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What Feminist Science Studies Can Offer

If philosophers of science could ignore the wider social context of science for much of the twentieth century, *feminist* philosophers of science could not. That wider social context, after all, was the site of inequality for women—inequality in jobs, inequality in wages, inequality in expectations and treatment both in the home and outside it—and science, feminists were discovering, was helping to perpetuate that inequality. Never mind the history of misogyny in the research output of such fields as psychology and biology. Never mind the history of women's neglect in the research output of other fields such as economics and medical research. Never mind the appalling lack of opportunities for women or downright exclusion of women as practitioners in the histories of all the sciences. The jarring fact was that it was all continuing even at the end of the twentieth century. Rather than helping the cause of equality—by replacing prevailing ignorance and prejudice and misinformation about women with more adequate perspectives—science was doing just the opposite. If combating androcentrism and sexism in society was the first order of business for feminists, combating androcentrism and sexism in science was surely the first order of business for those feminists who were philosophers of science. But how?

The task demanded interdisciplinary collaboration and received it. Feminist scientists and historians of science exposed sexism and androcentrism in such fields as anthropology, sociology, political science, medical research, psychology, biology, and archaeology, and they exposed, as well, the obstacles female scientists faced in those and other fields. And feminist philosophers of science, along with feminist scientists and historians, investigated the actions that needed to be taken in response. What resulted was not only a rich array of resources for dealing with sexism and androcentrism in science—and, by extension, racism and heterosexism and classism and the like—but also an important set of beginnings for generating a contextualized (and even, as we shall see, a politicized) philosophy of science.

THE METHODOLOGICAL APPROACH TO SEXISM IN SCIENCE

Doubtless the earliest mode of response to androcentrism and sexism in science came from feminist scientists. Many of these scientists pointed out that a great deal of androcentric and sexist science was, by the lights of traditional scientific methodology, simply bad science—science that failed to satisfy accepted standards of concept formation or experimental design or data analysis or the like (see, e.g., Hubbard 1979; Bleier 1984; Fausto-Sterling 1985). If only such standards were rigorously followed, it was suggested, the problems of sexism and androcentrism in science would be, at the very least, much reduced.

The whole issue of premenstrual syndrome (PMS) was a case in point. By the 1980s, PMS had become a biomedical problem of significant proportions. Estimated by some to affect as many as 80, 90, or even 100 percent of all women, it had by then been linked to impaired concentration, impaired physical coordination, impaired judgment, decreased efficiency, and lower performance in school and at work. PMS had even been linked to deviant behavior and diminished moral and legal responsibility. In two sensational murder trials in the United Kingdom, for example, the courts found PMS to justify reduced sentences on the grounds of the defendants' impaired capacity to control their behavior. The conclusion drawn from all this, of course, was that women, due to their reproductive biology, were less capable than men of holding positions of power and responsibility in society (Rittenhouse 1991; Easteal 1991; Chrisler and Caplan 2002).

Yet the science that supported this conclusion was deeply flawed. To begin with, there was no agreed-upon definition of PMS. Instead, PMS was variously linked to a very large number of "symptoms"—headaches, epilepsy, depression, dizziness, hoarseness, asthma, nausea, constipation, bloating, increased or decreased appetite, low blood sugar, joint and muscle pains, heart palpitations, glaucoma, skin disorders, breast tenderness, and a host of others (100 to 150 in all)—and none of these nor any combination of them was taken to be a necessary or definitive diagnostic sign of PMS. Moreover, most of these conditions were ones that everyone experienced from time to time, conditions that did not necessarily indicate any disease state at all, much less the specific disease state that PMS was supposed to be. Nor was there agreement regarding how severe these symptoms had to be in order to count as PMS. As a result, a woman suffering from a few low days and a woman suffering from suicidal depression might both be diagnosed as having PMS. And there was no agreement, as well, regarding the necessary timing of the symptoms. Thus, some studies of PMS looked only at the day or two preceding menstruation, while others looked at the week preceding, and still others looked at the two weeks preceding (i.e., the entire luteal phase, from ovulation until the start of menstruation). Some studies included some or all of the menstrual

phase as well. And there were other problems. For example, most researchers were unable to ascertain which phase of the menstrual cycle their subjects were actually in at the time when symptoms were being monitored, since, among other reasons, menstrual cycles are not all of the same length.[1] And when researchers eliminated as subjects women with infrequent or variable cycles, choosing instead to focus only on those women with ideal, regular, 28-day menstrual cycles, they ended up gathering data on a very skewed segment of the overall population. The frequent absence of (male or anovulatory, amenorrheic, premenarcheal, or postmenopausal female) control groups was also a problem, since without such controls, it was impossible to determine if reported symptoms were a function of the menstrual cycle rather than other factors. The result of all of these problems and others besides was that no conclusion regarding women's lesser capabilities could be validly drawn from this research. Not only were the individual studies flawed, but there was also little comparability among them to help compensate for those flaws (Parlee 1973; Abplanalp 1983; Fausto-Sterling 1985; Chrisler and Caplan 2002).

Equally serious methodological problems beset the sexist and androcentric research described in chapter 1. It was there pointed out, for example, that until the 1990s, diseases such as heart disease that affect both sexes were defined as male diseases, studied primarily in white, middle-aged, middle-class males, and clinically handled accordingly. As a result, heart disease in women (who, as it turns out, differ from men in symptoms, patterns of disease development, and reactions to treatment) was often not detected and not properly managed when it was detected. Such problems could be—and ultimately were—handled simply by following accepted methodological procedures, such as designing clinical studies with groups of subjects that were more nearly representative of the patient population at large (see, e.g., Rosser 1994 and "Women's Health Research" 1995). Similarly, at least many of the problems relating to PMS could have been handled simply by following such accepted methodological procedures as designing appropriate controls and measurement procedures (e.g., screening subjects for other medical or psychiatric disorders that may influence their PMS symptom profile). One survey (Chrisler and Caplan 2002) suggests that this is still not being done.

For many feminist scientists of the past, then, rigorously applying accepted standards of good science was the way to eliminate at least much

1. The most commonly used method to determine cycle phase—the calendar method—was also the least reliable. Researchers would count ahead from the date of a subject's last menstrual period to the date that they would estimate would mark the subject's next premenstrual phase. This method assumed not only that a subject's menstrual cycles would all be of the same length (despite fluctuations in life stresses etc.) but also that a subject would have a reliable memory of the date of her last period. The most precise method, on the other hand—hormonal assay—was also the least commonly used due to its expense (because behavioral-science researchers lack training in biochemistry, they have to hire others to do the assays) (Chrisler and Caplan 2002).

of the sexism and androcentrism in science. For some of the feminist scientists, however, those accepted standards needed quite a bit of refurbishing in order to meet the goal. Feminist sociologist Margrit Eichler (1988), for example, proposed a set of "guidelines for nonsexist research" to supplement what science students and researchers were already expected to live up to. Her guidelines covered all aspects of the research process, from the title right down to the policy recommendations that might follow from the research results, and they included such items as the following regarding research concepts:

- In order to determine whether a concept is androcentric, that is, has a referent restricted to males even though it presents itself as generally applicable to both sexes (e.g., "the suburb as a bedroom community," which conceptualizes the entire population of men, women, and children of the suburbs in terms of those individuals, originally primarily men, who leave in the morning to go to their paid work), "ask yourself the following questions: To whom or what does the concept appear to refer (who is the theoretical referent)? To whom or what does the concept *empirically* refer? Does it seem to refer to both sexes but empirically refer to one sex only? . . . If your answers to these questions indicate a mismatch between theoretical and empirical referents, the two must be made to match, either by changing the concept and making it sex-specific or by changing the content and making it applicable to both sexes" (Eichler 1988, 139–140 and see also 26).
- In order to determine whether a concept is "premised on a double standard," that is, "is based on an attribute that is potentially present in both sexes but is treated differently on the basis of sex" (as "head of household" or "head of family" is when used to refer to an adult married male, while an adult married female is referred to simply as "spouse," the attribute, here, being "is married"), "ask yourself the following question: Is the concept premised on an attribute that is present in both sexes but is operationally defined in such a manner that it will categorize females and males differently? . . . If you answered 'yes' to this question, you need to create a concept that categorizes females and males equally if they display equal attributes" (141–142 and see also 89–90).
- In order to determine whether a concept is "asymmetrical," that is, "describes a situation, trait, or behavior that is theoretically present in both sexes but is linked to one sex only" (as "unwed mother" is when not linked with "unwed father" or "maternal deprivation" is when not linked with "paternal deprivation"), "ask yourself the following question: Does the concept refer, in a sex-linked manner, to a situation, trait, or behavior that exists for both sexes? . . . If you answered 'yes' to this question, change the concept so that it expresses *human* attributes in sex-nonspecific terms" (142).

In all, Eichler's guidelines distinguished, illustrated, and offered recipes for eliminating seven different types of sexism in their most frequently occurring forms. Still, the guidelines made no pretense to being comprehensive. Indeed, Eichler's method for generating them was quite unsystematic: "My method was simple: I went into a library and picked up whatever recent issue of journals from different disciplines was lying on top in the journal pigeon holes. I assumed that it would make little difference which journal or issue I picked, and that I would find at least one example of sexism in every single one. Sadly, this turned out to be correct" (10).[2] In addition, Eichler's method relied on her perhaps limited knowledge of at least some of the scientific fields she covered, and as she herself pointed out, "the more trenchant critiques (and, one hopes, nonsexist alternatives) will have to emerge from within the various disciplinary groups" (132). Nevertheless, Eichler's guidelines were an important beginning.

Meanwhile, practitioners within the various disciplinary groups *were* offering their own more trenchant critiques and nonsexist alternatives. Feminist psychologist Carolyn Sherif, for example, suggested that "dominant beliefs about the proper way to pursue knowledge have made psychological research peculiarly prone to bias in its conception, execution and interpretation" (Sherif 1979, 63). These dominant beliefs included the view that psychology, in order to be truly scientific, should model its methods, concepts, and goals on those of the physical sciences—should, for example, emphasize physiological or biochemical variables and variables defined by performance on psychological tests or manipulation of circumstances in research situations. If it was to avoid the problem of bias, however, what psychology needed to do instead, urged Sherif, was to take account of such factors as the background, personal history, and gender of subjects and experimenters. It also needed to pursue research outside the laboratory or in naturalistic settings. In the past, for example, psychologists frequently reported that women were more suggestible or influenceable than men. But when psychologists started taking into account such factors as the gender of the researcher, the experimental context, and the interests and self-definition of subjects, their research results changed. They then found that women were more suggestible with a male researcher when the topic was socially defined as one of male interest, but also that men were more suggestible with a female researcher when the topic was socially defined as one of female interest. In short, failing to consider such factors as whether a topic was of concern to a subject and the gender of the researcher in relation to the topic had the effect of producing biased, spurious results of male/female difference and female inferiority within psychological research.

2. "The ease with which examples can be drawn from academic journals demonstrates the pervasiveness of sexism" (Eichler 1988, 9).

For their part, feminist biologists such as Ruth Bleier (1984), Evelyn Fox Keller (1983 and 1985), and Sue Rosser (1990) (see Longino 1994, 1995, 1997, and 2008) recommended that such traditionally central scientific values as consistency with established knowledge, simplicity, and explanatory unification be replaced by such alternative values as novelty, ontological heterogeneity, and complexity of relationship. The reason was that novelty, unlike the traditional consistency with established knowledge, would help to free science from a sexist and androcentric past. Ontological heterogeneity, unlike the traditional simplicity and explanatory unification, would help to make women's dissimilarities from men visible as signs not of women's inferiority to men, where men are taken as the sole norm or standard of comparison, but rather as signs of women's *differences* from men, where women and men have equal standing. And complexity of relationship, again unlike the traditional simplicity and explanatory unification, would help to steer theorizing away from simple dominant-subordinate conceptions of nature that naturalize social domination (as in older active-sperm-fertilizes-passive-egg models of fertilization) toward more complex interactive conceptions of nature (as in newer understandings of the interactive contributions to fertilization of both egg and sperm). Other feminist scientists called for still further discipline-specific as well as general methodological reforms for science.

THE IDEAL OF VALUE-FREE SCIENCE

A possible rationale for all of these reforms was provided by the ideal of value-free science—a possible rationale with impressive credentials. The ideal of value-free science, after all, was as old as modern science itself, and in its long and distinguished career it had garnered support from such varied sources as the seventeenth-century idea that nature is merely matter in motion, devoid of qualities such as good and evil; the eighteenth-century idea that science deals with facts and that facts are distinct from values; the nineteenth-century idea that the sciences should be impartial resources for the solution of social problems; and the twentieth-century idea that the establishment of scientific truths is a purely epistemic affair (see, for more details of this history, Proctor 1991). And the ideal of value-free science, in turn, supported just the right conclusion. According to value-free science, scientific investigations had to be kept strictly free of ethical or political commitments. Since sexism and androcentrism—whether in scientific concepts or data or methods or assumptions or hypotheses—embodied such commitments, they simply did not belong in science. Indeed, they biased science and thereby jeopardized science as a source of objective knowledge. Hence one of the tasks of an adequate scientific methodology properly applied was to screen out sexism and androcentrism (as well as racism and heterosexism), and when a methodology failed to do that, a reform of that methodology was called for. And this was

precisely what feminist scientists pressing for more rigorous application of acceptable methodologies as well as methodological reforms were doing.

Of course, by the end of the twentieth century, the ideal of value-free science had fallen into disrepute. Mainstream historical scholarship had suggested that the work of even the greatest scientists—even scientists like Boyle and Darwin and Freud and even, perhaps, the great Newton and Einstein themselves—had been shaped by social values (see, e.g., Bernal 1971; Elkana 1982; Shapin and Schaffer 1985; Gilman 1993; Ruse 1999). If our conception of science, including our conception of objective science, was to be true to actual science, it could hardly ignore such science as this. Mainstream sociological research, in addition, had suggested that such value-informed science was all but inevitable. Indeed, any scientific contribution, we were told, was a product of a particular time and place, of a particular social and cultural location, of particular interests and values; a "view from nowhere," from a psychological and sociological vantage point, was simply naive (see, e.g., Knorr-Cetina 1981; Knorr-Cetina and Mulkay 1983; and Latour 1987). The ideal of value-free science, in short, seemed unlikely to be a viable ideal, useful for actual science. Mainstream philosophical analysis, finally, went one step further. It challenged the very distinction between social values and the scientific—the distinction between, for example, social values and economists' data about poverty or sociologists' and psychologists' measures of domestic abuse or archaeologists' accounts of human evolution and human flourishing or medical researchers' criteria of health and disease (see, e.g., Putnam 2002 and Dupré 2007). The ideal of value-free science, in short, according to this line of reasoning, might actually have been incoherent.

None of this necessarily tarnished the reputation of the ideal of value-free science among feminists, however. To begin with, none of it was especially new to feminists. Feminist scientists and historians and philosophers had already exposed sexism and androcentrism in even the greatest science (see, e.g., Merchant 1980 and Keller 1985 on early modern science; Hubbard 1979 on Darwin; and de Beauvoir [1949] 1972 and Horney 1950 on Freud), feminists had already regretfully acknowledged that such sexist science was to be expected from sexist societies, and feminists had already regretfully acknowledged how difficult it sometimes was to distinguish science from values, so permeated could science be, and sometimes was, with sexism. What's more, none of what mainstream historians and sociologists and philosophers of science had disclosed was especially damaging to the ideal of value-free science. If the greatest science was sexist, that did not mean that sexism in science was therefore acceptable. It simply meant that even the greatest science was not acceptable—not objective enough, simply not good enough. If sexist societies produced sexist science, science hardly distinguishable from mere prejudice, that did not mean that the ideal of value-free science was not useful for such societies but rather that it was extremely useful—and desperately needed—for precisely such societies. The ideal of value-free

science, after all, offered hope that all could be made right—that science would be able, finally, to provide objective information about women and, in the process, expose and remove society's prejudice against women, not simply reinforce and perpetuate it. Thus, for example, feminist biologist Ruth Hubbard could admit that "There is no such thing as objective, value-free science. An era's science is part of its politics, economics and sociology: it is generated by them and in turn helps to generate them" (Hubbard 1979, 10) and quickly add that

> As scientists, we learn to examine the ways in which our experimental methods can bias our answers, but we are not taught to be equally wary of the biases introduced by our implicit, unstated and often unconscious beliefs about the nature of reality. To become conscious of these is more difficult than anything else we do. But difficult as it may seem, we must try to do it if our picture of the world is to be more than a reflection of various aspects of ourselves and of our social arrangements." (10–11)

And Hubbard could even go on to acknowledge in a footnote that although some "have characterized this process as 'trying to push a bus in which one is riding' . . . I would say that, worse yet, it is like trying to look out of the rear window to *watch* oneself push the bus in which one rides" (33, n. 2).

But the reputation of the ideal of value-free science *was* tarnished for feminists in a different way.[3] The work of feminist scientists clearly seemed more adequate and objective than the sexist and androcentric science it was designed to replace. That much was well documented by feminist historians of science such as Londa Schiebinger (1999). But it also seemed to be as shaped by feminist values as sexist science had been shaped by sexist values—for example, in the questions asked (think of Eichler) or the factors taken to be relevant when designing experiments (think of Sherif) or the factors taken to be relevant when constructing or evaluating hypotheses (think of Keller and Bleier and Rosser). And if feminists' science was shaped by feminist values, then according to the ideal of value-free science this should have made feminists' science just as subjective and inadequate as the sexist and androcentric science it was designed to replace. Feminist scientists struggled to respond. Some said that the function of feminist values in their research was purely motivational and not really a part of that research (Fausto-Sterling sometimes seemed to move in this direction; see, e.g., 1985, chapter 7). Others said that the function of feminist values in their research was not as a replacement for sexist values but as a new kind of methodological control to prevent the entry of sexist values into that research. "We have come to look at feminist critique

3. This was not the only reason feminists became disenchanted with the ideal of value-free science, of course. For example, some feminists argued that objectivity understood as value freedom was a masculinist conception, one that eschewed emotional attachment to the objects of knowledge and also discouraged women as knowers.

as we would any other experimental control," one widely quoted group of scientists said.

> Whenever one performs an experiment, one sets up all the controls one can think of in order to make as certain as possible that the result obtained does not come from any other source. One asks oneself what assumptions one is making. Have I assumed the temperature to be constant? Have I assumed that the pH doesn't change over the time of the reaction? Feminist critique asks if there may be some assumptions that we haven't checked concerning gender bias. In this way feminist critique should be part of normative science. Like any control, it seeks to provide critical rigor, and to ignore this critique is to ignore a possible source of error. (Biology and Gender Study Group 1988, 61–62; see also Eichler 1980, 118: "Feminist science is non-sexist" science)

Some even said that feminist science *was* after all just as subjective as sexist and androcentric science—that objectivity, in fact, was in principle impossible to achieve, by anyone (see, e.g., Stanley and Wise 1983). But in the end, feminist scientists concluded that a new and more adequate understanding of scientific objectivity was needed, one better equipped than the ideal of value-free science to deal with the problems of sexism and androcentrism in science and the scientific responses of feminists. Unfortunately, feminist scientists had few suggestions as to what that new understanding might be.[4]

Now, years later, feminist scientists are in scarcely better shape. True, a great deal of insightful work on the question of scientific objectivity has now been contributed by feminist philosophers of science, but no clear replacement for the ideal of value-free science has yet emerged. Promising candidates are available, of course, but as a group these candidates do not provide consistent advice to scientists or policy makers for ridding science of sexism and androcentrism (as well as racism, etc.) and sometimes even undercut one another. So a careful examination and comparison of their credentials is long overdue. Three of the most promising of these new understandings of scientific objectivity will be considered. Keep in mind the need for which this consideration is being carried out. The ideal of value-free science promised to play for feminist scientists both an epistemic role and a political role—promised to provide both a way to achieve objective knowledge and (by ridding science of sexism, androcentrism, and other inegalitarian values) a way to achieve social reform. And, of course, the two roles were connected, for

4. Feminist historian of science Elizabeth Fee was quoted approvingly when she emphasized the importance to an acceptable notion of scientific objectivity of "the willingness to consider all assumptions and methods as open to question and the expectation that ideas will be subjected to the most unfettered critical evaluation" (1983, 16); Eichler added "a commitment to 'truth-finding'" and "a clarification and classification of values underlying the research" (1988, 14); Bleier (1984, 204) added "critical self-reflection" ("a part of this process is recognizing the degree to which investment of ego and pride in one's previously stated beliefs and theories may corrupt the scientific approach"); and so on. But that's about as far as the discussion got.

the epistemic role was to make possible the political role; objective knowledge was to make possible social reform. Any acceptable replacement for the ideal of value-free science will have to play these two roles as well.

THE SOCIAL VALUE MANAGEMENT IDEAL OF SCIENCE: A SOCIAL APPROACH TO SEXISM IN SCIENCE

Doubtless the most well known of the candidates available to take the place of the ideal of value-free science is the one put forward by feminist philosopher of science Helen Longino (see, for what follows, Longino 1990 and 2002). It relies, to begin with, on a disambiguation of the notion of scientific objectivity. "Science is thought to provide us with a view of the world that is objective in two seemingly quite different senses of that term" (Longino 1990, 62). In one sense, scientific objectivity has to do with the truth of scientific claims to knowledge. In the second sense, scientific objectivity has to do with the distinctive procedures scientists use to obtain that knowledge—what philosophers of science have called scientific rationality. "Common wisdom has it that if science is objective in the first sense it is because it is objective in the second" (63). So the key to the more adequate understanding of scientific objectivity sought by feminist scientists of the past is a more adequate understanding of scientific rationality. And for Longino, that will be a social understanding.

Indeed, the problem, according to Longino, is that feminist scientists of the past focused on the scientific methods of individual scientists to screen out of science social values such as sexism and androcentrism. But no scientific methods, however rigorous and however rigorously applied, can be guaranteed to screen out the various values and interests that scientists bring to their research. To be sure, scientists' values and interests can and do determine which questions they investigate and which they ignore, can and do motivate the background assumptions they accept and those they reject, can and do influence the observational or experimental data they select to study and the way they interpret those data, and so on. The ideal appropriate for science, Longino thus suggests, is not the ideal of value-free science but the "social value management" (Longino 2002, 50) ideal of science. According to this ideal, all social values should be welcomed into science—indeed, encouraged—and all social values, and the science they engender, should be subjected to criticism. So there is a kind of neutrality here, akin to the ideal of value-free science. The only restrictions, in fact, have to do with the social organization of scientific communities. These communities, Longino insists, will have to have, first, public venues for criticism, such as journals and conferences; second, publicly recognized standards—substantive principles as well as values—by reference to which the criticism can be made; third, "uptake" of the criticism

(i.e., the beliefs of the scientific community as a whole and over time, as measured by such public phenomena as the content of textbooks, the distribution of grants and awards, and the flexibility of dominant world views, will have to change in response to the critical discussion taking place within it); and fourth, "tempered equality" of intellectual authority among all parties to the debate, among whom "all relevant perspectives [that can serve as sources of criticism] are represented" (Longino 2002, 131). A science will, then, be *objective—rational*—to the degree that it satisfies these four conditions, to the degree that it permits what Longino calls "transformative criticism" (1990, 76). And the output of such a science will constitute *knowledge*, even if that output is inspired and informed by social values, if the community that practices it meets these conditions and the output conforms sufficiently to its objects to enable the members of the community to carry out their projects with respect to those objects.

Thus, for Longino's social value management ideal of science, scientific rationality ("objectivity") is a function of the workings of scientific communities, not—as with the ideal of value-free science and the more formal methodological approach it rationalizes—the attitudes and behavior of individual scientists.[5] Will this new ideal with its social approach prove more successful than the old ideal with its methodological approach in dealing with sexism and androcentrism in science in a defensible way? What credentials assure us that it will? In her book *The Fate of Knowledge* (2002), Longino does much to exhibit these credentials. She shows her social value management ideal of science informed by some of the most important findings of sociohistorical research. She shows her ideal informed, as well, by the enduring insights of epistemological reflection. At the same time, she shows her ideal able to integrate all of these findings and insights into a coherent account of science, an account free of the confusions that have frequently accompanied them. Most important, however, Longino implies that her ideal embodies just what we have been looking for to replace the ideal of value-free science, just what we mean by terms such as *knowledge* and *rationality*. Granted, this last accolade somewhat strains credulity. After all, all of us, before Longino wrote, thought we had some handle on the meanings of these terms, but doubtless most of us had no handle on what Longino describes, tempered equality and public venues and uptake and the rest, not even a preanalytic, prearticulated version of what she describes.[6] No matter. If Longino's candidate is

5. This is the point that most clearly distinguishes Longino's approach from that of the feminist scientists mentioned in note 4 above.

6. Certainly, the sorts of questions Longino says must be answered in order to complete her analysis—for example, "In determining what counts as inappropriate exclusion of dissenting perspectives, does it matter what kind of issue is involved?" and "What bearing should greater cognitive authority have on the attribution of intellectual authority, understood as the capacity to participate in critical discussion and thus to contribute to critical understanding?" (Longino 2002, 133)—certainly these sorts of questions do not seem answerable by yet another round of reflection on the meaning of *knowledge* and related terms.

most comfortable in the dress of traditional analytic philosophy, then it is in the dress of traditional analytic philosophy that it shall be given its examination.

On, then, to the imaginary tale, de rigueur in traditional epistemology. But this time, the tale is not about some solitary epistemic agent named Smith, his lucky happenstances, and his (usually unsuccessful) claims to knowledge, as in days of old, but about a scientific *community* named Smith—or, rather, named PETERS, that is, the Privileged, Exclusive, Talented, Elite, Royal Society. PETERS is made up of a subset of the privileged and talented of society S, but PETERS is also a very elite society, very exclusive. It excludes all those, albeit sometimes talented, persons who fall into various unfavored classes (the nonprivileged, the underprivileged). And PETERS has power—it is, after all, a royal society. So PETERS, knowing where its bread is buttered and also sharing in the perspectives of the butterers, pursues a particular kind of cognitive enterprise, one that serves its particular needs and interests. PETERS, of course, is a scientific society, concerned with understanding the world and interacting with it successfully. But PETERS is also a privileged, exclusive, talented, elite, royal scientific society, and that leaves a definite mark on the parts of the world it seeks to understand and the ways it seeks to interact with them. So, for example, PETERS investigates physical and chemical questions related to its concern with war making and military preeminence, PETERS investigates biological and psychological questions related to its concern with the maladies that afflict the privileged and the reasons they are superior nonetheless, PETERS investigates archaeological questions related to its concern with the routes by which the privileged have achieved their superior state of development, and so forth. And PETERS's concepts and theories and models and methods and standards and values reflect these concerns, these privilegecentric and privilegist goals.

Our question is, would PETERS, over time, produce *knowledge* for its members? We can imagine that PETERS regularly holds conferences and publishes journals in which all its members are encouraged to participate and in which all are treated equally. We can imagine that in these venues, prolonged and frequently heated critical exchanges take place, exchanges that pay scrupulous attention to shared standards. We can imagine that follow-up exchanges regularly take place as well. And we can imagine that the intellectual products that emerge from all of this activity conform well enough to their objects to enable PETERS to pursue its (privilegecentric and privilegist) projects to its (or, rather, its members') satisfaction. We can even imagine that, after some time, PETERS invites, even encourages, members of the underprivileged classes—at least, their talented members—to join its ranks, master its methods and standards and values and concepts and models and theories, and contribute to its (privilegecentric and privilegist) projects; we can even imagine that PETERS encourages these underprivileged ones to develop "alternative points of view" that can serve as a "source of criticism and new perspectives" (see Longino 2002, 132), so that

finally "all relevant perspectives are represented" (see Longino 2002, 131) in PETERS's exchanges, that is to say, all perspectives relevant to the satisfaction of PETERS's privilegecentric and privilegist goals. Would PETERS now be producing knowledge for its members? It would seem that Longino's candidate must answer yes, although it should answer no. And would that knowledge—if it is knowledge—be free of privilegecentric and privilegist prejudices and thereby a suitable springboard from which to bring about social reform in society S rather than a reinforcement of those same prejudices? It would seem that we must answer no.

"Stop the examination," I hear you saying. "It's unfair! It's rigged! Longino's candidate is getting pushed in a direction it does not want to go. When Longino says that in order for a scientific community's critical interactions to generate knowledge, 'all relevant perspectives' must be represented, she does not only mean all perspectives that might serve that community's goals; she means all perspectives that might relate in any way at all to those goals, that is to say, all perspectives that might support them, or clarify them, or develop them, or add to them, or revise them, or replace them, and so on. 'Such criticism,' she says, 'may originate from an indeterminate number of points of view, none of which may be excluded from the community's interactions without cognitive impairment'" (Longino 2002, 133).

OK. Start the examination again. Imagine once again that PETERS finally encourages members of the underprivileged classes to join its ranks and develop alternative points of view—all relevant alternative points of view—that can serve as sources of criticism and new perspectives. Would PETERS now be producing knowledge for its members? And would that knowledge—if it is knowledge—be free of privilegecentric and privilegist prejudices and thereby a suitable springboard from which to bring about social reform in society S? We cannot now say simply that PETERS's cognitive output would have to serve its original privilegecentric and privilegist goals and thereby serve the status quo in society S, for over time PETERS's cognitive output might evolve in all sorts of ways as a result of the critical discourse occurring in it. The underprivileged ones in PETERS, though trained in its privilegecentric and privilegist research traditions, might come to have the wherewithal to develop alternatives to some of those traditions, perhaps aided by changes over time in PETERS or in PETERS's science or in PETERS's surrounding society S. The underprivileged ones in PETERS might even succeed over time in building significant support for some of these alternatives, might even succeed in crystallizing new research traditions around some of them that parallel in many ways the older traditions, might even bring about the replacement of some of the older traditions. Women, after all, originally largely excluded from Western science and then, when included, trained in its androcentric and sexist research traditions, still came to have the wherewithal to develop alternatives to some of those traditions, aided by the sheer numbers of women—the "critical mass" of women—in some research areas, and aided by the women's

movement in society at large as well as by changes in other academic fields. Women even succeeded over time in building support for some of these alternatives, even succeeded in crystallizing new feminist research programs around some of them to compete with the older programs, even succeeded in replacing some of the older programs. The underprivileged ones in PETERS, though trained in its privilegecentric and privilegist research traditions, then, might come to have the wherewithal to replace them. But then again, they might not. They might not have egalitarian political movements in society S to aid them, they might have political backlash instead; they might be stymied by available mathematical resources or instrumental technologies or preferred modes of analysis; they might be affected by funding cutbacks or staffing problems or family needs. Certainly, women in science have been thwarted by such factors as these, and certainly, women in science have met with far less than unbridled success in trying to rid science of sexism and androcentrism.

So, what is the upshot? If what PETERS produces is *knowledge* for its members according to Longino's social value management ideal of science, this knowledge need not be free of privilegecentric and privilegist prejudices,[7] need not be a suitable springboard from which to bring about social reform in society S. If Longino's ideal fulfills the epistemic role of the old ideal of value-free science, in short, it still may not fulfill the political role.[8] Well, so what? Had the ideal of value-free science been acceptable, it would have provided a way to rid science of sexist and androcentric values and the like and thereby promote social equality. But the ideal of value-free science was *not* acceptable. So why should its successor have actually to do what it, itself, merely promised but could not actually do? Why, in short, should the successor of the ideal of value-free science have to play a political role along with an epistemic role? Then again, if we excuse the successor of the ideal of value-free science from playing its predecessor's political role, we not only lose scientific knowledge as an ally in the fight for social justice, we set scientific knowledge up as part of the problem, part of what reinforces and perpetuates prejudice rather than exposes and removes it. Is there a better way to go?

7. This may be a reason to deny that what PETERS produces is knowledge after all—that is, to deny that Longino's social value management ideal of science fulfills the epistemic role of the old ideal of value-free science.

8. Of course, Longino might say at this point that if the requisite political movements or analytic methods or mathematical resources or instrumental technologies or funding or staffing or family supports were not there to aid the underprivileged ones in PETERS, then "all relevant perspectives" were not there in PETERS, either. Hence the conclusion to be drawn is not that the social value management ideal of science fails to fulfill the political role of the old ideal of value-free science but that the social value management ideal of science has not been provided with a genuine test case to see if it does. But such a response on Longino's part would threaten to make her candidate's fulfillment of the political role of the ideal of value-free science true by definition (of "all relevant perspectives are represented"). It would also threaten to make her candidate unrealizable in practice—just the sort of problem that caused the need to replace the ideal of value-free science to begin with.

THE EMPIRICIST IDEAL OF SCIENCE: A NATURALIST APPROACH TO SEXISM IN SCIENCE

Consider a second candidate, put forward by feminist philosophers of a more naturalist bent than Longino. This candidate—call it the empiricist ideal of science—rejects a priori prescriptions regarding the proper composition of scientific communities or the proper conduct of inquiry. It rejects, as well, the single-minded focus of its predecessor on scientific practice to the exclusion of scientific outcome. What the empiricist ideal advocates, instead, is a close look at *successful* scientific practice in order to identify those of its features that contribute to and explain its success. For the empiricist ideal, in fact, scientific rationality just *is* whatever contributes to and explains scientific success (see, e.g., Antony 1993, 1995; Solomon 2001). Can the empiricist ideal, with this understanding of scientific rationality, fulfill the political as well as epistemic roles of the old ideal of value-free science? What reasons suggest that it can?

When we take a close look at successful scientific practice during the last three decades, we find that a great deal of that part of it that is gender-relevant has been produced by feminists. We find, that is to say, that the contributions of feminists—the wide-ranging critiques of traditional science in such fields as psychology, sociology, economics, political science, archaeology, anthropology, biology, and medical research and the new research directions and research results forged in the wake of those critiques—these contributions have been not only free of sexism and androcentrism but also more empirically successful than the sexist and androcentric science that went before (see, e.g., Schiebinger 1999 and Creager, Lunbeck, and Schiebinger 2001 for the kinds of wide-ranging changes in science that have occurred due to feminism). What features of this successful science explain its success? There are at least two possibilities, both offered by feminist naturalists. The first—the standpoint hypothesis—suggests that the success of feminists' science may be due in large part to the fact that feminists tend to be women and women, other things being equal, tend to be in a better position than men to detect sexism and androcentrism in science and replace them with more adequate perspectives. After all, feminist philosopher of science Sandra Harding reminds us, sexism and androcentrism directly benefit men, whereas they oppress women. As a consequence, women in general—and women scientists in particular—are more likely than their male counterparts to be critical of such values. "They have less to lose by distancing themselves from the social order; thus, the perspective from their lives can more easily generate fresh and critical analyses" (Harding 1991, 126; see also Wylie 2003 and 2007, which treat standpoint theory more explicitly as a naturalist hypothesis). The second—the values hypothesis—suggests that the success of feminists' science may be less a function of feminists' standpoints than of feminists' values, where values, like any other apparently nonepistemic feature of scientific practice (such as competitiveness or the

desire for credit for one's accomplishments), need not function as hindrances but might actually function as aids in the acquisition of objective knowledge. Indeed, supporters of this second hypothesis point out that cases in which feminist values have clearly influenced science (e.g., by motivating particular lines of research or the maintenance of particular social structures) *have* been cases in which the science produced is not only free of sexism but also more developed and more empirically adequate than before (see, e.g., Antony 1993, 1995; Campbell 2001; Anderson 1995, 2004; Wylie and Nelson 2007). Now, if either of these hypotheses is correct—that is to say, if feminists through the one means (standpoints) or the other (values) *produce* scientific success, more scientific success than sexists—then, of course, the scientific ideal that defines rationality in terms of the production of scientific success will thereby guarantee that feminist science is rational science (i.e., more rational science), and sexist science is not (is less). If either of these (feminist naturalists') hypotheses is correct, in short, then the empiricist ideal of science *will* fulfill the political as well as epistemic roles of the old ideal of value-free science.

So runs the empiricist ideal's defense. It hinges, of course, on the correctness of at least one of the above feminist naturalist hypotheses. But is either one of these hypotheses correct? Consider, to begin with, the standpoint hypothesis. What kind of evidence do we have in its favor? Certainly, women scientists have achieved breakthroughs that the men scientists before them did not, breakthroughs that seemed clearly connected to the fact that they were made by women. For example, biological anthropologist Sarah Blaffer Hrdy reports that, in the face of abundant, generally available evidence to the contrary, modern sexual selection theory up to the mid-1970s still featured sexually aggressive, promiscuous males actively courting sexually "coy," passive females. Males were very much the focus of investigation, and such topics as female promiscuity and the effect of female social status and female expertise in child-rearing on female reproductive success were ignored. With the significant influx of women into primatology starting in the 1970s, however, all that changed. The women brought with them new, nonandrocentric, nonsexist perceptions and questions and hypotheses about female primates. And the result for primatology, says Hrdy, has been a new focus on female reproductive strategies and, with it, a fundamental rethinking of sexual selection theory (Hrdy 1986). Again, in her 1971 study of 30 impoverished black teenage girls, sociologist Joyce Ladner explains that because she was a black woman, with a black woman's socialization and life experiences—in effect, a black woman's perspective—she found herself dealing with her black female subjects' problems on a personal level as well as on a professional level, in terms of the sociological concepts ("social deviance," "social disorganization," "social pathology," etc.) she had been taught. And what came out of this "double consciousness" was not only a quite radical reconceptualization of the girls' pregnancy, school dropout, and other problems ("a very

healthy and successful adaptation, given their limited resources, had been made by all of these girls to a set of very unhealthy environmental conditions") but also a quite radical reconceptualization of significant parts of sociological theory and methodology (Ladner 1971).

Certainly, then, women scientists have achieved breakthroughs that men scientists have not, breakthroughs that seemed closely connected to the fact that they were made by women. But women scientists have also made contributions indistinguishable from those of the men, contributions sometimes just as sexist and androcentric as those of the men. For example, a December 2006 study analyzing 40 years of gender-difference research published in four journals of the American Psychological Association (a broader range of journals was also studied in less depth, to assess representativeness of the findings) showed that males are still treated as the norm in this research against which girls and women are measured. The striking point to be noted here is that between 1965 and 2004, the years covered in the study, the journals studied ceased to be male-dominated: roughly equal numbers of females and males were represented among both the published authors and the research subjects. In short, this new study shows that women psychologists' research regarding gender differences continues to be just as androcentric as men's (and also that men's research is sometimes just as free of sexism and androcentrism as women's—the study authors, after all, are a man and a woman) (Hegarty and Buechel 2006). But there are also plenty of more familiar examples to the same effect: the originator and most militant promoter of the idea of premenstrual syndrome (PMS) was a woman physician and researcher, Katharina Dalton; the scientists who seek to explain greater male than female aggressiveness in terms of prenatal hormone exposure include many prominent women researchers (e.g., Anke Ehrhardt and June Reinisch); the scientists who seek to explain the differences in women's and men's status in society in terms of biologically based cognitive differences include many prominent women researchers (e.g., Sandra Witelson and Doreen Kimura), and so on (see, for more details, Fausto-Sterling 1985).

Of course, few standpoint theorists extend an automatic epistemic/political advantage to women scientists. The women's standpoint they speak of must be discovered and developed through a collective process of political struggle—through some sort of feminist consciousness raising and solidarity building. Even Alison Wylie, who has presented survey evidence to show that it was women archaeologists' standpoint as women, not their feminist consciousness (which half the time they denied having), that brought about the dramatic changes in archaeology that began in the late 1980s (Wylie 1997)—even Alison Wylie has qualified this claim. In "Doing Social Science as a Feminist: The Engendering of Archeology," for example, she speaks of "feminist commitments and insights" as having played "a critical, if often indirect, role" in producing the changes (Wylie 2001, 28). And Sarah Hrdy also points to the influence of feminism to explain how women primatologists were able to bring about a fundamental rethinking of sexual selection theory in the 1970s and 1980s. Indeed, she speculates

that women primatologists identified with primates of their own sex, just as the men primatologists had done before them, and allowed this identification to influence their research focus. But because feminism was then changing the self-perception of these women, their identification with female primates was simultaneously leading them to their new nonandrocentric, nonsexist perceptions and questions and hypotheses about female primates. Some such feminist consciousness is necessary, according to standpoint theorists, in order to achieve the relevant standpoint, the sort of standpoint that leads to more adequate, nonsexist science. And perhaps it is this feminist consciousness that is lacking in the cases in which the contributions of women scientists are just as inadequate and sexist as those of their male counterparts. But exactly what kind of feminist consciousness is necessary is never specified, certainly not clearly enough to allow the standpoint hypothesis to be put to a reasonable test. And to the extent that a feminist consciousness is held to be crucial, to that extent the standpoint hypothesis starts metamorphosing into the values hypothesis.

Of course, the values hypothesis has problems of its own. Even in the case of the so-called feminist contributions to science over the last three decades, its advocates would be hard pressed to show that the progress that was made in every case was the effect of feminist values rather than other factors. Sometimes, in fact, the point is acknowledged by those reflecting on that progress. For example, the Biology and Gender Study Group claims only that the eye-opening studies that led to new models of fertilization and sex determination in the 1980s "can be viewed as feminist-influenced critiques of cell and molecular biology": "It should be noted that the views expressed in this essay may or may not be those of the scientists whose work we have reviewed. It is our contention that these research programs are inherently critical of a masculinist assumption with these respective fields. This does not mean that the research was consciously done with this in mind" (Biology and Gender Study Group 1988, 68, 74, n. 5). The values hypothesis has deeper problems than the solidity of its evidence, however. After all, it treats feminists' egalitarian values as merely causally relevant "social factors" or "social biases," on a par with other factors such as competitiveness or the desire for credit or other values such as sexism or racism.[9] All of these become possible aids to the

9. Antony (1993) and Campbell (2001) classify together sexism and racism along with feminist social values as "biases." They then make a distinction between "good biases" (those that "facilitate the gathering of knowledge," that is, those that "lead us to the truth") and "bad biases" (those that "lead us away from the truth"). In short, "*we must treat the goodness or badness of particular biases as an empirical question*" (Antony 1993 215, emphasis hers; and see Campbell 2001, 196, who quotes Antony approvingly, although he tries for a more elaborate kind of naturalism in Campbell 1998). Solomon (2001) also classifies together sexism, racism, and egalitarian social values, now as "ideology," and goes on to classify together these ideological social factors ("decision vectors" or causes for theory choice) with other "non-empirical" decision vectors such as birth order, desire for credit, deference to authority, and competitiveness. But, for her, an equal distribution of such nonempirical decision vectors among competing theories is what generally helps to produce "normatively appropriate" science.

production of scientific success, and all must be empirically tested to see if they are. Any of them will do, we are led to infer, if only they can prove their mettle in scientific research. (This is made explicit when "good" social values in science are *defined* as those that aid the acquisition of objective knowledge, those that are epistemically fruitful.) So if, for example, a close comparative study of German medical science before, during, and after the Third Reich discloses that Nazi social values produced the best scientific results, the most abundant and most empirically successful science, then Nazi social values would be good values and should therefore be welcomed into science. Or if such a study discloses that Nazi social values produced a science just as successful as the others but no better, then it should be a matter of complete indifference whether Nazi social values or the other sciences' values should find their way into science, since none of the values would be justified over the others. And this is remarkable given that one of the main factors that brought about the success of Nazi medical science was the absence in it of good social values—for example, the absence (sanctioned by Nazi social values) of moral constraints on human experimentation.

This is not to say that the epistemic success (or failure) of a scientific research project tells us nothing about the justifiability of the social values that guide it. But what it tells us must take into account a great many other factors besides that outcome—for example, which scientists were involved in the project, the level of their talents and training, and the conceptual, material, and social resources at their disposal. Factors such as these help to explain the failure of research guided by arguably good social values (such as some of the egalitarian social values guiding Lysenko) and also help to explain the success of research guided by clearly bad social values (such as the racist social values guiding the Nazis). But, of course, moral and legal principles, as well, are relevant to the assessment of the social values that guide scientific research—think of the respect for individual autonomy and self-determination and the Hippocratic Oath's admonition that physicians should "abstain from all intentional wrongdoing and harm" that informed the response to Nazi medical research in the Nuremberg Doctors Trial and the Nuremberg Code on human experimentation that followed (Katz 1996). And these moral and legal principles, in turn, are themselves informed by factual considerations, including the factual considerations that result from scientific inquiry. What all of this shows is that the assessment of the social values that guide a scientific research project, whatever the epistemic outcome of that project, is a complex, multifaceted undertaking.

The upshot is that the hypotheses underwriting the empiricist ideal's defense—the values hypothesis no less than the standpoint hypothesis—are just that, hypotheses, and quite problematic hypotheses at that. They show at most that the empiricist ideal *may* be able to fulfill the epistemic and political roles of the old ideal of value-free science; they give us no strong reasons for thinking that the empiricist ideal *will* be able actually

to do so. This is, unfortunately, the same sort of conclusion we came to when we considered Longino's proposed social value management ideal of science. There, again, we found that her ideal may be able to fulfill the political as well as epistemic roles of the old ideal of value-free science, but we were left with no strong reasons for thinking that it would actually do so. If we are to pin our hopes for a science that is at once truly rational and truly a basis for social reform on a new understanding of scientific rationality (objectivity), we need a stronger candidate for that understanding than these.

THE IDEAL OF SOCIALLY RESPONSIBLE SCIENCE: A POLITICAL APPROACH TO SEXISM IN SCIENCE

There is, however, one more candidate awaiting examination—a less sophisticated candidate, by far, than the others but with a certain down-to-earth, home-spun charm. It can be called the ideal of socially responsible science. Coming after the other candidates, as it is, this candidate has had the opportunity to learn from their insights as well as their mistakes. Thus, like the ideal of value-free science, the ideal of socially responsible science recognizes that sexism and androcentrism must be rooted out of science if science is to replace prevailing ignorance and prejudice and misinformation about women with more adequate perspectives, but unlike the ideal of value-free science, the ideal of socially responsible science also recognizes that rooting sexism and androcentrism out of science is tantamount to implanting egalitarian social values into science. Like the social value management ideal of science, on the other hand, the ideal of socially responsible science recognizes that social values inevitably enter into science, but unlike the social value management ideal of science, the ideal of socially responsible science also recognizes that we, as a society, have a definite say—through funding priorities and restrictions, for example—as to what these social values will be. Indeed, given that science is both a profound shaper of society and a profound beneficiary of society, these social values should be chosen so as to meet the needs of society, including the justice-related needs of society. And finally, like the empiricist ideal of science, the ideal of socially responsible science recognizes that scientific rationality must be defined in terms of scientific success, but unlike the empiricist ideal of science, the ideal of socially responsible science also recognizes that scientific success must be defined in terms of social success—human flourishing, what makes for a good society—as well as empirical success. Under the ideal of socially responsible science, in short, our scientific views—and hence, ultimately, our generally accepted knowledge—would no longer be plagued by sexism and androcentrism (as well as racism and other inegalitarian values) simply because those would be the morally justified political conditions under which scientific research would be pursued.

The ideal of socially responsible science is thus able to fulfill the political role of the old ideal of value-free science. But is it able to fulfill the political role by safeguarding science as a genuine source of knowledge—as the ideal of value-free science aspired to do—or is it able to fulfill the political role by sacrificing science as a genuine source of knowledge? In short, is the ideal of socially responsible science able to fulfill the epistemic role as well as the political role of the ideal of value-free science? This is the question we need answered if we are to determine whether the ideal of socially responsible science can fill the position now vacated by the ideal of value-free science. Since the ideal of socially responsible science seems to be the ideal to which many feminist scientists now subscribe, it will be helpful to examine it in the context of what these scientists are actually doing. A positive outcome of the examination will then provide not only a rationale for what these scientists are doing but also a very concrete model of what other scientists can and ought to do. And a negative outcome will provide an equally concrete model and rationale for what these and other scientists ought not to do.

Consider, then, a new psychological research program described by Carolyn West, concerned with the problem of domestic violence in the United States (West 2002; and see West 2004). The aim of this program is complex: to uncover the similarities in intimate-partner violence within the black and white communities of the United States without negating the experiences of black women and simultaneously to highlight the differences within the black and white communities without perpetuating the stereotype that black Americans are inherently more violent than other ethnic groups. This aim requires charting a new course for research. For example, it requires broadening the definition of partner violence to include psychological, emotional, verbal, and sexual abuse as well as physical abuse. It also requires changing the ways violence is measured—from merely counting violent acts and measuring their severity (which focuses on discrete male behaviors) to taking into account the contexts, motives, and outcomes of the violent acts (which focuses on female experiences) using a combination of qualitative and quantitative research methods, including listening to the voices of battered women. All this dramatically transforms the picture of racial similarities and differences drawn from past research—the picture according to which, for example, black women, when compared with their white counterparts, are significantly more likely to sustain and inflict aggression, especially aggression involving weapons and culminating in hospitalization. The new research program involves other changes as well, such as a revision of measurement scales to reflect more than the experiences of white European Americans taken as the norm and investigations of within-group differences in the black and white communities to determine whether what appear to be racial differences are not simply socioeconomic differences instead. And the program involves integrating participants into every stage of the research process, from planning to implementing, interpreting, and disseminating

results, in order to reduce one-sided research interpretations. The result is the kind of research that both motivates social reform and helps to bring it about.

Our question is, what are the effects of the egalitarian social values that operate within West's research program? Do they compromise the justifiability of the knowledge the program provides? First, what are these values? They seem to be "Women deserve to live without fear of violence from domestic partners" and "Black women deserve the same opportunities as white women to live in such partnerships." These values are well justified both inside and outside feminist theory; they should be uncontroversial. Second, what role do these values play in the program? Remember that West makes it a central part of her aim not to perpetuate the stereotype that black Americans are inherently more violent than other ethnic groups. The reason is that this stereotype in people's minds—in the minds of researchers and politicians and service providers, for example—makes it more likely that black women's needs related to domestic violence will be treated less seriously than white women's needs, or even ignored altogether. After all, if violence is perceived as inevitable—as somehow innate or unique to the black culture—intervention efforts are more likely to be perceived as futile. In short, if black women deserve the same opportunities as white women to live in domestic partnerships free of violence—West's egalitarian social value—then the stereotype connecting blacks and violence must not be perpetuated. This means that West's research program, as far as empirically possible, must highlight the similarities in domestic violence within the black and white communities and seek to explain whatever dissimilarities appear within these communities in terms of social differences such as racism and poverty.[10] But none of this must obscure in any way black women's experiences, since to do so would again be to shortchange black women's needs and, hence, fail to provide black women the same opportunities as white women to live in partnerships free of violence.

The upshot: West's research program is controlled through and through by sound egalitarian social values. But it is equally controlled through and through by sound epistemic values. Although the science here is thoroughly politicized, in short, it is not at the expense of its mission to provide

10. One example West presents:

> Black feminist thought can make a significant contribution by keeping the focus on historical perspectives. During slavery and well into reconstruction, Black women witnessed their husbands, fathers, sons, and brothers being abducted by slave owners, police officers, and Klansmen. For the contemporary Black woman, having her partner arrested may be reminiscent of these earlier historical traumas. Although she wants the violence to stop, she may be reluctant to thrust her batterer into a system that is discriminatory, hostile, and overcrowded with Black males. Batterers realize this and will often use this history to further manipulate their partners. Black feminists recommend that this history be acknowledged while simultaneously holding African-American men accountable for their abuse. (2002, 229)

genuine knowledge. And this should not be the least bit surprising. After all, research such as West's cannot fulfill its social objectives, cannot effect improvements for battered women in both the black and white communities, unless it does fulfill its epistemic objectives, unless it does get a firm handle on the reality it means to reform. But this means that research such as West's, with its two kinds of interrelated objectives, social and epistemic, shaped by two kinds of values, social and epistemic, should be judged by two kinds of standards, not one—by moral/political standards as well as by epistemic standards. Such research should be found wanting if it fails sound epistemic requirements. But it should also be found wanting if it is shaped by unacceptable social values. How else can science take its rightful place in the forefront of social change?

WHERE YOU TAKE OVER THE EXAMINATION OF THE IDEAL OF SOCIALLY RESPONSIBLE SCIENCE

So, do the egalitarian social values that operate within West's research program compromise the justifiability of the knowledge the program provides? They do not. The candidate has successfully answered our question.

"Not at all," you exclaim, voice rising. "The answer given is too quick. Indeed, the answer given makes it look as though the epistemic objectives and the social objectives of a research program such as West's can never conflict, so that the social objectives, or the social values that lie behind them, can never contaminate the knowledge produced. But this is far too optimistic. After all, what if the stereotype that black Americans are inherently more violent than other ethnic groups were true?[11] The egalitarian-value-directed research program described would never allow this truth to be discovered, and the ideal of socially responsible science would never allow any less egalitarian research program to be pursued—say, one that straightforwardly investigated the truth of the stereotype by searching for cultural factors associated with violence, cultural factors that differ from one ethnic group to another. So, in this case, social objectives and epistemic objectives would clearly conflict, and the ideal of socially responsible science would sacrifice the epistemic objectives for the sake of the social. This means that the ideal of socially responsible science cannot

11. Note that West begins by explaining that the studies conducted to date present a "contradictory" picture of racial differences in domestic violence. "In summary, some researchers found similar rates of partner violence across racial groups. . . . In contrast, other investigators discovered that Black women, when compared to their White counterparts, were significantly more likely to sustain and inflict aggression. Moreover, they were more likely to be victims of severe violence. This pattern was reported at every stage on the relationship continuum" (2002, 218). West's research program is a socially and epistemically sophisticated way to deal with this contradictory situation. What is now being suggested is that there may be other ways, and perhaps even better ways, to deal with it that should also be considered.

be relied on to fulfill the epistemic role as well as the political role of the old ideal of value-free science."

"Not so," comes the candidate's reply. (This candidate is not about to concede defeat!) "If the stereotype connecting blacks and violence were true, that truth *could* be discovered with West's program. All the program requires, remember, is that dissimilarities in domestic violence within the black and white communities be explained, *as far as empirically possible*, in terms of social differences such as racism and poverty. The program does not guarantee that any of these explanations will be successful. Indeed, if the stereotype connecting blacks and violence were true, all of these explanations at best would have limited success (depending on whether they also were true), and that would provide (indirect) support for the stereotype. And since a central aim of the program is to make black women's experiences with domestic violence as visible as white women's experiences, the dissimilarities between the two would be made visible as well—just those dissimilarities whose failure to be socially explained would count in favor of the stereotype. So neither the ideal of socially responsible science nor West's particular research program sanctioned by that ideal makes knowledge unreachable. Nor do they 'contaminate' the knowledge produced. They simply channel science's search for knowledge in some directions and away from others in response to the needs we present as a society."[12]

"But the 'channeling' runs very deep!" you retort, irritation in your voice. "It affects not only research questions but also, as we have seen, such aspects of research as concepts (e.g., the concept of 'partner violence' itself); measurement scales and techniques; methods of subject selection; strategies of data collection, analysis, and interpretation; and even methods of publishing and disseminating results. It may even affect other central

12. We can think of West's program as a Lakatosian research program (see Lakatos 1970). Her denial of the stereotype that black Americans are inherently more violent than other ethnic groups is part of the "hard core" of the program. Her instructions to highlight similarities and to explain away dissimilarities between the black and white communities are part of the "negative heuristic" of the program that protects the hard core from refutation. And her instructions regarding how to do this—for example, to revise concepts such as "partner violence" to uncover similarities and to formulate hypotheses to explain dissimilarities in terms of social factors such as racism and poverty—are part of the "positive heuristic" of the program. Finally, although Lakatos never considered social values as playing a legitimate role within scientific research programs, what motivates West's program are her egalitarian social values. What the candidate has just been saying is that there are conditions under which it will be rational to abandon (to consider "refuted") West's research program, conditions that Lakatos tried to describe in detail. Notice, however, that the abandonment of West's research program would not necessarily justify the abandonment ("refutation") of West's egalitarian social values, for reasons that were made clear above. For example, if it were concluded that the stereotype about black Americans is true (which is the denial of the program's hard core), say, because violence is inherent in black culture, it would not follow that black women do not deserve the same opportunities as white women to live without fear of violence from domestic partners. See, for example, the complicated debate about the relationship between feminism and multiculturalism in Okin 1999.

aspects of the research process, such as consideration of the consequences of error and setting acceptable levels of risk (see, e.g., Douglas 2000). So the ideal of socially responsible science and the research programs it sanctions may not make knowledge unreachable or contaminate the knowledge produced. But they surely slow down the production of knowledge if the channeling is in the wrong direction. If the stereotype connecting blacks and violence were true, for example, the fastest way to discover that truth would doubtless be to investigate the stereotype directly. Not knowing whether the stereotype is true, however, the most plausible way to proceed would be to pursue multiple research programs—the stereotype-focused research program as well as West's egalitarian-value-directed research program, for example. Not only would this be the most efficient way to proceed, but it would also provide valuable comparative assessments of programs in addition to the direct empirical assessments available to each individual program. It would also provide the most thorough assessments, since one program might generate data relevant to another that the other had no access to itself, data with which it nevertheless has to deal. Pursuing multiple research programs would also make more likely the discovery of multiple causal factors and a more complex understanding of the subject at hand. Limiting science to 'socially responsible' research, by contrast, places unnecessary obstacles in the way of science's search for truth."

"That's not true!" gasps the candidate. "Socially responsible research, of course, cannot be guaranteed to produce truth. But neither can socially irresponsible research. Nor can socially responsible research—or socially irresponsible research—be guaranteed to be efficient in its search for truth, or more efficient than the other. We simply cannot say, a priori, what kind of research will produce the best results. If the stereotype connecting blacks and violence were true, for example, would scientists more likely discover that truth, or discover it more quickly or easily, if they explored all plausible ways in which blacks could be inherently disposed to violence, or would they more likely, or more quickly or easily, discover that truth if they explored all plausible social factors that could explain the dissimilarities in violence within the black and white communities? If it be said that the former 'direct' approach would obviously be better, it must be noted that many in the black community would not cooperate with that approach, whereas they would cooperate with the latter, socially responsible approach (see West 2002 and 2004 for the 'culture of silence' that has surrounded the problem of domestic violence in the black community, the reasons for it, and the methods that have proven valuable to overcome it). That lack of cooperation would have a profound effect on 'efficiency.' It must also be noted that the latter, socially responsible approach, no less than the other, could make use of multiple research programs, with all of the benefits those bestow. So the comparison would not have to be between (as you seemed to suggest) West's program plus the stereotype-focused research program on the one side versus West's

program alone on the other. The socially responsible (second) side of the comparison could include, in addition to West's program, any number of other socially responsible alternative or complementary research programs. And of course, it would matter what all of these various research programs were like, which scientists were pursuing them, how much funding they had at their disposal, what background knowledge and conceptual and technological resources they could draw on, etc., etc. The upshot is that you simply cannot assume that limiting science to socially responsible research will slow science down in its search for truth."

"But what if it did?" the candidate continues. "What if the efficiency of research *were* compromised by the restrictions imposed by the ideal of socially responsible science? What grounds are there for saying that these restrictions would then constitute '*unnecessary* obstacles in the way of science's search for truth' when these restrictions—the social values like West's egalitarian values imposed by the ideal of socially responsible science—would be *justified*? Everyone concedes that the value of efficiency in research has its limits, that there are other values, including other social values, that are more important. It might be far more efficient for searching out the truth, for example, if scientists simply ignored the risks to human subjects or society or the environment posed by various lines of research and ethics committees and publishers and funders and the public at large allowed them to do so. But acting in this way would be unconscionable despite the epistemic efficiency it might offer. The ideal of socially responsible science simply extends these constraints already recognized as appropriate for science. In so doing, it does not sacrifice science as a genuine source of knowledge but merely acknowledges that science has other goals and other responsibilities besides its epistemic ones. Thus, it might be more efficient for searching out the truth about domestic violence in the black community if scientists pursued any research they pleased—for example, the stereotype-focused research program in addition to West's approach—irrespective of its effects on the black community. But acting in this way would again be unconscionable. After all, the stereotype-focused research program begins with a characterization of blacks born of prejudice, with no serious empirical backing, and dignifies it by making it the subject of scientific research. It thereby suggests that the characterization has some plausibility (if it had none, why would scientists bother to investigate it?). And so, the stereotype-focused research program helps to keep the stereotype alive, paradoxically, even while it may be accumulating evidence against that stereotype and as one result (there are others) decreases the likelihood that black women will receive the help they deserve to combat domestic violence. West's program, in contrast, does none of this—is explicitly designed to do just the opposite—even though it also, indirectly, investigates the stereotype. The difference is that West's program aims to help the black community with the knowledge it gathers and is in an excellent position to do just that. The stereotype-focused research program seems aimed to do just the opposite and is in

an excellent position to do just that. Small wonder that West has received an award from the black community for the work she is doing—the Outstanding Researcher Award from the Institute on Domestic Violence in the African American Community—whereas it is safe to say that the stereotype-focused research program would meet with a very different response."

WHERE YOU COME TO A DECISION

Is it now clear that the ideal of socially responsible science can fulfill the epistemic role as well as the political role of the old ideal of value-free science?

"If it is," you reply, "that will still not suffice to justify embracing it. The reason the ideal of value-free science generally fell into disrepute, remember, was that it could not be put to use. Even what we take to be the greatest science failed to exemplify it, and sociologists and philosophers of science assured us that most science never would exemplify it, never could exemplify it. Something similar happened for feminist scientists as well, for they found that their science, though it clearly seemed more adequate than sexist science, still fell as far short of the ideal of value-free science as that sexist science. In short, the ideal of value-free science failed to be a viable ideal, useful for actual science. Is the ideal of socially responsible science similarly inapplicable?"

"Not at all," boasts the candidate. "Unlike value-free science, socially responsible science *is* possible. Indeed, it exists. As noted at the outset, feminist scientists such as West are among the scientists who are doing it. This does not mean that all that we currently consider the greatest science *is* socially responsible science. That has to be determined on a case-by-case basis, and feminist scientists and historians and philosophers of science, among others, have already done some of this work. But it does mean that there actually are concrete models available to other scientists and science policy makers that help to show them what the ideal of socially responsible science amounts to and how it can be put into practice. And it also means that—"

Here the candidate is interrupted by you, waxing impatient: "Questions concerning values—including the 'values that meet the needs of society,' the values that the ideal of socially responsible science aims to entrench in science—are highly controversial. Even feminists, who agree on so many things, are far from agreement concerning what their egalitarian social values amount to and how they can best be put into practice—for example, exactly what a gender-equal society would be like and how it should be pursued. So isn't the ideal of socially responsible science just as inapplicable as the ideal of value-free science, since no one can agree concerning what would satisfy it, concerning which values would meet the needs of society?"

"You are not *listening*," replies the candidate. "Of course, there is disagreement concerning values, including the 'values that meet the needs of society.' But there is also crucially important agreement, especially concerning the concrete issues that affect people's day-to-day lives. Regarding West's research, for example, it is uncontroversial that women deserve to live without fear of violence from domestic partners, the value that underlies West's research. But it is equally uncontroversial that women deserve to live without fear of rape, sexual harassment, incest, and other forms of violence directed at women and that women deserve equal educational opportunities with men, equal employment opportunities with men, equal opportunities for health care, and so on. These values are not only uncontroversial in Western cultures, they are also attested to in the policy declarations and activities of such international organizations as the United Nations, the International Labour Organization, the World Health Organization, and Amnesty International. These are the kinds of shared values that motivate and inform feminist research in such fields as psychology, sociology, economics, political science, archaeology, anthropology, biology, and medical research. And this is the kind of research that exemplifies the ideal of socially responsible science."

"So your question should be," the candidate continues, "not *is* the ideal of socially responsible science applicable to real science under real (e.g., our current) social conditions but *how extensively* is this ideal applicable to this science under these conditions. That is to say, can the shared social values that shape the research of feminist scientists come to shape the research of other scientists as well, and can other social values that meet the needs of society but do not now shape research be added to them? These are large-scale empirical questions, but fortunately, there is enough empirical evidence currently available to at least begin to answer them. Certainly, the long-term flourishing of feminism in some fields (e.g., primatology, cultural anthropology, paleontology, and developmental biology) and its recent growth in others (e.g., archaeology) give cause for optimism. Primatology is a particularly good example (see, for what follows, Fedigan 2001). This field has wholeheartedly embraced feminist ways of doing research—in pursuing research that rescues female primates from their previous second-class status in the theoretical understandings of the field (as merely mothers, as merely passive resources for males), in pursuing research that answers questions of importance to women (e.g., regarding male parenting roles or the evolution of female sexuality), in pursuing research that uses new female-friendly conceptual tools (e.g., sampling methods that more readily include females), and so on. Primatology has embraced such feminist ways of doing research even though very few of its practitioners see themselves as feminists and even though the standard attitude of these practitioners is that politics does not belong in science. More significant, the reasons these practitioners give for doing so—that it makes for better *science*, that it is *scientifically* right to consider questions from a female as well as a male perspective, to research issues of concern to women as well as men, and

about females as well as males—give cause to be hopeful that further applications of the ideal of socially responsible science are possible."

"But other fields tell a different story!" you interject.

The candidate falls silent for a moment, then continues: "Other fields have made some of the same changes as primatology but only under duress. U.S. medical research is an example. Only since 1993, when Congress passed the National Institutes of Health Revitalization Act, which mandated the inclusion of women and minority men in publicly funded U.S. biomedical research and made funding contingent on that inclusion, has the neglect of females in both basic and clinical research been curtailed. Earlier initiatives, such as NIH's 1986 guidelines requiring grant applications to include female subjects in medical testing and research, were generally ignored (Rosser 1994; Schiebinger 1999). And still other fields have made few, if any, changes—economics, for example, in which women's needs and priorities in the family as well as the larger society remain invisible or inadequately treated (Nelson 1996a and 1996b; Waring 1997; Ferber and Nelson 2003; Fineman and Dougherty 2005). Do these cases show that the ideal of socially responsible science is of limited applicability? Not at all. The case of U.S. medical research shows that economic incentives—not only public funding but very possibly also tax incentives for industry-funded science and conditions on the tax-exempt status of foundation-funded science—can be a powerful method to bring about socially responsible science. The case of economics shows that the need in some areas has never been greater. And both cases show that much hard work will have to be done to determine for each of the various fields of science how best to achieve what the ideal of socially responsible science recommends. But this is the kind of work that makes sense only if, and after, the ideal of socially responsible science is adopted. For this work will answer the question, not *how extensively* is the ideal of socially responsible science applicable, but *how* can it be made *more applicable*."

Do you have any more questions? Or is the examination over? And if so, what is its outcome? Has the ideal of socially responsible science shown both that it can fulfill the epistemic and political roles of the old ideal of value-free science and that it will actually get the job done, not fail to apply itself? Has it shown, in short, that it merits the position now vacated by the ideal of value-free science? Since I have put forward this candidate (and also argued in its defense), you should already know where I stand. But you have followed the examination as closely as I, and you have posed most of the questions, so you play a role here, too. Indeed, the decision, I think, now depends on you.

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