Feminism, Science, and Democracy

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Over the last two decades, leftist thought has challenged the traditional view that the natural sciences provide an a priori and irreplaceable safeguard to the fundamental principles of freedom and democracy. In that view, the value neutrality of science was believed to provide the best available defense against ideological coercion: the conjunction of scientific rationality and liberation was taken for granted. Now we have come to recognize those beliefs as themselves constituting a form of ideology. In the growing body of literature of the social studies of science, historians and sociologists of science have begun to demonstrate the many ways that the work of scientists—far from being value neutral—has served, and continues to serve, the economic and political interests of special groups. The identity of scientific rationality and liberation can no longer be taken for granted. Indeed, to many, scientists themselves have come to be seen as a special-interest group, and science as a special interest—even as a means of social control. Seemingly forgotten is the radical potential of science embraced by so many leftist and liberal thinkers of the past.

From a feminist perspective, the criticism that has emerged over the last twenty years is not radical enough, and from a scientific perspective, it is too sweeping. Taken together, however, these two claims can point the way to an important and fruitful new perspective on the sociology of scientific knowledge, and even suggest the ways in which the radical potential of science might be restored.

Both feminists and scientists remind us of the distinctiveness of the commitments and claims that define the scientific enterprise. Science, they agree, is distinguished from other activities by its particular claims to objective knowledge and power. But what for scientists has always been a source of pride is now becoming for feminist theory the principle point of departure of a new critique.[1]

1 See Elizabeth Fee, "Is Feminism a Threat to Objectivity," *Science for the People*, October 1982; Evelyn Fox Keller, "Feminism and Science," *Signs* 7, no. 3 (Spring 1982); and Sandra Harding, "A Feminist Strong Program in Epistemology," in *Discoverying Reality: Feminist Perspectives in Epistemology, Methodology and Metaphysics*, ed. S. Harding and M. Hintikka (The Hague: Reidel, 1983).

It is not, some feminists argue, simply that the commitment and claim to objectivity obscures, and indeed protects, the practice of interests, but more seriously, that the very claim to disinterest and detachment expresses a kind of interest: it reflects the values embedded in our cultural ideal of masculinity.

Support for this observation is abundantly provided by scientists themselves. Modern science is an institution that has not only been produced by men, but it has been defined and shaped by frequent reference—explicitly and implicitly—to masculine ideals. "Let us establish," Francis Bacon wrote in the seventeenth century, "a chaste and lawful marriage between Mind and Nature." The new science was to be, again in Bacon's words, a "Masculine Birth in Time"—distinguished from earlier conceptions of knowledge by its uniquely virile strength. In the debates that took place later in that century over the form the new science was to take, the language of gender remained prominent: when the Royal Society was established in 1662, its founding fathers committed themselves to the promise of a truly "masculine philosophy." Henceforth, the equation between scientific and masculine was promoted not simply by the exclusion of women from science, but more generally by a growing division between objective and subjective, male and female. In all this, science was an active force. If George Simmel was right over fifty years ago when he wrote "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,"[2] then we must acknowledge the important role that the success of science has played in the growing idealization of (male) objectivity and the correlative devaluation of (female) subjectivity. It is not hard to see that in the sexual and emotional division of labor that has resulted, the winners have been men and the losers women.

But feminists are inclined to worry about these developments for other reasons as well. They worry about the conjunction of the commitments of science to objectivity and to power. The function of the "chaste and lawful marriage" that Bacon advocated was, he explained, to "[lead] you to Nature with all her children to bind her to your service and make her your slave." The masculinity of the new philosophy would be affirmed by the exercise of power. The promise of science—"the restitution and reinvesting of man to the sovereignty and power . . . which he had in his first state of creation"—was to be fulfilled through the domination of nature. History has shown Bacon to have been more than prescient. Indeed, it is precisely the power that science, and scientists, have come to wield that causes such alarm among the critics of science. No one living in the nuclear age can regard the potential for power that Bacon foresaw in science with the equanimity and optimism of an earlier age. And short of ultimate destruction, critics of science have also sensitized us to the ways that knowledge of phys-

2 Quoted by Karen Horney, "The Flight from Womanhood," reprinted in *Women and Analysis*, ed. Jean Strouse (New York: Dell, 1975).

ical nature, together with the authority of science, can be invoked as an instrument for the domination of human nature. But where others might see these excesses as an abuse of power, (at least some) feminists see them as deriving from the equation between science and power. Once again, we are invited—not only by the pervasive cultural definition of power as masculine, but yet more explicitly by the language of the Baconian vision—to ask whether the centrality of power and domination to the goals of science does not reflect a male perspective. And finally, the further question arises: how does a commitment to objectivity serve the interests of power and domination? In promoting a critical psychological distance between subject and object, perhaps objectivity itself constitutes an invitation to domination.

Such a line of inquiry can lead in any of three directions. First it can lead to the rejection of science, in favor of a more purely "female" culture. This avenue, I suggest, serves neither the interests of women, science, nor of democracy. Instead it promotes the very separation of spheres that modern culture prescribes. It is what most women have traditionally done. By accepting the intellectual and emotional division of labor on which that separation is premised, it leaves science—and power—to men, and simultaneously forfeits the possibility of a more universal and hence more democratic science.

The second tack, in which we are led to imagine what a different science, a feminist (or feminine) science might be, encounters similar problems. Such an approach takes to heart the lessons learned from an appreciation of science as a social phenomenon—produced by human beings acting in a particular social, economic, and political context. In fact, without the revolution that has occurred in our understanding of scientific thought, the extension of a feminist critique of any kind to the natural sciences could only have a reactionary effect. As long as the course of scientific thought was regarded as exclusively determined by its own logical and empirical necessities, then the contention that the different experiences and ambitions of women might give rise to a different mode of thought could only reinforce the traditional view that women are unfit for science. It is important to recognize that an understanding of the social influences on the development of scientific thought is a necessary prerequisite to the political possibility of a feminist theoretic in science. But the notion that one might envision a different kind of science altogether is a consequence of what Hilary and Steven Rose, in their critiques of leftist relativism, have called "an overly socialized conception of science."

An overly socialized conception of science poses the same risks for leftists and feminists. Insofar as science comes to be understood as expressing nothing more than the interests of special groups, the obvious next step would seem to be to pursue the development of a science expressing the interests of different (perhaps larger) groups with different (perhaps more benign) interests: a "science for the people," or a "feminist science." The problem with this line of thought is that science, so understood, ceases to be recognizable as the activity that the people we call scientists engage in. This is a problem far more serious than scientists' failure of imagination; it reflects a semantic default. What is it after all that we mean by the term "science" if even its claims to epistemological distinction are subject to referendum, when the demarcation of science dissolves into nothing but a social distinction?

I want to suggest that the proper route of a feminist analysis, as of a leftist analysis, is neither the rejection of science nor the search for a "different" science, but a third alternative—attempting to identify the ways that science has been shaped by social forces while at the same time remaining cognizant of what is distinctive about science. The recognition that science is not and has never been—indeed cannot be—free of ideology does not oblige us to regard it as pure ideology. Belief in a simple parsing of facts and values, reason and feeling, cultural and scientific may itself be an expression of ideology and serve manifold functions; it does not follow, therefore, that facts are values, reason is feeling, or that science is the sum total of the social forces which shape it. Sociobiology, or I.Q. studies—two favorite whipping posts of feminists and leftists—are suspect not only because they serve to support existing inequalities of wealth and power; they can also be criticized as bad science. In this spirit, then, objectivity may be acknowledged as an a priori goal of science that, however heavily invested it has historically been in masculine values, nonetheless has meaning and value that transcends gender: as the search for maximally reliable knowledge of the world around us, it is a quintessentially human goal. It is the presumption that any particular stance, or practice, can achieve total objectivity that effectively serves to obscure vested interests. In the same spirit, one can also acknowledge the universal value of the power of science to expand human competence, and simultaneously argue that the preoccupation with control and domination—either of nature or of other humans—represents a deformation of that goal. What a feminist analysis needs to address is the ways that conceptions of objectivity and power, which I take to be the natural goals of science, have been skewed by masculine values.

For this, a critique of science is required which mistakes neither its rhetoric for its practice, nor any particular set of practices for the venture as a whole. Science, as we know it, is not uniform either as theory or as practice. Individual scientists are drawn into the profession out of a variety of motivations, trained, and integrated into different subcultures that employ different methodologies to solve different kinds of problems, and—both as individuals and as members of communities—invoke a wide range of values and preferences in their selection of "best" theories. Shared commitments to the knowability of nature, to the dependence of truth on both experimental replication and logical coherence, allow us to identify them as members of a common group. But the differences among them provide the means by which we can begin to sort out the complex relations between science and ideology.

In attending to such differences, a feminist and a scientific perspective can be mutually reinforcing: both require respect for the range of actual practices that have constituted the scientific enterprise. I suggest that the pluralism revealed by close attention to lived scientific history permits us to envision a science less bound by the ideology of dominant groups without having to invent a "different kind of science." By attending to the evolutionary pressures that operate on the diversity of values, goals, and interests coexisting in science, we can begin to discern the ways that ideology contributes to the determination of the uses to which science is put, the choice of problems scientists study, the methodological styles they judge to be most "legitimate," and the explanations they find most "satisfying."

Feminist analyses have begun to identify the effects of a masculine bias operating on all four levels. Not surprisingly, it is the last—the influence of ideology on explanatory preferences—that is the most difficult to identify. I will devote the remainder of this essay, therefore, to an attempt to illustrate how ideology in general, and a masculine ideology in particular, can help to shape the very descriptions of nature that emerge from scientists' desks and laboratories. My examples are drawn from the recent history of biology. I suggest a reading of that history which indicates a systematic preference for those theories and explanations that internally reflect the same preoccupation with power and domination that is expressed in so much scientific discourse.

Throughout this century, biologists have debated the relative value of two kinds of explanations of cellular organization—explanations that might be described as organismic as opposed to particulate theories, hierarchical as opposed to nonhierarchical, or interactionist as opposed to "master molecule" theories. The history of biology shows that, whether the debate has been over the primacy of the nucleus or the cell as a whole, the proponents of hierarchy have consistently tended to prevail. I offer two instances of this debate to illustrate. The first, a particularly important example for contemporary biology, comes from the history of Barbara McClintock's career.

Barbara McClintock is a distinguished cytogeneticist who is presently being widely acclaimed for her discovery, over thirty years ago, of the phenomenon of genetic transposition: the movement of genetic elements from one chromosomal site to another. She began the particular body of research for which she is now so celebrated when she was already at the peak of her career, after she had earned the respect and admiration of her colleagues around the world. Yet not only was this research not accorded the kind of attention a scientist of her stature might have expected; it hardly evoked any interest at all. Indeed, her work on transposition and controlling elements, first presented in 1951, earned her the reputation of eccentricity; some called it "mad." Today, she is deluged with awards and prizes for this same work, which is now seen as having anticipated one of the most important recent developments in molecular biology—"jumping genes."

The fact remains, however, that for all her recent acclaim, McClintock's vision of cellular organization, like her vision of science, remains a difficult one for most of her colleagues to understand, much less embrace. Both speak to traditions representing persistent but distinctly minor themes in the history of science—traditions of which we sorely need to be reminded.

McClintock's vision of cellular organization begins, and ends, with a concern for the interaction of its parts; her vision of science with what she calls "a feeling for the organism." Although she has always focussed her immediate attention on the most minute details of chromosomal variation, her sense of the living quality of the material she studied remained a constant and critical aspect of her relation to that material. It informed her interests, her expectations, and her form of attention. A plant, she explains, is not just a piece of plastic, but rather something that grows, somethat that is constantly affected by its environment. 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.

For McClintock, "a feeling for the organism" is not just a way of talking; it is a necessary prerequisite for scientific knowledge. Over and over again, she reminds us of the need to "let the material speak to you," the need to let it "tell you what to do next." And the best way to do this is to recognize yourself as part of the system. She shares with all other scientists a commitment to the pursuit of objective knowledge, but in her understanding of the term objectivity does not preclude an interaction between subject and object. She also shares with other scientists a belief in the power of science to enhance human competence. But power, in this vision, is a power born more out of love than an interest in domination. And just as her vision of science is constituted of attention to the individual and an interest in the organic relatedness of all its parts, so her vision of cellular organization reflects the same ingredients.

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. As she sees it, the genome functions "only in respect to the environment in which it is found." From her work comes the conclusion that the program encoded by the DNA is itself subject to change; transposition, to McClintock, is a mechanism of genetic regulation that permits the cells to meet the needs of the organism. It implies that the concept of a master control residing in a single component of the cell is inappropriate: control resides instead in the complex interactions of the entire system.

The rediscovery and acceptance of transposition requires a better explanation of thirty years' resistance on the part of McClintock's colleagues than that she was "wrong." It requires that we look into the kinds of presuppositions, and ideological commitments, that made her formulations seem—as they continue to seem to many—so unacceptable. Even now, the acceptance of transposition does not imply an embrace of McClintock's vision. To most of her colleagues, her language remains alien and unfamiliar; it jars with the predominant rhetoric of modern biology.

Many issues were involved in the discord between McClintock and her community, but in part her story is a story of conflict between a community that became increasingly committed to the view of genes, and later DNA, as the central actor in the cell—that which governs all other cellular processes—and an individual whose view was that genes, or DNA, constitute only one part of the cell. As such, it is a model of the conflict between "master molecule" and interactionist theories that pervades biological history.

One other example may help to illustrate this conflict more concretely. It comes from my own work as a mathematical biologist. In the late 1960s, in an attempt to understand the origin of difference in an initially undifferentiated system, my colleague Lee Segel and I set to work on the problem of aggregation in the cellular slime mold *Dictyostelium discoideum. Dictyostelium* has the remarkable property of existing alternatively as single cells or as a multicellular organism. As long as food is available, the single cells are self-sufficient, growing and dividing by binary fission. But when starved, these cells undergo internal changes that lead to their aggregation into clumps that, as they grow bigger, topple over and crawl off as slugs. Under appropriate environmental conditions, the slug stops, erects a stalk, and differentiates into stalk and spore cells; the spores subsequently germinate into single-celled amoebae.

The onset of aggregation is the first visible step in the process that eventually leads to the cellular differentiation observed in the multi-cellular organism. Prior to aggregation, no difference among cells is apparent. But once it occurs, a differential environment is created that could presumably be the basis of subsequent differentiation. The question is, what triggers the aggregation?

Following in the spirit of Alan Turing (who had demonstrated that a hypothetical system of interacting chemicals, reacting and diffusing through space, could generate a regular spatial structure that, he speculated, could provide a basis for subsequent morphogenetic development), Segel and I examined the stability of a homogeneous field of interacting but undifferentiated cells. We showed that the conditions for instability, i.e., for the onset of aggregation, would be met by certain developmental changes in individual cells, *without prior differentiation.* That these changes actually take place was independently corroborated by experiment.

Our aim in this effort was explicit: to offer an alternative to the widespread view that special cells were needed to initiate aggregation. There were at least two reasons for seeking an alternative to this view: first, no evidence for such "special" cells existed; and second, it was known that when the centers of aggregation patterns are removed, new centers form—i.e., aggregation is undisturbed.

Nonetheless, shortly after Segel and I published our model, it was supplanted by a model that revived the earlier view. In this model, aggregation occurs not as a field effect of interacting cells, but as a result of signals emitted by certain "pacemaker" cells. Although attractive in many ways, to us it had one serious flaw: it failed to address the problem of the origin of the difference that gave rise to these "pacemakers." It soon became obvious that the question that we had considered the central issue was *not* the question of interest to most biologists or mathematical biologists working on the subject. In fact, the pacemaker view was embraced with a degree of enthusiasm that suggested that this question was in some sense foreclosed. The assumption of pacemaker cells was felt to be so natural and it so readily explained the phenomena, that the question we had begun with simply disappeared.

In the years that followed, the word "pacemaker" became a basic and common term of the literature. Despite the continuing lack of evidence for or against their existence, researchers came to take their reality for granted. In fact, the force of the pacemaker concept has been so great that it soon began to be applied to other phenomena—chemical reactions—occurring in patently uniform media.

From my original point of view, the enthusiasm for pacemakers was simply perplexing. But my recent study of Barbara McClintock's career has suggested to me that this story provides another, and an especially simple, instance of the predisposition for the kinds of explanation that posit a single central governor. It illustrates that such explanations appear both more natural and conceptually simpler than global, interactive accounts. But simplicity too is a relative term. In mathematical sciences in general, it is true that "hierarchical" models do lend themselves more readily to the kinds of mathematics that have been developed; they are mathematically more tractable. But we need to ask: What accounts for mathematical tractability? Might it not be that prior commitments—ideological pressures—influence not only the kinds of explanations that are felt to be satisfying, but also the very analytic tools that are developed to deal with such explanations? The original model that Segel and I proposed was (as were all the models of that time) highly over-simplified. It failed to incorporate the more complex internal dynamics of the individual cell that we now know to be important to aggregation. But ten years ago we lacked the mathematical techniques for such an analysis. In the intervening period, however, we have witnessed a tremendous growth in the study of nonlinear equations. As a result of this effort, it is now possible to develop the analysis of cellular interactions, without the positing of "pacemakers," far further than we were able to do then. Indeed, some of that analysis has now been carried out, and the question of the existence of "pacemakers" can once again be raised.

The moral of both stories should be apparent. I am suggesting that both might be read as illustrative of a widespread commitment to kinds of explanations that satisfy an interest in or predisposition toward hierarchy, control, and even domination. I suggest further that the prevalence of this interest within the scientific community is a result not of an intrinsic commitment of science to control and domination but of a relatively straightforward selection process. Individuals are attracted to science by the ways that science advertises itself; those drawn by an ideology of power and domination will tend to select themes consistent with that ideology. In this way, the community's definition of "good" science reflects not only experimental replicability, logical coherence, and explanatory power, but also ideological "fit." But at the same time both stories serve to remind us that ideology is not binding. Diverse values, goals, and interests continue to coexist in the practice of science despite pressures to conform. And the commitment that scientists share to the pursuit of objective knowledge—despite differing conceptions of objectivity—allows for the possibility that, sometimes, ideologically "unfit" themes will prevail. But for this liberatory, even radical potential of science to flourish, we must learn to credit and support those values, goals, and interests that have survived the dominant ideological pressures. The result might indeed be a different science: certainly it would be a more democratic one.