

New Directions, Really?

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1. Introduction

I want to discuss Sandra Harding's claims to the effect that issues concerning race and gender lead, or should lead philosophy of science in new directions. In particular I wish to concentrate on her attempt to extend her previous feminist critique of science to include concerns about racism as well. That attempt is best found in her recent book *Whose Science? Whose Knowledge?* (1991, page numbers without further attribution will refer to this book) and in her contribution to this symposium.

I have chosen to concentrate on Harding's views because she is considered as the leading feminist philosopher of science by many observers and because her feminist critique of science is meant to apply to all of the sciences, including the physical sciences (as is her critique on behalf of indigenous people everywhere). Being thus presumably representative of the proposed "new directions", her views will be particularly useful to explore the subject of this symposium.

Until now philosophy of science has not considered either gender or race as truly significant factors in our philosophical understanding of physical science. After examining Harding's work I find little on which to base a new appeal to the discipline.

To be fair, there is some apparent initial plausibility to Harding's quest. Attitudes about race and gender are social values, and social values, according to much work of the last thirty years, can become assumptions in the construction and justification of scientific theories. Science can be seen as embedded in a social context, and thus the understanding of science requires the study of science in that context. Moreover, there are examples, particularly in the social sciences and in biology, in which it is not difficult to see that prevailing racist and sexist notions influenced the content of many influential views, that is, of presumably scientific views.

Lest we commit a serious mistake in reasoning, however, we must realize that even though social values, perhaps even race and gender, may influence the content of some science, we may not automatically conclude that attitudes about race and gender do so influence all the sciences. In particular, evidence that biology has been influenced is thus not evidence that physics and astronomy have been too. Moreover, that something may be the case does not show that it is the case. To show that it is the

case may demand carrying out a very large task, a task that would require the participation of philosophy, history, sociology, and many other disciplines, and that would also require a decent understanding of the particular sciences under investigation. Such a task may indeed constitute a new, interdisciplinary direction for research in philosophy of science.

Nevertheless the discipline as a whole does not seem to believe that this is a legitimate task for practitioners of the field to undertake. I presume that this statement describes accurately the attitude in our discipline, or at least the perception of the discipline by those whose main interests can be found in race and gender issues.

There could be several motives for such an attitude. It may be that upon reflection we find no good reason to suppose that attitudes about race and gender have anything significant to do with the development of sciences like physics and astronomy; or that the practical effect of any such influence would not be worth bothering about. Or, alternatively, philosophers of science may believe that the standards of the proposed sub-field (race and gender) are so low that we are not inclined to give the matter a second thought.

A compelling case by Harding (or some other "new directions") may constitute enough of an achievement to change the situation. But I am afraid that my report will not be very encouraging.

2. Social Values and the Content of Physical Science.

Harding offers some general considerations for supposing that race and gender have an effect upon the nature of science. These considerations are based on critiques that she says can "bring to the surface of feminist science discussions the class, race, and imperialist projects of the West in which the sciences and their technologies are implicated." (p.36) But those critiques turn out to be claims such as "science is a social problem because the society that shapes it is a social problem." (p.36) Even assuming that all societies in which science is practiced are social problems, or that Western society in particular is a social problem, this critique does not seem to amount to much. Society, Western society, also shapes music, movies, food, and even feminism. It would be hard to imagine a human activity practiced in the West that would thus not be "a social problem."

What we do not get from Harding is any hint of how society shapes physics so that, say, today the weight of scientific opinion is thrown in support of "cosmic censorship", or against it.

In any event such critiques should do little to alter most philosophers' skepticism concerning the lack of significance of racism and sexism for their understanding of physical science. This skepticism is buttressed by some obvious considerations from the history of science.

Consider the thesis that because a society is racist, racism must infect the physics of that society, and not just its hiring practices but its actual content (which is the more interesting philosophical thesis). It is well known that in the first part of this century European society was extremely anti-semitic. During the years 1905-1920 anti-semitism ran wild in Europe, we might say, therefore we should expect European physics between 1905 and 1920 to reflect that anti-semitism clearly. But of course that is nonsense. 1905-1920 shows the emergence and triumph of Einstein's theories of Special and General Relativity, as well as the influence of his work on the photo-electric effect on the initial development of quantum physics. The work of that genius, a Jew, can hardly be considered anti-semitic.

Nevertheless, perhaps we should finesse the thesis a bit: it is the social values of the practitioners that influence the content of science. I suspect that if we were to accept this version of the thesis we would then have to give up the strong connection Harding seeks to show between racism and science - but let that pass. Should we then seek to find clear signs of Jewishness in the dominant physics of 1905-1920? The Nazis did claim that Relativity was "Jewish physics," but as far as I know they never did much to justify their claims.

As we can see, even if some social values and attitudes prove significant for our understanding of physical science, it is yet to be shown that racism and sexism are amongst them. Given the rather well grounded skepticism on the subject, it is thus difficult to see how the proposed "new directions" are going to get a toe in the door.

3. Harding's "Strong Objectivity"

Perhaps we should examine now what Harding herself considers to be the main *new* philosophical direction in which considerations of race and gender take our understanding of the nature of science. This "new" direction, presumably, leads us to what she calls "strong objectivity." The case she offers has a theoretical and an empirical aspect. I will consider them in turn.

The theoretical aspect is the following. Harding points out that science is limited. The views held by scientists make assumptions, and it is difficult to discover those assumptions when they are shared by the whole group. Thus in order to discover and criticize those assumptions we need to examine the group's views from the outside.

As far as this goes, it all is very sensible but not very original. Indeed it sounds a great deal like Feyerabend's case for pluralism (1975), which is itself an extension to the philosophy of science of J.S. Mill's epistemic arguments in *On Liberty*. Feyerabend backs up his philosophical analysis with examples from the history of science. For example, he points out numerous instances in which scientific "progress" resulted from looking at important problems from the perspective of views that had long been considered defeated, even worthless (heliocentrism, atomism, etc.).

Harding has narrower concerns, though: "starting thought from women's lives" (and from the lives of indigenous peoples) "increases the objectivity of the results of research by bringing scientific observation and the perception of the need for explanation to bear on assumptions and practices that appear natural or unremarkable from the perspective of the lives of men in the dominant groups." (p. 150). This approach results in a "strong objectivity" that offers a rational compromise between "objectivism" (i.e., the "judgmental absolutism characteristic of Eurocentrism") and "judgmental relativism" (i.e., the view that each person's "judgment about the regularities of nature and their underlying causal tendencies must be regarded as equally valid." (p. 139)) As she sees it, pluralism as well as standard versions of conceptual relativism are unable to keep this "judgmental relativism" at bay. This plays into the hands of the dominant groups, who benefit from objectivist justifications of science, (p. 143) for from "the perspective of objectivism, judgmental relativism appears to be the only alternative." (p. 139)

Harding's "strong objectivity" goes beyond pluralism apparently in that in enacting it we come "to value the Other's perspective and to pass over in thought into the social condition that creates it - not in order to stay there, to 'go native' or merge the self with the Other, but in order to look back at the self in all its cultural particularity from a more distant, critical, objectifying location." (p. 151) Her scenario is not one of mere pluralistic respect between different points of view, but rather one in which "the countercultures

can envision and even occasionally already enact: the fundamental tendencies of each must permeate each of the others in order for each movement to succeed." (p. 156)

An important achievement of "strong objectivity" is, presumably, that it may resolve the problem of reflexivity that dogs social accounts of scientific knowledge, and particularly the "strong programme" of sociology of knowledge. There is a sense in that the sociologist undermines the objective authority of natural science by tracing the social origins not only of the "worst" but of the "best" scientific beliefs. The sociologist's account of science, however, should also be expected to have social origins and thus it is similarly undermined. How does Harding resolve this problem? By making the sociologist unearth his own assumptions, i.e., his own "cultural values and interests." This is achieved by "a strong notion of reflexivity" which requires that "objects of inquiry be conceptualized as gazing back in all their cultural particularity and that the researcher...stand behind them, gazing back at his own socially situated project." (p. 163) The reason is, of course, that many of the relevant social causes of his own beliefs can be seen "only from locations far away from the scientist's actual daily work. 'Strong reflexivity' requires the development of oppositional theory from the perspective of the lives of those Others." (p.163)

Much of Harding's position should be found initially plausible by a good many philosophers of science. But I am afraid that is precisely because many of us have made similar points about the value of pluralism. Many of these defenses of pluralism stress heavily the value inherent in the challenge from "oppositional theory" - Feyerabend (1975) and Lakatos' "methodology of research programmes" (1970) to name just two of them. In Feyerabend (1975, 1987) one even finds many of the points that Harding wants to make about the contributions, past and potential, of Third World cultures to Western science. If the objective of "gazing back" from an "indigenous culture" is that we may discover assumptions about our own, the point has already been made better by Feyerabend and others. I will attend to her claim of having resolved the problem of reflexivity shortly.

Harding's suggestion of a new direction here is not only questionable, it is also motivated by a false dilemma. She describes the field as facing a choice between narrow objectivism and vulgar relativism. But especially in the last thirty years we have seen a great deal of serious work that avoids the extremes. Although Harding equates absolutism with an objectivism based on the value-neutrality thesis, it is clear that there are many kinds of absolutists. And some of them think highly of conceptual relativity and pluralism. Clifford Hooker (1987), for example, believes that there is such a thing as the structure of the universe or the truth about the universe. That would make him a believer in an absolute truth (i.e., an absolutist or objectivist). But he also believes that we are nowhere near that truth at the present, and that conceptual relativity and pluralism are excellent strategies to help us move in the direction of that truth. A century before him, J.S. Mill held that at any one time we are at best in possession of only part of the truth. Thus by considering alternatives seriously we may either replace some of our error with truth or come to understand our portion of truth better. Or else we may also replace the present, partial truth with another, which though still partial, may be better adapted to the needs of the time.

Relativists also shy away from the notion that all views are equally valid. Close attention will reveal that such is the case in Feyerabend's practical relativism (1987, 1991a, 1991b). And, if I may be so bold, it is clearly the case with my own evolutionary relativism or my theory of relative truth (1981, 1991).

In addition to these and several other objectivists and relativists there are many other philosophers who do not buy the dilemma. Take, for example, internal realists such as Toumela (1985) and Putnam (1987), who also calls his view "pragmatic realism."

Apart from her failure to provide a truly new direction, it is by no means clear that Harding solves, or resolves, in any way the problem of reflexivity with her "strong reflexivity." Let us take a sociologist of molecular biology who has "discovered" the social origins of certain entrenched beliefs in our understanding of the cell. He has immersed himself in molecular and cell biology in their "full particularity" and now "gazes back" from that standpoint to his own position. Perhaps he can now unearth the sociological (or other) assumptions on which his "discovery" is based. It would seem that this particular objective would be better served if he "gazed" at his own position from the vantage point of a different sociological theory, but let us say nonetheless that by "gazing back" from molecular biology he manages to unearth his own assumptions, to understand his own position better, etc. All this may be very be valuable (it is one of the most appealing features of scientific pluralism) but it does not solve the problem of reflexivity at all. Unearthing one's assumptions does not justify them, does not make them "objective" in any way. Profiting from the experience, in whatever fashion, does not change this result in the slightest. On the contrary, if discovering the social origins of beliefs undermines their authority, a successful "gazing back" would only make more explicit the sociologist's problem of reflexivity.

I personally think that the problem of reflexivity is much ado about little, that conceptual relativists such as Feyerabend and the early Kuhn do not suffer from it, and that it can be handled within my own theory of relative truth. But a discussion of these issues would distract too much from the task at hand.

Another problem with Harding's "strong reflexivity" is that by "gazing back" from another's point of view, even from the Other's, one can only hope to unearth **some** assumptions, to discover **some** origins of beliefs, etc. This limitation would make it impossible for the sociologist of science to comply with Harding's directive to gaze back "at his own socially situated project in all its cultural particularity and its relationships to other projects of his culture." (p. 163, my emphasis) Gazing back from some Other standpoint may leave untouched some aspects of the cultural particularity of the researcher in question. To achieve what truly strong objectivity and reflexivity demand, it seems that the researcher will not only have to look at his own project from many other standpoints (in fact, "in all its cultural particularity" may demand gazing back from an infinity of standpoints). Furthermore, it could be argued that the results of gazing back from some Other standpoint need to be gazed back at from **another** standpoint. Unless we could claim that those results are themselves not socially situated.

The directive to "gaze back" from just **one**, but completely different culture, was a tall order to begin with. Even leaving aside issues concerning translation, incommensurability, and the like, coming to understand another culture as thoroughly as Harding demands, in its full particularity, seems beyond the reach of most of us. In other pluralistic views there was generally a division of labor within a discipline - an interaction between two competing research programs, for example. I find Harding's optimism in this regard interesting, although for other than epistemic reasons.

Let me explain. The grounds of such optimism appear to be her belief that "When one examines the social meanings of science and technology they reveal a curious coincidence between masculinism and Euro-centrism." (p. 245) The coincidence must be striking, for "Many features that North Atlantic feminists attribute to stereotypes and ideologies of masculinity appear also as the target of African and other Third World critiques of Eurocentrism." (p. 245) Convergence of ideas is often taken by some writers as a good indication that they are on the right path, when not as outright evidence. Harding seems to find reason for feeling heartened by this convergence between Feminist and Third World "indigenous" critical writings on science. She asks, "Why are there such similarities between the distinguishing characteristics of femininity and

Africanity?" Her "complex" answer is that "the conceptual frameworks of sexual and racial stereotypes are mutually constructing."

This position falls prey to several internal and external objections. I will consider one of each below; but before I do so I would like to point out that the convergence that Harding sees as reinforcing her attempt to connect gender and race could be explained in a simpler way. Those critical accounts of which she so much approves are for the most part written by "standpoint" or other neo-Marxist theorists. Whatever the independent merits of their views, it is not surprising that those views converge on issues about which they make similar assumptions and have similar attitudes, and which they tackle with similar objectives in mind, in accordance with similar guidelines and rhetoric.

My first, and internal, criticism of Harding's optimism is this. She is pleased by the convergence of feminist and indigenous views about Euro-centrist science. But those are not true indigenous views. They are neo-Marxist views, that is, ultimately, views of the Third World through the eyes of a 19th Century German philosopher. That is, through the eyes of another Dead White Male.

We are not confronted with criticisms of "Western" science from the point of view of West-African science of the 12th Century, nor from that of a Venezuelan "llanero" of the early 20th Century. The neo-Marxist accounts appeal to her because they accord with her own "civilized" preferences. She is of course entitled to her preferences. But it would be downright presumptuous of her to imagine, let alone require, that representative "indigenous" accounts should be expressed in the language of neo-Marxist Enlightenment. Indeed, unless Marxism is a Kantian category of non-Western people, the mere fact that those accounts are expressed in neo-Marxist language should already be a strong indication that they are *not* indigenous.

So much for taking the Other seriously. This situation is aggravated by the short shrift she and other U.S. neo-Marxist thinkers give to actual Third World societies. Let me illustrate this external criticism by speaking about their comparison of North and South America. North Americans are heirs of Europe and therefore oppressors. By contrast, South Americans are not heirs of Europe which makes them, therefore, victims of oppression.

Consider, however, the following:

- (1) both cultures were founded by European cultures.
- (2) South Americans are generally better educated about European culture because we learn it in high school and our high schools are superior to those in the U.S. When I was an adolescent I had to take classes in European geography, history, literature, and philosophy (two years of each), to say nothing of European science (two years of physics, two of chemistry, etc.). Of course, our culture, or cultures, is not strictly European. We have introduced many variations. But the North American cultures are not strictly European either, as Europeans are always very quick to point out.
- (3) The only salient difference, other than social wealth, seems to be that the population of South America is statistically *darker skinned*.

Should this difference in skin color suffice for the U.S. radicals to assume automatically that South Americans are not heirs of European culture whereas *they* are (though they may be ashamed of it)?

Isn't this attitude not just presumptuous and arrogant but downright *racist*? What right do they have to dispossess an entire continent of an important part of its cultural

heritage? Should not South Americans themselves be allowed to have a say on the matter?

Isn't it also condescending and racist of them to decide for us what our values and political attitudes should be, let alone what doctrinaire format we should follow to express our thoughts about ourselves, purely on the basis of our (statistically darker) skin color?

Judging from the theoretical aspects of Harding's case on behalf of race and gender issues, I see little to recommend it as a new direction for philosophy of science. Harding's case, however, tries to draw some empirical support from the work of a few writers who argue that the Third World has had its share of scientific accomplishments, some of which influenced the development of Western science. I for one grant that non-Western cultures have accomplished many things worthy of admiration. Nevertheless I am afraid that Harding's use of history does not serve her case as well as she might have hoped. Constraints of space prevent me from treating the matter in great detail, but I will bring up two considerations.

The first is that I imagine that the overwhelming majority of philosophers of science will probably grant the point without much prompting. I remember that as part of my graduate training I read Neugebauer's *The Exact Sciences in Antiquity* (1952), and that Neugebauer delighted in pointing out how so many of the scientific accomplishments commonly attributed to the Greeks, particularly in mathematics, actually were achieved much earlier by the Babylonians, the Egyptians, and other cultures. I am not sure how a historical task, the conclusion of which is a point already granted by most philosophers of science, will serve as the start for a new direction to the discipline.

The second consideration is that some of the examples borrowed by Harding are, to be honest, quite embarrassing. She claims, for instance, that in West Africa between 1200 and 1400 the Dogon "*knew* that a small star, *invisible to the naked eye*, had an *elliptical orbit* around the star Sirius that took fifty years to complete." (My emphasis, p. 223) Some of us may still remember that this was one of the examples used by Von Daniken, of *Chariots of the Gods* fame, to "prove" that extraterrestrials had visited the Earth. We may also remember the job *Nova* did on this item. Now Harding uses this "fact of history" to show that the Dogon had a science worthy of respect, apparently not realizing that if this story is true the Dogon had a science not just comparable to the Western science of the period, but actually vastly superior even to the physical science of Newton's time, several centuries later. Surely a claim this extraordinary deserves far more historical analysis and support than its naked assertion (and a reference), which is all Harding provides.

3. Standards of Scholarship.

This brings me to discuss the second motive why the discipline does not seem to share Harding's sense of the significance of race and gender issues. As I mentioned earlier, there seem to be two main motivations to which one may appeal when trying to lead a discipline in new directions (I do not deny that the vagaries of fashion, or the arbitrariness of political pressure may play a part, even a large one, but then I am only considering here what I take to be an honest appeal to reasonableness). The first motivation concerns the presence of a perceived extraordinary, or at least compelling achievement in a new area. I do not believe that Harding's theoretical case can be so characterized. The second motivation has to do with high standards of scholarship. Or to be more precise, with the fact that work of high quality is being produced by using the new approaches, work that inspires, or should inspire scholars to try their hands at the proposed new approaches. On the other hand, when the work produced by using the new approaches is very sloppy, there is an understandable hesitancy to follow suit.

Of course, sloppiness may well be in the eye of the beholder, and it may happen that when an observer makes such a charge the fault lies instead with his failure to understand the new approaches. Nevertheless my impression is that much of the work along the lines of these “new directions” is severely handicapped by a pervasive carelessness. In this respect Harding’s book is rather typical, as I have gathered from reading the main works recommended by the feminist philosophers of science themselves. But let me give some examples and let the audience decide.

In complaining about androcentric biases in science, Harding writes:

... it was only twenty years ago that James Watson could devalue and ridicule in print - and with a macho hubris that signaled expected approval - the work of Rosalind Franklin in the discovery of the structure of DNA. It was Franklin’s work as well as theirs that permitted Watson and Sir Francis Crick to win a Nobel Prize. Why was it not also awarded to her? (p. 24)

Watson’s book, *The Double Helix* (1968), describes how it felt to be a young, smart-alecky James Watson in the pursuit of the structure of DNA. Part of the appeal of the book lies in Watson’s realization, as he and his research matured, that he has treated several people unfairly, particularly in the case of Rosalind Franklin, as he points out in several passages. A few lines are in order:

In 1958, Rosalind Franklin died at the early age of thirty-seven. Since my initial impressions of her, both scientific and personal (as recorded in the early pages of this book), were often wrong, I want to say something here about her achievements. The X-ray work she did at King’s is increasingly regarded as superb. The sorting out of the A and B forms, by itself, would have made her reputation; even better was her 1952 demonstration, using Patterson superposition methods, that the phosphate groups must be on the outside of the DNA molecule. (1968, p. 132)

After describing with great admiration some of her other important work - earlier he had called it “first-rate science” (1968, p. 124, p. 126) - he ends his book like this:

... both [Crick and Watson] came to appreciate greatly her personal honesty and generosity, realizing years too late the struggles that the intelligent woman faces to be accepted by a scientific world which often regards women as mere diversions from serious thinking. Rosalind’s exemplary courage and integrity were apparent to all when, knowing she was mortally ill, she did not complain but continued working on a high level until a few weeks before her death. (1968, p. 133)

As for why she was not awarded the Nobel Prize, the explanation does not require appeal to “androcentric biases.” Watson and Crick were awarded the Nobel Prize in 1962. Franklin died in 1958. The Nobel Prize is not awarded posthumously.

As many other feminist philosophers of science, Harding makes use of psychoanalytical “studies”. According to one such study (p. 28) the reason why boys and men want to enter and remain in science is that they have been encouraged by parents and society to develop a facility in abstract thought in order to become more manly. Facility in abstract thought is thus not only a sign of manliness in the West but also a highly valued trait for a career in science. Girls and women, of course, are encouraged otherwise.

Now, I grew up as a boy and eventually became a man. I have lived in several Western societies and visited many more. But I have never seen, or even heard of a society in which facility for abstract thought was generally considered a sign of manli-

ness. If anything manliness is achieved in spite, not because of such facility. Boys who are so intellectually adept tend to be considered nerds, wimps, sissies. When it comes to manliness, facility for abstract thought is barely a notch above taking ballet lessons.

Similarly unreliable claims pepper the feminist accounts of science that depend heavily on psychoanalysis or its offshoots. Whatever the merits of psychoanalysis, deconstructionism, or some of the other common tools of feminist literature on science, it must be realized that many philosophers of science think very poorly of those disciplines, *on independent grounds*. Already burdened in this fashion, the rhetorical task of the “new directions” is made even more difficult by the persistent failure to be on top of the material.

On p. 39, Harding says, “*Contrary to Darwinian and other interpretations of evolutionary theory, females too have evolved.*” (My emphasis) How can one read the *Origin of Species* and say this? Darwinism would make no sense at all. She makes even more careless statements about Newtonian mechanics (pp. 84–85), and declares “constitutive of science” the deterministic idea that “all physical events and processes have causes even if we can’t always know what they are,” (p. 86) as if quantum physics did not exist. Arguing for the influence of society on science, she asserts (p. 80) that critics have detected in the theory of relativity (discovered in Switzerland in 1905) the social values exhibited in the Weimar Republic in Germany from 1918 to 1927.

We all make mistakes in print. I am sure I have made my fair share. But few of us in the field approach this prodigious rate (I have only given a small sample). The situation is not much better when the topic is philosophy of science itself. For example, Harding seems unaware of the large literature on pluralism I mentioned earlier, at least she makes no reference to it. Her characterizations of philosophers’ views are not any more felicitous than her examples from science. A Popperian, for example, should find very strange Harding’s account of objectivism. And so on.

4. Conclusion

In addition to indicating the reasons for my skepticism concerning the invitation to take these “new directions”, the purpose of pointing out these problems is twofold. First it explains, in large measure, the reason why issues of gender and race are not considered very significant to the subject matter of our discipline. Second, it provides an honest rejoinder to the charge often made by some feminists that philosophers of science make derisive remarks about their feminist scholarship, not because their scholarship is deficient, but because of lingering sexist prejudices. The rejoinder is that, at least in some significant instances, their scholarship is very deficient.

I do not wish to suggest that all the work in feminist philosophy of science is of poor quality. I do suggest that Harding’s work, which has been much praised and recommended by feminist thinkers, does not set an example that the discipline should follow, and that her connections between feminist and Third World concerns are particularly distressing. I trust it is clear that my observations can only be generalized to the extent that Harding’s work turns out to be typical, and then only to feminist work on physical science or on the nature of science in general. I have presented no objection to the view that in sciences dealing with humans, or with behavior, unfair stereotypes may become part of “commonsensical” beliefs. The biases in the subject-matter of the physical sciences seem to be of a different kind.

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