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Gender and the Biological Sciences

Feminist critiques of science provide fertile ground for any investigation of the ways in which social influences may shape the content of science. Many authors working in this field are from the natural and social sciences; others are philosophers. For philosophers of science, recent work on sexist and androcentric bias in science raises hard questions about the extent to which reigning accounts of scientific rationality can deal successfully with mounting evidence that gender ideology has had deep and extensive effects on the development of many scientific disciplines.

Feminist critiques of biology have been especially important in the political struggle for gender equality because biologically determinist arguments are so often cited to 'explain' women's oppression. They explain why it is 'natural' for women to function in a socially subordinate role, why men are smarter and more aggressive than women, why women are destined to be homebodies, and why men rape. Genes, hormones, and evolutionary processes are cited as determinants of this natural order and ultimately as evidence that interventions to bring about a more egalitarian and just society are either useless or counterproductive.

The critiques of biology are also *epistemically* important because of the position that biology occupies in the usual hierarchy of the sciences —somewhere between physics, on the one hand, and the social sciences, on the other. Very often feminist critiques of the social sciences are dismissed out of hand by philosophers of science on the grounds that the social sciences aren't science anyway; and so the feminist critiques, however devastating, are said to tell us nothing about the nature of real science. It is, however, not quite so easy to dismiss biology as pseudo-science; and so the critiques in this area assume added significance. If we are to infer in light of the feminist critiques anything about the nature of science (its

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rationality, its objectivity, its degree of insulation from social influences, its character as an individual or collective enterprise), then the biological sciences are perhaps the best place to start. Hence this essay. It has four parts. In section I, several case studies of gender ideology in the biological sciences are reviewed. This review provides a common stock of examples for discussion purposes and the opportunity to indicate very briefly how standard theses in philosophy of science can provide partial illumination of them. In section II, the possible epistemic significance of these case studies (and others like them) is addressed in light of alternative conceptions of science available in the feminist literature. The third part of the essay develops an account of the relation between contexts of discovery and justification that makes room for the sorts of social and cultural influences on science exemplified by gender bias while still allowing room for fairly robust notions of objectivity and rationality. Finally, in section IV, an attempt is made to locate this account vis-à-vis others represented among feminist critiques of science.

I | Some Case Studies

Consider first a 1988 article entitled 'The Importance of Feminist Critiques for Contemporary Cell Biology,' authored by the Biology and Gender Study Group at Swarthmore College.[1] The article discusses the ways in which contemporary research is still shackled by outmoded models of the relationship between egg and sperm in reproduction. In particular, commitment to the Sleeping Beauty/Prince Charming model of egg and sperm may have blinded researchers and theoreticians to some of the facts about human reproduction. Just as women are seen to be passive and men active, so traditionally have egg and sperm been assigned the traditional feminine and masculine roles. The egg waits passively while the sperm heroically battles upstream, struggles against the hostile uterus, courts the egg, and (if victorious) penetrates by burrowing through, thereby excluding all rival suitors. The egg's only role in this saga is to select which rival will be successful.

The notion that the male semen awakens the slumbering egg appeared as early as 1795 and has been influential ever since. In the last fifteen years, however, some rival accounts have challenged the old narrative by making the egg an energetic partner in fertilization. For example, using electron microscopy it can be shown that the sperm doesn't just burrow through the egg, as previously thought. Instead, the egg directs the growth of small, finger-like projections of the cell surface to *clasp* the sperm and slowly draw it in. This mound of microvilli extending to the sperm was discovered in 1895 when the first photographs of sea urchin fertilization were published; but it has largely been ignored until recently.

What matters for our purposes here is not whether the newer theory is entirely correct (it is still controversial), but that its very existence as a rival to the more established views throws into sharp relief the questionable assumptions of the older model. It shows us how pre-existing theoretical assumptions inform which questions we ask, which hypotheses we investigate, and which data we decide to ignore as evidentially insignificant. These considerations are sometimes relegated to the context of discovery and are said to be epistemically irrelevant to the actual content of science. This is a topic to which we shall return later. In the meantime, let us investigate some cases in which the controverted question is not whether some data are evidentially significant at all, but which interpretation should be placed upon the same data as the result of differing theoretical commitments.

Many feminist criticisms of primatology and sociobiology focus on the fact that male struggle, male competition, and male inventiveness are portrayed as the bases for human evolution. In familiar passages from *The Descent of Man* quoted by Ruth Hubbard and other critics, Darwin attributes evolutionary development in human beings almost exclusively to male activity.

> [Men have had] to defend their females, as well as their young, from enemies of all kinds, and to hunt for their joint subsistence. But to avoid enemies or to attack them with success, to capture wild animals, and to fashion weapons, requires the aid of the higher mental faculties, namely observation, reason, invention, or imagination. These various faculties will thus have been continually put to the test and selected during manhood.

'Thus,' the discussion ends, 'man has ultimately become superior to woman' and it is a good thing that men pass on their characteristics to their daughters as well as to their sons, 'otherwise it is probable that man would have become as superior in mental endowment to woman, as the peacock is in ornamental plumage to the peahen.'[2]

The influence of Darwin's androcentric bias has not been limited to evolutionary biology, since that theory functions as an auxiliary hypothesis in many other disciplines. Consider, for example, anthropology. If one holds the view that man-the-hunter is chiefly responsible for human evolutionary development, one interprets fossil evidence in light of the changing behavior of males. Helen Longino and Ruth Doell, for example, in a very important 1983 article called 'Body, Bias, and Behavior: A Comparative Analysis of Reasoning in Two Areas of Biological Science,'[3] trace the ways in which the androcentric account attributes the development of tool use to male hunting behavior. Some recent work, however, has suggested that up to 80% of the subsistence diet of what used to be called hunter-gatherer societies came from female gatherers. If that is the background theory informing one's interpretation of the evidence, then quite a different account of that same evidence emerges. This is how Longino and Doell summarize the point:

> By contrast [with the androcentric account], the gynecentric story explains the development of tool use as a function of female behavior, viewing it as a response to the greater nutritional stress experienced by females during pregnancy, and later in the course of feeding their young through lactation and with foods gathered from the surrounding savanna. Whereas man-the-hunter theorists focus on stone tools, woman-the-gatherer theorists see tool use developing much earlier and with organic materials such as sticks and reeds. They portray females as innovators who contributed more than males to the development of such allegedly human characteristics as greater intelligence and flexibility. Women are said to have invented the use of tools to defend against predators while gathering and to have fashioned objects to serve in digging, carrying, and food preparation.

Again, what matters here is not that the gynecentric hypothesis be *true* but rather that it makes obvious the extent to which the standard interpretation of the anthropological evidence has been colored by androcentric bias.

The cases examined so far are instances in which attention to the theory-ladenness of observation or the underdetermination of theory by data shed some light on the way in which pre-existing theoretical commitments regarding sex and gender may influence decisions about which questions get asked, which data must be accounted for and which can safely be ignored, as well as which interpretation among those that are empirically adequate is actually adopted. There are other cases in which attention to the Duhem-Quine thesis is helpful. Even if the body of relevant data has already been strictly delimited with preferred interpretations settled upon, and even if the test hypothesis has been selected, it is still to some extent an open question how one ought to respond to apparently falsifying information. Although one may simply reject the test hypothesis, it is also possible to pin the blame for a failed prediction on one of the background assumptions that was used to generate the failed prediction. The arrow of *modus tollens,* in other words, may be redirected away from the test hypothesis and toward one or more of the auxiliaries. This, of course, can be a perfectly respectable and useful response to failed prediction; but it does raise interesting questions about what factors (social as well as more narrowly 'cognitive') motivate our decisions to protect some hypotheses from falsification. It also draws attention to the important role played in theory assessment by our background assumptions, a role that is particularly crucial in the present discussion since so few of our background assumptions about sex and gender have been subjected to systematic scrutiny.

Certain hypotheses seem to survive one falsification after another, with the blame for failure and the subsequent adjustment always being located elsewhere in the system of beliefs. I have in mind here recent developments in neuroanatomy which are directed to explaining intelligence differences between women and men, particularly as these relate to alleged male superiority with respect to mathematical and spatial ability. Anne Fausto-Sterling, in her book *Myths of Gender*,[4] has surveyed some recent theories; the following examples are taken from her discussion.

It has been suggested that spatial ability is X-linked and therefore exhibited more frequently in males than in females; that high levels of prenatal androgen increase intelligence; that lower levels of estrogen lead to superior male ability at 'restructuring' tasks. Some have held that female brains are more lateralized than male brains and that greater lateralization interferes with spatial functions. Others have argued that female brains are *less* lateralized than male brains and that less lateralization interferes with spatial ability. Some have attempted to save the hypothesis of X-linked spatial ability from refuting evidence by suggesting that the sex-linked spatial gene can be expressed only in the presence of testosterone. Others have argued that males are smarter because they have more uric acid than females.

None of these hypotheses is well-supported by the evidence and most seem to be clearly refuted. What is interesting for our purposes is that for many researchers the one element of the theoretical network they are unwilling to surrender in the face of recalcitrant data is the assumption that there must be predominantly *biological* reasons for inferior intellectual achievement in women.

Some have found this situation reminiscent of nineteenth-century craniometry's well-known attempt to explain inferior female intelligence by appealing to brain size. This is a case also discussed by Fausto-Sterling. The 'bigger is better' hypothesis foundered on the elephant problem (if absolute size were the true measure of intelligence, elephants should be smarter than people). So it was suggested that the true measure of intelligence lay in the proportion of brain mass to body mass; but this proportion favored women, and so the hypothesis was quickly rejected. The proposal that greater intelligence is linked to a lower ratio of facial bones to cranial bones ran afoul of the 'bird problem.' So it was suggested that the frontal lobes are the seat of the intellect, and men have bigger frontal lobes; the parietal lobes are larger in women. This hypothesis was surrendered when newer research pointed to the parietal lobes as the seat of the intellect. So the data were re-evaluated to show that *really* women have smaller parietal lobes . . . and so the saga continued. The one component of the theoretical network that scientists were unwilling to give up in the face of apparent falsification was the underlying assumption that women are *biologically* determined to be less intelligent than men. It is no wonder that feminist critics find the same pattern reinstated in current debates about gender and mathematical ability.

In the preceding cases, appeal has been made to such standard philosophical theses as the theory-ladenness of observation, the underdetermination thesis, and the Duhem-Quine thesis in order to suggest how gender ideology could permeate the biological sciences even on fairly standard accounts of theory appraisal. In these cases, we might want to say that external values have been imported into science; but the values are *implicit* in these cases and often exposed only in light of a rival hypothesis embedding conflicting values. The situation is different in the last set of cases in this rapid review of the literature. In the medical sciences, values or norms are often quite explicit. When one has to judge who is healthy and who is diseased, what body types are desirable and which not, the concepts involved are explicitly normative as well as descriptive. This opens the door for types of gender bias other than those discussed above. In one type, different ideals are set for male and female; these ideals are said to be 'complementary' but really only the male is seen as fully human. Another type of bias occurs when a single norm is adopted for both males and females, but is in actuality a male rather than human norm.

A nice historical example of the complementarity problem is developed in Londa Schiebinger's excellent book, *The Mind Has No Sex? Women in the Origins of Modern Science*.[5] Schiebinger documents the changes that occurred in representations of male and female anatomy as a concerted effort was made in the eighteenth century to ground gender differences in anatomy. If differences between masculinity and femininity could be located in the *bones* of the organism, in its infrastructure, then there would be a modern scientific account of difference, and it would no longer be necessary to rely on the old heat models of Aristotle and Galen to do the job.

Prior to this time, male and female skeletons had been portrayed as similar; they were not sexualized. Sometimes the sex of the skeleton was not identified; sometimes the front view was represented as male, the back view as female. But all this changed in the years between 1730 and 1790.

> The materialism of the age led anatomists to look first to the skeleton, as the hardest part of the body, to provide a "ground plan" for the body and give a "certain and natural" direction to the muscles and other parts of the body attached to it. If sex differences could be found in the skeleton, then sexual identity would no longer depend on differing degrees of heat (as the ancients had thought), nor would it be a matter of sex organs appended to a neutral human body (as Vesalius had thought). Instead, sexuality would be seen as penetrating every muscle, vein, and organ attached to and moulded by the skeleton.[6]

The male and female ideals that emerged were very different from one another. The male skeleton was typified by a big head and strong shoulders; its animal analogue was the horse, which sometimes appeared in the background of male skeletal drawings. The female skeleton had a large pelvis, a long elegant neck, and a smallish head. She had much in common with the ostrich who sometimes decorated her portrait. Those skeletons which approximated most closely to cultural ideals of masculinity and femininity were favored for just that reason over drawings that were in some sense more accurate.

It is worth noting that one way the largeness of the female pelvis and smallness of the head are emphasized is by depicting a very narrow rib cage. Fausto-Sterling points out that there may have been more than just the power of ideology at work here. It may be that some of the corpses on which the drawings were modeled had their rib cages compressed by long-term use of the corset. This reminds us again that Ruth Hubbard[7] and others are correct when they argue that it is wrong to think of the body as a purely biological infrastructure onto which the socio-cultural crud of gender accretes. Although the distinction between 'sex' as biological and 'gender' as socially assigned has in many respects served feminist theorizing well, it has sometimes led to the mistaken assumption that all biological attributes are given in some absolute sense. *Sex* as well as gender is socially constructed, at least in part. Such 'physical givens' as height, bone density, and musculature are to a large extent determined by cultural practice.

The skeletal case is one in which the male and female norms are said to be complementary, but the male is treated as more fully human. In other cases, there is allegedly a single human norm, but on closer inspection it turns out to be masculine. It has been suggested that the treatment of menstruation, pregnancy, and childbirth as diseases or medical emergencies may be traced to the fact that these are not things that happen to the ideal healthy human being who is, of course, male. The ideal healthy lab rat is also male. His body, his hormones, and his behaviors define the norm; so he is used in experiments. Female hormones and their effects are just *nuisance variables* that muck up the works, preventing experimenters from getting at the pure, clean, stripped-down essence of rathood as instantiated by the male model. Insofar as the female of the species is truly a rat (or truly a human being), she is covered by the research on males. Insofar as she is not included in that research, it is because she is not an archetypal member of her own species. The dangerous effects of such research procedures, especially in the biomedical sciences, are just now being documented. For far too long, the assumption underlying these experimental designs (that males are the norm) simply went unchallenged.

Elisabeth Lloyd is writing a book called *All About Eve* on the development of the female orgasm, and it includes a lovely example of a male norm masquerading as human. Various sociobiologists, when advancing theories about the evolutionary origins of the female orgasm, have cited detailed statistics about the nature, length, frequency, and repeatability of orgasm in order to support their origin stories. When tracking down their footnotes, Lloyd discovered that these statements—which were being used to explain the origins of the *female* orgasm—were in fact based on data about *male* orgasms. The sleight of hand was typically accomplished by referring to the male subjects as 'individuals' rather than males!

II | Varieties of Feminist Critique

Case studies such as those canvassed above are interesting in their own right, but they leave open the question of what we are to make of them. Two contexts in which this question arises interest me particularly.

1 In the feminist literature the question that has been foremost in the last few years is whether these case studies are examples of 'bad science' or whether, on the contrary, they show that science is intrinsically and irredeemably androcentric.

2 In philosophy of science the question too often has been: what does this have to do with philosophy of science?

The two questions are related, and I should like to tackle them together. With respect to the first, Sandra Harding's tripartite taxonomy of feminist epistemologies has been extremely influential.[8] In order to deal with the bewildering diversity of feminist critiques of science, Harding proposes dividing them into three categories: feminist empiricism, standpoint epistemologies, and feminist postmodernism.

'Feminist empiricism' diagnoses failures such as those sketched above as failures of science to live up to its own ideals. Androcentric bias has gotten in the way of rigorous application of scientific method; but if the canons of science had been adhered to faithfully, episodes such as those above could have been avoided. For feminist empiricism, the standpoint of the knower is epistemically irrelevant; any bias originating from that standpoint will be eliminated by proper application of objective methods.

This assumption is denied by 'standpoint epistemologists' who argue that the credentials of the knowledge claim depend in part on the situation of the knower. Just as Hegel's slave could know more than the master, so women (or feminists) may enjoy an epistemic advantage over men (or non-feminists). A science based upon the standpoint of women would be an improvement over current science, according to standpoint epistemology. In this sense it is still a 'successor science' project, since its aim is to produce a *better* (epistemically superior) account of the world. A number of problems have been pointed out with this approach, but the most damaging criticism has been the insistence that there is no single feminist standpoint. Just as the standpoint of women differs from that of men, so also the standpoint of poor women differs from that of rich women, the standpoint of black women from that of white women, the standpoint of lesbians from that of heterosexual women, and so on. On what grounds could one of these be privileged over the other as a standpoint from which to describe the world?

This fracturing of identities and hence of standpoints has led some theorists to embrace what Harding calls 'feminist postmodernism' by giving up altogether the endeavor to become more and more objective and by accepting the existence of an irreducible plurality of alternative narratives about the way the world is. The notion of a scientific method that might allow us to transcend the constraints of culture, time, and place is repudiated once and for all by feminist postmodernists. Transtheoretical criteria for rationality and objectivity are dismissed as products of a masculine mythology, and the 'successor science' project is abandoned.

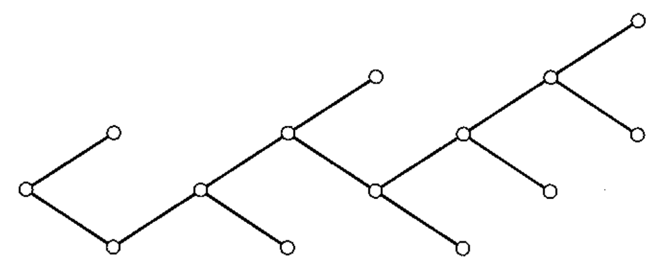
Although Harding's taxonomy has been very helpful in facilitating analyses of the diverse philosophical commitments of feminist critics, I fear that it also tends to obscure a promising possibility, one that takes into account the ways in which social structures (like the institution of gender) affect the very content of science without surrendering altogether the ideal of rational theory choice. In the following section, such a position is sketched.

III | Science and Shared Social Values

Traditionally, philosophy of science has been quite willing to grant that social and psychological factors (including perhaps gender) play a role in science; but that role has been a strictly delimited one, contained entirely within the so-called context of discovery, or alternatively within those episodes called 'bad science' in which the canons of rationality were clearly violated in favor of other interests. (The Lysenko Affair is a standard example here.) In the context of discovery or theory generation, says the traditional story, anything goes: the source of one's hypotheses is epistemically irrelevant; all that matters is the context of justification. If you arrived at your hypothesis by reading tea leaves, it doesn't matter so long as the hypothesis is confirmed or corroborated in the context of justification. You test the hypothesis in the tribunal of nature and if it holds up, then you're justified in holding on to it—whatever its origins. The idea here is that the canons of scientific theory choice supply a sort of filter which removes social, psychological, and political contaminants as a hypothesis passes from one context to the next.

This view made a certain amount of sense in the first half of this century when models of theory evaluation held that hypotheses were compared directly to nature. But this account, which shears the context of discovery or theory generation of all epistemic significance, makes no

sense at all given current models of scientific rationality that view theory choice as irreducibly *comparative*. That is, we now recognize that one does not actually compare the test hypothesis to nature directly in the hope of getting a 'yes' or 'no' ('true' or 'false') answer; nor does one compare it to all logically possible rival hypotheses. We can only compare a hypothesis to its extant rivals—that is, to other hypotheses which have actually been articulated to account for phenomena in the same domain and developed to the point of being testable. So the picture underlying current debates regarding theory choice looks something like this:



Each of the nodes is meant to represent a decision point at which the scientist must choose among alternative rivals. Methodological objectivists argue that so long as the proper machinery of theory assessment is brought to bear at each of the nodes, the rationality of science is preserved. How the nodes were generated in the first place is irrelevant, so long as the right decisions are made at each juncture. There may be interesting sociological stories to tell about the generation of the various alternative hypotheses, but sociological influences are effectively screened from affecting the content of science by the decision procedure operating at the nodes. This procedure will tell us which theory is preferable to its extant rivals on purely objective grounds.

My point, however, is that even if we grant for the sake of argument that scientific method is itself free of contamination by non-cognitive factors and that the decision procedure operates perfectly at the nodes, nothing in this procedure will insulate the content of science from sociological influences *once we grant that these influences do affect theory generation.* If our choice among rivals is irreducibly comparative, as it is on this model, then scientific methodology cannot guarantee (even on the most optimistic scenario) that the preferred theory is true—only that it is epistemically superior to the other *actually available* contenders. But if all these contenders have been affected by sociological factors, nothing in the appraisal machinery will completely 'purify' the successful theory.

Suppose, for the sake of example, that the graph represents the history of theories about female behavior. These theories may in many respects be quite different from one another; but if they have all been generated by males operating in a deeply sexist culture, then it is likely that all will be contaminated by sexism. Non-sexist rivals will never even be generated. Hence the theory which is selected by the canons of scientific appraisal will simply be the best of the sexist rivals; and the very *content* of science will be sexist, no matter how rigorously we apply objective standards of assessment in the context of justification. In fact, the best of the sexist theories will emerge more and more highly *confirmed* after successive tests.

So, if my account is right, it doesn't necessarily follow that the presence of androcentrism and sexism in science makes rational theory choice impossible, but it *does* follow that scientific method *by itself* as currently understood cannot be counted upon to eliminate sexist or androcentric bias from science. Note that methodological rationalists can still have (approximately) monotonic progress. Every choice among alternatives may be a rational choice. Science can (in principle) get better and better. But this in no way guarantees that the content of science is insulated against social influences. Once you grant that social factors may influence the context of theory generation, then you *have* to admit that they may also influence the content of science. You can't just give theory generation to the social scientists and expect to exclude them at some later date through the rigorous application of epistemic virtue. That is akin to closing the barn door after the horses have escaped.

Let me make the same point in a different way. One of David Bloor's[9] favorite arguments in support of social influences on theory content is based on the well-known underdetermination thesis. This is, of course, the claim that the data cannot pick out a single theory which uniquely accounts for them. There are, in principle, an infinite number of rival contenders that could do the same job. So, Bloor argues that if the data aren't completely determining our theory choices, then something else must be doing the job—and, of course, his favorite candidates for that job are sociological in character.

Larry Laudan's reply[10] is that there is an unfortunate tendency in the recent literature to overestimate underdetermination. Underdetermination, he says, would be a problem if we were actually faced with an infinitude or even a pair of empirically adequate rival theories. But, as a matter of fact, he says, we never encounter such an embarrassment of riches. We're lucky if we get even *two* rivals that are credible contenders for theory acceptance. We're certainly never faced with more than a small handful of competing alternatives. And we can always find (at least in the passage of time) good cognitive reasons for preferring one of these to the others. So, he concludes, although the underdetermination thesis may pose nice problems in principle, these never figure into *actual* theory choice. This is, of course, another way of stating his claim that theory choice is irreducibly comparative in nature—that our choices will always be among a finite class of extant alternatives, not among an infinitude of in-principle rivals. Since there will always be good reasons for preferring one of the extant rivals to the others, he claims that Bloor's invocation of social determinants is effectively undercut.

Notice, however, that there is an important sense in which this argument strategy simply begs the question. We can still ask why just *this* class of contenders was generated, given that others were equally compatible with the data. To say that once the rivals are fully articulated, our choice among them can be rational is to leave untouched the prior question of how our options came to be determined in the particular ways that they are. As long as Laudan concedes (as he does) that non-cognitive factors play a role in the posing of questions, the weighting of problems, and the initial articulation of theory, he cannot be sure that these factors will be eliminated in the context of justification. I stress once again that it is his attempt to maintain the *conjunction* of two views that gets him into trouble. The first of these views is that the context of discovery is normatively insignificant; the second is that theory appraisal is irreducibly comparative in nature. Once the second claim is made we *must* grant that factors affecting theory generation acquire normative significance.

The argument here is *not* that we should abolish the distinction between contexts of discovery and justification, but that we must recognize that on a comparative model factors that influence theory development and theory generation must necessarily influence our confirmation practices and hence the very content of science.

It is important to stress here that this argument about confirmation practices applies not only to test hypotheses but also to the auxiliary assumptions that jointly constitute the relevant background theory. How a particular piece of evidence bears on a hypothesis depends in large measure upon the collateral assumptions that come into play. It is here that the relationship between biology and the social sciences is particularly interesting because the traffic between the two is largely at this level. This is illustrated in some of the examples I cited earlier in this paper. For instance, in the man-the-hunter example, the relevant auxiliary assumptions are imported from evolutionary biology. In particular, it is the assumption that it is the male struggle for survival that drives the human evolutionary process that dictates in large measure what should count as evidence and how it should be interpreted. Conversely, in the Sleeping Beauty/Prince Charming model of the egg-sperm interaction, the biological data are informed by sociological assumptions about appropriate male and female roles. Donna Haraway's work in primatology[11] provides nice examples of how experimental design is influenced by background assumptions. She has traced the development of primatology since 1900, showing how political principles of hierarchy and male dominance have been embedded in that science, re-enforcing a theory of primate social organization in which a large, aggressive male is portrayed as defending a hierarchically organized troop and territory, enjoying first choice in food, sex, and grooming, and deciding troop movements. Consequently, when Carpenter undertook his highly acclaimed work on rhesus monkeys, he removed dominant males to test his organizing hypothesis about the source of social order but undertook no control study in which other members of the group were removed. We can't control for every possible variable in our experimental designs; so which we take into account depends on what our background theory tells us may be relevant. If the components of that background theory are never called into question or challenged by a serious rival, our experimental practices will continue to embody potentially problematic assumptions.

The claim here is *not* that the traffic in auxiliary assumptions makes a pernicious form of holism inevitable or that these auxiliaries are not themselves (potentially) testable,[12] but that they provide points at which gender biases from one discipline are easily transported into another. Furthermore, because of the pervasiveness of gender ideology in our culture, these assumptions generally are not called into question and are sometimes not even noticed. It is usually the case that they come to light only in the presence of a rival hypothesis.

The argument here is not restricted to hypothetico-deductive forms of confirmation and cannot be evaded by an appeal to Clark Glymour's 'bootstrapping' model.[13] Bootstrap confirmation does not make background assumptions dispensable but explicitly recognizes their crucial role: 'Hypotheses are not tested by themselves but only in relation to their fellows within the theory. Confirmation is a three-place relation, not a two-place relation. Large parts of the theory may be invoked in confirming, from given evidence, any of its hypotheses.'[14]

I have been arguing all along that *even if we grant that the standards of theory assessment are free of contamination by non-cognitive factors*, nonetheless, non-cognitive values may permeate the very content of science. Stating the thesis in this way seemed useful because it avoided the messy controversy regarding the culture-bound evolution of scientific method itself. Even *granting* the transcendence of method, in other words, the scientific product could itself be radically culture-bound.

I should mention in bringing this line of argument to a close, however, that what has been granted for the sake of argument is probably not plausible in the final analysis. Scientific method itself is developed and articulated by culture-bound individuals and so the arguments which applied at the object level of theory content will likely apply at the meta-level of theory evaluation as well. Although we may have *good reasons* for making certain methodological changes, (e.g., for moving from single blind to double blind experiments), our methodological choices will be limited by the range of alternatives already actualized.

Finally, I should touch very briefly on the implications of the preceding argument regarding the scope of models of rationality and its implications for science policy.

These appear to be the two alternatives: (1) We could simply acknowledge the reduced scope of models of rationality and make more modest claims for the objectivity of science; or (2) We could attempt to enlarge our model of rationality so that it takes into account the context of theory generation. That is, if we acknowledge that the context of theory generation has normative significance, then we may want to alter science policy in the light of a new normative account of theory generation.

Once we recognize that the content of science is affected by the social arrangements that govern its practice and production, then those social arrangements acquire *epistemic* significance as do the affirmative action programs and other interventions undertaken to alter those social arrangements. Any adequate philosophy of science will have to take this into account.

IV | Reviewing the Situation[15]

How does the account sketched above fit into Harding's taxonomy of feminist critiques of science? Clearly it shares much in common with so-called 'feminist-empiricism' insofar as it is a successor science project that aims at ever-increasing objectivity and rationality through the use of established scientific methods. It parts company with feminist empiricism, however, in at least two important respects. First, it recognizes that current methodologies simply do not take into account the epistemic significance of the social arrangements that govern the activities scientists undertake and the products they produce. Any adequate methodology will have to control for the biases introduced by these social arrangements just as it has to control for other sources of bias. (It has become fashionable recently to eschew talk of 'bias' on the ground that such talk implies the possibility of science that is entirely free of bias. I don't think the implication holds, and so I continue to speak of gender bias. We aim to eliminate other forms of partiality without thinking we'll ever be entirely successful; the same regulative ideal seems perfectly serviceable in discussions of androcentrism.)

Second, the feminist empiricism described by Harding does not appear to challenge the assumption in much traditional methodology that the rationality of the scientific community is just individual rationality writ large, a simple summation of individual rationalities. In the account sketched above, it is the rationality of the scientific community that is enhanced by inclusion of diverse strategies at the individual level. The kinds of bias discussed above can be systematically addressed only at the community level; no adequate program of *individual* rehabilitation could be prescribed in advance. Only the inclusion of diverse standpoints will bring about the conditions under which change is possible.

Is the current proposal then a kind of standpoint epistemology? Not precisely: epistemic privilege on this analysis does not attach to the individual woman (or feminist) but to the community that includes her standpoint along with others. The individual standpoints on this account are starting points. Furthermore, it is important to stress that on this analysis nothing depends on women having a different psychological make-up from men or different 'ways of knowing.' The distinctive mark of the work of the feminist critics cited above is not that it is holistic, intuitive, subjective, emotional, nurturant, or non-linear. Instead, what gives it focus and distinction is the fact that it is informed by a social and political viewpoint different from that which has dominated science and science studies.

Couldn't men have done exactly the same work? Yes, it is logically possible. But the connection here is not about necessary or sufficient conditions. It is about contingencies: about causal factors that operate, not from a God's eye point of view nor in the infinite long run, but here and now. It is not a logical necessity but also no accident that the advent of certain scientific hypotheses coincided with increased political power for women and increased representation of women in the academy and scientific communities.

Does the position advanced here have any affinities with feminist postmodernism? The overlap is minimal but not non-existent. My position is perfectly compatible with the rejection of metaphysical realism (perhaps that is even required) but not with the wholesale rejection of objectivity and rationality. The important point is that these two (metaphysical realism and objectivity) are separable, a point too often obscured in the postmodern literature. (Indeed, one often gets the impression from postmodern accounts that the logical positivists were metaphysical realists.)

I find feminist postmodernism unattractive for the usual reasons. I believe that feminist theories in science are *superior to* (cognitively preferable to) their sexist rivals, not simply that they provide alternative narratives. And I believe that postmodernism with its emphasis on fractured identities as well as on epistemic relativism provides no adequate basis for the political action feminism requires. There is much of value, however, in postmodernism's emphasis on the requirement of *local* problem-solving. Gender bias manifests itself in different ways in different sciences. There is no single 'feminist method' that will reveal and eliminate that bias. There is no 'feminist paradigm' that can be imposed from above and no reason to believe (as many postmoderns appear to believe) that gender bias in physics, for example, will be of the same kind or degree as that in biology. Real change in science will occur only when specific rival theories are developed by scientists who have both a thorough grounding in their own disciplines and a commitment to questioning biases introduced by social arrangements of science.

I believe, therefore, that it is possible to do justice to the range and depth of gender bias in the biological sciences without sacrificing altogether the traditional ideals of objectivity and rationality; but doing so will require that we take into account the social structure of science. Case studies of the sort summarized in the first part of this paper show the necessity of coming to grips with the ways in which social factors can influence the development of science, and they demonstrate the extent to which some standard philosophical tools can partially illuminate the origins and diversity of ideological biases in science. These tools, however, are inadequate to the task at hand so long as they are embedded within an outmoded and indefensible conception of the scientific process that limits the influence of social factors to the context of discovery. Mainstream philosophy of science continues to ignore feminist critiques of science at its own peril.[16]

Notes

1. The Biology and Gender Study Group, 'Importance of Feminist Critiques for Contemporary Cell Biology,' in *Feminism and Science*, Nancy Tuana, ed. (Bloomington: Indiana University Press 1989) 172–87.

2. Charles Darwin, *The Descent of Man* (1871). Cited by Ruth Hubbard in 'Have Only Men Evolved?' in *Biological Woman: The Convenient Myth*, Ruth Hubbard, Mary Sue Henifin, and Barbara Fried, eds. (Cambridge, MA: Schenkman 1982), 17–45.

3. Helen Longino and Ruth Doell, 'Body, Bias, and Behavior: A Comparative Analysis of Reasoning in Two Areas of Biological Science,' *Signs* 9 (1983), 206–27.

4. Anne Fausto-Sterling, *Myths of Gender: Biological Theories About Women and Men* (New York: Basic Books 1985).

5. Londa Schiebinger, *The Mind Has No Sex? Women in the Origins of Modern Science* (Cambridge, MA: Harvard University Press 1989) 189–213.

6. Ibid., 191.

7. See *The Politics of Women's Biology* (New Brunswick: Rutgers University Press 1990).

8. Sandra Harding, *The Science Question in Feminism* (Ithaca, NY: Cornell University Press 1986). See also S. Harding, *Whose Science? Whose Knowledge? Thinking From Women's Lives* (Ithaca, NY: Cornell University Press 1991).

9. David Bloor, *Knowledge and Social Imagery* (London: Routledge and Kegan Paul 1976).

10. Larry Laudan, 'The Pseudo-Science of Science?' *Philosophy of the Social Sciences* 11 (1981), 195–97.

11. See, for example, 'Primatology Is Politics by Other Means,' *PSA 1984*, vol. 2 (East Lansing, MI: Philosophy of Science Association 1985).

12. See Helen Longino, *Science as Social Knowledge* (Princeton: Princeton University Press 1990) [a portion of which is excerpted above, pp. 170–91].

13. Clark Glymour, *Theory and Evidence* (Princeton: Princeton University Press 1980).

14. Ibid., 151.

15. The argument in this section was also sketched in my essay, 'Birth of a New Physics or Death of Nature?' in *Women and Reason*, E. Harvey and K. Okruhlik, eds. (Ann Arbor, MI: University of Michigan Press 1991).

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