Women, Science, and Technology

A Reader in Feminist Science Studies

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GENDER AND SCIENCE
An Update

Evelyn Fox Keller

THE MEANING OF GENDER

Schemes for classifying human beings are necessarily multiple and highly variable. Different cultures identify and privilege different criteria in sorting people of their own and other cultures into groups: They may stress size, age, color, occupation, wealth, sanctity, wisdom, or a host of other demarcators. All cultures, however, sort a significant fraction of the human beings that inhabit that culture by sex. What are taken to be the principal indicators of sexual difference as well as the particular importance attributed to this difference undoubtedly vary, but, for fairly obvious reasons, people everywhere engage in the basic act of distinguishing people they call male from those they call female. For the most part, they even agree about who gets called what. Give or take a few marginal cases, these basic acts of categorization do exhibit conspicuous cross-cultural consensus: Different cultures will sort any given collection of adult human beings of reproductive age into the same two groups. For this reason, we can say that there is at least a minimal sense of the term "sex" that denotes categories given to us by nature.[1] One might even say that the universal importance of the reproductive consequences of sexual difference gives rise to as universal a preoccupation with the meaning of this difference.

But for all the cross-cultural consensus we may find around such a minimalist classification, we find equally remarkable cultural variability in what people have made and continue to make of this demarcation; in the significance to which they attribute it; in the properties it connotes; in the role it plays in ordering the human world beyond the immediate spheres of biological reproduction; even in the role it plays in ordering the nonhuman world. It was to underscore this cultural variability that American feminists of the 1970s introduced the distinction between sex and gender, assigning the term "gender" to the meanings of masculinity and femininity that a given culture attaches to the categories of male and female.[2]

The initial intent behind this distinction was to highlight the importance of nonbiological (that is, social and cultural) factors shaping the development of adult men and women, to emphasize the truth of Simone de Beauvoir's famous dictum, "Women are not born, rather they are made." Its function was to shift attention away from the time-honored and perhaps even ubiquitous question of the meaning of sexual difference (that is, the meanings of masculine and feminine), *to* the question of how such meanings are constructed. In Donna Haraway's words, "Gender is a concept developed to contest the naturalization of sexual difference" (1991:131).

Very quickly, however, feminists came to see, and, as quickly, began to exploit, the considerably larger range of analytic functions that the multipotent category of gender is able to serve. From an original focus on gender as a cultural norm guiding the psychosocial development of individual men and women, the attention of feminists soon turned to gender as a cultural structure organizing social (and sexual) relations between men and women,[3] and finally, to gender as the basis of a sexual division of cognitive and emotional labor that brackets women, their work, and the values associated with that work from culturally normative delineations of categories intended as "human"—objectivity, morality, citizenship, power, often even, "human nature" itself. From this perspective, gender and gender norms come to be seen as silent organizers of the mental and discursive maps of the social and natural worlds we simultaneously inhabit and construct—*even of those worlds that women never enter.* This I call the symbolic work of gender; it remains silent precisely to the extent that norms associated with masculine culture are taken as universal.

The fact that it took the efforts of contemporary feminism to bring this symbolic work of gender into recognizable view is in itself noteworthy. In these efforts, the dual focus on women as subjects and on gender as a cultural construct was crucial. Analysis of the relevance of gender structures in conventionally male worlds only makes sense once we recognize gender not only as a bimodal term, applying symmetrically to men *and* women (that is, once we see that men too are gendered, that men too are made rather than born), but also as denoting social rather than natural kinds. Until we can begin to envisage the possibility of alternative arrangements, the symbolic work of gender remains both silent and inaccessible. And as long as gender is thought to pertain only to women, any question about its role can only be understood as a question about the presence or absence of biologically female persons.

This double shift in perception—first, from sex to gender, and second, from the force of gender in shaping the development of men and women to its force in delineating the cultural maps of the social and natural worlds these adults inhabit—constitutes the hallmark of contemporary feminist theory. Beginning in the mid 1970s, feminist historians, literary critics, sociologists, political scientists, psychologists, philosophers, and soon, natural scientists as well, sought to supplement earlier feminist analyses of the contribution, treatment, and representation of men and women in these various fields with an enlarged analysis of the ways in which privately held and publicly shared ideas about gender have shaped the underlying assumptions and operant categories in the intellectual history of each of these fields. Put simply, contemporary feminist theory might be described as "a form of attention, a lens that brings into focus a particular question: What does it mean to describe one aspect of human experience as 'male' and another as 'female'? How do such labels affect the ways in which we structure the world around us, assign value to its different domains, and in turn, acculturate and value actual men and women?" (Keller 1985:6).

With such questions as these, feminist scholars launched an intensive investigation of the traces of gender labels evident in many of the fundamental assumptions underlying the traditional academic disciplines. Their earliest efforts were confined to the humanities and social sciences, but by the late 1970s, the lens of feminist inquiry had extended to the natural sciences as well. Under particular scrutiny came those assumptions that posited a dichotomous (and hierarchical) structure tacitly modeled on the prior assumption of a dichotomous (and hierarchical) relation between male and female—for example, public/private; political/personal; reason/feeling; justice/care; objective/subjective; power/love, and so on. The object of this endeavor was not to reverse the conventional ordering of these relations, but to undermine the dichotomies themselves—to expose to radical critique a worldview that deploys categories of gender to rend the fabric of human life and thought along a multiplicity of mutually sanctioning, mutually supportive, and mutually defining binary oppositions.

FEMINISM AND SCIENCE

But if the inclusion of the natural sciences under this broad analytic net posed special opportunities, it also posed special difficulties, and special dangers, each of which requires special recognition. On the one hand, the presence of gender markings in the root categories of the natural sciences and their use in the hierarchical ordering of such categories (for example, mind and nature; reason and feeling; objective and subjective) is, if anything, more conspicuous than in the humanities and social sciences. At the same time, the central claim of the natural sciences is precisely to a methodology that transcends human particularity, that bears no imprint of individual or collective authorship. To signal this dilemma, I began my first inquiry into the relations between gender and science (Keller 1978) with a quote from George Simmel, written more than sixty years ago:

> The requirements of . . . correctness in practical judgments and objectivity in theoretical knowledge . . . belong as it were in their form and their claims to humanity in general, but in their actual historical configuration they are masculine throughout. Supposing that we describe these things, viewed as absolute ideas, by the single word "objective," we then find that in the history of our race the equation objective = masculine is a valid one (cited in Keller 1978:409).

Simmel's conclusion, while surely on the mark as a description of a cultural history, alerts us to the special danger that awaits a feminist critique of the natural sciences. Indeed, Simmel himself appears to have fallen into the very trap that we are seeking to expose: In neglecting to specify the space in which he claims "validity" for this equation as a *cultural or even ideological space,* his wording invites the reading of this space as a biological one. Indeed, by referring to its history as a "history of our race" without specifying "our race" as late-modern, northern European, he tacitly elides the existence of other cultural histories (as well as other "races") and invites the same conclusion that this cultural history has sought to establish; namely, that "objectivity" is simultaneously a universal value and a privileged possession of the male of the species.

The necessary starting point for a feminist critique of the natural sciences is thus the reframing of this equation as a conundrum: How is it that the scientific mind can be *seen* at one and the same time as both male and disembodied? How is it that thinking "objectively," that is, thinking that is defined as self-detached, impersonal, and transcendent, is also understood as "thinking like a man"? From the vantage point of our newly "enlightened" perceptions of gender, we might be tempted to say that the equation "objective = masculine," harmful though it (like that other equation woman = nature) may have been for aspiring women scientists in the past, was simply a descriptive mistake, reflecting misguided views of women. But what about the views of "objectivity" (or "nature") that such an equation necessarily also reflected (or inspired)? What difference—for science, now, rather than for women—might such an equation have made? Or, more generally, what sorts of work in the actual production of science has been accomplished by the association of gender with virtually all of the root categories of modern science over the three hundred odd years in which such associations prevailed? How have these associations helped to shape the criteria for "good" science? For distinguishing the values deemed "scientific" from those deemed "unscientific"? In short, what particular cultural norms and values has the language of gender carried into science, and how have these norms and values contributed to its shape and growth?

These, then, are some of the questions that feminist theory brings to the study of science, and that feminist historians and philosophers of science have been trying to answer over the last fifteen years. But, for reasons I have already briefly indicated, they are questions that are strikingly difficult to hold in clear focus (to keep distinct, for example, from questions about the presence or absence of women scientists). For many working scientists, they seem not even to "make sense."

One might suppose, for example, that once such questions were properly posed (that is, cleansed of any implication about the real abilities of actual women), they would have a special urgency for all practicing scientists who are also women. But experience suggests otherwise; even my own experience suggests otherwise. Despite repeated attempts at clarification, many scientists (especially, women scientists) persist in misreading the force that feminists attribute to gender ideology as a force being attributed to sex, that is, to the claim that women, for biological reasons, would do a different kind of science. The net effect is that, where some of us see a liberating potential (both for women *and* for science) in exhibiting the historical role of gender in science, these scientists often see only a reactionary potential, fearing its use to support the exclusion of women from science.[4]

The reasons for the divergence in perception between feminist critics and women scientists are deep and complex. Though undoubtedly fueled by political concerns, they rest finally neither on vocabulary, nor on logic, nor even on empirical evidence. Rather, they reflect a fundamental difference in mind-set between feminist critics and working scientists—a difference so radical that a "feminist scientist" appears today as much a contradiction in terms as a "woman scientist" once did[5]. . . .

THE MEANING OF SCIENCE

Although people everywhere, throughout history, have needed, desired, and sought reliable knowledge of the world around them, only certain forms of knowledge and certain procedures for acquiring such knowledge have come to count under the general rubric that we, in the late twentieth century, designate as science. Just as "masculine" and "feminine" are categories defined by a culture, and not by biological necessity, so too, "science" is the name we give to a set of practices and a body of knowledge delineated by a community. Even now, in part because of the great variety of practices that the label "science" continues to subsume, the term defies precise definition, obliging us to remain content with a conventional definition—as that which those people we call scientists do.

What has compelled recognition of the conventional (and hence social) character of modern science is the evidence provided over the last three decades by historians, philosophers, and sociologists of science who have undertaken close examination of what it is that those people we call (or have called) scientists actually do (or have done).[6] Careful attention to what questions get asked, of how research programs come to be legitimated and supported, of how theoretical disputes are resolved, of "how experiments end" reveals the working of cultural and social norms at every stage.[7] Consensus is commonly achieved, but it is rarely compelled by the forces of logic and evidence alone. On every level, choices are (must be) made that are social *even as* they are cognitive and technical. The direct implication is that not only different collections of facts, different focal points of scientific attention, but also different conceptions of explanation and proof, different representations of reality, different criteria of success, are both possible and consistent with what we call science.

But if such observations have come to seem obvious to many observers of science, they continue to seem largely absurd to the men and women actually engaged in the production of science. In order to see how cultural norms and values can, indeed have, helped define the success and shape the growth of science, it is necessary to understand how language embodies and enforces such norms and values. This need far exceeds the concerns of feminism, and the questions it gives rise to have become critical for anyone currently working in the history, philosophy, or sociology of science. That it continues to elude most working scientists is precisely a consequence of the fact that their worldviews not only lack but actually preclude recognition of the force of language on what they, in their day-to-day activity as scientists, think and do. And this, I suggest, follows as much from the nature of their activity as it does from scientific ideology.

LANGUAGE AND THE DOING OF SCIENCE[8]

The reality is that the "doing" of science is, at its best, a gripping and fully absorbing activity—so much so that it is difficult for anyone so engaged to step outside the demands of the particular problems under investigation to reflect on the assumptions underlying that investigation, much less, on the language in which such assumptions can be said to "make sense." Keeping track of and following the arguments and data as they unfold, trying always to think ahead, demands total absorption; at the same time, the sense of discovering or even generating a new world yields an intoxication rarely paralleled in other academic fields. The net result is that scientists are probably less reflective of the "tacit assumptions" that guide their reasoning than any other intellectuals of the modern age.

Indeed, the success of their enterprise does not, at least in the short run, seem to require such reflectivity.[9] Some would even argue that very success demands abstaining from reflection upon matters that do not lend themselves to "clear and distinct" answers. Indeed, they might argue that what distinguishes contemporary science from the efforts of their forbears is precisely their recognition of the dual need to avoid talk *about* science, and to replace "ordinary" language by a technical discourse cleansed of the ambiguity and values that burden ordinary language, as the modern form of the scientific report requires. Let the data speak for themselves, these scientists demand. The problem is, of course, that data never do speak for themselves.

It is by now a near truism that all data presuppose interpretation. And if an interpretation is to be meaningful—if the data are to be "intelligible" to more than one person—it must be embedded in a community of common practices, shared conceptions of the meaning of terms and their relation to and interaction with the "objects" to which these terms point. In science as elsewhere, interpretation requires the sharing of a common language.

Sharing a language means sharing a conceptual universe. It means more than knowing the "right" names by which to call things; it means knowing the "right" syntax in which to pose claims and questions, and even more critically it means sharing a more or less agreed-upon understanding of what questions are legitimate to ask, and what can be accepted as meaningful answers. Every explicit question carries with it a complex of tacit (unarticulated and generally unrecognized) presuppositions and expectations that limit the range of acceptable answers in ways that only a properly versed respondent will recognize. To know what kinds of explanation will "make sense," what can be expected to count as "accounting for," is already to be a member of a particular language community.

But if there is one feature that distinguishes scientific from other communities, and that is indeed special to that particular discourse, it is precisely the assumption that the universe scientists study is directly accessible, that the "nature" they name as object of inquiry is unmediated by language and can therefore be veridically represented. On this assumption, "laws of nature" are beyond the relativity of language—indeed, they are beyond language, encoded in logical structures that require only the discernment of reason and the confirmation of experiment. Also on this assumption, the descriptive language of science is transparent and neutral; it does not require examination.

Confidence in the transparency and neutrality of scientific language is certainly useful in enabling scientists to get on with their job; it is also wondrously effective in supporting their special claims to truth. It encourages the view that their own language, because neutral, is absolute, and in so doing, helps secure their disciplinary borders against criticism. Language, assumed to be transparent, becomes impervious.

It falls to others, then, less enclosed by the demands of science's own self-understanding, to disclose the "thickness" of scientific language, to scrutinize the conventions of practice, interpretation, and shared aspirations on which the truth claims of that language depend, to expose the many forks in the road to knowledge that these very conventions have worked to obscure, and, in that process, finally, to uncover alternatives for the future. Under careful scrutiny, the hypothesized contrast between ordinary and scientific language gives way to a recognition of disconcerting similarity. Even the most purely technical discourses turn out to depend on metaphor, on ambiguity, on instabilities of meaning—indeed, on the very commonsense understanding of terms from which a technical discourse is supposed to emancipate us. Scientific arguments cannot begin to "make sense," much less be effective, without extensive recourse to shared conventions for controlling these inevitable ambiguities and instabilities. The very term "experimental control" needs to be understood in a far larger sense than has been the custom—describing not only the control of variables, but also of the ways of seeing, thinking, acting, and speaking in which an investigator must be extensively trained before he or she can become a contributing member of a discipline.

Even the conventional account scientists offer of their success has been shown by recent work in the history, philosophy, and sociology of science to be itself rooted in metaphor: The very idea, for example, of a one-to-one correspondence between theory and reality, or of scientific method as capable of revealing nature "as it is," is based on metaphors of mind or science as "mirror of nature." Simple logic, however, suggests that words are far too limited a resource, in whatever combinations, to permit a faithful representation of even our own experience, much less of the vast domain of natural phenomena. The metaphor of science as "mirror of nature" may be both psychologically and politically useful to scientists, but it is not particularly useful for a philosophical understanding of how science works; indeed, it has proven to be a positive barrier to our understanding of the development of science in its historical and social context. It is far more useful, and probably even more correct, to suppose, as Mary Hesse suggests, that "[s]cience is successful only because there are sufficient local and particular regularities between things in space-time domains where we can test them. These domains may be very large but it's an elementary piece of mathematics that there is an infinite gap between the largest conceivable number and infinity" (1989:E24).

In much the same sense, the idea of "laws of nature" can also be shown to be rooted in metaphor, a metaphor indelibly marked by its political and theological origins. Despite the insistence of philosophers that laws of nature are merely descriptive, not prescriptive, they are historically conceptualized as imposed from above and obeyed from below. "By those who first used the term, [laws of nature] were viewed as commands imposed by the deity upon matter, and even writers who do not accept this view often speak of them as 'obeyed' by the phenomena, or as agents by which the phenomena are produced."[10] In this sense, then, the metaphor of "laws of nature" carries into scientific practice the presupposition of an ontological hierarchy, ordering not only mind and matter, but theory and practice, and, of course, the normal and the aberrant. Even in the loosest (most purely descriptive) sense of the term *law*, the kinds of order in nature that laws can accommodate are restricted to those that can be expressed by the language in which laws of nature are codified. All languages are capable of describing regularity, but not all perceivable, nor even all describable, regularities can be expressed in the existing vocabularies of science. To assume, therefore, that all perceptible regularities can be represented by current (or even by future) theory is to impose a premature limit on what is "naturally" possible, as well as what is potentially understandable.

Nancy Cartwright (1990) has suggested that a better way to make sense of the theoretical successes of science (as well as its failures) would be to invoke the rather different metaphor of "Nature's Capacities." In apparent sympathy with Mary Hesse, as well as with a number of other contemporary historians and philosophers of science, she suggests that an understanding of the remarkable convergences between theory and experiment that scientists have produced requires attention not so much to the adequacy of the laws that are presumably being tested, but rather to the particular and highly local manipulation of theory and experimental procedure that is required to produce these convergences. Our usual talk of scientific laws, Cartwright suggests, belies (and elides) both the conceptual and linguistic work that is required to ground a theory, or "law," to fit a particular set of experimental circumstances and the material work required to construct an experimental apparatus to fit a theoretical claim. Scientific laws may be "true," but what they are true of is a distillation of highly contrived and exceedingly particular circumstances, as much artifact as nature.

TURNING FROM GENDER AND SCIENCE TO LANGUAGE AND SCIENCE

The questions about gender with which I began this essay can now be reformulated in terms of two separable kinds of inquiry: The first, bearing on the historical role of public and private conceptions of gender in the framing of the root metaphors of science, belongs to feminist theory proper, whereas the second, that of the role of such metaphors in the actual development of scientific theory and practice, belongs to a more general inquiry in the history and philosophy of science. By producing abundant historical evidence pertaining to the first question, and by exhibiting the in-principle possibility of alternative metaphoric options, feminist scholars have added critical incentive to the pursuit of the second question. And by undermining the realism and univocality of scientific discourse, the philosophical groundwork laid by Kuhn, Hesse, Cartwright, and many others, now makes it possible to pursue this larger question in earnest, pointing the way to the kind of analysis needed to show how such basic acts of naming have helped to shape the actual course of scientific development, and, in so doing, have helped to obscure if not foreclose other possible courses.

The most critical resource available for such an inquiry is the de facto plurality of organizing metaphors, theories, and practices evident throughout the history of science. At any given moment, in any given discipline, abundant variability can be readily identified along the following four closely interdependent axes: the aims of scientific inquiry; the questions judged most significant to ask; the theoretical and experimental methodologies deemed most productive for addressing these questions; and, finally, what counts as an acceptable answer or a satisfying explanation. Different metaphors of mind, nature, and the relation between them, reflect different psychological stances of observer to observed; these, in turn, give rise to different cognitive perspectives—to different aims, questions, and even to different methodological and explanatory preferences. Such variability is of course always subject to the forces of selection exerted by collective norms, yet there are many moments in scientific history in which alternative visions can survive for long enough to permit identification both of their distinctiveness, and of the selective pressures against which they must struggle.

The clearest and most dramatic such instance in my own research remains that provided by the life and work of the cytogeneticist, Barbara McClintock. McClintock offers a vision of science premised not on the domination of nature, but on "a feeling for the organism."[11] For her, a "feeling for the organism" is simultaneously a state of mind and a resource for knowledge: for the day-to-day work of conducting experiments, observing and interpreting their outcomes—in short, for the "doing" of science. "Nature," to McClintock, is best known for its largesse and prodigality; accordingly, her conception of the work of science is more consonant with that of exhibiting nature's "capacities" and multiple forms of order, than with pursuing the "laws of nature." Her alternative view invites the perception of nature as an active partner in a more reciprocal relation to an observer, equally active, but neither omniscient nor omnipotent; the story of her life's work (especially, her identification of genetic transposition) exhibits how that deviant perception bore fruit in equally dissident observations.

But history is strewn with such dissidents and deviants, often as persistent and perceptive but still less fortunate than McClintock. Normally, they are erased from the record, in a gesture readily justified by the conventional narrative of science. Without the validation of the dominant community, deviant claims, along with the deviant visions of science that had guided them, are dismissed as "mistakes," misguided and false steps in the history of science. What such a retrospective reading overlooks is that the ultimate value of any accomplishment in science—that which we all too casually call its "truth"—depends not on any special vision enabling some scientists to see directly into nature, but on the acceptance and pursuit of their work by the community around them, that is, on the prior existence or development of sufficient commonalities of language and adequate convergences between language and practice. Language not only guides how we as individuals think and act; it simultaneously provides the glue enabling others to think and act along similar lines, guaranteeing that our thoughts and actions *can* "make sense."

WHAT ABOUT "NATURE"?

Still, language does not "construct reality." Whatever force it may have, that force can, after all, only be exerted on language-speaking subjects—for our concerns here, on scientists and the people who fund their work. Though language is surely instrumental in guiding the material actions of these subjects, it would be foolhardy indeed to lose sight of the force of the material, nonlinguistic, substrata of those actions, that is, of that which we loosely call "nature." Metaphors work to focus our attention in particular ways, conceptually magnifying one set of similarities and differences while dwarfing or blurring others, guiding the construction of instruments that bring certain kinds of objects into view, and eclipsing others. Yet, for any given line of inquiry, it is conspicuously clear that not all metaphors are equally effective for the production of further knowledge. Furthermore, once these instruments and objects have come into existence, they take on a life of their own, available for appropriation to other ends, to other metaphoric schemes.

Consider, for example, the fate of genetic transposition. McClintock's search for this phenomenon was stimulated by her interest in the dynamics of kinship and interdependency; it was made visible by an analytic and interpretive system premised on "a feeling for the organism," on the integrity and internal agency of the organism. To McClintock, transposition was a wedge of resistance on behalf of the organism against control from without. But neither she herself nor her analytic and interpretive framework could prevent the ultimate appropriation of this mechanism, once exhibited, to entirely opposite aims—as an instrument for external control of organic forms by genetic engineers.

McClintock's vision of science was unarguably productive for her, and it has been seen to have great aesthetic and emotional appeal for many scientists. But it must be granted that her success pales before that of mainstream (molecular) biology. In the last few years (in part thanks to the techniques derived from genetic transposition itself), it is the successes and technological prowess of molecular biology rather than of McClintock's vision of science that have captured the scientific and popular imagination. These successes, and this prowess, cannot be ignored.

We may be well persuaded that the domain of natural phenomena is vastly larger than the domain of scientific theory as we know it, leaving ample room for alternative conceptions of science; that the accumulated body of scientific theory represents only one of the many ways in which human beings, including the human beings we call scientists, have sought to make sense of the world; even that the successes of these theories are highly local and specific. Yet, whatever philosophical accounts we might accept, the fact remains that science as we know it works exceedingly well. The question is, Can any other vision of science be reasonably expected to work as well? Just how plastic are our criteria of success?

Feminists (and others) may have irrevocably undermined our sense of innocence about the aspiration to dominate nature, but they/we have not answered the question of just what it is that is wrong with dominating nature. We know what is wrong with dominating persons—it deprives other subjects of the right to express their own subjectivities—and we may indeed worry about the extent to which the motivation to dominate nature reflects a desire for domination of other human beings.[12] But a salient point of a feminist perspective on science derives precisely from the fact that nature is not in fact a woman. A better pronoun for nature is surely "it," rather than "she." What then could be wrong with seeking, or even achieving, dominion over things per se?

Perhaps the simplest response is to point out that nature, while surely not a woman, is also not a "thing," nor is it even an "it" that can be delineated unto itself, either separate or separable from a speaking and knowing "we." What we know about nature we know only through our interactions with, or rather, our embeddedness in it. It is precisely because we ourselves are natural beings—beings *in* and *of* nature—that we *can* know. Thus, to represent nature as a "thing" or an "it," is itself a way of talking, undoubtedly convenient, but clearly more appropriate to some ends than to others. And just because there is no one else "out there" capable of choosing, we must acknowledge that these ends represent human choices, for which "we" alone are responsible. One question we need to ask is thus relatively straightforward: What are the particular ends to which the language of objectification, reification, and domination of nature is particularly appropriate, and perhaps even useful? And to what other ends might a different language—of kinship, embeddedness, and connectivity, of "feeling for the organism"—be equally appropriate and useful? But we also need to ask another, in many ways much harder, question: How do the properties of the natural world in which we are embedded constrain our social and technical ambitions? Just what is there in the practices and methods of science that permit the realization of certain hopes but not others?

Earlier in this essay, I attempted to describe the shift in mind-set from working scientist to feminist critic. But to make sense of the successes of science, however that success is measured, the traversal must also be charted in reverse: Feminist critics of science, along with other analysts of science, need to reclaim access to the mindset of the working scientist, to what makes their descriptions seem so compelling.

For this, we need to redress an omission from many of our analyses to date that is especially conspicuous to any working scientist: attention to the material constraints on which scientific knowledge depends, and correlatively, to the undeniable record of technological success that science as we know it can boast. If we grant the force of belief, we must surely not neglect the even more dramatic force of scientific "know-how." Although beliefs, interests, and cultural norms surely can, and do, influence the definition of scientific goals, as well as prevailing criteria of success in meeting those goals, they cannot in themselves generate either epistemological or technological success. Only where they mesh with the opportunities and constraints afforded by material reality can they lead to the generation of effective knowledge. Our analyses began with the question of where, and how, does the force of beliefs, interests, and cultural norms enter into the process by which effective knowledge is generated; the question that now remains is, Where, and how, does the nonlinguistic realm we call *nature* enter into that process? How do "nature" and "culture" interact in the production of scientific knowledge? Until feminist critics of science, along with other analysts of the influence of social forces on science, address this question, our accounts of science will not be recognizable to working scientists.

The question at issue is, finally, that of the meaning of science. Although we may now recognize that science neither does nor can "mirror" nature, to imply instead that it mirrors culture (or "interests") is not only to make a mockery of the commitment to the pursuit of reliable knowledge that constitutes the core of any working scientist's self-definition, but also to ignore the causal efficacy of that commitment. In other words, it is to practice an extraordinary denial of the manifest (at times even life threatening) successes of science. Until we can articulate an adequate response to the question of how "nature" interacts with "culture" in the production of scientific knowledge, until we find an adequate way of integrating the impact of multiple social and political forces, psychological predispositions, experimental constraints, and cognitive demands on the growth of science, working scientists will continue to find their more traditional mind-sets not only more comfortable, but far more adequate. And they will continue to view a mind-set that sometimes seems to grant force to beliefs and interests but not to "nature" as fundamentally incompatible, unintegrable, and laughable.

Notes

1. A somewhat different view is given by Tom Laqueur (1990).
2. See, for example, Gayle Rubin (1975).
3. See, for example, Rubin (1975) and Catherine MacKinnon (1988).
4. Of course, scientists are not the only ones who persist in such a mistranslation; it is also made by many others, and even by some feminists who are not themselves scientists. It is routinely made by the popular press. The significant point here is that this mistranslation persists in the minds of most women scientists even after they are alerted to the (feminist) distinction between sex and gender.
5. Indeed, a striking number of those feminist critics who began as working scientists have either changed fields altogether or have felt obliged to at least temporarily interrupt their work as laboratory or "desk" scientists (I am thinking, for example, of [the late] Maggie Benston, Ruth Hubbard, Marian Lowe, Evelynn Hammonds, Anne Fausto-Sterling, and myself).
6. In large part, stimulated by the publication of Thomas S. Kuhn's *The Structure of Scientific Revolutions*, in 1962.
7. See, for example, Galison (1988); Pickering (1984); Shapin and Schaffer (1985); Smith and Wise (1989).
8. The discussion that follows begins with a recapitulation of my remarks in Keller (1985: 129–32).
9. For an especially interesting discussion of this general phenomenon, see Markus (1987).
10. O. E. D., s.v. "law." The discussion here is adapted from the introduction to Part III, Keller (1985).
11. McClintock's own words, as well as the title of my book on this subject, Keller (1983).
12. See Keller (1985), Part II.

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