

Between an epistemology of gender and a gendered epistemology.

A review of Sandra Harding's The Science Question in Feminism.

by David Mertz.

Sandra Harding starts her book, The Science Question in Feminism, with a distinction between "the woman question in science" and that question named in the title of her book. We shall find it useful to increase this to a tripartite distinction: between what we might call, "the question of women in science," "the woman question is science," and "the science question in feminism." On each of these questions there is a particular "answer" that feminists should like to give. Harding gives the desired answer to the first two of these question, but as much as she would like to, is unable to give us any satisfactory answer to the third. It would be useful to say what these desired answers are.

Feminists who ask "the question of women in science" ask, in essence, why there are so very few, both today and in times past. Copious work has been done to show the presumption in this question is accurate, and a great deal has been done to show why<sup>1</sup>. Harding gives us an answer why there are so few women in science. This answer, though, however good it may be is much more historical than philosophical. As a philosopher rather than an historian, I shall have nothing to say about it.

"The woman question in science" is the question of how scientists have treated and should treat women as an analytic or theoretical category. Feminists argue, quite correctly, that many scientists (who have, not incidentally, been men) have come to conclusions which reflect sexist beliefs. Harding is not the first to suggest that even to have a category 'woman' demonstrates that sexism is motivating the theory. Again, we shall come back to what Harding says on this question. However, this question is not as interesting, either to Harding or to the rest of us hoping for a radical feminist critique of science, as is the final one.

"The science question in feminism" is the title of Harding's book, and hence, presumably the question of most interest to her. The answer which feminists qua feminists would like to give is that the methodology of science is inherently masculinist and/or androcentric. Harding wishes she could give this answer. On most every page we see this wish, both in and between the lines of text. But, ultimately, Harding cannot seem to suggest in any satisfying way that the methodology of science is inherently masculinist and/or androcentric. All she can do (or does in this book) is to express the devout, and admirable, wish to call science's methodology androcentric; point to and criticize the androcentric practices of actual scientists; and criticize science's methodology in ways which are not specifically feminist. All of these things are worthwhile and important endeavors, but they do not live up to either the title or the stated goals of the book. None of this is to say that a specifically feminist critique of science's methodology is not possible. I believe such a critique is possible, and shall make some remarks about at least one way in which it could be pursued.

Let us be clear on what is required by a specifically feminist critique of science's methodology. This feminist critique is not merely a critique of "bad science," it is a critique of "science-as-usual." Harding introduces this distinction, and the distinction is important to understanding her attempts at making her more radical statements. A critique of bad science would include criticism of the ways in which sexist or androcentric scientists have produced sexist and androcentric ontologies and metaphors. However, a critic of bad science could still hold that stricter adherence to existing methodological norms could overcome the shortcomings of particular scientists-- or even of particular theoretical frameworks. A critique of science-as-usual does more than this. It recognizes, "the fundamental value-ladenness of knowledge-seeking and thus

*the impossibility of distinguishing between bad science and science-as-usual* (p.22)." That is: for the critic of science-as-usual, and this includes Harding, values must enter into every aspect of knowledge; there is no value-neutral knowledge. I would tend to agree with this claim and shall press what I believe is a deeper claim, and one which Harding needs to pursue in order to make a critique of science's methodology viable. Namely, there is something fundamental to human experience and cognition which systematically distorts all knowledge production. I believe I shall be able to say what this "something" is.

The value-ladenness of knowledge-seeking which Harding points out does not lead to a relativism which claims that for every set of values there is a different knowledge, and we have no basis for deciding between the knowledges.

Rather, Harding asks (expecting an affirmative answer),

whether it is possible that some kinds of value-laden research are nevertheless maximally objective. For example, are overtly antisexist research designs more objective than overtly sexist or, more important[ly?], 'sex-blind' (i.e. gender-blind) ones? (p.23)

A feminist critic of science's methodology will probably claim that "women (or feminists, whether men or women) as a group are more likely to produce unbiased and objective results than are men (or nonfeminists) as a group (p.25)."

Harding provides a good example of this. Two hypotheses are, in some sense, in competition in explaining human evolution of tool use. These are the "man-the-hunter" and the "woman-the-gatherer" hypotheses. The former, and earlier, hypothesis was, as a matter of historical fact, developed by male scientists; and by scientists who saw men as active agents in society and women as passive receptacles. This assumption gets played out in theory formation by framing the questions of human evolution in terms of the pressures to evolve on males of the

species. Once the question is asked this way, the only answer has to involve the hunting activity of men. The "woman-the-gatherer" hypothesis was developed, as a matter of historical fact, by women and by feminists who are both women and men. It is probably a better hypothesis, and is one that could only have been developed by throwing off androcentric assumptions. Women and/or feminists are in a unique position to throw off these assumptions. The hypothesis itself is merely a refocussing of attention onto the fact that women have always been responsible to provide a greater share of nutritional intake than have men. There are reasons to believe that this was also true of our distant ancestors?

There are two shortcomings of Harding's claim that women (or feminists) are more likely to produce objective results, neither of which speaks against the claim itself. The first is that Harding does not show that methodological norms of science are not in themselves sufficient to eventually overcome the androcentrism of particular scientists. The second is that Harding does not even give any reason (beyond a kind of prima facie plausibility) to suppose that women scientists are any less infected with androcentric beliefs than are men. I do not doubt that these matters can be shown, but Harding fails to do so in any clear way. Just at the key juncture we are left to fill in the connections for ourselves. Throughout the book Harding makes claims which are reasonable, and perhaps true, but fails to provide real evidence.

Such an example occurs later in the book, in chapter 6. Here Harding is trying to give an example of a specific way in which feminine or feminist thinking differs from masculine or masculinist thinking. She says,

[F]eminist thinking has produced a new comprehension of the relationship between organisms, and between organisms and their environment. The organism is conceptualized "not in terms of the Darwinian metaphor, as the passive object of selection by an

indifferent environment, but as [an] active participant, a subject of the determination of its own future." (p.145)

However, Harding leaves it opaque why this comprehension is feminist, even given that it is superior. Barbara McClintock is said to share this conception, and she is, of course, a woman; but this surely does not suffice to call this "new comprehension" 'feminist'. Rather, the reason seems to lie in the discussion in the previous chapter of object-relations theory. For those of us who possess only a passing familiarity with object-relations theory this connection is far less than clear – and it is never spelled out in detail by Harding.

Central to filling in the gaps I have mentioned, and the many more like them which have remained unmentioned, is a coherent theory of the differences in the conceptualization and/or cognition which women and men undergo. We cannot say that women are more likely to reach objective scientific conclusions than are men, *until we can say how women and men are cognitively different*. A brief suggestion of this is made in chapter 5, in the discussion of object-relations theory -- but all that Harding does therein is repeat a few conclusions of a few thinkers, outside of any coherent framework<sup>3</sup>.

In order to speak of the cognitive differences between men and women we need to have a framework in which to speak of human cognition generally. I would like to, briefly, suggest such a framework -- one which will, hopefully, make some of the conclusion which Harding wishes to draw better supported. In particular, I should like to point to two related ways in which human cognition (feminine or masculine) is structured: metaphorically and prototypically. The metaphorical structure of human thinking is discussed by George Lakoff and Mark Johnson in their book Metaphors We Live By. The prototype structure of

cognition was shown in a systematic way by Eleanor Rosch<sup>4</sup>. A review of both these topics is contained in George Lakoff's Women, Fire and Dangerous Things.

To claim, as do Lakoff and Johnson, that human cognition is structured metaphorically is to claim that many concepts are only structured at all by virtue of being compared, implicitly or explicitly, to more familiar and more concrete concepts. The concepts which most often serve as the objects of comparison are those which have the most direct experiential basis. For example, the concepts which have perhaps the most direct experiential basis are those relating to spatial orientation. It is no accident that our concepts of morality, emotions, etc. are structured by an up/down orientational metaphor; and those of time, relationships, etc. are structured by a front/back orientational metaphor<sup>5</sup>.

The other manner which I shall mention in which cognition is structured is by prototypes. A category does not pertain to all its members in the exact same way. Rather, some one member or few members which are deemed the "best examples" stand metonymically for the whole category. The fringe members are considered members of the category by virtue of having certain conventionally chosen similarities with the central members. This relates closely to "base-level categorization." Certain categories are learnt easiest and earliest; are the ones we use to name a given object, *ceteris paribus*; and have characteristic sensora and activity associated with them. For example, if we see an animal we are more likely to say, "that is a dog," than "that is a mammal," or "that is a cockerspaniel." The category 'dog' is learned by children before either the more general, 'mammal', or the more specific, 'cockerspaniel'. Further, there are particular actions (at the level of motor activity) characteristically

associated with dogs, but none with mammals in general, or cockerspaniels in particular. It seems plausible to assert that prototypically structured categories have as a core a base-level category, together with conventional rules for determining if another type of thing is similar in the "right" way.

Harding is aware of these structures in cognition, at least within science. However, she only points them out in regard to their effect in causing androcentric theories or hypotheses. It seems plausible to assume cognition is structured metaphorically and prototypically, whether or not its conclusions are androcentric. It naturally follows that when men do science, and must structure abstract concepts on the model of more familiar and concrete concepts, they do so from a masculine perspective – and these familiar and concrete concepts may be very differently structured by men and women.

For example, let us look at what Harding says about traditional metaphorical structuring of scientific thinking:

We will see assumptions that the audience for these texts are men, that scientists and philosophers are men, and that the best scientific activity and philosophical thinking about science are to be modeled on men's most misogynous relationships to women – rape, torture, choosing "mistresses," thinking of mature women as good for nothing but mothering. (p.112)

Harding backs this up by pointing to the "rape and torture metaphors [for scientific inquiry] in the writings of Sir Francis Bacon and others (p.113)." Let us assume that, in fact, scientific inquiry has been structured by the more familiar and concrete (at least in Bacon's time) concept of rape. This is a best case of androcentrism in science. If the concepts of scientific inquiry are seen as sharing the structure of the male (rapist's) experience of rape, then women are closed out of understanding the way that male scientists experience the activity of scientific inquiry. Notice here, that this is not merely a matter of the faults of



individual scientists -- it is purported that the very activity of scientific inquiry is structured by the (male) activity of rape. If the structure of the activity is not pertinent to methodology, nothing is.

The counterpoint to this androcentric science is "a science grounded in women's experience (p.148)." According to Harding,

The subjugation of women's sensuous, concrete, relational activity permits women to grasp aspects of nature and social life that are not accessible to inquiries grounded in men's characteristic activities. (p.148)

The reason for this remains unclear within Harding's book, but if we keep in mind the metaphorical and prototypical structuring of cognition, we can make sense of this. Just as men have characteristic experiences which may be used to give structure to abstract concepts, so do women -- and it is at least possible that those experiences characteristic for women may serve as better guides for structuring scientific concepts.

The greater interaction between organism and environment which Harding claims is characteristic of feminist thinking is thought by her to be, in some way, based on women's characteristic experiences. It remains to be said what women's characteristic experiences are, and how these differ from men's. The experiences of women, it should be noted, are not completely disjoint from those of men -- both women and men move around in the same world, have the same sensory and cognitive apparatus, and come in contact with many of the same objects. Hence, more of the experiences which women and men use to structure abstract concepts will be the same than will be different.

The main effect of prototyping which leads to androcentrism, presented in Harding's book, is in the very category of gender. Harding says, "women are

assumed to be the bearers of gender and only men the bearers of culture [i.e. uniquely human activity]. (p.90)" The structure of cognition which underlies this kind of belief is prototypical. The "best example" of the category 'social person' or even merely the generic 'person' is a man. Hence, a gendered person must be a woman, or girl. The prototypical person also has a race: Harding quotes Scott Hull, "All the women are white, all the Blacks are men . . . (p.178)" This structure arises out of the base-level categories, 'man' and 'woman'.

Although, as Harding observes, "reproductive sex difference itself may not have always been commonsense observational givens [sic] for humans, (p.131)" it certainly has been for many hundred years in European society. Even at the level of characteristic motor activity, children today are required to behave very differently towards women and men. This forces gender upon us as a base-level categorization, and hence only one gender can be the best example of a person. That this gender is men may be more than accidental. Lacanian analysis insists that the very process of human development creates the system of gender, with the male gender metonymically representing the social order. The only thing accidental is that biological males are placed in the male gender.

Whether or not the Lacanian inevitability of casting men as the best example of 'persons' is correct, the effect of doing so is that social scientists study men and believe that they are studying persons in general. Harding quotes Millman and Kanter,

When male sociologists (or men in general) look at a meeting of a board of trustees and see only men, they think they are observing a sexually neutral or sexless world rather than a masculine world.  
(p.90)

The question remains, of course, whether women are any less likely to assume that men are the best example of persons; though probably feminists, either male or female are less likely to do so.

Harding is not always on equally firm ground in accusing science of androcentrism. For example, she says,

The androcentric ideology of contemporary science posits as necessary, and/or as facts, a set of dualisms -- culture vs. nature; rational mind vs. prerational body and irrational emotions and values; objectivity vs. subjectivity; public vs. private -- and then links men and masculinity to the former and women and femininity to the latter in each dichotomy. (p. 136)

We all agree that these dichotomies are posited, and even that they are equated with the dichotomy male vs. female. This does not constitute androcentrism, however; unless one claims that androcentrism is the sole basis of the base-level categories 'male' and 'female'. Women just as much as men, and feminists as well <sup>as</sup> ~~and~~ nonfeminists concretely experience the base-level distinction, male vs. female. Perhaps in an ideal world this distinction would not be made on such a fundamental level; but given that it is made, it provides an obvious experiential basis on which to structure less familiar and more abstract dichotomies.

The male vs. female distinction is not experienced in an identical way by women and men; and it is not the only experiential basis on which these abstract dichotomies could be structured. They could all be, and perhaps should be, structured by any of the following distinctions: up vs. down, friendly vs. hostile, close vs. distant, etc. However, even if we deeply believe that some other distinction would better structure the above dichotomies, or that they should not be made at all -- and even if we believe this qua feminists -- this does not imply that the structure imposed by the distinction male vs. female is androcentric.

Let me mention one more comment which Harding makes, and which requires, in order to defend, a framework of the sort I briefly describe. This framework is only stated in bits and pieces, if at all, by Harding. Harding says,

[I]t is reasonable to believe that the selective focus on purported sexual sameness across species and sexual differences within species is not only questionable but also a distinct consequence of androcentrism. (p.100)

I do not understand why either women or feminists would necessarily have a different focus. The manner in which the focus on sexual sameness across species probably comes about is through the base-level categories, 'female' and 'male'. These are formed by our experience with human beings, but are extended to structure our experiences of other species.

When the distinction man vs. women metaphorically constitutes the structure of the distinction female vs. male, in other species, it is natural to identify men with other males and women with other females. Similarly, the differences within species across sex is a product of the experientially based base-level distinction man vs. woman. The only criticism of the focus mentioned in the above quote which is specifically feminist would be a criticism of the base-level categories 'man' and 'woman'. However, feminists are far from univocal in making such a criticism – many feminists are specifically concerned to "praise the feminine," which certainly involves maintaining a very basic distinction between female and male. Any criticism of the focus on sexual sameness across species which does not criticize the gender category structure will, most likely, fall easily within the methods of normal critical scientific methodology<sup>6</sup>.

I should like to turn aside, briefly, to a tack which I think can serve to ground "a science grounded in women's experience." Women and men, from a very early age, experience the world in different ways. The texture of the objects which

infants handle differs in correlation with the gender of the infant (and I say 'gender' not 'sex' deliberately – infants are gendered in the eyes of society from birth, or before; even before they have a gendered psyche). The manners of handling objects which are reinforced differs thus also; as do the colors with which an infant is characteristically presented; the sounds which she hears (i.e. human voices, which are in one tone to male infants, and in another to female infants); the way she is held and handled by other people; the actions toward other people which are reinforced; et nauseum<sup>7</sup>. Because of this, we should expect that girls and boys have formed significantly different conceptions of what characterizes absolutely basic categories such as, 'object', 'action', 'person', 'cause', etc.

For example, the base-level category 'person' is certainly defined in our cognition, at least in part, as "an entity (or whatever) toward which one (namely, myself) behaves in such-and-such a manner." But the manner specified must be different for those becoming girls and those becoming boys. Similarly, our most basic conception of what it is to be an object is shaped, in part, by the tactile properties of the objects which we first handled, and those which we most often now handle. Who knows if women who had handled rag dolls as infants would have developed the same "*billiard-ball molecular theory*" as did men who had handled wooden blocks as infants.

I may be committing something of an anachronism here, as I do not have any idea what sort of toys European babies of several centuries ago played with; but I think it is clear enough how this example illustrates conceptual differences between women and men. It may be that the basic concepts which are characteristic of women (in our cultures, I hasten to add) would better serve to

structure the process of some or all scientific inquiry. I have no idea what a "rag-doll molecular theory" would be like, but it just might be a better one than is our traditional theory. It would be specious to ask whether the suggestion with which I start this paragraph is a criticism of science's methodology.

Methodology *simply does not enter!*

A more feminine molecular theory would still, of course, have to be tested, examined for implications and coherency, etc.; but what seems to be at issue here is the "context of discovery" not the "context of justification," as we say in philosophy of science. Methodological norms have rarely had anything to do with the context of discovery (except very recently, i.e. in the work of Donald Campbell), and as I shall discuss below, the context of justification is equally free of methodological norms in actual practice.

The final three chapters of Harding's book are concerned to deny the role of methodological norms within the context of justification. These chapters, however, have very little to do with feminism. Harding merely repeats the now familiar claims of the "radical" philosophers of science, notably Kuhn. She wishes that these philosophers had something to say about feminism, but cannot quite get them to. For example, Harding says that we should, in a Kuhnian way,

understand knowledge-seeking as a fully social activity -- one that will inevitably reflect the conscious and unconscious social commitments of inquirers. From this perspective, it cannot be either merely accidental or irrelevant that most social studies of science, like their empiricist-guided ancestors, are loath to consider the effects on science of gendered identities and behaviors, institutionalized gender arrangements, and gender symbolism.  
(p.201)

We can agree that gender identities, et al., constitute part of the social commitments of scientists, past and present. But these commitments are not the only ones which scientists have had, nor are they different in kind from other

commitments. All Harding really does is combine a respect for Kuhn with an unambitious claim: namely, "people are, and have been, gendered."

The final, concluding chapter also seems singularly unambitious. This is representative:

It would be historically premature and delusionary for feminism to arrive at a "master theory," at a "normal science" paradigm with conceptual and methodological assumptions that we all think we accept. Feminist analytical categories should be unstable at this moment in history. We need to learn how to see our goal for the present moment as a kind of illuminating "riffing" between and over the beats of the various patriarchal theories and our own transformations of them, rather than as a revision of the rhythms of any particular one (Marxism, psychoanalysis, empiricism, hermeneutics, postmodernism . . .) to fit what we think at the moment we want to say (p.244)

What then are feminists to do? Harding confines them to the margins of scientific practice; at best nipping at the heels of scientists when they are furthest misled by androcentrism. For feminism to bring about, or even suggest, any substantial improvement in science, it must suggest a new scientific practice -- a positive, concrete way for scientists to carry out science. Harding eschews doing so.

## Footnotes:

1. See, for example, Margaret Rossiter 1982, Women Scientists in America: Struggles and Strategies to 1940. Baltimore, MD: Johns Hopkins University Press.

2. See Adrienne Zihlman 1978, "Women in Evolution, Part II: Subsistence and Social Organization among Early Hominoids," in Signs: Journal of Women in Culture and Society. University of Chicago Press, v.4 no.1; Nancy Tanner and Adrienne Zihlman 1976 "Women in Evolution, Part I: Innovation and Selection in Human Origin," Signs v.1 no.3; Sarah Blaffer Hrdy 1981, The Woman That Never Evolved, Cambridge, MA: Harvard University Press.

3. To wit: Nancy Chodorow 1978, The Reproduction of Mothering, University of California Press; Dorothy Dinnerstein 1976, The Mermaid and the Minotaur: Sexual Arrangements and Human Malaise, New York: Harper and Row; Jane Flax 1978, "The Conflict between Nurturance and Autonomy in Mother-Daughter Relationships and within Feminism," in Feminist Studies v.4 no.2.

4. See, for example, Eleanor Rosch 1981, "Prototype Classification and Logical Classification: The Two Systems," in New Trends in Cognitive Representation: Challenges to Piaget's Theory, ed. E. Scholnick, Hillsdale, NJ: Lawrence Erlbaum Associates; Eleanor Rosch 1978, "Principle of Categorization," in Cognition and Categorization, ed. Rosch and Lloyd, Hillsdale, NJ: Lawrence Erlbaum Associates.

5. We say, for example that one has HIGH or LOW moral stature; that one is feeling UP or DOWN; that the future is AHEAD of us and the past BEHIND us; that a relationship is moving FORWARD or slipping BACKWARD; etc.

6. Such a focus will, after all, eventually produce testable hypotheses about the actual gender differences in different species.

7. Look at, for example, Parsons, Talcott and Robert F. Bales 1955, Family, Socialization and Interaction Process, The Free Press, Glencoe IL; or any intermediate sociology text covering gendering.

## Additional Texts

George Lakoff 1987, Women Fire, and Dangerous Things, University of Chicago Press.

George Lakoff and Mark Johnson 1980, Metaphors We Live By, University of Chicago Press.