A Question of Evidence

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I outline a pragmatic account of evidence, arguing that it allows us to underwrite two implications of feminist scholarship: that knowledge is socially constructed and constrained by evidence, and that social relations, including gender, race, and class, are epistemologically significant. What makes the account promising is that it abandons any pretense of a view from nowhere, the view of evidence as something only individuals gather or have, and the view that individual theories face experience in isolation.

Truth and belief, W. V. Quine once noted, are sticky concepts; they stick to each other (Quine 1981a, 38). Evidence is equally sticky. Consider how strange things become when we are asked to unstick belief and evidence:

> We sit and think, but do we sit and believe? The White Queen, indeed, professed to do so: "When I was your age, I always did it for half-an-hour a day. Why, sometimes I've believed as many as six impossible things before breakfast." (Quine 1987, 19)

"But it will be agreed," Quine continues, "that the White Queen was atypical." Decisions to believe independently of any evidence "real or imagined," he notes, "stretch the term 'belief' beyond belief" (Quine 1987, 19). Belief is (or ought to be) constrained by available evidence. But it is not similarly constrained by truth. We do not, without further explanation, understand someone's claim to believe what is obviously inconsistent with available evidence. But we do understand belief of what is plausible, supported by available evidence, but in fact false. In particular cases, it is easier to unstick belief from truth than to unstick belief from evidence (or at least from the absence of counterevidence).

"Intellectual progress," John Dewey claimed, "usually occurs when we abandon questions and both of the alternatives they assume—an abandonment that results from their decreasing vitality and a change of urgent interest." We move on, he argued, not because we answer questions but because we

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get over them (Dewey 1910, 313). If Tarski and Davidson are right, "What is truth?" is such a question (though "What is true?" is not). Theories of truth, insofar as they invite talk of correspondence or facts, do not advance our understanding of the world. But the same cannot be said of evidence. It is a substantial concept and belief remains well stuck to it.

Indeed, any claim to forgo both the concept and the use of evidence is patently disingenuous. No claim or theory worth our attention comes unconnected to evidence. With feminist communities, including but by no means limited to those currently engaged in debates about epistemology, what Christine Di Stefano calls "the falsely innocent indifference" that may be appropriately attributed to postmoderns would be at least paradoxical (Di Stefano 1990, 77). For feminists, the nature of evidence, and hence a theory of evidence, is of the essence. But I stress that it is evidence, and theories thereof, of which I am speaking, not truth. I suspect that some considerable part of the misunderstanding among feminists involved in discussions of epistemology can be traced to confusions between evidence and truth.

Philosophical epistemology, a central feature of the background against which feminist epistemologies and debates about epistemology have developed, has created a litany of mutually implicated alternatives—epistemology *or* sociology of knowledge, modernism *or* postmodernism, objectivism *or* relativism—as well as the questions and categories that present these as authentic and exhaustive options. There is an increasing awareness in feminist discussions that it is such alternatives and the questions that prompt them that we need to get over (for recent discussions, see Nicholson 1990; Alcoff and Potter 1992). To advance that project, I outline here a pragmatic account of evidence. My claim is that the account allows us to underwrite the insights and implications of various feminist epistemologies, and at least some of the theories and projects with which these epistemologies are interdependent, and to abandon some of the questions underlying the litany of dichotomies listed above.

"Pragmatism," as Richard Rorty notes, "is a vague, ambiguous, and overworked word" (Rorty 1982, 160). My account of evidence is pragmatic in the sense that on that view there are no pretheoretic notions of evidence, no standards or methods laid down prior to the business of constructing theories to explain and predict what we experience. Rather, it is within our various epistemological projects—in daily life, in science, in philosophy, in politics—that notions and standards of evidence emerge, concomitantly with the unfolding of those projects. The point, I will argue, holds for all of our notions of evidence—from our more general notions (that all of it is, in the end, sensory, for example, or, alternatively, that there are innate ideas), to notions that are more context-specific (e.g., that instrument readings constitute evidence for events on the subatomic level or that "imperfections" are evidence of evolution). In explicating the account, I make use of Quine's arguments for holism and for the radical interdependence of epistemology and other current

theories, extending these in light of issues raised in and by feminist science criticism.

There is precedent, of course, for using the term "pragmatism" to describe Quine's views; Quine himself uses the term, as have feminists who have discussed his work (e.g., Addelson 1992; Harding 1986). I have also used "empiricist" to describe his views (Nelson 1990), as he himself does.[1] The connection between the two is naturalism: Quine's pragmatism is the result of his rejection of foundationalism, and of his commitment to the empiricist thesis that in the end the evidence for our various theories and claims is sensory—a thesis that comes, as he puts it, "after physics, physiology, and psychology, not before" (Quine 1966a, 212).[2] Defining science broadly as inclusive of all efforts to explain and predict experience, Quine argues that "the scientific system, ontology and all, is a conceptual bridge of our own making, linking sensory stimulation to sensory stimulation" (Quine 1981b, 20).[3] So stated, the view is compatible with any social constructivist account of knowledge. But naturalism precludes a wholesale skepticism about evidence. Hence Quine continues:

> But I also [have an] unswerving belief in external things—people, nerve endings, sticks, stones. . . . Now how is all this robust realism to be reconciled with the barren scene that I have just been depicting? The answer is naturalism, the recognition that it is within science itself, and not in some prior philosophy, that reality is to be identified and described. (Quine 1981b, 21)

That naturalism underwrites Quine's argument that epistemology is radically interdependent with other current knowledge and undertakings (Quine 1960, 1966a, 1969):

> Epistemology . . . is not logically prior somehow to common sense or to the refined common sense which is science; it is part rather of the overall scientific enterprise, an enterprise which Neurath has likened to that of rebuilding a ship while staying afloat in it. (Quine 1966b, 240)

Elsewhere I have argued that several of Quine's positions support important implications of feminist science critiques and epistemologies: that knowledge is socially constructed *and* constrained by evidence; that canons of evidence, metaphysical commitments, and epistemology are interdependent; and, indeed, that all of our ways of organizing and explaining experience share a radical interdependence (Nelson 1990). Here I make explicit the account of evidence at work in these positions and further naturalize it in light of knowledge provided by feminist scholarship. What will emerge is a view of evidence more inclusive than what Quine envisioned (but also, my arguments indicate, "naturally" extended) and commensurate with two further implica-

tions of feminist scholarship and activism: that social relations of gender, race, class, and culture are epistemologically significant[4] and that the primary epistemological agents are multiple, overlapping, and evolving communities.

What makes the account I will offer promising for feminists are three shifts it incorporates. One is the abandonment of any pretense to a view from nowhere, as well as the fantasy, as Susan Bordo recently characterized a view implicit in some postmodern arguments, of being everywhere (Bordo 1990, 136); this view of evidence and the arguments I will advance and rehearse in support of it insist that we are always "here." A second is the abandonment of the assumption that evidence is something only individuals can gather or have; the account indicates that evidence is fundamentally communal (see Nelson 1992). Third, and relatedly, the account abandons the assumption that specific theories and claims develop or face experience in isolation, insisting instead on an inclusive and dynamic system of evidential relations.

Recognizing the tenacity of the categories, questions, and assumptions that have underwritten the litany of dichotomies noted earlier, I begin to make the case for a pragmatic or naturalized account of evidence by briefly discussing two other familiar and contested notions, truth and epistemology.

I. Truth and Epistemology

The most basic point to note about truth is that exploring the possibility of a feminist theory thereof may be a viable future project, but it is a different project from the present one in which I explore the questions of what *warrants* beliefs and claims and what warrants our distinguishing claims as "more" or as "less" warranted. As I see things, these are questions about evidence, not about truth. But perhaps there is more that should be said.

I don't suggest that feminists (or anyone else) "give up" on truth. I assume that, unlike the White Queen (when we are not being disingenuous), we believe to be true what we say to be true and that "What is true?" remains a viable, indeed a crucial, question. But at present I know of only two ways to maintain more than a "disappearance" or "redundancy" theory of truth (the view, as Quine puts things, that "truth is immanent" [Quine 1981b, 21]). One is to define truth in terms of internal consistency, to subscribe to a coherence theory of truth. While I will argue that coherence with a current body of knowledge and practices is one of the constraints on specific theories and projects, it is not a sufficient criterion. Our collective experience indicates that nature is a point of resistance with which we need to contend, and the hypothesis that there is a world that constrains what it is reasonable to believe is woven through most of our theories because it makes the most sense of what we experience.

The second option, which gives the world its due, defines truth in terms of correspondence, in terms of some notion of "fitting" or "being true to" the

facts. But correspondence, and theories of truth that incorporate some version of it, are also beset with problems. Not the least of these is that correspondence presumes a dualism of "organizing schemes and that which is waiting to be organized" (Davidson 1974), and the latter presumes a view from nowhere and that there is "something" by virtue of which claims are "true."

It may be that some other approach will be found to save the intuition underlying theories of truth. Sandra Harding's recent suggestion that we can and should aim at less partial and perverse theories (Harding 1990) seems to me on the mark, but not to require a theory of truth to underwrite it. I think it can be underwritten with an adequate account of evidence, and I regard naturalism as one of the requirements for adequacy.

An important premise underlying my project is that epistemology, like other notions with which it is deeply connected—including evidence, truth, and knowers—is a contested notion and, like these others, a dynamic one. History attests to the fact that epistemologies emerge, evolve, and fade in relation to new undertakings, experience, and knowledge: e.g., in modern thought, in positivism, in Marxism, and, more recently, in feminism and in the variety of responses to the demise of foundationalism, which include sociology of knowledge, naturalized epistemologies, and postmodern arguments against epistemology. This historical process has implications for current debates about feminist epistemology.

One implication is that epistemology was not defined once and for all by Cartesian or any other epistemology, and hence we are currently faced with a number of interesting, interconnected, and *open* questions, including "Who knows?" "How is knowledge generated?" and "What is the nature and strength of evidence available to us?" Put another way, we would need an argument that did not rely on the tenets of Cartesian epistemology (or some other) to be warranted in concluding that epistemologies are necessarily wedded to the existence of "one truth," to abstract individualism, or to foundationalism or that they are by their "very nature" both partial and universalizing. Lacking such an argument, the questions I have noted remain open (see Nelson 1992; Nicholson 1990; Alcoff and Potter 1992).

A second implication is that epistemologies, theories about how knowledge is constructed and about the evidence available for and used in such constructions, are deeply connected to other current theories and undertakings. For those who have abandoned foundationalism with its promise of "extratheoretic" groundings and standards, there are several ways to construe that relationship and its implications. One response is to construe epistemology as an allegedly "purely descriptive" enterprise, as those working in the strong program in sociology of knowledge propose, a proposal that at least acknowledges that epistemology is dynamic.[5] Another response is to abandon epistemology altogether, as postmoderns advocate; my earlier comments suggest that the proposal assumes a view of epistemology undermined by its

history. A third is to "naturalize" epistemology, as Quine and some feminists advocate: to acknowledge that an epistemology is radically interdependent with other knowledge and undertakings and should be pursued as such and that the justification of beliefs and theories (if it occurs) will be on the basis of their coherence with experience rather than by appeal to a current epistemology.[6] A fourth response is to try to have it both ways: to acknowledge (at least implicitly) the interdependence of epistemologies and (other) current knowledge and projects but to attempt to salvage a justificatory role for epistemology (Harding 1986), perhaps by embracing the circularity involved and arguing that one circular system is better than others (Jaggar 1983).

The account of evidence I will advocate supports the third option, that we naturalize epistemology. It indicates that the grounds appropriate to judging theories, claims, and methods are broader than a favored epistemology (although appeals to epistemology will also be made): that they encompass experience, as well as other knowledge and undertakings with which, according to this account of evidence, epistemology shares a radical interdependence. Hence, a naturalized view of evidence allows us to retain the prescriptive aspects of epistemology without appeal to alleged "first principles" or foundations.

The most direct support for the claims made in this section awaits the account of evidence in the next. And perhaps the best way to conclude these introductory remarks is to make clear some pragmatic suspicions concerning traditional epistemologies and theories of truth by paraphrasing Rorty's description of William James and Dewey. So long as we see Quine and other pragmatists as having a "theory of truth," we get them wrong; so long as we see pragmatists as having a "theory of knowledge" in the sense of a "first knowledge"—in the sense of a theory to *justify* other theories, methods, and practices—we get them wrong. In either case, as Rorty says of James and Dewey, we ignore their "criticisms of the assumption that there ought to *be* theories about such matters" (Rorty 1982, 160). But evidence is another matter.

II. Evidence

We have learned, or perhaps relearned, much about evidence in the last four decades.[7] We now recognize that what we say and believe about the world, the social worlds and natural worlds within which we function and of which we are part, far exceeds all the evidence we have or ever will have. There is "slack," to use Quine's term, between all of our theories and the evidence we have for them (Quine 1966b, 241). Put another way, it is compatible with our collective experience that we will eventually abandon our current theories (though not, of course, all at once) for theories that are commensurate with much of our

experience to date but incompatible with our present theories (Longino 1990; Nelson 1992).

We have also learned that indefinitely many theories might equally well organize and explain what we experience, that we are not warranted in assuming there is a unique, true theory of nature awaiting discovery. As I have made the point, it is commensurate with our collective experience that an alternative theory of nature that did not include Boyle's law (or for that matter any "law"), a theory of nature that organized things differently, might equally well explain and predict what we experience (Nelson 1992; Potter 1992). Minimally, then, there is no one "most probable" account of the world, and hence we need some further argument to warrant the assumption that there is (nonetheless) one true account. Quine makes the argument this way:

> Even if we bypass such troubles [as are raised by assuming a final organon of scientific method] . . . we have no reason to suppose that man's [*sic*] surface irritations even unto eternity admit of any one systematization that is scientifically better or simpler than all possible others. It seems likelier, if only on account of symmetries or dualities, that countless alternatives would be tied for first place. Scientific method is the way to truth, but it affords even in principle no unique definition of truth. (Quine 1960, 23)

I have argued that another lesson of the last forty years is that there is nothing in our collective experience to warrant the assumption that our sensory organs are sufficiently refined to discriminate a "best" theory or "most probable" theory (if there is such a thing) from alternative candidates (Nelson 1992). It is commensurate with that experience and with our knowledge (e.g., in empirical psychology and evolutionary epistemology) that our sensory organs are refined to a degree that (so far) they enable us to survive by organizing and predicting relevant future experience. But there is nothing to warrant the inference that they are adequate to the task of encompassing *all* that goes on. Indeed, it is an implication of current evolutionary theory that our sensory organs are the product of "jury-rigging" and probably only one of the possible functional results (Gould 1989).

Feminist philosophers of science have made use of the first two lessons (and I am suggesting we also make use of the third) to remind us that provided we remain committed to taking evidence seriously, the door is not closed on *any* question (although not all, of course, are worthy of pursuit), including the question of whether gender or politics could have anything to do with "good" or with "serious" science. They have also used them in the service of more specific arguments concerning the role of background assumptions and values in theory acceptance, in their evaluations of particular theories and research

programs, and in their assessments of the emphases of traditional philosophy of science (see, e.g., Longino 1990; Nelson 1990; Potter 1992).

The final lesson I note here, a lesson that, while making use of the others, contributes important—and in some ways unique—insights into the nature of evidence, has resulted from feminist participation and interest in knowledge building. I focus again on feminist science criticism. It is an implication of that body of criticism—not only of its findings but of its existence and evolution as well—that the experience and knowledge we bring to bear on the theorizing we do in science will include that shaped by the social relations of gender, race, and class that characterize our society. Nor, many of us have argued, can we take the lesson of that body of criticism to mean that stricter methodological controls are needed to "filter out" these factors and relationships, which surely are present in feminist science criticism (Harding 1986, 1991; Longino 1990; Nelson 1990; Seigfried 1990; Tuana 1989). Rather, we need at the very least more authentic views of the relationships between knowledge and social and political relations than were possible given the categories (or lack thereof) of much of traditional epistemology.[8]

One can, of course, view these lessons as "the bad news." The first three underscore limitations on the evidence available to us; the fourth broadens the factors relevant to our knowledge and undertakings (including science) to encompass social relations, politics, values, and other factors long regarded as a threat to objectivity, if not the very antitheses of evidence. But I suggest that the inclusiveness appropriately understood is not to be feared. Full support for my suggestion awaits the account of evidence, but both are foreshadowed in the arguments so far rehearsed. Each of these arguments makes use of things we know (the "we," of course, requires specification),[9] and each presumes that knowledge (e.g., in empirical psychology, evolutionary theory, philosophy of science, and feminist science criticism) and collective experience ("collective," of course, also requires specification) constitute evidence for our views about evidence. In other words, the limitations and the inclusiveness are immanent and do nothing to further the case for skepticism or relativism.

The alternative to acknowledging these lessons provides no comfort. Holding on to the view that there is one most probable theory, or that knowledge will someday be complete, or that our sensory organs are sufficiently refined to encompass all that happens, or that there are real boundaries between "serious" knowledge and the social and political relations that characterize our society would be, *from here*, at best an article of faith. As such, it would be no more warranted, no more defensible, than any other article of faith.[10]

More to the point, we do not need to settle for an article of faith; there is a view that allows for evidence and reasonable belief without derivability from unshakable foundations, without certitude. The view accepts coherence, and with it explanatory power, as a measure of reasonableness. I begin to explicate

it using a description of research in high-energy physics offered by physicist Leon Lederman.

Lederman begins by noting that "no subatomic particle is ever observed directly":

> Two particles collide and spew debris and new matter inside the accelerator. Physicists infer the existence of new particles from the fact that they collide with other particles which leave electromagnetic tracks in a $65 million detector. Think of a bus that drives by your house every day. One afternoon while you're at work the bus collides with a Subaru. The bumper flies off the Subaru and hits your mailbox, which is hurled through your window. When you come home, you look at the pattern of shattered glass and say, "Hmmm, a Subaru." That's not unlike what high-energy physicists do for a living. (Lederman 1989, 43)

We can use Lederman's analogy in making a first start at answering the question, "What is the evidence for subatomic particles and for claims about their behavior?" Underwriting or supporting the view that electromagnetic tracks and debris are evidence of new subatomic particles, there is, most obviously, a body of theory and accepted practices in which subatomic particles figure directly, as well as other theories and methodological commitments in physics, mathematics, and technology. Lederman's analogy also reveals a connection between a theory that posits subatomic particles and so-called common-sense knowledge of macroscopic objects and events, for it is by virtue of the latter—in this case, instrument readings indicating the presence of debris in a $65 million particle detector, and the analogy drawn between such debris and what happens when macroscopic objects collide—that the evidence for, including the explanatory power of, subatomic particles becomes apparent.[11] Lederman is also clearly presuming broad metaphysical and methodological commitments, including the views that there are objects and events that are not "directly" observable that explain more systematically what happens on the macroscopic level and that particular macroscopic events (instrument readings, for example) are evidence for these.

My claim is that the theories, methods, and commitments that have emerged as underwriting claims about subatomic particles, some within science and some of a broader reach, constitute a large part of the evidence for those claims (see also Nelson 1992). That is, the terms "underwrite" and "support" serve to identify *essential* relationships, for the "system" sketched above, to use a Quinean metaphor, is akin to an arch, each "piece" supporting, serving as evidence for, and being supported by others (Quine 1960, 11). Knowledge about subatomic particles and claims about their behavior are not isolable from a larger system of theories, practices, and standards of evidence. Hence there

is no discrete piece of evidence that warrants them. The evidence includes an extensive body of scientific theories, technologies, methodologies, standards, and practices; knowledge and experience of macroscopic objects and events and the standards we use to identify these; and broad metaphysical and methodological commitments, including those noted above.[12]

This is only part of the story, of course, for internal consistency is not an adequate criterion for reasonable beliefs and viable explanations. Part of the evidence for physicists' current knowledge about subatomic particles is experience—theories that posit subatomic particles both organize and are compatible with our experiences—and such theories have explanatory power—they allow us to explain and to predict some of what happens. Our consideration of Lederman's statement also has implications for "experience": namely, that it can not be afforded a foundational status.[13] The sensory experiences that are currently *possible* and viewed as *relevant* to claims about subatomic particles are shaped and mediated by a historically specific system of theory and accepted practices—a system that also constitutes part of the evidence for such particles.

I was once asked if this view of evidence was appropriate to "nontheoretical" entities. The short answer to the question is that there are no such entities. Part of the evidence for mailboxes, Subarus, and instrument readings is provided by the larger systems of theories and practices in which such objects figure; and part of the evidence is provided by the fact that theories that include such objects (as well as political parties and social movements) help us to organize, explain, and predict some of what happens. Our abilities as individuals to recognize such objects depend as deeply on communal theories and practices as do those of the physicist to recognize electromagnetic tracks, for it is such theories and practices that organize our sensory experiences into coherent and recoverable accounts (Nelson 1990, 1992).

Quine once combined these several arguments. Noting that "any defense of the molecular doctrine has to do with its indirect bearing on observable reality" and that, from the point of view of molecular theory, physical objects are "just posits" that help us to organize what we experience, he argued that "we should do well to conclude: such, then, at bottom, is what evidence is, both for ordinary bodies and for molecules" (Quine 1966b, 233-38). The conclusion yields a naturalized and holistic account of evidence: standards of evidence, standards that determine what we recognize and countenance as evidence, emerge within the processes through which we generate knowledge. Our understanding of evidence emerges concomitantly with our success in understanding those very processes.

III. A Naturalized Account of Evidence

I turn next to an example from feminist science criticism as a way of further developing this account of evidence and to show that it is compatible with

what I previously cited as implications of feminist scholarship: that knowledge is both socially constructed and constrained by evidence and that social relations, including gender, race, and class, are epistemologically significant. I will also briefly address its implications for the agents of epistemology. The example is biologist Ruth Bleier's criticism of a research project in neuroendocrinology devoted to finding a hormonal basis for sex differences in hemispheric lateralization (Bleier 1984, 80-109).

A fundamental problem with research into a hormonal basis for sex-differentiated lateralization, Bleier argued, is that the central assumption underlying it—the assumption that there *are* sex differences in lateralization—is not itself warranted. The assumption, she noted, is in fact borrowed from a research project in reproductive neuroendocrinology that has attempted to establish such differences but has never succeeded in doing so (Bleier 1984, 84). Bleier cited additional and fundamental problems underlying research into sex-differentiated lateralization: there is no evidence for a correlation between hemispheric lateralization and cognitive abilities (e.g., visuospatial abilities) that does not rely on circular reasoning; the alleged sex differences in cognitive abilities to be explained by lateralization have not been established (indeed, they remain a matter of considerable debate within empirical psychology); the arguments used to support the hypothesis of sex-differentiated lateralization rely on circular reasoning and androcentric assumptions; and two of the more widely discussed studies that use lateralization to explain "women's lack of success in science and mathematics" contradict each other about its cause, with one claiming that it is due to a lesser degree of lateralization and the other citing a greater degree of lateralization (Bleier 1984, 80-109 *passim*). At the conclusion of an extensive analysis, Bleier attributed to sexism the flaws inherent in such research, the failure to recognize or acknowledge them, and the proliferation of studies into a biological basis for sex-differentiated cognitive abilities.

> What the studies are in fact trying to explain are the . . . differences that exist between the sexes in status, privilege, or power within known industrial, patriarchal systems, and they do this through attempting to scientifically establish biologically determined, sex-differentiated cognitive [characteristics] . . . that would make women's subordinate position inevitable. (Bleier 1984, 109)

I will not evaluate the viability of the two research programs mentioned or Bleier's claims about the specific flaws underlying or incorporated in them.[14] Rather, I will use the issues raised in her critique to support the account of evidence argued for in the preceding section and to naturalize it further in light of feminist science criticism, including Bleier's arguments. Note, first, that even for those who reject a holistic account of evidence, the borrowing of

hypotheses is not a mark of flawed science. To the contrary, synthesizing hypotheses and results is commonly regarded as a mark of good science. (But, of course, presenting results as if the hypothesis one has borrowed has been confirmed when it has not been, as Bleier documented in this and other cases, *is* a mark of flawed science.)

Second, while historical studies suggest clear connections between research into sex differences and social and political context (and, as I argue below, constitute evidence supporting feminist suspicions about such research), the evidence for the viability of individual research projects into sex differences and for the hypotheses at work in these projects (e.g., that hormones are related to sex-differentiated fetal brain organization) encompass *more than* androcentric and sexist assumptions (see Longino 1990). Some of the questions, hypotheses, and models Bleier cited as unwarranted do appear to be so when considered in isolation—the assumption, for example, of a causal relation between hormones and sex-differentiated lateralization, in the absence of results establishing (or even lending credence to) sex-differentiated lateralization. But, as Bleier's critique and those of other feminist critics indicate, these questions and hypotheses are strongly connected to other working hypotheses and research programs that contribute to the apparent plausibility of a hormonal basis for sex differences in lateralization. These include research in empirical psychology into possible correlations between lateralization and cognitive abilities, research in reproductive endocrinology linking androgens to sex differences in fetal rat brains, an extensive literature claiming that sex differences in mathematical abilities have been established, as well as the research in neuroendocrinology that Bleier noted, devoted (at least in part) to establishing sex-differentiated lateralization (Bleier 1984; Longino 1990; Nelson 1990).

Now a hundred research projects, interconnected and even interdependent at various levels, do not necessarily add up to one larger good research project, particularly if all incorporate wrongheaded assumptions, and I concur that there is evidence which indicates that research into a biological basis of sex differences in cognitive abilities is wrongheaded. But we need a framework broader than individual projects or hypotheses to encompass the evidence that actually supports the research Bleier and other feminists have criticized. Equally important, we need such a framework to arrive at an adequate account of the evidence that supports feminist criticisms of these projects and of the larger enterprise of attempting to discover or establish sex differences in cognitive abilities and/or a biological basis for those differences.

That framework, I suggest, is provided by a naturalized view of evidence that does not erect artificial boundaries between politics and science: that recognizes a broad system of theories and practices, including those of science (construed narrowly), "common sense," and politics, as constituting part of the evidence for individual research projects into sex differences—and for

feminist criticism of them. This view is naturalistic, for the expansion of evidence to include politics has emerged concomitantly with feminist experience and knowledge. Construing evidence broadly and naturalistically, we can point to the following as evidence supporting feminist claims that research into sex-differentiated lateralization is unwarranted: the historical and current fascination within the sciences to study (or establish) sex differences, and the fact that such interest intensifies during public debates about "women's position"; a staggering list of proposed explanations for women's inferior cognitive abilities; the fact that although explanations of women's alleged inferiority have often contradicted each other, the "results" have remained consistent; the androcentric (and often sloppy) assumptions incorporated within research questions (one of Bleier's examples is the research question, "Why are males masculine?"); and the fact that research into a biological basis for sex-differentiated cognitive abilities persists even as other current research, as well as the gains being made by women in mathematics and the sciences, undermines its rationale, a point to which I return below.

In other words, part of the evidence underwriting our suspicions about this research is a history that would be laughable if the results were not so tragic, a history with strong parallels to the present; and part of the evidence consists of other knowledge and developments—which, in fact, are available to those engaging in the research. Included in the last category (but by no means exhaustive of it) are research into postnatal neurobiological growth that indicates that the development of the structures necessary to and enabling cognitive functioning requires a sociolinguistic environment—and hence like other aspects of neurobiological development, is not driven solely by biology; psychological and sociological research documenting both the role of environment in the development of cognitive skills and differences in the "environments" of girls and boys relevant to such development; and the fact that girls are beginning to close the gap in (what are arguably gender-biased) math achievement tests and other areas, a gap the research in question is purportedly designed to explain (see, for example, Bleier 1984; Longino 1990; Nelson 1990).

It is in light of such evidence—some emerging within specific sciences and some within broader contexts, including history and politics—as well as in view of the flaws Bleier and other scientists have documented that we are warranted in concluding that androcentric and sexist assumptions are factors, indeed, that they are functioning as evidence, in current research into sex-differentiated lateralization.

IV. Conclusion

It is a consequence of the several arguments advanced and rehearsed here that there is no simple formula for distinguishing viable theories or research

programs from nonviable ones, that such judgments require extensive, multifaceted, and always incomplete evaluation (see Longino 1990; Nelson 1992; Seigfried 1990). When we acknowledge this, we raise the level of frustration of those who want feminists to answer "How could a theory about electrons be related to gender or politics?" in twenty-five words or less; but then were we to switch roles to ask a physicist to defend the theory of subatomic particles or a philosopher to defend modal logic, given the same restriction, it would become clear that we are, to vary Neurath's metaphor, in the same boat. More importantly, when we acknowledge this, we block the all-too-facile conclusion that the lesson provided by feminist scholarship is (what we all knew all along) that politics is inherently distorting of research and is itself a mark of "bad science." Politics, including an intense interest in gender and other social relations, is neither overcome nor absent in feminist science criticism. To deny its relevance, we would need to assume either that feminists (for some reason other than their politics) just happen to be better scientists (more careful observers, more creative theoreticians, more logically adept, etc.) or that many scientists have deliberately distorted their accounts of humans and other species to construct androcentric theories (Harding 1986; Longino 1990; Nelson 1990). My argument is that the important lesson to take from the last forty years is that individual theories neither develop nor face experience in isolation; that the evidence available, relevant, and appropriate includes theories, assumptions, projects, and values of a broad reach including politics; and that the evidence (when it is interesting) is neither arbitrary nor unable to be evaluated. These are among the more far-reaching implications of feminist science criticism, and they are central features of the view of evidence I am advocating.

It is a consequence of this view of evidence and an implication of the analysis of research into sex differences in cognitive abilities undertaken in the previous section that a naturalized feminist epistemology is a viable enterprise. In constructing accounts of how knowledge is generated and of the evidence available and used in such constructions, feminist epistemologists can and should appeal to, among other things, feminist experience and knowledge, acknowledging the radical interdependence between epistemology and other knowledge and undertakings. The relationship is better characterized as a spiral than as circular, for in attempting to understand the knowledge and standards accepted by another community or those of our own communities, we may find that we need to reconstruct the experiences of those involved (e.g., neuroendocrinologists) to make the most overall sense and/or to alter the assumptions with which we began—to abandon some of the standards of evidence and/or revise our views about what we know. Both kinds of reconstruction have, of course, characterized feminist scholarship to date.

My arguments also indicate the need for an additional reconstruction relevant to current debates about feminist epistemology and to the naturalized

epistemology I am advocating. It concerns who it is who knows, and I touch on it briefly here (see also Nelson 1990, 1992). On the view of evidence I have outlined, claims to know are shaped and made possible, and ultimately subject to, community criteria—the standards and knowledge constructed by communities within which we come to know (see Addelson 1992; Harding 1991; Longino 1990). As I have put the point, "acceptable answers to the question 'Who knows?' include 'Many of us,' 'All of *us*,' 'Everyone' . . . but only very problematically, 'Only me' " (Nelson 1990, 255-56). My claims to know are subject to the knowledge and standards constructed by the various communities of which I am a member; indeed, I have the ability to know only because there are such communities, and both my communities and I will judge my claims by reference to communal standards and knowledge. Hence the primary epistemological agents are groups—or, as the foregoing suggests, communities.[15]

This claim might be understood in two ways. One is that what comes to be recognized (and some of it "certified") as knowledge (recognized by our friends in the first case; cited in footnotes, published in refereed journals, printed in boldface in textbooks, in the second) is the result of negotiations between, consensus achieved by, or other activities engaged in by individuals who, *as individuals*, know in some logically or empirically "prior" sense.

But evidence is not an individual matter. And so the claim is stronger, the "we" a primitive: the collaborators, the consensus achievers, and, in more general terms, the agents who generate knowledge are communities, not individuals. Individuals do know, of course; but your knowing or mine depends on *our* knowing, for some "we." More to the point, you or I *can* only know what *we know* (or could), for some "we." Hence the "agents" in the current debate concerning research into sex-differentiated lateralization, the appropriate focus of epistemologists, are dynamic communities, including at least neuroendocrinology, feminist communities, and their overlap (consider feminist endocrinologists), as well as the larger social and political community of which these are subcommunities.

The reconstructions that have resulted from feminist scholarship, including those undertaken and proposed here, are compatible with and supported by a view of epistemology and other current knowledge and undertakings as mutually supporting, without benefit or need of pretheoretic or extrasystematic foundations.

> Neurath has likened science to a boat, which, if we are to rebuild it, we must rebuild plank by plank while staying afloat in it. . . . Our boat stays afloat because at each alteration we keep the bulk of it intact as a going concern. (Quine 1960, 3-4)

A feminist, naturalized account of evidence is also a going concern.

NOTES

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1. It is not clear, however, that those of us who attribute pragmatism or empiricism to Quine are using the same understandings of either. See, e.g., Harding 1986; Longino 1990; Nelson 1990; and Tuana 1992.

2. As Charlene Haddock Seigfried points out, Dewey's instrumentalism or pragmatism "was often simply known as his naturalism" (private correspondence). See, e.g., Kennedy 1970.

3. Ironically, Quine also wants to maintain a boundary between politics and science. If we adopt the holism and naturalism he advocates and I pursue here, no such boundary can be drawn. See also Nelson 1990.

4. I make use here of Harding's insight that gender is a social relation (Harding 1991). Seigfried points out that the original pragmatists, in particular Mead and Dewey, recognized race, class, and culture—and, to a lesser extent, gender—as epistemologically significant and that "the extension of gender [follows logically]" from their recognition of other social relations (private correspondence).

5. The qualification reflects my view that all such accounts make at least implicit use of current knowledge and assumptions.

6. See, for example, Addelson 1992; Duran 1991; and Nelson 1990, 1992.

7. This paragraph and the next two repeat arguments made in Nelson 1992.

8. Similar arguments are found in Code 1991; Duran 1991; Harding 1991; Longino 1990; Nelson 1990; and Alcoff and Potter 1992.

9. It will become clear that I view the homogeneous "we" of some mainstream epistemology as no less a fiction than the abstract "every man" of traditional epistemology. See also Code 1991; Harding 1991; and Nelson 1992.

10. Elsewhere I argue that such articles of faith may be a consequence of a commitment to epistemological individualism (Nelson 1992).

11. I qualify "common-sense" knowledge to remind us that it is as dynamic and theoretical (and sometimes as contested) as "scientific" knowledge.

12. See also Nelson 1990. For an alternative and comprehensive account of empiricism, as well as arguments against the holism I am advocating, see Longino 1990.

13. Appeals to experience can become vacuous unless the notion of what constitutes experience is further specified. Here and elsewhere I rely on Quine's notion of experience as the firings of sensory receptors (Nelson 1990, 1992). But as those discussions and this indicate, we do not experience those firings; we experience the world.

14. Bleier's analysis was more extensive than my summary indicates. See also Longino 1990; Nelson 1990; and Tuana 1989.

15. Seigfried notes that "the community of knowledge of feminist epistemology has historical roots in C. S. Peirce's community of scholars, which Dewey extended to actual communities . . ." (private correspondence).

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