# Introduction

In the mid-1970s—the very early days of my shift from "doing" science to thinking about what goes into the production of scientific knowledge—the notion that both that process and its products reflect social norms seemed very radical, not only to my scientific colleagues, but even to most historians and philosophers of science. At that time, Kuhn's *The Structure of Scientific Revolutions* (1962) was still widely viewed as "revolutionary" (Wade 1977), perhaps second only to the even more subversive (and certainly more sweepingly irreverent) work of Paul Feyerabend. Whatever his role in undermining the received view of scientific knowledge, for Kuhn, "science" remained a distinctive endeavor; its internal dynamics, even if neither autonomous nor impervious, still conceptually distinguishable from social ("extrascientific") factors. Perhaps because of my own history as a scientist, I too took as a given the distinctiveness of the values and the practices responsible for the growth of scientific knowledge. From my earliest inquiries into "gender and science," even while I focused on the subjective dimensions of scientific commitments to "objectivity," I distinguished between beliefs about science I called "mythlike" and those commitments I saw as "indispensable." In the introduction to *Reflections on Gender and Science*, I wrote:

> [S]cientists' shared commitment to the possibility of reliable knowledge of nature, and to its dependence on experimental replicability and logical coherence, is an indispensable prerequisite for the effectiveness of any

scientific venture. What needs to be understood is how these conscious commitments (commitments we can all share) are fueled and elaborated, and sometimes also subverted, by the more parochial social, political, and emotional commitments (conscious or not) of particular individuals and groups (Keller 1985:11).

In the late 1970s, when I began my inquiry into the psychosocial (historically "masculinist") dimensions of our dominant scientific traditions, such a venture seemed (to me and to others) not only more difficult but potentially even more radical than Kuhn's, perhaps especially insofar as it was aimed at transforming these dominant traditions—as I eventually put it, at "taming [their] hegemony." But relative to the starting point of many critics writing today, it can appear quaintly conservative. My aim was neither the repudiation of science nor its replacement, but rather "the reclamation, from within . . . of science as a human instead of a masculine project, and the renunciation of the division of emotional and intellectual labor that maintains science as a male preserve." Even my critique of the ideal of objectivity was intended "to help clarify the substructure of science in order to preserve the things that science has taught us, in order to be more objective" (Keller 1985:178). In other words, though I well understood that "science" has never been a unified endeavor,[1] I nonetheless regarded it as sufficiently coherent to be distinguishable from other endeavors, and correspondingly, as distinctively valuable. Not surprisingly, the confidence and respect I continued to have for "science" was lost on those critics—mostly from the ranks of "working scientists," but sometimes also from the history and philosophy of science—who read in my work only an attack on both *science* and *objectivity*.

Over the last decade, however, the autonomy (as well as the number) of scholars who study science as observers rather than as practitioners has grown considerably, and a sea change has occurred, among those of us whose academic occupation it has come to be to think about such things, in our understanding of the growth of scientific knowledge. Increasingly, historians, philosophers, and sociologists of science writing today take the social location of the

[1] As Hacking puts it, "The sciences . . . are composed of a large number of only loosely overlapping little disciplines many of which in the course of time cannot even comprehend each other" (1983:6).





natural sciences as their starting point, exploring the influence of economic, political, and cultural factors at every stage of the production of scientific knowledge—on how questions are posed, how research programs come to be legitimated, how theoretical disputes are resolved, on "how experiments end."[2] At every level, choices can be seen to be made that are social *even as* they are cognitive and experimental. For many, especially for those who think of themselves as "post-Kuhnians," the very distinction between internal ("scientific") and external ("extrascientific") dynamics has come to be thought of as an ideological phantasm. Steven Shapin observes that "there is as much society inside science as outside"; in a similar spirit, Bruno Latour argues that the divide between human and non-human actors (or between Society and Nature) is itself a product of negotiations and resource networks. In such an ambience, any residual allegiance to "science" or appeal to "nature" (including my own) has come to be seen as retrograde, anachronistic, even as "foundationalist"; visions of transforming or "reclaiming" science (rather than, for example, simply dethroning it) as correspondingly innocent and naive.

Yet I am not about to recant. Along with many of my colleagues, I continue in the effort to articulate, and occupy, a "middle of the road" position. Indeed, as *science studies* has tended toward the dissolution of all distinctive boundaries demarcating the sciences, toward an ever greater focus on institutions, politics, cultural contexts, and language, I have found myself leaning more and more in an opposite direction—feeling the need for more attention to the logical and empirical constraints that make scientific claims so compelling to scientists, as well as to the technological prowess that makes them so compelling to the world at large. There is no question that the work of historians, philosophers, and sociologists of science over the last two decades has made it impossible to think of "nature" either as simply given or as available to any kind of "mirroring." What we know or claim to know about the natural world comes to us in our own constructions—constructions that are inevitably shaped by our cultural and linguistic frames. If I nonetheless continue at times to refer to something called "nature," it is for lack of a better word to denote the world of prelinguistic and pretheoretical

[2] The title of Peter Galison's recent book (1987).

phenomena, constraints, and opportunities in which we reside, and with which we, as part of that world, must negotiate our survival, our accounts and representations, and, perhaps above all, our technological achievements.

Indeed, it is precisely because of the testimony of our technological prowess, because science as we know it "works" so extraordinarily well (that is, because it so effectively meets so many of the goals set for it), that I have become increasingly uncomfortable with the limitations of my initial preoccupations with scientific representations of "nature," and correspondingly compelled to think about the force and efficacy of these representations. The concept of force, however—as I note in "Critical Silences in Scientific Discourse" (this volume)—implies directionality as well as magnitude. Accordingly, the need is not simply to account for the efficacy of scientific knowledge, but, at the same time, to examine critically what it is that knowledge works so well *at*. Inevitably, this entails a shift from thinking about science as "representing" to thinking about representations themselves as tools for "intervening." As Hacking writes, "Theories try to say how the world is. Experiment and subsequent technology change the world. *We represent in order to intervene, and we intervene in the light of representations*" (1983:31, my emphasis). And part of what it means to think about the force and efficacy of scientific knowledge is to think about the force and efficacy of language. It means to take seriously the scientist's inevitable (and usually exasperated) question: "But what does language have to do with what we actually do?"

My "linguistic turn," like Hacking's,[3] is thus toward the constitutive role of language in scientific thought and action in the practice of working scientists. As such, it represents a shift from my earlier preoccupation with the frailties of description, and in one respect at least, a departure from my initial confidence in the possibility of identifying certain beliefs as "mythlike," as distinct from other beliefs that are, by implication, "myth-free." Such a notion now seems to me suspiciously reminiscent of the old demarcation between "truth" and "ideology," or between "good science" and "value-

[3] And, also, of course, Kuhn's, Cartwright's, and that of numerous others. In addition to Hacking (1983) and Kuhn (1989), see, especially, the papers of Buchwald, Cartwright, Hacking, et al., in Kuhn's forthcoming *Festschrift*.





laden science," demarcations that are themselves residues of the copy theory of truth.

Since "nature" is only accessible to us through representations, and since representations are necessarily structured by language (and hence, by culture), no representation can ever "correspond" to reality. At the same time, however, some representations are clearly better (more effective) than others. The question that has plagued much recent philosophy of science is how to make sense of this latter statement in the absence of a copy theory of truth. But the difficulty dissolves if we search for the meaning of "better" in a comparison of the uses to which different representations can be put, that is, in the practices they facilitate. From such a perspective, scientific knowledge is value-laden (and inescapably so) just because it is shaped by our choices—first, of what to seek representations *of*, and second, of what to seek representations *for*. Since uses and practices are obviously not value-free, why should we even think of equating "good" science with the notion of "value-free"? Far from being "value-free," *good science* is science that effectively facilitates the material realization of particular goals, that does in fact enable us to change the world in particular ways. Some of the goals it enables us to realize are goals that almost all people might share, others are more restricted. (*Good science* might also enable the realization of goals that most people would reject—that is, that most people would regard as bad.)

In this sense, *good science* typically works to bring the material world in closer conformity with the stories and expectations that a particular "we" bring with us as scientists embedded in particular cultural, economic, and political frames (see, for example, Keller 1989). What distinguishes it from other successful institutions and practices is precisely its disciplined interaction with the material constraints and opportunities supplied by that which, for lack of a better word, I still call "nature." Scientific "method" is just the name we give to the assorted techniques that scientists have found effective for assessing, subverting, or exploiting those constraints and opportunities, and "disciplines" the name we give to local and institutionalized conventions about how best to proceed toward particular, more or less collectively endorsed, goals.[4]

[4] The manifest difficulty historians of science have in clearly delineating particular

We have come to recognize the *course* of science as a social product, and in that recognition, we have gained the freedom to look critically at its ends, to resist scientists' own tendency to naturalize their current habits and directions. All too well we have learned that "good science" may be efficacious, even while directed to ends we might deplore. But we cannot use our critical freedom to social effect without a clear understanding of science's virtues and strengths—above all, of the nature and conditions of its efficacy.

In brief, then, the course of science is mediated by its sources of external support, by institutional self-reinforcement, and by language. Language simultaneously reflects and guides the development of scientific models and methods. It also helps shape the ends toward which science aims, if only because we gravitate to problems we're equipped to formulate and solve. But language is hardly free. What counts as a usable, effective, and communicable representation is constrained, on the one hand, by our social, cultural, and disciplinary location, and on the other hand, by the recalcitrance of what I am left, by default, to call "nature." The language of scientists is limited by what they learn to think and say as individuals, as members of a discipline, and as members of one or, more usually, several larger communities; it is simultaneously limited by what they can do, individually and collectively, in their ongoing material interactions with the objects of their inquiry.

The essays collected in this volume are explorations within these limits. They all, in various degrees and in various ways, reflect the shifts in my concerns that have followed upon the publication of *Reflections*. Some are directly concerned with questions of gender, others are not. Before introducing them individually, let me therefore briefly review the shifting place of gender both in my own work, and in science studies more generally.

The route from the practice of science to the study of its history and philosophy is a well-traveled one, and people make that transition for all sorts of reasons. For me, involvement in the 1970s in the development of experimental, interdisciplinary college pro-

disciplines (especially, perhaps, in the life sciences—molecular biology and developmental biology are prime examples) attests to the de facto variability that persists in spite of and even between different institutional attempts to ensure consensus. (For relevant discussions of just these issues, see Soderqvist 1986 and Lenoir 1992.)





grams, in the women's movement, and in psychoanalysis were all critical. It was the intersection of these three experiences that made it possible to ask, and to take seriously, questions about the history and philosophy of science that, for me at least, were entirely new. My debt to the closely linked set of concerns that were emerging around me, soon called "feminist theory," was especially great. But "feminist theory" was at one and the same time both too broad and too narrow a label for my own questions. As I cast about for a more accurate tag for these, the term "psychosociology of scientific knowledge" seemed apt, but unwieldy. Moreover, as the work evolved, the role of gender norms—a category at once socially constructed and individually internalized—increasingly came to the fore in the arguments I was developing; "Gender and Science" emerged as the label that stuck. But what this term gained in catchiness, it lost in fidelity to my original intent, for, from the very beginning, I had seen "gender" as but one route into a far larger set of issues.

Over the last decade, the term "Gender and Science" has risen to conspicuous prominence (both in this country and abroad) as a catch-all designator for all studies in the history, philosophy, and sociology of science having to do with women, sex, or gender. Undoubtedly, both the expanded usage of this term and the growing visibility of the work it denotes attest to the positive impact contemporary feminism has begun to have on science studies. At the same time, however, and in the same process, the term itself has become increasingly problematic.[5] In large part, the problem derives from the tacit slippage between "gender" and "women" invited by this expanded usage, and the corresponding elision of the many differences in disciplinary and political interests addressed by this body of work.[6] But another problem, of particular urgency from the perspective of my own interests, has also become evident: While the label "Gender and Science" may have proven unduly expansive in relation to the needs of feminists, it has proven unduly restrictive in relation to the needs of science studies: It has tended to "ghet-

[5] The difficulty of nomenclature quickly becomes apparent if one attempts to sort through recent bibliographies in the history of science under the title "Gender and Science" and tries to distinguish the various literatures—on women in science, on scientific constructions of women, or on the role of gender ideology in science—from one another.

[6] For an elaboration of these points, see Keller (1992).

toize" inquiries about "gender" and insulate them from other pursuits in the history, philosophy, and sociology of science.

Accordingly, part of the motivation for this book is to distinguish the particular strand of "Gender and Science" studies concerned with the role of gender ideologies *in* science, and to embed it in a more general historiographic and philosophical pursuit. The essays included here were written between 1985 and 1991 and are arranged by continuities of theme rather than by chronology.

The essay that appears first, "Gender and Science: An Update," is intended as an overview of the evolution of my own thinking on this subject and inevitably reflects the thinking and writings of many others. Because it is an overview, it stands apart from the other essays as Part I. Beginning explicitly with the meaning of "gender" and the questions that the lens of gender brought into focus in feminist theory, I attempt to sketch the logic of the relation between these questions and certain more general issues in the history and philosophy of science as that logic emerged in my pursuit of the implications of the questions raised in *Reflections.*

Part II includes a series of essays that can roughly be grouped under the rubric of "secrets." The first in this section, "From Secrets of Life to Secrets of Death," was originally written for presentation at the "Kanzer Seminar in Psychoanalysis and the Humanities" (New Haven, Spring, 1986). It begins by invoking the psychoanalytic perspective that was so prominent in *Reflections,* but that is absent from the review offered in Part I, as well as from all the subsequent essays. This essay is included here, with its psychoanalytic orientation intact, partly to make a methodological/political point. Even though I have since found it strategically impossible to proceed with psychodynamic explorations of scientific postures, I stand by that earlier work; I continue to believe in the value, and perhaps even in the ultimate indispensability, of psychodynamic approaches to the study of science, notwithstanding the ill favor with which they are currently received. For both good and bad reasons, most historians, philosophers, and sociologists of science have come to regard psychoanalysis, and even the very idea of the individual subject on which it depends, as something of an embarrassment. However, as even this essay should make clear, the "subject" on which at least traditional psychoanalysis depends is in no sense either independent of or an alternative to other forms of social structure (or





"discourse"): Individual subjects are as much constituted by social structures as social structures are constituted by individual subjects, and the occlusion of one is as serious an error as the occlusion of the other, in science studies as elsewhere. Psychoanalysis, despite its problems and deficiencies, continues to provide some of our only tools for thinking about both individual and collective subjectivities.

But the principal point of this essay lies elsewhere, in an attempt to ground the analysis of subjectivity in its sociopolitical and material effects. Like the other essays in Part II, it reflects my growing preoccupation with the material consequences of science, nowhere more dramatically in evidence than in the successes of nuclear physics and molecular biology, that is, in the production of technologies of life and death. My starting point is the analysis of the language of "secrets," and the various uses to which this metaphor can be put.

The next essay, "Secrets of God, Nature, and Life," continues this theme by attempting to trace the historical trajectory of the trope of "secrets" from the earliest days of modern science to the present. A close rereading of Robert Boyle's classic essay, "A Free Inquiry into the Vulgarly Received Notion of Nature," illustrates the use of metaphor in the reorientation between men, women, and nature that Boyle explicitly sought to effect, and that is both reflected and facilitated by the shifting referent of "secrets" in the larger discourse of his time.

"Critical Silences in Scientific Discourse" also reflects my continuing preoccupation with technologies of life and death. It was written for presentation at the Institute for Advanced Study at Princeton during my year in residence there (1987–88). Much of that year was devoted to a search for a conceptual framework adequate to an account for the efficacy of scientific representations while at the same time acknowledging their cultural dependence—that would enable me to steer clear of the Scylla of "social relativism" and the Charybdis of "scientific realism." As the time approached for my public presentation, to an audience composed largely of physicists and social scientists, I became increasingly conscious of the extraordinary divergence between the ways in which these two groups thought about science. Accordingly, part of the purpose of this essay was to make this gap visible, on the assumption that, once visible, it would be seen as insupportable. That is, with the naivete of a new convert, I hoped to make what I had come to see as the critical problem in science studies evident, and therefore compelling, to both groups. I surely cannot claim to have succeeded in this ambition; however, I *have* subsequently found the framework sketched out in this essay to be useful. Especially, it has provided me with a way of thinking about the intersection of cognitive, psychosocial, economic, political, and technical interests against which the efficacy of any research trajectory needs to be judged.

This framework clearly informs the next essay, "Fractured Images of Science, Language, and Power." Here I pursue the question only raised in the previous essay, namely, that of how particular social and material ambitions have helped to guide the choice of scientific theory or representation in the history of molecular genetics. More specifically, I focus first on the influence the anticipated achievements of nuclear physics had on the thinking of the geneticist, H. J. Muller, and later on the influence the actual demonstration of those achievements had on the consolidation of much of Muller's theoretical agenda in the dominant research program of molecular biology.

The essays comprising Part III of this book are more technical, and somewhat less grandiose in their ambitions. All four address the question of how language works in the specification of actual research agendas—in the first three essays, of evolutionary biology, and in the last, in molecular biology. In Part I of "Language and Ideology in Evolutionary Theory," I explore the ways in which the language of competition, with all its vicissitudes, has served as a vehicle for the importation of cultural norms into evolutionary biology—in particular and most conspicuously, of the "Hobbesian" norm of "all against all" into mathematical ecology. "Competition" is a term with both technical and colloquial meanings, and slippage between the two is both routine and consequential. It is precisely such slippage that "permits the simultaneous transfer and denial of its colloquial connotations" into technical contexts.

In Part II of the essay, my focus is on population genetics where, I argue, "The Language of Reproductive Autonomy" has critically bolstered the traditionally individualist bias of evolutionary biology. Analysis of the semipopular and technical uses of "competition" (in the first part of the essay), and of the language of reproductive autonomy (in the second), enables me to consider the extent to which animate forms of contemporary biological discourse have been "suc-





cessfully deanimated and mechanised," and the extent to which "their mechanical representations [have] themselves been inadvertently animated, subtly recast in particular images of man."[7]

The same general argument is continued in "Demarcating Public from Private Values in Evolutionary Discourse," where it is extended to an analysis of how arguments involving kinds of "individual" other than the organism (for example, the species, the group, or the gene) are constructed. Here the focus is less on definitions of "competition" and "individual" (and the inadvertent slippage between ordinary and technical usage to which these terms are prey) and more on the ways in which tacit assumptions about the necessary properties of "individual" have affected recent debates in population genetics and mathematical ecology, particularly in the implementation of the chosen methodology of evolutionary biology, "methodological individualism."

The closing essay focuses on a recent debate not in evolutionary biology, but in molecular genetics. "Between Language and Science: The Question of Directed Mutation in Molecular Genetics" returns to a question that was a leitmotif in *A Feeling for the Organism* (Keller 1983): the repeated return of the "ghost of Lamarck" in twentieth-century biology, and its relation to the "central dogma" of molecular genetics. The clear implication of McClintock's work on transposition is that "the genetic apparatus . . . is a more complex system, with more complex forms of feedback, than had been previously thought. Perhaps," I went on to suggest,

> the future will show that its internal complexity is such as to enable it not only to program the life cycle of the organism, with fidelity to past and future generations, but also to reprogram itself when exposed to sufficient stress—thereby effecting a kind of "learning" from the organism's experience. Such a picture would be radical indeed, and it would be one that would do justice to McClintock's vision: it would imply a concept of genetic variation that is neither random nor purposive—and an understanding of evolution transcending that of both Lamarck and Darwin (pp. 194–95).

In this essay, I trace the linguistic construction of the dichotomy

[7] It is the work of these essays that provided my primary incentive for *Keywords in Evolutionary Biology* (Keller and Lloyd 1992).

between "random" and "purposive" (and between "Lamarckism" and "Darwinism"), and the persistence of this opposition, in the exchanges initiated by John Cairns's most recent challenge to neo-Darwinian orthodoxy (1988).

The essays collected in this volume do not provide answers to the questions that occupy contemporary historians, philosophers, and sociologists of science. Instead, they reveal critical convergences between such questions and those that have grown directly out of feminist inquiries. It is my hope that, by illustrating some of the complex interconnectivities between gender, language, and science, they can contribute to our collective and ongoing efforts to understand how science works within the plenitude of sometimes converging and sometimes conflicting human desires.



# PART I

# 1

# Gender and Science: An Update

## Introduction

### *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

This essay adapted from Keller (1990), with excerpts from Keller (1987).

[1] A somewhat different view is given by Tom Laqueur (1990).

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.[4] From this perspective, gender

[2] See, for example, Gayle Rubin (1975).

[3] See, for example, Rubin (1975) and Catherine MacKinnon (1988).

[4] In the most recent literature, discussions of gender have become yet more so-





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.*[5] 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

phisticated as feminist scholars have begun to shift their focus away from the unifying force of gender norms within particular culturally homogeneous systems, to such inhomogeneities as class and race prevailing within ostensibly unitary cultures. But within the confines of those few worlds that can be said to be culturally homogeneous—such as, for example, the almost entirely white, upper and middle-class, predominantly Eurocentric world of modern science—analysis of the force of gender and gender norms remains relatively straightforward. Indeed, the very exclusivity of this tradition provides one of the few cases in which, precisely because of its racial and class exclusivity, the variables of race and class can be bracketed from the analysis. It must be remembered, however, that the concept of "gender" that appears in such an analysis is one that is restricted to a particular subset of "Western" culture.

[5] See, for example, Sandra Harding (1986) for a useful summary of these multiple yet interacting meanings of the term gender.

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.[6]

[6] Of course, scientists are not the only ones who persist in such a mistranslation;





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.[7]

I need only recall my own trajectory from practicing scientist to feminist critic to appreciate the magnitude of difference between these two mind-sets, as well as the effort required to traverse that difference. In the hope that my experience, with its inevitable idiosyncracies, might prove helpful in furthering our understanding of the more general problem, I offer a reconstruction of that trajectory.

## *From Working Scientist to Feminist Critic*

I begin with three vignettes, all drawn from memory.

1965. In my first few years out of graduate school, I held quite conventional beliefs about science. I believed not only in the possibility of clear and certain knowledge of the world, but also in the uniquely privileged access to this knowledge provided by science in general, and by physics in particular. I believed in the accessibility of an underlying (and unifying) "truth" about the world we live in, and I believed that the laws of physics gave us the closest possible approximation of this truth. In short, I was well trained in both the traditional realist worldviews assumed by virtually all scientists and in the conventional epistemological ordering of the sciences. I had, after all, been trained, first, by theoretical physicists, and later, by

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.

[7] 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).

molecular biologists. This is not to say that I lived my life according to the teachings of physics (or molecular biology), only that when it came to questions about what "really is," I knew where, and how, to look. Although I had serious conflicts about my own ability to be part of this venture, I fully accepted science, and scientists, as arbiters of truth. Physics (and physicists) were, of course, the highest arbiters.

Somewhere around this time, I came across the proceedings of the first major conference held in the United States on "Women and the Scientific Professions" (Mattfield and Van Aiken 1965)—a subject of inevitable interest to me. I recall reading in those proceedings an argument for more women in science, made by both Erik Erikson and Bruno Bettelheim, based on the invaluable contributions a "specifically female genius" could make to science. Although earlier in their contributions both Erikson and Bettelheim had each made a number of eminently reasonable observations and recommendations, I flew to these concluding remarks as if waiting for them, indeed forgetting everything else they had said. From the vantage point I then occupied, my reaction was predictable: To put it quite bluntly, I laughed. Laws of nature are universal—how could they possibly depend on the sex of their discoverers? Obviously, I snickered, these psychoanalysts know little enough about science (and by implication, about truth).

1969. I was living in a suburban California house and found myself with time to think seriously about my own mounting conflicts (as well as those of virtually all my female cohorts) about being a scientist. I had taken a leave to accompany my husband on his sabbatical, remaining at home to care for our two small children. Weekly, I would talk to the colleague I had left back in New York and hear his growing enthusiasm as he reported the spectacular successes he was having in presenting our joint work. In between, I would try to understand why my own enthusiasm was not only not growing, but actually diminishing. How I went about seeking such an understanding is worth noting: What I did was to go to the library to gather data about the fate of women scientists in general—more truthfully, to document my own growing disenchantment (even in the face of manifest success) as part of a more general phenomenon reflecting an underlying misfit between women and





science. And I wrote to Erik Erikson for further comment on the alarming (yet somehow satisfying) attrition data I was collecting. In short, only a few years after ridiculing his thoughts on the subject, I was ready to at least entertain if not embrace an argument about women in, or out of, science based on "women's nature." Not once during that entire year did it occur to me that at least part of my disenchantment might be related to the fact that I was in fact not sharing in the *kudos* my colleague was reaping for our joint work.

1974. I had not dropped out of science, but I had moved into interdisciplinary, undergraduate teaching. And I had just finished teaching my first women's studies course when I received an invitation to give a series of "Distinguished Lectures" on my work in mathematical biology at the University of Maryland. It was a great honor, and I wanted to do it, but I had a problem. In my women's studies course, I had yielded to the pressure of my students and colleagues to talk openly about what it had been like, as a woman, to become a scientist. In other words, I had been persuaded to publicly air the exceedingly painful story of the struggle that had actually been[8]—a story I had previously only talked about in private, if at all. The effect of doing this was that I actually came to *see* that story as public, that is, of political significance, rather than as simply private, of merely personal significance. As a result, the prospect of continuing to present myself as a disembodied scientist, of talking about my work as if it had been done in a vacuum, as if the fact of my being a woman was entirely irrelevant, had come to feel actually dishonest.

I resolved the conflict by deciding to present in my last lecture a demographic model of women in science—an excuse to devote the bulk of that lecture to a review of the many barriers that worked against the survival of women as scientists, and to a discussion of possible solutions. I concluded my review with the observation that perhaps the most important barrier to success for women in science derived from the pervasive belief in the intrinsic masculinity of scientific thought. Where, I asked, does such a belief come from? What is it doing in science, reputedly the most objective, neutral, and

[8] This story was subsequently published in Ruddick and Daniels's *Working It Out* (1977).

abstract endeavor we know? And what consequences does that belief have for the actual doing of science?

In 1974 "women in science" was not a proper subject for academic or scientific discussion; I was aware of violating professional protocol. Having given the lecture—having "carried it off"—I felt profoundly liberated. I had passed an essential milestone.

Although I did not know it then, and wouldn't recognize it for another two years, this lecture marked the beginning of my work as a feminist critic of science. In it I raised three of the central questions that were to mark my research and writing over the next decade. I can now see that, with the concluding remarks of that lecture, I had also completed the basic shift in mind-set that made it possible to begin such a venture. Even though my views about gender, science, knowledge, and truth were to evolve considerably over the years to come, I had already made the two most essential steps: I had shifted attention from the question of male and female nature to that of *beliefs about* male and female nature, that is, to gender ideology. And I had admitted the possibility that such beliefs could affect science itself.

In hindsight, these two moves may seem simple enough, but when I reflect on my own history, as well as that of other women scientists, I can see that they were not. Indeed, from my earlier vantage point, they were unthinkable. In that mind-set, there was room neither for a distinction between sexual identity and beliefs about sexual identity (not even for the prior distinction between sex and gender upon which it depends), nor for the possibility that beliefs could affect science—a possibility that requires a distinction analagous to that between sex and gender, only now between nature and science. I was, of course, able to accommodate a distinction between belief and reality, but only in the sense of "false" beliefs—that is, mere illusion, or mere prejudice; "true" beliefs I took to be synonomous with the "real."

It seems to me that in that mind-set, beliefs per se were not seen as having any real force—neither the force to shape the development of men and women, nor the force to shape the development of science. Some people may "misperceive" nature, human or otherwise, but properly seen, men and women simply *are*, faithful reflections of male and female biology—just as science simply *is*, a faithful reflection of nature. Gravity has (or is) a force, DNA has





force, but beliefs do not. In other words, as scientists, we are trained to see the locus of real force in the world as physical, not mental.

There is of course a sense in which they are right: Beliefs per se cannot exert force on the world. But the people who carry such beliefs can. Furthermore, the language in which their beliefs are encoded has the force to shape what others—as men, as women, and as scientists—think, believe, and, in turn, actually do. It may have taken the lens of feminist theory to reveal the popular association of science, objectivity, and masculinity as a statement about the social rather than natural (or biological) world, referring not to the bodily and mental capacities of individual men and women, but to a collective consciousness; that is, as a set of beliefs given existence by language rather than by bodies, and by that language, granted the force to shape what individual men and women might (or might not) do. But to see how such culturally laden language could contribute to the shaping of science takes a different kind of lens. That requires, first and foremost, a recognition of the social character (and force) of the enterprise we call "science," a recognition quite separable from—and in fact, historically independent of—the insights of contemporary feminism.

### *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).[9] 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.[10] 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.

[9] In large part, stimulated by the publication of Thomas S. Kuhn's *The Structure of Scientific Revolutions*, in 1962.

[10] See, for example, Galison (1988); Pickering (1984); Shapin and Schaffer (1985); Smith and Wise (1989).





## Language and the Doing of Science[11]

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.[12] 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

[11] The discussion that follows begins with a recapitulation of my remarks in Keller (1985:129–32).

[12] For an especially interesting discussion of this general phenomenon, see Markus (1987).

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.[13] 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

[13] Mary Hesse points out, "Neurons come in billions and their possible linkages in megabillions, while the words of a language come only in thousands and sentences cannot in a lifetime be long enough to match the antics of the neurons. There can't be a word or a sentence to cover every particular thing" (1989, p. E24).

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."[14] 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.

[14] O. E. D., s.v. "law." The discussion here is adapted from the introduction to Part III, Keller (1985).





## 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."[15] 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."

[15] McClintock's own words, as well as the title of my book on this subject, Keller (1983).





## 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.[16]

[16] For McClintock, there remains a very serious question about the extent to which such convergences can in fact be said to have taken place—in particular, about the extent to which the meaning she attached to "transposition" is consonant with the current use of that term. For further discussion of this point, as well as of other issues pertinent to McClintock's research, see Keller (1983).

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.[17] 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

[17] See Keller (1985), Part II.





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 mind-set 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.



# PART II