Against Epistemological Chasms: The Science Question in Feminism Revisited

The rejection of "science" by some feminists has been core to the development of alternative epistemologies, whether standpoint or postmodern (Harding 1986, 1991; Nicholson 1990), despite the argument of others (e.g., Keller 1992). Yet this rejection has often been based on caricatured or outdated conceptions of science. The insights of modern sociology and philosophy of science[1] need to be integrated in order to revise such conceptions of knowledge, though this is not to argue that there are not many problems relating to the gendering of scientific knowledge.[2] Feminist analysis should be bolder about its truth claims, rather than retreating into a defensive stance about partial knowledges. Science is not a mirror of nature (Rorty 1980), but neither is it a mirror of culture. Science is poised both in between and as a part of each of these, and there is a need for concepts and metaphors that avoid the temptation of reductionism in either direction. This article is an attempt to contribute to such an analysis.

Signs (1997) has rightly reopened the debate about standpoint epistemology, and this article seeks to build on Susan Hekman's (1997) critique while offering an alternative resolution to the dilemmas she exposes. The defensive posture of feminist analysis initially adopted in the 1970s and 1980s has become embedded within epistemologies that emphasize the difference between the knowledge derived from women's experiences and those of men, and, later, also the knowledges among women from different cultures. Standpoint epistemology is based on the presumption of a chasm between the knowledge of the oppressed and that of the oppressor, in which the oppressed develop their own practices in order to develop better knowledge. Yet contemporary philosophy and sociology of science (Quine

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[1] See, e.g., Quine 1960, 1981; Bhaskar 1979, 1989, 1997; Latour 1987, 1993.

[2] Rose 1983, 1994; Fausto-Sterling 1985; Keller 1985; Martin 1987; Jacobus, Keller, and Shuttleworth 1990; Lloyd 1993; Scheman 1993; Spanier 1995; Longino and Hammonds 1996.

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1960, 1981; Latour 1987) imply that conceptions of such epistemological chasms are unsustainable. Likewise, the alleged separateness of situated knowledges (Haraway 1988, 1989) is undermined. The concept of "community" used by some feminist philosophers (e.g., Keller 1992; Nelson 1993) to grasp the social nature of science has connotations of boundaries that are unhelpful to the development of a hegemonic feminist knowledge project. In this article I will explore an alternative way of conceptualizing the social nature of science that resists the traps of relativism, of chasms, and of retreatism into partial situated knowledges. I draw on the work of Willard Quine and Bruno Latour on the connectedness of bodies of knowledge, especially the concept of network, in order to achieve this.

Thus, from two interrelated critiques—that "science" has been set up as a straw man and that the conceptualization of the "social" nature of knowledge has been too bounded—I argue against the rejection of scientific method within women's studies and for a concept of the social that is based on networks rather than community to facilitate a more outgoing and confident feminist knowledge project.

The truth of the oppressed

Standpoint epistemology makes a claim to authoritative knowledge not through the procedures of science but through the status of the oppressed as the bearer of truer knowledge. Feminist standpoint epistemology trusts the authority of female experience, that is, the standpoint of women,[3] on the basis of two presumptions. First, it assumes that the oppressed, in this case women, can see more clearly than those who are blinded by their interests involving them in the dominant ideology (Hartsock 1985). This has parallels with a Marxist heritage, especially that articulated by Georg Lukacs (1971), in the notion of the transcendental subject, who not only can see clearly the causes of oppression but also constitutes the source of liberation. In contrast, the powerful are considered to be swept up in their own self-deluding dominant ideology. Second, it is based on the notion that direct experience is less mediated, and hence less distorted, and thus a better basis on which to build a knowledge of gender relations than knowledge that is mediated by the theories of distant men (Smith 1988, 1997; Shiva 1989). In some writings (e.g., Smith 1988, 1997), this view might be considered parallel to the notion of the significance of understanding and empathy within the sociological traditions of *verstehen* and phenomenology developed in the work of Max Weber and Alfred Schutz. Together,

[3] Stanley and Wise 1983; Hartsock 1985, 1997; Smith 1988, 1997.

these strands have facilitated the development of the preferred (at least at one historical moment) research method in feminist sociology of keeping close to the accounts of women—indeed in some versions a preference for in-depth qualitative interviewing of women over methodologies that involved abstraction, such as those utilizing theorizing or statistics.[4] At some points the critique of quantitative methods became not merely that it was inherently positivist but that, because of the absence of women's experience, it fell outside a feminist methodology, although eventually this view has given way to a greater pluralism.[5]

Sandra Harding (1986, 1991, 1993) has created the most authoritative set of distinctions within the field of feminist epistemology. While sensitive to the strengths as well as weaknesses of each of the three different epistemologies that she identifies, she favors standpoint epistemology, arguing that when there is a conflict between two knowledges, the one of the oppressed is to be preferred: "When people speak from the opposite sides of power relations, the perspective from the lives of the less powerful can provide a more objective view than the perspective from the lives of the more powerful" (Harding 1991, 269–71). Similarly, Vandana Shiva has argued from an ecofeminist viewpoint that "Third World women have privileged access to survival expertise" (1989, 224). More recently, in debate with Hekman, Patricia Hill Collins has defended feminist standpoint epistemology in terms of the priority of issues of power and politics over claims to truth: "The amount of privilege granted to a particular standpoint lies less in its internal criteria in being truthful . . . and more in the power of a group in making its standpoint prevail over other equally plausible perspectives" (1997, 380). "Fundamentally," Hartsock asserts, "I argue that the criteria for privileging some knowledges over others are ethical and political rather than purely 'epistemological'" (Hartsock 1997, 372).

In recognition of the diversity of women's lives there has been a tendency for standpoint epistemology and postmodernist epistemology to be drawn closer together to a position in which many knowledges are situated in relation to specific social groups or positions (Haraway 1989; Nicholson 1990; Harding 1991). This shift has been made primarily to accommodate the issue of difference, since it is thought that there cannot be one standpoint when women are so divided by phenomena such as ethnicity, "race," class, sexual orientation, generation, and physical capacity (Harding 1986; Haraway 1989; Collins 1995).

This epistemological stance requires the identification of the communi-

[4] Oakley 1981; Graham 1983; Reinharz 1993; Maynard 1994.

[5] Smith 1988; Kelly, Burton, and Regan 1994; Maynard 1994.

ties of the oppressed; thus there has been a multiplication of standpoints based on the recognition of different communities. For instance, Collins provides an account of knowledge based on the experiences of African-American women. "Living life as an African-American woman is a necessary prerequisite for producing Black feminist thought because within blackwomen's communities thought is validated and produced with reference to a particular set of historical, material, and epistemological conditions" (Collins 1995, 539).

Donna Haraway is perhaps the most influential writer who has attempted this synthesis of postmodern feminist epistemology with standpoint epistemology by utilizing multiple standpoints. Haraway advocates situated knowledges in her development of the issue of specific standpoints: "Feminist objectivity is about limited location and situated knowledge" (1988, 583). Haraway describes herself as situated within and between four positions: first, what she considers to be Latour's thoroughgoing social constructionist analysis of scientific practice; second, standpoint theory; third, the voices of scientists themselves, though their "realism" is regarded as merely an "aesthetic"; and, fourth, an interpretation based on the theories and politics of gender and race (1989, 8). Haraway deploys the techniques of literary analysis to the texts produced by science, while not denying the rule-bound methods and disciplines that produced the texts. Haraway describes and advocates her form of knowledge as a story. It is not relativism, in that there are criteria to decide between rival knowledges, but these criteria are aesthetics and values, rather than truth claims: "I would suggest that the concept of constrained and contested story-telling allows an appreciation of the social construction of science. . . . Story-telling is a serious concept, but one happily without the power to claim unique or closed readings. . . . The aesthetic and ethic latent in the examination of story-telling might be pleasure and responsibility in the weaving of tales" (1989, 8). Such themes are continued in her later work on technoscience (1997), which also deploys a literary mode of analysis.

The initial development of the project of feminist standpoint epistemology took place in a very specific historical context. It was intended to help create an intellectual space for feminist analysis in what was seen as an inhospitable, even hostile, environment in the 1960s and 1970s. It was related to the development of the women's liberation movement at that time and, in particular, to the use of consciousness-raising groups as a political/intellectual mode of feminist knowledge/power creation and development. A powerful feminist movement had developed in the context of male dominance of knowledge-creating institutions (universities, medicine, psychia-

try, churches), a dominance not merely in their gender composition but of their intellectual agendas. How could feminists respect "science" when the bodies of scientific authority tried to pass off as scientifically established knowledge such bizarre male fantasies as Freud's conception of female sexuality (Freud 1977), the notion that rape and domestic violence were the rare products of a handful of sick men (West, Roy, and Nichols 1978), and the claim that women in the labor market were paid what they were worth (Mincer and Polachek 1974)? In the context of an absence of institutionalized intellectualized feminist traditions, consciousness-raising groups were developed to create oppositional knowledge based on experience, which was seen as the only, or perhaps the least patriarchally contaminated, knowledge resource available. Within this specific feminist practice there was the institutionalization of the lack of trust in the dominant knowledge industries, such as universities, which were seen as the creatures and products of the dominant groups. It is perhaps not surprising that a theory of knowledge that privileged experience and rejected theoretical and technical mediation developed in this climate during the 1960s and 1970s or that it has endured, even though by the 1980s and 1990s feminist beachheads have been established in the academy.

Yet are ethics, politics, and aesthetics adequate criteria for the evaluation of knowledge? Is "story telling" really the best that feminist social science can offer? I argue that the feminist retreat from modernism, rationality, and science is mistaken.

Science as straw man

Feminist standpoint epistemology and postmodern epistemology often rest on a rejection of "science" and "modernist" modes of reasoning as adequate or sufficient for feminist analysis (Harding 1986, 1991; Nicholson 1990). This rejection is not justified. The account of science in such writings is often oversimplified. First, science is equated with empiricism, which is then falsely conflated with positivism (Harding 1991), to the neglect of the sophisticated and diverse role and nature of reflexive theorization. Second, science is described as monolithic (Haraway 1988), when it is actually internally divided, full of contestation, and subject to change as a result of challenges. Third, science is caricatured as absolutist, as claiming to have discovered the truth about nature and society, despite its internal debates and its continual replacement of old theories with new. Contemporary sociology and philosophy of science undermine these accounts of science as monolithic and absolutist (Quine 1960; Latour 1987, 1993). Fourth, modernist modes of reasoning are often smuggled in unrecognized

through the back door (McLennan 1995), since they are actually indispensable for argumentation and in order to avoid the problem of relativism.

Empiricist?

Harding's (1986, 1991) conflation of the concepts of science and empiricism has led to an unfortunate presumption that a science-based epistemology does not include a significant place for theorization, even though Harding does sometimes appear ambivalent on this point. This may not be a necessary consequence of the use of the term *empiricism,* but it has nonetheless become presumed within feminist epistemology, at least partially because of the way Harding establishes her authoritative categories. Thus it becomes necessary here to spell out the place of theory in conceptions of science, given this current restrictive set of connotations of the term *empiricism.*

Harding (1991, 79, 110) alleges that science is not only empiricist but also mostly positivist. She cites as support Roy Bhaskar's (1989, 64) suggestion that positivism is the unreflective or spontaneous consciousness of science, although Bhaskar did not carry out studies of scientists himself. She suggests that empiricism involves seeing observational data as the primary and direct basis of our knowledge about the world and its laws of operation, with only a small involvement of theorization, although there is a certain ambiguity and ambivalence in her account of this: "In this sense, empiricism defends experience rather than ideas as the source of knowledge and is thus contrasted with rationalism. Few contemporary scientists or philosophers would want to give such a small role to reason as this definition implies—no one is an empiricist of that sort today—yet many natural and social scientists insist on such remnants of empiricist philosophy as the primacy of observations and 'pure data' and the necessity of knowing that one has the facts" (Harding 1991, 112).

While Harding here acknowledges the role of theory and argument in science, she suggests that this is not the practice of contemporary science. She alleges that the confusion lies with the scientists themselves, not those who give accounts of science:

> Scientists sometimes confuse that philosophy of science called "empiricism" with the idea that it is a good thing to collect evidence about the empirical world. All philosophies of science recommend the latter. Empiricism is that account of such practices associated paradigmatically with Locke, Berkeley and Hume and claiming that only sensory experience is the only or fundamental source of knowledge. It contrasts with theological accounts that were characteristic of Eu-

> ropean science of the Middle Ages, with rationalism, and with Marxist philosophy of science. However, from the perspective of standpoint theory, it also shares key features with one or another of these three philosophies. For example it borrows the monologic voice that seems proper if one assumes the necessity of a unitary and coherent subject of knowledge, as do all three. (Harding 1993, 76)

Feminist empiricists, Harding suggests, turn to improved and more rigorous empirical research in order to overcome the biases of androcentric values (1991, 111–12). And, according to Harding, they fail because "thought that begins from the lives of the oppressed has no chance to get its critical questions voiced or heard within such an empiricist conception of the way to produce knowledge" (1993, 56). However, Harding's account, which reduces science to a primary focus on collecting sensory data, is unduly narrow and restrictive. There are problems in its accounts of the philosophy of science, in its representation of empiricism, and in understanding how scientists proceed in practice.

Harding underestimates the range of activity within science when she describes scientific practice as primarily one of data collection, devoid of theorization. Rather, the discovery of connections and the development of concepts are core to scientific practice. Harding inappropriately conflates the practical belief of some practicing scientists that there is something out there to be discovered with a naiveté about theoretical and conceptual debate. Further, she is incorrect to reduce empiricism to positivism. It is not disputed that the case against simple positivism has been made many times: all observations are theory laden, and so contain presuppositions that themselves need investigation in a chain or net of investigations that constantly tests and retests theories that can be falsified only in a provisional manner (Hempel 1966; Pawson 1989). But positivism and empiricism should not be conflated. Positivism is long surpassed in the social sciences, as has been argued thoroughly in the philosophy and sociology of knowledge (Tudor 1982; Hage and Meeker 1988). For instance, in social statistics it is regarded as elementary that a correlation is not the same thing as a cause but merely a piece of empirical data that can be used within an argument. Empiricism as a philosophy of science has developed a long way beyond positivism. While observation is an irreducible input in scientific knowledge, its relationships with other bodies of knowledge, with theory and with common sense, underpin the more sophisticated empiricism of Quine (1960, 1981), thus taking account of the social processes and context of science.

The existence of a nonpositivist science is not provided for within

Harding's trilogy. Yet there is a significant tradition of philosophical and sociological work in this area, some of which addresses gender, and some of which does not.[6] Bhaskar, for example, has long argued for a philosophy of the social sciences that takes account of theory, of unseen social structures, and holds a view that there is something out there to be discovered. But, unfortunately, Bhaskar's realist philosophy is little discussed by Harding (apart from references to his view that scientists are positivists). Yet it might be argued that realism is precisely an example of a theory of knowledge that has been developed to take forward these issues of theory and data in the analysis of the social. His philosophy is now widely used, especially in philosophy of social science in Britain.[7] It is an epistemology that presumes that there is a world out there—one that can never be fully known but that can be approached by a mix of empirical research and theory building. It presumes that explanations do not reside in the surface observations that can be made, in the empirical data that can be gathered, but can be constructed on the basis of theory building toward which such data make a necessary but insufficient contribution. Explanations need more than data gathering and must account for the relationships between unobservable social structures. Thus realism presumes a key, though not determining, role for theory.

One of the consequences of Harding's tendency to reduce science to an empiricism devoid of theorization is to drive readers toward one of the other two options she offers. But modern philosophy and sociology of science show that it is not accurate or fair to reduce science to an empiricism that is devoid of theorization and connection to other bodies of knowledge. The practice and the philosophy are more sophisticated. Science should not be so quickly dismissed. Realism is a fourth alternative to Harding's trilogy, an alternative to the early naive empiricist positivism that does not require a leap to either standpoint or postmodern epistemologies.

Monolithic?

Much of the feminist critique of science rests on a view that it is a unified, cohesive body of authoritative knowledge that inappropriately assumes the status of truth. Haraway (1988, 584) alleges that science is a monolithic arrogant body that does a "god trick." This phrase is often repeated approv-

[6] Those that address gender include Longino 1990; Nelson 1990; Keller 1992; Lloyd 1993; Spanier 1995. Those that do not include Keat and Urry 1975; Benton 1977; Bhaskar 1979, 1989, 1997; Tudor 1982; Hage and Meeker 1988; Layder 1990; Sayer 1992.

[7] Keat and Urry 1975; Benton 1977; Bhaskar 1979, 1997; Walby 1986, 1990, 1997, 2001; Sayer 1992; Archer 1995.

ingly in writings on feminist epistemology, and in the repetition it becomes a caricature.

Scientists are accused of making false claims to truth, of misplaced confidence in science's knowledge production—in short, of hubris (Harding 1986). They are accused of making claims to universal truth that cannot be justified and of lacking in the humility that is presumed more appropriate to partial knowledge. Thus, a "god trick" within contemporary science is both presumed to be the norm and simultaneously condemned (Haraway 1988).

Modern scholarship within the sociology of science has shown that science-in-the-making is based on constant questioning and internal critique, with knowledge claims contested—always considered provisional—and "facts" constantly being created.[8] Studies of laboratory life show that actual scientific practice is continually riddled with conflict and negotiation, contestation and compromise, argument and disagreement (Latour and Woolgar 1979).

It is useful to make a distinction between the period of science-in-the-making and the stage when results are considered established (Latour and Woolgar 1979; Latour 1987). In the former there is extensive disputation. In the latter, the findings are treated as "facts" and the prior history of contestation is not held out for public display. The final "inscriptions," or texts such as academic articles, that enable the results of laboratory science to be communicated more widely rest on this prior process of contestation, even if the transformation of provisional claims into widely accepted, stable "facts" demands that the prior history of contestation is erased. Thus any simple dismissal of empirically based science founded on science's monolithic nature and arrogant truth claims should be rejected as misrepresenting the processes involved. At best, it misunderstands the final stage of consolidation of knowledge as if it were the whole of science, while neglecting the prior processes of science-in-the-making.

Indeed, Haraway (1989, 1997), in her detailed empirical work on the history of primatology and contemporary technoscience, is well aware of the controversy and dispute underlying the changing content of knowledge. However, these detailed accounts do not circulate as widely as her more polemical summary statements, and these are not a widely cited part of her work. Rather, there is approving replication of the quote on the "god trick" as characterizing science. The dominant reading of Haraway is based on a neglect of her in-depth analysis of scientific controversy and based on her shorter writings that refer to science as authoritative (1990).

[8] Latour and Woolgar 1979; Lyotard 1984; Law 1991a, 1991b; Pickering 1992, 1995.

The significance of such divisions and contestations within science is that there is not the permanent and irrevocable closure against women that standpoint theory implies. Women are not just the passive victims of a monolithic science. Rather, there are spaces and places where feminists have joined the arguments and won.

Two examples of changes in understanding of forms of gender inequality as a result of feminist research are in the areas of male violence against women and the inequality of men's and women's wages. Feminist researchers changed the view that male violence against women is rare to one in which it was recognized as a widespread social problem. Before feminist social scientists engaged with the field, the dominant conception of male violence against women was that it was the rare product of a few mentally ill men (West, Roy, and Nichols 1978) about which little needed to be done. Rigorous feminist research demonstrated that male violence was too widespread to be explained in such a way (Russell 1982, 1984; Johnson 1996), that it had a devastating effect on women who were subjected to it (Kelly 1988), that it restricted the movements of the majority of women in their search for safety (Johnson 1996), and that the criminal justice system was culpable in its treatment of the issue (Adler 1987). Today it is not only an issue for feminists in a handful of Western countries but is regarded as a global issue at the level of the United Nations, having been placed on the international human-rights agenda. Second, research led by feminists refuted the claim that the gap in wages between women and men was just reward for women since they had not invested in their education and employment because of their commitment to the family (Mincer and Polachek 1974) and showed that it was partly the result of deeply sedimented discriminatory practices against women, such as occupational segregation (Hartmann 1976; Treiman and Hartmann 1981). Feminist scholarship made most impact when it involved the building of links with mainstream bodies, such as, in the case of male violence, Statistics Canada (Johnson 1996), and in the case of the wages gap, the National Research Council (Treiman and Hartmann 1981).

"Science" is not a unified patriarchal whole. Controversy is the norm in science-in-the-making. Feminists have joined the argument and sometimes have won—and, in so doing, they have transformed the field of inquiry.

Smuggled-in modernist assumptions

In practice, many critics of science and modernism actually utilize modernist methods in their argumentation. Even as they condemn "science," they actually utilize core aspects of its methods. They retain the notion that it is possible to evaluate forms of knowledge and that, as a result of either

empirical investigation or greater theoretical coherence, theirs is superior to those they attack. This is done in order to avoid a slide into relativism that can otherwise result. Modernist conceptions of knowledge improvement and the criteria by which it can be done are necessary to avoid this trap.

Relativism, of course, has to be avoided since it is internally contradictory in that it undercuts its own epistemological position, removing any vantage point from which to argue the superiority of its own case (Collins and Yearley 1992; Pickering 1992). Any writer who argues for relativism simply has no position from which to assert that her argument is right and that the opposite to relativism is wrong. Evelyn Fox Keller (1992) has rightly argued that relativism is extremely problematic as a feminist project. In practice, most writers retreat from the brink.

Harding's goal is to create knowledge about women that is less "distorted" (1991, 105). This is an argument for better, not merely different, knowledge. This is not just differently situated knowledge but knowledge that derives its status from evaluation against a more general standard. Knowledge closer to women is better, not in an essentialist way because it is closer to women, but rather because this positioning has the effect of making it less distorted. That it is less distorted is conceptually independent of being close to women, except that the latter is alleged to be the route to getting there. Harding's basic concept and goal of "less distorted" knowledge, then, is consistent with modernist and scientific methods, even if her route to getting there is not.

Shiva (1989) rejects science that is associated with Western technology and development. This kind of science is considered to be at the root of the problems of maldevelopment and the ensuing ecological crisis in the third world. She argues that the knowledge of ordinary women is to be preferred to that of Western scientists. Her explicit arguments for the "recovery of the feminine principle" and the need to include the expertise of women appear to locate her work within a standpoint epistemology (1989, 14). However, third-world women's knowledge is preferred not simply because it is derived from women as some politically or ethically preferred category but rather because third-world women have greater expertise, which makes their inclusion the route to more accurate knowledge. This route is more effective because these women have greater access to empirical data about detailed aspects of ecological matters on the ground as a result of their greater number and range of observations over a longer period than those "scientists" who work in distant laboratories and have less access to the relevant empirical data. They know more about the range of actual conditions, which those in distant laboratories find difficult to

replicate accurately. Shiva is not rejecting empirical data gathering; she is embracing it and arguing for better ways to do it. Neither is she arguing against theorizing on the basis of these data. In short, Shiva is arguing for better science, even though her text appears to be an argument for standpoint epistemology or for a situated knowledge. She holds firmly to the criteria of evaluation that lie at the heart of a modernist approach and within scientific method.

Likewise, Linda Nicholson (1990), Jane Flax (1990), and Susan Hekman (1990) all start with denunciations of modernists yet end up reclaiming selected parts through the back door in order to retain a platform on which to prioritize feminist goals over others or one knowledge claim over another (see also McLennan 1995). The return of the repressed claim to truth takes many forms. Thus in all of these positions, there is, despite a critique of science, a hesitation at embracing a full-blown relativism and a cautious endorsement of the project of seeking to cumulatively improve knowledge.

Going beyond seeing science as the mirror of either nature or culture

Where next? Feminist analysis needs a theory of science that avoids reducing it to either a mirror of nature or a mirror of culture: an approach that retains conceptions of rationality and reason at its center, even if these are seen as plural, that can make greater claims to truth than the relativistic conception of "story telling," and that can analyze the errors of science in relation to gender.

Latour's work can be fruitfully adapted to this purpose, even though it does not engage with issues of gender.[9] While Latour conceptualizes science as a social practice, he does not neglect the significance of its procedures; thus he argues for the erasure of the boundary between science and society. While reducing any special mystique of science, his approach endorses the power of various forms of rationality and reasoning across the ostensible science/nonscience divide.

Erasing this divide can help ensure that the nature of argumentation in science is not "reduced" to that of nonscience and then to politics and power but rather allows the clarity to see the multiplicity of ways people practice rationality in their lives, as best they can, whether they are in the knowledge industries or not. This is an upgrading of the conception and understanding of rationality to a wider public, not the diminution of the one space previously held as its special repository, science.

This insight can be found buried in many accounts but is then dis-

[9] Latour and Woolgar 1979; Latour 1987, 1988, 1993.

counted and lost from the main analysis. Indeed, Richard Rorty suggests that ultimately even Kuhn shares a standard list of procedures of rationality that cross the alleged science/nonscience boundary: "accuracy, consistency, scope, simplicity, and fruitfulness" (Rorty 1980, 327). He continues, "We might be tempted to say that it would be 'unscientific' to permit any values save these to influence our choice" (1980, 327). While it is possible to argue over the extensiveness of such a list, perhaps noting that more agreement could be found if it were restricted to "accuracy, consistency, and fruitfulness," nonetheless there are generally accepted procedures of rationality. Such conceptions of rationality crossing over the alleged science/nonscience domains can be found explicitly defended in the work of Latour. I would argue that it should be applied to feminist epistemology also.

This revised conception of science places at its heart a process of argumentation and reasoning within a set of agreed procedures of validation. This has similarities with the work of Jürgen Habermas (1989, 1991), whose theory of communicative action also captures the significance of argumentation and the power of reasoning in the development and improvement of knowledge under specific conditions of human interaction. Habermas thus sees the possibility of reason that is not reduced to power. This is echoed in the work of Helen Longino (1990), who also recommends the recognition of a set of specified procedures through which reasoned debate can take place, including practical equality for those who participate in it.

However, while Habermas tends to see a search for consensus, I think this underplays the significance of the role of disagreements. Like Lyotard (1984), I would argue against Habermas's (1989) idea that the route to truth is through a striving for consensus, in the goal of perfect communication. Rather, the practice and source of legitimation of science in our era is its ability to produce that which is new and hence necessarily involves change and contestation, rather than its contribution to an old metanarrative of the progress of humanity (Lyotard 1984).

Metaphors of the social

Knowledge is socially created and how we assess it depends on socially agreed-upon criteria. This insight is common to most contemporary sociology and philosophy of knowledge, whether feminist or not;[10] notions that knowledge can be based on an individual knower have long been surpassed. The utilization of different conceptualizations of the social is the

[10] Kuhn 1970, 1977; Foucault 1972, 1979; Longino 1990; Nelson 1990, 1993.

issue here, not whether knowledge is social. It is how they conceive of the nature of the social that differentiates contemporary positions. How should the social be appropriately recognized?

The second major point of this article is that the alleged separateness of communities of knowers that underpins standpoint epistemologies is mistaken. I think that the boundaries drawn between different knowledges are made too firmly as a result of a conception of the social that implies rigid boundaries. The social is often operationalized via the concept of community. For instance, Haraway suggests that "situated knowledges are about communities not about isolated individuals" (1988, 590), while Lynn Nelson (1993) suggests the notion of "epistemological communities." But "community" is only one form of the "social," and one that is special rather than general: It implies boundaries and divisions between us and them, and it leads thinking about the social in too narrow and bounded a direction.

Standpoint epistemology depends for its claims on chasms between the knowledge of the oppressed and that of the oppressor (Collins 1995, 1997). Haraway's account of partial and situated knowledges likewise presumes separation between these differently located knowledges. This notion of epistemological chasms is paralleled in the notion of irreconcilable sociological perspectives, and it is fundamental to notions of incomparable cultural difference. Such a view is not unique to feminist analysis, and indeed a popular reading of Kuhn would appear to support such a claim. Kuhn has been a key influence on the wider development of the conception of knowledge as based on bounded social communities. He argued that scientific knowledge is generated in scientific communities, and that the criteria as to what passes as scientific knowledge are held by these communities and communicated to new members through scientific education and practice (1970). Scientific revolutions involving changes in paradigm involved contestations between these scientific communities. However, it can be argued that this is to misinterpret Kuhn and that he only thought there was incommensurability at the point of contestation over paradigm change (1977). Nevertheless, even if inaccurate, the view that Kuhn held that knowledge was based in incomparable paradigms rooted in distinct scientific communities has become widespread.

This concept of community has been adopted by several feminist philosophers of science.[11] Nelson (1990), like many other social scientists, understands Kuhn to be arguing for the incommensurability of paradigms. She develops the concept of epistemological communities in order to oper-

[11] Haraway 1988; Longino 1990, 1993; Nelson 1990, 1993; Keller 1992.

ationalize her notion of the social location of knowledge—that there is a community that sets the standards and criteria by which knowledge becomes knowledge or science not mere belief (1993).

But the concept of community is a problematic way to express the social nature of scientific endeavor. *Community* is commonly used to express the social, especially specifically located social phenomena. However, the term comes with a series of connotations that are less than helpful in understanding the social nature of science. The concept of community holds strong connotations of internal consensus, of shared norms and values that are bounded and separate from other communities. It implies a social entity that is relatively fixed, static, and homogenous. Communities are special forms of the social and should not be equated with the social. It is a problematic concept because it is makes it difficult to recognize multiple and overlapping forms of social divisions and social inequalities; it does not lend itself easily to conceptualizing change and fluidity; and it emphasises chasm over connectedness.

The importance of difference has been central to much recent feminist theory (Felski 1997). We have seen the development of concepts that link knowledge and the social in order to attempt to grasp this adequately—for instance, the concept of "situated knowledges" (Haraway 1988) or "location" (Braidotti 1994). These metaphors draw on spatial analogy, but they do not actually fully grasp the issue of difference, since they still imply specific fixed points, a range of homogeneous places. Even Braidotti's heroic attempts to move beyond stasis to "nomadism" do not avoid this problem, since it simply provides multiple points among which we move. I think the problem is that these terms are too close to the concept of community and are still rooted in the more static concept of identity, even as they try to capture a more fluid conception of difference. There is a need for a broader concept of the social in the analysis of science and knowledge that would more adequately capture the diversity and fluidity of the social together with deeply sedimented relations of inequality and that would enable an avoidance of the chasms implied by bounded conceptions of community.

The connectedness of knowledge is strongly argued by Quine (1960, 1981), who offers a holistic conception of knowledge in which different kinds of knowledge are ultimately linked. Quine does not hold that different knowledge systems are incommensurable (Nelson 1990): "Rival systems of analytical hypotheses can conform to all speech depositions within each of the languages concerned and yet dictate, in countless cases, utterly disparate translations, not mere mutual paraphrases, but translations each of which would be excluded by the other system of translation" (1960,

73). This is an argument, widely noted but insufficiently acted upon, about the indeterminacy of translation, the underdetermination of theory by evidence, and the empirical slack in theory formation. It is key to the defeat of the notion that observation plus logical deduction is sufficient for theory development. But it also implies that "a statement about the world does not . . . usually have a separate field of empirical consequences it can call its own" (Gibson 1982, 3). This means that theories are not incommensurable, since they share empirical fields. Ultimately, there are connections between theories, based on shared observations, even if these are initially interpreted or translated within different frames of reference. Thus, paradigms or epistemologies are not separated by chasms, and evidence can be used to compare theories (Nelson 1990). Rather than paradigm revolutions, Quinean philosophy underpins conceptions of gradualism in theory change. For instance, to explain the sharing of empirical fields, Quine uses Neurath's metaphor of a boat that is kept afloat but that undergoes constant revision. Quinean philosophy entails a radical lack of separation between scientific and nonscientific modes of rationality. It implies a shared set of rules for the improvement of knowledge across all domains, and it advocates a search for coherence, a search for simplicity of explanation. This epistemology is based on an underlying conception of human rationality, which underpins Quine's spirited defense of the human capacity for holistic reasoning and rejection of relativism:

> Have we now so lowered our sights as to settle for a relativistic doctrine of truth—rating the statements of each theory as true for that theory, and brooking no higher criticism?—Not so. The saving consideration is that we continue to take seriously our own particular world-theory or loose total fabric of quasi-theories, whatever it may be. Unlike Descartes, we own and use our beliefs of the moment, even in the midst of philosophizing, until by what is vaguely called scientific method we change them here and there for the better. Within our own total evolving doctrine, we can judge truth as earnestly and absolutely as can be; subject to correction, but that goes without saying. (Quine 1960, 24–25)

Despite these arguments, Quine does not go on to develop a concept of the social to underpin his epistemology, instead preferring a more behaviorist model of the knowing subject. However, Nelson (1990, 1993), who has been one of the main advocates of the relevance of Quine for feminist philosophers, tries to develop his work using the Kuhnian concept of community. But, given the critical discussion above, is the concept of commu-

nity really the best way to operationalize the concept of the social sought by Nelson?

So what would be a better way of conceptualizing the social in these analyses of knowledge? One possibility is that of "network." Network is a better concept or metaphor than community for thinking the nature of the social processes involved. Networks have to be built and constantly revitalized. They have nodes. They are flexible. They are in dynamic equilibrium. They engage our notions of structure and action simultaneously. They can be of any size. They have no limits. They are of varying degrees of strength and durability. They link via other networks to an even wider set of networks. They do not draw on the notion of inherent essential identity. And while they allow for difference and unevenness, they do not imply chasms.

This concept of networks is developed primarily from a reinterpretation and application of the work of Latour. The concept of network is a way of viewing social connectedness while not reducing it to fixed structures or holistic communities. This use of the concept of network can also be found in several recent attempts to deal with the connections between society and science and between society and technology. Manuel Castells (1996, 1997, 1998), for instance, uses it in order to grasp the impact of the new information technologies on human capacity for communication. The concept of network is the central metaphor of the social used to facilitate the understanding of the changed forms of interconnection between people as a result of technological innovations. Society is not homogenized as a result of these new global information technologies, but its networked interconnections make new forms of social arrangements more likely. The concept or metaphor of network in theories of science and knowledge is most developed in the work of Latour (1987, 1991) and Michel Callon (1991). Latour argues that new knowledge projects require the linking of various diverse social groups and objects for the knowledge to become counted as science. For instance, by following scientists in action Latour (1987) shows how Pasteur was implicated in a variety of networks that led to the establishment of his theory of microbes as authoritative. A whole series of entities contributed in quite different ways to the politico-scientific movement involving Pasteur's theories of microbes, including hygienists, biologists, sanitary engineers, and medical doctors. Latour is able to show how quite diverse and distinct groups become implicated within a network and contribute to the development of a new environment. Pasteur was able to change the face of modern medicine, hygiene, and food processing not simply by the power of his laboratory science but because of a wider network of scientists and others. If he had

stayed within the boundaries of his laboratory and his specific scientific community, he would have had little effect on the world; it was the networked linkages that enabled his profound influence.

Likewise, it might be argued that feminist research needs not only to build its own theories but also to reach out to wider scientific and other networks. It needs vocabularies, theories, and epistemologies that enable networked linkages. But is the concept of network sufficient to grasp the nature of the social? The metaphor is good for many purposes, especially to remind researchers of the social links among inquirers that make scientific inquiry most successful. But it does not follow that it is good for all purposes, when other conceptualizations of the social might be more appropriate. However, it has many advantages over the concept of "community."

Conclusion

Whom is the knowledge created by women's studies for? Is it really only for one of various "communities" of women, divided by epistemological chasms? I argue that it does have a wider relevance. Indeed, if women's studies is to have the capacity to develop knowledge that will be regarded as relevant by wider agents of change, it must utilize procedures that are widely considered authoritative. Otherwise, the knowledge generated by all this feminist research will have little purchase on the world. It will not have the possibility to gain the authority to change existing forms of knowledge. Confinement to specific communities is not a route to change. Network is better than community as a way to conceptualize the social in relation to the development of such knowledge.

There have been many critiques of modernism and of science.[12] However, not only are these usually oversimplified arguments in which modernist assumptions are smuggled in through the back door in order to sustain the argument (McLennan 1995), they are also undermined by contemporary philosophy and sociology of science. The philosophy and sociology of science has long critiqued any absolutist claims to truth,[13] and no practicing scientist would make such an absolutist claim. Moreover, the consequences of the dismantling of the imagined boundary between the forms of rationality in science and nonscience have not been fully worked through, especially in feminist theories of knowledge. I have argued against epistemological chasms and incomparable cultural values using the

[12] For example, Harding 1986, 1991; Nicholson 1990; Hekman 1997.

[13] Keat and Urry 1975; Benton 1977; Latour and Woolgar 1979; Tudor 1982.

insights of a philosopher of knowledge, Quine, and of a sociologist of science, Latour. I hope to reinvigorate the project of rationality and systematic knowledge cumulation and improvement within women's studies. I seek to reject false and outdated epistemological strictures that put limits on the generalization of such projects. This is not a return to positivism but an attempt to build on the power of doubt within a social scientific enterprise to stimulate confidence in the possibility of knowledge improvement, rather than retreatism into partial and localized knowledges separated by chasms of epistemology and cultural values. It is an argument for rigorous methodology for feminist questions and an argument that feminist analysis can and should claim that it generates the best available knowledge, while rejecting the two extreme poles of absolutism (or perfect knowledge) and relativism.

By this approach I mean a commitment to a realist methodology, developing theories and methods that involve observations predicated on the assumption that there is a world out there that ultimately acts as a check, as a form of resistance, to the development of theory (though, of course, one that can never be known absolutely) (Sayer 1992). This approach is committed to theorization, acknowledging that observations will always be theory laden. Observations are not presumed to have a direct relationship to explanation; rather, causation can only be investigated with a theory, as well as with data (Pawson 1989; Sayer 1992).

I am arguing that it is a mistake to read current philosophy and sociology of science as defenders of relativism. Rather, they provide new and different conceptions of the process of knowledge development. These arguments against epistemological chasms, and for realism, are not meant to imply that power is not relevant to the knowledge available about women in contemporary society (see Longino and Hammonds 1990). Nor is it meant to imply that feminist research findings are not distorted in the media (Faludi 1991) or that the absence of women from senior positions in universities and funding boards has no implications for the resources available for research on gender questions. But it does mean that retreatism to merely partial knowledges or to relativism are the wrong responses.

Feminist scholarship can and does challenge traditional orthodoxies, leading in some instances to new opportunities for some women to live fuller lives. It is most successful in affecting public policies not when it claims a special status but when it claims to have better and more authoritative knowledge than any other. It is most effective when it utilizes an epistemology that links to wider knowledge networks rather than to more specific communities.

These arguments are not meant to imply that there is no field or

discipline of women's studies. The field is defined by the questions of study—the analysis of the differences and inequalities in the construction of gender—and has many common theoretical and substantive debates that cross-cut conventional disciplinary boundaries. There are many questions pertaining to gender relations that have been neglected by the mainstream disciplines. These need addressing. But women's studies as a field does not need its own specialist epistemology in order to defend its place in the academy.

The conclusion that there are no epistemological chasms means that there is no justification for the separation of bodies of knowledge on the basis of socially divided groups. Standpoint epistemology falls with this. This is because conceptions of epistemological chasms are necessary for feminist standpoint theory, since without this theories can address each other and evidence can be used to evaluate rival theories.

While science is not a mirror of nature, neither is it a mirror of culture. We should not simply move from one extremist pole to another, from an absolutist conception of science to a relativist one. Feminist epistemology should learn from the new postrelativist sociology of science that our choices are not so limited. The power of doubt and of argument should not be underestimated as routes to the improvement of knowledge.

There is no need to accept a special or marginal status for knowledge about gender inequality. Why should feminist researchers not claim as much scientific status as any other form of systematic knowledge? Why should such researchers not declare their work to be the best knowledge that there is, although always subject to revision? Why admit any caveat that it is restricted to a certain category of knower? Any caveat will be used in the broader world to downgrade the relevance of the research findings. Any such caveat is now inappropriate and can be a hindrance to the creation of a feminist scholarship that can affect public policies that affect women's lives. Examples of this include research on the extent and nature of male violence against women supporting the change in policing of rape and domestic violence, the nature of discriminatory practices against women in employment on the revision of laws and procedures regulating the labor market, the impact on the policies of the World Bank of structural adjustment policies on women. When policy changes are sought from male-dominated decision makers, the ability to appeal to scientific authority and to general rather than situated knowledge is an asset for feminist campaigners.

The search by early women's studies for an epistemology of its own is perhaps best understood in terms of this search for certainty. But it is no longer needed. It has become a hindrance. Rather, we should have confi-

dence in the arguments, in the theory and data, that have been developed within women's studies and feminist analysis as producing better knowledge of the world as a result of its methods, integrity, and the new questions it dares to ask.

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