Consensus, Dissensus, and Democracy: What Is at Stake in Feminist Science Studies?

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If feminists argue for the irreducibility of the social dimensions of science, then they ought to embrace the idea that feminist and non-feminist scientists are not in collaboration, but in fact defend *different* interests. Instead, however, contemporary feminist science studies literature argues that feminist research improves particular, existing scientific enterprises, both epistemically (truer claims) and politically (more democratic methodologies and applications). I argue that the concepts of empirical success and democracy at work in this literature from Longino (1994) and Harding (1996), to Longino (2002), Gilbert and Rader (2001), and Keller (2001) are not sufficiently critical, and fail to do justice to the truly revolutionary work done by feminist scientists. I offer the beginnings of an epistemology of dissensus (as opposed to consensus), using the work of Haraway (1978), Lyotard (1984, 1998), and Ziarek (2001). How would such an epistemology relate to feminist discussions of the possibility of democratic, responsible knowledge?

1. Introduction. The questions I will pose here are motivated by a trend in recent literature in science studies, and specifically in texts by feminist scientists and philosophers of science. I have in mind the most visible texts to appear over the past two or three years, texts like Helen Longino's *The Fate of Knowledge* (2002), Evelyn Fox Keller's *Making Sense of Life* (2002), Miriam Solomon's *Social Empiricism* (2001), and the two excellent collections, *After the Science Wars* (2001) and *Feminism in Twentieth-Century Science, Technology, and Medicine* (2001). These titles have in

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‡This article is the result of my three years of as "science" reviewer for *The Year's Work in Critical and Cultural Theory*. I am grateful to the editor, Martin McQuillan, for the opportunity. I here develop some of the ideas which originated in my contribution to volume 12 of that journal.

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common the sense that this literature is able to stand back a bit and take in some bigger picture, to take stock of a large-scale phenomenon from the outside, looking in, making this phenomenon coherent, framing it within the correct questions, offering a structural stability. The phenomenon in question is the effect of feminist theory on the production of scientific knowledge, technology and epistemology itself. Of these texts, I will look closely at *Feminism in Twentieth-Century Science, Technology, and Medicine*, because it explicitly poses the question of feminism's "success" in the sciences, from the vantage point of the turn of the century. The volume is the result of the workshop, "Science, Medicine, and Technology: What Difference Has Feminism Made?" held in October 1998 at Princeton University.

2. Same Stakes. The authors assembled in this collection agree that the effects of feminism on scientific and philosophical practices have been revolutionary, in the most positive sense of the word. This success is described (Creager, Lunbeck, Schiebinger 2001, 5) in terms of "collaboration," "contribution," and invariably in the rhetoric of rigor, reliability, and solidity, a defensive rhetoric which reminds skeptics that, in spite of its overt politics, feminist analysis also consists of "good history, sharp critical thinking, good biology, and precise use of language." In other words, skeptics need not worry that feminist contributions will in any way compromise the integrity and rigor with which scientists identify. In addition, they need not object that the role of feminism in the sciences has been purely critical. All of the essays in the volume argue for the "constructive" contributions of feminist theory, for the position that research and scholarship with a feminist edge have added something to the sciences in question, changing them by making them bigger (because more comprehensive) and better (because more accurate).

Those disciplines which have had the biggest influx of women practitioners and feminist theorists have, accordingly, benefited most from analyses which fit Longino's broad definition of feminist work as work "with a commitment 'to prevent gender from being disappeared'" (Wylie 2001, 28).[1] These are most notably primatology, developmental biology, and archaeology, because these three sciences are, in the words of Scott Gilbert and Karen Rader (2001, 92), "in the business of telling us who we are and how we came to be." They offer—or at least we fantasize that they do—histories, what are often called "natural histories," narratives of a past which is assumed to be insulated from the contingencies of modern, human history. And the latest literature defends the contributions of fem-

1. Wylie cites Longino (1994).

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inist science and feminist science studies by arguing that the work of feminists, work motivated by the commitment to prevent gender from being disappeared, has resulted in more accurate narratives of who we are and how we came to be, with more complete models, fewer gross errors, greater attention to detail, and more careful, reflexive, and responsible natural histories.

In the field of archaeology, for example, making visible the tools and practices of women, in particular, results in a richer, more detailed story about the earliest inhabitants of the Americas, and, literally, "different archaeological 'facts' established by excavation" (Wylie 2001, 31). Gender-literate research has shown "that Paleo-Indians depended on a much more diversified set of subsistence strategies than acknowledged by standard 'man-the (mammoth/bison)-hunter' models." In other cases, feminist work can act as a corrective, demonstrating that gender stereotypes have resulted in errors in identifying the sexes of skeletons or in accounts of technological developments in prehistoric societies, thus clearing the way for more reliable sexing techniques and better accounts of development (Wylie, 2001, 32–33). In the particular essay I cite here, Alison Wylie points out that in addition to these improvements in existing areas of archaeology, feminist attention to the work and lives of prehistoric women has opened up entirely new areas of inquiry, areas which "have been assumed to be archaeologically inaccessible—that is, repetitive domestic activities involving perishable materials and utilitarian tools that often leave little durable archeological record." She mentions increased interest in netting and basketry industries, domestic food processing activities, and shellfishing—all activities associated with women and sometimes children, the study of which requires "creative use of indirect methods of analysis" because they have left behind almost no artifacts (2001, 35).

Attention to gender in developmental biology has also yielded better scientific practices. Gilbert and Rader argue that feminist geneticists and embryologists have transformed the vocabulary of the field, calling for new vocabulary which is less sexist, and which thus results in "a more scientifically congruent view of the world" (2001, 92). They cite the work of Emily Martin ([1991] 1996), whose studies of literature on fertilization expose the fantasies of gender at work in the vocabulary of reproduction, including the rhetoric of waste, passivity, and failure associated with female reproductive physiology, and the rhetoric of production, agency, and virility associated with that of the male. Martin's work explores more recent research in fertilization, which highlights the active role of the egg, and emphasizes chemical interactions between egg and sperm, rather than the mechanical interactions at work in more traditional accounts, the fantasy of "may the best sperm win," and for the rhetoric of "penetration." She draws attention to the social assumptions behind biological metaphors

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and imagery and proposes that the new research requires better, more egalitarian metaphors (Martin [1991] 1996, 103–115). Evelyn Fox Keller also cites Martin as the prime example of how a critique of vocabulary and imagery can help "restore equity in the symbolic realm" and "open up new cognitive spaces" (Keller 2001, 105). In addition to critiques of language, feminist biologists have offered critiques of research programs, which have resulted in better research, most notably in the area of sex determination. Since Aristotle, we have believed that femaleness was the default state. For this reason, until very recently, researchers identified only those genes responsible for testis formation, since it was assumed that there were no genes responsible for ovary formation. When feminist biologists discovered that "sex determination is actually a bifurcating path, and both testis- and ovary-formation are active, gene-directed events," scientists looked for—and found—ovary-forming genes. That was in the 1990s, quite a while after Aristotle. Should we feel shocked at how truly innovative feminist research has been in this area, Gilbert and Rader reassure us that "numerous men are involved in these critiques, and each of these critiques has been advanced in the name of making the science more rigorous" (Gilbert and Rader 2001, 90).

Of course, it is in the field of primatology that feminist theories have enjoyed the most public success, so much so that in the collection I am discussing, Linda Fedigan (following Haraway 1978) concludes that primate studies might be considered a branch of feminist theory (Fedigan 2001, 46, 48). Here, too, feminist critique has yielded new research practices, by indicating how gender stereotypes influence sampling practices, directing everything from which species is considered most important to observe for what purposes to which individuals of that species it is appropriate to observe. Elizabeth Lloyd's essay, "Pre-theoretical Assumptions in Evolutionary Explanations of Female Sexuality," demonstrates how androcentrism has dictated which phenomena count as scientifically significant. She describes the methodological problems created by the assumption that orgasm in female primates is connected to reproduction in the same way that it is in male primates, or, as she puts it, the assumption that "female sexuality doesn't make sense unless it is in the service of reproduction." The result of this assumption is that researchers simply fail to study those orgasms which are not associated with intercourse or oestrus, because they will not help us "make sense" of the evolution of orgasmic response in female primates. Lloyd's critique shows how a male-normative attitude in primatology limits research and distorts results (Lloyd [1993] 1996, 91–102). Fedigan considers this kind of critique to be less a specifically feminist contribution, and more a general contribution to the quality of the science: raising the standards of evidence (Fedigan 2001, 51).

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As another example of feminist methodological innovation, Fedigan cites Haraway's demand that scientists begin granting agency to female bodies as part of a larger movement in primatology to grant agency to animals, rather than treating them as "a passive resource" (Fedigan 2001, 57). New methodology, in turn, yields new "facts," and Fedigan cites a body of literature where precisely this has happened: Research influenced by feminist critiques has resulted in significant changes in our beliefs about primate social behaviors, and in new models of these societies, which tend to grant more agency to females and non-alpha-males, rather than focusing on the social function of male aggression. Feminist theory has acted as a further corrective by highlighting the degree of anthropomorphic projection present in primatological description. The result, writes Fedigan, is that we are more acutely aware of the "dangers of . . . project[ing] Western gender role stereotypes onto animal patterns and onto our human ancestors" (Fedigan 2001, 53). This awareness requires analyses of how gender- and other stereotypes shape language in general, thus extending the field of primatology to include feminist discourse analysis as part of its methodology.

It appears, then, that when we take stock of the difference that feminist work has made in the sciences over the past 100 or 50 or 25 years, we can reasonably argue for its legitimacy and success: Feminist theories and critiques have contributed positively to methods and results in many areas of science, they have yielded new observations and aided in the construction of new and better natural histories, histories which we have epistemic, scientific reasons to believe are more accurate than the (let us call them) pre-feminist histories. I say that we have epistemic reasons to believe in the accuracy of these narratives because, as we have seen, feminists have worked side by side with non-feminist scientist, often using their tools or offering improvements on those tools and methods, collaborating with them in the service of 'good science' rather than social change. I am especially struck by the passage I have already cited, where Gilbert and Rader inform us that feminist critiques of research in sex determination and early brain development also include the work of men (of which Scott Gilbert, a biologist, would be an example), and that this feminist, co-educational work yields a more scientifically congruent view of the world. This passage assures us that if a critique of science is co-educational, then at the very least it cannot be accused of being politically motivated, of representing special interests. Instead, Gilbert and Rader seem to be saying, we are all in it together: a universal 'we' telling a more accurate universal history of 'us'.

3. Different Stakes. Donna Haraway warned against this as far back as 1978. In "Animal Sociology and the Body Politic, Part II: The Past Is

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the Contested Zone," she points out that "telling stories of the human past is a rule-governed activity," and a great deal of research which calls itself feminist has done little to change the rules. Instead, this research tells "of a different human nature, of different universals," but fails to "leave the traditional space of science" (reprinted in Haraway 1996, 67). This failure is significant, given that scholars in feminist science studies often tell another story, which is in direct conflict with the narrative of consensus and collaboration I describe above.

One of the most significant contributions of feminist science and feminist science studies was supposed to be an ethic of strong reflexivity, of critical attitude towards not only the object but also the subject of study, as an essential component of research. Elizabeth Lloyd cites Longino's redefinition of objectivity in science as

> resulting from the critical interaction of different groups and individuals with different social and cultural assumptions and different stakes. Under this view, the irreducibility of the social components of the scientific situation is accounted for—these social components are, in fact, an essential part of the picture of scientific practice. (Lloyd [1993] 1996, 100)

However, the notion that an objective science must be reflexive, undogmatic, and critically accessible is hardly a feminist innovation—this idea was developed explicitly, and with an explicit political agenda, in Popper's work. The *feminist* innovation is the emphasis on *dis*sensus, a term I borrow from and use in the spirit of Ewa Ziarek's *Towards an Ethics of Dissensus: Postmodernity, Feminism, and the Politics of Radical Democracy* (2001), as the alternative to epistemological models which rely fundamentally on consensus as the telos of critique. Thus, the feminist innovation is the turn to dissensus between "different groups and individuals with different social and cultural assumptions and different stakes." For Popper, on the contrary, critique is limited to critiques between scientists, or at least critiques between people with more or less the same stakes (democracy, knowledge of the truth, freedom of thought, etc.). If feminist scientists argue for the irreducibility of the differences between stakes, then they ought to embrace the idea that feminist and non-feminist scientists have *different* stakes and defend *different* interests. Indeed, Lloyd says as much when she argues that one of the positive contributions of this feminist, reflexive stance is that if we can show science to be political, it will "lose at least *some* independent authority in the political arena" (Lloyd [1993] 1996, 101). Notice that we are no longer talking about epistemic reasons for supporting feminist research, but political reasons.

If we insist on the irreducibility of normative commitments, and on

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their essential role in all scientific discourse, then we must take seriously Haraway's vision of science as a competition for power and resources between groups with different stakes and mutually exclusive interests. In several of the essays in *Simians, Cyborgs, and Women*, she insists that the most socially irresponsible thing to do is to continue to homogenize interests and discourses, and to continue to have as our goal "a picture of human universals, of human nature as the foundation of human culture" (Haraway [1978] 1996, 70). Either we are in search of theories of universal history of the universal 'we'. or we argue for the impossibility of the universal 'we' and for the particular, interested nature of all histories, especially the ones which claim to be universal.

In contrast to Haraway, Sandra Harding's account of feminist standpoint theory (or "strong objectivity"), results in conclusions which, I suggest, place her in the "universal history" camp. In order to hear the voices of the marginalized, Harding writes, "strong objectivity requires that scientists and their communities be integrated into democracy-advancing projects for scientific and epistemological reasons, as well as moral and political ones" (Harding [1993] 1996, 254). This meta-narrative of democracy is worrisome: If the only projects which count as socially and epistemically responsible are democracy-advancing projects, and we are all in agreement about what is meant by "democracy," then in what sense are we still talking about groups and individuals with different cultural assumptions and different political stakes? Harding states, without arguing for this point, that "democracy-advancing values" are not only politically desirable, but that they actually result in epistemically better sciences, having "systematically generated less partial and distorted beliefs than others" (Harding [1993] 1996, 246). Gilbert and Rader concur with this when they state that feminist critiques of research have resulted in a more scientifically congruent view of the world. If your science is democratic, it seems, it is more likely to be epistemically successful. From this perspective, of course, it makes no sense to think of feminist standpoint epistemologies as a threat to objectivity or good scientific practice. We should remember, however, the dangers of arguing for the connection between a particular political stake and scientific accuracy—the most racist, sexist, heterosexist, and otherwise "undemocratic" research will yield a scientifically congruent view of the world in the context of particular racist, sexist, and heterosexist political stakes. We should be suspicious of this language which conflates political pragmatics and epistemic success.

Thus, I suggest that from the perspective which reads Haraway's work as robustly postmodern, the recent feminist literature, which answers Harding's call for democracy, consensus, and collaboration in the service of truth, appears uncritical in some significant ways. Helen Longino's *The*

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Fate of Knowledge (2002) also de-emphasizes the differences between feminist and non-feminist stakes. Chapter Eight, "Pluralism and Local Epistemologies," argues for epistemic plurality versus unification, complexity versus simplicity, and local knowledges versus universal knowledge, using many real examples from contemporary biological research (Longino 2002, 175–202). Longino ends the chapter, however, by stating that we should consider the debates between competing interests and approaches as "critical interaction that advances all of them rather than as a duel requiring a single victor" (Longino 2002, 189). What remains to be shown, however, is the value of advancing all interests, or how that idea is particularly democratic. Why should I, for example, work towards advancing the interests of a group which benefits from my political and economic disenfranchisement? Are critiques of scientific practice not in existence because someone somewhere wants to promote some interests (say, the interests of some disenfranchised social group) and to substantially interrogate an existing authority?

Indeed, instead of emphasizing the difference between the stakes of feminist science studies and liberation epistemologies like Popper's, Longino takes the role of feminism in social studies of science to be part of the time-honored tradition of philosophers who "affirm the sociality of knowledge," citing Mill, Pierce, and Popper as examples of what she calls "predecessors" of her own project in this latest book (Longino 2002, 3). Linda Martín Alcoff makes a similar move at the opening of her essay "On Judging Epistemic Credibility: Is Social Identity Relevant?" where she places feminist philosophers in the canon of respectable philosophers like "Kant, Locke, Russell, and the Vienna Circle, who unashamedly declared and defended the political motivations of their work." Alcoff continues (2001, 53–55): "Feminist philosophy and feminist epistemology represent a continuation of the tradition in which philosophers openly avow their political aspirations." From the postmodern perspective, however, the question remains: Why should feminist epistemologies wish to belong to any existing tradition of political philosophy? What do they gain from such a belonging, and at what price? Or, to put it differently, if feminist theorists interrogate epistemology, should they not also interrogate the politics behind the tradition of politically motivated epistemologies?

Instead of linking onto concepts like political motivation, democracy, freedom, and even science, as they have been shaped historically and canonized, perhaps it is the task of feminist epistemologies to robustly interrogate these very concepts, their historical shaping, and their canonization. Such interrogations, reflexivity on this level, are what would make an epistemology feminist. Or, to put it differently, in order to prevent gender from being disappeared, feminist philosophy of science should

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return to its initial project of introducing far more than the dimension of gender to scientific discourse. Or, to put it differently again, and to speak in even broader strokes: The history of modern philosophy from Kant to the Vienna Circle will not provide the critical paradigms which feminism needs to affect the necessary transformations, because the history of modern philosophy does not offer a sufficiently critical reading of concepts like democracy, knowledge, responsibility, subjectivity, and science. On the contrary, it is modernity itself which is incommensurable with (we could even say, stands in the way of) a radical transformation of science. I thus propose that instead of linking retroactively onto the history of modern philosophy from Kant to the Vienna Circle in order to trace their philosophical lineage, feminist science studies ought to link onto the body of work which offers the most robust critiques of modernity, including not only the modern conception of science, but also of democracy, freedom, history, and of the very concept of philosophical lineage itself.

4. Towards an Epistemology of Dissensus. In "Universal History and Cultural Differences," Jean-François Lyotard describes modernity as a series of narratives (Christian, scientific, speculative, Marxist, and capitalist), which have one thing in common: "[T]hey all situate the data supplied by events within the course of a history whose end, even if it is out of reach, is called freedom" (Lyotard 1998, 315). These "narratives of emancipation," function by means of a universal 'we' moving towards freedom, and incorporating outsiders, who are particularized, by giving them access to the possibility of emancipation by means of "becoming universal": "[L]et the people have a political voice, the worker a social voice, the poor an economic voice, let the particular seize hold of the universal" (Lyotard 1998, 316).

From the perspective of Lyotard's account of the postmodern, the very possibility of this way of understanding humanity is in crisis: "[T]he real question is whether or not there is a human history" (Lyotard 1998, 321). The fantasy of the knower who is at once the hero of a liberation narrative by moving from particularity to universality ultimately suffers from a crisis of legitimation. Since a free, inclusive science depends on the idea of a fully autonomous agent involved in ethical and political decisions, the narrative of emancipation effects a division of reason into science, or cognitive or theoretical reason, on the one hand, and ethics or politics, or practical reason, on the other. This division ultimately delegitimizes the genre of discourse called science, "indirectly, by revealing that it is a language game with its own rules . . . , and that it has no special calling to supervise the game of praxis." The delegitimation of the modern notion of science opens the road "for an important current of postmodernity:

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Science plays its own game; it is incapable of legitimating other language games" (Lyotard 1984, 39–40).

In the wake of the failures of the great modern narratives, the possibility of a universal history with emancipation as its end, and the possibility of a 'we' which is the subject of that history are both fully delegitimized. According to Lyotard, politics is "the contingency of what follows" from this crisis of legitimation.

> There are several possible ways to follow on, and we [the 'we' whose very identity is in question as it faces this crisis] have to decide between them. Even if we decide nothing, we still decide. Even if we remain silent, we speak. Politics depends entirely upon how we follow on from one sentence to the next. This is not a matter of the volume of the discourse, nor of the importance of the speaker or the addressee. One of the sentences which are currently possible will become real, and the question is: Which? A description of [the crisis] does not give us even the beginnings of an answer to that question. This is why the word *postmodernity* can refer simultaneously to the most disparate prospects. (Lyotard 1998, 319, author's emphasis)

Donna Haraway's work from the late seventies signals this condition of crisis and calls for feminist science studies to exploit precisely this disparity, to realize the potential for real contest on the political level, on the level of meta-narrative. In contrast to the "consensual critique"-model of science as democratically-oriented, critical interaction which benefits everyone, Haraway proposes a "dissensual critique"-model, starting from the assumption that contest and polemic are irreducible and epistemically valuable, and indicating that dissent qua dissent does real work in the production of knowledge. For Haraway, dissent functions as an end in itself, not as a means to greater clarity and community. Unlike Longino's call for pluralism, for endlessly many knowledges, Haraway is interested in the differences between knowledges as politically and epistemically valuable. This is consistent with Miriam Solomon's claim that dissent is valuable only when "different theories have different empirical successes," but, unlike Solomon's social empiricism, the dissensual-critique model begins from the assumption that this is never not the case (Solomon 2001, 101).

From the Lyotardian perspective, when we tell a story which incorporates the work of feminist scientists into a universal history of a homogeneous 'we', with democracy and truth as coextensive stakes, we continue to ensure the erasure of gender. I suggest that feminist philosophies of science would benefit from participation in contemporary discussions of radical democracy, which offer conditions for the possibility of what Lyotard describes as the real feminist contribution, the "guerilla war of

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skirmishes and raids in a space and time other that those imposed for millennia by the masculine logos." Moving in this direction will make urgent and perhaps give a new tone to many of the questions which are currently posed only in the peripheries of science studies, questions such as: What is democracy? What are the details of the difference, if Solomon is right that there is one, between democratic science and "a democratic scientific community, which may or may not produce epistemic fairness" (Solomon 2001, 149)? If science and praxis are 'games' which cannot supervise each other, if ethics does not regulate knowledge, or vice versa, what would it mean to know responsibly? How could we imagine science as political, in Lyotard's sense, but not democratic, in Harding's sense? It is these questions with which I would like to conclude, and to indicate possible beginnings for dialogue between feminist science studies and postmodern political thought.

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