

Taking Gender Seriously in Philosophy of Science

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The invitation to participate in this symposium was accompanied by a proposal indicating that the symposium topic was "Can we do philosophy of science without taking into account the gender, race, and class of scientists?" My own views on the relation of gender, race, class, and science contain an answer to this question, but in the main they run slightly askant of it. Since my previous work bringing together philosophy of science and questions of gender has consisted in using philosophy to illuminate the role gender and associated ideologies play in certain selected research programs, I took the question as a challenge to articulate a closer relation between gender questions and the philosophy of science or—better—between gender questions and my own approach to the philosophy of science.

Let me make two preliminary remarks. First of all, I think of gender, race and class as features of social structure first and as characteristics of individuals only secondarily. That is, individuals in a given context are of a gender, race, or class because those are significant elements in that context, and not vice versa. So, race, class, and gender as structural features of scientific communities are of at least as great an interest as the race, class, and gender of scientists. Because the claims of feminist and other critics of science are often interpreted as claims about the individual members of scientific communities, it is worth, I think, emphasizing the point.

Secondly, there is another element of this brew as relevant as these social markers and that is political-intellectual orientation to them. Back in the heady days of late sixties and early seventies radical politics, I and my fellow middle class members of left-wing political organizations used to emphasize the distinction between class position and class stand. We could not escape our middle-class origins, nor were we ready, nor did it seem appropriate, to abandon our then prospective or fledgling middle class professions. The concept of class stand, however, enabled us to see that we could argue for and take actions which either benefitted others at the expense of our socio-economic class or whose ultimate aim was the abolition of the class distinctions in which we were enmeshed and by which we had been shaped. Nothing is ever so simple, and in retrospect, there was probably a certain amount of self-delusion abroad amongst us. But the distinction is important and one often lost sight of in discussions of the role of gender and race, and of the role of majority women and minority men and women in the sciences. While gender and race have a lot to do with the experi-

ence of aspiring and actual scientists, while they have much to do with the content of the sciences, and while who does science matters, these do not collapse into the same issue. In one of the areas of research that has drawn the sharpest feminist criticism—behavioral neuroendocrinology—many of the researchers are women. Gender as a property of individuals has almost no explanatory role here, although gender as a social gradient of power has a strong explanatory role. The question I want to raise, then, in addition to the question posed to the symposiasts, is the following: What would a philosophy of science that is sensitive to gender, race, and class as features of social structure look like—what would its questions be? what would make it philosophy rather than sociology or anthropology or history?

To the first question, then: can we do philosophy of science without taking into account the gender, race, and class of scientists? I've argued for a view I call contextual empiricism (Longino 1990). While experience (experiment, observation) constitutes the least defeasible legitimator of knowledge claims in the sciences, the evidential relevance of particular elements of experience to hypotheses is mediated by background assumptions operating at many levels. What controls the role of background assumptions is interaction among scientists, interaction consisting in criticism of assumptions involved in observation, of assumptions involved in reasoning, of assumptions involved in thinking a given hypothesis plausible, of assumptions involved in the application of particular methods to the solution of particular problems. To be successful in uncovering such assumptions, criticism must proceed from a variety of points of view, ideally as many as are available.

This account, I maintain, has at least two consequences. 1) It allows us to see that the same process accounts for both the suppression and the expression of social values, interests and ideology in the sciences. Idiosyncratic values are suppressed, while values held by all members are invisible (as values, interests, or ideology). These are, therefore, not available for control by discursive interactions. 2) It identifies the producer of knowledge, the knower, as the community rather than the individual scientist. This means that certain features of community structure are important to the knowledge productive capacity of a community. I've discussed four such features. There must be

- a) avenues for the expression and diffusion of criticism;
- b) uptake of, or response to, criticism;
- c) public standards by reference to which theories, etc. are assessed.
- d) equality of intellectual authority.

This fourth feature provides the answer to the symposium question. The degree to which all four features are exemplified in a given scientific community is a measure of its objectivity, or to use another, less burdened term, its epistemic reliability. Members of a community will assess a theory or hypothesis in relation to the standards operative in that community. Outsiders will assess a theory or hypothesis advanced and supported by a given community in relation to its and their own standards and in reference to the exemplification of the four components of knowledge productive capacity. (Since individuals are members of different communities, any given individual can, of course, act as both insider and outsider with respect to any given community; these boundaries are porous even though policed in various ways; and the distinction between insider and outsider is as much a matter of rhetorical positioning as of training and successful apprenticeship.) Given all this, it follows from the fourth component that gender matters, as do race and class. That a scientific community consists entirely or primarily of

members of one or the other sex, race, or social class, is *prima facie* evidence of its failure to extend equality of intellectual authority to all potentially qualified members. So, gender, race and class structure are features of a scientific community that must be taken into account when assessing its epistemic reliability.

Perhaps an acceptance of this condition accounts for the attempts to naturalize alleged cognitive inferiorities of Euro-American women, dark-skinned people in Africa, Asia, and the Americas, and members of the working class. For, could it be shown that such individuals were constitutionally less capable, or incapable, of producing knowledge, their absence from knowledge productive communities might not constitute a violation of condition four. This is why the demonstrations that the research attempting to show the biological basis of alleged cognitive deficits fails to meet standards of empirical adequacy are philosophically interesting. They reveal a community simultaneously restricting participation and legitimating its exclusionary practices in such a way as to disarm in advance criticism of that rationale.

What can we say about the epistemic reliability in general of such a community? That is, what can we say not just about the reliability of research to disqualify participation by certain groups, but about all the work done by a community self-constructed in this way? Such questioning lies behind the rejection of science and of rationality by many feminists. Seen in this way this rejection appears not as simple luddism, but as an understandable response to a self-undermining project. Those of us who take a different course, who do not reject science out of hand, must insist on a distinction between scientific inquiry as a human project and its pursuit by historically and geographically situated communities. But it is a philosophical problem to articulate what science might be in a way that avoids both collapsing it into its inflected instantiation in a particular community and fleeing toward the discredited transcendentalism and universalism of one of those communities. It is a scientific project to discredit the naturalizing research, and it is a historical, sociological and anthropological project to reveal the gendered, class and racial structure of given scientific communities. The associated philosophical projects (no matter who does them) include both 1) thinking about the epistemological legitimacy of a systematically exclusionary scientific community and 2) articulating a conception of scientific inquiry and of knowledge that can survive those investigations and that can warrant the allegiance of those who have hitherto been excluded from or marginalized in its pursuit.

There is other work for gender and race sensitive philosophy of science. I'm going here to focus on gender sensitivity and on ideas related to my third criterion. That condition requires public standards by reference to which hypotheses, data, assumptions, and practices are assessed. Here, gender, race, and class recede and political/intellectual orientation to their roles in structuring society becomes more salient. In claiming that public standards are required for a knowledge productive community, I am not claiming that there is a single set of standards that characterizes all scientific communities. I'm claiming instead that there is a pool of standards—cognitive, substantive, and practical—that communities draw on in regulating themselves. Criticism and endorsement, as well as the proffering of alternate explanatory models, are made germane to a given community by appeal to some one or more of the standards it recognizes. Different, but overlapping, sets from this pool characterize different communities. One project, therefore, is the identification and articulation of those standards or values that might characterize a gender sensitive or feminist knowledge productive community.

Under sway of an ideal of unified science, this might seem like a universalist or absolutist undertaking, i.e. an attempt to characterize in some absolute way the sci-

ences that will replace contemporary sciences. Not so. At least, not necessarily so. One activity of philosophers of science is to study the cognitive goals of given scientific communities, to ask what is required for their attainment, to ask what goals are implied by the activities of those communities, to ask about the character and attainability of those goals. In the work of feminist scientists and philosophers, historians and critical sociologists and anthropologists of science we can discern a number of values endorsed as appropriate to such communities, as suited to the attainment of the goals of a knowledge productive community understood as feminist. The role of the philosopher is not to prescribe values and practices from the vantage point of nowhere, but to inquire into the conceptual relations among the various values put forward, their grounds and coherence. In so doing the philosopher becomes a participant in a community dialogue from which will emerge sets of standards by reference to which feminist scientists will engage in the discursive interactions constitutive of a knowledge productive community. Such a community is to be evaluated as much as any other by reference to the standards of epistemic reliability. Its standards, as much as its hypotheses, theories, assumptions, data, and methods, require criticism from a variety of points of view. They must then be understood not as guarantors of truth but as provisional criteria of adequacy for the community which has endorsed them.

What might these standards be? At least six standards or virtues have been proposed, explicitly or implicitly, in the literature on feminism and science. They are either used to validate or criticize hypotheses and models or proposed as desirable new standards to replace or supplement mainstream standards. Some are shared with non-feminist communities, others are not. The list of six I propose to discuss should be understood as a sample, rather than as a definitive set. Nevertheless, I believe it has features that would characterize any such list. In particular, I think any list will contain, as this one does, formal, substantive, and social or practical elements. The virtues and their sources are as follows.

1) *Empirical Adequacy*. A good deal of feminist effort has, as I indicated above, gone into discrediting research programs that purport to show a biological etiology for differences ascribed on the basis of sex. The (feminist) scientists involved in this effort—Ruth Bleier, Anne Fausto Sterling, Richard Lewontin, Ruth Doell—have concentrated on showing that such research fails minimal standards of empirical adequacy, either through faulty research design or improper statistical methodology. The standard of empirical adequacy is one shared with race and class sensitive research communities as well as with most mainstream communities. Empirical adequacy is not a sufficient criterion of theory and hypothesis choice. So, other values come into play in theory, hypothesis, and model assessment.

2) *Novelty*. Several thinkers have endorsed the novelty of a model or theory as a value. Sandra Harding seems to have done so in her earlier book (1986), when she calls both for “successor science” and for “deconstructing the assumptions upon which are grounded anything that resembles the science we know.” Donna Haraway’s (1989, 1992) invocations of the visions of certain science fiction writers can also be seen as an appeal for or endorsement of a departure from familiar views, for the sake of a new framework (or new frameworks). Nothing less, she suggests, will be appropriate for the new circumstances of 21st century life. Treating novelty as a virtue reflects a doubt that mainstream theoretical frameworks are adequate to the problems confronting us, as well as a suspicion of frameworks developed in the exclusionary context of modern European and American science. It may be that this criterion is appropriate only so long as feminism has oppositional status. I’m not sure about this, partly because I’m not sure that feminism has any status apart from an oppositional one.

3) *Ontological heterogeneity*. This criterion is drawn from two quite different sorts of discussion in the feminist literature on the sciences. Feminists writing about biology have urged that we take account of individual difference among the individuals and samples that constitute the objects of study. Barbara McClintock's attention to the individual kernels of a cob of corn (which helped her to recognize an underlying pattern of mutability) has been taken as a paradigm of what a feminist attitude to nature ought to be (Bleier 1984). Primatologist Jeanne Altmann has insisted on methods of observation that descriptively preserve the differences among the primates and groups of primates that she studies (Altmann 1974, Haraway 1989). Other feminists in science as well have rejected ontological homogeneity and have taken heterogeneity as a value. I think this is connected to the second discussion I draw on here: the rejection of theories of inferiority. Theories of inferiority are supported in part by an intolerance of heterogeneity. Difference must be ordered, one type chosen as the standard, and all others seen as failed or incomplete versions. Theories of inferiority which take the white middle class male (or the free male citizen) as the standard grant ontological priority to that type. Difference is then treated as a departure from, a failure to fully meet, the standard, rather than simply difference. Ontological heterogeneity permits equal standing for different types, and mandates investigation of the details of such difference. Difference is resource, not failure. Nowhere is this more dramatically endorsed than in Donna Haraway's intrepid embrace of artificialism and of science fiction, which she lauds for their diffusive possibilities, their rejection of purity, or ontological homogeneity, and their insistence on the specific and the local in all their heterogeneity.

4) *Complexity of Relationship*. Many feminist scientists have taken complex interaction as a fundamental principle of explanation. Evelyn Keller's (1983) account of the work of Barbara McClintock and her (1985) defense of an interactionist perspective in *Reflections on Gender and Science* may provide the best known example, but scientists from icons like Ruth Bleier and Anne Fausto Sterling to much less well known practitioners have eschewed single factor causal models for models that incorporate dynamic interaction, models in which no factor can be described as dominant or controlling and that describe processes in which all active factors influence the others. This perspective has been employed in areas ranging from neuroscience to cell biochemistry by scientists self-consciously practicing science as feminists.

5) *Applicability to Current Human Needs*. Many, but not all feminists in the sciences have stressed the potential role of scientific understanding in improving the material conditions of human life, or alleviating some of its misery. (Rosser 1987) Scientific inquiry directed at reducing hunger (by improving techniques of sustainable agriculture, soil preservation, etc.), promoting health, assisting the infirm, protecting or reversing the destruction of the environment, is valued over knowledge pursued either for political domination, i.e., science for "defense", or for knowledge's sake. As expressed in feminist contexts, this is not just a call for more applied science, but for research that can be directed towards meeting the human and social needs traditionally ministered to by women. This virtue is endorsed in conjunction with the final one I will mention.

6) *Diffusion of Power*. This criterion is the practical version of the fourth criterion, the one favoring models that incorporate interactive rather than dominant-subordinate relationships in explanatory models. This one gives preference to research programs that do not require arcane expertise, expensive equipment, or that otherwise limit access to utilization and participation. This feature has emerged as a value in a number of different contexts. Feminists in engineering and economics have condemned the requirement of mathematical achievement far beyond what is required for successfully engaging in these fields. Other feminists, such as Hilary Rose (1983)

and Ruth Ginzberg, have urged a revamping of traditional distinctions to include widely distributed practices such as midwifery as scientific practices. They urge that such practices be used as models for feminist science practice. Feminist health professionals urge a preference for medical practices and procedures that empower the individual woman either to make decisions about her health or to retain control over her own body. And ecofeminists and feminists in developing regions urge the development of technologies that are accessible and locally implementable. Some implementations of computer technology are valued for their ability to connect different but highly specific sites in widely spread, potentially global communication networks. Other implementations, for example, the centralization of power made possible by computer monitoring of job performance and other functions are more problematic from the perspective of this standard. Diffusion or decentralization of power interprets the above cited elements of the applicability criterion as knowledge of soil conservation, intensive small scale sustainable agriculture, promoting health by preventive measures such as improved hygiene rather than high-tech interventive measures available only to the few, protection of the environment by conservation and widely dispersed renewable energy technologies.

As I indicated above, this list is only a sample, even a ragbag. It is enough to indicate where further work lies. One philosophical task consists in more of the same, i.e. reviewing the literature (and conversation) on feminism and science and on gender and science for other standards or values both explicit and implicit. A further task involves thinking about the interrelation of the standards discovered: Do they require further interpretation? Are some components of others? Is this provisional distinction into formal, substantive, and practical useful or obfuscatory? Are there more than one set? If so, what are the relations between them? Do the values and standards proposed do the job they are required to do, are they sufficient for the accomplishment of recognized feminist goals with respect to the sciences? Are there additional constraints on scientific practice that bear articulation? How do they get exemplified in particular research programs? How do they get implemented in the laboratory? In the discursive interactions among scientists thinking of themselves as feminists? What relation do they bear to virtues, goals, and standards advanced in other oppositional scientific communities? Each of these questions can generate significant research that not only makes philosophical sense of the notions of feminist and/or oppositional science, but that also deepens our understanding of mainstream science. Such research will also help to give content to the pluralist conception of scientific inquiry.

My own view is that whatever lists of standards are drawn up will be subject to the same sorts of limitations articulated by Thomas Kuhn (1977) for the values he claimed to be involved in theory choice, i.e. requiring further interpretation to be applied in a given research context, not simultaneously satisfiable, not subject to hierarchical ordering or algorithmic application. But these points remain to be demonstrated about the alternative list (or its cognates). All this requires cooperation between feminist philosophers, or philosophically-minded feminists, and feminist scientists. I think, however, that we are at the stage when we can just proceed with the project, rather than having to defend it. Indeed some, including Alison Wylie, the chair of this session, are doing just that.

This approach does not require ideological purity or (in spite of the criterion of novelty) the invention from whole cloth of a new science. If we take anything from Haraway's work it ought to be the fragility of boundaries, particularly of the distinctions we use to define ourselves. Instead of demanding or pretending reinvention, we should acknowledge our relation and indebtedness to that from which we differentiate ourselves. The articulation of these criteria of adequacy facilitates not so much a new

science, but opportunistic appropriation, selection and recombination from existing research programs, as well as the introduction of new values and standards to the study of a given bit of nature.

You might well say—what's specifically feminist or gendered about these standards? Empirical adequacy, as observed above, is a staple of most philosophers of science, even if we wouldn't all gloss it in the same way. The advocacy of ontological heterogeneity is a staple of many Marxists; the advocacy of models of genuine interaction a theme of radical environmentalists and ecologists, and so on. This question belongs to a species of question sometimes asked with the subtextual intention of showing the irrelevance of gender or of feminism to science. But I shall take it charitably, as a genuine puzzlement, a puzzlement that I think can be removed by thinking not about the content of the standards, but about their grounds. I do not have the space to discuss the grounds of each of these standards, and will limit myself to the following brief remarks.

One of the interesting features of the particular standards I have articulated is that it is possible to offer various grounds in their support. All have some social theoretical grounds, but also either cognitive, aesthetic, or practical grounds. Take, for example, the criterion of ontological heterogeneity. It has epistemic grounds: a community characterized by diversity is more epistemically reliable. It also has social grounds: explanatory models that preserve ontological heterogeneity may naturalize heterogeneity in the social world, just as models that feature ontological homogeneity naturalize social homogeneity. This means that the standards themselves can't be dichotomized into cognitive or social. Secondly, one of the effects they all have in one way or another is to prevent gender from being disappeared. The disappearing of gender is the erasure from inquiry of a gradient of power that keeps women in a position of subordination. Whatever other grounds can be offered for them, their role in making gender a relevant axis of investigation gives them their status as feminist.

One consequence of identifying those values articulated or implied in feminist contexts and reflecting on their grounds is that we can then turn back to the values traditionally cited as examples of cognitive values and make comparable inquiries into their grounds. This might be an especially interesting exercise for values in apparent opposition to these. We might inquire whether the grounds for endorsing simplicity, for example are parallel to those for endorsing ontological heterogeneity or complexity of interaction. Such inquiry might reveal simplicity to be invested with the social at least as much as with the cognitive.

I've argued (1990) that the ideological dimensions of theories of human evolution or of the role of gonadal hormones in behavior, and in general of any theory accepted, are best revealed through comparison with alternatives. Just so, the ideological dimensions of mainstream standards of theory appraisal may be revealed by comparison with an alternative set.

All of this is programmatic and may well look very different after some sustained investigation. I'm also conscious that an important class of feminist critiques, those that focus on affective dimensions of the practice of science, has not been considered here. But I have shown that there is plenty of work for gender-sensitive philosophy of science. The questions I have listed all seem to me to require standard philosophical activity, especially given the cross-disciplinarity of present philosophy of science, and our interest in criteria and distinctions that map onto scientific practice. What's new is the content introduced by gender-sensitivity. This is only to be expected as philosophy becomes a human rather than parochial endeavor.

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