On Values in Science: Is the Epistemic/Non-Epistemic Distinction Useful?

Phyllis Rooney

Oakland University

1. Introduction

The debate about values *in* science in the last decade or so reflects an important shift in what many acknowledge as the "post-positivist" era in philosophy of science. It reflects the erosion of the fact-value distinction in at least one of its more simplistic forms (facts belong in science, values outside), and it marks the path to a more enhanced understanding of the roles of both facts and values in scientific inquiry. This discussion has, above all, contributed to what we might call a revaluation of value: from the point of view of the epistemologist or philosopher of science values are neither uniform nor uniformly "bad". Values are acknowledged in their variety of form and function, and some are seen to play a necessary role in the rational and cognitive development of scientific knowledge: these have been called alternatively *epistemic* values (McMullin 1983), *cognitive* values (Laudan 1984), *constitutive* values (Longino 1990).

However, as these terms suggest, a relatively firm distinction is still endorsed between epistemic and non-epistemic values (or constitutive and contextual values—Longino), with social, cultural, and personal values grouped together on the non-epistemic side. A central focus in the recent debate is whether, and if so how, non-epistemic values influence the "internal" development of science. I will argue that the development of an understanding of science in social context is impeded by this distinction as it is often deployed, that a more effective analysis must include a more explicit examination of the usefulness of the distinction itself. My approach is twofold. My more direct criticism includes an analysis of the lack of clarity or agreement about how the set of epistemic values is constituted, even among those who make substantial use of the distinction. A more indirect approach is motivated by an effort to understand how the feminist work in philosophy of science might best be developed within an account of the role of values in science. Here it is generally understood that a central concern is with the role of "androcentric values" in the development of science. While this focus has been effective in places, it should not be seen to circumscribe the extent of the feminist contribution to the debate. I will argue that much of the feminist work also encourages a more direct reevaluation of the epistemic/non-epistemic distinction itself.

PSA 1992, Volume 1, pp. 13-22

2. Epistemic Values: Fuzzy Sets of Fuzzy Criteria

Epistemic values are those that are usually taken as constitutive of the knowledge- and truth-seeking goals of the enterprise of science. Accuracy and consistency, for example, might be taken as the most obvious and least contestable of these values. However, when attempts are made to make these more precise (and especially epistemic values like fruitfulness and simplicity), various problems arise, not least among which is the issue about what constitutes scientific truth and knowledge in the first place.

One is immediately alerted to a problem here when one notices that among those who grant epistemological or philosophical weight to such a set of epistemic criteria there isn't anything resembling convergent agreement on what exactly is in that set. Thomas Kuhn lists his "standard criteria for evaluating the adequacy of a theory" as: accuracy, consistency, scope, simplicity, and fruitfulness (1977, p.322). Ernan McMullin "reworks [Kuhn's] list just a little" and lists his criteria in order: predictive accuracy, internal coherence, external consistency, unifying power, fertility, and then lists simplicity as "one other more problematic candidate" (1983, p.15-16). Helen Longino lists the "governing values and constraints [that] are generated from an understanding of what counts as a good explanation" as: truth, accuracy, simplicity, predictability, and breadth (1990, p.4). One notices immediately that the lists are not exactly identical. Kuhn's "scope" is presumably something like Longino's "breadth" and perhaps something like McMullin's "unifying power" though their subsequent discussions do not shed much light on the matter. It is interesting to note that Longino counts "truth" as a constitutive value whereas McMullin does not, and elsewhere he states that he calls his characteristic values "epistemic" because they are presumed to promote the truth-like character of science, "its character as the most secure knowledge available to us of the world we seek to understand" (p.18). In other words, McMullin takes truth to be something like a second-order value ("a sort of horizon-concept or ideal") determining in part what is to count as a first-order epistemic value. This slight difference touches upon another significant debate about whether to take truth as a primitive or derivative notion in developing our understanding of science. What this suggests is that we cannot naively assume that in positing the seemingly straightforward epistemic/non-epistemic distinction we automatically sidestep some of these difficult problems that already have a long history.

The fact that there is no clear consensus about what is included among the epistemic or constitutive values does not overly concern many of those who make the distinction. Kuhn readily admits that he selects five characteristics "not because they are exhaustive, but because they are individually important and, collectively, sufficiently varied to indicate what is at stake" (p. 321). (What exactly is it that is at stake?) Though McMullin makes a point about reworking Kuhn's list "just a little" he doesn't explain why that was necessary. Perhaps he thought that his terms were more descriptive of the values or characteristics in question, yet even then the lists do not match up. He does admit that the decision about whether a value is epistemic or non-epistemic in a particular context can sometimes be a difficult one to make, though he thereby implies that the decision can in theory, in time, be made. Longino prefaces her list with a "for example", indicating presumably that she is not presenting it as a comprehensive or exhaustive list.

One of the main reasons for the lack of strictness here is that these theorists use the distinction in large part to offset or demarcate the non-epistemic values, the personal, social and cultural values that have garnered special interest in debates about the social dimensions of science. A central question is whether or to what extent these latter values can be seen to operate in science. If it can be shown that such values play some

role in the cognitive development of science, in the development of scientific rationality itself, then we need to reconceptualize the objectivity and rationality of science in a way that reflects this and thus differs from traditional articulations in terms of "value-freedom". I argue in this paper that an analysis of the operation of the social within science warrants closer examination of the "epistemic" values themselves, and that this examination is to proceed in a way that undermines the usefulness of the epistemic/non-epistemic distinction itself. The fact that there is no consensus about what exactly the epistemic values are surely provides our first clue here. We haven't seen anything resembling a clear demarcation of epistemic values because there is none to be had.

For a start, it is not at all clear that epistemic or cognitive values exert the kinds of epistemic or consensus-forming force among scientists that philosophers sometimes project. An argument by Larry Laudan has a bearing on this point. He argues that there need be no agreement about basic cognitive values for scientists to agree about methodological matters and a broad range of factual claims, that "axiological differences can coexist with factual-level and methodological agreement" (1984, p.45).[1] On the other hand, McMullin stresses that the skills of (epistemic) evaluation are typically learned through practice in the scientist's training and experience and this has a lot to do with consensus-formation regarding proper methodology around data acquisition and evaluation. One must be careful then with the kinds of claims one makes using a methodology/axiology distinction. It is with this emphasis on the communal-practice acquisition of skills in the development of scientists' *value-judgments* that McMullin's explicit use of the term "value" (in citing epistemic criteria like accuracy and simplicity) becomes apparent. He notes particularly that the operation or "application" of these criteria is much more like a value-judgment than the application of an algorithm. (In general, as we know, they cannot function as algorithms—in only very limited contexts might there be an algorithmic test for consistency, for example. This is, of course, one of the many things we have learned from positivism.) Value-judgment enters here in two ways, evaluation and valuing. It is possible for different scientists to *evaluate* the fertility of a particular theory differently, for example, and different scientists might weigh or *value* these different epistemic criteria differently. We can now cite many examples from social and historical studies of science where a specific theory scored well in terms of predictive accuracy, for instance, yet a competing one scored better in terms of simplicity and fruitfulness, and where their respective proponents differed in how they weighed or valued the relevant epistemic criteria. Though, as will soon become apparent, we would generally do well not to lay the differences to rest there.

The two "openings" here that encourage naming these criteria "values" leave a lot of room for the operation of what are termed "non-epistemic values", and this works in a way that surely fundamentally challenges the distinction itself. Given that epistemic values do not—more exactly cannot—function as algorithms (even among those who might be said to agree in valuing a particular criterion highly) there is no set procedure that dictates when and how such a value is applied. The crucial factor here is not that a particular criterion operates but *when* and *how* it does and, I argue, non-epistemic factors are encoded within the *when* and the *how*, not, as is regularly supposed, in some cognitive gap that is left over after accepted epistemic criteria and rules of inference are applied to the evidentiary base. That there is such a cognitive "gap" somewhere is generally taken to be established by the underdetermination thesis. Where and how this gap is filled in is what is centrally at issue here, and it has non-trivial implications for the way in which we articulate the rationality and objectivity of science and for the role that the epistemic/non-epistemic distinction is seen to play in those articulations.

This point can be illustrated with the well-known example concerning the disagreement between Bohr and Einstein about the acceptability of quantum theory. McMullin

notes that the predictive successes of the theory counted much more heavily with Bohr than with Einstein who was in turn concerned that the theory did not rate well in coherence and in consistency with the rest of physics. They thus had, in McMullin's estimation, very different views as to what constituted a "good" theory. He adds, however, that disagreement in "substantive metaphysical beliefs" about the nature of the world "also played a part" (1983, p.17). But surely this is the crucial link that needs to be filled out. How did differences in metaphysical belief inform the way each understood the simplicity or coherence of the theory or even the significance of predictive success? Simplicity is a complex multi-faceted characteristic and all sorts of "non-epistemic" factors can determine how a particular theory is perceived as simple for a particular scientist in a particular context. It is surely not simply a matter of their both apprehending the same simplicity or coherence in the theory but valuing it differently... and adding that *in addition* there were other non-epistemic factors to take into account.

Metaphysical beliefs can, McMullin claims, function as "non-standard epistemic values" in some contexts. This is perhaps the most interesting of McMullin's axiological groupings and one that substantially undermines his epistemic/non-epistemic division. These non-standard epistemic values are typically embedded in a philosophical or theological worldview: he concedes, for example, that theology functioned for Newton as an epistemic factor, as "a set of reasons that Newton thought were truth-bearing" (pp.19-20). Elsewhere he argues that in order to understand Descartes' science it is necessary to grasp the importance for him of his epistemological principles concerning "clear and distinct ideas". The Bohr-Einstein disagreement was rooted in a "deep metaphysical divergence" about whether the universe displayed a fundamental coherence and order (1984, pp.130-31). The sense of "metaphysics" deployed here is of course broader than that relating to the fundamental nature of physical matter—it is a term that acquires wider philosophical and theological import. However, McMullin stresses the *epistemic* role of these factors and seeks to distance himself from the sociologists of science at this point, at least insofar as he sees them as simply providing a socio-psychological analysis of such factors. The debate here in many cases doesn't revolve around when and where these "non-standard" factors operate, but around the different epistemological / sociological / psychological theories one constructs around their operation. We do well to remember these "theories" are just that, and are thus themselves the site of contestation concerning explanatory power. One consequence of this debate is the realization that the epistemological-sociological border is continually being redefined (though McMullin might want to say "refined").

With this set of "non-standard epistemic values" hasn't McMullin undermined the force of his epistemic/non-epistemic distinction? Here are values that have a specific cultural-historical location yet are providing clear cognitive value in the form of metaphysical or epistemological background principles. He thinks that in time, however, the progressive march toward conformity between theory and world will "sift out" any non-epistemic factors that may have been instrumental in original theory-formation. He thus invokes something of a context of discovery/ context of justification distinction in supposing that as science progresses, in the "slow process of shaping our thought to the world" in the continued application of epistemic value-judgment, the non-epistemic (which may have served a necessary epistemic function at one time) will gradually sift out from the epistemic. Yet, if some social and cultural factors get grafted into epistemic value-judgment—for example, get normalized into the language of science itself or get embedded into the structures and models therein—how can the continued application of epistemic value-judgment be expected to yield such a sifting? Given McMullin's earlier admission that to the extent that scientific observation is theory-dependent it is also indirectly value-impregnated (p. 14), it is not clear that one can readily appeal to some neutral value-free court of future observation and

experiment that will in time provide the "right" distillation. As he well admits, the "first" epistemic value of predictive accuracy itself involves value-judgment. This is not to deny that all sorts of things are continually being sifted in and out of science as it progresses in time; it is simply to say that it is not at all clear that we can tell the full story of this "progress" (not even the full "rational" story) in terms of this progression of the "epistemic," the "non-standard epistemic," and the "non-epistemic."

Longino also sets out to understand the rational and the social in science by examining the role of "constitutive" and "contextual" values in the development of science.[2] She motivates a central thesis in her book with a rhetorical question: "The traditional interpretation of the value freedom of modern natural science amounts to a claim that its constitutive and contextual features are clearly distinct from and independent of one another. Can this distinction, as commonly conceived, be maintained?" (1990, p.4). Longino's answer is clearly "no" but it is not always clear what exactly this "no" entails. It means at least claim (1): it is wrong to suppose that science is value-free in that we can accommodate its cognitive development and import solely within an account of constitutive values, while granting that contextual values are of interest solely within an historical or sociological account of scientific development. Claim (1) essentially vitiates the constitutive/contextual distinction *only insofar* as it is readily aligned with the rational/social (or the now maligned internal/external) distinction. However, for the purposes of my argument in this paper I am especially concerned with whether or to what extent Longino is suggesting a stronger claim (2): a better or fuller account of the cognitive development of science requires abandoning the constitutive/contextual distinction altogether (though it may be useful for preliminary discussion). There are places where she comes close to (2). At one point she discusses how various ways of conceptualizing the objects of (scientific) knowledge can "help to show how contextual values are transformed into constitutive values" (p. 100). However, the contextual/constitutive distinction remains central in her book and with specific examples she is careful to note when she takes a particular value to be constitutive or contextual. Yet, I would argue that some of those same examples taken one step further entail (2), and thus there is some tension in the book around where exactly she stands between (1) and (2).

Longino is not simply interested in science as theoretical *product* and, in particular, in what happens during times of revolutionary change, of grand theory contestation (as in the oft-cited Ptolemy-Copernicus or Bohr-Einstein examples). Some of the epistemic issues have been skewed, she thinks, by the overemphasis on these kinds of examples. She argues that by drawing attention to the practices of normal science, to science as a process, we can gain valuable insight into the influence of contextual values on these practices, on the description of data, the determination of questions as worthwhile or not, and on the specific and global assumptions that motivate background assumptions facilitating inference from evidence to (otherwise underdetermined) hypothesis. This stress on science as *process*, (like McMullin's stress on the role of community practice and skills in the formation of epistemic value-judgment), is surely something that is very valuable in this discussion. Yet, this stress also shifts the focus away from an understanding of epistemic values as criteria that are somehow applied from the outside, as epistemic measurements used to evaluate competing theories during times of theory contestation. The shift to science in process parallels the shift in our appreciation of epistemic values that I am arguing in this paper—the shift to an appreciation of the development of epistemic factors *within* the context of theory development, as encoded within the many complex elements that go into the making of a sophisticated scientific theory. With their adherence to their respective distinctions, it is this extra shift that both Longino and McMullin don't seem to want to make, and this leaves something missing in the analysis of their examples they draw upon.

Longino pays significant attention to the linear-hormonal model as that is used in the explanation of physical and cognitive human behavior, and specifically as it used to explain purported sex differences. She compares this model with an alternative explanatory model, the selectionist or social-cognitive model. She compares them first with respect to their constitutive-methodological differences. She argues that neither theoretical perspective can muster constitutively based arguments sufficient to exclude the other, thus suggesting that it is contextual interests and values that motivate adherence to one or the other theoretical framework. In the following chapter (ch. 8) Longino outlines some of the political and social interests served by the adoption of one model over the other, and, in particular, the implications for reinforcing gender dimorphism that the linear-hormonal model supports. One is thus left with the impression (which Longino may ultimately not want to leave us with) that while both theories can be maintained on some constitutive-evidentiary grounds, theory choice comes down to one's anticipation or dislike of the social-contextual implications of the two theories. What seems to be missing here is an analysis of the way in which these background interests or values get worked into the theories right from the start, get embedded into the forms of language, data description, and question formation as these develop along with the development of the theory in question... and ultimately get embedded in the ways in which constitutive values like simplicity and fruitfulness get understood within the context of the particular theory.[3]

In comparing the constitutive bases of the two models Longino argues that the linear-hormonal model scores high in theoretical unification and simplicity, both because it supports unification across mammalian species, and also because on that model certain aspects of social behavior can be treated on a continuum with other physiological effects of prenatal gonadal hormonal exposure. But surely part of the constitutive force of "simplicity" in this model is due to the fact that in it gender dimorphism is motivating in part the very understanding of biological determinism itself, rather than the other way around. Increasingly, feminist work in this area is suggesting that we simply cannot suppose that a clear theoretical understanding of biological determinism was developed and then theorists "happened to notice" that this was somewhat different for women and men! What is suggested instead is that gender dimorphism became constitutive of the understanding of biological functioning, and thus in effect became constitutive also of the "constitutive" value of the simplicity or fruitfulness of the linear-hormonal model itself insofar as it was constructed to "explain" biological determinism. Yet, an understanding of the impact or value of gender dimorphism clearly has contextual social and cultural underpinnings in a society that supports gender order and hierarchy, and political power issues loom on the horizon when it becomes apparent that those who got to develop the theory represented overwhelmingly only one of the dimorphic forms in question. A fuller understanding of the complex cognitive development in this and other such examples requires careful historical scrutiny clearly, and as I indicate in this example, ultimately involves adopting position (2) above and not simply (1). In discussing and comparing the constitutive and contextual features of these models separately (even while she argues that both are *necessary* for a full understanding of the cognitive adoption of either theory) Longino clearly implies that they work somewhat separately in the overall picture.

In examining these two important accounts of the role of values in science I have drawn attention to the significance of the distinction between epistemic-constitutive and nonepistemic-contextual values in the two accounts, yet I have also shown that some of the most interesting arguments that emerge from those same accounts go a long way toward undermining that same distinction. McMullin does this in large part with his introduction of non-standard epistemic values, and an exploration of the workings of Longino's contextual values requires an understanding of the way in

which these become constitutive of particular scientific research programs. Thus, in a sense, these and other theorists who present us with these sets of epistemic values are correct in presenting them as fuzzy sets of fuzzy criteria, but perhaps not for the reasons they think. Values like simplicity and fruitfulness, even accuracy and coherence, are multi-faceted. We might think of simplicity as a pragmatic or aesthetic quality, simplicity with respect to language, or with respect to cognitive or perceptive models used, simplicity with respect to ease in understanding...where all of these may overlap in interesting ways. As we have seen, the "application" of a "constitutive" criterion in a scientific context requires a complex background of languages, practices, and skills within which all sorts of constitutive-contextual features are already encoded.

3. Feminism, Science, and Values

I was also drawn into a closer examination of this issue through an exploration of feminist work in philosophy of science. Here the role of "patriarchal" or "androcentric" values is taken as a central concern. The project is then seen to involve locating these prime examples of contextual social and cultural values, and showing how they operate in the scientific contexts in question. However, this approach does not encompass the full impact or possibility of the feminist work. In addition, this is often the way that those unfamiliar with the full range of feminist work in this area seek to project it. This can have the effect of marginalizing it as at most a "subarea" in social studies of science, thereby constricting its philosophical import as these theorists construe such import.

This is not to say, however, that the ongoing work of locating the workings of androcentric values is not a very important and necessary part of the feminist project, and significant insights about the development of the biological and anthropological sciences have been achieved with this focus (Hubbard 1982, Bleier 1984, Fausto-Sterling 1985). Numerous examples are given where androcentrism operates in the description of behavior, and especially in the way automatic valuings are assigned to purported gender differences. Ruth Hubbard provides examples of the operation of androcentric and Victorian values in Darwin's evolutionary theory, in the predictable roles accorded "active" males and relatively "passive" females. However, as I argued in connection with the linear-hormonal model earlier, the discovery of separable articulable androcentric values does not exhaust the full impact of this work. Anne Fausto-Sterling details assumptions about male-female differences in the different biological theories of "higher cognitive functioning" in this past century (pp.13-60). She doesn't simply locate specific points where androcentric bias sneaked in. She details in effect how these assumptions became constitutive of the scientific articulation of higher cognitive functioning itself (in the X-linked hypothesis, for example), something which also presents a potentially serious problem for naturalistic circumscriptions of rationality. In evolutionary theory, on the other hand, attention has been given to the importance of variability as a mechanism of survival: a feminist analysis must also include an examination of the way in which the assumption that the male was the "more variable" sex helped circumscribe the theoretical significance and placement of the concept itself within the overall theoretical framework. Donna Haraway's work in primatology (1989) is not adequately described in terms of the articulation of androcentric and ethnocentric values. She provides insights into the inextricable link between cultural valuations and the development of the rhetoric and discourse of primatology studies, insights that can be exploited in our analysis of the rhetorical dimensions of other areas of scientific inquiry. Similarly, Evelyn Fox Keller's work on gender and science details insights into the ways in which culturally-infused language has had an impact both in the development of such fields as molecular biology and quantum mechanics, and in the theoretical interpretations of the "objectivity" of the knowledge that emerged in such fields (1985, pt.3; 1990). Arguments to the effect that all of these influences did not significantly affect

the "internal" cognitive development of the sciences in question become especially tricky when we note that the clarity of the demarcation of the "internal" is itself called into question with feminist work on the historical cultural gendering of such divisions as objective/subjective, rational/social, and science/nonscience (Rouse 1991). In other words, it is not at all evident how one can readily make such arguments without thereby making assumptions that are essentially question-begging.

There are various reasons why these more far-reaching implications of the feminist work are not generally acknowledged. First, while some feminists might describe their work simply in terms of locating and uprooting androcentric values, this is much more likely to come from non-feminists describing that work. Michael Ruse's discussion provides a good example here (1984). He frequently refers to the values that concern or interest "the feminists". Working from McMullin's epistemic/non-epistemic framework he takes the values that are "of particular interest to our feminist writers" to be non-epistemic and wants to allow room for the values that feminists *do* endorse by questioning whether non-epistemic values are to be gradually sifted out as McMullin wants. Ruse oversimplifies the feminist work here, and to a certain extent misrepresents McMullin's thesis also. In particular, he doesn't include a discussion of McMullin's non-standard epistemic values which may well be the most interesting ones from the point of view of incorporating all of the feminist work, especially when the insights that emerge in that work (like the non-standard epistemic values) go a long way toward undermining the force of the epistemic/non-epistemic distinction itself.

While, contra Ruse, feminist work in science cannot simply be identified with sets of values that "the feminists" uniformly reject or endorse, that work does shed some light on the workings of various values in science. It shows, first of all, that the influence of "androcentric" and other cultural values in science is much more complex than we might initially suppose. Though these values have what McMullin would probably agree are "non-epistemic" origins, it is not at all clear that the automatic prognosis for them over time pointed to their eventually being sifted out rather than grafted into the various sciences, informing the constitutive formations of those sciences. Why, we might ask, have some of the fairly obvious examples of gender and race bias become visible only relatively recently, despite philosophers' and scientists' long-held proclamations about the value-freedom of science? In addition, discussions about androcentrism regularly meet with resistance or hostility from many of those within science, so one cannot say that science as we have known it to date necessarily welcomes calls to examine and sift out certain kinds of non-epistemic factors, calls that according to its stated truth-seeking, value-free mission it surely ought to welcome. The gradual separation of the non-epistemic from the epistemic does not seem to be as *constitutively* guaranteed as McMullin would like. It sometimes seems to require specific "external" historical political shifts, such as the admission of women into science, or the development of second-wave feminism. These clearly did not come from constitutive "truth-seeking" impulses internal to the institutions and practices of science itself.

Such a truth-promoting impulse is at the heart of McMullin's epistemic/non-epistemic distinction, and my argument here provides additional reasons for us to question that distinction. In addition to the more direct problems outlined earlier in the lack of clarity of the distinction, I have also maintained that the distinction is largely undermined by insights that have resulted from the development of feminist work in science. With continuing development of this work we start to get new glimpses of the ways in which values we *now* recognize as "non-epistemic" became encoded into constitutive features of specific theories—in effect became part of what constituted the truth or simplicity of those theories—and thus could remain for so long impervious to the "progress" of science as that was facilitated by those same constitutive features.

The way forward then is not to seek to regroup the epistemic/non-epistemic division but to adopt different attitudes toward it. First, it may still be quite fruitful to use the epistemic/non-epistemic as something like a continuum scale in given scientific contexts. It may well be important to identify specific factors (in a given context) as non-epistemic with respect to the agreed-upon truth-seeking practices of a given scientific community at a particular time. One might even add that that is precisely what constitutes particular research programs as "scientific". Second, now that the epistemic/non-epistemic "divide" emerges as a continuum site of active renegotiation in the continual reconstitution of specific scientific inquiries, we are surely invited to become active humanistic philosophers of science again. We are no longer passive observers charting out the philosophical landscape of a divide antecedently or transcendentally mapped out by something called Truth, or the Progress of Science. Once we understand how cultural and social values can in time, with systematic theoretical cognitive rearticulation, become encoded into constitutive features of the rationality and objectivity of particular scientific endeavors, into features that are genuinely epistemically compelling for given scientific communities, we are invited to gain greater insight into how that occurs, and we are invited to develop that insight within the context of specific philosophical concerns and questions. We get to show how the undermining of an epistemic/non-epistemic divide need not involve dismissing heretofore "epistemic" criteria as hopelessly reductive or relative. We do not have to deny the importance of a relatively stable epistemic background for a given scientific community over an extended time. We get to ask questions about how particular ways of valuing inform particular ways of knowing, of belief formation, of rational compulsion, without thereby dismissing the significance of our various ways of knowing, or without suggesting that epistemology disappears into value theory. We are then in a better position to more effectively address questions about whether our various ways of knowing in science are reflecting the kinds of values we seek to promote into a viable future, and to understand how to bring about change when we do not like what we see emerging in our future. Above all, we need to continually monitor how our forms of scientific inquiry are answering to the overarching interconnecting values of environmental integrity and universal human dignity. For it is these surely that rest at the heart of the impulse toward science.

Notes

[1]It should be noted here, however, that in this context Laudan seems to take "cognitive values" to be much the same thing as "cognitive goals" or "aims" and this detracts somewhat from the relevance of his argument to the issue at hand. Kuhn, Longino, and McMullin have somewhat similar conceptions of epistemic "value" in these discussions, and would distinguish these values from what might be termed the broader aims or goals of science such as knowledge, truth, or good explanation, however contested these latter might be. In this context at least these latter are more the focus of Laudan's discussion.

[2]One cannot assume here that McMullin's epistemic/non-epistemic distinction is exactly the same as Longino's constitutive/contextual one, especially since neither addresses the other's work in this context, and also since they proceed to do somewhat different things with their respective distinctions. (A distinction is as good as the use to which it is put, one might argue!) My argument in this paper does not depend on their being identical, and in fact suggests that they cannot since neither distinction can be precisely circumscribed.

[3]In places Longino seems to indicate that this is precisely the kind of long-term analysis that her thesis invites. In this book she might be seen to be mapping out the philosophical groundwork of an understanding of science and values within which that project is to be carried out. However, what I am concerned about is whether the way in which she lays that out—specifically with respect to the constitutive/contextual distinction—ultimately restricts the full development of that project

References

Bleier, R. (1984), *Science and Gender: A Critique of Biology and its Theories on Women*. New York: Pergamon Press.

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

Haraway, D. (1989), *Primate Visions: Gender, Race, and Nature in the World of Modern Science*. New York: Routledge.

Hubbard, R. (1982), "Have Only Men Evolved?", in *Discovering Reality*, S. Harding and M.B. Hintikka (eds.). Dordrecht: Reidel, pp. 45-69.

Kuhn, T. (1977), "Objectivity, Value Judgment, and Theory Choice", in his, *The Essential Tension*. Chicago: University of Chicago Press, pp. 320-339.

Keller, E.F. (1985), *Reflections on Gender and Science*. New Haven: Yale University Press.

_____. (1990), "From Secrets of Life to Secrets of Death", in *Body/Politics: Women and the Discourses of Science*, M. Jacobus, E. F. Keller, and S. Shuttleworth (eds.). New York: Routledge

Laudan, L. (1984), *Science and Values: The Aims of Science and Their Role in Scientific Debate*. Berkeley: University of California Press.

Longino, H.E. (1990), *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry*. Princeton: Princeton University Press.

McMullin, E. (1983), "Values in Science", in PSA 1982, Volume 2, P.D. Asquith and T. Nickles (eds.). East Lansing: Philosophy of Science Association, pp. 3-28.

_____. (1984), "The Rational and the Social in the History of Science", in *Scientific Rationality: The Sociological Turn*, J.R. Brown (ed.). Dordrecht: Reidel, pp. 127-163.

Rouse, J. (1991), "The Politics of Postmodern Philosophy of Science", *Philosophy of Science* 58: 4.

Ruse, M. (1984), "Biological Science and Feminist Values", in *PSA 1984*, Volume 2, P.D. Asquith and P. Kitcher (eds.). East Lansing: Philosophy of Science Association, pp. 525-542.