Science as Social Knowledge

Values and Objectivity in Scientific Inquiry

HELEN E. LONGINO

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CHAPTER EIGHT

Science in Society

ONE OF the implications of the previous chapter is that the dependence of particular studies on the linear-hormonal model does not invalidate them as scientific research. While their conclusions are certainly contestable, they are not thereby the products of bad methodology. No set of data is evidence for a hypothesis independently of some background assumption(s) in light of which the data acquire evidential relevance. Thus, reliance on an explanatory model or set of background assumptions does not demonstrate bad methodology. Moreover, the label "bad science" can only be applied in light of criteria that are operative within a particular field of research. The generation and presentation of sloppy data, but not (or not necessarily) the interpretation of data, deserves the label of bad science.

Scientific research, however, is not only evaluated as "pure" inquiry answerable to internal standards but as a basis for social action and policy, and increasingly as a ground for values and ideals. Here a much more complex approach to assessment is required. This chapter has two main goals. I wish first to review the real or purported implications of biological research for social action and sociocultural values. This discussion of the material from the last two chapters will strengthen the claim that it is the linear-hormonal model that underlies the behavioral neuroendocrinological studies discussed in Chapter Six and Chapter Seven and not some other set of assumptions. My second main task is to show how the contrasting approaches to scientific knowledge support different attitudes towards these implications.

THE VALUE OF FACTS

The relationship between science and values in Euro-American culture has been transformed since the Renaissance. What Newtonianism accomplished on behalf of physics, Darwinism has promised to do on behalf of biology. As we all learn in elementary intellectual history a considerable part of the resistance to the Copernican account of heavenly motion is value-centered: the role of humans in God's plan, the uniqueness of the human habitat, and the place of God in the universe. John Donne's Anniversary poems are eloquent expressions of the disturbance caused by the ideas of the new physics and of the diminishing power of value in relation to brute fact. Once the universe was conceptualized as a machine, it was no longer suffused with value and no longer a suitable location for God. After the publication of Newton's *Principia Mathematica* the transfer of power as far as the physical sciences were concerned was complete. From then on theological claims had to pass the test of consistency with Newtonian physics: rational proofs of God's existence gave way to empirical arguments like the argument from design.

The introduction of evolutionary theory paralleled the earlier conflict. Even if ideas of God had had to be accommodated to the rigors of mechanical science, in the early nineteenth century humans and human life were still centers of value distinct from the world around them. God, too, could be thought of as having created the mechanical universe in the geological and biological form in which it was then known. New theories of geological change began the challenge that culminated with the publication of Darwin's *Origin of Species*. Outrage at yet another displacement of humans, this time into a species of primate, thundered from pulpits and reverberated through the Fleet Street press. Almost as immediately, however, social theorists embraced Darwinism and used it to legitimate social inequality.

We are still living in a period of transformation. In the United States the Scopes trial only temporarily vindicated science against religion. The last ten to fifteen years have witnessed a small but vocal and well-financed coalition of religious antievolutionists putting the fear of God into textbook publishers and school boards and bringing evolutionary theory back into courtrooms. The debate about evolution is, however, political rather than scientific, and most pundits (rightly or wrongly) perceive antievolutionists as part of the radical right-wing fringe rather than part of the mainstream.

The political impact of fundamentalism contrasts sharply with the centrality of evolutionary theory in areas of the biological sciences seemingly far removed from the study of origins. It contrasts also with the reverence accorded biological theorizing in the secular atmosphere of mainstream U.S. culture. The infiltration of sociobiology into sociology and political science, for instance, continues a trend begun in the nineteenth century, even though in some cases the naive crudities of Spencerism and social Darwinism are avoided. Just as Newtonian mechanics provided models and metaphors for eighteenth-century social, political, and psychological thought, so biology is the source of models and metaphors for twentieth-century thought. Secular culture in the United States does not provide alternative absolute values to substitute

for religious ones. Rather, the sciences, buttressed by their contemporary spectacular practical successes, have come to fill the void left by religion. Contemporary journalists cite controversial scientific research as fact, reinforcing cultural stereotypes and prejudices. They treat Nobel Prize winning scientists as experts on topics far beyond their special competence, thereby creating a new priesthood.[1]

As should be clear from the argument of the preceding chapters, this as easily gives rise to circularity as to the grounding of value in the bedrock of fact. Here I will explore some of the ways in which ideas articulated or assumed in a scientific context are taken up in the culture or prepared for absorption by the culture. Both specific hypotheses and the frameworks within which they are evaluated are taken to have implications for cultural values and interests. Some of these implications are clear to and made explicit by the researchers; others are not. In a final section I shall develop the implications of different accounts of scientific knowledge for the understanding of this phenomenon.

Science in Society

Scientific claims and ideas have an influence on public (governmental) policies, on the social values informing policy, on informal policies, and on cultural ideals. By informal policies I mean institutional practices or policies of action that are generally accepted but not legally or administratively articulated or prescribed. Many medical, educational, and social welfare practices are governed by informal policies as well as by official policies to whose violation is attached some form of official sanction. Informal policies have their source in social values, as for instance welfare policies and their implementation are informed by social attitudes towards motherhood. By cultural ideals I mean norms of behavior or types of individual behavior accepted as desirable within a culture. Such norms or types are presented as the best sort of variety within a kind to be. While there may be no official sanctions attached to failing to satisfy or to aspire to satisfy such ideals, one runs the danger of encountering derision and discrimination by such refusal.

My aim in this section is to explore the kinds of impact certain types of scientific research on the biological bases of human behavior, cognition, and temperament have on a chosen set of social values and cultural ideals. My focus will be on the work that has been reviewed in prior chapters, though I will bring in comparable associated research

[1] See Nelkin (1987) for an analysis of the effects of science journalism.

programs from time to time. The normative questions I will review include social equality, the ideals of personhood and the associated ideals of sexuality, and the ideals of personal liberty and responsibility.

Social Equality

By social equality we mean something both deeper and less tangible than political equality. Political equality means something like formal equality of access to the formal decision-making institutions of a community, state, or nation. Thus, "one person, one vote" is a slogan of political equality, while institutionalized means of limiting access to the vote such as poll taxes, literacy tests, and more blatant methods of disenfranchisement are signs of political inequality.

Social equality has in part to do with image and status—two individuals are social equals if they associate with the same group of people, go to the same status parties and entertainments, have similar weight in their communities—and in part to do with entitlement to a society's resources. Our equal opportunity policies guarantee (or are intended to guarantee) to everyone an equal chance at achieving high status and the entitlement to resources that accompanies such status. They represent a compromise between a commitment to hierarchy (in its contemporary guise of meritocracy) and a commitment to social equality—from each according to ability and to each according to contribution. (Never mind that we have a peculiar method of measuring contribution.) The idea of equal opportunity requires that each person not be hindered by societally created obstacles at the outset of her or his quest for a satisfying life. This idea has given rise to a variety of new education programs designed to compensate for the obstacles imposed by past discrimination and exclusion. Such programs assume that innate differences in ability are uniformly spread throughout social classes and that compensatory education will eventually mean a distribution of significant social groups—the sexes and racial groups—in proportion to their distribution throughout the population.

Research supporting the biological basis of observed group differences has a contrary implication, one not lost on the champions of such research. The implications of the work of A. A. Ehrhardt and her various collaborators and fellow gender role researchers, discussed in Chapter Six and Chapter Seven, extend quite deeply and subtly into the educational realm. Their behavioral neuroendocrinology attributes, at a minimum, higher levels of "energy expenditure" to the male hormonal profile than to the female one. This reinforces expectations of boisterous and aggressive behavior from boys and encourages teachers to treat the greater aggressiveness of males (noted, for in-

stance, in classrooms with limited computer access) as inevitable. Similarly, the acceptance of a biological basis for the apparent emphasis placed by girls and women on relationships, family, and nurturance translates into reduced expectations for female achievement in nontraditional endeavors. The traditional practice of tracking boys into athletics and "shop" and girls into home economics and secretarial courses can be seen as giving educational expression to beliefs about natural differences. Affirmative action for women and girls can be expected to produce little or no change if women are biologically disposed to avoid the type of gainful activity hitherto reserved for males, if women instinctively gravitate toward activities involving less self-assertion and more other-nurturance. The type of affirmative intervention at an early age that encourages young women to think about nontraditional adult roles for themselves is equally doomed by biology. As the subtitle of a recent book indicates, we will have come to "the limits of nonsexist childrearing."[2]

The work of Ehrhardt et al. translates vaguely into support for traditional roles—whether in implicitly permitting teachers to allow boys to get away with less self-discipline and greater physical expressiveness or to encourage girls to be quiet and more domestically oriented. Research on cognition and intelligence translates much more dramatically into educational policy. The suggestion that racial differences in average I.Q. scores are biologically based was used explicitly to support claims that efforts at compensatory education for Black children were futile.[3] Similarly, the recent work arguing for a biological basis for sex-differentiated performance levels on mathematics and spatial skills tests is presented as (1) an explanation for the absence of women from the sciences (they can't do it) and (2) reason to abandon "math anxiety" or other compensatory programs for girls. A comment by Hugh Fairweather on a review of studies of sex differences in brain asymmetry applies just as aptly to this related work:

> All in all one is most distressed . . . at the lack of *thinking* as opposed to *data gathering* that has taken place in this area in the last decade. Surely it is time we looked to do more than collect suggestive, ostensibly value-free, isolated pieces of natural history. It is not so much that questions such as "why" and "does it matter" remain unanswered—they have yet to be asked.[4]

[2] Stein (1984). For an attempt to make explicit the political implications of this book (and therefore of the biological work on which it relies) see Sobran (1984).

[3] Jensen (1969).

[4] Fairweather (1980), p. 325.

Just as with the work on gender role behavior this work on cognition shows no sign of reflection or analysis on the part of the researchers. Correlation after correlation is produced with no attempt to understand just what it is that is being measured or its relation to associated phenomena. Nevertheless, again like the gender role behavior work, the cognition research is absorbed into the literature, creating in some circles a fact and its explanation. This work is appealed to in other contexts as explaining the low representation of women in such fields as engineering, architecture, and the physical sciences.[5] And, if women are underrepresented in these fields because of an inherent liability, then various programs, such as affirmative action recruiting or educational programs to help young women overcome "math anxiety" and other resistances to mathematics and the sciences, are a waste of time, energy, and money. Equal opportunity requires that we remove socially created obstacles, not that we erase individual differences. The danger of uncritical reception of the cognition research is not that women may not be given an equal opportunity to apply for positions in these fields but that the social programs designed to enable them to compete on an equal footing will be cut.

Benbow and Benbow and Benbow and Stanley warn that one cannot infer from their work that any individual woman will not score in the very high ranges or perform at very high levels in work requiring mathematical ability. The point they seem to be making with this warning is that one should not discourage a person from pursuing mathematical study just because she is a girl. The implicit message, however, is that nothing need be changed to ensure that those girls who do have mathematical ability will be able to exercise it. Thus, they quite miss the point of the "politically motivated" objections to their work. As Alice Schafer and Mary Gray put the matter in a *Science* editorial, objectors are concerned about the potential impact of this work on funding agencies such as the NSF.[6] These agencies are not likely to allocate money to remedy a situation that is brought about by "natural" causes rather than by social injustice.

Ideals of Personhood

Ideals of personhood are models to which we aspire or whose realization is urged upon us. Aristotle's magnanimous man of virtue was one such ideal. Such ideals characterize individuals more or less imperfectly. To attribute the status of ideal to a description implies that the

[5] For discussion see Haas and Perruci, eds. (1984).

[6] Schafer and Gray (1981).

better a person exemplifies that description, the better sort of person she or he is. The content of an ideal may be identical with that of a stereotype. The difference between ideal and stereotype is a difference of function. A stereotype has a descriptive function, and an ideal has a prescriptive function. Our concepts of masculinity and femininity are examples of such dual function contents. We stereotype males and females when we describe (and act towards) them as though all men equally exemplified masculinity and all women femininity. It is clear that these gender concepts are also prescriptive ideals. We have terms of derogation for those who depart noticeably from them: for instance, "sissy" and "tomboy," "bitch" and "wimp."[7] And the famous Broverman study of the early 1970s showed that psychotherapists, those guardians of personal identity, tended to have dichotomous conceptions of the "healthy," that is, ideal, personality for males and females.[8]

Models of masculinity and femininity confront us in the vehicles of popular culture, in children's toys, and in our imaginations. Their existence as ideals means that individuals will try to mold themselves to the appropriate image to some degree. The molding often involves reshaping of the image: the cowgirl in frilly pink but still serviceable western gear. It also means that individuals who endorse the ideals will see themselves and others as conforming to them even when they do not. This is less a matter of simple self-deception than of directing one's attention to certain features and not others: to the pink rather than to the sturdy leather.

The regulative or prescriptive character of masculinity and femininity is also evidenced in the character of the judgments we make of those whom we must acknowledge as not realizing those ideals. They are either moral failures (the sissy and the tomboy, the wimp and the bitch) or victims of nature. The category of transsexual is designed to accommodate those whose physical bodies fail to accord in some way with their subjectively felt gender identification. By defining the transsexual as in need of surgical or hormonal therapy to correct nature's mistake, the gender dimorphic ideal is preserved.[9] Its role as an ideal is further revealed by the study of cultures with nondimorphic gender. Some American Indian cultures, for instance, have (or had) the category of

[7] Sandra Harding usefully distinguishes three levels of gender attribution: cultural/symbolic, social, and individual. See Harding (1986), p. 18. What I am calling ideals of personhood are articulated at the level of culture and function as ideals to the extent that we expect individuals to conform to them.

[8] Broverman et al. (1970).

[9] See Kessler and McKenna (1985).

"berdache"; which refers to individuals who took on or became a gender other than that assigned to them at birth. The berdache was a socially recognized, often honored, kind of person in these societies. While the different cultures vary in the modes of institutionalizing cross-gender roles, cross-gender individuals are not victims of a mistake requiring surgical correction but a member of a distinct classification that stands alongside what Western Europeans would identify as masculinity and femininity.[10]

Gender concepts play a similar role in the discussion of homosexuality. Europeans identify gender in alien cultures using individuals' sexual attachments as a criterion. Given a dimorphic gender classification, homosexuality, like transsexualism, is viewed as an endocrinological/developmental pathology. The goal of studying homosexuality, as evidenced in the titles and conclusions of many of the scientific articles on the subject, is its management and control, including its prevention. In an interview with science journalist Jo Durden-Smith, neuroscientist Roger Gorski reveals this underlying aim:

> There's something reductive and scary about a situation in which you *might* be able to ask a mother whether she wants testosterone treatment to avoid having a homosexual son. And of course we know nothing like enough yet about the actions of hormones to come to such broad general conclusions. Nevertheless, what Dörner is saying is very suggestive.[11]

The issue is not whether Gorski finds this prospect "scary" but that such intervention is seen as the expected end of research on the hormonal bases of homosexuality. Gorski is referring here to some of Gunther Dörner's ideas. In the discussion in the previous chapter I noted some of the problems in Dörner's theory of dimorphic male and female "mating centers." In order to overcome those problems Dörner has invoked the notion of maternal stress to explain the selective inactivation of fetal testosterone. Gorski is presumably thinking of treatments that would circumvent or override the effects of maternal stress (or whatever else is preventing the release or utilization of testosterone by the developing male fetus). His response makes clear the direction of understanding towards the goals of management and control.

Dörner's theory also reveals the mutually reinforcing effects of scientific and cultural imagery. Homosexuality is (still!) proscribed by the culture as both contranormal and wrong. Science, in the person of the

[10] Blackwood (1984). See also Martin and Voorhies (1975) and Williams (1986).

[11] Durden-Smith (1980), p. 96.

behavioral endocrinologist, remains liberally neutral on the moral questions but provides marvelous imagery to support the culture's judgments of contranormality. Homosexuality as well as other deviations from the dimorphic gender ideal are rather like cases of switched parts. In the ideal or normal case all the female parts from genes to neurons are united in one body and all the male parts in another body. The result is two types of individual: one with female reproductive capacity, feminine behavior, and a sexuality oriented towards men, the other with male reproductive capacity, masculine behavior, and a sexuality oriented towards women. Differences from that norm are a matter of mixing parts—mating and other brain centers, hormones, genitals—innappropriately and can be prevented or corrected by proper hormonal management or surgical therapy. It's like an auto parts store. Proper management of the inventory means that the right parts will reach their destinations. Carelessness means that a part for the diesel model will be installed in the gasoline-powered model and vice versa, with eventual disfunction.

The power of this view is most clearly evidenced in the feminist thought that assumes as explanandum the problematic of gender dimorphism. Many feminists have felt that the appropriate response to the biological determinism just described is to seek social and psychological explanation for dimorphism rather than to attempt to explode the very idea as a category applicable to individuals. The highly influential work of Nancy Chodorow is an example of feminist research that reinforces the assumption of dimorphism. Her version of object relations theory attributes the persistence of gendered individuals to the asymmetric relations of male and female children to their primary attachment figure (their mother).[12] Androgyny theory, too, continues the myth of dimorphism by positing two axes of personality.[13] Its radical break is to suggest that each of the ideals actually represents incomplete personhood and that wholeness resides in their integration. In both these debates masculinity and femininity are thought of as real elements of a dichotomy emerging from the observation of human experience rather than as cultural constraints imposed on that experience. Even though the goal of these analyses is to point the way towards overcoming gender dichotomization—or at least the differential value placed on women and men—by accepting a dichotomous classification system and hence neglecting the actual variety of human expression, they reinforce the dichotomy as much as they challenge it.[14]

[12] Chodorow (1978). See also Dinnerstein (1977). For criticism of such accounts of gender from a different perspective see Spelman (1989), pp. 80–113.

[13] For discussion of the concept and ideal of androgyny see Stimpson (1974); Warren (1982); Beardsley (1982). Psychologist Sandra Bem, who once advocated androgyny as an alternative ideal, now urges the development of "gender aschematism," which avoids some of the problems of androgyny. The concept of gender schemata also avoids the problem of treating masculinity and femininity as inherent characteristics. See Bem (1985).

This form of response works synergistically with the biological determinism it opposes to reinforce the status of general gender dimorphism as an ideal of personhood. As long as feminists counter theories of biological determination of gender difference and sexual orientation with competing environmental explanations of their origin, the discussion will revolve around the dimorphic center. As long is it does so, biologically oriented scientists and thinkers will continue to advance biological determinist theories. As long as they do so, dimorphism will remain unexamined as reality and as ideal. As long as dimorphism remains an ideal, individuals will attempt to conform to it. And, finally, as long as individuals attempt to conform to it, dimorphism will appear to be enough of a reality to require explanation. This schematic loop suggests the ideological power of gender-dimorphic concepts. The etiology of gender role behavior and sexual orientation is certainly more complex—indeed, the fact of nonconformity demands a more complex account. My point here is that as long as dimorphism remains at the center of discourse, other patterns of difference remain hidden both as possibility and as reality. In particular, the idea that there could be a multiplicity of modes of personality organization linked to sex and sexuality—a multiplicity of genders constructed at the intersections of biological sex, sexual orientation, reproductive status, class, race, and sexual ideology or morality, for instance—remains submerged.

Political Ideals

The final aspect of influence I wish to address is the impact of biological research on our ideals of liberty, autonomy, and responsibility. These concepts are central to our traditions of moral appraisal and political equality. I shall briefly indicate their interconnections before discussing the implications of some of the biological research.

Political liberty involves two sorts of freedoms. The negative freedom is the individual's freedom from (unwarranted) governmental control in the conduct of her or his life. The positive freedom is the right and ability to participate actively in the decisions that must be

[14] The same can be said of the new celebration of femininity in some texts of French feminism.

made at a level, broadly speaking, of community. (City, state, nation are the traditionally recognized units of political authority. Some feminist political theory suggests that we include family as well, or at least that the exclusion of family from the domain of the political results in an incoherence when women are explicitly included.[15])

The ideal of political liberty presupposes both personal autonomy and personal responsibility. By personal autonomy I mean independent decison making. This does not imply random or arbitrary choice but decision making resting primarily on one's own values, beliefs, and deliberation as opposed to action or decision that is primarily a product of forces outside the self. Nor does it imply decision making divorced from a social context. The values, et cetera, upon which autonomous decision making rests may have their origins in an individual's culture; what makes them one's own is that one endorses them as such. By personal responsibility in this context I mean effective decision making. This means that those decisions reached independently for the most part result in actions described in the decisions' propositional contents. This idea is the basis of our colloquial notion of responsibility, that is, that an individual's actions can be attributed to her or his intentions. Clearly the status of political liberty as an ideal rests on assumptions regarding the empirical possibility of self-determination by those on behalf of whom personal liberty is claimed. Autonomy and responsibility, while empirical assumptions relevant to personal liberty, also partake of the status of ideals. They acquire this status through being perceived as the achievements of the maturing human, realized to a greater or lesser degree in each of us. As ideals they receive different interpretations and different valuations in different cultures. What concerns us here in the context of biology is the relation of these assumptions underlying our political values to assumptions and claims in biological theorizing.

The research discussed in Chapter Seven presents us with distinctly contrasting sets of relevant assumptions. The behavioral endocrinology work which supports the view that certain behaviors result from prenatal hormonal organization of the brain proceeds on the basis of assumptions about animal modelling and what was termed linearity as well as on the basis of methodological atomism. I shall discuss each of these and contrast their implications with those of the quite different group selective theory of cortical function.

The implications of the assumptions of linearity and "zoocentric" theorizing are apparent in the choice of language by their proponents.

[15] Compare Okin (1979).

Donald Pfaff, for instance, speaks of biology as *limiting* us. The focus is on the constraining rather than on the enabling aspects of biology. In an essay entitled "The Neurobiological Origins of Human Values" Pfaff takes reciprocal cooperation to be paradigmatic of ethical action.[16] He argues that reciprocal cooperation can be analyzed into four modules, each of which can be accounted for in fairly straightforward neurophysiological terms: (1) representing an action, (2) remembering its consequences, (3) associating the consequences with oneself, and (4) evaluating the consequences. According to Pfaff, "except where motor acts which require neocortex for their very execution are involved, ethical behavior may consist of a series of relatively primitive steps, in which, especially in their association with positive or negative reward, neurologically primitive tissue in the limbic system and brainstem, play the crucial roles."[17] Ethical behavior is analogous to reproductive behavior in being susceptible to biological analysis if properly analyzed into its constituent steps.

I am not concerned with evaluating Pfaff's particular claims about the neurological mechanisms underlying each module but wish to draw attention to the reconceptualization of action that this analysis involves. An instance of reciprocal cooperation is the outcome of a sequence of physiological steps. It is classified as reciprocal cooperation because it is describable by a rule of cooperation, not because the behaving individual has followed such a rule. This classification procedure is similar to that which applies the name "altruism" to self-sacrificing behavior in ants. In Pfaff's description the idea of following a rule—the rule "do unto others as you would have done unto you" that he isolates as an ethical universal—is squeezed out of the account. A rule worth its salt as a rule, as Wittgenstein and others have taught us, is one that can be followed or not, which can be broken. There's no question here of the organism deciding whether or not it will follow the rule, or of deciding which rules apply in a given situation, or of sorting through conflicting moral demands. In fact, Pfaff supposes that the putting of self in place of the other—what we might in other contexts call imaginative empathy—could be a matter of failing to distinguish self and other, of forgetting whether the remembered consequences of previous instances of the proposed act occurred to one's self or to the/an other.

Pfaff thus reduces deliberation to relatively low-level processing, most of which can be carried out subcortically. It is subsumed within

[16] Pfaff (1983).
[17] Ibid., p. 149.

the paradigm of simple appetition realized in the limbic system. His procedure is analogous to that of many biologists concerned with the implications of their disciplines for ethics. Ethical behavior loses its status as behavior engaged in out of principle or in order to conform to notions of right and good. It becomes instead behavior that conforms to the writer's idea of what is right and good. The problem of understanding ethical behavior becomes the problem of producing it—of knowing what interventions are likely to increase its frequency. The implication for notions of autonomy and responsibility are clear. As traditionally understood, they no longer characterize human action. Decisions originate in the nervous system of the individual but not in those portions of the nervous system in which the higher cognitive processing involved in conscious inference, valuation, and deliberation is presumably realized. Decision making is thus not subject to conscious deliberation and reflection. These higher level cognitive phenomena become epiphenomenal relative to those neural processes that effectively cause behavior. Consequently, the specific human abilities that ground the claim to political liberty are eliminated by this theoretical treatment of action.

The other side of the undermining of this ideal is the positive support offered for various forms of intervention into individual decision making. The concept of action underlying the behavioral endocrinology program facilitates the medicalization of all sorts of human behavior. Even now rapists are, in some states, being treated with hormones to reduce their libido, as though rape were a matter of excess libido, an individual affliction to be individually cured. Homosexuality has only recently been removed from the American Psychiatric Association's list of personality disorders. And those uncomfortable with their homoeroticism can seek medical interventions in the form of hormone therapy. The point here is not whether such treatments would work but the conception of our own natures that we are encouraged to adopt. Human capacities for self-reflection and deliberation become idle epiphenomena—distractions from the real causal processes producing our behavior; processes at levels to which only the scientist or physician have access. Usually only deviations from acceptable behavior (whatever that may be) are medicalized. Criminality, always a favorite, is once again being given a biological treatment.[18] No one begins by asking for the biological determinants of heterosexuality or of acceptable social behavior, but the consequences of the medical model must be that these, too, are products of biological events early in our development. Although both the favorable and the critical reception of these

[18] Wilson and Herrnstein (1985).

ideas generally ignores this aspect, Pfaff's discussion of ethical decision making beautifully delineates this model's erosion of the idea of the person as a being capable of making and acting on intelligent decisions (whether right or wrong) about her or his own life.

Yet another impact on social ideals flows from the methodological atomism characterizing the research. The human activities studied must be described by externally measurable properties in order to develop their analogies to the animal behaviors whose physiological bases are more accessible to researchers. This behaviorist redescription of action situates it outside of the social context that gives it meaning. The full dimensions of what is decided upon, as well as the descriptions under which it is chosen, are lost from view. Not only is human behavior redescribed as analogous to animal behavior but both animal and human behavior are perceived in analogy to mechanistic systems. Such redescription supports a particular interpretation of political liberty—as the freedom to pursue one's own interest unhindered by external constraints. Social interactions are understood as enhancing or deflecting from one's pursuit of that interest rather as collisions between elastic bodies can change the direction and/or velocity of their motion. The claim of a right to liberty under this impoverished conception is easily given up in the face of biological research that purports to show that the motions constituting behavior are under the control of factors other than an agent's conscious deliberation.[19] Furthermore, the atomistic view of behavior encourages the individual who feels uncomfortable with her- or himself and those from whom she or he seeks help to see the problem as an individual rather than a social one.

Implications of the New Neurophysiology

The study of the biological bases of behavior need not be incompatible with the richer conception of liberty outlined above. I will support this point by briefly reviewing some of the implications of the group selective theory discussed in the previous chapter. Theorists focussing on higher brain function problematize human behaviors that as far as we know are unique to the species: the writing of symphonies, the construction of undecidable mathematical theorems. The questions are not couched in terms of understanding the physiological conditions sufficient to produce the kinds of behavior in question but in terms of understanding in a general way what kind of neurophysiological processes are necessary for intelligent, reflective, self-conscious, creative activity. By asking what the character of brain processes underlying complex human behavior must be the inquiry emphasizes the enabling

[19] See Winch (1958); also Taylor (1971).

rather than the limiting aspects of biology. Because the seat of cognition and intention is located in an organ that is common to all individuals of whatever category, this approach is compatible with assumptions of human equality in the capacities that matter to our status as persons. Because that organ develops its unique or individually differentiated qualities in interaction with experience, multipotentiality rather than limitation emerges as the character of the physiological contribution to behavior. The study of the role of higher brain processes embodying cognition and intention in mediating action returns both autonomy and responsibility to the person. The emphasis on the brain's plasticity and responsiveness to environment allows a role for processed social influence. The brain is an organ integrating inputs from physiology, environment, and, via phasic reentry, its own functions such as memory and self-awareness. Decisions are understood as the result of that integration, rather than as the summation of physiological, environmental, and memory vectors. This places control of action back in the individual consciousness without denying the biological nature of that consciousness or the role of social interactions in the formation of self.

The view of the brain that both guides and is emerging from these studies is thus, at this point, one that makes sense from the perspective of the political ideals outlined above and in light of which those ideals are realizable. The modern versions of these ideals have their origins in the Enlightenment illusion of the self's transparency to itself. The neurobiological work shows that we can reject this illusion without also having to reject the idea that we should strive for forms of political organization that assume individual autonomy and responsibility.

The secular character of contemporary culture means that ideas developed in a scientific context for purposes of research can have a profound impact on social values and ideals, just as social relations and cultural frameworks provide basic models of relationship to be elaborated in the research laboratory. The models in science and the social assumptions have ostensibly different functions, the generation of new knowledge and the guiding of action respectively. The incompleteness of both domains, however, means that each remains open to the other as a source of legitimation. Philosophical views about the nature of scientific knowledge direct us to contrasting assessments of this relationship.

Implications of the Metascientific Views

The account of scientific knowledge developed in the earlier sections of this book is a form of contextualism that understands knowledge as

the historical product of interactions between contextual factors such as social needs, values, and traditions and practices of inquiry such as observation, experiment, and reasoning. This account was defended by appeal to certain features of the relation of evidence and hypothesis and certain features of the formative episodes of modern Western science. It is contrasted with the objectivism and scientism associated with logical empiricism and with the self-ratifying internalism of Kuhn (and of Laudan). These three approaches offer distinct perspectives on the relevance of scientific ideas to social, cultural, and political principles, practices, and ideals. In the preceding sections of this chapter I have detailed the alleged social, cultural, and political implications of certain research programs. These implications have either been explicitly endorsed by those pursuing these programs or represent a simple and, in some instances, naive extension of those programs to the social world. In this section I wish to spell out the implications of the metascientific accounts for our understanding of the extrascientific relevance of scientific theories and hypotheses.

Positivism and Realism

Positivism has different implications depending on which of its associated theses are emphasized. It can be understood as a form of epistemological reductionism. Under this aspect what is relevant to knowledge claims are experience and formal reasoning. If this means that empirical claims should be held to empirical criteria, there would be little to quarrel over. Positivists go even farther, of course, and claim that the very meaningfulness of a statement depends on there being experiential (observational) methods of verifying it. Only those things can be known which can be experienced or which have experiential consequences. Scientific knowledge is the systematically ordered set of accumulated observations expressed in sentences. This has well-known implications for the relation between facts and values. First of all, value claims, not being observationally verifiable, are cognitively meaningless. Values are not potential objects of knowledge but subjective preferences, and value claims are simply expressions of feeling. Secondly, scientific claims will always have priority over value-based claims in the question of what to believe. This follows a fortiori from the proposition that value claims are cognitively meaningless in contrast with scientific claims.

The second aspect of the positivist analysis that bears on this issue is what might be called its epistemological atomism. One implication of the positivist view is that research claims are independent of one another and can be analyzed in isolation. Thus, in assessing the consequences of a claim for social issues one need only examine the evi-

dence for the claim and the particular social or value question under examination. The context in which the research is done is relevant to the assessment neither of the scientific claim nor of its alleged social implications.

One can see these implications deployed in certain of the media debates about science in society. A number of authors writing about research on sex differences assert that alleged findings of that research take precedence over feminist demands for sexual equality and over gay liberationist and feminist demands for an end to the idealization of masculinity and femininity. Thus Jo Durden-Smith and Diane deSimone, the authors of a book of popular journalism, *Sex and the Brain*, write: "[T]he constant scientific debate . . . threatens, in its spreading implications, the liberationist assumptions of feminists and homosexuals. And it undercuts the idea of absolute sexual equality for all."[20] They then go on to say:

> The differences between men and women—all the differences in brain and body and inheritance, in ability, fragility and immunity—are fundamental to our human biology. . . . This knowledge may not serve the turn of some of the entrenched institutions of our society, including big business. It may not suit the psychologized politics-for-self that is the current expression, all too often, of feminism and the other sexual liberation movements. . . . But it may lead to a greater understanding . . . of the essential integrity of the male and female body.[21]

Such overblown rhetoric confuses political equality with biological sameness. My point is that this conflation is encouraged by the positivist tenets.

In a similar vein, in a letter to the *New York Review of Books* psychologist Sandra Witelson attempts to discredit the idea that women have been unjustly turned away from scientific inquiry by appealing to cerebral asymmetry and other work on the biological bases of behavioral sex differences. "And what if natural differences are found to be partly responsible for sex differences in behavior? A physical basis of thought does imply scientific determinism of behavior. Unfortunately, this position is unpalatable to many because it is mistakenly thought to deprive human beings of free will."[22] As the authors of *Sex and the Brain* also insinuate, only wishful thinking prevents one from accepting the truths of science. These sentiments echo B. F. Skinner's similar ridicule of those who objected to his behaviorist objectification of human action in *Beyond Freedom and Dignity*.[23]

[20] Durden-Smith and deSimone (1983), p. 99.
[21] Ibid., p. 299.
[22] Witelson (1985), p. 53–54.

Atomistic positivism supports the false impression that a research program, indeed the whole of scientific knowledge, is constructed additively by the joining of many independent research findings that turn out to be related. The theoretical claims and assumptions of a program are understood as following from the accumulated data rather than as playing any role in determining the collection of data and structuring their interpretation. Each supposed finding is presented in isolation without specification of its scientific context or of that context's larger sociocultural context. Each individual finding, no matter how close to insignificance, can be added to others to create the impression of an overwhelming case for some claim, for example, major structural and functional sex differences in the brain. The claim is not understood as developed in a social context of wild ideas about sex differences or in a research context whose primary purpose is the development of an account of behavior as determined as much as possible physiologically. In fact, Smith and deSimone present the work they discuss as conducted against the (mistaken) spirit of the age. Perhaps there was a moment in the 1970s when sexual egalitarianism seemed in the ascendancy. But this moment cannot have lasted long enough to be noticed by very many.

Another side of logical positivism, which it shares with the scientific realism that is its contemporary successor, is its implicit objectivism and scientism. This can be expressed as follows: there is a truth of matters, and the methods described by positivists and realists are adequate to the discovery of that truth. This assumes the capacity of the methods of empirical science, construed as guarantors of context independence, to fully reveal the actual character of things. This approach, applied to actual sciences, conceals the ambivalence noted in earlier chapters between the prescriptive and descriptive intentions of positivism. Prescriptive positivism provides criteria for the justification of belief and knowledge claims—hypothesis acceptance. Descriptive positivism claims that a particular field of inquiry satisfies these prescriptive requirements. Biologists who seek the biological foundation of value take value questions to admit of true (or false) answers. Those answers are obtainable by the methods of empirical science. Roger Sperry gives voice to this notion in his book *Science and Moral Priority*:

[23] Skinner (1971).

> Instead of separating science from values, the current [Sperry's] interpretation leads to a stand in which science—in its purest sense as a means of revealing an understanding of man and the natural order—becomes the best source, method, and authority for determining the ultimate criteria of moral values and those ultimate ethical axioms and guidelines to live and govern by.[24]

Granted, Sperry envisions an ontologically more permissive neuroscience than some might countenance, but this, too, he feels to be mandated by empirical methods. George Pugh describes his book *The Biological Origin of Human Values* as explaining " 'human values' as manifestations of a built-in *value system*, which is an essential part of evolution's basic 'design concept' for a biological 'decision system.' "[25] Pfaff's theory about the biological bases of morality is another example of the denial of independent meaning to ethical statements.

Thus one version of positivism involves the displacement of value-based claims by fact-based claims. Demands for equal treatment on the basis of fundamental sameness give way to proofs of difference. The second version treats value claims as themselves decidable on the basis of factual investigation. To reject the value implications requires showing that the scientific claim is false. History shows us that this is not effective, as new claims spring up to replace the old. In earlier chapters I've shown how inquiry is dependent upon assumptions establishing the relevance of data to hypotheses. I then showed that in a variety of research contexts those assumptions include value-based assumptions. The empiricistically inclined lay person, looking to scientific research for guidance in complex matters of social policy or cultural ideals, is as likely as not to provide the final conclusions of a circular argument.

There is a final expression of positivist views that should be noted. Biologist Helen Lambert has deplored the waste of intellectual and political energy spent on arguments regarding the biological basis of sex differences in behavior.[26] She seems inclined to accept on face value much of the research purporting to demonstrate such a basis. Contrary to many of the authors surveyed in this section, however, she attempted to separate the research findings from their commonly alleged implications for social action. The distribution of social benefits, she argued, should be independent of the outcome of sex differences research. We are not, as a society, bound to accept biologically based sex differences as immutable or as implying that individuals should be

[24] Sperry (1983), p. 113.
[25] Pugh (1977), p. 5.
[26] Lambert (1978).

tracked towards occupations suitable to their group's innate endowments or lack of them. Lambert seems to be insisting on the primacy of the commitment to equality and on a strong interpretation of that commitment, that is, to doing what is necessary to assure equal distribution of benefits. Biological differences (just like socially based differences) require that we compensate in the relevant ways.

While Lambert's approach turns aspects of the positivist view on their head, it suffers from two problems. One, of course, is the lack of political will. United States society, in spite of our rhetoric, does not have a primary commitment to equality. Decision making at many levels is governed by beliefs about differences and similarities, which are transmuted into beliefs about superiority and inferiority. Furthermore, the commitment to equal opportunity is a commitment to removing *socially* created obstacles, not "natural" ones. Secondly, even in engaging in compensatory measures, beliefs about the nature of the differences for which one wishes to compensate will determine the precise character of the compensatory action. Thus, research about their bases is relevant even if equal distribution of social benefits is preferred to distribution according to ability, contribution, or some other differentiated measure.[27] Overcoming biological differences in ability is likely to involve different sorts of interventions than changing the social conditions that result in different abilities.

Wholism

Wholism is the view that the meaning of any statement can only be understood in the context of the entire theory to which it belongs, that no part of a theory can stand independently of the whole. All meanings are theory-laden, and theories are thus incommensurable. The wholist position, too, can support two different stances regarding the relevance of scientific inquiry to norms, ideals, and values. Neither of these treats scientific research as an independent source of validation for such norms. It is not surprising, therefore, that we do not find expressions of these views among the scientists asserting the relevance of science to the solution of social problems and conflicts. I shall quickly sketch the two ways in which one can develop the wholist position.

If one holds that value-based assumptions are excluded from scientific theory, it might be argued that scientific claims are logically irrelevant to value claims because they are embedded in different theories. The meanings of any shared terms are different, and different valida-

[27] The recent EEOC case against Sears is an example of this. See Hall and Cooper (1986).

tional methods are appropriate to the two types of theory. For example, the term "natural" might occur in a scientific claim and in a moral claim. Suppose one claim is "The dominance of men over women is natural" and the other is "Neither the state nor individuals should attempt to alter natural relationships." If these statements are part of different theories, then "natural" is defined by each theory, and what is declared natural in one may not be so in the other. An emphasis on incommensurability, then, dictates the irrelevance of ideas developed and defended in the context of a scientific research program to cultural values and ideals.

For their part the scientists who invoke Kuhn to explain how particular social and cultural values have affected a body of research seem to make use of a different form of wholism. They seem to think that the political valence of scientific theorizing is inevitably a function of observational data laden with cultural assumptions and values. If we follow Hesse in saying that the theory/assumptions with which the observations are laden are not, or not necessarily, part of the theory whose support is in question, the account shades into the contextualist one discussed below. A strong wholism, by contrast, would hold those assumptions to be part of the theory purportedly supported by such data.

In this interpretation of wholism the ensemble of data, theory, and assumptions must be understood as a whole. Each element, including value-based assumptions, can only be understood in the context of the others. In this case value claims that are a part of a theory would be validated by the factual data of the theory as much or as little as any other claim is. Given the incommensurability of theories, however, value conflicts could not be settled independently of any theory. Thus, there would be no independent way of choosing between a theory that claims that some relationship is natural and one denying this, or between a theory prohibiting interference in natural relationships and one permitting it. If we are appraising theories in a scientific context and using Kuhnian criteria of evaluation such as problem-generating and problem-solving capacity, this is not a problem. If, however, we move outside the research context to that of social action and public policy, the resulting circularity becomes vicious and invalidating. On neither interpretation of wholism, then, can scientific inquiry be understood as independently relevant to the support of values or ideals.

Contextualism

The contextualist has greater flexibility than adherents of either of the internalist positions just discussed. The contextualist seeks not to eliminate but to understand the role of contextually based assumptions in scientific reasoning. Some background assumptions may involve conceptual, metaphysical, and normative dimensions that elude assessment by strict empirical criteria. Others may be subject to fairly straightforward empirical assessment. Arguments that use factual hypotheses to undermine or support claims about values provide good subjects for study. When the support for the relevant factual hypotheses is made explicit, it often turns out to include those same value claims or their presuppositions. The contextualist takes the presentation of arguments or positions such as those outlined above as an incentive to further investigation, not as an imperative for assent.

The work on mathematical ability, for example, rather than compelling the assent suggested in Witelson's letter quoted above, invites an exploration first of the assumptions and argumentative structure of the work and second of their relation to their social and cultural context. Several assumptions regarding the tests of ability were isolated earlier. These included: (A_1) there is only one form in which mathematical ability is expressed; (A_2) that form is expressed in performance on standardized tests such as the M-SAT; (A_3) the content of a problem has no bearing on the formal properties of a problem nor on an individual's grasp of those properties. A further assumption concerns the subjects taking the tests: (A_4) the appropriate measure of mathematical education is the amount of time spent in classes devoted to specific mathematical subjects.

These assumptions about what the tests measure and about the uniformity of preparation of the subjects tested facilitate the interpretation of variation in test performance as variation in innate mathematical ability. The contextualist must ask (1) what reasons can be offered for these assumptions and (2) what interests are served by the unchallenged persistence of these assumptions. What the tests test are mathematical performances and abilities of the sort that are used and expressed in the world in which the tests are devised. The fourth assumption, about the uniformity of preparation, has been persuasively rebutted by (1) the observation that male and female children are provided with toys and play experiences that encourage the development of quite different skills and (2) studies showing that girls and boys receive different treatment from the same teachers in the same classroom.[28] It is deficient on straightforward empirical grounds.

The first three assumptions are less straightforward and have not been investigated systematically. The first, in particular, might be

[28] Buerck (1985).

thought to mimic common sense: What is mathematical ability if it is not the ability to do what mathematicians do? That most mathematicians are male and thus are likely to have had different sorts of formative experiences than women of comparable class and race is salient only to those who have been excluded from careers in mathematics or whose abilities are impugned by the research in question, not to those whose position and success relative to others is justified by the research. The interests of the latter are served by not challenging the assumptions, indeed in not even seeing that they are assumptions. Certain developments in the study of mathematics education, particularly in the study of mathematics learning by groups hitherto excluded from scientific, mathematical, and technical subjects, do suggest that they may be problematic.[29] There is as yet, however, no conclusive reason to accept or reject them, but there is surely a certain amount of conventionality in deciding the boundaries of mathematics, a conventionality which leaves room for the play of various sorts of contextual interests and values.

Similarly, the persistence of research that implicitly eliminates autonomy and the basis of liberty and responsibility from our concepts of human nature can be understood as a function of the convergence of a number of contextual interests. The professional interests of biologists are surely served by research that brings as much of human behavior as is possible under biological control. The interests of sociomedical bureaucracies, however, are served by research that promises to reconceptualize human behavior as the product of discrete, measurable, and manipulable factors.[30] While the interventions sanctioned by the various theoretical directions currently being pursued are applied only to "deviants" and criminals as corrective (and thus "humane") approaches, the very idea of such intervention gives tremendous prescriptive power to the categories of normality and deviance. That one's behavior and dispositions could be medically corrected implies a norm worthy of such effort. This in itself is sufficient to create a degree of voluntary conformity to the norm, a self-regimentation that even further reinforces the rule's status as a norm.[31] The interests of a bureaucracy that requires a cooperative population to

[29] For the role of early play experience see Fennema and Sherman (1977). For classroom experience see Becker (1981) and Gore and Roumagoux (1983).

[30] Donna Haraway has documented the convergence of sociopolitical and scientific concerns in twentieth-century primatology. See Haraway (1978, 1985b).

[31] The work of Michel Foucault on knowledge and power brings out the basis in individual self-policing of order in the bureaucratic state. See Foucault (1977, 1978, 1980).

effectively exercise power are also served by the scientific legitimation of informal distinctions between normal and abnormal.

The components or auto parts conception of the bases of human personality (at least as it relates to sex and gender), moreover, reverberates in a troublesome way with the high tech aesthetic of late twentieth-century industrial societies. While robots and other instruments of machine intelligence can be understood as extending our manual and intellectual capacities, the auto parts view has a more reductive flavor. We become ourselves an integrated component system, plugged into the circuit like any other machine.[32]

As in all of the work discussed in this chapter, the point is not that these interests direct the research in any overt way. Rather, they create a climate in which the assumptions that shape the research are taken for granted, as a part of common sense, and are to that extent immune from scrutiny. Scientific knowledge, as I argued earlier, rests on a bed of presuppositions about what questions are important, what sorts of connections are meaningful, about the general direction of causal relations (or more precisely, about which causal relations are worth investigating or establishing). Research programs that apply the same models reinforce each other and their shared presuppositions without ever needing to subject them to direct examination. Those presuppositions that cohere with the interests of the (sub)culture of the researchers will not be seen as assumptions but, if seen at all, as self-evident truths.

As a view about scientific reasoning contextualism is quite consistent with a modified empiricism, understood as a prescriptive theory. Such a modified empiricism, cleansed of restrictions on meaningfulness and purged of assumptions about the absolute or fixed character of observations, would restrict instead those things we could be said to know. What we can know is what we can experience. The conclusions of inferences from experience that must use additional substantive assumptions as premises cannot be known absolutely. We give the name "knowledge" to the complex and more or less coherent sets of hypotheses, theories, and experimental-observational data accepted by a culture at a given time because this body of ideas functions as a public fund of justification and legitimation for new hypotheses as well as for action and policy.[33] This socially created knowledge which integrates experience and the needs and assumptions of a culture is true relative

[32] Haraway (1985a) alludes to the ambivalence inherent in our contemporary relationship to machines. The cyborg imagery aptly conveys its charged duality.

[33] The coherence referred to here is at best within fields or subfields and not across fields. Compare the discussion in the final section of Chapter Ten.

to those assumptions and, to the extent that those assumptions are context-dependent, is relative to that context. If scientific knowledge is social knowledge, to hold scientific claims to strict empirical criteria is to remain agnostic with respect to the context-independent truth or falsity of many of them. Unlike the empiricist-absolutist, the contextualist does not expect scientific claims to be capable of displacing value claims, nor are claims that are not experientially verifiable meaningless. Unlike the wholist, the contextualist can accept the primacy of experience as arbiter of knowledge claims. Certain claims, however, are not susceptible to direct empirical confirmation and so cannot be known to be true or false.

According to this view, shifts in theoretical orientation and in the relative centrality of observational data are comprehensible as the expression of complex interactions between what is known, what is assumed, and social-cognitive needs. While the official picture of a field presented in its textbooks is the picture of a uniform and consistent understanding, the background from which this understanding emerges/is selected contains alternative interpretations of the data included in the textbook picture as well as data inconsistent with it. The selection represents guesses about where a field is going, which itself is a function of what a society (those in a society with the power to effect their preferences and privilege their needs) thinks it should know or wants to know. Shifts in the official picture involve the saliency not necessarily of new facts and ideas but of facts and ideas, some accepted, some submerged, that in connection with social-cognitive needs assume sufficient coherence to constitute a uniform story.

In assessing claims about the social and ethical implications of some current research, therefore, the contextualist looks both to its larger scientific context *and* to its cultural context, to the framework of theory and assumptions within which it is embedded and to the needs and values they promise to satisfy.

CHAPTER NINE

Science and Ideology

SOME of the political critics of science have gone beyond a critique of particular research programs to argue that modern science, or the modern practice of science, is inherently oppressive. They have raised a corollary demand for a "new" science—a science that is liberatory rather than harnessed to the forces of domination and oppression. What can the analysis developed so far offer in response to such demands? In this chapter I shall consider the idea of a feminist science and use the ideas presented in Chapter Seven and Chapter Eight to provide an illustration of one possible type of feminist science. I shall then discuss the ideas of a group of contemporary thinkers who address the relations between politics, ideology, and science, and then focus on the convergences and divergences of my analysis with the views expressed by several neo-Marxist scientists, Jürgen Habermas, Michel Foucault, Evelyn Fox Keller, and Donna Haraway. Ideology may indeed operate globally in mainstream science, but counterideologies, if they are to be useful in changing science, must be brought to bear locally on specific research programs.

FEMINIST SCIENCE?

The hope for a feminist theoretical natural science has concealed an ambiguity between content and practice. In the content sense the idea of a feminist science involves a number of assumptions and calls a number of visions to mind. Some theorists have written as though a feminist science is one whose theories encode a particular world view, characterized by complexity, interaction, and wholism. Such a science is said to be feminist because it is the expression and valorization of a female sensibility or cognitive temperament. Alternatively it is claimed that women have certain traits (for example, dispositions to attend to particulars and interactive and cooperative social attitudes and behaviors rather than individualist and controlling ones) that enable them to understand the true character of natural processes (which are complex and interactive).[1] While proponents of this interactionist view see it as

[1] This seems to be suggested in Bleier (1983); Rose (1983); and in Sandra Harding's early work, for example, Harding (1980).

an improvement over most contemporary science, it has also been caricatured as soft—or antimathematical. Some women in the sciences who feel they are being asked to do not better science but inferior science have responded angrily to this characterization of feminist science, thinking that it is simply new clothing for the old idea that women can't do science. I think that the interactionist view can be defended against this response, although that requires rescuing it from some of its advocates as well. However, I also think that the characterization of feminist science as the expression of a distinctive female cognitive temperament has other drawbacks, the greatest being that it conflates feminine with feminist. While it is important to reject the traditional derogation of the virtues assigned to women, it is also important to remember that women are *constructed* to occupy positions of social subordinates. We should not uncritically embrace the feminine.

This characterization of feminist science is also a version of recently propounded notions of a "women's standpoint" or a "feminist standpoint" and suffers from the same suspect universalization that these ideas suffer from. If there is one such standpoint, there are many: as Maria Lugones and Elizabeth Spelman elucidate in their article "Have We Got a Theory for You!: Feminist Theory, Cultural Imperialism, and the Demand for 'The Woman's Voice,' " women are too diverse in our experiences to generate a single cognitive framework.[2] In addition, the sciences are themselves too diverse for me to think that they might be equally transformed by such a framework. The account of scientific knowledge defended here makes another conception of feminist science possible. By focussing on science as practice rather than content, as process rather than product, we can reach the idea of feminist science through that of doing science as a feminist.

Let me illustrate this point by talking about approaches to the biology of behavior. In chapters Six through Eight, I analyzed the logical structure and social implications of research on the biological bases of alleged gender difference and sex-related behavior. The behavioral endocrinology studies discussed in Chapter Six are vulnerable to criticisms of their data and of their observational methodologies. They also show clear evidence of androcentric bias—in the assumption that there are just two sexes and two genders (us and them), in the designation of appropriate and innappropriate behaviors for male and female children, in the caricatures of homosexuality, in the assumption of male mathematical superiority. While these sexist assumptions do affect the

[2] Lugones and Spelman (1983).

way the data are described, causal inferences from the alleged data are mediated by the linear-hormonal model that functions as a background assumption. To put it crudely, fetal gonadal hormones organize the brain at critical periods of development. The organism is thereby disposed as an adult to respond in a set series of ways to a range of environmental stimuli.

In Chapter Seven and Chapter Eight I contrasted this model with an alternative model of the role of the brain in behavior, drawn from the selectionist theory of brain development and function. Such a model allows not only for the interaction of physiological and environmental factors but also for the interaction of these with a continuously self-modifying, self-representational (and self-organizing) central processing system. While my preferences are undisguisable, I have tried to remain analytically neutral and have presented the constitutively based arguments that can be made for both approaches. In work with my colleague, Ruth Doell, however, we have been more partisan.[3] In particular, we have argued that a model of at least the degree of complexity characterizing the selectionist model is necessary to account for the human behaviors studied in the sex hormones and behavior research and that if gonadal hormones function at all at these levels, they will probably be found at most to facilitate or inhibit neural processing in general. The strategy we take is to argue that the degree of intentionality involved in the behaviors in question is greater than is presupposed by the hormonal influence researchers and to argue that that degree of intentionality implicates the higher brain processes.

Abandoning my polemical mood for a more reflective one, as I have done here, I want to say that in the final analysis commitment to one or another model is strongly influenced by values or other contextual features. The models themselves determine the relevance and interpretation of data. The linear or complex models are not in turn independently or conclusively supported by data. I doubt, therefore, that value-free inquiry could reveal the efficacy or inefficacy of intentional states or of physiological factors like hormone exposure in human action. I think instead that a research program in neuroscience that assumes the linear model and sex gender dualism will show the influence of hormone exposure on gender role behavior. And I think that a research program in neuroscience and psychology proceeding on the assumption that humans do possess the capacities for self-consciousness, self-reflection, and self-determination, and then asks how the structure of the human brain and nervous system enables the expression of these

[3] Doell and Longino (1988).

capacities, will reveal the efficacy of intentional states (understood as very complex sorts of brain states or processes).

While this latter assumption does not itself contain normative terms, I think that the decision to adopt it is motivated by value-laden considerations—by the desire to understand ourselves and others as self-determining (at least some of the time), that is, as capable of acting on the basis of concepts or representations of ourselves and the world in which we act. (Such representations are not necessarily correct and are surely mediated by our cultures; all I claim is that they are effective factors in human action.) I think further that that desire on Ruth Doell's and my part is, in several ways, an aspect of our feminism. Our preference for a neurobiological model that allows for agency, for the efficacy of intentionality, is partly a validation of our (and everyone's) subjective experience of thought, deliberation, and choice. One of the tenets of feminist research is the valorization of subjective experience, and so our preference in this regard conforms to feminist research patterns.

There is, however, a more direct way in which our feminism is expressed in this preference. Feminism is many things to many people, but it is at its core in part about the expansion of human potentiality. When feminists talk of breaking out and do break out of socially prescribed sex roles, when feminists criticize the institutions of domination, we are thereby insisting on the capacity of humans—male and female—to act on perceptions of self and society and to act to bring about changes in self and society on the basis of those perceptions. (Not overnight and not by a mere act of will. The point is that we act.) And so our criticism of theories of the hormonal influence or determination of so-called gender role behavior is not just a rejection of the sexist bias in the description of the phenomena—the behavior of the children studied, the sexual lives of lesbians, et cetera—but of the limitations on human capacity imposed by the explanatory model underlying such research.[4]

While the argument strategy we adopt against the linear model rests on a certain understanding of intention, the values motivating our adoption of that understanding remain hidden in that polemical context. Our political commitments, however, presuppose a certain understanding of human action, so that when faced with a conflict between these commitments and a particular model of brain-behavior relationships we allow the political commitments to guide the choice.

[4] Ideological commitments other than feminist ones may lead to the same assumptions, and the variety of feminisms means that feminist commitments can lead to different and incompatible asssumptions.

The relevance of my earlier arguments about value-free science to the issue of feminist science should be becoming clear. Feminists—in and out of science—often condemn masculine bias in the sciences from the vantage point of commitment to a value-free science. Androcentric bias, once identified, can then be seen as a violation of the rules—as "bad" science. Feminist science, by contrast, can eliminate that bias and produce "good," more true or gender-free science. From that perspective the process I've just described is anathema. But if scientific methods generated by constitutive values cannot guarantee independence from contextual values, then that approach to sexist science won't work. We cannot restrict ourselves simply to the elimination of bias but must expand our scope to include the detection of limiting interpretive frameworks and the finding or construction of more appropriate frameworks. We need not, indeed should not, wait for such a framework to emerge from the data. In waiting, if my argument is correct, we run the danger of working unconsciously with assumptions still laden with values from the context we seek to change. The idea of a value-free science presupposes that the object of inquiry is given in and by nature, whereas the contextual analysis shows that such objects are constituted in part by social needs and interests that become encoded in the assumptions of research programs. Instead of remaining passive with respect to the data and what the data suggest, we can, therefore, acknowledge our ability to affect the course of knowledge and fashion or favor research programs that are consistent with the values and commitments we express in the rest of our lives. From this perspective the idea of a value-free science is not just empty but pernicious.

Accepting the relevance of our political commitments to our scientific practice does not imply simple and crude impositions of those ideas onto the corner of the natural world under study. If we recognize, however, that knowledge is shaped by the assumptions, values, and interests of a culture and that, within limits, one can choose one's culture, then it's clear that as scientists/theorists we have a choice. We can continue to do establishment science, comfortably wrapped in the myths of scientific rhetoric or we can alter our intellectual allegiances. While remaining committed to an abstract goal of understanding, we can choose to whom, socially and politically, we are accountable in our pursuit of that goal. In particular we can choose between being accountable to the traditional establishment or to our political com-

rades.[5] The feminist scientist is responsive to the ideals of a political community as well as to some subset of the standards endorsed in her or his scientific community. These allegiances are themselves interactive, as the political ideals may indicate a priority ordering for the scientific standards and vice versa. One colleague has suggested that we can choose to be thus accountable to a world larger than both. I suppose this is so, as long as this world is a definable social community whose members can hold us accountable and not an imagined one or nature itself.

In focussing on accountability and choice this conception of feminist science differs from those that proceed from the assumption of a congruence between certain models of natural processes and women's inherent modes of understanding.[6] It also raises the question of what sort of choice is involved here. Let me address this issue first. To adopt a political framework is to adopt assumptions about human nature and potential. Radical and feminist scientists are poised at the center of tensions between the views embedded in certain scientific research programs and those embedded in their political allegiances. To the extent that they use one or the other in assessing research they are making a choice. Clearly the choice is not arbitrary, but neither is it dictated by data. Obviously model choice is also constrained by (what we know of) reality, that is, by the data. But reality (what we know of it) is, I have already argued, inadequate to uniquely determine model choice. The political choice involved may not be the simple choice of one set of assumptions over another but may be located at another level of thought and analysis. In examining the reasoning in support of some hypothesis in a contested area, however, there is some point at which one discovers political commitment. One may choose, for example, which of one's personae will be admitted to the laboratory. From the perspective of one persona, which includes a plethora of related values, beliefs, attitudes and practices, one assumption or model is clearly correct. From the perspective of another, it is not. In doing science we are, therefore, bringing other considerations to bear, implicitly or explicitly, in adopting one or another theoretical perspective.

The feminist theorists mentioned above have focussed on the relation between the content of a theory and female values or experiences, in particular on the perceived congruence between interactionist, wholist visions of nature and a form of understanding and set of values

[5] This description of the choice facing scientists presupposes that inquiry is always located in a political context. This follows from understanding knowledge as socially produced. Social processes always have political dimensions.

[6] Compare note 1, above.

widely attributed to women. In contrast, I am suggesting that a feminist scientific practice admits political considerations as relevant constraints on reasoning, which through their influence on reasoning and interpretation shape content. In this specific case those considerations in combination with the phenomena support an explanatory model that is highly interactionist, highly complex. This argument is so far, however, neutral on the issue of whether an interactionist and complex account of natural processes will always be the preferred one. If it is preferred, however, this will be because of explicitly political considerations and not because interactionism is the expression of "women's nature."

The accountability I describe does not demand a radical break with the science one has learned and practiced. The development of a "new" science involves a more dialectical evolution and more continuity with established science than the familiar language of scientific revolutions implies.[7] As I argued in Chapter Four, in order to survive and attract participants any new program of explanation or research must satisfy some of the standards/values characterizing the scientific community within which it is proposed. The gynecentric woman-the-gatherer model in human evolution studies is an example of a model that both expresses an alternative social vision and meets the standards set by the field in which it is proposed. In particular, only frameworks that make possible ordered interactions with a particular scientific subject matter will ever get serious attention.

These remarks about feminist science hold, *mutatis mutandem*, for oppositional or radical science generally. Social political values and interests are encoded differently in different fields and hence engage different oppositional commitments. In some areas, such as the complex of research programs having a bearing on the understanding of human behavior, certain moves, such as the one described above, seem quite obvious. In others it may not be clear how to express an alternate set of values in inquiry, or what values would be appropriate, nor even what the political dimensions of a field are. The first step, however, is to abandon the idea that scrutiny of the data yields a seamless web of knowledge. The second is to think through a particular field and try to understand just what its unstated and fundamental assumptions are and how they influence the course of inquiry. Knowing something of the history of a field is necessary to this process, as is continued conversation with other feminists.

The feminist interventions I imagine will be local, that is, specific to

[7] See Laudan (1985) for a gradualist model of scientific change.

a particular area of research; they may not be exclusive, that is, different feminist perspectives may be represented in theorizing; and they will be in some way continuous with existing scientific work. The accretion of such interventions, of science done by feminists as feminists and by members of other disenfranchised groups, has the potential, nevertheless, ultimately to transform the character of scientific discourse.

Knowledge and Politics

A number of social theorists, as well as science scholars, have developed views about scientific knowledge that bear on the questions discussed here. These ideas have been alluded to or directly discussed in earlier chapters. In this section I will briefly discuss those views and examine how they converge with or diverge from the approach developed here. What will emerge most clearly from these discussions is (1) the tension between epistemological analysis and metaphysical views (in which I include certain social and political principles) and (2) the difficulty of simultaneously critiquing existing or establishment science and pointing towards a new liberatory science. Epistemology cannot be made the basis of a new world view, it can only open the way out of our current ones.

Neo-Marxism

Engels once took pains to argue for a scientific socialism. Marxism provided us, according to Engels, with a scientific analysis of economics and history. Engels and Marx presupposed a certain conception of science, and this sort of argument is not frequently heard these days. Science—as a real historical phenomenon—has become as problematic for Western Marxists as the disciplines of philosophy, economics, and history once were for Marx. A number of radical scientists have in fact mounted critiques of contemporary science that draw their inspiration from Marxist analysis.

These new or neo-Marxist accounts of scientific knowledge are characterized by three main themes. One is that the dystopic applications of modern science—the domination of political life by thermonuclear weapons, new particle beam weapons, and other monsters of annihilation; the control of human potentiality through genetic engineering; the proliferation of toxic wastes from science-based technologies; the displacement of human labor by automation—are not a misuse of socially neutral science but the inevitable result of bourgeois science. A second theme is the rejection of reductionism, which radical

scientists take to be characteristic of bourgeois science and partly to blame for the inappropriate technologies. Reductionism reflects the bourgeois interest in centralized control. And a third theme is that a more adequate, even emancipatory, science is possible and that adoption of its methodology will reveal the truths of nature. Thus, contemporary, mainstream science is both morally and socially noxious and a misrepresentation of natural relations and processes.

Hilary Rose and Steven Rose have produced a number of anthologies of radical scientific writing as well as essays of their own calling for an emancipatory science.[8] Such a science, in their view, will have overcome the split between the object and the subject and the rational and the emotional and it will no longer be dominated by instrumental rationality. It will be characterized by democratic social relations, that is, the abandonment of elitism, and its theories will incorporate a dialectical view of nature.[9] Hilary Rose has used women's health clinics as examples of what an emancipatory science and its practice might look like.[10] In these settings work tends to be organized in a collective, egalitarian manner, and knowledge is produced through blending objective and subjective. A woman's own experience of her body is an integral element of the medical knowledge developed in these settings, and the capacity of staff to identify with, rather than detach from, their clients is essential to achieving such integration. Rose and Rose believe that scientific knowledge is progressive, that hypotheses can be confirmed or disconfirmed or, as they put it, can be more or less "in accord with the world's materiality." While they clearly believe that dialectical science will provide a more accurate picture of natural processes, they do not indicate how we might know this to be the case, or why we we should believe it. They seem, that is, to be urging the potential empirical superiority of the dialectical approach but do not offer empirical grounds for thinking it so.

Richard Levins and Richard Lewontin in their book *The Dialectical Biologist* also call for a science that incorporates a dialectical view of nature.[11] Their conception of dialectical science is both displayed in exemplars and explicitly outlined. Their dialectical world view is characterized by two basic ideas: (1) things are internally heterogeneous, so there is no least unit of analysis and (2) the correct division or decomposition of wholes into parts varies depending on what aspect of the whole is being investigated. Levins and Lewontin argue further that

[8] Rose and Rose, eds. (1976, 1979); Dialectics of Biology Group (1982).
[9] Rose and Rose (1982), pp. 50–59.
[10] Rose (1983).
[11] Levins and Lewontin (1985).

the internal heterogeneity of things means that change must be explained in terms of opposing processes united in an object. Evolutionary theory provides one illustration of dialectics in nature. Levins and Lewontin argue that traditional, reductive science sees organisms as produced by selective forces beyond their control—the environment. They propose instead that organisms both make and are made by their environments. Here the whole is a particular ecosystem. Their dialectical approach demands that no element be given sole causal efficacy. Rather these elements—organisms and their environments—are much more complexly related such that each acts upon the other. The changes in the ecosystem of which they are parts depend upon their interactions. The sense in which it is appropriate to speak of opposing processes here is the sense in which the direction of causal influence is reciprocal rather than unidirectional.

Both of the approaches discussed have in common an unargued, bedrock commitment to a nonreductionist, dialectical view of nature. Not only is this view morally and politically preferable to mainstream and reductionist science but it is truer to nature. Since, however, the scientific methodology acceptable to these analysts is one that presupposes the truth of a dialectical world view, it is not clear that there is any way of finding out whether a reductionist or dialectical science really is truer to nature.

The spirit, therefore, of these analyses might be better served by seeing them as urging a reconception of objects of inquiry in particular fields—specifically as urging their colleagues to abandon questions presupposing unidirectional or linear causal relations and to understand objects as constituted partly of the parts of which they are wholes and partly of the wholes of which they are parts. If this shift could be accomplished on internalist grounds, there would be less struggle over its acceptance. I argued in Chapter Four that proponents of alternative theories must appeal to some standards held by the scientific community that they wish to persuade but that these standards are heterogeneous and can provide grounds for resistance to change as well as grounds for change. Internalist grounds would be drawn from these standards but would not be sufficient to decide the issue, or force the shift. This is particularly so if, for some, the standards include a commitment to a reductionist and linear mode of analysis; that is, if doing science just consists in the analysis of objects conceived atomistically and mechanistically. The relation of a dialectical method to a dialectical world view, then, is that adopting a dialectical world view requires adopting a dialectical method of inquiry. The latter will reveal the complexity of nature understood dialactically. But so will a reductionist methodology reveal the ultimate simplicity of nature conceived atomistically.[12]

According to this analysis, the neo-Marxists are understood as advocating an alternative vision of nature and natural processes largely on moral and sociopolitical grounds. Certain consequences of scientific inquiry on the mainstream model have been (and promise to continue to be) so horrific as to require an alternative. In this regard the neo-Marxists stand on the same ground as the feminist scientist. In order to practice science as a feminist, as a radical, or as a Marxist one must deliberately adopt a framework expressive of that political commitment. This does not mean ceasing to do science but doing science that reveals different relationships. The advocacy of the dialectical method is a call to those who espouse the political commitments of these authors: if you share our political beliefs, here is a way to do science that expresses those beliefs.

Jürgen Habermas

Political and social theorist Jürgen Habermas discusses scientific knowledge explicitly in his early work *Knowledge and Human Interests* and implicitly in his more recent work on communicative pragmatics, particularly in his account of truth.[13] In *Knowledge and Human Interests*, which presents in a formal way his thinking of the early and mid 1960s and which was published in Germany (as *Erkenntis und Interesse*) in 1968, Habermas was concerned with rolling back the claims of positivism. He rejected positivistic scientism on both external and internal grounds. From an external point of view positivism is judged mistaken in its extension of empiricism to the knowledge of persons and of social institutions whose understanding requires interpretation of meaning.[14] From an internal perspective positivism fails its own tests of meaningfulness.[15] Habermas's alternative proposal is that knowledge is constituted by fundamental cognitive human interests and that different kinds of knowledge are constituted by different kinds of interest. These interests are revealed by or discovered in what Habermas calls frames of reference, the ultimate frameworks of justification for knowledge claims. In the appendix to *Knowledge and Hu-*

[12] See Taylor (1986) for additional discussion of Levins' and Lewontin's dialectics.

[13] Habermas (1971). My reading of Habermas has benefited greatly from reading McCarthy (1978); Geuss (1981); and "Habermas and Postmodernism" in Jay (1988).

[14] Habermas (1971), pp. 301–308.

[15] Ibid., pp. 71–90.

man Interests the three types of knowledge-constitutive interests are identified as (1) technical, (2) practical, and (3) emancipatory.[16]

In particular, Habermas states that the "empirical-analytic sciences" (presumably mathematics and the natural sciences) are constituted by a technical cognitive interest, that is, an interest in "technical control over objectified processes." An ultimate ground of justification for any mathematical or natural science theory is that it promotes such control. In saying that such a frame of reference or interest establishes rules for the construction of theories and for their testing, Habermas's claims parallel my thesis that certain contextual values are integrated into inquiry as constitutive values.[17] Our examples are similar also—namely the interest in establishing control over and/or predictability of natural phenomena. Habermas seems, however, to suggest, at least in *Knowledge and Human Interests*, that this interest in combination with experience will generate acceptable theories and also that the preeminence of this interest is not itself historically constituted but constitutes some sort of transcendental ground of knowledge.[18] I have argued that inquiry is subordinated to human needs in the sense that the sorts of things we need to know about some aspect of phenomena/experience are projected onto the phenomena through the questions constituting our inquiry. The specific forms of these needs develop historically, as do our means of satisfying them. Thus, in the sixteenth and seventeenth centuries the need of Europeans to gain greater control over certain material processes gave rise to the mechanical philosophy, which characterized matter in a particular way. This characterization both permitted certain understandings of the mechanical workings of things to develop and gave moral license to certain heretofore forbidden interactions with the natural world. Habermas' knowledge-constitutive interests, by contrast, are described as transcendental limits that make objectivity (in the natural sciences) possible. If they are subject to historical change, Habermas does not explain how. Nor does he explain what the relation might be between the abstract cognitive interest in technical control or "feedback-monitored action" and particular interpretations of that interest.

Another point of partial convergence concerns the social character of knowledge. In *Knowledge and Human Interests* Habermas claims that the intersubjectivity of a community of investigators is the ground of clarification of metatheoretical problems of the natural sciences.[19]

[16] Ibid., p. 308.
[17] Ibid., p. 309.
[18] Ibid., pp. 307, 311.
[19] Ibid., p. 149.

Such intersubjectivity is possible only through interpretive knowledge, which is distinct from instrumental, technical knowledge, and incorporates a practical interest, that is, an interest in mutual understanding that can ground social action. I have argued, by contrast, for the necessary engagement of intersubjectivity and a multivocal community of scientists in the resolution of theoretical disputes and not just of metatheoretical ones. Habermas's purpose is to show that empirical knowledge (which is required for human, "purposive-rational," action) cannot ground itself and that there must, therefore, be forms of knowledge, defined by correlative cognitive interests, other than the technical-empirical. The ground of purposive action—that is, the knowledge-constitutive interest in technical control—is incomplete, and, argues Habermas, this incompleteness points to the interpretive-hermeneutic knowledge required for understanding others. It is not clear, however, whether this incompleteness as a ground of knowledge is a descriptive incompleteness, that is, a failure to encompass all forms of possible human knowledge, or an analytical incompleteness, that is, a failure to account fully for the instrumental knowledge required for purposive human action.

This ambiguity is not inconsequential, for it prevents us from seeing clearly what the Habermasian account would have us say about the two conflicting approaches to the biology of human behavior discussed in earlier chapters. Would Habermas argue that the hormonal account is an inappropriate (scientistic) attempt to extend the methods of the empirical sciences to an area belonging to the historical-hermeneutical sciences? That it is an instance of the illusion of the completeness of the interest in technical control as a ground of knowledge? Surely if it can be used to predict successfully (within an acceptable margin of error), it cannot be said to be illusory. Would he argue instead that the two approaches are both empirical accounts whose conflict generates a metatheoretical problem? According to this approach, the biology of human behavior remains within the framework of the empirical sciences, but the ground of these sciences, the interest in technical control, is inadequate to decide between competing models. Habermas does not explain how such decisions might be made. Presumably they and similar problems constitute the metatheoretical issues that the community of investigators (scientists) must decide through dialogue. Since one of the questions at issue is the type of control afforded by the two models, it would seem that the knowledge-constitutive interest in technical control is not a sufficient ground but must be given an interpretation in/by a given scientific context. Thus, the extent to which it can

constitute what Habermas calls a transcendent ground of knowledge is unclear.

Another way to see this problem is as a challenge to Habermas's view that the domains of knowledge marked out by the several knowledge-constitutive, or cognitive, interests are independent. One could argue that the very possibility of hermeneutic understanding requires that one rather than another biological model be adopted, that is, the one within which it makes sense to ascribe intention and effective subjectivity to oneself and other human beings. This line of argument clearly dissolves the boundaries between technical and practical knowledge. The incursion, however, is not the familiar scientistic appropriation of the practical domain of human interaction but the reverse.

These neo-Kantian concerns with categorization and transcendence have given way in Habermas's later work to a more linguistically based investigation of human interaction.[20] The linguistic approach upon which Habermas builds is the pragmatic philosophy of language of John Austin and John Searle. This later work seems an elaboration of the claim in the earlier work that the intersubjectivity (dialogue) of a community of investigators is the necessary completion of the ground of empirical knowledge. His theory of communicative action includes a theory of truth that is similar to the account of objectivity developed in Chapter Four. This discussion of Habermas's theory is an opportunity, therefore, to reinforce the distinction between objectivity and truth. For Habermas those statements are true to which all participants in an ideal speech situation would agree. An ideal speech situation is one of completely free and uncoerced communication in which all have an equal chance to participate and equal power to impose their views (that is, the power of rational persuasion alone). A number of objections to this analysis of truth are well-known, and I shall mention only the most relevant for my purposes.

Devices similar to the ideal speech situation have been used by other contemporary political philosophers to describe the position from which one's interests as a human being rather than as a member of a particular social class could be identified.[21] However well such devices might work for the identification of real or legitimate interests, that is, for self-ascriptions of some kind, their relevance to other-ascriptions, which scientific assertions of necessity are, is not clear. What guarantees are there that the statements we would agree on in the ideal speech

[20] Habermas (1970a and 1970b).

[21] Rawls (1971) and Dworkin (1977) both use such devices.

situation would be accurate representations of reality? As Mary Hesse has argued, one needs some independent way to discover that those statements about which consensus is reached are indeed true before one can use consensus in an ideal speech situation as a criterion of truth.[22] Indeed one would also need some accounting of the connection between such consensus and truth before relying on consensus in those cases where the independent methods of verification are not in operation.

Matters are not improved by taking consensus to be a definition rather than a criterion of truth. What are we to say about changes in consensus over time? Has the truth of statements changed correspondingly? Just as problematic for a criterion or a definition is the possibility that there be no consensus regarding a particular question. Why should we suppose that we would all eventually come to agree that a particular theory is correct? And does it follow from a lack of consensus that there is no truth of a matter?

Both Habermas's truth and the scientific objectivity discussed above are socially achieved. Objectivity in the social account is, however, independent of truth. It refers not to the representational character of a thesis but to the conditions in which such a thesis becomes accepted. It may be accepted as true, but it does not follow, even in an ideal speech situation, that it really is true. If one understands Habermas not as offering a definition or criterion of truth but as analyzing the conditions under which consensus can be used as a criterion of truth, then it would be a necessary condition that consensus be achieved (or achievable) under the conditions of an ideal speech situation. What is not clear is that it could ever be a sufficient condition.

Habermas's conception of scientific inquiry is still, as Thomas McCarthy has noted, rooted in positivist conceptions of science.[23] Theories in natural science are described as collections of empirical generalizations rather than as attempts to use descriptions of one sort of process to explain other sorts. Habermas accepts a positivist account of natural science and is concerned only to restrict it to natural science. Because the Peircean embellishments, such as emphasis on the communitarian nature of inquiry, are attempts to resolve some of the dilemmas produced by positivism's overextension of empirical methods, they are not developed in a way that responds to epistemological problems arising within the sciences. The general rules of rational speech and inference that could operate in an ideal speech situation are not

[22] Hesse (1980), pp. 206–231.

[23] McCarthy (1978), pp. 60–68.

powerful enough to arbitrate between competing theories and their metaphysical-methodological contexts. Methodologies and rules of inference that permit the articulation of particular theories in relation to bodies of experience and data are specific to particular sociohistorical contexts and cannot be abstracted from those contexts. The alternative, oppositional frameworks discussed in the section on feminist science cannot be accommodated within Habermas's theory of natural science. In trying to clear a space for an autonomous social and critical theory, he has ceded nature to the positivists.

Michel Foucault

On the surface Foucault's writings on science seem not to address at all the concerns of this volume or traditional philosophical questions about the nature of scientific knowledge and understanding generally. His reluctance to theorize "in the grand style" means that we can find few general claims to hold on to or display as Foucault's theory. The implications of his work for the philosophical questions are considerable, however, if not transparently clear. I shall focus on the elimination of the knowing subject and the interconnections of power and knowledge.[24]

The Archeology of Knowledge is a theoretical volume that systematizes the analytic strategies deployed in *The Order of Things*.[25] Foucault develops a formidable armory of technical terms used to describe the emergence of a field of knowledge or scientific discipline. Several themes recur in the stages of his presentation and persist as well in his later work. Foucault is concerned with discourse, that is, with the characteristic ways of talking and writing about a subject matter. He attends to the rules of formation of a discourse, the coemergence of a discourse and its object, and the network of social, political, and economic relations within and upon which the discourse takes shape. This focus on ways of talking about something resembles the linguistic turn in twentieth-century analytic philosophy. It displaces the knowing (or believing) subject from the center of philosophical attention that it occupied from Descartes to Kant and makes questions about how an individual or group of individuals comes to believe or justifies a belief irrelevant. Just as beliefs or statements have no independent existence but are meaningful only in the context of a discursive practice, so the individual must be located in some network of authority and her/his

[24] This discussion of Foucault's ideas has benefited from my reading of Dreyfus and Rabinow (1983) and Sheridan (1980).

[25] Foucault (1970, 1972).

position in the network known in order that sense be made of her/his utterance.

Some discourses or "discursive formations" give rise to a science. This occurs when (informal) norms of verification are transformed into formal criteria. This way of seeing the emergence of a science of something requires that we abandon the idea that there is a unique one-to-one correspondence between descriptive sentences and the facts. Instead science, or scientificity, is to be seen as one stage of development of discursive formations: a stage in which norms of verification and coherence are incorporated into a fixed set of formal criteria. The discourse acquires a regimentation and organization such that only those statements that have been subjected to the rules are admissible as (1) worthy of discussion and (2) true or false. These rules, of course, are dependent on their historical context. For Foucault, therefore, the attainment of scientific status is not the shedding of ideology but the enshrinement of ideology in a science. Ideology enters a discursive formation in the rules of formation and norms of verification. Far from an accidental feature of a science, it is at the heart of a science.

The process Foucault describes here involves or can involve two processes discussed above. One is the inclusion of metaphysical—social, economic, political—assumptions among the background assumptions directing reasoning in a field, as the linear-hormonal model facilitating inferences about the causes of behavior incorporates assumptions embedded in what Foucault would call the discursive practices of the field of behavioral neuroendocrinology. The displacement from the center of analytic attention of the individual who knows or believes and its replacement with a focus on the discursive practices through which knowledge is created draws attention to the social character of knowledge production and to the residence of knowledge not in individuals but in their interactions and in the products of those interactions. The second process is the convergence of contextual and constitutive values in the constitution of a field of inquiry, as the social and political needs that require knowledge for their satisfaction direct the kind of knowledge sought and hence specify acceptable forms of solution to cognitive problems.

Our accounts differ in other respects, however. For example, Foucault in his later writings stressed the interaction of power and truth, suggesting, perhaps inadvertently, that inquiry is driven in a singular way by the requirements of power relations.[26] I say "inadvertently" because it may be Foucault's rhetoric and readers that suggest a

[26] Especially Foucault (1977, 1978, and 1980, pp. 78–133).

monolithic science rather than Foucault himself. Nevertheless, when Foucault states that knowledge is essential to power and that the direction in which power or ("biopower") pushes is toward totalizing normalization, he seems to suggest that knowledge and power produce each other in a univocal way.

Is there room in this scheme for other forms of knowledge? I have argued that oppositional science can be pursued at least to a limited extent and that there are areas of inquiry that may remain neutral with respect to power (in some of its forms). While the behavioral neuroendocrinology discussed in earlier chapters is clearly parallel to and coordinated with the bureaucratic drives towards the normalization of certain types, some neurophysiology is neutral with respect to what Foucault calls biopower and can be used in the creation of oppositional sciences. What remains problematic is the extent to which knowledge can successfully oppose or resist cooptive absorption by power. This question may be political and sociological rather than philosophical, however, depending on the possibility of contrary forms of power or, to use alternate terminology, depending on the extent to which alternative needs or interests can provide an organizing focus for alternative or oppositional knowledge. Foucault's forms of resistance seem mute rather than articulate, generated as they are in brute reaction to the physical experience of power. Thus the development of oppositional knowledge in Foucault's view may simply be a realignment of power and not an escape from the drive towards totalizing normalization.

Evelyn Fox Keller

Evelyn Keller has used object relations theory to explain how the natural sciences are permeated by an ideology of domination. Whereas Habermas sees the interest in technical mastery of an object theorized as other as constitutive of the empirical sciences, Keller suggests that it is a deformation of cognitive aims. In her view conceptions of what counts as scientific knowledge are informed by a (mis)understanding of objectivity that has its roots in infantile experience, which is itself shaped by social norms and structures.

Keller develops her argument in *Reflections on Gender and Science* on several different levels, which coexist in some tension with each other.[27] She is, first of all, making a claim that, historically, scientific inquiry is characterized by a plurality of contesting theories as well as contrasting visions of what inquiry should be. In spite of this plural-

[27] Keller (1985).

ism, however, one image of science and one kind of theory have tended to predominate, at least in the modern period. In the prevailing view the aim of science is to achieve objective knowledge, where the criteria of objectivity are emotional distance from and potential control of the object of knowledge. The search for objectivity is thus misidentified, in Keller's view, with the search for control over natural phenomena. As a consequence those theories are accepted which analyze their subject matter as objects or systems of control. Models of self-organization and strong forms of interaction may be proposed, but reductive and mechanistic explanations are consistently privileged by the scientific establishment.

A second strand of her argument has to do with the explanation for this state of affairs. Keller appeals to two convergent phenomena: the metaphoric genderization of scientific inquiry in the seventeenth century and the processes of individual psychological development. The seventeenth century saw a fierce struggle between two approaches to scientific inquiry, some aspects of which were described in Chapter Five. The conception of inquiry that ultimately triumphed was one that, among other things, envisioned the seeker after knowledge as male and the object of knowledge (nature) as female, and which described the activity of inquiry in language used to describe the male pursuit of females: rape and courtship. Keller sees the union (or collapse) of cognitive and affective models as having its origins in the Platonic identification of goodness and truth and hence of the highest form of love with the highest form of knowledge. The seventeenth-century adaptations of this identification appeal to *eros* rather than *phile* and reflect changing conceptions of gender as well. The successful version is a heterosexual fantasy of control and submission that makes science a properly masculine endeavor and made both women and nature appropriate objects of domination.

Because knowledge has thus been given an affective dimension, psychological accounts of emotional development can provide an insight into the absorption of social norms by the emotive structure of individual psyches and their consequent projection into nature as structures of knowledge. Feminist object relations theory provides the analytic tool Keller needs to demonstrate this relationship. Keller distinguishes two sorts of autonomy and, correlatively, of objectivity. Static autonomy is the condition of a self created in opposition to another (in particular, the mother). It is characterized by constant anxiety over the maintenance of the self's boundaries, an anxiety relieved by attempting to control all others who threaten those boundaries. Dynamic autonomy, on the other hand, is the condition of a self created through dif-

ferentiation from but also relatedness with others. It is characterized by tolerance for ambiguous boundaries and a sense of agency exercised in a context of interacting agents.

These psychological orientations to the world produce corresponding cognitive ideals, expressed as interpretations of objectivity. Objectivity in general is defined by Keller as "the pursuit of a maximally authentic, and hence maximally reliable, understanding of the world around oneself."[28] Static objectivity is a search for knowledge that radically severs subject from object, just as the statically autonomous self is rigidly delineated from others. It is nonreflexive and keeps the self outside the realm of inquiry. Dynamic objectivity, by contrast, "aims at a form of knowledge that grants to the world around us its independent integrity but does so in a way that remains cognizant of, indeed, relies on, our connectivity with that world."[29] The psychological development of male children in this society in general tends to produce individuals characterized by static autonomy, who need to dominate others in order to maintain their own sense of self and identity and whose pursuit of knowledge is the attempt to understand a world of objects radically separate from and different from the self.[30] In Keller's language their pursuit of knowledge is an attempt to understand the natural world through a framework that relieves the (neurotic) anxiety about boundaries. The metaphoric identification of scientific inquiry with men's sexual relations to women, which makes both women and nature objects of domination, facilitates the recruitment into science of those whose ideal of knowledge is characterized by static rather than dynamic objectivity. Thus, scientific inquiry and the theories it produces are permeated by the ideology of domination: the relations among objects of study are typically described in the language of control and domination, and objects of study are characterized in such a way as to make of them proper targets of domination.

Keller's mode of explanation explains how it is that the sciences are, for the most part, directed towards the goal of controlling nature. It is because the persons who become scientists are psychologically ori-

[28] Ibid., p. 116.

[29] Keller (1985), p. 117.

[30] Keller's analysis here inherits a disabling universalism from object relations theory. The family structure within which this psychological profile develops is characteristic only of middle-class families in industrialized societies (and no longer many of them). If these phenomena are restricted to the middle class, then one cannot explain the social predominance of the cognitive ideals without a class-based or class sensitive analysis. If the cognitive ideals are not a class-restricted phenomenon, then the appeal to middle-class family structures is off the mark.

ented to such a goal.[31] But this is not the only point that Keller wishes to establish. She has herself a vision of what scientific inquiry could be. At this third, constructive level of her argument, however, her position becomes less clear. In the third section of *Reflections on Gender and Science* Keller seems to argue for two different, and not necessarily consistent, visions. One argument is a plea for pluralism, expressed most clearly in the Epilogue: "A healthy science is one that allows for the productive survival of diverse conceptions of mind and nature."[32] The contrary voice is the one heard in the chapter on dynamic objectivity and in the introduction to Part III. This voice urges a particular view of nature. Nature has its own integrity, is "orderly in its complexity, rather than lawful in its simplicity."[33] Interactionism, rather than control, is the watchword here. This philosophy of nature is vindicated in two of the case studies in Part III in which reductivist "master molecule" or "pacemaker" models are argued to be inferior to more complex and interactionist alternatives. The view of nature underlying these alternatives is made possible by dynamic rather than static objectivity. Dynamic objectivity provides more adequate and reliable representations of nature than are possible through static objectiviity.

There are two puzzles here. One is raised by the potential contradiction between the call for pluralism and the endorsement of interactionism. Keller's antipathy to relativism leads her to claim that the form of objectivity she endorses produces more reliable understandings of nature. Her respect for the achievements of modern science leads her to plead not for the replacement of current theories by these more reliable ones but for tolerance of diversity in the sciences. This apparent inconsistency could be resolved by saying that dynamic objectivity produces not one but several theoretical perspectives, that is, that there could be many interactionisms. While preserving a limited pluralism, however, this solution excludes mechanistic and reductionist theorizing, an exclusion that seems contrary to the spirit of pluralism and to the gradualism she endorses.

A second puzzle is related to her claims for interactionism and dynamic objectivity. How do we know that interactionist models are, other things equal, more reliable than reductivist or mechanical ones? Keller has given us reasons to think that dynamic objectivity is the cog-

[31] This mode of explanation seems to me, nevertheless, incomplete. In Chapter Five I mentioned research suggesting other factors—economic and political—that direct scientific inquiry towards control and mastery of natural processes. See also Longino (1988).

[32] Keller (1985), p. 178.

[33] Ibid., p. 136.

nitive goal of an emotionally undistorted and mature personal orientation to the world. What still requires explaining is the relation of that attitude to truth or reliability. Keller presents dynamic autonomy as an attitude that generates theories and models with certain characteristics. As she herself notes about Barbara McClintock's models of transposition and gene action, reductionistically inclined biologists claim that, in the end, the self-organizing and strongly interactive aspects of McClintock's models can be eliminated or understood in reductionist terms. These aspects of the theory, then, are neither self-evident nor generally viewed as compellingly supported by the data. The decisive reason for accepting them is their consistency with a particular philosophy of nature.

It is at this stage of the argument that Keller's position is most problematic. She has not yet provided the materials to show the inherent superiority of this philosophical approach over competing ones. If we look to *Reflections on Gender and Science*, the best support we could find for this philosophy of nature is that it is the cognitive outcome of a healthy psychological orientation to the world. Keller would undoubtedly reject this as a ground. For what justifies the claim that this orientation (that is, that of dynamic autonomy) is healthy? The answer that it is productive of less neurosis, or psychological pain, is vulnerable to the question raised above, namely what the relationship is between less psychological pain and truth. The answer that it is more conformable to reality begs the question.

One way to escape this dilemma is to detach psychological from epistemological and metaphysical virtues and to portray the interactionist philosophy of nature as an explicit component that is subject to rational criticism and evaluation. This procedure would treat interactionism generally (or, preferably, domain-specific articulations of interactionism or other nonreductionist views) as background assumptions establishing the relationship between certain sorts of observable or experimental phenomena and models and theories of natural processes. As I have argued, arguments about background assumptions are not definitive, and changes in prevailing background assumptions are brought about by changes in the goals of the sciences.

One of the great merits of Keller's discussion is to show how more general philosophical positions are expressed in particular scientific research programs. Her urge to comprehensiveness, however, prevents her from distinguishing between what one might say as a student of the sciences and what one might say as a scientist or philosopher of nature. In the first capacity one might argue for pluralism as the most reasonable position from an epistemological point of view. In the second capacity one must work within some framework, and a commitment to pluralism can only undermine one's implicit claim to correctness. A particular view put forward among others in a pluralistic context must be defended by arguments other than those supporting the context itself.

Donna Haraway

Historian of science Donna Haraway has been engaged in a very detailed study of twentieth-century primatology.[34] She situates her work in what she describes as a global oppositional movement to retell the Western origin stories that rationalize European economic and cultural imperialism. Primatology is the focus of her analysis because "the scientific practices and discourses of modern primatology participate in the preeminent political act in western [*sic*] history: the construction of Man."[35] As do other ideas belonging to political discourse, like freedom and equality, our ideas about what is just depend critically on our assumptions about human nature. The study of apes and monkeys, like the biology of behavior, is one attempt to define what is natural in that nature. Haraway establishes this thesis in two ways. One is to show the direct connection between the research programs of leaders in the field such as Robert Yerkes in the 1920s and Sherwood Washburn in the 1960s and 1970s and explicitly political agendas. Conference sponsorship, funding, and correspondence are all examined to demonstrate this connection. The other is through an analysis of primatological discourse. The texts of primatology reveal a recurrent obsession with otherness, boundaries, and origins and continually cast their subject matter in the language and categories of political economy. This second internal line of argument intersects more closely with the philosophical questions of knowledge and understanding, and in the following discussion I shall attend more closely to it than to the external argument.

Haraway's dissections of primatology are dense essays in politics and science that use the analytic techniques of deconstruction and narrative theory to support a reading of science as politics and science as culture. I will attempt to outline the view I read in this work in order to establish the points of connection with the philosophical issues discussed above.

Haraway, first of all, sees the sciences not only as a legitimator of

[34] This discussion draws on Haraway (1978, 1979, 1981–1982, 1985a, 1985b, 1988).

[35] Haraway (1985b), p. 489.

domination but as a resource for those who resist. The sciences, therefore, are neither univocal nor simplistic impositions of cultural mythology upon the natural world. Primatology in the twentieth century has in fact become increasingly sophisticated methodologically. Distinctions between the validity of observations made in the wild compared to those made of animals in captivity are only the beginning. A number of field workers, among them many women such as Jeanne Altmann, pioneered the development of protocols for the nontendentious (less anthropomorphic) description of primate behavior. Haraway presents primatology as governed by methodological (constitutive) rules that determine what counts as good science in primate studies—rules, however, that are negotiated and renegotiated in the actual practice of this field.

Good science is, however, not equivalent to objectivity. However rule-governed and technical the basic data gathering may be, the field's discourse is nevertheless characterized by metaphoric systems and what Haraway calls "core narratives" that both direct observations and serve to interpret them. The individual primate and the primate troop are constituted as objects of knowledge by these metaphoric systems, which mediate researchers' interactions with their objects of study. The effect of this metaphoric constitution is that, even though the apes and monkeys are the same organisms, as the conceptual system changes, so does the object of knowledge. What the researchers have "really" been investigating are systems of production characterized by internally generated principles of control. They use the data of primate behavior to work out how a natural system such as an individual organism or population of such organisms instantiates such principles. It is for this reason that it would be inappropriate to speak of a metaphor *simpliciter*. For the concepts from one domain are not simply transferred to the subject matter of another in order to explain the latter. Rather, the object of inquiry remains the first domain, whose scope has been extended to include the second.

Haraway's most dramatic demonstration of her claim about the "true" subject matter of primatology involves showing how the conceptual-metaphoric system of the discipline has changed. Organisms in the nineteenth and early twentieth century were described as systems of production and reproduction whose chief organizing principle was a hierarchical division of labor. Thus the concern with dominance in primate troops. The question is how dominance hierarchies are generated in a population or subpopulation—what sorts of interactions among the animals (aggression, competition) lead to their emergence. During and after World War II the study of organisms was partly absorbed by information science; organisms came to be studied as if they were information-generating and information-processing systems.[36] Organisms came to be described as systems of biotic components, and fields such as genetics and immunology, which are most concerned with coding, moved to the center of biology. Other fields, including, importantly, animal behavior, became recast in cybernetic language. The science is still about systems of production, but the kind of system has changed. The organism/population is no longer a factory but an information-processing system. The change, of course, has to do with changes in the political economy, in its transformation by electronic engineering into a massive electronic network. The major innovations in production, robotics, and genetic engineering are exercises of coding and decoding. As Haraway says in one of her articles, the artificial, counternatural character of the objects of knowledge in information science makes suspect the natural character of the familiar organismic categories they've replaced.[37] One is just as historically conditioned as the other. This is not to say that one or both are unnatural but that the natural is not an unconditioned given. Animal ethology never was about the animals directly, unmediated by conceptual or metaphoric structures that construct an object of knowledge, but about systems the animals were taken to instantiate.

Haraway is less concerned with the epistemological questions raised by her analysis, or with providing a method that could be applied elsewhere, than with decoding and deconstructing the specific texts of primatology—disclosing the ways in which they retell the story of male and white ("technologically advanced," "rational") development and superiority or, in some cases, tell a new story—of female activity and power, of cooperative societies conquered and coopted by aggressive ones, of resistance and sabotage, and so on. She herself describes her work as "about the social production of artifacts and meanings."[38] While her narratological analytic framework is superficially indifferent to the traditional questions of philosophers of science, Haraway's account of the subject matter of primatology should stimulate philosophers to new accounts of explanation and theories. In addition, the particular textual readings she offers suggest ways of analyzing the logic of those texts. It is possible, for instance, to bring the apparatus of theory-laden and "story-laden" observation and background assumption to bear on them to see with the help of Haraway's readings

[36] Haraway (1979 and 1981–1982).
[37] Haraway (1985a), pp. 80–81.
[38] Haraway (personal communication).

where description and where reasoning are shaped by the political, economic, and cultural contexts within which these primatological inquiries take place. The approaches are complementary rather than contradictory.

Whatever the potential harmony of analytic methods, in some of her early work Haraway has expressed disdain, if not outright contempt, for the rhetoric of truth and objectivity that is the mark of scientific texts.[39] And indeed given the degree of dissent and variability that characterizes animal behavioral fields such as primatology, a suspicion of such rhetoric is highly appropriate. It does leave those who would try to draw epistemological lessons from her analysis either in a quandary or in the quagmire of relativism.[40] The quandary results, of course, from attending to her claims about the value of the sciences. One presumes it is different from the value of literature, but the extraction of meaning from scientific texts by the methods of literary analysis obscures what distinctive value the activities that produce such texts might have. Haraway seems to recognize this in a recent essay.[41] Only objectivity-as-transcendence or what Haraway calls "the God trick" is rejected now. She urges in its place a self-aware partiality.[42] Knowledge is always knowledge in a situation, from a certain point of view. It is, therefore, both incomplete and perspectival. Objectivity is recognition of the local, mediated, situated, and partial character of one's knowledge.

This position is, on the face of it, ambiguous. If the recognition is the recognition by an individual, then the knowledge characterized as objective could still be an individual's knowledge, or beliefs. Haraway urges another reading. The recognition of partiality is a recognition of the "historical location of discourses, tools, and 'subjects.' "[43] Subjects come into being in social fields of meaning, and discourses are themselves social. Partiality in this sense involves the potential of connection with other discourses to generate other partial and mutable systems of understanding. I have argued that scientific knowledge is the result of complex processes of criticism, modification, and incorporation, that is, of the transformative interrogation of ingredients that are themselves socially produced, if also individually claimed. It is not the individual recognition of partiality or, as used to be said, of one's subjectivity but the subjection of hypotheses and theories to multivocal criticism that makes objectivity possible. While the first reading is at variance with the position for which I have argued, the second is more compatible with it: reflexivity is communitywide, and the openness of partial knowledge facilitates transformation.

[39] Haraway (1978), p. 59.

[40] Sandra Harding, for example, draws the following lesson from Haraway (1985a): "Haraway's argument would lead to an epistemology that justifies knowledge claims only insofar as they arise from enthusiastic violations of the founding taboos of Western humanism"; Harding (1986), p. 193. This seems to read Haraway as saying "anything goes, as long as it is in revolt against the dominant culture's founding principles."

[41] Haraway (1988).

[42] Ibid., p. 589.

[43] Haraway (personal communication).

Conclusion

All the thinkers discussed in this chapter agree that human interests have played a crucial role in the construction of scientific knowledge to date. They differ about the kinds of interests and their exact mode of operation, and even more starkly about the possibility of a new science and the epistemological merits of such a science. Both Keller, in at least one of her voices, and the Marxists write as though the problem is method. Thus, adopting the right method—whether it be dialectical as for Levins and Lewontin or interactionist and attentive to difference as for Keller—will enable us to develop a better (more true?) account of natural phenomena. Natural phenomena are held to possess an intrinsic order whose understanding requires a proper key. Foucault's and Haraway's concerns with the mutual constitution of knowledge and power make them much more skeptical both of the prospect of a right method and of a new, epistemologically superior science or set of sciences. The creation of local and partial discourses responsive to alternative and oppositional values may produce something like knowledge, but they would deny the possibility of knowledge detached from particular points of view. There is in fact the whiff of an accusation of bad faith in Haraway's critical discussion of replacement projects. To seek a unified, if different, knowledge of the natural and social worlds is to seek power of the sort we reject when exerted over ourselves. The goals driving the search for such knowledge are unworthy of an emancipatory politics.

While I think it right to be suspicious of such projects, their rejection leaves unanswered the question of how the human species will or can address the global problems that require informed action. The various degradations of the environment, from the destruction of the world's rain forests to the evaporation of the ozone layer, pose technical and political problems. So does the need for clean and renewable energy sources. These are not problems from whose consideration we can excuse ourselves on political grounds, for we will all be affected by what-

ever actions (including inaction) are taken. Thus, while Habermas's analysis of the natural sciences is the least satisfactory, his insistence that some form of the traditional notion of knowledge must be retained seems responsive to the very real problems of how we as a species are to survive into the next centuries. So, too, is the insistence of the feminist scientist on developing a biology of thought and action consonant with a democratic politics in which women are full participants.

In thinking about the possibility of a feminist science I drew on elements of the analysis provided in earlier chapters to conclude that one could practice science as a feminist by (1) recognizing the ways in which the background assumptions of mainstream science facilitated certain conclusions and excluded others and (2) deliberately using background assumptions appropriately at variance with those of mainstream science. This kind of feminist science, or more generally of oppositional science, is always local and respectful of some of the standards of a specific scientific community. And it requires a mainstream or established tradition to which it is opposed and with which it is in some form of dialectical tension. Can oppositional science be transformed into successor or new science? Only a change in the social relations of the context in which science is done can effect such a transformation. Thus, which among any set of oppositional or muted scientific voices becomes the new science depends on what social relations and associated cognitive needs characterize a changed context. While eschewing the concept of a single truth or the hope of a singular epistemological blessing, we can nevertheless rank theories as to their acceptability, in particular their worthiness as bases for collective action to solve common problems. That theory which is the product of the most inclusive scientific community is better, other things being equal, than that which is the product of the most exclusive. It is better not as measured against some independently accessible reality but better as measured against the cognitive needs of a genuinely democratic community. This suggests that the problem of developing a new science is the problem of creating a new social and political reality.

CHAPTER 10

Conclusion: Social Knowledge

THE VIEW of scientific knowledge and reasoning that I have developed and applied in this book turns out to be an empiricist one. It is, however, a modest, pared down empiricism, one that shuns metaphysical meaning postulates and restricts itself to epistemology: what we can know is what we can experience. While modest, it is nevertheless a powerful tool for the analysis of the truth claims scientific inquiry. In this final chapter I will develop the implications of the analyses of the previous chapters, raise some of the broader questions regarding science and values, and indicate some of the directions for further research.

CONTEXTUAL EMPIRICISM

Overview

I have set out to address several related questions. Is there an account of scientific reasoning and knowledge that enables us to make sense of scientific debates involving both ideology and evidence? Is there a place for other than epistemic values in science? Can we make philosophical sense of the idea of socially constructed knowledge? To what extent can scientific research be a neutral arbiter of disputes about human nature? In particular, what can the biological sciences tell us about gender? Of current views about the relation between science and social values, neither the approach that ascribes value-laden science to methodological inadequacies nor the approach that admits all values by denying the power or relevance of methodologies provides satisfactory answers to these questions.

In developing an alternative answer I focus on the cognitive practices in science, particularly on reasoning between data and hypotheses, that is, evidential reasoning. Treating reasoning as a practice reminds us that it is not a disembodied computation but takes place in a particular context and is evaluated with respect to particular goals. I argued that evidential reasoning is always context-dependent, that data are evidence for a hypothesis only in light of background assumptions that assert a connection between the sorts of thing or event the data are and