

# Feminist Philosophy of Science: High Hopes<sup>1</sup>

Evelyn Fox Keller and Helen E. Longino, *Oxford Readings in Feminism: Feminism & Science*. Oxford: Oxford University Press, 1996. Pp. v + 289. A\$110 HB.

Sally Gregory Kohlstedt and Helen E. Longino, *Women, Gender, and Science: New Directions*. Chicago: University of Chicago Press, 1997. Pp. 222. US\$25 PB.

Lynn Hankinson Nelson and Jack Nelson, *Feminism, Science, and the Philosophy of Science*. Dordrecht: Kluwer, 1996. Pp. v + 311. US\$115 HB.

By *Cassandra L. Pinnick*

## *Introduction: Raising the Bar*

**W**hat do we want from the work of feminist philosophers of science? We should expect feminists to at least provide a convincing critique of the presuppositions and arguments of rival theories of scientific knowledge and evidence that feminist philosophy of science is itself a worthy alternative. This might be accomplished by feminist theory enjoying 1) a recognised ability to disclose important new problems or problems addressed in a less than satisfactory way, or 2) demonstrated success in resolving traditional problems that have confounded other accounts, or 3) by the enunciation of better methods to realise shared scientific aims. None of this is controversial.

Yet the criteria that are appropriate to assess feminist philosophy of science are still controversial. This is so especially because the central claim made by feminist philosophy of science is that it is wholly new—and better. This central claim resounds throughout the feminist critique of

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science and no feminist philosopher of science would deny that feminist science is better than ordinary science. This is so, feminist theory asserts, on account of the special feminist insight into nature. And, having distinguished feminist philosophy of science from the run-of-the-mill, feminists say that their method ought to be assessed on feminist terms. So as not to either mislead (or disappoint) readers of this review, I should say that *I* bring to the review no privileged perspective, feminist or otherwise. Indeed, so far as the epistemology of science is considered, I would deny that there *is* any set of privileged credentials, such as feminist arguments presume. Nevertheless, and in spite of the centrality of the shaky claim to methodological privilege, I do maintain that feminist philosophy of science is to be taken seriously. Feminist philosophy of science is a serious critique. Its advocates hold their own arguments to high standards and seek impressive aims.<sup>2</sup> It remains to consider the success evident in achieving these aims.

My review here assesses three additions to the arsenal of feminist philosophy of science.<sup>3</sup> For the most part the material is either written by, edited by, or stocked with papers by personages well-known for their association with this academic niche. But there are also interesting contributions by others, who are also familiar, though not on account of their association with feminist theory. I devote sections to Kohlstedt and Longino, and to the Nelsons' book, but just a short opening section to the *Oxford Readings*.

### *Les Doyennes*

There are no males writers in *Feminism and Science*, and no junior faculty either. Moreover, as the editors themselves point out in their introduction, each and every "contributor to this volume [is] white and Western" (p. 13). This exceptional fact is excused by the editors on the grounds that the "history of the subject" and "the demographics of participation in this inquiry" required "choice" and that the available choice was "pre-made". Keller and Longino tell readers that the papers will guide a study of gender and science toward "a deeper grasp of the interconnections between race, gender, and colonialist ideologies as manifested in the sciences" (p. 13)

Here is how Donna Harraway, in her paper "The Science Question in Feminism", contributes to this deeper grasp:

We seek not the knowledges ruled by phallogocentrism (nostalgia for the presence of the one true Word) and disembodied vision. We seek those ruled by partial sight and

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limited voice—not partiality for its own sake but, rather, for the sake of the connections and unexpected openings situated knowledges make possible (pp. 258–259).

The epistemic virtue Harraway makes out of partiality and situated knowledges is as unsupported, here and elsewhere, as Harding's thesis that women's experience provides a privileged insight into nature (see Niiniluoto below).

Although the editors promise that papers in this book “raise new problems for the theory of knowledge . . . and in fact begin to forge new approaches to this subject” (p. 8), ‘newness’ amounts to no more than slogans, such as “science with a human face” done without the “‘logic of domination’—namely, the historic interest of modern science in prediction and control” (p. 10). If you want to know what feminist philosophers of science *promise*, this is your book. If you wish to know anything that will aid in measuring how well feminists *succeed* in making good on these promises, look elsewhere. Despite the fact that the papers here are contributed by principal figures in feminist philosophy of science, this book is not one for the philosopher.<sup>4</sup>

### *Nelson and Nelson*

Though not my favourite, this is a really good book. The authors are ‘philosophers’ philosophers’, and the book is a battery of sophisticated efforts to confront the central, philosophical questions which pertain to a feminist philosophy of science. The Nelsons have designed this volume to measure how feminist philosophy fares on issues such as cognitive and non-cognitive values, underdetermination, relativism, realism, and objectivity. But we also find a few papers addressing less well-motivated issues, such as “Views from Multicultural Global Feminisms, and from Feminist Phenomenology”. For the most part, the papers are challenging and serious efforts to deal with the important issues. The introduction is a worthy beginning.

The Nelsons begin by saying that the essays are located at the “intersection” of the “rapidly-evolving areas of inquiry, ‘mainstream’ philosophy of science and feminist philosophy of science” (p. ix); that feminist science is “marked by significant development in the last two decades” (p. x), and that feminist-based analysis has “been evolving apace” (p. xi). These descriptions raised my expectations. In the end, however, I was struck more by how much the individual authors *claimed* than by how much progress they made in justifying those claims.

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The Nelsons' own comments on specific papers to some extent bear out this criticism. For instance, on Elizabeth Potter's paper, "Underdetermination Undeterred", they say that she addresses the "disagreement" between those philosophers of science who cling to "rationalist approaches" and those "feminist science scholars" who would reject the "thesis that all science that is influenced by political factors and/or nonconstitutive values is ipso facto bad science" (p. xiv). But this is a phoney contrast, which the editors should have acknowledged. To my mind, Laudan is Potter's best case, and she does conclude that Laudan "failed to show that the underdetermination thesis is false" (p. 135). But Laudan is not concerned to *falsify* the thesis. He does not worry about the truth or falsity of the thesis. Instead, he shows that it fails to support the epistemological and methodological conclusions drawn in its name. Indeed, if one could force him to speak here in terms of truth and falsity, he would probably prefer to call the thesis true, albeit completely impotent. Given the importance of the underdetermination thesis to the feminist critique of science, this collection would be seriously lacking were it to ignore the thesis. But we need a paper that meets the philosophical arguments, rather than one that goes wide of the mark.

Moving on, the Nelsons tell us that "Barad, Rouse, and Lloyd reject the claims that feminist scientists and science scholars advocate relativism, reject standards of rational inquiry, and/or are aptly described as 'anti-science' [and] Lloyd's analysis suggests that the charge of relativism made against the sociology of science is itself misplaced" (p. xvii). This is pretty arresting commentary, and I turned directly to Elisabeth Lloyd's contribution. She avoids any head on confrontation with charges of relativism, referring instead to Shapin and Schaffer's *Leviathan and the Air-Pump* (Chicago, 1985) and attempts to make a case that Shapin and Schaffer are improperly maligned for making relativist claims. But Lloyd does not direct her argument against relativism. She does not even make an effort to refute Shapin and Schaffer's view that "it is ourselves and not reality that is responsible for what we know" (Shapin and Schaffer, p. 344). Instead, her efforts are, so to say, a *non sequitur*: she asks readers to credit the sociologists for the 'corrective force' their book has brought about.

[I]n the wake of centuries of historical accounts of science in which the overpowering majesty of nature and the beauty and perfection of its rational order were portrayed as the engine driving scientific progress itself, recent historians have been developing more accurate and complete accounts . . . There is, thus, a corrective force to Shapin and Schaffer's book regarding the history of science itself (p. 232).

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Presumably, Lloyd credits Shapin and Schaffer's with having 'corrected' a mistaken tendency to portray the history of science, and science itself, as an account of the very world that science seeks to describe and explain, all the while overlooking the impact that scientists themselves have on these descriptions and explanations. My reaction is two-fold. First, Lloyd is surely mistaken in giving credit to Shapin and Schaffer, *circa* mid-1980s, for calling to task simplistic history of science. For the impact of political, sociological, economic, and so forth, factors on science had been signalled at least since the early 1960s. Secondly, the "more accurate and complete accounts" to which Lloyd refers are anything but non-controversial in regards to accuracy and completeness.<sup>5</sup> Lloyd's use of Shapin and Schaffer as good history of science is both out of date and, I think, suspect.

I now turn to Ron Giere's, "The Feminism Question in the Philosophy of Science". Here, Giere extends in intriguing fashion a critique of Logical Positivism to support the idea of feminist philosophy of science. His argument rests on the premise that current philosophy of science "derives mainly from European sources transmitted by refugees displaced by World War II" (p. 6), all of whom were German-speaking and bound together by a particular philosophical perspective on science, but "none of these philosophers occupied positions of great influence, whether intellectual or institutional, within the German speaking philosophical world" (p. 6). Giere provides good reason to ponder over the mechanisms that propelled the refugees into positions of influence in the U.S., and what role, if any, their previously marginalised stature might have played in their ultimate academic prominence. In this connection he claims, surprisingly, that the discovery-justification distinction, a doctrine central to empiricist philosophy of science, was not established by argument (p. 8). Rather it was result of "an assumption forming the conceptual context within which arguments were formulated. The only way to understand why those doctrines were held is to inquire into the historical origins of their role in that [the refugee] movement" (p. 9). Giere's essay has a place in this volume because he promises to link his ideas with others that support the 'feminist critique' of science, though I have not space to show how this is accomplished.

Susan Haack's paper "Science as Social?—Yes and No", is a hard-hitting attack on what she says is, the "wrong direction" taken by feminist philosophers of science. In her view, this wrong direction has fostered "a project that is neither sound epistemology nor sound feminism" (p. 79). Haack's concerns are worth emphasising:

For generations, talented girls were discouraged from science because of ill-founded ideas about women's (in)abilities. Now

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there is a danger that talented girls will be discouraged by ill-founded ideas about the masculine or masculinist values with which science is allegedly imbued . . . Not to mention—but I feel I must—the waste of talent and energy if women interested in the epistemology of science come to feel that they must restrict themselves to approaches certified as ‘feminist,’ or be guilty of complicity with sexism (p. 90).

Another excellent paper is Ilkka Niiniluoto’s “Relativism in Feminist Epistemology”. Niiniluoto takes on “[Sandra] Harding’s bold thesis that women’s experiences should be preferred both by men and women” (p. 48). The reader will follow him through a painstakingly sympathetic read of the full scope of Harding’s published work in support of this thesis. In the end, he concludes that “Harding’s thesis that all science should preferably be based on women’s experiences remains unwarranted” (p. 150). It is highly instructive to follow Niiniluoto through to this conclusion. Finally, I would say that while Kluwer is well-known for its publication of serious philosophy, and this book is no exception, the press is to be chided for the small-point type which is hard on the eyes.

### *A Real Gem*

*Women, Gender, and Science: New Directions* is superb—even if I do not always agree with all of it—well-deserving of the balance of this review. It is exactly the sort of inquiry into the nature of feminist philosophy of science that we need and I’m not sure that there is another quite so good. While the level of philosophical sophistication is not always the highest, the book is to be recommended as it is sensitive to the history of science and because it does very well what any feminist philosophy of science book should do: provide exposition of feminist philosophy of science and show the progress it has made towards achieving its aims.

I will start with Keller’s article “Developmental Biology as a Feminist Cause?”. She writes that “if one looks at the maternal effect mutants identified over the years, especially in the early years, one sees that more than half of them were identified by women” (p. 23). This is, or could be, an important point for the feminist critique, and its central claim. Keller’s paper gets moving on all the correct questions: were the observed effects ultimately significant to research and to the direction of future research? Was the ratio of women to men observers, at that time, significant? Are there further data, from subsequent study, which indicate that more women observers in this field point to comparatively better success than

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having fewer women? I cannot say that Keller provides answers, but she is homing in on the correct questions.

By contrast, Alison Wylie's paper, "The Engendering of Archaeology", is less helpful but instructive in its failure. She says "the puzzles that dominate Paleo-Indian research are quite literally created by the pre-occupation with male-associated (hunting) activities" (p. 84). Does she mean that a feminist archaeology would resolve the puzzles or that the puzzles are non-genuine? The puzzles must either be resolved by feminist archaeology, which it has not yet done; or, the puzzles must be shown to be non-puzzles. Wylie's conclusion that "critiques that bring into view the pervasive ways in which social and political factors shape inquiry... should be the beginning of a new kind of discussion, which feminists are especially well situated to carry forward" (p. 98). She has discussed numerous women and their work, but she has written not one word that would demonstrate her contention that feminists are "especially well situated to carry forward" a new kind of discussion. Nor has she said much concerning what such a discussion would be like. We would like to know!

The following three essays go together. The success of each in itself in advancing the claims of feminist philosophy of science is uneven, but taken together they make interesting use of history in the service of philosophy. Estelle Cohen's study, "What the Women at All Times Would Laugh At", is a remarkable piece based on the feminist writings of Francois Poullain de la Barre (1647–1723). (Pouillain, an important early champion of feminist causes—he precedes Mill by two hundred years—turns out to be something of a female supremacist, though this is not central to Cohen's argument.) Cohen tells us that:

Apart from defending women's equal (or superior) capacity to learn science, practice medicine, and fill public offices, Poullain was probably responsible for developing and circulating a number of arguments that were to become almost common currency in debates about sexual equality up to about 1780. Among these were the following: reading science requires less time and intelligence than doing needlework (p. 135).

The second paper is Elvira Scheich's, "Science, Politics, and Morality" about Lise Meitner (nuclear fission) and Elisabeth Schiemann (genetics). Here we read how both Meitner and Schiemann worked in pre-World War II Germany and were affected in their professional and personal lives by the war. Scheich develops a careful case-study of the marginalised Meitner and the insider Schiemann. Unfortunately, the study never makes

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clear just why Scheich juxtaposes Meitner and Schiemann: it seems that the two just happened to have met, become friendly, and later corresponded, but it is never clearly shown how this connection was epistemically significant. Even more disappointing, two huge questions remain unasked. Did Meitner fare any worse because she was a woman? (Perhaps Scheich thought the point too obvious to argue for?) Did those scientists (male or female) who left the German academic environment sooner fare better than did those who remained longer and until forced to leave? Finally, Scheich makes a sobering point that concerns U.S. complicity with Nazism. She writes that "The majority of German scientists continued their research under the Nazis as if nothing had changed. Biological and genetics research in Germany during the 1930s met international standards and was funded by the Rockefeller Foundation until 1942" (p. 158).

Margaret Rossiter's paper "Which Science? Which Women" is the best. She opens with the admonition "it is time to sort out what we do know and to set out some categories before we try to pull these case studies into some larger whole" and continues "the single most important indicator or predictor of a woman's experience in science is the proportion, though changing over time, of women in her field or subfield" (p. 169). However, as Rossiter is quick to note, "actual data for any period or country before the 1970s are hard to come by" (p. 169). Anyway, she describes a four point research program for the feminist critique of science. It seems to me that this is really along the right track and so I let Rossiter do the talking. Point one, concerning women rising through the professional hierarchy of science:

One important matter to consider is the hierarchical distribution of women within a field, as the proportion of women usually drops off at each higher level. . . the dynamics that lead to these differences ought to be studied more. Lack of advancement may be the result of ghettoization (pp. 179–180).

Point two, on strategy concerning choice of research topics:

If we had a taxonomy of fields and women's usual experience within them, we might be able to make better comparison. . . Thus if we had a way to tease out and evaluate the several factors involved and assess their relative importance in comparative cases, we might be better able to understand and explain the overlapping and interlocking processes, to identify what is the pattern (and maybe why) and what the exception or the oddity, if any. Then we might also be able to



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identify strategically important research topics—because they promise to expose some key factors (p. 183).

Point three, on the importance of explaining women's choice of research emphasis:

We know next to nothing about which women are in any of these fields. . . My lone impression of American women in microbiology, to suggest an example, is that a lot of them had fathers who were physicians (p. 184) .

Point four, concerning methodological advice on how to use the history of science as a profession:

Our continuing goal should be to break down or subdivide the aggregate entity called 'science' into its sub-specialities. . . then we may hope to integrate women more fully. . . The result may be a less ghettoized, richer, and more comprehensive history of science than we have had to date (p. 185).

Finally, Rossiter makes some provocative remarks concerning "occasional women leaders" in science. She remarks that "they would rarely be feminists" (p. 172). Her observation raises that nagging problem in feminist philosophy of science, namely the lack of criteria to identify *feminist* philosophers of science, or *feminist* scientists (as opposed to women philosophers of science, or women scientists, *simpliciter*), and the need to do so before the core claim made by feminist critics of science can ever be intelligibly investigated or argued.

### Conclusion

This review is predicated on both an unabashed bias toward historically-informed philosophical argument about science and a belief that philosophy of science is in reasonably good shape, so that any pretension to reform it needs to be motivated by a compelling argument that not only convincingly critiques the received theoretic, but also provides an alternative one. *Ad populum* appeals to "Mars vs. Venus science" or to the strictures of phallogocentric reasoning patterns are not on target. What is on point are answers to the question of how well feminists are making good (if they are) on their claim to do better philosophy of science, and better science. Feminist philosophers of science should present data that force the 'old guard' to take note. Astonishingly, most feminist philosophers of science show no interest in data collection or analysis. Others, notably

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Rossiter and those others in the Kohlstedt-Longino book, have information and argument to be reckoned with. That, at least, is a good start.

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### Notes

1. Thanks to my colleagues, J. Garrett and L. Mayhew, for earlier discussions concerning this area of research.
2. However, too often, when in critique mode, feminist philosophy of science takes aim at theories of science which no philosopher holds. This is especially the case with respect to Logical Positivism. In the feminist critique, it appears, falsely, to be alive and well. Also, although history of science these days plays a central role in theories of science, feminists appear to regard their own use of history as a unique reason to value feminist philosophy of science.
3. I also read Jan Duran, *Philosophies of Science/Feminist Theories* (Boulder, 1998). But this is much more a general introduction to philosophy of science than a contribution to feminist philosophy of science. It is not without merit. Duran's Chapter 6, "The Advent of Feminist Theory", is a good overview and she introduces Harding, Fox Keller, Haraway, Longino, Nelson, Hubbard, Gilligan. As a text, however, it is short on critical balance. For instance, when discussing Longino's idea that "the production of Knowledge is crucially determined by the gatekeeping of peer review" (p. 109), Duran should have taken notice of Stephen Cole's *Making Science* (Cambridge, Mass, 1992). Given this sort of omission, Duran's text would be a useful instructional tool only when heavily supplemented by outside resources.
4. The book could work as a Women's Studies text. But better is Daphne Patai and Noretta Koertge, *Professing Feminism: Cautionary Tales from the Strange World of Women's Studies* (New York, 1994).
5. I have discussed contributions by philosophers of science on the need for *history* of science in philosophy of science in Pinnick and Gale, "Philosophy of Science and History of Science: A Troubling Interaction", *Journal for General Philosophy of Science*, forthcoming). For critical appraisals of recent sociological histories of science, Pinnick "What's Wrong with the Strong Programme's Case Study of the 'Hobbes-Boyle 'Dispute?'" in N. Koertge (ed.), *A House Built on Sand: Exposing Postmodernist Myths about Science* (Oxford, 1998): and Gale and Pinnick, "Stalking Theoretical Physicists: An Ethnography Flounders", *Social Studies of Science* 27 (1997).

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