5

Subjects, Power, and Knowledge: Description and Prescription in Feminist Philosophies of Science

Helen E. Longino

I. Prologue

Feminists, faced with traditions in philosophy and in science that are deeply hostile to women, have had practically to invent new and more appropriate ways of knowing the world. These new ways have been less invention out of whole cloth than the revival or reevaluation of alternative or suppressed traditions. They range from the celebration of insight into nature through identification with it to specific strategies of survey research in the social sciences. Natural scientists and laypersons anxious to see the sciences change have celebrated Barbara McClintock's loving identification with various aspects of the plants she studied, whether whole organism or its chromosomal structure revealed under the microscope. Social scientists from Dorothy Smith to Karen Sacks have stressed designing research *for* rather than merely about women, a goal that requires attending to the specificities of women's lives and consulting research subjects themselves about the process of gathering information about them. Such new ways of approaching natural and social phenomena can be seen as methods of discovery, ways of getting information about the natural and social worlds not available via more traditional experimental or investigative methods.

Feminists have rightly pointed out the blinders imposed by the philosophical distinction between discovery and justification; a theory of scientific inquiry that focuses solely on the logic of justification neglects the selection processes occurring in the context of discovery that limit what we get to know about. Methods of discovery, or heuristics, are in effect selection processes that present for our consideration certain sorts of hypotheses and not other sorts. Feminists have

identified heuristic biases—androcentrism, sexism, and gender ideology—that limit the hypotheses in play in specific areas of inquiry and have also pointed out that alternative heuristics put different hypotheses in play. However, a theory of scientific inquiry that focuses solely on methods of discovery presents its own difficulties. In particular, a given heuristic method that puts certain hitherto suppressed or invisible hypotheses into play is not ipso facto ratifiable as a producer of knowledge, as distinct from interesting or even plausible ideas. Something more is required before we can speak of knowledge (or even confirmation) as opposed to plausibility. One way to articulate the distinctions I am urging is to treat analysis of the context of discovery as a primarily descriptive analysis of how hypotheses are generated and to treat analysis in the context of justification as involving a normative or prescriptive analysis regarding the appropriate criteria for the acceptance of hypotheses. This is problematic because philosophers in the past who made this distinction sometimes concluded that only the context of justification is worthy of philosophical analysis. Nevertheless, ignoring the context of justification for the context of discovery is equally problematic. I wish in this essay to explore some of the tensions between descriptivism and normativism (or prescriptivism) in the theory of knowledge, arguing that although many of the most familiar feminist accounts of science have helped us to redescribe the process of knowledge (or belief) acquisition, they stop short of an adequate normative theory. However, these accounts do require a new approach in normative epistemology because of their redescription.

Although this essay focuses on issues in the epistemology of science, it bears on general issues in epistemology in two ways. First, to the extent that "science" simply means knowledge, an analysis of scientific knowledge is an analysis of knowledge. Second, philosophy of science to a large degree relies on general epistemological principles. Critical discussion of their adequacy for the philosophy of science is relevant to, although not conclusive regarding, their tenability in a general theory of knowledge. To the extent that human knowledge is not coextensive with scientific knowledge, however, remarks bearing on science are only partially relevant to knowledge in general.

The relevance relations from general epistemology to scientific knowledge are even less direct. In contemplating the problems of developing new and more appropriate knowledge, it is tempting to suppose that epistemology could provide the key that would unlock the right door—that if we could just get the epistemology right, we would get the science right, too. Surely one source of this belief is the close relationship between the science and the philosophy done

at the beginning of the modern period. Does not the epistemology of Descartes and of Locke have something to do with the theories of nature that took hold during the Seventeenth Century? Another is reflection on the persistence of misogynist views in biological theories, from the various subfields of evolutionary theory to theories of development. If one hallmark of the modern period is the development of rule-based inquiry, something in the justification rules must account for this persistence. If getting the epistemology wrong accounts for harmful science, getting the epistemology right must be the key to better science. This is probably an oversimplification of the thinking that has underlain the attraction to epistemology for many feminist scholars outside of philosophy, but I do not think it is too far off the mark. And although I do think that new approaches in the theory of knowledge would alter some of our attitudes in and about science, I also think that the relationship between epistemology—the theory of what practices produce knowledge—and science—what counts as knowledge—in any given period is more complicated than the temptation allows. We cannot produce knowledge of the world on the strength of a general theory of knowledge.

Nor can we simply dismiss the accumulated knowledge of the natural world produced by the traditional methods of the natural sciences. These sciences have transformed conditions of life in industrialized portions of the world, both conceptually as models of knowledge and materially through science-based technologies. Why, then, do some of us feel so uneasy not only about the theories directly concerning females and gender but also about the very nature of scientific knowledge and the power it creates? After all, even feminists who wish to change the sciences are also, by that very ambition, expressing a hope for power. There are surely various sources for and locations of this uneasiness. Those of us who are feminists have been struck by the interlocking character of several aspects of knowledge and power in the sciences. Women have been excluded from the practice of science, even as scientific inquiry gets described both as a masculine activity and as demonstrating women's unsuitability to engage in it, whether because of our allegedly deficient mathematical abilities or our insufficient independence. Some of us notice the location of women in the production of the artifacts made possible by new knowledge: swift and nimble fingers on the microelectronics assembly line. Others notice the neglect of women's distinctive health issues by the biomedical sciences, even as new techniques for preserving the fetuses they carry are introduced into hospital delivery rooms. The sciences become even more suspect as analysis of their metaphors (for example, in cell biology and in microbiology) reveals

an acceptance (and hence reinforcement) of the cultural identification of the male with activity and of the female with passivity. Finally, feminists have drawn a connection between the identification of nature as female and the scientific mind as male and the persistent privileging of explanatory models constructed around relations of unidirectional control over models constructed around relations of interdependence. Reflection on this connection has prompted feminist critics to question the very idea of a scientific method capable of adjudicating the truth or probability of theories in a value-neutral way.

Although the sciences have increased human power over natural processes, they have, according to this analysis, done so in a lop-sided way, systematically perpetuating women's cognitive and political disempowerment (as well as that of other groups marginalized in relation to the Euro-American drama). One obvious question, then, is whether this appropriation of power is an intrinsic feature of science or whether it is an incidental feature of the sciences as practiced in the modern period, a feature deriving from the social structures within which the sciences have developed. A second question is whether it is possible to seek and possess empowering knowledge without expropriating the power of others. Is seeking knowledge inevitably an attempt at domination? And are there criteria of knowledge other than the ability to control the phenomena about which one seeks knowledge? Feminists have answered these questions in a number of ways. I will review some of these before outlining my own answer.

II. Feminist Epistemological Strategies 1: Changing the Subject

Most traditional philosophy of science (with the problematic exception of Descartes's) has adopted some form of empiricism. Empiricism's silent partner has been a theory of the subject, that is, of the knower.[1] The paradigmatic knower in Western epistemology is an individual—an individual who, in several classic instances, has struggled to free himself from the distortions in understanding and perception that result from attachment. Plato, for example, maintained that knowledge of the good is possible only for those whose reason is capable of controlling their appetites and passions, some of which have their source in bodily needs and pleasures and others of which have their source in our relations with others. The struggle for epistemic autonomy is even starker for Descartes, who suspends belief in all but his own existence in order to recreate a body of knowledge cleansed of faults, impurities, and uncertainties. For Descartes, only those grounds available to a single, unattached, disembodied mind

are acceptable principles for the construction of a system of beliefs. Most subsequent epistemology has granted Descartes's conditions and disputed what those grounds are and whether any proposed grounds are sufficient grounds for knowledge. Descartes's creation of the radically and in principle isolated individual as the ideal epistemic agent has for the most part gone unremarked.[2] Locke, for example, adopts the Cartesian identification of the thinking subject with the disembodied soul without even remarking upon the individualism of the conception he inherits and then struggles with the problem of personal identity. Explicitly or implicitly in modern epistemology, whether rationalist or empiricist, the individual consciousness that is the subject of knowledge is transparent to itself, operates according to principles that are independent of embodied experience, and generates knowledge in a value-neutral way.

One set of feminist epistemological strategies, sometimes described as modifications or rejections of empiricism, can also, and perhaps better, be described as changing the subject. I will review three such strategies of replacement, arguing that although they enrich our understanding of how we come to have the beliefs we have and so are more descriptively adequate than the theories they challenge, they fall short of normative adequacy. The strategies identify the problems of contemporary science as resulting from male or masculinist bias. Each strategy understands both the bias and its remedy differently. One holds out the original ideal of uncontaminated or unconditioned subjectivity. A second identifies bias as a function of social location. A third identifies bias in the emotive substructure produced by the psychodynamics of individuation.

Feminist empiricism has by now taken a number of forms. That form discussed and criticized by Sandra Harding is most concerned with those fields of scientific research that have misdescribed or misanalyzed women's lives and bodies. It's not clear that any feminist scholars have totally conformed to the profile identified by Harding, but certain moments in the analyses offered by practicing scientists who are feminists do fit this model.[3] At any rate, feminist empiricism (*sub* Harding) identifies the problems in the scientific accounts of women and gender as the product of male bias. Typical examples of problematic views are the treatment of the male of the species as the locus of variation (and hence the basis of evolutionary change for a species), the persistent treatment of male difference as male superiority, the assumption of universal male dominance, and the treatment of sexual divisions of labor in industrialized societies as the product of biological species evolution. Each of these involves neglecting contradictory empirical information. It should be no surprise that a focus

on these sorts of problems suggests their solution in replacing the androcentric subject of knowledge with an unbiased subject—one that would not ignore the empirical data already or easily available. From this perspective, certain areas of science having to do with sex and gender are deformed by gender ideology, but the methods of science are not themselves masculinist and can be used to correct the errors produced by ideology. The ideal knower is still the purified mind, and epistemic or cognitive authority inheres in this purity. This strategy, as Harding has observed, is not effective against those research programs that feminists find troublesome but that cannot be faulted by reference to the standard methodological precepts of scientific inquiry. I have argued, for example, that a critique of research on the influence of prenatal gonadal hormones on behavioral sex differences that is limited to methodological critique of the data fails to bring out the role of the explanatory model that both generates the research and gives evidential relevance to that data.[4]

Another approach is, therefore, the standpoint approach. There is no one position from which value-free knowledge can be developed, but some positions are better than others. Standpoint epistemologies notice systematic distortions in description and analysis produced by those occupying social positions of power. Traditional Marxists identified the standpoint of the bourgeoisie as producing such distortions, whereas feminists have identified the standpoint of men (of the dominant class and race) as equally distorting. Nancy Hartsock and other feminist standpoint theorists have argued that the activities of ruling-class men produce a knowledge of the world characterized by abstractness and impersonality, that their own politically structured freedom from the requirements of re/producing the necessities of daily life is reflected in the kind of understanding they produce of the social and natural world.[5] Women's work, by contrast, is characterized by greater interaction with material substances, by constant change, and by its requirement of emotional investment in the form of caring. Not only does women's characteristic activity and relation to the means of production/reproduction produce its own unique form of understanding, but also women who become self-conscious agents in this work are able to incorporate men's perspectives as well as their own and hence to develop a more accurate, more objective, set of beliefs about the world.

By valorizing the perspectives uniquely available to those who are socially disadvantaged, standpoint theorists turn the table on traditional epistemology; the ideal epistemic agent is not an unconditioned subject but the subject conditioned by the social experiences of oppression. The powerless are those with epistemic legitimacy, even

if they lack the power that could turn that legitimacy into authority. One of the difficulties of the standpoint approach comes into high relief, however, when it is a women's or a feminist standpoint that is in question. Women occupy many social locations in a racially and economically stratified society. If genuine or better knowledge depends on the correct or a more correct standpoint, social theory is needed to ascertain which of these locations is the epistemologically privileged one. But in a standpoint epistemology, a standpoint is needed to justify such a theory. What is that standpoint and how do we identify *it?* If no single standpoint is privileged, then either the standpoint theorist must embrace multiple and incompatible knowledge positions or offer some means of transforming or integrating multiple perspectives into one. Both of these moves require either the abandonment or the supplementation of standpoint as an epistemic criterion.

Standpoint theory faces another problem as well. It is by now commonplace to note that standpoint theory was developed by and for social scientists. It has been difficult to see what its implications for the natural sciences might be. But another strategy has seemed more promising. Most standpoint theorists locate the epistemic advantage in the productive/reproductive experience of the oppressed whose perspective they champion. A different change of subject is proposed by those identifying the problems with science as a function of the psychodynamics of individuation. Evelyn Fox Keller has been asking, among other things, why the scientific community privileges one kind of explanation or theory over others. In particular she has asked why, when both linear reductionist and interactionist perspectives are available, the scientific community has preferred the linear or "master molecule" theory that understands a natural process as controlled by a single dominant factor. This question was made vivid by her discussion of her own research on slime mold aggregation and the fate of Barbara McClintock's work on genetic transposition.[6]

Keller's original response, spelled out in *Reflections on Gender and Science,* involved an analysis of the traditional ideal of scientific objectivity, which she understood as the ideal of the scientist's detachment from the object of study.[7] In her view, epistemic and affective ideals are intermingled, and from the psychoanalytic perspective she adopted, distorted affective development—autonomy as exaggerated separateness—was expressed in a distorted epistemic ideal—objectivity as radical detachment. Drawing on and developing object relations theory, she attributed this "static autonomy" to the conditions under which boys develop psychologically: exaggerated separateness is a solution to the anxieties provoked by those conditions. Keller analyzed

the consequent ideal of static objectivity as generating and satisfied by accounts of natural processes that foreground controlling relationships—for example, accounts of organismic development as determined by the individual's genetic program. She, therefore, proposed an alternative conceptualization of autonomy, contrasting static autonomy with what she called dynamic autonomy, an ability to move in and out of intimate connection with the world. Dynamic autonomy provides the emotional substructure for an alternative conception of objectivity: dynamic objectivity. The knower characterized by dynamic objectivity, in contrast to the knower characterized by static objectivity, does not seek power over phenomena but acknowledges instead the ways in which knower and phenomena are in relationship as well as the ways in which phenomena themselves are complexly interdependent. Barbara McClintock's work has offered one of the most striking examples of the effectiveness of such an approach, although interactionist approaches have also been applied in areas besides developmental biology. McClintock's work, long ignored, was finally vindicated by developments in molecular biology of the 1970s—the acknowledgment of genetic transposition in the prokaryotes that had been the model organisms for contemporary molecular genetics. Dynamic objectivity is not presented as a typically feminine epistemological orientation but as an alternative to an epistemological orientation associated with both masculine psychological development and masculinist gender ideology. But however much interactionist approaches might appeal to us, and however much dynamic objectivity might appeal to us, there isn't a general argument to the truth of interactionism or to the epistemological superiority of dynamic objectivity.

Both standpoint theory and the psychodynamic perspective suggest the inadequacy of an ideal of a pure transparent subjectivity that registers the world as it is in itself (or, for Kantians, as structured by universal conditions of apperception or categories of understanding). I find it most useful to read them as articulating special instances of more general descriptive claims that subjectivity is conditioned by social and historical location and that our cognitive efforts have an ineluctably affective dimension. Classical standpoint theory identifies relation to production/reproduction as the key, but there are multiple, potentially oppositional relations to production/reproduction in a complex society, and there are other kinds of social relation and location that condition subjectivity. For example, one of the structural features of a male-dominant society is asymmetry of sexual access. Men occupy a position of entitlement to women's bodies, whereas women, correspondingly, occupy the position of that to which men

are entitled. Complications of the asymmetry arise in class- and race-stratified societies. There may be other structural features as well, such as those related to the institutions of heterosexuality, that condition subjectivity. Because each individual occupies a location in a multidimensional grid marked by numerous interacting structures of power asymmetry, the analytical task is not to determine which is epistemically most adequate. Rather, the task is to understand how these complexly conditioned subjectivities are expressed in action and belief. I would expect that comparable complexity can be introduced into the psychodynamic account.

Treating subjectivity as variably conditioned and cognition as affectively modulated opens both opportunities and problems. The opportunities are the possibilities of understanding phenomena in new ways; by recognizing that mainstream accounts of natural processes have been developed from particular locations and reflect particular affective orientations, we can entertain the possibility that quite different accounts might emerge from other locations with the benefit of different emotional orientations. Although either transferring or diffusing power, the strategies discussed so far have in common a focus on the individual epistemic agent, on the autonomous subject. (The subject in the second and third approaches comes to be in a social context and as a consequence of social interactions, but its knowledge is still a matter of some relation between it and the subject matter.) The standpoint and psychodynamically based theories recommend certain new positions and orientations as superior to others but fail to explain how we are to decide or to justify decisions between what seem to be conflicting claims about the character of some set of natural processes. On what grounds can one social location or affective orientation be judged epistemically superior to another? Normative epistemology arises in the context of conflicting knowledge claims. Naturalism, or descriptivism, in epistemology presupposes that we know what we think we know and asks how. But the existence of comparably persuasive incompatible claims calls into question whether we know at all, requires that we reexamine what we take to be adequate justification, and may even call into question our very concept of knowledge.

Feminist science critics have provided analyses of the context of discovery that enable us to see how social values, including gender ideology in various guises, could be introduced into science. Some theories that have done so go on to recommend an alternate subject position as epistemically superior. But arguments are missing—and it's not clear that any particular subject position could be adequate to generate knowledge. Can a particular subject position be supported

by an a priori argument? It can, but only by an argument that claims a particular structure for the world and then identifies a particular subjectivity as uniquely capable of knowing that structure. The problem with such arguments is that they beg the question. The one subject position that could be advanced as epistemically superior to others without presupposing something about the structure of the world is the unconditioned position, the position of no position that provides a view from nowhere. Attractive as this ideal might seem, arguments in the philosophy of science suggest that this is a chimera. Let me turn to them.

III. Feminist Epistemological Strategies 2: Multiplying Subjects

The ideal of the unconditioned (or universally conditioned) subject is the traditional proposal for escaping the particularity of subjectivity. Granting the truth of the claim that individual subjectivities are conditioned, unconditioned subjectivity is treated as an achievement rather than a natural endowment. The methods of the natural sciences constitute means to that achievement. Some well-known arguments in the philosophy of science challenge this presumption. As they have received a great deal of attention in the philosophical literature, I shall only mention them here in order to bring out their relevance to the general point. The methods of the natural sciences, in particular, have been thought to constitute the escape route from conditioned subjectivity. The difficulty just outlined for the feminist epistemological strategy of changing the subject, however, has a parallel in developments in the philosophy of science. Both dilemmas suggest the individual knower is an inappropriate focus for the purpose of understanding (and changing) science.

In the traditional view, the natural sciences are characterized by a methodology that purifies scientific knowledge of distortions produced by scientists' social and personal allegiances. The essential features of this methodology—explored in great detail by positivist philosophers of science—are observation and logic. Much philosophy of science in the last twenty-five years has been preoccupied with two potential challenges to this picture of scientific methodology—the claim of Kuhn, Feyerabend, and Hanson that observation is theory laden and the claim of Pierre Duhem that theories are underdetermined by data. One claim challenges the stability of observations themselves, the other the stability of evidential relations. Both accounts have seemed (at least to their critics and to some of their proponents) to permit the unrestrained expression of scientists' subjective preferences in the content of science. If observation is theory

laden, then observation cannot serve as an independent constraint on theories, thus permitting subjective elements to constrain theory choice. Similarly, if observations acquire evidential relevance only in the context of a set of assumptions, a relevance that changes with a suitable change in assumptions, then it's not clear what protects theory choice from subjective elements hidden in background assumptions. Although empirical adequacy serves as a constraint on theory acceptance, it is not sufficient to pick out one theory from all contenders as the true theory about a domain of the natural world. These analyses of the relation between observation, data, and theory are often thought to constitute arguments against empiricism, but, like the feminist epistemological strategies, they are more effective as arguments against empiricism's silent partner, the theory of the unconditioned subject. The conclusion to be drawn from them is that what has been labeled scientific method does not succeed as a means to the attainment of unconditioned subjectivity on the part of individual knowers. And as long as the scientific knower is conceived of as an individual, knowing best when freed from external influences and attachment (that is, when detached or free from her/his context), the puzzles introduced by the theory-laden nature of observation and the dependence of evidential relations on background assumptions will remain unsolved.

It need not follow from these considerations, however, that scientific knowledge is impossible of attainment. Applying what I take to be a feminist insight—that we are all in relations of interdependence—I have suggested that scientific knowledge is constructed not by individuals applying a method to the material to be known but by individuals in interaction with one another in ways that modify their observations, theories and hypotheses, and patterns of reasoning. Thus scientific method includes more than just the complex of activities that constitutes hypothesis testing through comparison of hypothesis statements with (reports of) experiential data, in principle an activity of individuals. Hypothesis testing itself consists of more than the comparison of statements but involves equally centrally the subjection of putative data, of hypotheses, and of the background assumptions in light of which they seem to be supported by those data to varieties of conceptual and evidential scrutiny and criticism.[8] Conceptual criticism can include investigation into the internal and external consistency of a hypothesis and investigation of the factual, moral, and social implications of background assumptions; evidential criticism includes not only investigation of the quality of the data but of its organization, structuring, and so on. Because background assumptions can be and most frequently are invisible to the members of the

scientific community for which they are background and because unreflective acceptance of such assumptions can come to define what it is to be a member of such a community (thus making criticism impossible), effective criticism of background assumptions requires the presence and expression of alternative points of view. This sort of account allows us to see how social values and interests can become enshrined in otherwise acceptable research programs (i.e., research programs that strive for empirical adequacy and engage in criticism). As long as representatives of alternative points of view are not included in the community, shared values will not be identified as shaping observation or reasoning.

Scientific knowledge, on this view, is an outcome of the critical dialogue in which individuals and groups holding different points of view engage with each other. It is constructed not by individuals but by an interactive dialogic community. A community's practice of inquiry is productive of knowledge to the extent that it facilitates transformative criticism. The constitution of the scientific community is crucial to this end as are the interrelations among its members. Community level criteria can, therefore, be invoked to discriminate among the products of scientific communities, even though context-independent standards of justification are not attainable. At least four criteria can be identified as necessary to achieve the transformative dimension of critical discourse:

1. There must be publicly recognized forums for the criticism of evidence, of methods, and of assumptions and reasoning.
2. The community must not merely tolerate dissent, but its beliefs and theories must change over time in response to the critical discourse taking place within it.
3. There must be publicly recognized standards by reference to which theories, hypotheses, and observational practices are evaluated and by appeal to which criticism is made relevant to the goals of the inquiring community. With the possible exception of empirical adequacy, there needn't be (and probably isn't) a set of standards common to all communities. The general family of standards from which those locally adopted might be drawn would include such cognitive virtues as accuracy, coherence, and breadth of scope, and such social virtues as fulfilling technical or material needs or facilitating certain kinds of interactions between a society and its material environment or among the society's members.
4. Finally, communities must be characterized by equality of in-

> tellectual authority. What consensus exists must not be the result of the exercise of political or economic power or of the exclusion of dissenting perspectives; it must be the result of critical dialogue in which all relevant perspectives are represented.

Although requiring diversity in the community, this is not a relativist position. True relativism, as I understand it, holds that there are no legitimate constraints on what counts as reasonable to believe apart from the individual's own beliefs. Equality of intellectual authority does not mean that anything goes but that everyone is regarded as equally capable of providing arguments germane to the construction of scientific knowledge. The position outlined here holds that both nature and logic impose constraints. It fails, however, to narrow reasonable belief to a single one among all contenders, in part because it does not constrain belief in a wholly unmediated way. Nevertheless, communities are constrained by the standards operating within them, and individual members of communities are further constrained by the requirement of critical interaction relative to those standards. To say that there may be irreconcilable but coherent and empirically adequate systems for accounting for some portion of the world is not to endorse relativism but to acknowledge that cognitive needs can vary and that this variation generates cognitive diversity.

Unlike the view from nowhere achievable by unconditioned subjectivity or the view from that somewhere identified as maximizing knowledge, this notion of knowledge through interactive intersubjectivity idealizes the view from everywhere (perhaps better thought of as *views* from *many wheres*). These criteria for objective communities represent not a description of actual scientific communities but a set of prescriptions that are probably not anywhere satisfied. Nevertheless, they provide a measure against which actual communities and, indirectly, criteria for the comparison of theories can be evaluated. For example, theories accepted in different communities can be compared with respect to the conditions under which the critical dialogue concerning a given theory has occurred. Although there are any number of objections that advocates of such a notion must address, I will confine myself here to one major problem, the answer to which opens up some future directions for feminist analysis and scientific practice.

IV. Dilemmas of Pluralism

This sort of account is subject to the following dilemma.[9] What gets produced as knowledge depends on the consensus reached in the scientific community. For knowledge to count as genuine, the com-

munity must be adequately diverse. But the development of a theoretical idea or hypothesis into something elaborate enough to be called knowledge requires a consensus. The questions must stop somewhere, at some point, so that a given theory can be developed sufficiently to be applied to concrete problems. How is scientific knowledge possible while pursuing socially constituted objectivity? That is, if objectivity requires pluralism in the community, then scientific knowledge becomes elusive, but if consensus is pursued, it will be at the cost of quieting critical oppositional positions.

My strategy for avoiding this dilemma is to detach scientific knowledge from consensus, if consensus means agreement of the entire scientific community regarding the truth or acceptability of a given theory. This strategy also means detaching knowledge from an ideal of absolute and unitary truth. I suggest that we look at the aims of inquiry (at least some) as satisfied by embracing multiple and, in some cases, incompatible theories that satisfy local standards. This detachment of knowledge from universal consensus and absolute truth can be made more palatable than it might first appear by two moves. One of these is implicit in treating science as a practice or set of practices; the other involves taking up some version of a semantic or model-theoretic theory of theories.

Beginning with the second of these, let me sketch what I take to be the relevant aspects and implications of the semantic view.[10] This view is proposed as an alternative to the view of theories as sets of propositions (whether axiomatized or not). If we take the semantic view, we understand a theory as a specification of a set of relations among objects or processes characterized in a fairly abstract way. Another characterization would be that on the semantic view, a theory is the specification of a structure. The structure as specified is neither true nor false; it is just a structure. The theoretical claim is that the structure is realized in some actual system. As Mary Hesse has shown, models are proposed as models of some real world system on the basis of an analogy between the model and the system, that is, the supposition that the model and the system share some significant features in common.[11] Models often have their start as metaphors. Examples of such metaphoric models are typical philosophers' examples like the billiard ball model of particle interactions or the solar system model of the atom. What many feminists have pointed out (or can be understood as having pointed out) is the use of elements of gender ideology and social relations as metaphors for natural processes and relations. Varieties of heterosexual marriage have served as the metaphoric basis for models of the relation between nucleus and cytoplasm in the cell, for example.[12] The master molecule approach to

gene action, characterized by unidirectional control exerted on organismal processes by the gene, reflects relations of authority in the patriarchal household. Evelyn Fox Keller has recently been investigating the basis of models in molecular biology in androcentric metaphors of sexuality and procreation.[13] When Donna Haraway says that during and after the Second World War the organism changed from a factory to a cybernetic system, she can be understood as saying that the metaphor generating models of organismic structure and function shifted from a productive system organized by a hierarchical division of labor to a system for generating and processing information.[14] Alternatively put, cells, gene action, and organisms have been modelled as marriage, families, and factories and cybernetic networks, respectively. Supporting such analysis of particular theories or models requires not merely noticing the analogies of structure but also tracing the seepage of language and meaning from one domain to another as well as studying the uses to which the models are put.[15]

The adequacy of a theory conceived as a model is determined by our being able to map some subset of the relations/structures posited in the model onto some portion of the experienced world. (Now the portions of the world stand in many relations to many other portions.) Any given model or schema will necessarily select among those relations. So its adequacy is not just a function of isomorphism of one of the interpretations of the theory with a portion of the world but of the fact that the relations it picks out are ones in which we are interested. A model guides our interactions with and interventions in the world. We want models that guide the interactions and interventions we seek. Given that different subcommunities within the larger scientific community may be interested in different relations or that they may be interested in objects under different descriptions, different models (that if taken as claims about an underlying reality would be incompatible) may well be equally adequate and provide knowledge, in the sense of an ability to direct our interactions and interventions, even in the absence of a general consensus as to what's important. Knowledge is not detached from knowers in a set of propositions but consists in our ability to understand the structural features of a model and to apply it to some particular portion of the world; it is knowledge of that portion of the world through its structuring by the model we use. The notion of theories as sets of propositions requires that we view the adequacy of a theory as a matter of correspondence of the objects, processes, and relations described in the propositions of the theory with the objects, processes, and relations in the domain of the natural world that the theory purports to explain; that is, it requires that adequacy be conceptualized as truth. The

model-theoretic approach allows us to evaluate theories in relation to our aims as well as in relation to the model's isomorphism with elements of the modeled domain and permits the adequacy of different and incompatible models serving different and incompatible aims. Knowledge is not contemplative but active.

The second move to escape the dilemma develops some consequences of treating science as practice. There are two worth mentioning. If we understand science as practice, then we understand inquiry as ongoing, that is, we give up the idea that there is a terminus of inquiry that just is the set of truths about the world. (What LaPlace's demon knew, for example.) Scientific knowledge from this perspective is not the static end point of inquiry but a cognitive or intellectual expression of an ongoing interaction with our natural and social environments. Indeed, when we attempt to identify the goals of inquiry that organize scientific cognitive practices, it becomes clear that there are several, not all of which can be simultaneously pursued.[16] Scientific knowledge, then, is a body of diverse theories and their articulations onto the world that changes over time in response to the changing cognitive needs of those who develop and use the theories, in response to the new questions and anomalous empirical data revealed by applying theories, and in response to changes in associated theories. Both linear-reductionist and interactionist models reveal aspects of natural processes, some common to both and some uniquely describable with the terms proper to one but not both sorts of model. If we recognize the partiality of theories, as we can when we treat them as models, we can recognize pluralism in the community as one of the conditions for the continued development of scientific knowledge in this sense.

In particular, the models developed by feminists and others dissatisfied with the valuative and affective dimensions of models in use must at the very least (given that they meet the test of empirical adequacy) be recognized as both revealing the partiality of those models in use and as revealing some aspects of natural phenomena and processes that the latter conceal. These alternative models may have a variety of forms and a variety of motivations, and they need not repudiate the aim of control. We engage in scientific inquiry to direct our interactions with and interventions in the world. Barbara McClintock was not a feminist, but she was in part reacting against the gendered meanings in natural philosophy, meanings which shut her out of inquiry; Ruth Hubbard advocates interactionist perspectives out of more explicitly political commitments; feminists and others concerned with the environment reject the control orientation of technocrats effective in the short term for more complex models that

can address long-term change and stasis in the ecosystem. If we aim for effective action in the natural world, something is to be controlled. The issue should be not whether but what and how. Rather than repudiate it, we can set the aim of control within the larger context of overall purposes and develop a more refined sense of the varieties of control made possible through scientific inquiry.

A second consequence for feminist and other oppositional scientists of adopting both the social knowledge thesis and a model-theoretic analysis of theories is that the constructive task does not consist in finding the one best or correct feminist model. Rather, the many models that can be generated from the different subject positions ought to be articulated and elaborated. Very few will be exclusively feminist if that means exclusively gender-based or developed only by feminists. Some will be more appropriate for some domains, others for others, and some for none. We can't know this unless models get sufficiently elaborated to be used as guides for interactions. Thus, this joint perspective implies the advocacy of subcommunities characterized by local standards. To the extent that they address a common domain and to the extent that they share some standards in common, these subcommunities must be in critical dialogue with each other as well as with those subcommunities identified with more mainstream science. The point of dialogue from this point of view is not to produce a general and universal consensus but to make possible the refinement, correction, rejection, and sharing of models. Alliances, mergers, and revisions of standards as well as of models are all possible consequences of this dialogic interaction.

V. Conclusions

Understanding scientific knowledge in this way supports at least two further reflections on knowledge and power. First of all, the need for models within which we can situate ourselves and the interactions we desire with the natural world will militate against the inclusiveness required for an adequate critical practice, if only because the elaboration of any model requires a substantial commitment of material and intellectual resources on the part of a community.[17] This means that, in a power-stratified society, the inclusion of the less powerful and hence of models that could serve as a resource for criticism of the received wisdom in the community of science will always be a matter of conflict. At the same time, the demand for inclusiveness should not be taken to mean that every alternative view is equally deserving of attention. Discussion must be conducted in reference to public standards, standards which, as noted above, do not provide

timeless criteria, but which change in response to changes in cognitive and social needs. Nevertheless, by appeal to standards adopted and legitimated through processes of public scrutiny and criticism, it is possible to set aside as irrelevant positions such as New Age "crystalology" or creationism. To the extent that these satisfy none of the central standards operative in the scientific communities of their cultures, they indeed qualify as crackpot. Programs for low-tech science appropriate to settings and problems in developing nations may, by contrast, be equally irritating to or against the grain of some of the institutionalized aspects of science in the industrialized nations, but as long as they do satisfy some of the central standards of those communities, then the perspectives they embody must be included in the critical knowledge-constructive dialogue. Although there is always a danger that the politically marginal will be conflated with the crackpot, one function of public and common standards is to remind us of that distinction and to help us draw it in particular cases. I do not know of any simple or formulaic solution to this problem.

Second, those critiques of scientific epistemology that urge a change of subject preserve the structures of cognitive authority but propose replacing those currently wielding authority with others: a genuinely unbiased subject in one case, a differently located or a differently formed subject in the other. Either no assumptions or different assumptions will be engaged in the knowledge-constructive process. In the position I am advocating, which makes salient those features of knowledge construction made invisible by more traditional accounts, the structures of cognitive authority themselves must change. No segment of the community, whether powerful or powerless, can claim epistemic privilege. If we can see our way to the dissolution of those structures, then we need not understand the appropriation of power in the form of cognitive authority as intrinsic to science. Nevertheless, the creation of cognitive democracy, of democratic science, is as much a matter of conflict and hope as is the creation of political democracy.

Notes

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1. Empiricist philosophers have found themselves in great difficulty when confronting the necessity to make their theory of the knower explicit, a difficulty most eloquently expressed in David Hume's Appendix to *A Treatise of Human Nature,* ed. L. A. Selby-Bigge (Oxford, UK: Clarendon Press, 1960).

2. The later philosophy of Wittgenstein does challenge the individualist ideal. Until recently few commentators have developed the anti-individualist implications of his work. See Naomi Scheman, "Individualism and the Objects of Psychology" in *Discovering Reality,* ed. Sandra Harding and Merrill Hintikka (Boston: Reidel, 1983), 225–44.

3. Harding has treated Marcia Millman and Rosabeth Kantor's Introduction to their collection, *Another Voice* (New York: Doubleday, 1975) and my essay with Ruth Doell, "Body, Bias and Behavior," from *Signs* 9, 2 (Winter 1983) as exemplars of feminist empiricism. The latter is discussed extensively in Harding's *The Science Question in Feminism* (Ithaca: Cornell University Press, 1986). Because the article nowhere claims that masculinist bias can be corrected by application of current methodologies in the sciences, I have always found the discussion in *The Science Question* a puzzlingly perverse misreading.

4. Cf. Longino, "Can There Be A Feminist Science?" in *Hypatia* 2, 3 (Fall 1987); and chapter 7 of Longino, *Science as Social Knowledge* (Princeton: Princeton University Press, 1990).

5. Cf. Nancy Hartsock, "The Feminist Standpoint: Developing the Ground for a Specifically Feminist Historical Materialism," in Harding and Hintikka, *Discovering Reality,* 283–310.

6. Cf. Evelyn F. Keller, "The Force of the Pacemaker Concept in Theories of Slime Mold Aggregation," in *Perspectives in Biology and Medicine* 26 (1983): 515–21; and *A Feeling for the Organism* (San Francisco: W. H. Freeman, 1983).

7. Evelyn F. Keller, *Reflections on Gender and Science* (New Haven: Yale University Press, 1984).

8. For argument for and exposition of these points, see Longino, *Science as Social Knowledge,* especially chapter 4.

9. Thanks to Sandra Mitchell for this formulation.

10. My understanding of the semantic view is shaped by its presentations in Bas van Fraassen, *The Scientific Image* (New York: Oxford University Press, 1980); and Ronald Giere, *Explaining Science* (Chicago: University of Chicago Press, 1988); as well as by conversations with Richard Grandy and Elisabeth Lloyd. Nancy Cartwright's views on explanation, as developed in *How the*

Laws of Physics Lie (New York: Oxford University Press, 1983) have deeply influenced my thinking.

11. Mary Hesse, *Models and Analogies in Science* (Notre Dame: Notre Dame University Press, 1966).

12. The Gender and Biology Study Group, "The Importance of Feminist Critique for Contemporary Cell Biology," in *Hypatia* 3, 1 (1988).

13. Evelyn Fox Keller, "Making Gender Visible in the Pursuit of Nature's Secrets," in *Feminist Studies/Critical Studies*, Teresa de Lauretis, ed., (Bloomington: Indiana University Press, 1986), 67–77; and "Gender and Science," in *The Great Ideas Today* (Chicago: Encyclopedia Britannica, 1990).

14. Donna Haraway, "The Biological Enterprise: Sex, Mind, and Profit from Human Engineering to Sociobiology," in *Radical History Review* 20 (1979): 206–37.

15. This is the strategy adopted in chapter 8 of *Science as Social Knowledge*.

16. This point is developed further in *Science as Social Knowledge*, chapter 2.

17. For a somewhat different approach to a similar question, see Philip Kitcher, "The Division of Cognitive Labor," in *Journal of Philosophy* LXXXVII, 1 (January 1990): 5–23.

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Edited and with an Introduction by
LINDA ALCOFF and
ELIZABETH POTTER

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