

How Not to Criticize Feminist Epistemology: a Review of Scrutinizing Feminist Epistemology

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Scrutinizing Feminist Epistemology, edited by Cassandra Pinnick, Noretta Koertge, and Robert Almeder (2003), (henceforth, SFE) offers a systematic critique of feminist epistemology. It aims to show that the entire enterprise is a failure. How should a critique that purports to decisively undermine an entire field of inquiry be evaluated? I propose the following standards. First, accuracy. The critique must accurately represent the field as it stands today, paying close attention to what its actual proponents say, in context. It should not attack straw men or beat dead horses killed long ago in debates internal to the field. Second, perspective. To be illuminating, a critique should not simply deploy its presuppositions against its rivals, but make them explicit, situate them relative its rivals in a field of possibilities, explain why these are the possibilities, and why its presuppositions should be accepted rather than rivals. Third, normative consistency. The critique should live up to the same normative standards it applies to its object. Let us assess Scrutinizing Feminist Epistemology by these standards.

1. Accuracy

SFE levels four major charges against what it takes feminist epistemology to stand for. First, political correctness: feminist epistemologists hold that political criteria should be deployed to pre-empt or override scientific reasoning based on evidence, in order to suppress truths that are inconvenient to the feminist agenda, and promulgate falsehoods congenial to it. Second, tribalism: feminist epistemologists think that all women (or all feminists) do, or should, think alike. In particular, they do or ought to adopt some common “feminine” epistemic style or methodology, applicable to all fields of inquiry, held to be superior, both in generating more objective knowledge and in serving women's interests. Third, self-defeating conservatism: feminist epistemology defeats its own aims in taking nonwestern, women's, or “feminine” values as an uncriticized given, even when these values underwrite sex and caste oppression. Fourth, cynicism: feminist epistemologists reject the quest for objectivity and truth as an impossibility, and regard the claim to pursue it is a mask for a power play that in practice serves the interests of white heterosexual Western men at everyone else's expense. I shall argue that all of these criticisms are based on gross misrepresentations of feminist epistemology.

(a) Political Correctness

Susan Haack, Noretta Koertge, and Robert Almeder accuse feminist epistemology of political correctness. Haack says the aim of feminist epistemology is to legitimate the idea that “feminist values should determine what theories are accepted” (12).[1] Such politicized inquiry leads to “sham reasoners seeking only to make a case for some foregone conclusion” (15), and threatens “honest inquiry,” which she characterizes as “research not informed by political ideas at all” (16). Koertge says feminist epistemologists want “to place ideological

constraints on the content of science,” (230) raising the specter of Lysenkoism (229). Almeder says that they aim “to dogmatize and indoctrinate by eliminating open discussion,” (190) and by replacing “traditional canons of evidence and argument” with the test of “whether [an idea] furthers the political interest of the oppressed” (193). Political correctness is the most serious charge against feminist epistemologists, since it accuses them not merely of error or cynicism, but of intellectual dishonesty, dogmatism, and even tyranny. What is the evidence for it?

Haack cites Helen Longino’s underdetermination argument for permitting values to influence theory choice. Longino observes that the evidence by itself underdetermines hypothesis choice; it supports a given hypothesis only in conjunction with background assumptions. In practice, at the cutting edge of research, incompatible background assumptions are available for interpreting the evidence. Since various background assumptions could be legitimately selected for any reason, no logical or methodological principles prevent scientists from choosing some on account of their congruence with their moral or political values. Longino concludes that feminists may select their background assumptions on account of their congruence with feminist values. In so selecting her background assumptions, the feminist scientist “admits political considerations as relevant constraints on reasoning” (Longino 1990, 193). This is the key passage Koertge cites to support her claim that Longino “advocate[s] curtailing the hypothesis space” by political values in the context of justification--i.e., at the point at which the truth of hypotheses is being determined (227).

Haack and Koertge suggest several worrying possibilities that Longino’s defense of feminist science might raise. First, a hypothesis might be accepted, even if the evidence suggests it is false, because it serves some political interest to believe it. Similarly, a hypothesis might be rejected, even if the evidence suggests it is true, because it serves some political interest to disbelieve it. This is the worry suggested by Haack’s analogy of feminist science with “sham reasoners” (15), and Koertge’s suggestion that feminist epistemologists might want to “make an exception” in favor of “hypotheses without an empirical basis” when they are “deemed . . . politically progressive” (231). Second, political constraints on the investigation of acceptable hypotheses might be imposed on the scientific community as a whole, thereby undermining their intellectual freedom, as well as preventing some scientists from discovering certain truths (16, 229). Third, science would be hobbled by rancorous political debates over which political values should constrain acceptable hypotheses (229-30). Fourth, the epistemic authority of science would be undermined, as people came to see that scientists were reaching conclusions on the basis of political considerations rather than the evidence (230). Finally, in cases where the evidence is not sufficient to decide between rival hypotheses, Longino’s argument would let us “choose to believe whatever theory suits our political purposes,” when the correct stance to take in this case is a suspension of judgment (12).

Does Longino’s defense of feminist science really lead in these worrisome directions? Interpretations of others’ views should be tested for consistency with what they say directly about the issues in question, and with their other core ideas. In Longino’s case, the other core ideas are her account of the standards of evaluation for scientific theories, and her account of objectivity. With respect to standards of evaluation, Longino is an empiricist. While different scientists may stress different values (e.g., reductionism vs. ontological heterogeneity), one standard is mandatory for all scientists: empirical adequacy--“that is, truth of the observationally determinable portion of theories or models” (Longino 2001, 185–6; See also Longino 1993, 261–3; Longino 1994, 476). With respect to objectivity, Longino argues that an epistemic community’s conclusions are objective, or count as knowledge, insofar as they

are the product of “effective critical interactions.” Such interactions “transform the subjective into the objective, not by canonizing one subjectivity over others, but by assuring that what is ratified as knowledge has survived criticism from multiple points of view” (emphasis mine) (Longino 2001, 129). To assure that critical interactions are effective, an epistemic community must (1) establish public forums for criticism, (2) change its beliefs and theories in response to criticism, (3) in accordance with shared public standards, and (4) recognize a “tempered” equality of intellectual authority among inquirers, which allows taking their possession of cognitive virtues or vices, but disallows taking someone's social position or power, as grounds for taking someone seriously or discounting their views (Longino 2001, 128–135; Longino 1990, 76–81).

These well known aspects of Longino’s epistemology plainly rule out the worries Haack and Koertge express about the implications of Longino’s argument. The first worry, that a community might accept empirically disconfirmed hypotheses for political reasons, is ruled out by Longino’s requirement that theories be empirically adequate. Haack and Koertge might reply that the disconfirming evidence might never be gathered, or superior hypotheses never get the chance to be developed, because the community rules out their investigation from the start, for political reasons. But this second worry is ruled out by the objectivity requirements. Epistemic communities must hold themselves open to criticism. No one is entitled to dictate theory choice to others, because all enjoy tempered equality of intellectual authority. “Individuals have doxastic autonomy” (Longino 2001, 154). “A diversity of perspectives is necessary for vigorous and epistemically effective critical discourse” (Longino 2001, 131). Even among the subset of inquirers doing science as feminists, no particular articulation of feminist values is entitled to dominance over the rest. “Different feminist perspectives may be represented in theorizing” (Longino 1990, 194). This rules out the third worry, that the progress of science will be impaired by endless squabbling about what the politically correct values are. Since no one has the authority to demand that others adopt their values, each is free to choose background assumptions in accordance with their own personal values.

What about the fourth worry, that the epistemic authority of science will be undermined once people become aware that theory choice is being influenced by political considerations? Since all theories must meet the test of empirical adequacy, this worry could arise only in the case of “ties,” where rival research programs can each claim empirical successes and neither is refuted by the evidence. Longino clearly states that in such cases, “as long as both frameworks offer coherent and comprehensive accounts of the relevant data, neither can displace the other” (Longino 1990, 130). In other words, the scientific community should not consolidate a position around one or the other view. Individual scientists should remain free to further develop either theory. Longino therefore does not disagree with Haack about the proper epistemic attitude the community should take in cases of empirical “ties.” So the fifth worry is also baseless. And this provides an answer to the fourth worry. Although individual scientists may continue to pursue one or another theory because it is more congruent with their political goals, no particular theory will be incorporated into a scientific consensus, to the exclusion of its rivals, until it uniquely survives criticism from all points of view, including those grounded in opposed political perspectives. Longino specifically denies that her account of science can “grant to some form of feminism or to any other social or political program an exclusive grant to truth” (Longino 1993, 270).

What, then, does Longino mean when she says that the feminist scientist “admits political considerations as relevant constraints on reasoning”? Doesn't this mean, as Koertge thinks, that Longino “advocate[s] curtailing the hypothesis space” by political values in the context

of justification? And what could it mean when Longino says we may choose background assumptions, and hence theories, in light of our values? Janet Kourany, the only contributor to SFE sympathetic to feminist epistemology, gives us a clue (210). Before scientists begin to gather evidence, they must make numerous critical choices, not only about the part of nature they will investigate, but about the questions they want answered, the terms in which they will describe that part of nature, their measuring tools and procedures for eliciting data, and so forth. They make these choices in light of their goals--some of which, Longino observes, may be political. These choices inherently constrain, in advance, the hypotheses that can be investigated. Yet such constraints are unavoidable preconditions for getting research underway. Of course, other scientists make other choices, and so are subject to different constraints. There is no question of imposing a single set of constraints on the entire body of researchers, since this would undermine the conditions of objectivity, which require pluralism. Values enter at this stage of research--what is conventionally called "the context of discovery"--not the context of justification, insofar as the latter is understood narrowly, as the point at which the truth or warrant of hypotheses is determined. Even hard-core logical positivists allowed values to influence theory "choice" at this stage--i.e., the choice of theories to develop and investigate, rather than to consolidate into the body of accepted scientific findings or shared beliefs. Koertge allows this, too. Longino's theory therefore does not violate Koertge's constraints on legitimate inquiry.

Matters may seem otherwise, because feminist epistemologists work with a richer conception of both the context of discovery and the context of justification than many other philosophers of science do. In standard accounts of the discovery/justification distinction, the only issues discussed in the context of discovery are the selection of topics to investigate and the causal origins of hypotheses. Then the account jumps to the context of justification, where the data have already been gathered, and the main issue is the bearing of evidence on the truth of hypotheses. Feminist epistemologists frequently focus on what's missing from the context of discovery in the standard accounts--the innumerable, frequently value-laden choices concerning research design, methodology, and model construction that constrain the subsequent possibilities of discovery (Anderson 1995b; Anderson 2004; Harding 1993, 56–7, 70; Longino 1990, 83–102). Feminist epistemologists have also sometimes used the term "context of justification" to refer to more than the process of determining the truth or warrant of theories. Theories are evaluated with respect to all of the goals for which they were constructed, including their utility in facilitating successful practice, and their relevance for answering particular, often value-laden, questions (Anderson 1995a; Anderson 1995b). If we construe the context of justification narrowly, to the consideration of truth or warrant alone, then feminist empiricists have strictly adhered to the orthodox view that the political desirability of reaching certain conclusions cannot determine their truth or warranted assertability--i.e., their entitlement to be incorporated into the body of accepted scientific claims.

This is not news. Feminist empiricists and their close allies have painstakingly explained these points many times before, often in direct response to critics of feminist epistemology who have accused them of "political correctness" (Anderson 1995b; Lacey 1999; Lloyd 1995; Lloyd 1997; Tiles 1987). The critics of feminist epistemology, including those represented in SFE, have shown zero uptake of these careful replies to their criticisms. They simply recycle the same accusations over and over again, on the basis of innuendo, superficial misreadings of texts, and quotations out of context. Haack's first article in SFE, for example, is reprinted unrevised, although I had replied to it in detail years ago (Anderson 1995b). As I have shown in the case of Longino's theory, which provides the central point of departure for feminist

empiricist discussions of values in science, the critics' accusations of political correctness cannot withstand even the most basic tests of credibility. To read them again in SFE reminds me of how evolutionary theorists must feel when they encounter the latest creationist assault: they are trotting out that old, long-refuted objection from the Second Law of Thermodynamics again? How many times must one refute such accusations, before one is entitled to ignore them, on the ground that the accusers have failed to live up to the terms of reasoned debate?

This question arises with most force with respect to Almeder's charges of political correctness. Unlike Haack and Koertge, Almeder does not cite any argument of any feminist epistemologist to substantiate his charge. Instead, he draws up a bill of indictment concerning the practice of feminist academics in general and challenges them to prove their innocence. Men, he says, "are regularly prohibited. . .from teaching in women's studies programs" because, "being men, their thinking is infected with the bias of the male oppressor" (191). Women's studies courses, he alleges, will not assign the work of critics of feminist epistemology such as Susan Haack, or people he calls "conservative equity feminists." And so on (190-1). The terms of Almeder's challenge are unacceptable. He has set himself up as prosecutor, judge, and jury, exempting himself from any obligation to produce evidence for his charges, since by his rules, feminists are guilty until proven innocent. This is not academic discourse. It is an inquisition. I shall discuss the implications of Almeder's conduct below. For now it is enough to observe that, having removed himself from academic discourse, he has not presented his challenges in a form entitled to normal academic response.

(b) Tribalism

Susan Haack, Noretta Koertge, Robert Klee, and Cassandra Pinnick accuse feminist epistemologists of tribalism--insisting that all women, or feminists, do or ought to think alike, in conformity with sexist stereotypes about feminine cognition. Koertge, for example, says that feminist philosophy of science was "strongly influenced by women's studies" which "greatly emphasized models of 'women's ways of knowing' and 'tended to affirm various stereotypes about female mentality and 'valorize' them" (47). Supposedly, feminist epistemologists claim that thinking in stereotypically feminine ways does or will make people both better scientists and better able to advance the interests of the oppressed. The contributors to SFE voice several objections to this position. First, Haack complains that feminist epistemologists fail "to appreciate each woman's individuality" when they echo "old, sexist stereotypes" of feminine mentality and encourage women academics to follow them (250). Second, Klee suggests that feminist epistemologists claim that "increasing the amount of feminism-informed inquiry throughout all of science must . . . lead to greater accuracy in any and all domains of inquiry" (37). He argues that such global claims about the scope of feminism-informed inquiry are indefensible. At best, such inquiry could be expected to improve theories with sociopolitical content, not theories in the natural sciences. Third, Pinnick argues that Sandra Harding lacks data to support her principle contention of standpoint epistemology, which Pinnick claims is that "feminists, as marginalized persons or as a marginalized social group, do science better than nonmarginalized persons" (24).

In contrast with the charges of political correctness, here one can find feminist texts that advance tribalist theories (and even more that offhandedly invoke untheorized tribalist claims). Carol Gilligan (1982), for example, is famous for arguing that women have a special "feminine" mode of moral reasoning, based on considerations of care rather than justice. (However, few have noticed that she thinks this mode of moral reasoning, like men's "justice"

mode, is immature, or that she advances a common ideal of moral reasoning for men and women alike). Nancy Hartsock (1983), in her original statement of feminist standpoint epistemology, argued that “feminine” cognitive styles, in their affirmation of relationality and life-affirming goals, and repudiation of dichotomous, oppositional thinking and masculine focus on death and domination, provide a superior standpoint from which to envision possibilities for overcoming oppression and building a better society. (However, as with Gilligan, Hartsock made no claim that such styles were globally superior for all domains, including physics and chemistry).

It is telling that the most unequivocal statements of tribalism in feminist epistemology date from its infancy more than 20 years ago. In failing to trace criticisms of such tribalist claims by other feminist epistemologists, and replies by advocates of standpoint epistemology, SFE paints a misleading picture of the current state of play of tribalist ideas in women's studies. Since SFE appears to be written by and addressed to readers ignorant of this history, it is worth recounting some of it here. Three debates internal to women's studies can put SFE's tribalist picture into perspective: the equality/difference debates, the feminist methodology debates, and the debates over feminist standpoint epistemology.

Contrary to Koertge's view that women's studies was dominated by advocates of a special female difference, from the start this view was hotly contested by feminist advocates of women's equality. Equality feminists argued that the empirical basis for asserting feminine differences in cognition was weak (Fausto-Sterling 1985; Tavris 1992). Moreover, the valorization of stereotypical feminine traits in practice encouraged women to remain in the confining settings in which these traits found their most congenial context (Tong 1993). This appears to be the view of the advocates of SFE, especially Haack (250). However, the claim of cognitive “equality” or “sameness” with men was also found to be defective, in uncritically accepting the androcentric valorization of stereotypically masculine mental qualities such as emotional detachment. Some feminist epistemologists argued that the cognitive potential of stereotypically feminine cognitive styles, such as those emphasizing emotion, should be taken more seriously, but detached from claims about sex differences in the expression of such styles (Jaggar 1989). Such a project would be naturalistic in temper, and hence averse to overambitious global claims about the epistemic worth of gender-symbolized cognitive traits. Rather, it would focus on the contingent epistemic advantages such traits may have in particular contexts of inquiry--for instance, moral inquiry (Jaggar 2000). The upshot of these debates within feminist epistemology, as in women's studies generally, was not simply to affirm the “difference” pole, but to seek a way beyond both “difference” and “equality.”

A similar dynamic played out in the feminist methodology debates. In the 1970s and early 1980s, feminist researchers considered the question of whether there was a single distinctive feminist methodology applicable across the human sciences. Some researchers, objecting to the homogenization of women's experience entailed by quantitative research, argued that true feminist research ought to stick to qualitative descriptions drawn from their subjects' own reports (Mies 1983). They argued that the feminist commitment to valorize women's experience, after centuries of silencing and neglect, meant that researchers should accept at face value what women report about their experiences (Stanley and Wise 1983). Again, such claims were strongly disputed by other feminist researchers. For example, Jayaratne and Stewart (1991) cogently argued that whether qualitative or quantitative methods should be used depended on the question being asked. Greaves and Wylie et al (1995) argued that quantitative, structured information gathering, which uses categories devised by the researcher rather than the individual subject of study, could help women. For example, by

showing victims of domestic abuse that other women have had similar experiences, it could relieve their sense of isolation. Sandra Harding (1987a, 1) made a decisive case for methodological pluralism, arguing “against the idea of a distinctive feminist method of research.” She also warned against “loyalty to gender”—that is, uncritically valuing supposedly feminine cognitive styles, since femininity itself is defined by and plays a supporting role in the unjust gender system that feminists want to change (Harding 1993, 59). Far from advocating or practicing a monolithic feminine or feminist methodology, contemporary feminist research is strongly pluralist, in both theory and practice. Numerous books and anthologies on feminist methodology reflect this pluralism (Burt and Code 1995; Fonow and Cook 1991; Harding 1987; Nielsen 1990; Reinharz 1992).

In the case of feminist standpoint theory, too, critical reaction within feminist circles was powerfully transforming. Feminist critics observed that there could not be a single standpoint of women, since women are differently situated by other social positions, such as race, class, and sexual orientation—a point stressed by black feminist standpoint theorists, feminist empiricists, and feminist postmodernists alike (Collins 1990; Longino 1989; Lugones and Spelman 1986). These debates led to a consensus on two points concerning any viable version of standpoint epistemology (Wylie 2003, 28). First, it rejected “essentialism,” which entails a rejection of any claims that women or feminists do or ought to think alike. Second, it rejected the attribution of “automatic epistemic privilege” to any particular standpoint. The two-pronged feminist consensus is lost on Klee and Pinnick, who represent standpoint epistemologists as asserting a blanket epistemic privilege on behalf of women, femininity, or feminists.

A closer reading of recent texts articulating feminist standpoint theory exhibits four major patterns of qualification to claims of epistemic privilege on behalf of marginalized standpoints. First, contrary to Klee’s charge, standpoint theory limits the scope of its claims. Its purpose is to generate knowledge that is useful to marginalized people in identifying their problems in social-structural terms and in overcoming them (Harding 1993, 56; Hartsock 1998, 236). Today’s standpoint theorists make no claim that the standpoint of the marginalized is privileged for generating knowledge of physics and chemistry.

Second, the critical advantage of standpoint epistemology lies in its “logic of discovery,” (Harding 1993, 56) not in affording an alternative, supposedly superior logic of justification or privileged access to the truth. “Marginalized lives provide the scientific problems and the research agendas—not the solutions—for standpoint theories” (Harding 1993, 62) (emphasis mine). If one’s aim is to produce knowledge that is useful to the marginalized in overcoming their systematic disadvantages, one ought to frame one’s research questions, devise one’s theoretical classifications, and so forth, with this aim in mind. For example, one should classify social phenomena in terms of their impact on the interests of the disadvantaged. Similarly, Hartsock (1998, 236–7, 240) emphasizes that views of the social world generated from the perspective of dominant interests are not false, but partial. The marginalized have contact with different aspects of social reality—aspects that are more revealing of the ways the status quo is unjust. Hartsock’s focus on which truths to investigate rather than on questions of the truth or falsehood of beliefs located in different social standpoints shows that she sees the critical advantages of standpoint epistemology to lie in the logic of discovery, not the logic of justification. Thus, feminist standpoint theorists, no less than feminist empiricists, respect the context of discovery/context of justification distinction, contrary to Pinnick’s suggestion that it leads to dishonest politicized research on a par with Shockley’s eugenics (22).

Third, the feminist standpoint is an achieved perspective, not to be identified with whatever women, or feminists, actually think, nor with thinking in a “feminine” way (Harding 1993, 58–59; Hartsock 1998, 236–7). It requires a pooling of experiences among the marginalized about their problems, reflection on how the social order puts people who occupy the same structural position into similar predicaments, and consideration of how collective action can change this social order. The experiences the marginalized have about their lives have the potential to be worked up into theories that are better able to overcome their systematic disadvantages. Research aiming to help them overcome their problems should start from their lives, but privileged researchers, including feminist academics, theorize from this starting point too. Pinnick’s charge (23), that Harding claims a privilege for feminist researchers on the ground that they are marginalized, is thus a plain misreading of her text. So is her claim (27) that Harding believes that “marginalized persons should take the place of present scientists in the ranks and at the cutting edge of science.” Anyone, including men, can start their research by sensitively engaging the problems of the marginalized, by, for instance, “learning to listen attentively to marginalized people” (Harding 1993, 62, 67, 68). Doing so will better equip them to think about “certain aspects of the social order,” including “gender systems” (Harding 1993, 60, 58), not all aspects of the universe.

Fourth, to the extent that any epistemic advantage is credited to the experiences of the marginalized, it is both contingent and limited in scope. One of the great contributions of feminist epistemology is its stress on the ways knowledge is socially situated, in that people’s experiences of the world are conditioned by their social roles and status, often described in terms of their parochial interests and values, and so forth. Given this socially shaped diversity of representations, it makes sense to investigate, in a naturalistic way, how it can be used as a resource for theorizing. In the most recent articulation of standpoint theory, the project is not to identify one epistemically privileged social perspective, but to identifying the contingent and local advantages different perspectives have with respect to representing certain aspects of the social world that are relevant for answering particular questions. This reminds us that certain positions of marginality may contingently afford better access to certain aspects of the world than privileged positions do--that we can learn from those less advantaged than ourselves, because of experiences they have had qua disadvantaged (Wylie 2003). Incorporating their experiences of the social world into our representations of it makes our representations more objective, in the sense of more complete, less partial.

Given all of these qualifications on the scope and nature of feminist standpoint theory's claim to epistemic privilege, which have been prominently featured in recent feminist writings, it is no wonder that Klee finds himself arguing, not with named feminists and quotations from their texts, but overwhelmingly with an imaginary feminist interlocutor. Pinnick does somewhat better, in quoting Harding’s work. But her chronic tendency to overstate the scope of Harding’s claims on behalf of the epistemic advantages of a feminist standpoint, her confusion between the thoughts of marginalized people and theorizing from a marginalized standpoint, and her misattribution of Harding’s claims about the context of discovery to the context of justification, fatally undermine her reading of Harding’s work. The charge of tribalism against feminist epistemology beats a horse killed long ago by feminist epistemologists themselves.

(c) Self-defeating Conservatism

Meera Nanda criticizes Longino's epistemology, and feminist postmodernism, for self-defeating conservatism: uncritically valuing actual women's, "feminine," and local nonwestern experiences and values, even when, as in India, they embody the very ideologies that oppress women and low caste Dalits (157, 160-5). In contrast with the other critiques of feminist epistemology in SFE, Nanda's paper makes a positive and brilliant contribution to philosophical understanding. She argues that Western Enlightenment values, materialist science, and American pragmatism, far from reinforcing sexism and colonialism when imported into India, constituted a liberating philosophy for India's Dalits (untouchables) and women. She describes how Bhimrao Ramji Ambedkar, a Dalit intellectual of the first half of the last century, launched a powerful liberation movement for India's Dalits, based on a synthesis of Deweyan pragmatism and Buddhism. Ambedkar saw that the ideology of caste was rationalized by holistic metaphysical views, according to which matter was informed by divine spirit. To undermine the caste system, Ambedkar used materialist science to undermine holism, and pragmatism to call caste traditions into question by subjecting them to scientific critique. The need to criticize specifically caste ideology (not just class, for example) came from Dalits' experiences of oppression under this system (179). But the tools of liberation were found in theories drawn from the West. Ambedkar's liberation movement inspired one million Dalits to convert to Buddhism as a means of escape from the Hindu caste system. Nanda forcefully argues that, in India today, "modern science is the standpoint of the oppressed" (181).

This is the kind of bracing philosophy that makes one's heart race: fresh, compelling, on a topic of great significance that, being unfamiliar to its audience, is truly enlightening. It is, as she suggests herself, a stunning illustration of how standpoint epistemology can actually achieve liberatory effects. It also demonstrates how science can be dedicated to the service of emancipation without degenerating into propaganda and political correctness--pace Haack, Koertge, and Almeder. Yet she casts her story as a critique of feminist epistemology! How could this be? Nanda rests her critique on three claims: that feminist epistemologists are committed to the view that Western science is inherently colonialist and sexist (157), that the experience and values of the oppressed are to be accepted uncritically, even if they have internalized dominant oppressive ideologies (160), and that "social and cultural values themselves are conceptualized as cultural givens and beyond the pale of rational criticism and reasoned change" (160-1). She criticizes Longino, in particular, for upholding feminist epistemic values such as holism, ontological heterogeneity, incorporation of local values into science, and rejecting consistency with established scientific claims--values which, in India, serve to reinforce caste and sexist oppression (164-5).

These claims about feminist epistemology are, to put the point mildly, howlers. Feminist epistemology aims, not to bash Western science, but to improve it (Lloyd 1997). It is based on confidence that scientifically rigorous empirical inquiry can be a liberatory tool, if we direct it to answer the questions arising from the predicaments of the marginalized, in ways they can use to overcome their marginalization. Far from accepting local values as uncriticized givens, feminist epistemologists, especially standpoint theorists, are centrally committed to testing established values against the experience of the oppressed of having these values manifested in their lives. As a feminist epistemologist, I have long advanced the fundamental pragmatist point that empirical and moral inquiry work in tandem, and in particular that facts can be used to criticize values (Anderson 1991; Anderson 1998; Anderson 2004). This has also been a central theme of Lynn Nelson's work (1990). As we have seen above, standpoint epistemology, too, does not simply accept whatever beliefs and values the oppressed happen to have. Standpoint theorists hold that a marginalized standpoint is achieved, not given,

forged from collective discussion and resistance to oppression, some of which is turned inward in self-criticism.

Nanda's criticisms of Longino also grossly misrepresent her views. It is true that Longino has defended feminist deployments of certain epistemic values, such as novelty (breaking with established scientific theories) and ontological heterogeneity, that, as Nanda shows, reinforce oppression in the Indian context. But she explicitly rejects the idea that they can be justified because they express a specifically "feminine" way of knowing (Longino 1994, 475). Nor are they valid simply because they are the local standards of an epistemic community. That they are local standards is descriptive, not normative. Longino stresses that there is nothing inherently feminist in these values. Other epistemic communities may adopt them for other reasons; and feminists may reject them if, in the context they are investigating, they do not serve the feminist interest in revealing the ways the gender system operates. Thus, these standards are provisional and subject to criticism and revision in light of deeper feminist goals (Longino 1994, 481).

In other words, Longino's defense of these values is contextual and contingent, as befits her naturalistic--that is, pragmatist--approach to moral epistemology:

[T]he arguments we can give for [traditional scientific values or alternative epistemic values] will be context-limited in their validity. I do not . . . want to claim that the virtues or criteria I've discussed have fixed and absolute socio-political meanings. . . . [H]eterogeneity could, in a context other than our own, fail to be a theoretical virtue with liberatory potential. . . . [T]hese standards--like the aspirations that ground them--are provisional and subject to modification as a consequence of interaction with other communities as well as with the world a community seeks to know (Longino 1997, 54).

A more concise summary of Nanda's own position would be hard to come by. Why, then, does Nanda insist on representing Longino as her enemy?

(d) Cynicism

Pinnick, Koertge, Haack, Almeder, and Nanda claim that feminist epistemologists are cynics, who believe that the pursuit of truth is impossible, and that those who profess to pursue truth are engaged in a sham, pursuing power politics under the guise of disinterested research. Their reasoning for this charge is based on two modes of argument invoked by feminist epistemologists: underdetermination arguments, such as Longino's, and postmodernist arguments. Let us consider each in turn.

Pinnick and Koertge are troubled by the supposed implications of underdetermination arguments. Pinnick complains that feminists are trying to show that "admitted logical gaps in scientific reason must be filled by noncognitive, sociopolitical, that is, arational, causal explainers" (25). Koertge claims that feminists use underdetermination to "argue that the scientific search for explanatory understanding is not only quixotic but immoral" and that "science has no deserved epistemic authority" (225). "Then they rely on historical case studies that allegedly show the content of scientific results to be permanently tainted with the ideological biases of scientists and those in power" (225).

Pinnick and Koertge get their modalities wrong. Feminists do not argue that logical gaps in scientific reason must be filled by sociopolitical causal explainers. They argue that the fact

that a theory has passed accepted tests of evidential confirmation in the context of justification cannot be used to rule out the possibility that sociopolitical values have influenced the content of science, via their influence on choices made in the context of discovery. Even the fact that the evidence plus supposedly purely cognitive values, such as simplicity and reductionism, uniquely supports a particular theory among all of the currently live options is not sufficient to rule out this possibility. For, as Longino shows, other cognitive values, such as a preference for complexity of relationship and ontological heterogeneity, could have been chosen, and there may be political grounds for preferring one set of cognitive values rather than the other (Longino 1997, 54). These are possibilities, not necessities. Whether they are realized in any actual instance requires careful empirical investigation.

Similarly, feminists do not argue that the content of science is permanently tainted with ideological biases of the powerful. They argue for the possibility and legitimacy of doing science as a feminist. That is, they defend the pursuit of empirical inquiry, in a way that rigorously adheres to the highest standards of evidential warrant, with the aim of discovering knowledge that is useful to women in liberating them from sexism. This is not cynicism or science-bashing; it is a positive affirmation of the potential of science to generate knowledge that serves the interests of the less powerful. In defending feminist science, feminists are demanding a seat at the science table, not trying to destroy it.

Pinnick and Koertge also manifest a shocking cynicism about sociopolitical values in assuming that any revelation of sociopolitical value influence on science “taints” it with “arationality.” This could be true only if sociopolitical values are not open to rational revision in light of argument and evidence. Few moral philosophers, even those of a noncognitivist bent, believe this. Moreover, this is not the general view among feminist epistemologists. Due to the usual disjunct between moral philosophers and philosophers of science, few feminist philosophers of science have closely considered the metaethical question. But those who have tend to regard moral inquiry as of a piece with scientific inquiry, and so as equally subject to rational discourse (Anderson 1998; Anderson 2004; Campbell 1998; Jaggar 2000; Nelson 1990). I know no feminist epistemologist, indeed no feminist at all, who regards sociopolitical values as closed to rational argument.

Koertge appears to project her own fears of the implications of value-laden science onto feminists when she claims that they use underdetermination arguments to strip science of its epistemic authority. She quotes no feminist who uses such arguments to this effect. Longino and Nelson, the two most prominent feminist advocates of underdetermination arguments, are science advocates who provide accounts of how science can be objective. To provide such an account is a way of recognizing the epistemic authority of properly conducted science.

Haack, Almeder, and Nanda aim their fire at feminist postmodernists. According to Almeder, they claim that “the pursuit of truth is a snare” (194). Nanda says that they “reduce the worldview, the methods, and the content of modern science to a sword that the powerful wield against the powerless” (157). Haack accuses them of a series of fallacies, based on a confusion of the features of what passes for truth with truth itself (240):

[W]hat is accepted as known fact is often enough no such thing, therefore the concept of known fact is ideological humbug; one’s judgment of the worth of evidence depends on one’s background beliefs, therefore there are no objective standards of evidential quality; science isn’t sacred, therefore it must be a kind of confidence trick (235).

All of their claims suffer from a serious defect: they cite and quote virtually no feminist postmodernists to support their claims. Almeder cites no feminist postmodernists at all. Haack refers only to Ruth Bleier, who is a biologist, not an epistemologist, in an undocumented quote that I could not trace to its original source, and which could easily be given a less cynical interpretation than Haack ascribes to it (240). Nanda names Sandra Harding as her sole representative of specifically feminist postmodernism. (She also attacks Longino, as discussed above, but Longino is no postmodernist.) She quotes Harding as saying that “the very methods and content of science are ‘deeply and completely. . . co-constructed’ by the modern West’s Eurocentrism and patriarchy” (157, inner quote Harding’s, outer quote Nanda’s). This is a misquotation. On the page cited, Harding says that modern science is “deeply and completely constituted” by “local resources.” Far from drawing a cynical conclusion from this, on the same page she says:

[N]ot all claims that sciences or science studies make are equally accurate. . . . Of course modern sciences are much more powerful and accurate in many respects than the sciences and technologies of other cultures (Harding 1998, 54).

Nanda also says: “Harding believes that a ‘woman scientist is a contradiction in terms’” (182n5, outer quote Nanda’s, inner quote Harding’s). Turning to Harding’s text, we find that the quoted words were posed as the question “Is a Woman Scientist a Contradiction in Terms?,” functioning as a heading for a discussion of the history of discrimination against women in science! It is plainly intended to describe and ridicule the sexist thinking of the male scientists who were practicing that discrimination (Harding 1986, 59). This is pretty much all the direct evidence SFE presents for the claim that feminist postmodernists are guilty of dismissing the pursuit of truth as a sham.

But doesn't everyone already know that feminist postmodernists are a bunch of science-bashing cynics? No doubt, much of the rhetoric of postmodernism is corrosive, and in the hands of people who know nothing about science, the results are frankly embarrassing, as the Sokal hoax demonstrated. But in the hands of someone who really knows and appreciates science--for example, Donna Haraway, who knows more science, and has a vastly richer and subtler appreciation of both its achievements and its limitations than all of the editors of SFE combined-- the results of applying postmodernist techniques of rhetorical analysis are highly illuminating. Postmodernism is not a monolith. Harding is a postmodernist, but believes in objectivity and even “accuracy,” as we see in the quote above. Haraway’s view of science is, at bottom, a combination of fallibilism, nominalism, antirealism, and the disunity of science. These are all respectable if highly contested positions in the philosophy of science. Haraway’s position is not so far from that of philosophers such as Ian Hacking and John Dupré, but we do not see the latter being demonized. Western Civilization will not be destroyed, scientific progress will not come to a screeching halt, universities will not be corrupted into propaganda machines, if their views turn out to be true.

To the extent that they are true--and in certain domains of inquiry, such as primatology, Haraway has made a strong case that they are (Haraway 1989)--her techniques of literary analysis of scientific narratives should be welcome in the toolbox of any competent practitioner of science studies. These techniques reveal the presence of choice in scientific representations, highlighting both the contingency and the constraints imposed on sense-making representations by the choices of narrative genre and metaphor that scientists make. Anyone who has sat around a table discussing with scientists how to design an empirical study to investigate certain questions, as I have, and as Haraway has, cannot fail to be

impressed by the vast number and range of choices that are made in the context of discovery, choices that are only loosely constrained by what is already known, and further choices made in the attempt to find meaningful patterns in the data that have been gathered. To highlight such choices, their contingency, and the constraints they impose on our scientific representations, is not remotely to charge scientists with propaganda, dishonesty or even error, but simply to point out that we are contributing something to the process of finding meaning in natural phenomena.

Haraway's project is to make us accept responsibility for those choices. This is not an attack on science. But it is an attack on a certain image of science. Galileo said in *The Assayer*, "I have read the book of nature, and it is written in the language of mathematics." Haraway is attacking Galileo's image of the scientist, innocently reading a book--or, to borrow Haack's metaphor, solving a crossword puzzle--authored by nature herself, in her own native language. And she does so, properly, in the name of epistemic responsibility (Haraway 1991). To deny that we have had some hand in composing the book is an attempt to escape responsibility for the implications of the choices we made in constructing our own representations of nature. To expose our hand is not to cynically debunk science, not to claim that our representations are unconstrained by the world, not to claim that we can make them however we like, just to satisfy our political interests.

Let us, following Haraway, consider the implications of the rhetorical choices made by the editors and several contributors in SFE. One of their leading implicit metaphors is the Science Wars, sometimes cast as the Cold War (15-16, 156, 185, 195-6, 229). The Cold War was an occasion for demonizing enemies within, for casting politically progressive Americans loyal to democracy and liberty as Communists and subversives. Some of the contributors to SFE seem to need enemies within, too. Feminist epistemologists are their fantasy of an academic fifth column. It is said that the first casualty of war is the truth. This is abundantly confirmed by SFE.

2. Perspective

We turn now to a subtler criterion for judging a critique of a research program: perspective. A good critique should illuminate the basis of its disagreement with the research program in question by getting critical distance on its own position in the debate. This requires that one make explicit one's own presuppositions, why one carves up the field of possibilities in the way that one does, why the rival research program might not accept that way of carving the field of possibilities, and why one's own way should be accepted. The critics of feminist epistemology in SFE often lack perspective: they don't see why feminist scholars are making the choices they do, and how these choices might make sense against an alternative understanding of the range of possibilities. I'll illustrate this point by considering Koertge's critique of feminist science studies and Almeder's critique of "radical equity feminism."

(a) Feminist Science Studies

Koertge argues that feminist case studies that purport to show the influence of gender ideology on science are based on faulty methodology. She does make explicit her own methodology: if there is a sufficient "scientific" reason for a scientist to accept a hypothesis, then there is no reason to invoke political reasons to explain why he did so (54, 58-9, 63). She complains that feminist historians of science violate this criterion, and so produce shoddy history of science.

Koertge does not seem to notice that her methodology has been hotly contested within science studies, and gone down to ringing defeat. Koertge is engaged in the project of rationally reconstructing scientific theories--showing that they are justified, by the evidence available at the time the theories were advanced, in conjunction with criteria of sound scientific reasoning that are accepted today. From an historical point of view, this methodology is anachronistic. The historian wants to understand the causal factors operating on the advocates of scientific theories at the time they were developed. Rational reconstruction should not be confused with real history. We can see why by considering Koertge's three applications of her methodology.

Koertge considers Elizabeth Potter's (2001) account of the role of gender politics in Boyle's theory of gases. Potter argues that, in Boyle's day, the available data underdetermined the choice between Boyle's mechanistic theory of gases, which admitted the possibility of a vacuum, and Linus' hylozoic theory of gases, which denied this possibility on the ground that nature "abhors" a vacuum. Radicals invoked hylozoism to support their egalitarian class and gender politics. Boyle opposed the radicals and so resisted the animistic metaphysics used to underwrite their politics, counterposing his mechanistic theory to it. Koertge challenges Potter's account, on the ground that Boyle had purely scientific reasons for opposing Linus' theory: in ascribing emotions and desires to nature, it did not meet Boyle's standard of intelligibility. Given that scientific criteria were sufficient to explain Boyle's theoretical choice, Potter has no justification for dragging in political grounds to explain it (58).

Koertge's rational reconstruction of Boyle's reasoning is anachronistic. It supposes that a norm of sound scientific reasoning advanced today--namely, that one should not reject a scientific hypothesis because one objects to its political implications--was accepted by Boyle. Boyle did not observe this norm. He rejected hylozoism because he thought it led to heathenism and idolatry (Potter 2001, 119–20). Moreover, the clear distinctions we see today between mind and matter (a distinction that underwrites the accusation of unintelligibility against hylozoism, which ascribed intentions to nature), science and religion, science and politics, facts and values, science and magic, were blurred in Boyle's day. Boyle shared with the hylozoists the assumption that the study of nature will reveal one's duties and goals in life (Potter 2001, 118–9). Given this premise, it is not unreasonable to reject a scientific theory on the ground that it leads one morally astray, as judged by an independent source of knowledge about how to live (in Boyle's case, the Bible), when there is an alternative available that is equally supported by the evidence and has moral implications congruent with that source.

Koertge also considers Londa Schiebinger's explanation of why Linneaus named mammals "mammals." According to Schiebinger (1993), Linneaus' choice was underdetermined by the evidence, since mammals have other characteristics in common besides mammary glands--for instance, hair on their bodies, and "hollow" ears. Indeed, the decision to focus on breasts as the distinguishing characteristic of mammals is odd, since male mammals lack them, and stallions even lack teats. Schiebinger connects Linneaus' choice to his gender politics. At the time, privileged women across Europe were sending their babies to lower class wet-nurses. This practice was controversial, not just for health reasons (the death rate among wet-nursed infants was high) but for political reasons as well: ensuring that women stay at home nursing their own babies was explicitly tied to keeping them out of politics. Linneaus was a leader in the movement against wet-nursing. Schiebinger argues that Linneaus chose to highlight breasts as the defining characteristic of mammals in part because this naturalized and so legitimated the political norm that mothers nurse their own babies.

Against this, Koertge advances an alternative “scientific” explanation of why Linnaeus chose mammary glands as the defining characteristic of mammals. Scientific nomenclature should highlight scientifically important features of the phenomena being classified. The source of an animals’ food in infancy is obviously biologically important; possession of hair or hollow ears was not obviously biologically important at the time. Since a purely scientific reason for the nomenclature is sufficient to explain Linnaeus’ choice, Koertge says, there is no need to invoke political reasons to explain it (54).

Koertge’s argument cannot fly. Schiebinger (1993, 392) observes that defining mammals as “suckling ones” would equally well call attention to their food in infancy, and have the added advantage of including all members of the class, not just the female ones, in the defining characteristic. But it would not be so politically salient as “mammals,” since it would only naturalize the norm that babies should feed from some woman’s breast, not that every mother should breast-feed her own infant. More importantly, Koertge’s standard for scientifically sound nomenclature, that it highlight scientifically important features, was neither advanced nor observed in Linnaeus’ day (Schiebinger 1993, 391–2). Linnaeus, for the most part, observed a norm of conservatism in nomenclature, carrying over the long-accepted Aristotelian terms for the different classes of animals. These terms did not necessarily track features with obvious biological significance. For example, the class of worms, Vermes, was named after the red-brown color of the earthworm, although worms come in many colors (Schiebinger 1993, 384). Linnaeus’ broke tradition in the nomenclature for animal classes only in the case of mammals (which, in the Aristotelian system, were Quadrupedia). Koertge’s rational reconstruction of Linnaeus’ nomenclature fails to track the historical practice she claims to explain.

Koertge similarly misses the boat in her critique of Anne Fausto-Sterling’s *Sexing the Body* (2000). Fausto-Sterling argues that the biological division of humans into two sexes is anatomically and developmentally unjustified, because it fails to account for intersexed individuals. She also criticizes sexually dimorphic classifications of entities other than individual animals, such as the division of steroids into male and female hormones. Such classifications reinforce the Procrustean bed of dualistic sex assignment, even though their biological justification is weak: all humans produce both estrogen and testosterone, which perform numerous functions having nothing to do with sex or reproduction. Koertge triumphantly objects that distinguishing male from female hormones was scientifically justified, because they were first respectively isolated from men’s and women’s urine (63).

This is a howler presented as a drubbing. Instead of refuting feminist science studies, it illustrates one of its main points: that there is no clear line between scientific and cultural ideas. “Men” and “women” are cultural classifications, imported into science in the context of discovery and not necessarily eliminated in the context of justification, even though they are freighted with political significance--exactly as feminist proponents of underdetermination arguments, such as Longino, Nelson, and Harding, have long argued. As Fausto-Sterling (2000, 116) puts the point, “scientists do not simply read nature to find truths to apply in the social world. Instead, they use truths from our social relationships to structure, read, and interpret the natural.” In failing to contend with this basic point of feminist science studies, Koertge unwittingly begs the question against her opponents.

Koertge might have noticed her own slip had she applied the same methodology to feminist science studies that she applies to the scientists in their case studies. Instead, she applies a

contrary methodology: if there is an irrational way to get the same conclusions that feminist science studies draws from its case studies, accuse the feminist scholars of using that method. Thus, she complains that Schiebinger's technique of linking Linneaus' classifications to gender politics proves too much, because one could always read gender ideology into any classification (51-2, 54). Koertge's accusation is unjust on many levels. It is not true that one can plausibly find gender ideology in any classification: try reading it into "quadruped" or "hollow-eared." Moreover, Linneaus' focus on mammary glands is gendered on its face. This is not a case of imaginatively projecting gender onto unsexed objects, such as bowls and pens. Finally, Schiebinger is manifestly not playing a simple parlor game of word associations, in which one "wins" as soon as one concocts a speculative gendered meaning behind any classification. She meticulously documents Linneaus' actual historical involvement in the gender politics of breast-feeding.

Koertge's irrationalist explanatory methodology for feminist science studies disregards empirical evidence, just as rational reconstruction does. Her asymmetry of explanatory methodologies for science and feminist science studies is also unjustified. Indeed, it is unfathomable, except as an illicit strategy to yield the foregone conclusions that science is free of gender ideology and feminist science critics are all wrong. How ironic, then, that SFE accuses feminist epistemologists of corrupting science to guarantee conclusions reached in advance of a fair assessment of the evidence.

(b) "Radical Equity Feminism"

Almeder divides feminism into two branches: "conservative equity feminism" and "radical equity feminism." Both branches of feminism, he says, share a common goal: ensuring that males and females have equal access to the traits, skills, and opportunities that confer advantage in society, and do not suffer discrimination in the rewards they can reap from exercising these traits and skills (183). But they disagree about the necessary means to this goal. Conservative equity feminists argue that capitalist democracies can and will achieve it. Radical equity feminists argue that this goal cannot be achieved without changing "biology, some gender roles, or the system of political economy" (188). Almeder argues that the vast majority of academic feminists are "radical equity feminists" (197-8n2) and that they should be faulted for dogmatically insisting on their own ideology, regardless of the evidence (185), and for refusing to engage "conservative equity feminists" such as Christina Hoff Sommers, in debate (191).

Almeder observes that academic feminists themselves do not draw his distinction between "conservative" and "radical" equity feminism. He is sure this is because radical equity feminists want to shut conservative equity feminists out of debate. A more plausible explanation is that this taxonomy is not an intellectually serious way to represent the range of feminist thought. It is largely irrelevant. The vast majority of research in women's studies is in fields such as anthropology, literature, history, psychology, sociology, medicine and public health. It is neither necessary nor helpful for researchers asking questions in these fields to take a stand on "conservative" versus "radical" equity feminism.[2] Almeder's classification does have prima facie relevance for the small part of women's studies devoted to "grand theory" in political philosophy. But his 1970s taxonomy, which compresses Marxist, socialist, and "radical" feminism (Firestone!) into the catch-all "radical equity feminism," is obsolete. It ignores the academy-wide demise of Marxism since the end of the Cold War, the decline of grand theory since the 1980s, the increasing engagement of feminist theory with issues of race, ethnicity, religion, multiculturalism, and international human rights, and the rise of

postmodernist feminism, which dramatically shifted feminist theory away from political economy and biological sex differences, toward issues of cultural representation, gender identity, and sexuality.

Even when we focus on issues that his political economy framework should be able to handle, Almeder's argument, that there is no issue of concern to feminists that cannot and will not be solved within the confines of capitalist democracy, is breathtakingly naive. Consider, for example, the feminization of poverty. Almeder claims that, since women are a majority, they should be able to vote in whatever economic policies of gender equity they like (186). Robert Nozick once argued that capitalism has little to fear from democracy, since it is easier for the top 50% to bribe the next 1% to join them, than it is for the bottom 50% to swing the middle 1% to their side. I leave it as an exercise for the reader to consider how Nozick's argument can be applied to class differences among women, to see why the feminization of poverty is such a stubborn issue in capitalist democracies.

Almeder's definition of "conservative equity feminism" also suffers from critical ambiguities. He contrasts "conservative equity feminism" with radical feminists, who want to change "some gender roles" (188), so presumably the conservative feminists want to keep current gender roles intact. Yet he leaves open the possibility that conservative feminists "would not require the institution of the biological family or anything like the classical nuclear family" (187), and could embrace a gender-equitable division of domestic and child-care labor, with male and female partners "working and sharing child-care equally" (198n6). This sounds an awful lot like a radical revision of "some gender roles." Is his point that he still envisions such revisions happening within a framework of normative heterosexuality (198n6)? Then his claim that "conservative equity feminism" accommodates the concerns of the mainstream National Organization of Women (187) is false, since NOW strongly endorses gay and lesbian rights to marriage. His account of the political economy of "conservative equity feminism" is equally slippery. Sometimes he suggests a narrowly meritocratic view of economic distribution, in which men and women have equal access to opportunities to develop merit, but must accept whatever the market rewards for their merit, in contrast with "equal distribution" (185). Other times he suggests that his view is compatible with a property system that provides "publicly funded birth control and abortion," "employer-funded maternity leaves," and even full compensation to women for the costs of having children (186-7). This sounds an awful lot like Scandinavian social democracy, which is called "socialism" in the U.S. If Almeder wants to call this "capitalism," because it still leaves the ownership of the means of production in private hands, and limit "radical equity feminism" just to those who want the comprehensive abolition of private property, then I wonder whether there are any "radical equity feminists" left in the post-Cold War world. Comprehensive state ownership and control of the economy is not an issue on the agenda of any significant feminist writing today.

Almeder's division of feminists into "conservative" and "radical" is analytically useless. Why then, does he insist on it? Because he casts the Science Wars as an ideological continuation of the Cold War, with "academic feminists" playing the role of communist subversives. During the Cold War, the United States deployed a similarly slippery dichotomy between capitalism and communism in an effort to force Third World countries to take sides. The ambiguity was deliberate. While seeming to offer Third World countries the possibility of substantial redistributive and regulatory policies, such as those found in the Western European social democracies, in practice those who joined the "capitalist" side were subjected to asymmetrical trade regulations, undemocratic IMF control of their economic policies, forced abolition of

social safety nets, and grievously burdensome debt payments. No wonder poor countries sought a third way in the nonaligned movement. The capitalist/communist dichotomy was never an intellectually serious way to divide the field of possibilities. It was a bludgeon used to force poor countries into political alignment with one or the other side. Almeder similarly uses the dichotomy between conservative and radical equity feminists as a bludgeon to force academic feminists to take sides. No wonder they, too, prefer not to engage this “debate,” as Almeder complains. There is no debating with a bludgeon. This of course does not mean that academic feminists ignore feminist advocates of capitalist democracy. John Stuart Mill and John Rawls arguably fit that bill, and are canonical figures in feminist political theory reading lists.

3. Normative Consistency

A sound critique of a field of work should apply the same normative standards to itself that it applies to the field it criticizes. SFE articulates several standards of acceptable work: (a) avoiding gross error; (b) avoiding tribalism; (c) avoiding cynicism; (d) avoiding political correctness; (e) civility. Let us consider how SFE measures up against its own normative standards.

(a) Avoiding gross error

Munévar articulates a strong standard of error avoidance: if one can find enough gross errors in a work, even if on incidental matters, it is so incompetent that its other theses aren't even worthy of serious consideration (142). He applies this standard to Sandra Harding's work, convicting her of numerous blunders--not knowing that the Nobel Prize is not awarded posthumously, not knowing that the library of Alexandria was destroyed long before the 17th c. scientific revolution in Europe, confusing the Dark Ages with the Middle Ages, being snookered by a hoaxer claim that a West African tribe had advanced astronomical knowledge centuries ago, and so forth. These are, indeed, embarrassing errors, albeit not critical to Harding's epistemology. Munévar concludes that her work is so substandard that it should be dismissed without seeing if anything worthwhile can be salvaged from it.

What if we applied the same standard to SFE? We have seen that SFE itself is riddled with gross blunders. The bulk of the book consists of flat misreadings of the work of feminist epistemologists that cannot withstand the simplest tests of consistency with what feminist epistemologists say. “The reader's embarrassment grows, with each amazing example” says Munévar of Harding (143)--a sentiment that neatly captures my own reaction to SFE. By Munévar's standard, then, SFE itself should be dismissed out of hand.

Most feminists reject Munévar's standard. Errors should be avoided, of course. But sometimes even seriously flawed work can be instructive. Gilligan's research is a case in point. Many feminists, including myself, reject its main theses and methodology. Despite these flaws, Gilligan has made several important contributions: revealing the androcentrism of Kohlberg's theory of moral development; launching a rich research program in moral philosophy on the ethics of care; inspiring naturalized approaches to moral epistemology (Jaggar 2000, 460). Harding's work is another. Contrary to Munévar's claim (154), Harding does not set the gold standard in feminist epistemology. That standard is set, as it is in the philosophy of science generally, by those doing original first-order research on science itself. Harding's work overwhelmingly consists of commentary on and synthesis of what the first-order researchers are doing. People who do this work do not always have full command of the

underlying science. Yet her work has been invaluable in setting agendas for feminist research, synthesizing trends, and bringing important first-order research to the attention of scholars working in the field.

Munévar's standard is motivated more by the desire to make a quick kill of a young research program than to learn anything. The feminist approach is more fruitful. Consider, for example, Soble's critique of Keller's idea of dynamic objectivity as manifested in Barbara McClintock's "feeling for the organism." Soble's critique is unusual in SFE for actually offering a close reading of a feminist text that seriously considers alternative interpretations of particular passages. (Crasnow offers the only other case). Yet even he rushes to judgment in concluding that Keller's notion of dynamic objectivity is either empty, trivial, absurd, or, if interpreted intelligibly, not novel, and in any event not testable (88-9). There are ways to develop her elusive idea into a substantive, interesting, testable hypothesis. For example, Sarah Hrdy (1986), the distinguished feminist primatologist, has raised some highly interesting questions, along lines similar to Keller's, concerning the epistemic fruitfulness of cultivating empathy for the primates under study. (Lest one think Hrdy is beating a tribalist drum, it's worth noting that, strongly counter to feminist stereotype, she's a hard-core sociobiologist.)

Applying the feminist standard (of being open to learning even from flawed work) to the object of my review, I do not dismiss SFE out of hand, even though it commits one blunder after another, oblivious to its own gross incompetence in interpreting feminist epistemology. Nanda's essay, in particular, is well worth rescuing from its own grievous errors. Crasnow's and Kourany's essays are models of respectful, intellectually serious critical scholarship, instructively read together (for they take opposite sides on the proper role of realism in the philosophy of science), and worthy of a more dignified venue than SFE.

(b) Avoiding tribalism

SFE tells us that it's wrong to think that all women think alike. But apparently SFE thinks it's ok to think that all feminist epistemologists think alike. Munévar infers from Harding's case alone that all work by feminist epistemologists is shoddy (154). Klee and Almeder go on for pages about how feminist epistemologists must think, without even bothering to link their speculations to any particular feminist text. SFE tells us that feminist epistemologists are a bunch of cynical, tribalist, politically correct traitors to truth.

(c) Avoiding cynicism

SFE tells us that it is wrong to be cynical about the project of advancing knowledge. Yet Pinnick and Koertge appear to be cynics about the project of advancing our moral knowledge. Science isn't an "arational" power struggle, but apparently ethics is. More importantly, SFE manifests a crude cynicism about feminist epistemology that turns out to be intellectually crippling. If one begins with a cynical premise, it's hard to work up enough charity toward the research program to think oneself out of it by engaging with the texts.

(d) Avoiding political correctness

Political correctness comes in two forms. The weaker form consists in research that, wittingly or unwittingly, is rigged to reach a foregone conclusion favored by one's political agenda. The stronger form consists in an attempt to censor a research program because one thinks it is

politically dangerous. Koertge's asymmetrical interpretive methodology--rational reconstruction for science, irrational reconstruction for feminist science studies--is hard to explain on any other ground than weak political correctness.

More significantly, Almeder's essay, despite its claim to repudiate political correctness, is overtly a rationale for strong political correctness. He attempts to rationalize the expulsion of "radical equity feminism" from the academy on political grounds. In three quick pages, Almeder attempts to rule out a priori the possibility that the achievement of feminist goals might require any other system besides capitalist democracy that leaves human biology (and gender roles?) intact (186-188). Given this "proof", he thinks it a simple matter to show that anyone who thinks otherwise, as "radical equity feminists" are defined to be, and "academic feminists" are presumed to be, is dogmatically committed "to a particular point of view no matter what the future may bring by way of possible disconfirming evidence" (185).[3] Since dogmatism amounts to a betrayal of the core commitment of the university--to evaluate political ideologies on the merits rather than to promulgate them uncritically--"the academic community should not support academic feminism" (185). In other words, the university should not permit radical feminist views to be aired, because they are inconsistent with the ideology of capitalist democracy, which is known in advance of such discussion to be the politically correct view.

(e) Civility

Almeder insists that academic discourse be conducted on terms of civility and cordiality with those with whom one disagrees (189). Yet this does not stop him from assuming an inquisitorial mode toward feminists, or from hurling invective. We know what's coming when Nazism and Lysenkoism are invoked (199)! His emotional tone and rhetoric, like Christina Hoff Sommers', is unhinged. Sommers eagerly leaps from supposed findings of error or omission to accusations of dishonesty (see Appendix). Yet Almeder can only think of insidious reasons why feminists refuse to debate her.

Conclusion

Scrutinizing Feminist Epistemology is a failure by my evaluative standards of accuracy, perspective, and normative consistency. It is a failure by its own evaluative standards of civility and avoiding gross error, tribalism, cynicism, and political correctness. A few contributions--Kourany's, Crasnow's, and, with substantial correction, Nanda's--are worth salvaging. Soble's essay contains some insights, but, being antecedently committed to finding nothing worthwhile in the work he considers, ultimately bears little fruit. On the whole, however, SFE is so grossly misleading about its subject matter that it cannot be recommended to anyone.

References

Anderson, Elizabeth. 1991. "John Stuart Mill and Experiments in Living." *Ethics* 102:4-26.

Anderson, Elizabeth. 1995a. "Feminist Epistemology: An Interpretation and Defense." *Hypatia* 10:50-84.

Anderson, Elizabeth. 1995b. "Knowledge, Human Interests, and Objectivity in Feminist Epistemology." *Philosophical Topics* 23:27-58.

- Anderson, Elizabeth. 1998. "Pragmatism, Science, and Moral Inquiry," ed. Robert Westbrook. In *In the Face of the Facts: Rethinking Moral Inquiry in American Scholarship*. Washington, D. C.: Woodrow Wilson Center Press.
- Anderson, Elizabeth. 2004. "Uses of Value Judgments in Science: A General Argument, with Lessons from a Case Study of Feminist Research on Divorce." *Hypatia* 19(1): 1–24.
- Burt, Sandra D, and Lorraine Code, eds. 1995. *Changing Methods: Feminists Transforming Practice*. Peterborough, Ont. Orchard Park, NY: Broadview Press.
- Campbell, Richmond. 1998. *Illusions of Paradox: A Feminist Epistemology Naturalized*. Lanham, Maryland: Rowman & Littlefield.
- Collins, Patricia Hill. 1990. *Black Feminist Thought: Knowledge, Consciousness and the Politics of Empowerment*. Boston: Unwin Hyman.
- Fausto-Sterling, Anne. 1985. *Myths of Gender*. New York: Basic Books.
- Fausto-Sterling, Anne. 2000. *Sexing the Body: Gender Politics and the Construction of Sexuality*. New York: Basic Books.
- Fonow, Mary, and Judith Cook, eds. 1991. *Beyond Methodology: Feminist Scholarship as Lived Research*. Bloomington: Indiana University Press.
- Gilligan, Carol. 1982. *In a Different Voice*. Cambridge, Mass.: Harvard University Press.
- Greaves, Lorraine, Alison Wylie, and the staff of the Battered Women's Advocacy Center. 1995. "Women and Violence: Feminist Practice and Quantitative Method." In *Changing Methods: Feminists Transforming Practice*, ed. Sandra Burt and Lorraine Code. Peterborough, Ont.: Broadview Press.
- Haraway, Donna Jeanne. 1991. "Situated Knowledges: The Science Question in Feminism and the Privilege of Partial Perspectives." In *Simians, Cyborgs, and Women: The Re-Invention of Nature*. New York: Routledge.
- Haraway, Donna. 1989. *Primate Visions*. New York: Routledge.
- Harding, Sandra G. 1998. *Is Science Multicultural?: Postcolonialisms, Feminisms, and Epistemologies*. Bloomington, Ind.: Indiana University Press.
- Harding, Sandra, ed. 1987. *Feminism and Methodology*. Bloomington: Indiana University Press.
- Harding, Sandra. 1986. *The Science Question in Feminism*. Ithaca: Cornell University Press.
- Harding, Sandra. 1987a. "Introduction: Is There a Feminist Method?" In *Feminism and Methodology*, ed. Sandra Harding. Bloomington: Indiana University Press.

Harding, Sandra. 1993. "Rethinking Standpoint Epistemology: 'What is Strong Objectivity?'," eds Linda Alcoff and Elizabeth Potter. In *Feminist Epistemologies*. Totowa, N.J.: Routledge.

Hartsock, Nancy. 1983. "The Feminist Standpoint: Developing the Ground for a Specifically Feminist Historical Materialism," Sandra Harding. In *Discovering Reality: Feminist Perspectives on Epistemology, Metaphysics, Methodology, and Philosophy of Science*, ed. Sandra Harding and Merrill Hintikka. Dordrecht, Holland: D. Reidel.

Hartsock, Nancy. 1998. "The Feminist Standpoint Revisited." In *The Feminist Standpoint Revisited and Other Essays*. Boulder, Col.: Westview Press.

Hrdy, Sarah. 1986. "Empathy, Polyandry, and the Myth of the Coy Female." In *Feminist Approaches to Science*, ed. Ruth Bleier. New York: Pergamon.

Jaggar, Alison. 1989. "Love and Knowledge: Emotion in Feminist Epistemology," eds Ann Garry and Marilyn Pearsall. In *Women, Knowledge, and Reality*. Boston: Unwin Hyman.

Jaggar, Alison. 2000. "Ethics Naturalized: Feminism's Contribution to Moral Epistemology." *Metaphilosophy* 31(5): 452–68.

Jayaratne, Toby, and Abby Stewart. 1991. "Quantitative and Qualitative Methods in the Social Sciences: Current Feminist Issues and Practical Strategies," eds Mary Fonow and Judith Cook. In *Beyond Methodology: Feminist Scholarship as Lived Research*. Bloomington: Indiana University Press.

Lacey, Hugh. 1999. *Is Science Value Free : Values and Scientific Understanding*. New York: Routledge.

Lloyd, Elisabeth. 1995. "Objectivity and the Double Standard for Feminist Epistemologies." *Synthese* 104:351–81.

Lloyd, Elisabeth. 1997. "Science and Anti-Science: Objectivity and Its Real Enemies." In *Feminism, Science, and the Philosophy of Science*, ed. Lynn Nelson and Jack Nelson. Dordrecht: Kluwer.

Longino, Helen. 1989. "Can There Be a Feminist Science?" eds Ann Garry and Marilyn Pearsall. In *Women, Knowledge, and Reality*. Boston: Unwin Hyman. Orig. pub. in *Hypatia* 2 (1987): 51–64.

Longino, Helen. 1990. *Science as Social Knowledge*. Princeton: Princeton University Press.

Longino, Helen. 1993. "Essential Tensions--Phase Two: Feminist, Philosophical, and Social Studies of Science," eds Louise Antony and Charlotte Witt. In *A Mind of One's Own*. Boulder, Col.: Westview Press.

Longino, Helen. 1994. "In Search of Feminist Epistemology." *Monist* 77:472–85.

Longino, Helen. 1997. "Cognitive and Non-Cognitive Values in Science: Rethinking the Dichotomy." In *Feminism, Science, and the Philosophy of Science*, ed. Lynn Nelson and Jack Nelson. Dordrecht: Kluwer.

Longino, Helen. 2001. *The Fate of Knowledge*. Princeton, N.J.: Princeton University Press.

Lugones, Maria, and Elizabeth Spelman. 1986. "Have We Got a Theory for You! Feminist Theory, Cultural Imperialism, and the Demand for 'The Woman's Voice,'" ed. Marilyn Pearsall. In *Women and Values*. Belmont, Cal.: Wadsworth. Originally published in *Women's Studies International Forum* 6 (1983): 573–581.

Mies, M. 1983. "Toward a Methodology for Feminist Research." In *Theories of Women's Studies*, ed. G. Bowles and R. Duelli Klein. Boston: Routledge & Kegan Paul.

Nelson, Lynn. 1990. *Who Knows? From Quine to a Feminist Empiricism*. Philadelphia: Temple University Press.

Nielsen, Joyce, ed. 1990. *Feminist Research Methods*. Boulder, Col.: Westview Press.

Pinnick, Cassandra, Noretta Koertge, and Robert Almeder, eds. 2003. *Scrutinizing Feminist Epistemology: An Examination of Gender in Science*. New Brunswick, N.J.: Rutgers University Press.

Potter, Elizabeth. 2001. *Gender and Boyle's Law of Gases*. Bloomington and Indianapolis: Indiana University Press.

Reinharz, Shulamit. 1992. *Feminist Methods in Social Research*. Oxford: Oxford University Press.

Schiebinger, Londa. 1993. "Why Mammals Are Called Mammals: Gender Politics in Eighteenth Century Natural History." *American Historical Review* 98(2): 382–411.

Stanley, Liz, and Sue Wise. 1983. *Breaking Out: Feminist Consciousness and Feminist Research*. London: Routledge and Kegan Paul.

Tavris, Carol. 1992. *The Mismeasure of Woman*. New York: Simon and Schuster.

Tiles, Mary. 1987. "A Science of Mars or of Venus?" *Philosophy* 62:293–306.

Tong, Rosemarie. 1993. *Feminine and Feminist Ethics*. Belmont, Cal.: Wadsworth.

Wylie, Alison. 2003. "Why Standpoint Matters." In *Science and Other Cultures*, ed. Robert Figueroa and Sandra Harding. New York: Routledge.

FOOTNOTES

[1]: Numbers in parentheses refer to page numbers of SFE.

[2]: I invite the reader to examine the Women's Studies faculty webpage at my own university, and consider how useless Almeder's taxonomy is for understanding the overwhelming majority of research questions posed there.

[3]: Almeder evidently has not considered that one might advocate an alternative system of political economy, not because the achievement of one's preferred goals is impossible under capitalist democracy, but because it is less likely under it. He also has not considered that, by attempting to rule out a priori other systems of political economy, he has convicted himself of dogmatism.