🖤 | THE UNIVERSITY OF CHICAGO PRESS JOURNALS



Feminist Epistemology: Implications for Philosophy of Science Author(s): Cassandra L. Pinnick Source: *Philosophy of Science*, Vol. 61, No. 4 (Dec., 1994), pp. 646-657 Published by: The University of Chicago Press on behalf of the Philosophy of Science Association Stable URL: http://www.jstor.org/stable/188340 Accessed: 02-05-2017 16:14 UTC

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at http://about.jstor.org/terms



Philosophy of Science Association, The University of Chicago Press are collaborating with JSTOR to digitize, preserve and extend access to Philosophy of Science

FEMINIST EPISTEMOLOGY: IMPLICATIONS FOR PHILOSOPHY OF SCIENCE*

CASSANDRA L. PINNICK†‡

Department of Philosophy Western Kentucky University

This article examines the best contemporary arguments for a feminist epistemology of scientific knowledge as found in recent works by S. Harding. I argue that no *feminist* epistemology of science is worthy of the name, because such an epistemology fails to escape well-known vicissitudes of epistemic relativism. But feminist epistemology merits attention from philosophers of science because it is part of a larger relativist turn in the social sciences and humanities that now aims to extend its critique to science, and Harding's "standpoint feminism" is the best-developed case. She attempts to make new use of discredited philosophical ideas concerning underdetermination, Planck's Hypothesis, and the role of counterfactuals in historical studies of science.

> The reason the feminist claims can turn out to be scientifically preferable is that they originate in, and are tested against, more complete and less distorting kinds of social experience. The experiences arising from the activities assigned to women, seen through feminist theory, provide a grounding for potentially more complete and less distorted knowledge claims than do men's experiences. This kind of politicized inquiry increases the objectivity of the results of research.

-Sandra Harding, "Feminist Justificatory Strategies"

1. Introduction. The central thesis of this article is that *feminist* epistemology should not be taken seriously. This is because any feminist epistemology which radically challenges traditional theories of knowledge is unable to resolve the tension between (a) its thesis that every epistemology is a sociopolitical artifact, and (b) its stated aim to articulate an epistemology that can be *justified* as better than its rivals.

To develop these issues, I concentrate on the influential work of S. Harding. Harding builds upon larger efforts to articulate a feminist perspective on society, culture, politics, and economics. She presents the

Philosophy of Science, 61 (1994) pp. 646–657 Copyright © 1994 by the Philosophy of Science Association.

646

^{*}Received February 1994; revised July 1994.

[†]For general inspiration and discussion, I acknowledge L. Laudan, R. Laudan, P. Hamlett, J. Maffie, L. Mayhew, and W. Schmaus.

[‡]Send reprint requests to the author, Department of Philosophy, Western Kentucky University, Bowling Green, KY 42101, USA.

strongest case for an epistemologically relativist, feminist critique of science, using various interpretations of T. Kuhn's *The Structure of Scientific Revolutions* (1970) and W. V. O. Quine's underdetermination thesis, the Strong Programme in the sociology of scientific knowledge, and general themes within the feminist critique of modern society. Her writings represent a forceful expansion of feminist theory into well-developed and mature areas of epistemology, and her works are cited widely in cognate fields, especially in the social studies of science.

Harding argues that feminists as epistemologists, as philosophers of science, and as scientists can and should improve science. I focus on Harding's epistemic claims on behalf of a feminist epistemology of science. Indeed, when read carefully, Harding says nothing about the plight of women generally; her arguments reach only to the fate of feminists (see Harding 1989a, 197), and she clearly takes the scope of "woman" to be distinct from that of "feminist" (1992b, 457). This differentiates her focus—at least when she writes as a philosopher of science—from that of traditional feminism which has interesting things to say about the political status of women.

I take epistemology of science to be concerned with questions about the nature of evidence for or against scientific beliefs, and with the critical assessment of the presuppositions and arguments of rival theories of scientific knowledge. One way to carry out this task is to look for indicators that a particular epistemology distinguishes itself from rivals. An epistemology might do this (1) by resolving traditional problems that have confounded other epistemologies, (2) by disclosing important new problems that have been overlooked or addressed in less-than-satisfactory means by its rivals, and (3) by using better methods to realize stated scientific aims. These epistemic benchmarks have special bearing on my critique of Harding's provocative theses about science. I focus on her controversial *epistemological* thesis that feminist and other "liberationist" theories of knowledge provide the only uncorrupted *objective* method for the evaluation of scientific claims.

2. Feminist Epistemology and Empirical Methods. Some feminist arguments attack the empirical method which is thought to provide science's epistemic rationale, unlike feminist critiques of science that focus on particular instances of sexual bias in science. Harding's criticisms, if correct, would demonstrate that empiricist epistemology and philosophy of science fail to live up to traditional empiricist standards of objective inquiry, driven by universalizable cognitive norms. She tries to show that science is an irretrievably male-biased tool of a sociopolitical power elite.

The problem with science, as Harding sees it, is not sexism. Instead, the problem is that scientific knowledge reflects a set of noncognitive interests and values which serve the political ends of Western-European, white males, while suppressing other social groups, "[Men] are a particularly poor grounding for knowledge claims since, as masculine, they represent the ruling part of society" (1989b, 274); "[S]cience is just one way of perpetrating and legitimating male dominance" (p. 281). Those in control of science are concerned with maintaining political power and with "obscuring the injustices of their unearned privileges and authority" (ibid.), thus the democratic ideal of science-for-all is impossible under the true conditions that motivate science.

Harding's remedy is not to strive for more diligence in rooting out intrusive political influences. Instead, she claims that only when the political influences that control science are acknowledged can scientific inquiry achieve genuinely objective results. For this reason feminist epistemology "sets the relationship between knowledge and politics at the center of its account in the sense that it tries to provide causal accounts to explain—the effects that different kinds of politics have on the production of knowledge" (1992b, 444). A feminist science can be genuinely objective because feminists' political status gives them a special vantage point from which to discharge the aims of science (1989b, 274). Although all epistemological perspectives distort the true nature of reality, Harding states that a feminist perspective is less distorting than others.

Despite its tone and reductionist tendencies (see, for example, Harding 1989c, 700), Harding's work does not belong to the science-bashing genre. A consistent thread in her writing argues for an improved science, not for its elimination. Her arguments on behalf of radical epistemological departures are based on the promise that a fundamental restructuring of its means will improve science. In particular, Harding argues that breaking the traditional identification of objectivity with neutrality or disinterestedness will result in better insight on nature and a concomitant improved capacity to do science. The neutrality ideal subverts scientific aims because it "defends and legitimates the institutions and practices through which powerful groups can gain the information and explanations that they need to advance their priorities" (Harding 1992a, 568).

Harding's scheme for feminist epistemology of science yields the surprising consequence that the *less* politically neutral the basis and conduct of scientific inquiry, the *more* objective the results, an anathema for most of us who are familiar with instances of politically-motivated science, such as Shockley's eugenics, or Brigham's and Grant's aptitude and intelligence-test designs. But surprising or counterintuitive theoretical consequences alone neither prove nor provide compelling grounds to suspect that Harding's arguments are wrong. Such consequences do prove the ambitious nature of her brief against the methodology of traditional empirical science. If correct, she forces a basic restructuring of empiricist ical claims that she advances.

epistemology and philosophy of science. Unfortunately, her arguments fail for the reason either that they rely on contested and dubious philosophical positions or that they lack data to support the interesting empir-

3. Feminist Standpoint Epistemology. A tradition in the philosophy and history of science holds that objective, politically-neutral inquiry maximizes the power to achieve scientific aims such as devising theories that are good predictors of natural phenomena over long periods of time for the kind of phenomena which they are designed to describe. Objectivity may be an ideal case, but despite shortfalls, historical evidence apparently supports its epistemological worth. Harding, in contrast, argues that objectivity in scientific research is a delusion, and as traditionally understood, no boon to science, "[T]he problem with the conventional conception of objectivity is not that it is too rigorous or too 'objectifying,' as some have argued, but that it is not rigorous or objectifying enough: it is too weak to accomplish even the goals for which it has been designed, let alone the more difficult projects called for by feminisms and other new social movements" (1992b, 438). To begin to comprehend Harding's novel claim, I review how her recent thinking on feminist epistemology bears on the role she assigns to objectivity in a feminist philosophy of science.

Harding's favored species of feminist epistemology is what she calls "feminist standpoint epistemology". Two basic claims underlie the theory. First, empiricist epistemology is based on the utopian ideal of objective inquiry that, in fact and in principle, impedes scientific progress. Thus, science cannot and should not strive to live up to the stated standards of empiricist epistemology, and feminist standpoint theorists reject the notion of disinterested, value-free, *objective*, scientific inquiry:

The feminist standpoint, like feminist empiricism, clearly asserts that objectivity never has been and could not be increased by the exclusion or elimination of social values from inquiry. . . [I]t is commitment to anti-authoritarianism, anti-elitism, and anti-domination tendencies that has increased the objectivity of science and will continue to do so. (Harding 1989a, 196)¹

This point is not argued successfully. First, Harding fails to show that we cannot "socialize" epistemology, but retain the concept of objective standards and rational inquiry that have been central to an empiricist the-

¹Harding claims that "feminist empiricism" puts us on the alert for what she calls "bad science", but is powerless to set in place safeguards that will prevent repeated instances of the male-biased practices and policies which pervert the spirit of scientific inquiry (1989b, 281–282).

ory of knowledge. (For discussion of this possibility see Laudan 1990, Kitcher 1990, and Goldman 1987.) Few philosophers of science presently deny that noncognitive factors play a role in science; yet, this concession to the effect of noncognitive influences on scientific belief does not endorse the slide to an arational account of science.

Harding's second contention—that feminists, being a "marginalized" social group, offer a better perspective on which to base scientific inquiry—is more interesting (1989b, 274).² She maintains that scientific results based on the perspective of marginalized persons, such as feminists, better represent nature and more nearly achieve a democratic ideal of knowledge than do scientific results based on male-oriented practices. Thus, feminist perspectives on nature are, in the true sense of the term, *objective* results, "Standpoint theory provides resources for the stronger, more competent standards for maximizing objectivity that can advance our abilities to distinguish between how different social groups want the world to be and how 'in fact' it is" (1990, 147).

Harding calls objectivity based on politically-guided scientific inquiry "strong objectivity" (1990, 1992b). This claim stands at the heart of feminist standpoint epistemology. If Harding presents—as she promises evidence that feminists, as marginalized persons or as a marginalized social group, do science better than nonmarginalized persons, she can show (1) that objectivity, a fundamental concept of traditional empiricist epistemology, must be redefined; and (2) that certain types of politicallysituated persons should be at the reins of science.

4. Philosophical Inducements for a Standpoint Epistemology. Before discussing the epistemic merits of strong objectivity, I discuss the philosophical impetus behind standpoint epistemology. Harding (1992a, 582, fn. 13) motivates feminist standpoint epistemology in several ways, but she grounds the theory on an interpretation of arguments that she and many sociologists of science attribute to Kuhn and Quine.

Whatever Kuhn's and Quine's intentions, feminist epistemologists, and their programmatic fellow travelers, clearly see Kuhn and Quine as having clinched the case for an arational, sociopolitical analysis of science. For example, Harding writes that "in effect, [Kuhn] showed that all of natural science was located inside social history. . . . [A]ny theory can always be retained as long as its defenders hold enough institutional power to explain away potential threats to it" (ibid., 582; see also 1992b, 440).

²Harding does not want to rule out that men, as well as women, may turn into feminists, and thus, presumably, take on an epistemically-privileged vantage point to criticize science (see Harding 1989b, 281). For this reason, I take her to intend that *feminists* belong to the class of marginalized persons, and that feminists are all those who see "through feminist theory", not just women.

For feminist standpoint epistemology, the noncognitive interests of Western-European, white males, who dominate and control science, fill the putative gap opened by Kuhn's and Quine's analyses of the epistemological foundations of scientific knowledge.

Harding's reliance on this interpretation speaks only to the already converted. If she wants to rework science from the inside out (Harding 1990, 146), then she needs arguments that will draw more than a yawn from philosophers of science who have expended considerable effort voicing objections to this use of the Kuhn-Quine corpus. Specifically, philosophers of science deny that the combined works of Kuhn and Quine license, even less necessitate, arational analysis of science, for the reason that no one has yet shown that admitted logical gaps in scientific reason must be filled by noncognitive, sociopolitical, that is, arational, causal explainers (see Laudan 1990 and Slezak 1991). Nor has anyone demonstrated the plausibility, much less the truth, of the existence claim that any number of possible interpretations are equally warranted under the conditions of a particular experimental project or environment. (For details of this particular criticism against appropriating Ouine's work to the relativists' cause, see Laudan and Leplin 1991.) Without answers to deflect these (and other) philosophical objections to the sort of use to which she puts Kuhn's and Quine's work, Harding's feminist theory of science is gratuitous, and the disinclined need pay it no attention.

Further inspiration mentioned by Harding for standpoint epistemology is an underlying intellectual debt to Marxist theory (1990, 140), and intellectual ties to the Strong Programme in the sociology of scientific knowledge (1992b, 463). (For discussion see Bloor 1976.) But, Harding notes, neither Marxist political theory nor Strong Programme sociology of science are sufficiently radical, "[T]he standpoint theorists see gender relations as at least as causal as economic relations in creating forms of social life and belief. . . . In contrast to Marxism, women and men are not merely (or perhaps even primarily) members of economic classes" (1989a, 197). She excuses Marxism on the grounds that it was not historically situated to be a successor epistemology. But she chides the Strong Programme for not seeing that gender issues need to be taken into account to fill out the program's analysis—hence the need to out-macho the Strong Programme with her demand for "strong" objectivity (1990, 146; 1992b, 463).

Harding does, however, embrace the Strong Programme as a source of convincing historical case studies which reveal the political nature of science that underwrites the feminist standpoint challenge (1992b, 460). Here again, Harding relies on controversial evidence to support her call for drastic epistemological change. The force of Strong Programme case studies, especially their success in establishing the conclusion that philo-

sophical accounts of scientific change can and should be replaced by arational accounts, has drawn sharp criticism. No Strong Programme case study successfully reduces science to politics, and in certain key instances the historical scholarship is selectively focused (see, e.g., Roth and Barrett 1990).

However, even without questioning the historical reliability of Strong Programme case studies, or the putative advantages attributed to the Strong Programme's reductionistic program, Harding finds no philosophical grounding here to motivate feminist standpoint epistemology. No Strong Programme case study shows *causal* connections between scientific belief and concomitant sociopolitical allegiances. At best, these studies establish temporal coincidence between cognitive and noncognitive commitments (ibid.). Still, even if some historical pattern of such temporal coincidence were demonstrated, the Strong Programme sample is too small to support generalization. In any event, none of the case studies allows for any familiar type of empirical control.

This leads to the final problem with Harding's appeal to the Strong Programme. The underlying methodology of this brand of sociological analysis of scientific change is fatally defective because each case study relies on counterfactual reasoning. And, in this instance at least, counterfactual reasoning is not persuasive because it is impossible within the venue of an historical case study to gather the necessary inductive evidence that could favor a particular explanation of the actual events over some other, allegedly possible, set of events or historical outcomes. Effective inductive regularities of the kind needed to support Strong Programme case studies are not of the following type: In similar instances of scientific change observed in the past, certain types of cognitive beliefs, Y, have been regularly associated with certain types of sociopolitical allegiances, X. Thus, we may hold that in the present circumstances, the presence of X-type of political alliances signals the presence of Y-type cognitive beliefs. If demonstrated by the Strong Programme's case studies, this kind of regularity possibly links cognitive beliefs to noncognitive causal factors (but, importantly, the causal issue would remain open). However, these kinds of regularities do nothing to establish the plausibility of the historical accounts that the Strong Programme reauires.

The Strong Programme needs to show that the historical record of science, so far as *rational* considerations are concerned, could always be different than it actually is. So, for example, had different political forces triumphed, some form of a priorism might have held sway in seventeenthcentury England rather than experimentalism. Now, if the Strong Programme argument is something more than the (obviously false) claim that the imaginability of a different outcome implies the genuine possibility that the outcome could be different, then inductive evidence of the following kind is needed: In the past, it has been observed that similar cases of scientific change have been resolved differently than they actually were resolved. However, it is impossible to observe or compare actual history with alternative histories, and the Strong Programme can only tell justso stories about how science might have been.

The Strong Programme's deep problems infect Harding's analysis. The Strong Programme and Harding each contend that complete plasticity of cognitive factors, in every case, supports the conclusion that particular instances of scientific change could have been resolved differently than they in fact were—even contradictory to the actual course of history; and that, furthermore, according to the Strong Programme argument, explaining the actual historical record of science necessarily reduces to an interplay of noncognitive causal factors. However, as I indicate, the kind of evidence required to substantiate Strong Programme reductionist claims is precisely what the Strong Programme seeks to prove, so that the Strong Programme's reliance on the historical record of science amounts to no more than a *petitio*. As such, Harding's cause is poorly served.

5. Marginalized Persons and Epistemic Privilege. I now return to Harding's (1992c, 186, 189) argument that because feminists are marginalized, they have a privileged perspective on nature. Although a marginalized perspective on nature is not infallible, it does provide a less distorted view than that from within the dominant group. This appears to be a good empirical claim, open to evaluation based on empirical data. One expects that Harding will turn her efforts to show that marginalized feminists have either a record of obtaining better results than nonfeminists and other nonmarginalized types, or that a small but remarkable body of data (inconclusive though it may be at the present) suggests that marginalized feminists could more successfully achieve scientific ends. The comparative success rates could be evaluated with regard to certain practical applications in, for example, the sciences of agriculture, medicine, or engineering. In the spirit of traditional empiricist philosophy of science, Harding's claim on behalf of feminist epistemic privilege has the welcome potential to move the discussion from an exchange of favored a priori, philosophical arguments to the relative merits of competing empirical claims.

However, this literature describes no effort to accumulate the kind of empirical data that could easily resolve matters in favor of the feminists. Philosophers of science have acknowledged the need for data in the face of challenges that seemed to come from Kuhn and Quine (Laudan et al. 1988); in their best interests, feminists should make a similar bow. To date, feminist standpoint epistemology offers no data to support the epistemological advice that marginalized persons should take the place of present scientists in the ranks and at the cutting edge of science.

To be fair to Harding's argument, she states that "historical precedents" establish that marginalized people are at the truly progressive frontiers of scientific change (1989b, 280). To co-opt her terminology, this is an instance of "bad-Kuhn" in her theorizing. The claim that only marginalized persons can effect change echoes Planck's Hypothesis which says that scientific change must wait for older scientists, those most entrenched in present scientific thought, to die off and be replaced by a new generation of thinkers who are less blinded to change and have no stake in maintaining the intellectual status quo.³

Rightly or wrongly, many thinkers (especially in the social sciences) regard this as an important truism. Harding joins ranks with present-day philosophers, historians, and sociologists who agree that age and entrenchment negatively affect the readiness with which scientists change their minds. (E.g., see Kuhn 1970, 151, and Feyerabend 1970, 203. Both Kuhn and Feyerabend quote Planck's principle in support of their thesis that scientific change is, at bottom, arational.) Enculturated minds might be more difficult to change than unformed minds. Mature scientists in the center of things presumably should be more committed to received views than beginners at the periphery of scientific circles.

These conventional truths suggest that the future of science rests with those who have a fresh approach—young scientists at the margins of scientific power—but do not require us to turn science over to, for example, marginalized feminists.

Furthermore, empirical data discredits the intuition underlying the Planck Hypothesis. For example, Hull et al. (1978) test the Planck Hypothesis against a particular episode in the history of science. The results establish that the connection between age or membership in a scientific elite and acceptance of a new scientific idea by those on the fringes of science is less important than Planck claimed. Indeed the statistical results indicate that if age correlates with an entrenched, nonmarginalized position of power in science, then older scientists and marginal, younger scientists adopt new scientific concepts at a similar rate.

Of course, this study and others like it do not foreclose the possibility that feminists have a privileged epistemological view on which science

³Planck has not been the only thinker in modern science to express this sentiment. Lavoisier, for example, remarked that "[t]he human mind gets creased into a way of seeing things. Those who have envisaged nature according to a certain point of view during much of their career, rise only with difficulty to new ideas" (quoted in Hull et al. 1978, 717). And, English biologist T. H. Huxley is notable for having advised that men of science ought to be strangled on their sixtieth birthday "lest age should harden them against the reception of new truths, and make them into clogs upon progress, the worse, in proportion to the influence they had deservedly won" (quoted in Huxley 1901, 117).

should be based. But if Harding and other standpoint feminist epistemologists intend their arguments to be taken seriously outside their own circles, then they must direct their efforts to designing studies that will generate data suggesting feminists do better science. Specifically, Harding needs to show that politically motivated research, under the guidance of feminists, accomplishes scientific aims better than research done under the auspices of the traditional empiricist, socially and politically disengaged, ideal inquirer. The empirical nature of Harding's claim on behalf of a feminist restructuring of science requires data to do the showing. At present, no data for any component of this thesis is cited in Harding's work.

6. Feminist Methods to Maximize Objectivity. Before concluding that feminist standpoint epistemology has nothing new and interesting to bring to an epistemology of science, I want to address three peculiarly feminist methods for maximizing objectivity that Harding (1990) summarizes.

First, issues important to women's lives, "distinctive features of women's social situations" (ibid., 140), have been overlooked in the course of scientific inquiry, and feminist scientists will affect the content of scientific research. However, this obvious truth does not demand radically restructuring empiricist epistemology. Indeed, one of feminism's strongest and most positive intellectual influences arguably has been in areas of scientific research that were long ignored but now command attention. Second, marginalized feminists have less to lose, and so they will be more inclined to question accepted scientific beliefs that need closer scrutiny (ibid., 145). As I have argued, despite its intuitive appeal, this is a variant of the unsuccessful Planck Hypothesis. Third, feminist standpoint epistemology is historically appropriate for this time (ibid., 146). However, no evidence supports this *ad populum* claim.

7. Closing Remarks. I have concentrated primarily on the lack of sound philosophical argument or empirical support for the most daring feminist epistemological proposals. The lack of empirical support is disappointing and damaging at present to the prospects for a feminist epistemology and philosophy of science. However, certain flagrant philosophical dilemmas cannot be ignored entirely.

It must be noted, first, that if Harding is correct that feminists are marginalized, and if it is correct that marginalization confers epistemic privilege, one wonders what happens when and if feminists achieve their goals. The standpoint case for feminist science hinges on the claim that feminists, by virtue of being a repressed political minority, acquire a special insight into the nature of natural processes. This is a blatant non sequitur. But, even worse, by this very argument, should feminists achieve political equality, they would thereby lose any claim to epistemic privilege, and feminist science would accordingly lose its claim to superiority over nonfeminist science.

Also, if Harding chooses to use the philosophical arguments that she believes license a standpoint theory of knowledge, arguments relying on Kuhn and Quine and theorizing associated with the Strong Programme, then she must own up to the logical consequences of such views. Thus, it becomes inconsistent for her to say, on the one hand, that every epistemology is a tool of the power elite and at the same time maintain that a particular epistemology, feminist standpoint, will generate "less distorted" methods and beliefs. The first claim forecloses the possibility of justifying the latter type of claim on behalf of any particular epistemology.

This problem is compounded when Harding's argument expands, as it does, to include "multiple perspectives" (see discussion in Harding 1992b). As she says, each of these "liberationist epistemologies" is credible. The "logic" of liberationist epistemologies "leads to the recognition that the subject of liberatory feminist knowledge must also be, in an important if controversial sense, the subject of every other liberatory knowledge project" (ibid., 455). But, what kind of advice does feminist standpoint epistemology have when the perceptions of different liberationist epistemologies conflict? None. And Harding views this as a welcome consequence, "In the contradictory nature of this project lies both its greatest challenge and a source of its great creativity" (ibid., 448).

A philosophy of science qua social science whose only goal is to tell inconsistent and incoherent stories is not very appealing or sufficiently ambitious.

REFERENCES

Bloor, D. (1976), *Knowledge and Social Imagery*. Chicago: University of Chicago Press. Feyerabend, P. (1970), "Consolations for the Specialist", in I. Lakatos and A. Musgrave,

- (eds.), Criticism and the Growth of Knowledge. Cambridge, England: Cambridge University Press, pp. 197-230.
- Goldman, A. (1987), "Foundations of Social Epistemics", Synthese 73: 109-144.
- Harding, S. (1989a), "Feminist Justificatory Strategies", in A. Garry and M. Pearsall, (eds.), Women, Knowledge and Reality. Boston: Unwin Hyman, pp. 189–201.
 - —. (1989b), "How the Women's Movement Benefits Science: Two Views", Women's Studies International Forum 12: 271–283.
- ———. (1989c), "Women as Creators of Knowledge", American Behavioral Scientist 32: 700–707.

——. (1990), "Starting Thought from Women's Lives: Eight Resources of Maximizing Objectivity", *Journal of Social Philosophy* 21: 140–149.

- ——. (1992a), "After the Neutrality Ideal: Science, Politics, and 'Strong Objectivity'", *Social Research* 59: 567–582.
 - —. (1992b), "Rethinking Standpoint Epistemology: What Is 'Strong Objectivity'?", *The Centennial Review 36*: 437–470.

———. (1992c), "Subjectivity, Experience and Knowledge: An Epistemology From/For Rainbow Coalition Politics", *Development and Change 23*: 175–193.

Hull, D.; P. Tessner; and A. Diamond (1978), "Planck's Principle", Science 202: 717-723.

Huxley, L. (1901), Life and Letters of Thomas Henry Huxley, vol. 2. New York: Appleton.

Kitcher, P. (1990), "The Division of Cognitive Labor", Journal of Philosophy 3: 5-22.

- Kuhn, T. (1970), The Structure of Scientific Revolutions. 2d ed. Chicago: University of Chicago Press.
- Laudan, L. (1990), "Demystifying Underdetermination", in R. Giere, (ed.), Minnesota Studies in the Philosophy of Science 14: 267–297.
- Laudan, L. and J. Leplin (1991), "Empirical Equivalence and Underdetermination", Journal of Philosophy 88: 449–490.
- Laudan, R.; L. Laudan; and A. Donovan (1988), Scrutinizing Science: Empirical Studies of Scientific Change. Dordrecht: Kluwer.
- Roth, P. and R. Barrett (1990), "Deconstructing Quarks", Social Studies of Science 20: 579-632.
- Slezak, P. (1991), "Bloor's Bluff: Behaviourism and the Strong Programme", International Studies in the Philosophy of Science 5: 241–256.