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# Women, Science, and Popular Mythology

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## INTRODUCTION

During the 1960s (a period sometimes referred to as the post-Sputnik era), the United States witnessed a major campaign to develop scientific talent. Stimulated by the growing concern for "lost" talent, a now familiar but at the time shocking fact emerged into public consciousness: The proportion of women in American science, while never high, had declined steadily since the 1920s and was then, in the mid 60s, only half of what it had been 40 years earlier. What was shocking about this fact was that it jarred our confidence in the progressive unfolding of egalitarian ideals. It brought to the fore a contradiction that we had been living with for many years: On the one hand, children were educated according to the ostensible principle of sexual equality; and, on the other hand, a dramatic opposition prevailed between what were seen as appropriate roles for adult men and women.

In the years that followed, the subject of women in science has received a great deal of attention. Data has been collected, reported, and analyzed by numerous individuals, government agencies, and special committees of professional societies; research and promotional efforts have been supported by the National Science Foundation and the National Institute of Education; and affirmative action programs have been instituted across the nation. Throughout this period, the reasons for the low representation of women in science, particularly in the upper echelons of the scientific community, were discussed and analyzed in numerous articles (Keller 1974, Kistiakowsky 1980, Rossi 1966, White 1970). In my own article, after enumerating the various impediments confronting a woman in science, I suggested that perhaps the single most powerful inhibitor was the widespread belief in the intrinsic masculinity of scientific thought (Keller 1974).

Today, after a decade of consciousness raising, active recruiting, and affirmative action, it is appropriate to ask: How much has changed? The answer depends on the focus of one's analysis, but a number of authors

have concluded that change has been disappointingly slow. In 1975, Betty Vetter (1975) wrote:

> Women scientists . . . have not achieved parity of opportunity despite the forces of affirmative action. The gains in the participation of women in the U.S. scientific enterprise are pitifully small, and the obstacles still standing in the path of those who wish to enter and participate fully in that enterprise always have been and still are enormous (p. 713).

In a more recent review of women in physics, Vera Kistiakowsky (1980) writes:

> In summary, the predominant impression gained from looking at the statistics is that there has not been very much change since the beginning of the century, or since the 1971 American Physical Society study (p. 35).

She notes, however, along with other authors, certain potentially important exceptions: The percentage of Ph.D.'s awarded to women has increased (finally exceeding what it was in 1920) and is still increasing; a few more women are on the faculties of the major research institutions; some overall increase in the proportion of women at the assistant professor level can be seen. On all levels, however, the numbers are still very small. Perhaps of greatest importance are changes in cultural attitudes we have witnessed over the past decade. The general acceptance of women in traditionally male roles has increased significantly, and the message that science is a man's field has become dramatically muted. No doubt, the growth in the number of women in undergraduate scientific and technological courses of study reflects these changes, suggesting the possibility of our seeing a greater proportion of women scientists in the future.

There are several reasons, however, to suspect the survival of deep cultural forces which continue to alienate women from science. Not only has the employment profile of women in the scientific professions remained essentially unchanged, but the number of women entering these professions is still relatively small. At the Massachusetts Institute of Technology (M.I.T.), for example, where the increase in the number of women students has been truly dramatic, the proportion today is still only about 19 percent. When compared with the success of some other traditionally male institutions in recruiting women students (e.g., Princeton, where 38 percent of the present student body is female), the growth at M.I.T. seems somewhat less impressive. But more worrisome yet are certain indications that the popular mythology of the masculinity of the scientific male mind continues to persist.

While in many circles it has become decidedly unchic to suggest that women should not be physicists, mathematicians, engineers, etc., in other

circles, indeed the very circles one might least expect to hear such things, that is precisely what is being suggested. There is a growing voice among contemporary feminists reasserting the age-old dichotomy between women and science on the one hand, and the affinity between women and nature on the other. At its most extreme, we hear from certain radical French feminists that: "The will to theory is the most pernicious of male activities" (Marks and De Courtivron 1979, p. xi). Tentatively, a number of feminist theorists in the United States are asking such questions as: Is objectivity a code word for domination? or, Is objectivity something feminists want? At the same time, and in something of the same spirit, women's close kinship with nature is being acknowledged and embraced in the birth of a new movement—Ecofeminism.[1] Underlying the convergence of ecology and feminism that this movement advocates is the equation between woman and nature. The ravages of nature for which science and technology are held responsible become, under this equation, ravages against women (see, e.g., Griffin 1978). In opposition to the coercive and manipulative relation that male science and technology have traditionally maintained to nature is offered a more sympathetic, intuitive, respectful, and loving relation—one that recognizes the interconnectedness of all things. It is argued that women, as kin to nature, are especially privileged to provide such a relation—that their expertise and concerns as mothers and nurturers both can be and historically has been brought to bear on environmental issues. In short, conservation and ecology are claimed as feminist concerns.

My point here is that, although it may now be muted in many circles, the mythology which has for so long divided women from science is hardly dead. It even seems to be undergoing a kind of renaissance. While liberals attempt to escape the belief system which identifies science as male and nature as female, the very same beliefs are being re-embraced by a number of feminists. Such a resurgence suggests deeper roots to these beliefs than might otherwise have been thought. It is my purpose to examine those roots, as well as to explore their implications for women and science, both now and in the future.

In order to do so, however, the mythology itself needs to be elaborated. Its ingredients are contained in a set of familiar claims expressing the kind of polarities which have long been noted as organizers of our language, our perceptions, and our reality. Far from being unrelated, these claims tend to cluster together with a revealing coherence and are, therefore, worth examining in toto. A sample set might read as follows:

1. Science is impersonal, women are personal; science deals with things, women with people;
2. "The basic feminine sense of self is connected to the world, the basic masculine sense of self is separate" (Chodorow 1978, p. 169). Relatedly,

it is claimed that objectivity requires a total removal of the self from the object of study;

3. "There are two ways of knowing: The male way of knowing in its highest development is objective, analytical, scientific investigation. The female way of knowing in the completest [sic] sense is the mother's intuitive knowledge of her baby" (Guntrip 1969, p. 261);
4. Science is reason, unalloyed by feeling. "Feeling is the female element while thinking is a male element" (Guntrip 1969, p. 261; see Mitroff 1974 for an extensive refutation of this claim);
5. Science is "hard" and toughminded; women are "soft" and sentimental;
6. Scientists are cold and asexual; women are erotic (see, e.g., the studies of Liam Hudson 1966, 1968 for documentation of the prevalence of this view);
7. The scientific mind is male, nature is female; the aim of science is the domination of nature;
8. Science seeks power, women seek harmony.

The endurance of belief in these claims obliges us to ask a number of questions: What do they derive from? What accounts for their endurance? and how do they affect women, science, and the relation between the two? My strategy in trying to answer these questions will be to examine the psychological basis of such beliefs, and then consider the bearing of these beliefs on a single illustrative example of women in science. But first, a few general remarks may help to orient the discussion that follows and explain the relevance of a psychological perspective.

## *POPULAR MYTHOLOGY ABOUT WOMEN AND SCIENCE*[2]

These claims, taken together, constitute a mythology. I say this not to imply that they are not true (they may or may not be) but, rather, to remove them from the realm of irreducible fact and to locate them in the realm of social construct. Insofar as they are widely held, they take on an ideological function, in the sense that Geertz uses this term—they become "maps of problematic social reality and matrices for the creation of collective conscience" (Geertz 1973). As such, they cannot be regarded as either simply true or simply false but, rather, by their very nature, they bear a degree of contingent truth. To the extent that both science and gender are socially constructed, culturally shared myths about these inevitably (at least in part) both reflect and shape the realities we observe. Such myths need not be either universal or unique to be functional—they may, and indeed do, coexist alongside other myths about science[3] (and even about women). The multiplicity of popularly held images, in fact, may be crucial to supporting the

diversity observed within science. But no alternative myths are available to effectively neutralize the impact of the central mythology under consideration here. Not only has that mythology helped guarantee that most scientists are men but, more important, as I will also argue, it has influenced our very definitions of science and helped to promote a particularly narrow, and perhaps even distorted conception of objectivity.

In this last claim, I make certain assumptions about the nature of the scientific enterprise and about objectivity which should be made explicit. One such assumption is that science is a more pluralistic enterprise than is suggested by this mythology or, for that matter, than is suggested by the conception dominant within any particular discipline at a particular time. Furthermore, it is this pluralist potential which allows for the influence of cultural forces. Much philosophical and historical analysis has undermined the view of science as impelled entirely by its own logical and internal dynamics, and the work of Hanson (1958) and Kuhn (1962) are crucial here. Kuhn's *The Structure of Scientific Revolutions* (1962) led to a spate of inquiries into the influence of social and political forces on the development of scientific theories. Far from implying a total relativity, or a denigration of science (as some have interpreted it), I take this effort to be consistent with an acceptance of the essential goals of science.

The essential goal of theory in general I take to be to represent our experience of the world in as comprehensive and inclusive a way as possible; in that effort we seek a maximal intersubjectivity. Our search for truth is objective insofar as it strives for a characterization of our experience which transcends local, parochial vantage points, which transcends the expression of particular needs and fears, and which accordingly supports consensual agreement. As such, objectivity can be understood as a quintessentially human goal, even if it is a goal which can never quite be achieved. One crucial, though by no means the only, function it serves is of enabling us to be master or mistress in our own house (or world)—a function we should not confuse, though we sometimes do, with domination. And even if, by virtue of our historical experience, we have no noun to express mastery in the feminine, we ought nevertheless be able to recognize the universality of that impulse.

Science may well have given us the most fully developed expression we have seen of what could almost be called a "drive" for objectivity, but it is crucial to distinguish between the objective effort and the objectivist illusion (or what Piaget calls the realist illusion). Piaget (1972) offers a way to do this:

> Objectivity consists in so fully realizing the countless intrusions of the self in everyday thought and the countless illusions which result—illusions of sense, language, point of view, value, etc.—that the preliminary step to every judgement

> is the effort to exclude the intrusive self. Realism, on the contrary, consists in ignoring the existence of self and thence regarding one's own perspective as immediately objective and absolute. Realism is thus anthropocentric illusion, finality—in short, all those illusions which teem in the history of science (p. 34).

What Piaget's distinction enables us to recognize is that, in despairing of the most simple-minded fulfillment of the goal of objectivity, namely, the objective realist's dream of providing an error-free description of the world "out there"—indeed, in despairing of *any* fulfillment of the goal of objectivity—we do not need to give up on objectivity as a process. The very use of the word as a noun is here (and in Piaget's quote as well) misleading; it is, in fact, a kind of trap, though one which our language constantly invites us into. It suggests the existence of a subjectless form of knowledge. Properly speaking, "objective" ought to be an adverb, rather than an adjective, and "objectivity" a shorthand for an ongoing process rather than a state or condition that has or ever can be reached. Acknowledging the unobtainability of the realist's dream—a dream which Piaget calls "anthropocentric illusion"—thus, need not diminish our commitment to objectivity, understood now as process, but, on the contrary, may enable us to reaffirm that commitment more fully than ever.

This, I suggest, we can do by extending the critiques of Hanson (1958) and Kuhn (1962), and those who have followed them, to include an inquiry into the influence of affective forces on scientific thought—to ask: What is the meaning of the realist, or objectivist, dream? What kinds of fantasies, fears, or wishes does it express? In short, what are the personal dimensions of the claims that science makes to impersonality?

In asking these questions, I find myself faced with a certain irony. Though I began this discussion with an explicitly feminist concern, my intention was to shift the inquiry to more general issues. It is all too likely, however, that the last questions I pose will, notwithstanding my own intent, nonetheless be perceived as yet (and simply) another aspect of my feminist concern. This point is elegantly, if somewhat facetiously, expressed by Mary Ellman (1968). In her introduction to *Thinking About Women*, she responds to the accusation that "women always get personal" by suggesting: "I'd say, men always get impersonal. If you hurt their feelings, they make Boyle's Law out of it" (p. xiii). My intention, however, is to introduce into the realm of theoretical discourse what has been traditionally—as part of the very same mythology I am attempting to analyze—merely a "woman's question."

What I am suggesting is that it is the personal, affective basis of the scientific impulse which lends coherence to the illusions and myths that pervade our thinking about science—which lend coherence to what I call its objectivist distortions, to the perception of scientific thought as masculine,

and to its confusion with power and domination. As I will now try to show, an exploration of this affective basis provides important keys to our understanding of the roots, dynamics, and endurance of these myths and illusions.

## *PSYCHOLOGICAL ORIGINS*

One way to get at the emotional substructure of this mythology is to look at the developmental matrix out of which we form our ideas about gender, about objectivity, about mastery, and about love. For this, I have found that branch of psychoanalytic theory known as object relations theory to be particularly useful. Object relations theory is an attempt to account for personality development in terms of both innate drives and actual relations with other objects (by which psychoanalysts mean subjects). In this way, it is considerably more amenable than traditional psychoanalytic theory to an integration of external cultural and internal psychological forces. At the same time, by focusing on the earliest levels of development, it permits an integration of our understanding of cognitive, affective, and gender development. What emerges is a development model which, notwithstanding the controversial nature of psychoanalytic thought, provides an explanation of the origin and entrenchment of an identification between impersonal, objective, and masculine, given the parenting and cultural arrangements in which we grow.[4]

According to psychoanalytic theory, and a good deal of psychological theory, we do not begin life with any sense of ourselves as autonomous agents, either male or female, interacting with a world apart from ourselves. Rather, that sense is built up over time, in a process which is profoundly influenced by our experience in the world and by our expectations of those around us. As infants, we live in an amorphous, psychologically undifferentiated state of symbiotic unity with the person who takes primary care of us—almost universally our mothers. Out of this state, our sense of self, of reality, and of gender must be forged. In the process, we acquire the capacity for both objective thought and psychic autonomy, i.e., for cognitive and emotional independence. Developmentally, and operationally, these two capacities work in concert. Both rest on a capacity for distinguishing subject from object. Both grow out of that very difficult and often painful process of sorting out self from other, of separation and individuation from the mother—the first and most ambiguously other. And both retain the tell-tale marks of their early developmental setting. Because of the context out of which these capacities are acquired, both objectivity and psychological autonomy remain subject to the influence of unresolved conflicts about that early separation. The way we characterize, define, and aspire to both objectivity and autonomy remains profoundly colored by, on the one hand,

our anxieties about being alone, and on the other hand, our anxieties about yielding up to the primitive temptations of going back, of giving up our hard won status of separateness and selfhood. These anxities can, and do, work to inhibit or exaggerate the move toward independence, be it cognitive or emotional.

The development of a gender identity feeds into this process in complex ways—ways that can be, and, for us, generally are different for the two sexes.

> Although children of both sexes must learn equally to distinguish self from other, and have essentially the same needs for autonomy, to the extent that boys rest their very sexual identity on an opposition to what is both experienced and defined as feminine, the development of their gender identity is likely to accentuate the process of separation. As boys, they must undergo a two fold "dis-identification from mother" (Greenson 1968)—first for the establishment of a self identity, and second for the consolidation of a male gender identity. Further impetus is added to this process by the external cultural pressure on the young boy to establish as stereotypic masculinity, now culturally as well as privately connoting independence and autonomy. The cultural definitions of masculine as that which can never appear feminine, and of autonomy as that which can never be realized, conspire to reinforce the child's earliest associations of female with the pleasures and dangers of merging, and male with both the comfort and loneliness of separateness. The boy's internal anxiety about self and gender is here echoed by the cultural anxiety; together they can lead to postures of exaggerated and rigidified autonomy and masculinity which can—indeed which may be designed to—defend against that anxiety and the longing which generates it (Keller 1978, pp. 425–26).

In this way, the very act of separating subject from object, objectivity itself, comes to be associated with masculinity. Under the combination of psychological and cultural pressures, that association can, and often does, lend both objectivity and masculinity to defensive overstatement.

The connections between this process and our myths about science work, as myths always do, through stereotypes—which is not to diminish their importance, for stereotypic images work in powerfully formative ways. One of the ways they work is by selection. Bernice Eiduson (1973) has commented on this. In summarizing the literature on the psychology of scientists, she observes:

> Scientists as a group seem to be caught up in the same stereotypes that the public holds about them, and, in fact, the researchers seem to have been drawn into science by some of the same fantasies and stereotypes (p. 15).

According to the model I have just described, the picture of science as autonomous, as objectivist, and as masculine—whether or not it is accurate—

would inevitably be particularly appealing to those for whom such a picture is emotionally both gratifying and functional. That such self-selection does in fact take place, seems to be confirmed by the psychological literature on personality characteristics of scientists (Eiduson 1962, McClelland 1962, Roe 1956). In turn, it is not hard to see how such selection could lead to a perpetuation and entrenchment of that picture—above and beyond its purely scientific justification.

What I have described here is a network of interactions between cultural values, gender development, and our (stereotypic) beliefs about science—a system which has, incidentally, a host of secondary consequences. For example, it allows for science to derive extra prestige from whatever masculinist biases exist in the culture, and simultaneously for what we call masculine to derive prestige from the value we place on science. In the process, what is called feminine—be it a branch of knowledge, a way of thinking, or woman herself—becomes devalued by the cultural value placed on science. These cultural values in turn feed back on the developmental process which, of course, never occurs in a vacuum. Children grow into, and aspire toward, gender identities that are defined by the culture. Thus a self-perpetuating and self-reinforcing system is maintained; and, while it is not immutable, the difficulty in dislodging it has to be understood in terms of all its facets.

It is beyond the scope of this chapter to try to situate this process in its social and historical context, as a full understanding of its dynamics ultimately requires.[5] The predominantly psychological perspective employed here is necessarily incomplete; nevertheless, it does provide an account which is sufficiently coherent and complete in its own terms to warrant our asking: what implications follow from it? It seems to me that this account can be read in two very different ways—one fairly pessimistic, and the other optimistic.

The pessimistic reading is that the breach which separates women from science is very deep, and its mending might require more of a change in social arrangements than many people would accept. It would seem particularly unhopeful to the extent that the burden of change is seen as falling entirely on women. Of course, that is not to say that, in a liberal climate, we won't see more women scientists than we have in the past but it does suggest: a) that they would remain a minority, and b) that they would tend to be self-selected by the same mechanism that I argue has in the past helped select male scientists. In that way, women in science could reproduce the two-culture split we are already so familiar with, possibly even a sharpened version of that split.

Alternatively, to the extent that science is itself malleable—in its self-conception and its ideological commitments—a more optimistic reading is possible. To the extent that the vision of science as objectivist, autonomous,

and masculine relaxes its hold, among scientists and nonscientists alike, we might envisage a professional climate not only more sympathetic to women, but more sympathetic to all those whose values (often described as humanistic) are at variance with this vision (see, e.g., Fee 1981). But such a suggestion immediately raises a question about what kinds of changes are actually imagined. What would it mean for science to change its self-conception and ideological commitments, and still be considered science?[6]

I suggest that the best way to answer this question is by looking more closely than we are accustomed at the range of activities to which the name science has actually been put. Science is not a monolithic structure, either intellectually or emotionally, despite its own stereotypes. Rather, one sees at work a constant interplay between contrasting themes, with the evolution of science reflecting a certain selection of dominant themes. In part, this selection is influenced by ideological issues and, in part, by the discoveries scientists make. Science is neither simply created nor simply discovered. It is both. The process by which internal and external factors interact in the development of science is what we understand least well; at the same time, it is that process that most critically needs to be explored, and, indeed, is the principal focus of much contemporary history of science. The way in which mythology functions in this development is through its participation in the process by which dominant themes are selected. But still, however dominant reigning "paradigms" may be, minor themes continue to be played, and sometimes even heard. Part of what Kuhn calls a "Scientific Revolution" is the emergence of what had been a minor theme as a major theme. The process by which such transformations take place is, of course, enormously complex, but a brief look at one such transformation which may now be occurring in biology might serve as an informative illustration.

## *IMPLICATIONS FOR SCIENCE: A CASE STUDY*

The central persona of this illustration is Barbara McClintock, long known as one of America's most eminent cytogeneticists. McClintock's career and her contributions to biology are the subject of a full-length book (Keller 1983), but certain features of that story can be briefly told here. These serve to exemplify the diversity of styles and themes in scientific research, and can simultaneously provide an occasion for considering the relevance (or irrelevance) of gender and gender stereotypes on styles of scientific thought.

Many years ago, Barbara McClintock embarked on a course of experiments which led her to an interpretation of genetic function and organization which at the time seemed too remote from the mainstream of biological thought for most biologists to comprehend, let alone accept. From a series of detailed microscopic and genetic analyses of mutable loci on the chro-

mosomes of the maize plant, she concluded that the genetic complement of maize is characterized by a degree of inherent instability. Certain genetic elements are capable of autonomous (i.e., not externally induced) transposition from one site in the chromosomal complement to another. Furthermore, this transposition plays a crucial role in the control of gene action and the regulation of developmental processes. In her first paper on this subject, McClintock (1951) suggested:

> The numerous phenotypic expressions attributable to changes at one locus need not be related, in each case, to changes in the genic components at the locus, but rather to changes in the mechanism of association and interaction of a number of individual chromosome components with which the factor or factors at the locus are associated. According to this view, *it is organized nuclear systems that function as units at any one time in development* (p. 34; emphasis added).

Today, thirty years later, transposition has become a well accepted phenomenon in molecular biology, and interest in its role in regulation and development is growing. Increasingly, many of those biologists who are most directly involved in these discoveries are beginning to suspect far more global mechanisms of genetic control than had earlier been assumed. Accordingly, a number of themes belonging to what might be called an organismic perspective, which had been muted for the three intervening decades, are now beginning to be heard.

For several reasons, the case of Barbara McClintock seems a particularly good one through which to try to address the question of whether female scientists, by virtue of being women, might introduce different styles and perspectives into scientific thought. Not only is McClintock a woman, but she is one whose rejection of gender stereotypes appears to be total. And even by the most conventional standards, she is an eminently toughminded, meticulous, rigorous, and "objective" scientist. At the same time, it also needs to be said that, in important ways, her posture as a scientist is strikingly unconventional. In both style of research and theoretical position, she resides outside of what has been, for the last thirty years, the dominant tradition in genetics. Certain elements of her methodological approach and even of the thrust of her theoretical conclusions might suggest to some the presence of a feminine style; and, although I will mainly argue against this interpretation, it is, nevertheless, useful to explore the temptation to so regard them.

The main conclusion which I believe emerges from a series of interviews I have conducted with Barbara McClintock[7] is that, while there clearly are aspects of her relation to her work which offer alternatives to the dominant mythology about science, these echo themes which are *not* alien to the

scientific enterprise. Rather, they are subthemes with a long tradition, even if at times they may seem almost entirely submerged. In some ways, these themes may seem especially compatible with female psychology (as we understand that to be), but they are by no means exclusive to it. In the end, I would argue that they are not only familiar in the thought of many male scientists, but are consonant with and perhaps even essential to the scientific enterprise as a whole.

Some examples might serve to elucidate these points. One of the predominant themes which emerged in these interviews was the importance of "letting the material speak to you," of being able to "let the material tell you what to do." Her chief criticism of the way science is normally done lies in the lack of respect for the system and the corresponding lack of awareness of one's own "tacit assumptions." She feels that much of the work is done because one wants to impose an answer on the system; too many researchers have the answer ready, and know what they want the material to tell them. Anything else it tells them, they don't recognize as there, or they think it's a mistake and throw it out. "If you'd only just let the material tell you!" But to be able to hear what the material has to tell you (the material, in her case, is the corn plant), one must have a "feeling for the organism." By this she means understanding how it grows, understanding its parts, understanding when something is going wrong with it. A plant, she explains, is not just a piece of plastic but, rather, is something which grows, which is constantly being affected by the environment, constantly showing attributes of its growth. In order to properly interpret what you see, it is necessary to "know" every individual plant. That requires watching the plant from the very beginning, for no two are exactly alike. Each one is different and you have to know that difference.

A "feeling for the organism" must follow the scientist into the laboratory, through all of his or her work. In McClintock's microscopic studies of *neurospora* chromosomes (so small that others had been unable to identify them), she found that the more she worked with the chromosomes, the "bigger and bigger" they got, until finally, "I wasn't outside, I was down there—I was part of the system." As "part of the system," even the internal parts of the chromosomes became visible. "I actually felt as if I were down there and these were my friends."

Much of this material suggests a kind of respect and attention—almost nurturant—that some people would see as the privilege of women. This last quote in particular seems to vividly illustrate a notion that the radical French feminist, Hélène Cixous, calls "super-seeing" in which (she quotes Clarise Lispector here), "My eyes ended up no longer distinguishing themselves from the thing seen" (1979). For Cixous, "super-seeing" is a peculiarly feminine capacity.

Yet, McClintock employs it, along with her capacity for respect, attentiveness, and her feeling for the organism, as a scientist. Nor, as a scientist, is she unique. To some extent, these traits are familiar characteristics of the naturalist tradition; others we think of as the marks of all truly creative scientists—even of the creative imagination in general. Ralph Waldo Emerson wrote: "I become a transparent eyeball; I am nothing; I see all." Clearly, it would be a mistake to think of these traits as being unavailable to male scientists. McClintock herself was trained and encouraged by men, and influenced by many (male) thinkers (some, e.g., the geneticist Richard B. Goldschmidt, with a somewhat similar bent) before her. It may be, however, that she was able to exploit certain modes of scientific thought more fully and more visibly than others—particularly in an age when they were becoming less visible—by virtue of being a woman and an outsider. That is, as a woman, she may have been freer to transcend the constraints that a commitment to the masculinity of the scientific mind imposes—she could be clearer about the place of what we call "feminine" virtues in scientific work than much of the rhetoric permits. In that case, it would be the corresponding failure of many male scientists to acknowledge and encourage the development of these traits that might reflect the presence of what could be called a masculinist bias in science.

Barbara McClintock is not an ecofeminist. She would be the first to reject the notion of a feminist science. At the same time, it has also to be said that not only would ecofeminists find her style of research sympathetic, they would find her vision of cellular organization even more so. In fact, the consonance between McClintock's theories of genetic organization and her approach to her subject of investigation are striking; they suggest a contradiction to the view that the psychology of research is irrelevant to the logic or the substance of scientific discovery. Here, an investigation premised on attention to the individual, on a "feeling for the organism," on "forgetting yourself," and "letting the material tell you what to do" resulted in discoveries which, in turn, led her to a picture of the cell as radically divergent from the dominant picture of molecular biology as was her methodological style. During the decades when the cell was becoming a relatively straightforward chemical machine to most biologists, to McClintock it was opening up in complexity. In lieu of the linear hierarchy described by the central dogma of molecular biology, in which the DNA encodes and transmits all instructions for the unfolding of a living cell, her research yielded a view of the DNA in delicate interaction with the cellular environment—an organismic view. In this view, one cannot consider the genome as such (i.e., the DNA) as being all important—far more important is the "overall organism." As she sees it, "The genome makes the necessary products according to a program that reacts to signals [coming] from elsewhere. . . . It will function only in respect to the environment in which it

is found." In McClintock's work, the program encoded by the DNA is itself subject to change. No longer is a master control to be found in a single component of the cell; rather, control resides in the complex interactions of the entire system.

Interaction is a key word here, and a favorite one for ecological advocates—male and female alike. It is also a crucial word in the long and complex history of organismic thought in biology. If that history has been less than successful, at least a partial explanation for that fact may be found in the ideological commitments which predispose scientists to favor "master control" over interactionist theories. Today, transposition has become a well accepted phenomenon, but the larger implications which McClintock saw in this work remain far from general acceptance. Biology finds itself at a philosophical crossroad; we cannot yet say which route it will take.

## *CONCLUSION*

In the view of science presented here, change depends less on the introduction of a specifically female culture into science than on the rethinking of sexual polarities and the abandonment of a sexual division of intellectual labor altogether. If McClintock's approach to science offers us a different model from the one we are most familiar with, it would be a mistake to yield to the temptation of interpreting that difference as a simple reflection of gender difference. Given the conditions that have historically prevailed for the entrance of women into science, it is generally not possible to regard women scientists as exemplars of a female culture. By necessity, their acculturation has almost always had to be anomalous. Nevertheless, by virtue of the fact that they have not been socialized as men, and have in general been forced to occupy positions peripheral to the dominant scientific culture, their perspectives and contributions to science often bear the marks of their peripheral status. As such, they can help illuminate the forces that shape the dominant culture, and, together with their male counterparts on the periphery, help point the way to a less gender-bound science. What is unusual about McClintock's approach to science must be understood not in terms of a commitment (on her part) to gender ideologies in any form, or to any notion of a feminine science, but precisely in her rejection of gender ideologies altogether, in her commitment to a life of the mind in which "the matter of gender drops away."

The question of whether gender will drop away from conceptions of science in the future, or whether we are doomed to play out ancient polarities between science and women, and possibly within science, remains finally something we can at this point only guess about. Barbara McClintock worked as a scientist in an age in which the gender mythology I have elaborated

here retained a strong hold on our thinking—within science as well as without. Now we are in an age of transition in which sex role ideology is relaxing its grip (at least in most circles) and in which women are becoming more visible in the scientific world. As a consequence, it is possible that sexual polarities are also disappearing from scientific thought.

However, more likely, the introduction of more women into science is not by itself sufficient to bring about such change. Scientific ideology is not, after all, *solely* determined by gender ideology. Rather, both ideologies have deep roots in the entire economic and political context in which science has evolved. If the conception of objectivity traditional to science is a parochial one, influenced by a particular ideology about gender, it is one which has also served our particular political and economic history exquisitely well. The suspicion that "the subject-object split legitimizes the logic of domination" (Fee 1981) is not unfounded, and even though the precise nature of the relation between objectivism and domination is an issue that remains in need of further examination, we have learned a great deal from recent scholarship (Rose & Rose 1980) about the interdependency of science and politics. If women are in a privileged position to bring the epistemological critique that is equally necessary for the liberation of science and the liberation of society, it is both because we have been especially vulnerable—viewed as passive, natural objects—to the logic of domination, and because our status as inhabitants of a different (a female) culture provides us with an invaluable perspective—the view from the periphery. But a feminist and psychological critique is only a beginning. Ultimately, it must work hand in hand with other social analyses of science in order to arrive at an understanding of the ways in which science has traditionally both reflected and contributed to the social and political structures we wish to change.

## *NOTES*

1. The first large meeting of this movement, "Women and Life on Earth: Ecofeminism in the 80's," convened at the University of Massachusetts, Amherst, March 21–23, 1980.
2. Key pieces of the argument that follows have been presented elsewhere (e.g., Keller 1982). I repeat them here, first, for the sake of completeness, and second, on behalf of readers unfamiliar with this literature.
3. There is, for example, also the image of the scientist engaged in a mystical search for truth, seeking transcendence rather than power, impassioned, in love with his subject. It might be noted, however, that this mythology tends to be reserved for the "great" or unusually creative scientist; and, despite popular accounts, it is more likely to be the normal than the revolutionary scientist who determines the character of science—indeed, who determines what can be accepted as revolutionary rather than, say, "crackpot."
4. This model has been elaborated in greater detail in Keller (1978).
5. A more comprehensive analysis is attempted in my forthcoming book, *Reflections on Gender and Science*.
6. The vision of a different science is a familiar theme. In the 60s, the plea for a science based

on a more erotic relation to reality achieved a good deal of popularity from the writings of Norman O. Brown and Herbert Marcuse, and now, in the 80s, the cry for a different, more loving, more life-respecting science is taken up again by the ecofeminists. There is little question however that such a vision is as romantic now as it was in the 60s. Perhaps where I most seriously depart from these visionaries is in my conception of science and of how it works.

7. These interviews were conducted between September 1978 and February 1979. McClintock quotes not otherwise cited refer to these interviews.

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