Towards a Female-friendly Philosophy of Science[1]

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1. The Relationship between Women and Science: Three Problems

When we think of the relationship between women and science, many of us think of a wholesome array of benefits bestowed on women by science: safer and less painful childbirth and lower infant mortality rates, labor-saving conveniences for the home like washing machines, frozen foods, and no-iron fabrics, the exposure and criticism of unhelpful superstitions and prejudices, and the like. For some time now, however, feminists have been pointing an accusing finger at science, urging that the relationship between women and science has been far from a beneficial one for women. Indeed, they have charged, science has generally excluded women from its most important activities, science has tended to leave women largely invisible in its knowledge and research, and science has often portrayed women, and things feminine, in negative terms when it *has* considered us.

Consider the matter of exclusion. In the past, we have been told, this exclusion took such obvious forms as denying women with scientific talents access to universities and other centers of scientific learning, denying them all but menial research roles with inadequate workspace and equipment and pay, and denying them membership in prestigious scientific academies and professional organizations (Schiebinger 1989; Rossiter 1982; Mozans 1991). More recently this exclusion has taken subtler forms: restrictive admissions quotas for undergraduate and graduate women students (or deliberate recruitment and selection by (masculine) gender), less financial assistance for women students, research positions with inferior workspace and equipment and pay, and with little authority or possibility of advancement, exclusion from the most important scientific meetings and collaborations and information networks, restricted access to prestigious scientific academies, and, of course, such newer phenomena as sexual harassment (Vetter 1987; Zuckerman, Cole, Bruer 1991; Dix 1987; Gornick 1990; Hornig, Hynes, Traweek, Keller, Turkle, Florman 1984; Weisstein 1977; Keller 1977). As a result, "women have been swelling the lower and middle ranks of science for years, yet still have not managed to pierce the upper scientific strata in anything beyond token numbers" (Angier 1991).

But exclusion of women is not the only charge feminists have leveled at science. A second charge is that science has tended to leave women largely invisible in its

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knowledge and research. For example, medical researchers have often failed to include females in animal studies in basic research as well as in clinical research unless the research centered on controlling the production of children. This has led, among other things, to drugs not adequately tested for women patients before being marketed and lack of information about the etiology of some diseases in women. Indeed, research on conditions specific to females (e.g., dysmenorrhea, incontinency in older women, and nutrition in post-menopausal women) has received low priority, and research on diseases (like heart disease) that affect both sexes has been primarily concerned with the predisposing factors for the disease in males (in this case, white, middle-aged, middle-class males), while very little research has been concerned with high risk groups of females (e.g., older women and poor black women who have had several children) (Rosser 1989).

In the social sciences abstract models based on male experience and male perception have been presupposed in the formulation of ongoing research projects. For example, the model of the rational actor in sociology has been "the abstracted model of organizational or bureaucratic man, whose motives, methods, and ego structure are organized by the formal rationality structuring his work role" (Smith 1979; see, as well, 1987). The model of human nature presupposed by contemporary mainstream political science has been that of a narrowly calculating masculine being "who adapts, conforms, and engages in self-interested behavior, rather than in action with a social as well as a private meaning" (Elshtain 1979). The model of the healthy, mature, socially competent adult in psychology has been that of a male adult rather than a female adult (Walker 1981). The problem is that social science "often assumes a 'single society' with respect to men and women, in which generalizations can be made about all participants, yet men and women may actually inhabit different social worlds" (Millman and Kanter 1987). It is only the men's world that social science takes to be the single social world, however. Thus, for example, in the conceptual schemes of sociology and economics all human activity is either work or leisure, a dichotomy that more accurately describes men's lives than women's. As a consequence, housework and volunteer work, which are not quite work (wage labor, part of the Gross National Product) and not quite leisure, cannot easily be conceptualized even though they form significant parts of women's experience. Nor can women's more concrete and caring modes of moral evaluation be easily captured within the Piaget-Kohlberg model of moral development in psychology, that was originally abstracted from male experience.

When science *has* considered women, on the other hand, it has often portrayed us in negative terms, and this is a third charge feminists have leveled against science. A favorite theme has been women's intellectual capacity. In the seventeenth century, for example, women's brains were said to be too "cold" and "soft" to sustain rigorous thought. In the late eighteenth century, the female cranial cavity was considered too small to hold a powerful brain. In the late nineteenth century, the exercise of women's brains was thought to shrivel our ovaries. In our own century, the way women process visuospatial information (supposedly by using the left hemisphere of the brain in addition to the right) supposedly makes women inferior in visuospatial skills (including mathematical skills) (Schiebinger 1989). But comparably negative stands have been taken with regard to many of women's other traits. Indeed, this situation is related to the problem of women's general invisibility, noted previously. That is to say, if men's bodily processes or social world or mode of psychological development, or whatnot, is the described state of affairs in science, the norm or standard, then to the extent that women's situation is different, it is no large step to conceiving of women as deviant, defective, or inferior. In this way women's moral and sexual and social development has been thought to be inferior because it does not fit the model of development applied to men.

2. A Role for Philosophy of Science?

These three charges—that science has generally excluded women from its most important activities, that science has tended to leave women largely invisible in its knowledge and research, and that science has often portrayed women, and things feminine, in negative terms when it *has* considered us—are not the only criticisms feminists have raised of science. But they are some of the most important and most extensively grounded of those criticisms, and they provide a strong indictment of science. Indeed, they suggest that science is a deeply unjust institution, and that many of its results and the methods used to obtain those results are inadequate as well. More than this, since science so profoundly shapes our attitudes and our world, they suggest that science is a prime perpetuator of the inequality women confront in society at large. And they auger more of the same inequality. For if women are among those most likely to make women visible and fairly portrayed in scientific knowledge and research, then the exclusion of women from the most important activities in science helps to perpetuate the invisibility and negative portrayals of women in science. But the invisibility and negative portrayals of women in science also help to perpetuate the exclusion of women from the most important activities in science. And just as these conditions in science help to perpetuate the inequality women confront in society at large, so too, the inequality women confront in society helps to perpetuate these conditions in science.

With so much at stake, the charges regarding women's exclusion from science, and treatment within scientific knowledge, deserve careful investigation and response. The question I would like to raise is, What role ought philosophy of science to play in this venture? Certainly, the history of science, whose task is to reconstruct science's past, has tended systematically to ignore significant parts of that past, including women's scientific contributions both within and outside organized science, the obstacles women scientists faced, and the treatment of women within the content and methods of science. And the sociology of scientific knowledge, whose task is to show that and to show how scientific knowledge is constitutively social right through to its technical core, has systematically ignored the *gender* component of the social. The history of science and the sociology of scientific knowledge have thus far, therefore, played significant roles in keeping invisible and intact women's exclusion from science and treatment within scientific knowledge. And by the same token, these fields can play a significant role in changing the situation for women in science, not only by informing us of the problems, but also by providing us with some of the conceptual resources we will need to solve them. Our understanding of science, however, is shaped at least as much by enquiries in the philosophy of science as by those in the history of science and the sociology of scientific knowledge. Has philosophy of science, also, helped to keep the problems of women in science invisible and intact, and can philosophy of science, also, offer anything useful for their solution?

3. The Prescriptive Program of Logical Empiricism

It is often said that the task of the philosophy of science, unlike that of the history of science and the sociology of scientific knowledge, is to characterize the "logic of science." In the heyday of logical empiricism that task was interpreted in a very narrow way: Characterizing the logic of science meant characterizing scientific research and the products of that research within the conceptualizations provided by formal logic and empiricist epistemology. Thus, logical empiricism aimed to show us what scientific explanations, empirical laws, theories, observation statements, the processes of hypothesis discovery and testing, and so forth, *were, in the sense of*, what logical and empirical conditions statements or inferences had to fulfill to *be* scientific explanations, empirical laws, theories, and the like. This was, needless to say, a very normative program: it

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served to describe what ideal science was, and what actual science was aspiring to be—and sometimes was, if it was truly science at its best. The program of logical empiricism, that is, served a prescriptive role to actual science, the foundation of which was formal logic and empiricist epistemology. What led to the downfall of logical empiricism—aside from the difficult logical and epistemological problems it confronted in carrying out its program—were the historical researches of Kuhn, Feyerabend, and others (see, for example, Kuhn 1962; Feyerabend 1962; and Toulmin 1961) showing that some of our greatest examples of science failed to satisfy the logical and empirical conditions of adequacy demanded by logical empiricism, and, in some cases, *were* some of our greatest examples of science *just because* they failed to satisfy logical empiricism's conditions of adequacy. Not only that, these historical researches of Kuhn, Feyerabend, and others seemed to suggest that science had no (one enduring) nature to be defined, at least no very specific nature like the one logical empiricism was seeking. Indeed, the nature of science's aims and methods, science's theories, explanations, evidence, and the like seemed to be different for different sciences, and for any one science at different times. What the history of science suggested, in short, was that logical empiricism had lost contact with actual science. In seeking to define science for all time, it was not describing *actual* science, even a truly great actual science, *at all*—which was, after all, the only science there was to describe. And, of course, in not describing actual science at all, logical empiricism neither informed us of the problems of actual science, including the problems concerning women, nor provided us with resources for solving them, for bringing us closer to the ideal science it described.

4. The Descriptive Program of the "New" Philosophy of Science

The prescriptive program of logical empiricism having failed in its task of characterizing the logic of science, most philosophers of science, influenced by Kuhn and others, shifted in the early '60s to a more straightforwardly descriptive program for philosophy of science. The program was now simply to report the actual processes and products of scientific research, past and present. Indeed, it was argued, how could philosophers of science presume to *prescribe* what science should be like when they had no very clear idea what science *was* like, when their "characterizations" did not in any way match the sophistication and diversity of actual science. Surely scientists are the appropriate authorities in matters of empirical knowledge, and philosophers should respectfully study what they do. The aim was, then, and still is, to *describe* general patterns of scientific change, clarify *actual* concepts, interpret *actual* theories, make epistemological sense of *actual* episodes in the history of science, all to understand the logic of science as an *actual* knowledge-producing enterprise.

At least this is what is *said* to be the aim of the descriptive program in the philosophy of science. When we consider the *results* of this program, however, the situation is not so clear. Consider Kuhn, who did more than anyone else to define the new program, both by the work he produced, and by the influence he exerted on others. Kuhn sought to provide a general descriptive account of science—its fundamental components and the process by which they develop, as well as the process by which they undergo fundamental change. When Kuhn proceeded to support/illustrate his general descriptive account of science, however, he referred exclusively to the work of scientists like Copernicus and Lavoisier, Newton and Einstein, those scientists generally regarded as the greatest of all time—all physical scientists, of course, and all men ... and white, and Western, and of upper/middle class origins. ("...We shall deal repeatedly with the major turning points in scientific development associated with the names of Copernicus, Newton, Lavoisier, and Einstein. More clearly than most other episodes in the history of at least the physical sciences, these display what all scientific revolutions are about" (1962, p. 6).) And Kuhn treated his descriptive account of

science as having normative impact as well, insinuating, for example, that the social sciences were still in their immature, pre-paradigm stages (see, for example, p. 15). Other philosophers of science seemed to follow suit. There was, of course, a barrage of critical discussion of Kuhn's general descriptive account of science (see, for example, Shapere 1966; and Lakatos and Musgrave 1970). Much of it concerned the nature and viability of Kuhn's fundamental concepts (like "paradigm" and "incommensurability") and of Kuhn's fundamental distinctions (between normal science and revolutionary science, for example). Much of it, again, concerned the nature and acceptability of the implications of Kuhn's general descriptive account of science (Was scientific change *irrational*, according to Kuhn? Is scientific knowledge really all *relative*?). But none of it seemed to question the strategy of first constructing a descriptive account of science to fit the work of the greatest (Western white male) physical scientists, and then normatively applying it to other cases of science. When philosophers raised the question of application at all, they tended to inquire whether Kuhn's account could helpfully be applied to cases of the greatest physical science (for example, What was the relation between Cartesian physics and Newtonian physics?). But they tended simply to *ignore* other cases of science.[2] Philosophers never seemed to *challenge* Kuhn's account by its failure to apply to these other cases of science. Lakatos seemed both to capture and to clarify what was going on when he explicitly set out to analyze the work of the greatest (Western white male physical) scientists with the aim of constructing a normative-descriptive account of science, an account that would at once *describe* this greatest science and *prescribe* to other science (see Lakatos 1976; see also, alas, Kourany 1982, which fit right into this trend!).

The descriptive program in the philosophy of science, pursued in just this way, could not have had any coherent justification, however—in fact, had to be in part a residue of logical empiricism's intense fascination with physical science and reductionism and the unity of science, and in part a manifestation of our continuing intense fascination with things Western and white and male. After all, the aim of the descriptive program was to empirically learn about science—science in general, any and all of it. So no one could have been expected to know, before the research was done, that the greatest science was representative of science in general, or could be used as a normative model for science in general, that white, Western, men's science was just like (or could be used as a normative model for) Western women's science, or Eastern science, or African science, that the aims and methods and standards of the greatest physical science could be fruitfully applied to the social sciences, and the like. Justified or not, however, what have been the consequences of this program? Since, most frequently, only the greatest physical science of white, Western, men scientists is given in support of our conceptions of science, our conceptions of science are shaped accordingly. When we think of science, we think of the greatest physical science of white, Western men scientists. And this may make women's actual exclusion from the most important activities of science appear more justifiable. Furthermore, with so much attention focused in this way on great physical science, women's invisibility and negative treatment within other areas of science become matters of small concern, if noticed at all. After all, women are not misrepresented within *physical* science, and if there are no gender-related problems within the content and methods of physical science, there are no gender-related problems within "real science," the only area of science that counts. And finally, with so much reverence bestowed on physical science, the social sciences have frequently felt impelled to model their research methods and goals on those of the physical sciences, with negative portrayals of women and other minorities as a result. For example, research design in psychology has tended to emphasize physiological or biochemical variables, and variables defined by performance on psychological tests or manipulation of circumstances in the research situation, and to de-emphasize the background, personal history, and gender of subjects and experimenters, as well as research

situations outside the laboratory or in naturalistic settings. And this has had the effect, some have claimed, of producing gender bias in, among other areas, achievement motivation research and research on influenceability or suggestibility (Sherif 1987).

5. Philosophy of Biology to the Rescue?

In his recent book *Science as a Process* (1988b), David Hull has set out to further the project Kuhn started thirty years ago in his *Structure of Scientific Revolutions*. On the basis of a more developed analysis than Kuhn provided of scientific community, Hull aims to give us "an evolutionary account of the interrelationships between social and conceptual development in science" (p. 12). Taking for granted that scientists desire, among other things, to gain recognition for their work (especially via the *use* of that work) from those working in their area whom they respect, Hull argues that this desire largely accounts for the social organization and behavior of scientists and, ultimately, the process of conceptual change. (For example, scientists behave in ways calculated to encourage other scientists to use their work, they use the work of others for the support it provides their own work, they form research groups to provide themselves with receptive audiences, and the like.) From the point of view of the issues addressed in this essay, also, Hull's book represents a further development of Kuhn's approach. For example, the book makes use of extended historical narratives to support its evolutionary account of science, narratives that cover the work of ordinary scientists as well as those generally regarded as among the greatest scientists, and women scientists as well as men scientists, and it focuses on biology rather than the physical sciences.

Still, from the point of view of the issues addressed in this essay, Hull's account of science gives one cause for concern. Consider, for example, a remark buried in a footnote (p. 390, note 4):

> In this book, I have ignored the claims made by some feminists that science is itself sexist. By this they do not mean simply that scientists have been and continue to be sexists, i.e., that their views about sexual dimorphism in the human species are frequently biased, but that scientific methods are themselves in some significant sense male-biased.... However, if I am right about the central role of competition and aggression in science and if these characteristics are more common among males than females (regardless of why), then there may be a sense in which the social organization of science is male-biased.

In short, Hull has studied Western, white, male-dominated biological science, he has found that competition and aggression play a central role in it, and he has declared, as a result, that the social organization of science (all science, everywhere?) may favor males over females. But Hull suggests no change in response to this. On the contrary, he cautions:

> ...The functional perspective does lead one to be somewhat cautious in attempting to change a system.... To the extent that a system is functionally organized, changes are sure to ramify, and these ramifications may well be extensive, not to say unpredictable. Unless one is willing to risk the destruction of the system that one wants to change, caution is called for. (pp. 355-56)

And in another place (Hull, 1988a, p. 154):

> Perhaps scientists could be raised so that they were not so strongly motivated by curiosity and the desire for individual credit, but I am not sure that the results would be worth the effort. In fact, such efforts, if successful, might bring

science to a halt. At the very least, in the absence of the mechanism which I have sketched, science could be likely to proceed at a very leisurely pace.

And while Hull cautions, he reassures: "Thus far, even though the same sorts of prejudices that permeate the rest of society have served to discourage certain groups from contributing to science as fully as they might, enough white, middle-class males have possessed sufficient talent and drive to fulfill the goals of science" (1988b, p. 389). The result, for Hull, seems to be this: Western, white male-dominated science, though perhaps unjust, has worked extremely well, and tampering with it is extremely risky, with no clear gain in sight. So why not just leave it alone? But this result is completely unacceptable. For one thing, Western, white, male-dominated science has not worked "extremely well": aside from the problems for women that we have discussed, for example, such science has brought us profound ecological problems and narrowly escaped (and sometimes not escaped) nuclear destruction along with the good things to which Hull would doubtless point. For another, there are no longer enough talented and motivated, white, middle-class males to fulfill the goals of science, and so we *must* now make it more hospitable to others, including women.[3] This would serve to open science up to the *whole* pool of talented, motivated young people, rather than just a part of that pool, and should thereby benefit science. It should also ameliorate the problems for women that we have discussed.

6. Conclusions, and a Few Suggestions

So what is the upshot? At the outset we considered three of the charges feminists have leveled at science: that science has generally excluded women from its most important activities, that science has tended to leave women largely invisible in its knowledge and research, and that science has often portrayed women, and things feminine, in negative terms when it *has* considered us. Noting the seriousness of these charges, I asked what role philosophy of science should play in responding to them. More specifically, I asked whether philosophy of science has helped to keep the problems of women in science invisible and intact, and whether philosophy of science can offer anything useful for their solution. The assumption, of course, was that the philosophy of science, at least as much as science itself, the history of science, and the sociology of scientific knowledge, shapes the conception of science of many persons—scientists and teachers and students of science, social scientists and historians of science, science policy makers, and the like—including many persons who will find themselves in a position to directly or indirectly deal with the problems of women in science.

We have found that two main programs in philosophy of science in our century—the prescriptive program of logical empiricism and the more descriptive program ushered in by Thomas Kuhn and others in the 1960s—have both helped in various ways to keep the problems of women in science invisible and intact. This leaves my second question—whether philosophy of science can offer anything useful for solving these problems. If the task of the philosophy of science is to characterize the logic of science, then one useful contribution philosophy of science can make to the solution of the problems of women in science is to investigate women's contributions to science as well as men's. It is really quite remarkable, after all, that the work of eminent women scientists like Marie Curie—the first person ever to win *two* Nobel Prizes—Barbara McClintock, and Rosalind Franklin, to mention only three examples, is only *rarely* considered by philosophers of science—and then only by *women* philosophers of science! Investigating these and other equally eminent women scientists' work along with the work of the equally eminent *men* scientists which *is* regularly considered by philosophers of science would help us to see science as a possible and appropriate activity for women as well as men.

But investigating the work of eminent women scientists is not enough. If women have done science differently from men—have been interested in different problems, accepted or assumed different factual claims, proposed different hypotheses, devised different methods, pursued different research programs, and the like—then philosophers of science must investigate that fact and represent the diversity in their characterizations of science—since these characterizations must portray *all* science, women's as well as men's. But if women have done science differently from men, that fact is not likely to show up in the *singular* achievements of truly eminent women scientists, women scientists, be it noted, who achieved eminence in sciences dominated by men. If women have done science differently from men, that fact is more likely to show up in more large-scale comparisons of the work of men scientists with that of women scientists who have been relatively free of men scientists' domination. These would include, for example, comparisons of the work of men's scientific communities with the work of women actively excluded from those communities—e.g., comparisons of the researches and practices of men gynecologists of the past with the researches and practices of women midwives. They would include comparisons of the research done in scientific fields before and after the influx of sizable numbers of women researchers—e.g., investigation of the theoretical and methodological changes in primatology in the 1970s and '80s (see, for example, Hrdy 1986; and see Oakley 1981. and the Biology and Gender Study Group 1988 for possible examples of other changes that women scientists have brought). They would include, as well, comparisons of men's research in fields like psychology and biology with the recent gender critiques of that research provided by women researchers.

In short, if philosophers of science are to fruitfully investigate women's contributions to science, they must include in this way consideration of the work of ordinary women scientists as well as that of the extraordinary ones. And if they do this, philosophers of science will help us to appreciate women's unique contributions to science, and will thereby help us to attain to a conception of science that significantly relates to women as well as men. In the process, philosophers of science will confront old philosophical questions in interesting new forms. For example, the old "demarcation question" of how science is to be distinguished from non-science will arise in a new way when philosophers of science consider the work of women of the past who were actively excluded from the training and practice of the official (men's) "science" of the times. The old realism—anti-realism question of whether science yields the truth about the world will arise in a new way when philosophers of science consider the possible methodological and substantive differences between men's and women's scientific work and wonder whether such work, despite its gendered features, can still yield the truth about a gender-independent reality. Old questions regarding the nature of scientific development, scientific explanation, and the like will arise in new ways as well.

If the task of the philosophy of science is to characterize the logic of science, then, one useful thing philosophers of science can do is to characterize the logic of women's science as well as men's. But another thing philosophers of science can do is to reflect on the result toward which this logic of science is directed. Realist philosophers of science, of course, say that science is directed toward true (or approximately true or eventually true) information about the world. Anti-realist philosophers of science say that science is directed toward merely useful, but not true, information about the world. And thus far, realists and anti-realists have not been able to come to any kind of agreement, and some (like Arthur Fine in 1984a and 1984b) have persuasively argued that they never will. Be that as it may, at least many realists and anti-realists should still agree on *one* point: that science is directed toward socially useful informa-

tion. For at least many realists would doubtless allow that science is directed toward true information *because* such information is useful to society as well as valuable in itself. And anti-realists would doubtless agree that science is directed toward information useful to society, though they would add that such information is not true (or, at least, that we have no reason for saying that it is). So, at least many realists and anti-realists should agree that science is directed toward socially useful information, though they would say other incompatible things besides. But this is a significant point of agreement: that science is directed toward socially useful information, and has succeeded so frequently in that venture, is the main reason scientific research is funded so generously. If "truth" were all that was at issue, or what primarily was at issue, something valuable in itself, scientific research would probably be funded the way art, or music, or literature is currently funded, which is to say, not very well at all.

If at least many philosophers of science can agree that the logic of science we seek to characterize is directed toward socially useful information, can we agree on anything more? Philosophers of science have spent little time thus far analyzing exactly what this "socially useful information" consists in. Indeed, "socially useful information" has tended to be interpreted as "empirically adequate" information, or information useful "for prediction and control," without a specification of for what and whose purposes such information is empirically adequate, or what is to be predicted and controlled, and by whom. If at least many philosophers of science can agree that science is directed toward socially useful information, we can begin to explore this question, keeping in view women's needs and situation in society as well as men's.

For example, in *Science as Social Knowledge* (1990) Helen Longino compares at length two research programs: a "linear-hormonal" program comprised of studies attempting to establish a prenatal hormonal basis for behavioral sex differences, cognitive sex differences, the etiology of homosexuality, and the like; and a "selectionist" program of studies addressed to the question, What sort of structure and functioning must characterize a brain capable of long- and short-term memory, learning and correction of memory, observational as distinct from conditioned learning, self-awareness, creativity, and mediation of action and experience? Longino suggests that neither of these programs is conclusively supported by its data. Indeed, she claims, the programs themselves "determine the relevance and interpretation of data," and as a result, "are not in turn independently or conclusively supported by data" (p.189). Longino concludes:

> I think ... 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 capacities, will reveal the efficacy of intentional states (understood as very complex sorts of brain states or processes). (pp. 189-190)

But Longino also suggests in her comparison that the linear-hormonal program and comparable research programs imply *gender inequalities* and much else besides—that the traditional notions of autonomy and responsibility, presupposed by our ideal of political liberty and central to our ideas of political equality and moral appraisal, fail to characterize human action (pp.151-154, 171-175). The selectionist program, on the other hand, strives to provide, not the physiological conditions sufficient to produce human behavior, but a general understanding of the kinds of neurophysiological processes necessary for intelligent, reflective, self-conscious, creative activity. As a con-

sequence, it does not have these untoward implications. "By asking what the character of brain processes underlying complex human behavior must be the inquiry emphasizes the enabling rather than the limiting aspects of biology" and "returns both autonomy and responsibility to the person" (pp. 175-176).

Assuming Longino is right in her analyses of the linear-hormonal and the selectionist research programs, what conclusions can we draw? If the logic of science we seek to characterize is directed toward "socially useful information," and "socially useful information" means simply "empirically adequate information," then the hormonal program and the selectionist program are on an equal footing. If, however, science is directed toward "socially useful information," and "socially useful information" means something like "empirically adequate information that supports a morally good society," then (provided traditional assumptions regarding the features of a morally good society are accepted) the selectionist program is far superior to the hormonal program—not only far superior, but the only one of the two that can ever possibly fulfill this goal of science. But the same kinds of comparisons can be made with regard to other cases and other scientific fields, and even between different scientific fields—between, for example, under-funded, but socially important projects that survive, somehow, in the social sciences and well-funded, empirically progressive projects within the physical sciences whose social value is, at best, distant. Which interpretation of "socially useful information" shall we choose, and how shall we develop it, and what evaluations of science and suggestions to scientists shall we make as a result? I think we need to concern ourselves with these issues—with the benefits and burdens that science actually contributes to society along with the "logic of science" that leads to them, and the "truth" of its products, and with the contributions that science *should* be striving for, and the role it *should* play in a good society. If we do, we will ultimately be uniting the "new" descriptive program in the philosophy of science with the prescriptive goals of its predecessor—prescriptive goals now based, however, not on logic and epistemology, but on moral and political philosophy instead. Such a combined descriptive/prescriptive program would do what philosophy has traditionally set itself to do—capture and clarify the established order, question it, and suggest alternatives to it. Such a combined descriptive/prescriptive program would thus be an important and needed addition to the various (descriptive) social studies of science, something that cannot always clearly be said of the current descriptive program in the philosophy of science. Of course the problems of women in science would have a central place in such a program. But it would include much else besides.

Notes

[1]I would like to thank the members of the philosophy departments of the University of Notre Dame and the University of Utah, and particularly Ed Manier and Phil Quinn, for helpful comments on this paper.

[2]An exception was geology. See the papers for the symposium "Philosophical Consequences of the Recent Revolution in Geology" in Asquith and Hacking 1981. But see, also, Ruse's comment in one of them ("What Kind of Revolution Occurred in Geology?" p. 240):

Given the fact that the major topic of debate amongst philosophers of science in the past fifteen years has been over the exact nature of a scientific "revolution," one might think that so dramatic a revolution so close at hand, in a science which is really not *that* technical (at least is not as incomprehensible to the outsider as modern parti-

cle physics), would have attracted immediate and detailed attention by the philosophical fraternity.... The revolution in geology has been greeted by philosophers of science with absolutely crashing silence.

Meanwhile, geologists themselves, as well as psychologists, sociologists, and practitioners of other ignored sciences struggled to apply Kuhn's account of science to their own fields. See, for example, the papers in the above-mentioned symposium, as well as the social science papers in Gutting 1980.

[3]Interesting research is now being done to figure out just how to pull this off. See, for example, Rosser 1990; and Manis, Sloat, and Davis 1990.

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