading" which Nye favors derives from the work of male writers such as Paul de Man.

7. I am skeptical of attempts to establish this by appeal to Object Relations theory, as in, e.g., the paper by Jane Flax in Sandra Harding and Merrill Hintikka, eds., *Discovering Reality: Feminist Perspectives on Epistemology, Metaphysics, Methodology, and Philosophy of Science* (Dordrecht: Reidel, 1983); Evelyn Fox Keller, *Reflections on Gender and Science* (New Haven and London: Yale University Press, 1985). Not only is the theory very speculative, it is also very vague, and its pertinence to the claim that women have different ways of knowing than men is tenuous at best.

Mary Field Belenky et al., *Women's Ways of Knowing* (New York: Basic Books, 1986) purports to offer direct, empirical evidence of "women's ways of knowing." In this, I think, it entirely fails. It reports only studies of women; and these studies do not replicate, with female subjects, the studies already undertaken by William Perry with male subjects (*Forms of Intellectual and Ethical Development in the College Years* [New York: Rinehart and Winston, 1970]). The authors chose to ask their subjects different questions than Perry asked his because they already believed that "there is a masculine bias at the heart of most academic disciplines, methodologies and theories" (p. 8)—a proposition, they claim, "convincingly argued" by feminist academics, among whom they mention Fox Keller and the authors collected in Harding and Hintikka, *Discovering Reality*. The issue here is not the merits or demerits of Perry's categories, but the fact that Belenky et al. make no attempt to study both men and women under one set of categories—surely a minimally necessary condition of discovering whether there are or aren't male and female cognitive styles. The question was begged in the design of the study.

8. And, I should add, that the capacity for original, creative philosophical thought is quite a rare and unusual talent. I recall, in this context, the observation attributed to Peirce by Eric Temple Bell: "There is a kink in my damned brain that prevents me from thinking as other people think" (*The Development of Mathematics* [New York and London: McGraw Hill, 1949], p. 519). It is just such individual idiosyncrasies—not the "group-think" apparently admired by some feminists—that philosophical (and scientific, artistic, etc.) innovation requires.

9. See, e.g., Alison Jaggar, "Love and Knowledge: Emotion in a Feminist Epistemology," in Anne Garry and Marilyn Pearsall, eds., *Women, Knowledge, and Reality* (Boston: Unwin Hyman, 1989), p. 146; Harding, *Whose Science? Whose Knowledge?*, p. 271; Nelson, *Who Knows: From Quine to a Feminist Empiricism*, p. 40. Some, seeing that the "privilege of disadvantage" thesis suggests greater privilege for the more marginalized, claim special epistemological privilege for lesbians (see, e.g., Marilyn Frye's essay in *Women, Knowledge, and Reality*, p. 77).

10. See, e.g., Nancy Hartsock's essay in Harding and Hintikka, *Discovering Reality*; Harding, *Whose Science? Whose Knowledge?*, p. 58.

11. Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago and London: University of Chicago Press, 1962). I owe to correspondence with Mary Hesse the neat observation that Kuhn is himself such an "outsider" (with respect to the philosophy of science, that is).

12. This labored phrase is necessary in order to make it clear that the issue concerns feminist criticisms focusing on the content of scientific theories, not feminist criticisms of the choice of problems on which scientists work, or of there being relatively few, and mostly relatively junior, women scientists. I am not saying that the latter kinds of criticism are never justified, only that they are not relevant to the line of argument under consideration here.

I should also make it clear that I am using the term "sexist" in such a way that a theory counts as sexist only if it is false. See my review of Harding and Hintikka, eds., *Discovering Reality*, in *Philosophy*, vol. 60 (1985), pp. 265-70.

13. Ruth Bleier's criticisms, in Bleier, ed., *Feminist Approaches to Science* (New York:

Pergamon Press, 1986), of some claims about hormonal determinism are among the most convincing. I have two cents' worth of my own to contribute here: the claim that male dominance is hormonally determined is confidently reiterated by critics of feminism such as Nicholas Davidson and Michael Levin, both of whom cite Steven Goldberg as their source; Goldberg cites a medical researcher called Money. Imagine my astonishment, then, on tracking down Money's work, to find that he says specifically that questions about dominance *were not addressed* in his study of genetic females exposed before birth to high levels of male hormones! For details, see my review of Davidson and Levin, in *International Studies in Philosophy*, vol. 23, no. 1 (1991), pp. 107-9.

Other feminist criticisms of sexism in scientific theorizing are to be found in, e.g., Anne Fausto-Sterling, *Myths of Gender: Biological Theories about Women and Men* (New York: Basic Books, 1986), and Helen Longino and Ruth Doell, "Body, Bias, and Behavior: a Comparative Analysis of Reasoning in Two Areas of Science," in Jean O'Barr and Sandra Harding, eds., *Sex and Scientific Inquiry* (Chicago: University of Chicago Press, 1989).

Let me make it as clear as I can that my view is that each feminist critique of this or that bit of scientific theorizing has to be considered on its own merits; of course, in some instances it may be difficult for someone outside the field to determine what those merits are. But I should also say that I am skeptical of the idea that sexism infects theorizing not only in the social sciences and biology, but also in the physical sciences; at any rate, I have never encountered a convincing example.

14. This seems an appropriate time for a comment about the use of the term "feminist empiricism," which is potentially confusing. In both *The Science Question in Feminism* (Ithaca, NY: Cornell University Press, 1986) and *Whose Science? Whose Knowledge?*, Sandra Harding distinguishes three positions within feminist epistemology: feminist empiricism, feminist standpoint theories, and feminist postmodernism; and she characterizes "feminist empiricism" as holding that feminist criticisms of sexism in scientific theorizing are criticisms of "bad science" (her scare quotes), not requiring any change in the appraisal of "science as usual." As Helen Longino observes ("Science, Objectivity, and Feminist Values," *Feminist Studies*, vol. 14, no. 3 [1988], p. 571), *this* "feminist empiricism" seems to be characterized just so as to be a foil to the feminist standpoint theories Harding favors; as I would say, "feminist" in "feminist empiricism" seems redundant. In this sense, Stephen Jay Gould, or myself, qualify as "feminist empiricists," even though we both deny that a specifically feminist epistemology is required. (See Gould's review of Ruth Bleier, ed., *Feminist Approaches to Science*, in *New York Times Book Review*, August 12, 1984, p. 7.)

But Nelson, who entitles her book *Who Knows: From Quine to a Feminist Empiricism*, is no such pallid creature; hers is a feminist empiricism which insists that feminist political considerations should determine theory choice.

15. My description is, of course, very simplified. But I think it is true to the spirit of the shift.

16. Helen Longino, "Can There Be a Feminist Science?" in Garry and Pearsall, eds., *Women, Knowledge, and Reality*, p. 206; Nelson, *Who Knows: From Quine to a Feminist Empiricism*, pp. 173-74, 187-88, 248. I have the impression that Longino favors the first of the two versions of the argument distinguished below, Nelson the second.

17. Willard Van Orman Quine, "Empirically Equivalent Theories of the World," *Erkenntnis*, vol. 9 (1975), pp. 313-28, and "Empirical Content," in Quine, *Theories and Things* (Cambridge, MA and London: Belknap Press of Harvard University Press, 1981), pp. 24-30. This passage from the latter (pp. 29-30) is unambiguous enough:

Being incompatible, the two theory formulations that we are imagining must evaluate some sentence oppositely. Since they are nevertheless empirically equivalent, that sentence must contain terms that are short on observational criteria. But then we can . . . pick out one of those terms and treat it as if it were two independent words, one in one theory formulation and another in the other. We can mark this by changing the spelling of the word in one of the two theory formulations.

Pressing this trivial expedient, we can resolve all conflict between the two theory formulations. . . .

*Theories and Things* appears in Nelson's bibliography, but I have not been able to find any discussion of this passage from "Empirical Content."

What I have said here, of course, falls way short of a thorough discussion of the underdetermination thesis, which would require, *inter alia*, consideration of the status of such pragmatic values as simplicity in theory choice. Two recent discussions which illustrate the complexities of the issues here are Laurens Laudan, "Demystifying Underdetermination," in *Scientific Theories*, ed. C. Wade Savage (Minneapolis, MN: University of Minnesota Press, 1990), pp. 267-97, and Brian Ellis, "What Science Aims to Do," in Paul Churchland and Christopher Hooker, eds., *Images of Science* (Chicago: University of Chicago Press, 1985), pp. 48-74.

18. Nelson, *From Quine to a Feminist Empiricism*, p. 248. See also Longino, "Can There Be a Feminist Science?" Once again, my impression is that Nelson favors the in-principle version of the argument, Longino the in-practice variant. See also Harding, *Whose Science? Whose Knowledge?*, pp. 57ff.

19. Or, more strictly speaking, whether the statement that *p* ought [not] to be the case could be evidence that *p* is [not] the case.

20. "[L]et us remember how common the folly is, of going from one faulty extreme into the opposite" (Thomas Reid, *Essays on the Intellectual Powers* [1785], in Thomas Reid, *Inquiry and Essays*, ed. R. E. Beanblossom and K. Lehrer [Indianapolis: Hackett, 1983], Essay VI, ch. 4, p. 262).

21. After Peirce, Michael Polanyi seems to me to have best understood these issues. See "The Republic of Science," in Marjorie Grene, ed., *Knowing and Being* (Chicago: University of Chicago Press, 1969), pp. 49-62. I think one might attribute Polanyi's insights in part to his having worked as a scientist, at different stages of his career, on both sides of the Iron Curtain, an experience which left him acutely aware of the dangers of politicizing science.

22. Reliabilists, however, confuse the two projects. Cf. chapter 10 of Susan Haack, *Evidence and Inquiry: Towards Reconstruction in Epistemology* (Oxford: Blackwell, 1993) for a more careful articulation of the differences between them. Note that I there argue that the "conduct of inquiry" project is more hospitable to pluralism, and to the social aspects of epistemology, than the "criteria of justification" project.

23. Implicit in this is a deflationary interpretation of the grain of truth in the "multiple standpoints" account of objectivity suggested by Harding in *Whose Science? Whose Knowledge?*

24. *Contra* Harding, *Whose Science? Whose Knowledge?*, p. 38: "Issues of access for women in the practices of science turn out to have . . . radical consequences for the logic of inquiry and explanation."

25. "Do not block the way of inquiry" is, according to Peirce, a proposition that "deserves to be written on every wall of the city of philosophy" (*Collected Papers*, 1.135).

26. *Webster's*: "Inquiry: search for truth, information or knowledge."

See Susan Haack, "The First Rule of Reason," paper delivered at a conference on New Topics in the Philosophy of C. S. Peirce, Toronto, October 1992 (forthcoming in a volume in memory of David Savan edited by Jacqueline Brunning and Paul Forester, to be published by University of Toronto Press) for a detailed defense of the claims made in this paragraph. The term "sham reasoning," and its characterization, are both due to Peirce.

In this paper I also suggest a diagnosis of the organizational pressures which encourage the fashion in contemporary philosophy for exaggerated claims (that developments in neurophysiology show epistemology misconceived, that feminism requires a radically new epistemology, etc., etc.). I agree with Longino, by the way, that to improve the condition of science would probably require changes in the ways in which it is presently organized and funded—though not, of course, that more politicization, provided it was of the "right" sort,

would constitute improvement.

27. Some admit this unambiguously, e.g., Elizabeth Gross, who writes: “[F]eminist theory . . . is not a true discourse. . . . It could be appropriately seen, rather, as a *strategy*, . . . [an] intervention with definite political . . . aims . . . intellectual guerrilla warfare” (“What is Feminist Theory?” in *Feminist Challenges*, eds. Carole Pateman and Elizabeth Gross [Sydney, London, and Boston: Allen and Unwin, 1986], p. 177; cited in David Stove, “A Farewell to Arts,” *Quadrant*, May 1986, pp. 8-11).

Consider also this passage from Nelson, *Who Knows: From Quine to a Feminist Empiricism*, p. 102:

‘Nazi Science’ [sic] indicates that . . . a mix of science and politics can enable cruelty and suffering. . . . But while the dangers are real, . . . the ‘noble lie’ [that politics can and should be kept out of science] is far more dangerous.

Others are more equivocal: e.g., Harding, who, after stating boldly that “[t]he truth—whatever that is—cannot set us free” (*Whose Science? Whose Knowledge?*, p. xi), suggests that feminist theorizing could be, if not “true,” “less false” (pp. 58, 185). The impression I get from *Whose Science? Whose Knowledge?* is that Harding’s view is that the notion of a theory’s being true is unintelligible, but the notion of one theory’s being less false than another is intelligible. This is pretty puzzling. However, in “Who Knows? Identities and Feminist Epistemology,” in Joan E. Hartman and Ellen Messer-Davidow, eds., (*Engendering Knowledge* (Knoxville: University of Tennessee Press, 1991), pp. 100-115, Harding suggests, instead, the much less startling thesis that scientists claim only that this or that theory is better supported by the evidence, not that this or that theory is true. (My thanks to Ruth Manor for drawing this paper to my attention.) This isn’t nearly so puzzling; it is, however, at odds with Harding’s insistence, both in *The Science Question in Feminism* and in *Whose Science? Whose Knowledge?*, that feminist criticisms of sexism in science lead inevitably to revolutionary epistemological conclusions.

28. Harding, *Whose Science? Whose Knowledge?*, p. 98: “The model for good science should be research programs directed by liberatory political goals.” And, p. 280: “The authority to say what is theoretically and politically adequate research and scholarship must remain [sic] in the hands of the marginalized.”

Duran, *Toward a Feminist Epistemology*, pp. 145-46: “[W]ould a model like the . . . computational model [of mind], be the result of politically incorrect theorizing that is, apart from being grossly androcentric, also the very sort of thing feminists have labeled oppressive to minorities, Third World points of view, and, indeed, to anyone who is not white, male and well-educated?” And now consider Conor Cruise O’Brien’s shrewd account of the insidiousness of political pressures within the academy:

Young scholars in . . . sensitive fields are likely to believe that if they write with excessive candor about certain realities . . . doors will close to them: certain grants will be out of reach, participation in certain organized research programs denied, influential people alienated, the view propagated that the young man is unbalanced or unsound. These fears may be exaggerated . . . but they are not without foundation. . . . Inevitably some young men . . . will adapt to this situation with such concessions as they believe are necessary. And the scholars who adapt successfully are likely to be highly influential in their fields in the next generation.

(“Politics and the Morality of Scholarship,” in *Morality and Scholarship*, ed. Max Black [Ithaca, NY: Cornell University Press, 1967], p. 73.) I invite you to enjoy the irony of O’Brien’s unselfconscious assumption that “young scholars” are “young men.”

29. George Orwell, *NineteenEighty-Four* (1949; Harmondsworth, Middlesex, UK: Penguin Books, 1954), pp. 184, 198.

30. I would like to thank my colleagues Edward Erwin, Leonard Carrier, Alan Goldman, Howard Pospel, and Harvey Siegel for helpful discussion of a draft of this paper, and Adrian Lerner, Ralph Sleeper, and David Stove for helpful correspondence.