
REVIEW SYMPOSIUM

ON THE VERY IDEA OF A FEMINIST EPISTEMOLOGY FOR
SCIENCE*

Sharyn Clough, *Beyond Epistemology: A Pragmatist Approach to Feminist Science Studies*. Lanham MD: Rowman and Littlefield, 2003. Pp. viii + 167. US\$24.95 PB.

By Elizabeth Potter

Many feminist epistemologists have taken up the work of mainstream philosophers and shaped it for feminist ends (as Lynn Hankinson Nelson, for example, has successfully used the work of W. V. O. Quine). Clough finds the work of Donald Davidson – an unlikely source for feminists – instructive for feminist epistemology and science studies. Her book is a sustained treatment of the turn to epistemology from empirical science critique. Analysing the work of such feminist thinkers as Ruth Bleier (*Science and Gender*, New York, 1984), Evelyn Fox Keller (“Feminism and Science”, *Signs*, 1982 and *Reflections on Gender and Science*, New Haven, 1985), and Helen Longino (“Can There be a Feminist Science?” *Hypatia*, 1987, and *Science as Social Knowledge*, Princeton, 1990), Clough maintains that they have worked under the spell of the epistemologically mistaken view, ‘representationalism’. According to this mistaken view, sensory ‘data’ are completely unconceptualised; the human mind must filter this unconceptualised ‘stuff’ through concepts before the stuff can be understood as anything, as red or smooth or as an object such as a cup or a ball. Minds have no direct access to the world except through these conceptualised data or ‘representations’.

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Davidson refers to the set of concepts through which we filter our raw data as ‘interpretive schemes’. In the representationalist view, our languages serve as such interpretive schemes. He has many objections to the view, but the most serious problem with it is that it invites global scepticism. The sceptic wants to know how we can be sure we have interpreted the raw data correctly; we could be totally wrong about the world as it really is.

Other science scholars such as Sandra Harding (*The Science Question in Feminism*, Ithaca, 1986, and *Whose Science? Whose Knowledge?*, New York, 1991) may not ‘focus’ on representationalism, but they still keep vestiges of it, Clough argues. Most notably, she maintains that Keller, Harding, and Longino take political and other values to be filters for interpreting raw data; and since each of them argues that the theories of the sciences depend upon the political schema and values scientists use to interpret data, we must infer that theories are relative to political and other such values. This relativism is self-defeating, inasmuch as Keller, Harding, Longino and other feminist critics of the sciences still hold that some theories are better than others – but Clough argues that their relativism makes this impossible.

The problem here arises from a representationalist use of the underdetermination thesis. Versions of this thesis (and there are many) have been useful for feminist epistemologists of science. The version that Clough attributes to Keller, Harding, and Longino states that:

every scientific theory is underdetermined by the evidence brought forward in its support; that is, theoretically, any particular piece of evidence can be used to support an infinite number of theories. Conversely, for any theory that fits the available evidence, there may be another theory that fits the same evidence equally well (Clough 2003 93).

For representational epistemologists, a corollary of the underdetermination thesis is that, given that some scientific theories are chosen over others, and given that the evidence does not determine which is better, choices must be based on other explanatory schemes, e.g. political ones, not just on the evidence. “All our knowledge becomes relativised to our conceptual filters. . . the best we can do is pick the theory screened through the most appealing (to feminists) and/or least partial conceptual scheme” (Clough: 92). Clough recognises that Longino, Harding, and Keller reject relativism, but, she says, “each is left with a watered-down prescription for feminist scientific method that is restricted to detecting how the

filter of culture intervenes between the world and scientific knowledge,” and advocating that scientists choose feminist and/or less partial theories (Clough: 92).

Clough further attributes to these three scholars the view that values are not amenable to reason or evidence (i.e., they are ‘non-cognitive’) and argues that this view is false. Values, she says, are not different in kind from factual beliefs; both are based on experience and reason.

Clough’s positive recommendation to feminist epistemologists and philosophers of science is that they stop doing epistemology – which she identifies with representationalist epistemology – and go back to empirical criticism of scientific theories that are harmful to women and other ‘Other’s. Feminist science scholars should work “to eradicate the harmful effects of sexism, racism, and other oppressive systems in all aspects of scientific research, laboratory by laboratory, research program by research program.” Such empirical studies of science will be “more effective and less harmful than our current epistemological focus” (Clough: 21).

To understand current feminist epistemology, especially epistemology of science, it is important to respond to Clough’s charge that feminist epistemologists take non-cognitive values as conceptual filters in Davidson’s sense and to her intriguing claim that values bear a class relationship to factual beliefs.

It is not at all clear that Harding and Longino, in particular, are Davidsonian representationalists. Here I shall take up the work of Longino, but I believe that Harding can also be successfully defended against Clough’s charge. Clough focuses on Longino’s *Science As Social Knowledge* (1990), in which we find that, according to Longino, there is no unique or intrinsic evidential relationship between evidence and the hypothesis or model for which it functions as evidence. Instead, the connections or regularities we appeal to in assessing evidential relations are connections or regularities from some point of view and are always subject to change. The objects, events and states of affairs providing evidence for hypotheses do not carry labels showing what they are evidence for. Instead, how one determines evidential relevance depends upon one’s background beliefs or assumptions (Longino: 43 and 45). Longino says that a background belief enables us to see *e* as evidence for a hypothesis *h*, not in the sense that we come to believe *h simpliciter* (although we might) but in the sense that we come to believe that

given the background assumption b , e makes h plausible. As Longino puts it, background assumptions are “beliefs in the light of which one takes some x to be evidence for some h and to which one would appeal in defending the claim that x is evidence for h ” (Longino: 44).

Moreover, in this view, data are not unconceptualised; to take an example Clough cites, Longino states that ancient chipped stones constitute the data to be interpreted through a man-the-hunter or woman-the-gatherer theory. Here the data are already conceptualised as chipped stones; they must be interpreted either as tools used by men for hunting or as tools used by women for preparing food. But this is not a representationalist epistemology in which concepts mediate or filter unconceptualised ‘stuff’. On the basis of many background assumptions, anthropologists must decide whether a chipped stone (not just some ‘stuff’) was used for hunting or for food preparation.

Nevertheless, Longino does argue that the available anthropological data in this case are so few as to underdetermine the choice of hypothesis and that most anthropologists decided matters on the basis of a man-the-hunter view, itself supported in part by unexamined gender assumptions. In *Science as Social Knowledge*, Longino did not explicitly argue that such social and political values are non-cognitive; in fact, she rejected the distinction between cognitive and non-cognitive values in favour of a distinction between constitutive (those values that constitute recognised criteria for theory choice, e.g. simplicity, consistency, etc.) and contextual (those non-constitutive values arising from the context in which knowledge is produced).

Clough points out that feminists need not urge anyone to choose an interpretive theory on the basis of non-cognitive political values; instead, feminists have good reason for favouring the more inclusive woman-the-gatherer interpretation: it does not rule out the possibility that men developed tools for hunting and there is plenty of past empirical evidence that “to ignore the role of women is to get the ‘human’ story drastically wrong”. A decision in favour of the woman-the-gatherer interpretation is “well supported by inductively observed instances of past scientific errors” (Clough: 116). This is a welcome contribution to the feminist critique of the sciences and brings us directly to questions about the relationship between values and scientific facts. How shall we understand moral and political values? In Clough’s feminist pragmatism, do they cash

out as facts? In what ways are contextual values analogous to facts? Longino has been at pains to show that they have an epistemic role in scientific reasoning; Clough has not yet spoken on the matter. For the chipped stones example, in place of Longino's non-constitutive value (i.e., contextual value) Clough substitutes a good inductive generalisation ('in the past, ignoring the role of women has led to wrong scientific accounts of human lives'), which is evidence that researchers have not followed the general methodological norm: 'consider all significant evidence when testing a hypothesis'. This leaves us wondering whether, for Clough, the unconscious negative value placed on women, e.g. "women are not important" has functioned in inferences to, for example, good medical hypotheses that could have been better if women's bodies were thought to be as important as men's, or whether it has led to bad medical science because it is a value, or whether it can be used like a factual belief for evidence as in the chipped stones case.

I suggest that the claim that values are not different in kind from facts should be understood to mean that many contextual values are analogous to facts in that they are subject to valid reasoning and correction by evidence. The assumption behind the claim that moral and political values are 'non-cognitive' is that these values are not subject to reason and evidence. But as Elizabeth Anderson notes in "Uses of Value Judgments in Science: A General Argument, With Lessons from a Case Study of Feminist Research on Divorce" (*Hypatia*, 2004), growing up, having human experiences such as disillusionment, etc. allows most people to learn from experience that some of their values are mistaken. Most people are capable of growing and learning in these ways. Some people are not. In such cases they are dogmatic, holding to some or all of their values regardless of the facts. But values themselves are not inherently dogmatic. One of the primary reasons that most people can learn from experience that their values are mistaken is because we take our emotional experiences – which Anderson defines as "affectively colored experiences with people or things or events" – to provide evidence that these people or things or events have value. For example, if we experience California redwoods with awe, we take this as evidence that they are splendid (Anderson: 9). While philosophers admit that we do this, traditionally they have been sceptical of whether emotional experiences are reliable sources of evidence for the value of anything.

Anderson agrees with John Deigh's argument in his "Cognitivism in the Theory of Emotions" (*Ethics*, 1994), that some emotional experiences have cognitive content; that is, some experiences are 'affectively colored experiences', and like most experiences, these have cognitive, usually representative, content. Moreover, such experiences are defeasible (though not as responsive to the world as beliefs). That is, we can find out that the representative content is erroneous, confused, etc. Thus, if we find out that the cognitive content of an emotional experience is defective in some way, we may also discount the importance of the feeling.

Such emotional experiences can function as evidence for values because these experiences are independent of our desires and ends. In Anderson's example, Diane desires elected office and values a political life. Despite her desires and values, she feels badly about the political life, disillusioned by campaign financing, political backbiting and small political gains. These emotional experiences do not depend on her desires and values and, in fact, undermine them. But an ally tries to persuade her that her 'disappointment with what seems to be a merely symbolic victory reflects an unduly narrow perspective'. Taken in isolation this victory achieves little, but 'in the long view it can be seen as fundamentally shifting the terms of debate. What seems like a hollow victory is a watershed event. This [factual] judgment could be tested over a longer stretch of experience'. The ally argues that Diane should continue to value the political life. This sort of persuasive argument is quite common and makes sense only because our emotions are responsive to reason and facts. And usually our emotions are reliable, though certainly not infallible, evidence for our value judgments. (The exceptions include emotions affected by drugs, depression, etc.) When it is clear that the representational content of an emotional experience is adequate, we can trust our emotions (Anderson: 9–10). We may conclude, then, that contextual values are not necessarily 'science-free,' i.e. are not non-cognitive, and need not be held dogmatically.

If (some) contextual values are subject to reason and to evidence, then they might legitimately be used in science. To distinguish legitimate from illegitimate uses of values in science, Anderson suggests that values are legitimately used in science if they do not drive research to a predetermined or favoured conclusion. Thus, contextual values legitimately influence science if (A) precautions are taken to ensure that research is not biased in relation to:

1. “the object of inquiry” (such that “it (truthfully) reveals only some of its aspects, leaving us ignorant of others”);
2. “its hypotheses” (i.e. it is not rigged (wittingly or not) to confirm them. “A good research design must allow its hypothesis to be disconfirmed by evidence;”);
3. “a controversy” (“such that it is more likely to (truthfully) uncover evidence that supports one side rather than all sides”).

On the other hand, one “research design is more *fruitful* than another, with respect to a controversy, if it is more likely to uncover evidence supporting (or undermining) all, or a wider range of sides of the controversy” (Anderson: 18–20). Thus, contextual values legitimately influence science if (B) the values are epistemically fruitful.

To understand how values can be epistemically fruitful, we can see how a theory might presuppose some value in a way that, for example, leads the researchers to classify their data according to that value: here the value provides a norm for classifying data. If the theory is more epistemically fruitful than rival theories for which the data is not classified according to this norm, then the value makes the theory more epistemically fruitful, i.e. the theory reveals more relevant facts that can count for or against it than do rival theories. Thus, using contextual values in science is sometimes epistemically justified. Scientists accept the theory on impartial grounds, namely that it is more fruitful than rival theories, not in spite of its presupposing a contextual value, but because of presupposing it (Anderson: 20). Finally, Anderson does not think all moral and political values have equal epistemic value, or that a contextual value legitimately used in one inquiry is legitimately used or fruitful in all research – from which it follows that feminist values are not always legitimately used in science and that patriarchal values are not always illegitimately used.

To show us the many places at which values can legitimately enter the research process, Anderson makes an expository division of the ‘stages’ of research. Researchers:

- (a) begin with an orientation to the background interests animating the field;
- (b) frame a question informed by those interests;
- (c) articulate a conception of the object of inquiry;
- (d) decide what types of data to collect;
- (e) establish and carry out data sampling or data generation procedures;
- (f) analyse their data in accordance with chosen techniques;
- (g) decide when to stop analysing their data; and
- (h) draw conclusions from their analyses (Anderson: 11).

At each ‘stage’, the evaluative presuppositions, evidence, and evaluative conclusions can interact.

This ‘theorisation’ of contextual values allows us to see that, whether they differ in kind from factual beliefs or not, many values are more similar to factual beliefs than irrational emotions in that they are subject to valid reasoning and to correction by evidence. Such an analysis of the ways contextual values work in science should be helpful to the feminist empirical science studies Clough recommends. We need not be misled by claims that good science is ‘value-free’, by anti-feminist claims that feminist values automatically prevent scientists from using facts to reach their conclusions, or by some feminist claims that ‘masculinist’ values invariably lead to bad science. Also, it remains an open question whether contextual values enter or impinge on a given piece of research; we must look case by case to find out. But to counter the presumption that contextual values can never legitimately be used in research, we need good epistemology of science to show that they can do so and to suggest how to look for them.

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In *Beyond Epistemology*, Sharyn Clough argues that feminist critics of science should distance themselves from general epistemological endeavours and return to local investigations of specific scientific claims. In particular, Clough targets what she sees as representationalist feminist epistemologies. The central problem with these epistemologies, she claims, is that they treat objective facts and subjective biases as metaphysically distinct. Because the objective and subjective worlds do not directly ‘touch’, subjective biases cannot be evaluated on the same grounds as empirical evidence. Given this, we can never know for certain if the subjective biases filtering reality distort that reality. This means that feminists who claim that androcentrism and sexism filter and probably distort the facts of science should also claim that feminism filters and probably distorts the facts of science. Feminist critics of science that explicitly or implicitly assume representationalism must therefore be sceptical of

their own critiques. Clough rightly contends that this scepticism seriously undermines feminist critiques of science.

To avoid such scepticism, Clough argues that feminists analysing science should drop representationalism and adopt a pragmatist approach. The pragmatist approach she develops is based on Davidson's philosophy of language, wherein "empirical data plays a causal role in establishing the content of all beliefs" (Clough 2003: 110). Because in Davidson's philosophy of language there is no metaphysical distinction between beliefs and "some nonbeliefs we call 'the evidence'" (Clough 2003: 109), or between subjective ideological filters and objective facts, his approach by-passes representationalism and its attendant problems with scepticism. Clough claims that feminists adopting this non-representational approach can similarly avoid scepticism and strengthen their critiques of science. Moreover, these critiques can proceed empirically, without need for specific epistemological grounding – for feminist political values are just as empirically-based as scientific facts. Values and facts are strands in the web of belief and are similarly amenable to confirmation, falsification, and correction.

Clough's book is important and timely. On the one hand, many humanities and social science scholars interested in the question of objectivity in science are misinterpreted or dismissed as relativists who care little for 'reality'; but on the other hand, many of these scholars have failed to consider adequately the issue of representationalism in their work, which may contribute to their being misinterpreted. Clough does much to clarify the issues at stake and her approach stands to reinvigorate the project of correcting sexist and androcentric bias in science at the local level of particular knowledge claims. Scientists, critics of science and philosophers of science should thus consider her arguments seriously. In this spirit, I wish to consider two problems that arise in response to Clough's claim that epistemologically-oriented feminist critics of biology assume representationalism. The first problem concerns tentativeness and the distinction between representationalist and non-representationalist ideological filtering in feminist critiques of science. The second problem is the concern that Clough undervalues the importance of conceptualising bias as a filter in feminist critiques of science. Addressing these problems would serve to strengthen further her argument.

The first problem arises from Clough's claim that the source of scepticism in feminist critiques of biology is the representationalist assumption that the subjective and objective worlds belong to different metaphysical categories. While Clough provides some evidence for this claim, it is unclear that representationalism is the actual source of this scepticism. Another plausible source is *tentativeness*. Fair reasoners must always be on guard in their own work for illegitimate biases introduced by omission and distortion. Feminists are experts at identifying bias and unfairness and those that are enculturated in the cognitive virtues of science should be especially critical of bias in their own perspectives. Additionally, given that feminists are still routinely accused of bias, hypersensitivity, and inappropriate anger, a heightened self-awareness of being or appearing biased is to be expected. Given this context, it is unsurprising that feminists express doubts and make unnecessarily tentative remarks about the veracity of their claims. My argument is that the expressions of scepticism that Clough identifies in feminist critiques of science may in fact often be understood instead as expressions of tentativeness.

To argue for tentativeness as a competing explanation for the sceptical-sounding remarks of feminist critics of science, I focus on Ruth Bleier's *Science and Gender* (1984), one of the feminist critiques that Clough identifies as representationalist. Bleier's use of language does make her vulnerable to Clough's charge that she problematically courts scepticism. For example, Bleier says:

I actually engage in the very activity I warn readers to question, if not distrust. Put differently, I present 'facts' to refute 'facts,' which I claim have been made [up] in the interests of the dominant group – white men. I offer feminist interpretations to replace patriarchal interpretations, which I say reflect the ideology, desires, and necessities of a particular interest group. I am indeed caught in my own trap! (Bleier 1984: 13).

But does Bleier really think she is caught in her own trap? Immediately following this remark, she says:

But perhaps I am not. As I will try to maintain throughout my work, I see any theory—feminist or patriarchal – as flexible and open to change. In fact, as a scientist and a political being, my mind lingers with pleasure when I encounter theories that allow for constant change, interaction, contradiction, ambivalence (Bleier 1984: 13).

Here, Bleier's language suggests openness to the idea that her critiques are amenable to confirmation, correction, and falsification in the light of evidence.

The interpretation that Bleier does not think she is caught in her own trap is substantiated by her emphasis on identifying flawed reasoning in biology – an emphasis that would be misplaced in any thoroughgoing scepticism. She says, for example:

I have demonstrated a number of basic conceptual and methodological flaws in the work of Sociobiologists, which include faulty logic; unsupported assumptions and premises; inappropriate use of language; lack of definitions of the behaviors being explained; and ethnocentric, androcentric, and anthropocentric biases underlying the questions that are asked, the language used, the selection of animal models, and the interpretation of data (Bleier 1984: 46).

The strategies that Bleier uses to identify flawed reasoning, as in the case of sociobiology above, set standards that are neither relativist nor sceptical in nature. Moreover, Bleier is open to the application of these standards to her own reasoning. So although Clough questions Bleier's ability to "persuade her readers of the truth of her claims" in light of her sceptical-sounding remarks, Bleier's standards of reasoning should contribute positively to her persuasiveness (Clough 2003: 61). Without such standards, Bleier's arguments could do little more than uncritically reconfirm her own biases.

Indeed, much of what Bleier says can be interpreted as displaying concern for deeply critical self-reflection, something she sees as missing from the work of male scientists, generally speaking. Bleier quotes from Evelyn Fox Keller's article "Feminism and Science" (1982) approvingly with regard to critical self-reflection, and speaks of "the courageous and difficult task of examining and questioning all of our assumptions and the very structure of our thought processes, all clearly born and bred within a profoundly stratified, hierarchical, patriarchal culture" (Bleier 1984: 206). Bleier expresses concern that subjectivity cannot be excluded from scientific work. In this context, she seems to cast subjectivity as corrupting ego and pride, arrogance, and poor self-awareness, rather than as an entity that is metaphysically distinct from the objective world (Bleier 1984: 204).

So the question boils down to whether or not Bleier holds, implicitly or otherwise, a dualist metaphysics. While Clough sees Bleier's talk of ideological filters as evidence of representationalism, such talk may refer to what I would consider to be non-representational ideological filters – that is, filters having the same metaphysical status as empirical evidence. Non-representational ideological filters can be understood as very complicated nodes of empirical

information that function in much the same way as representational filters in the distortion of evidence. However, the empirical status of non-representational filters would mean they are amenable to change, correction, improvement, and ultimately, reduction. Given the complexity, depth, and resistance of (non-representational) ideological filters, it could be said, without courting scepticism, that they cannot be completely eliminated: we will always have some non-representational filter or other with which to contend. Thus, while non-representational ideological filters need not entail scepticism, the persistence of these filters does mean that we should persistently exercise caution about our knowledge claims. This prescription could easily encourage sceptical-sounding remarks, without assuming a dualist metaphysic.

Thus far, I have argued that Clough may misidentify tentativeness as scepticism and that it would help to clarify the distinction between representationalist and non-representationalist ideological filters in feminist critiques of biology. This latter point concerning the need to distinguish more clearly between representationalist and non-representationalist filters is related to the second problem I raise in response to Clough's argument. Because Clough does not sufficiently clarify the distinction between representationalist and non-representationalist ideological filters, she may be inadvertently dismissive of the positive role that conceptualising bias acting as a filter can have in feminist critiques. Clough's association of ideological filtering with representationalist metaphysics suggests that if we are to avoid importing representationalist assumptions into our critiques we should avoid conceptualising bias as something that 'filters' evidence. This would be a mistake. The notion of non-representational ideological filtering is a valuable tool in explanations of androcentrism in biology and I argue for its importance using Clough's pragmatist case study of Margie Profet's hypothesis of menstruation as a sperm-borne pathogen defence and the entrenchment of this hypothesis in evolutionary biology ("Menstruation as a Defence Against Pathogens Transported by Sperm", *Quarterly Review of Biology*, 1993). I hold that while Clough's explanation of the non-functional status of menstruation in terms of entrenchment is very useful, there is a larger disciplinary issue involved, which does not make sense unless we invoke the idea that androcentrism filters experience and thereby structures the conceptual framework(s) in which research concerning female pathogen defence systems takes place.

Clough claims that the female capacity for defence against sperm-borne pathogens is not currently entrenched in evolutionary biology; that is, the capacity has not “appeared in a sufficiently large number of hypotheses” in that field (Clough 2003: 144). And, given that the more general idea of a female defence system against sperm-borne pathogens is not entrenched, it would be surprising if menstruation as a mechanism of pathogen defence were. However, while the female capacity for defence against sperm-borne pathogens is not entrenched in evolutionary biology, this is not the case elsewhere in biology: it is somewhat entrenched in immunology and reproductive immunology. Vaginas, like other mucosal surfaces, are considered to have immunological defence systems. A variety of defences against infection specific to the vagina and uterus are known, as are the means by which certain pathogens, like chlamydia and gonorrhoea, evade these defences. It is known that the immune system can clear bacteria and sperm from the uterus and it is thought that immune cells may be involved in menstruation. So, the lack of entrenchment of a female defence system against sperm-borne pathogens in evolutionary biology is partly a consequence of disciplinary gaps between evolutionary biology and immunology with respect to female biology.

What explains this gap? Part of the explanation concerns how functions relevant to female sexual activity and pregnancy are categorised in reproductive immunology and evolutionary biology. To see why, it helps to look specifically at the field of reproductive immunology. In reproductive immunology, investigations of female reproductive pathogen defence are marginalised by investigations focused on maternal–fetal conflict. This marginalisation is not simply due to a lack of entrenchment; it is due to a failure of hypotheses about female pathogen defence to apply to sex and pregnancy *simultaneously*. There is a peculiar disconnection between sex and pregnancy in immunological accounts of women’s defences. Immunology deals with pathogen defences relevant to sexual activity as part of the general study of mucosal defences. *Reproductive immunology*, on the other hand, focuses mainly on the maternal–fetal relationship, with an emphasis on maternal–fetal immunological conflict. This means that pathogen defence is most present to one’s mind in the context of sex; maternal–fetal conflict is most present to one’s mind in the context of reproduction.

This categorisation of certain immunological functions as belonging to the realm of sex or reproduction has special consequences for our understanding of menstruation. Because menstruation is classified implicitly under the rubric of ‘reproduction’, it falls outside the immunological focus on sexual activity in questions of pathogen defence. However, menstruation – defined negatively as the failure of implantation – also falls outside the category of maternal–fetal conflict in reproductive immunology. The end result is that menstruation has no immunological home: it does not belong to the context of sex in which pathogen defence is the concern, nor does it belong to the context of reproduction where maternal–fetal conflict is the concern. In immunology, then, the disassociation of sex and reproduction plays an important role in the failure to consider seriously the hypothesis that menstruation is a defence against pathogens. The same problem exists in evolutionary biology, wherein most considerations of pregnancy concentrate on parent–offspring conflict and ignore other interesting evolutionary issues raised by viviparity.

What explains the disassociation of sex, pregnancy, and menstruation in these areas of biology? A powerful explanation is that cultural assumptions and biases about women’s sexuality, motherhood, and the maternal–fetal relationship affect the construction of categories and channel information into these categories. It is important to be able to say here that evidence is understood *androcentrically*. Androcentrism structures immunological investigation of women’s immune capacities, with the consequence that seemingly obvious hypotheses never make it to the bench. If immunology were not affected by gender bias, it might have room for the hypothesis that menstruation has an immunological function, or perhaps *more* than one – pathogen defence and shedding immune cells prepared for blastocyst implantation – a delightful surprise after a history of neglect! It is not enough to show that hypotheses like Profet’s lack entrenchment: we want to know *why* the functionality of menstruation is poorly entrenched in evolutionary biology and immunology. For this, a larger system of biases must be examined and talk of ideological filtering is empirically warranted. Clough’s argument could be strengthened, then, by developing a more complete account of how non-representational ideological filters might function in pragmatist feminist critiques of science. Understanding filtering in non-representational terms is preferable to eliminating talk of bias-filtering altogether.

Finding ways to demonstrate fairness and critical self-reflection that also disable charges of scepticism and weakness is an important philosophical task. Experts in values – humanities and social science scholars – will have an important role to play in this for, as Clough contends, values are as much a part of the web of belief as scientific facts and just as amenable to empirical critique. Feminist critics of biology, however, have failed to articulate adequately their views on representationalism and scepticism and thus are vulnerable to Clough's criticisms. It is important to rid feminist critiques of science of representationalism where it exists and the appearance of representationalism where it does not. Moreover, feminist critics of science should more carefully articulate their reflections about fair reasoning and the effect that bias may have on their own work. However, in the absence of direct engagement of the metaphysical issue by feminists such as Bleier, it is difficult to identify representationalism as the key source of their sceptical-sounding remarks. But regardless of whether or not Clough identifies the source correctly, she exposes a serious problem, shows that feminists should address it, and introduces a promising solution. Her contribution to the clarification and restructuring of feminist science studies is thus substantial – and feminist critiques of science stand to improve greatly in response to her prescriptions.

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In *Beyond Epistemology*, Sharyn Clough argues that feminist science studies needs to reject epistemology because it continues to be plagued by the spectre of sceptical concerns with representationalism. Clough states that “within representationalism, beliefs are conceived of as representations of their objects ... [and] these beliefs are described as the subjective end-product of human sensory processing” (Clough 2003: 12). Furthermore, representationalism assumes a separation of the knower from the world that they know. Clough also asserts a positive thesis: in the move beyond epistemology, feminist science studies needs to employ an empirical

approach. She defines 'empirical' as the "uncontroversial view that any investigative project involves a comparison of the knowledge claim in question with our ongoing ideas theories and with our experience of the world" (Clough 2003: 15).

In my review of Clough's book I first question whether pragmatism moves beyond epistemology. I argue that the pragmatist project should not be viewed as a rejection of epistemology *per se*, but as a rejection of a certain type of epistemological project. Furthermore, a pragmatist epistemology provides us with an epistemology of knowing and doing and reorients the epistemological project instead of rejecting it. Second, I question the wisdom of moving beyond epistemology and argue that there are important reasons for continuing to do epistemological work. The recent projects in epistemology of ignorance have made this quite clear. Third, there are areas where I think Clough's criticism is on target, most importantly in the need to do more work in the analysis of specific scientific projects.

Pragmatism has long been concerned with representationalist epistemology. For example, in John Dewey's *Knowing and the Known* (Carbondale, IL, 1949) he referred to the "epistemological phase of modern philosophy" in which objects are "taken to exist on [their] own account prior to inquiry" (Dewey 1949: 324) and argues against the 'known' as mere, unmediated representations of objects separate from knowers. He further argued against representationalism stating that "epistemological magic is required to reveal" how the knower, utterly separate from what is to be known, 'achieves' its knowing (Dewey 1949: 83).

Like Clough, classical pragmatists want to move beyond epistemology, but they don't want to move beyond all of epistemology, just a certain type. What Clough refers to as representationalist epistemology, Dewey referred to interchangeably as 'historical epistemology', 'modern epistemology', and 'epistemological realism' and sometimes just 'epistemology' in scare quotes. In *Knowing and the Known*, Dewey argued that the problems of 'representationalist' epistemology have plagued philosophy for at least two centuries and that "no such problem was urgent in either ancient or medieval philosophy" (Dewey 1949: 324) even though they were engaged in theorising about knowledge in these periods. He also stated that "as far as this word [epistemological] directly or indirectly assumes separate knowers and knowns (inclusive of to-be-knowns) all

epistemological words are ruled out under transactional procedure” (Dewey 1949: 293). I take this quote and Dewey’s distinctions between different types of epistemological projects – those that need scare quotes or an additional label of ‘modern’ or ‘historical’ and those that don’t need these – to imply that a certain type of epistemology is the problem and that an epistemology that does not directly or indirectly assume separate knowers and knowns (in other words, a pragmatist epistemology) is desirable.

Furthermore, I take Dewey’s argument for transactional philosophy to be one based on epistemological concerns. In *Knowing and the Known* he described transaction as “[t]he knowings–known taken as one process” and rejected the “interaction” of dualistic theories of knowing (read: representationalism) as having viewed this process as being “separate components, allotted irregular degrees of independence”, thus seeing knowing and knowns as two separate entities” (Dewey 1949: 304). Dewey did at times hesitate to use the term epistemology but did not hesitate to engage in epistemology. *Knowing and the Known*, *Experience and Nature* (New York, 1958), and *Quest for Certainty* (New York, 1929) were many things, one of which being that they were epistemological projects. For Dewey and many of the classical pragmatists, among them James, Locke, and Mead, epistemology was a political and social project rooted in the methodology of experimental inquiry that demanded an engagement with the material world. Not to have engaged in their reformulated pragmatist epistemology would have meant losing the import of the relation between knowing and doing that was so essential for social and political change and so important for knowing and doing ‘better’.

Pragmatism took a clever route by never putting itself in the position of having to answer to the sceptic because the pragmatist dismissed the traditional representationalist conundrum that provokes the sceptic and begins with a different set of epistemological questions. In *The Will to Believe* James asked what are the practical consequences of believing *x*. Dewey stated in *Experience and Nature* that “[p]hilosophy must explicitly note that the business of reflection is to take events which brutally occur and brutally affect us, to convert them into objects by means of inference to their probable consequences” (Dewey 1949: 325). The pragmatist project engaged in epistemology, but did so in its own way and on its own terms with assumptions that rested upon embodied, embedded

knowers that transact with a world to generate meanings that have real consequences that have to be lived with.

One problem with Clough's text is that she doesn't look to classical pragmatism to guide her in her pragmatist project. As Charlene Haddock Seigfried points out in *Pragmatism and Feminism* (Chicago, 1996) many thinkers have made the mistake of assuming that the important aspects of pragmatism have been "assimilated into the very difference agendas of Willfred Sellars, W. V. O. Quine, Nelson Goodman, and Hillary Putnam" as well as Richard Rorty, while in reality they reject many of the most important tenets of classical pragmatism, most notably the intimacy between knowing and doing, i.e. pragmatist epistemology, and "the political and social dimension of pragmatism" (Seigfried 1996: 18). Unfortunately Clough's pragmatism is derived from these sources, with the addition of Donald Davidson and not those of classical pragmatism.

It does not behoove feminist science studies or pragmatism to give up the business of doing epistemology or asking epistemological questions. First, as I hope I made clear above there is more to epistemology than representationalist epistemology. Furthermore, there are more epistemological questions than the closely related ones that Clough sees as most strongly characterising epistemology: "By which epistemic criteria might we adjudicate between competing knowledge claims" (Clough 2003: 20, 120) or "Which epistemic criteria have we been trained to use in our adjudication of competing knowledge claims?" (Clough 2003: 26). Clough is also critical of developing normative criteria for adjudicating between knowledge claims (Clough 2003: 15, 93); so questions about how do we 'know better' are also off limits. But, there are other important epistemological questions such as 'What don't we know?', 'Why don't we know?', and 'Who knows?', to suggest just a few. Clough might be able to argue successfully that these questions fall under the realm of 'By which epistemic criteria might we adjudicate between competing knowledge claims?' or 'Which epistemic criteria have we been trained to use in our adjudication of competing knowledge claims?' If she can, this serves to show that these are still important questions. Who knows 'what don't we know?' and 'why don't we know?', even if they are questions about competing knowledge claims, have a significant stake in the link between epistemology, science, and ethics that cannot be abandoned by

feminist science studies or by anyone who is interested in libratory projects. The importance of these questions is best illustrated in the recent emphasis on epistemology of ignorance projects.

Charles Mills coined the term “epistemology of ignorance” in *The Racial Contract* (Ithaca, 1997). An epistemology of ignorance is an “*inverted epistemology . . . a particular pattern of localized and global cognitive dysfunctions (which are psychologically and socially functional), producing the ironic outcome that whites will in general be unable to understand the world they themselves have made*” (Mills 1997: 18, italics in the original). Mills argues that ignorance can feel similar to knowledge because it provides a worldview that is cohesive (“psychologically and socially functional”) with expectations of what the world is like. Mills uses this epistemological lens to analyse what we know about race and the racial contract in the U.S. Without this epistemological underpinning, his analysis would be impossible.

In “Coming to Understand” (*Hypatia*, 2003) Nancy Tuana employs Mills epistemology of ignorance to study the scientific and social norms surrounding what we know about female sexuality and female orgasm. In Robert Proctor’s *Cancer Wars* (New York, 1985) he utilises the epistemological aspects of ignorance, what he calls ‘agnatology’, to study *what* we claim to know about causes and cures of cancer as well as *who* claims to generate this knowledge. In each of these texts the insights from epistemology are what drive the critical analysis because it is the epistemological lens that makes clear what the scientific, social, and political stakes are in what is taken to be knowledge and what is intentionally kept from being known.

Pragmatist epistemology makes clear that theorising about knowledge and doing it as embodied, embedded, connected knowers that recognise the political stakes involved in getting to label who knows, how she goes about knowing, and what she should and does know is too important for us to relinquish. The overarching pragmatist project is an epistemology that is produced by material bodies in transaction with a material world confronting the epistemological problems that are characteristic of political, scientific, and social discourse and action. Clough is envisioning epistemology too narrowly and is not seeing the other avenues that epistemology has taken through pragmatism and feminist science studies.

Furthermore, to give up on epistemology is to give in to the demands of mainstream Anglo-American philosophy that frequently seeks to view feminist science studies as not being legitimate philosophy. Moving beyond epistemology would thwart the efforts of much of feminist science studies to find a space outside mainstream philosophy and to generate other ways of doing epistemology. No one philosophical camp should be able to narrowly circumscribe epistemology, which has been historically understood as the study of knowledge.

I think Clough is on track on at least two counts. She is right to ask feminist science studies to stop answering to the sceptic. There is too much important work to do in the field to have the spectre of the sceptic continually dogging feminist science studies. Feminist science studies needs to continue its move beyond representationalist epistemology and engage in epistemology that does critical work. The pragmatist insight that we can rework the ground rules of the epistemological project and move it from a dead-end, philosophically impotent project to a tool for social, political, and scientific change and progress is exactly what we should be doing, whether or not we are pragmatists.

Second, there does need to be an emphasis on empirical analysis in feminist science studies. Clough does an excellent job of providing a model of empirical analysis in her final chapter on the evolutionary basis for menstruation. It is insightful, interesting and pulls in a variety of rich resources. Unlike Clough, I do think that there is still a significant amount of empirical work being done in feminist science studies, though in some cases they do so in texts that take on some of the epistemological projects that Clough is concerned about. For example, Helen Longino uses case analyses in *Fate of Knowledge* (Princeton, 2002) that are similar to the type that Clough is advocating. However, she does seek to answer epistemological questions of the type of which Clough is critical. On the other hand, as Clough points out, there are works like Anne Fausto-Sterling's now classic *Myths of Gender* and her more recent *Sexing the Body* that are generated by empirical research and only address epistemological questions through the back door of empirical research.

Clough's *Beyond Epistemology* is an important addition to work in feminist science studies. It provides readers with a rich array of resources to begin asking questions about the project of

epistemology and motivates the reader to think more critically about her discipline. A pragmatist analysis of the methodology of feminist science studies may not move us beyond epistemology, but it will certainly help those in the field do it better. Furthermore, the call for a more pragmatist science studies is critical for keeping feminist science studies moving forward.

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By Catherine Hundleby

Sharyn Clough's *Beyond Epistemology* brings into focus important issues for feminist philosophy of science, and proposes a direction that aims to resist the pitfalls of representationalist metaphysics by abandoning epistemology's traditional concern with justification. Her project aims high in trying to supersede several decades of feminist epistemology, and although the direction of her positive project deserves support, I suggest there are still resources for her project in feminist epistemology. In particular, feminist standpoint theory evades the epistemological landmines that concern her, and yet retains a functional notion of epistemological justification.

Clough addresses classic pieces of feminist theorising about science, such as those by Keller, Harding, and Longino. In these texts she finds two connected elements of traditional philosophical epistemology: the idea that the development of general standards that would qualify beliefs as justified is the central business of epistemology; and the understanding of scientific justification by epistemologists as accordance with a set of general standards that hedge against scepticism, standards that provide an account of how the world can be accurately represented.

The search for such general standards for representation is the underlying project that Clough thinks feminists should avoid. She maintains that feminist engagement with the representationalist metaphysics underlying epistemology both necessitates the search for general standards *and* ensures that the standards can never be

met. If we stay within the confines of the representationalist model, then accuracy in representation – for instance achieving the objectivity that is the standard of justification in objectivist epistemology – will always elude us. We can never be assured that our standards are completely correct, an assurance needed to count them as justified. However well-established our standards for filtering out the unjustified from the justified beliefs may be, failure of those standards is always possible. The potential for them to be wrong or inadequate to new purposes thus seems to leave us vulnerable to scepticism. The failure to find a universal hedge against doubt thus dissolves objectivism into relativism. To the relativist, knowers have various frameworks for understanding that determine the content of their beliefs, and provide competing forms of justification.

However, by drawing on the work of Davidson, Clough argues that the notion of a filtering process that underlies both objectivism and relativism is incoherent, because the distinction between conceptual schemes and empirical beliefs is false. The value-laden schemes that structure and organise our beliefs are themselves also empirical beliefs. Failing to recognise this continuity hinders feminist attempts to fully avoid the traditional debate between objectivism and relativism – an avoidance that would recognise the success of human cognitive practices, such as science, yet also substantiate the value of (at least some) feminist perspectives. Instead of seeking universal epistemological standards, Clough argues that science studies – feminist and otherwise – must accept local empirical practices and standards, and use these to evaluate/test competing accounts of the world.

I suggest that Clough's assumption that feminist epistemologists have followed the representationalist mainstream in considering questions of justification in science to be 'simply a matter of testing', is mistaken. Clough perceives and contests a preoccupation in feminist epistemology of science with "detecting how the filter of culture intervenes between the world and scientific knowledge" (Clough 2003: 92). The idea that theories are justified insofar as they are free from moral and political content and that the main goal of science is the testing of theories – for objectivity, or empirical adequacy, or accuracy – is a logical positivist model of scientific practice that most feminist epistemologists have moved beyond. Reading accounts of justification as accounts of filtration, and identifying justification with testing, as Clough does, interprets

feminist epistemology through the lens of logical positivism. The positivists maintained that social, ethical and political values are necessary for heuristics – that is for generating theories in the context of discovery. So-called ‘non-cognitive’ values get filtered out and eliminated through testing, thus distinguishing the surviving beliefs in the context of justification.

The distinction between discovery and justification tends to be rejected in feminist philosophy of science. Instead, feminist epistemologists, such as Harding and Longino at least, try to negotiate the view that social and political values are as necessary a part of testing as of heuristics. That values continue to be present in tested theories, according to feminist epistemologists, suggests to Clough that “given that some scientific views *are* chosen over others, these choices must be *relative* to a political ‘worldview’” (Clough 2003: 93). This apparent relativism in feminist epistemology seems, to Clough, to reveal an assumption that there is a dichotomous relationship between interpretation and experience, the same assumption that undermines objectivist accounts of scientific knowledge that feminists resist. Objectivism falters under Davidson’s criticism that it relies on an incoherent notion of conceptual schemes. The model, in both relativism and objectivism, “whereby political values are conceived as nonbelief schemes through which the flow of sensory evidence for our empirically based beliefs is filtered” (Clough 2003: 14), is the view Clough describes as ‘representation-ism’. It creeps into feminist epistemology of science, according to her, via the Quinean underdetermination thesis, especially the view that for any theory that fits the available evidence, another theory might fit equally well (Clough 2003: 93).

An initial exception to this characterisation of feminist epistemology must be Longino, if not in her writings that Clough criticises, at least in “Subjects, Power and Knowledge” (Longino in Alcoff and Potter, *Feminist Epistemologies*, New York, 1993), where Longino raises much the same criticism against feminist ‘standpoint theorists’. Longino argues that in order for a perspective to be privileged, as standpoint theorists argue, there must be a prior perspective from which that privilege is justified. The implicit dependence on a prior justification opens the door to scepticism according to Longino, and also according to Clough (2003: 102), who echoes Ricard Rorty. Doubts arise about the validity of any perspective being used to criticise other perspectives, and cascade into a situation of universal

doubt (Longino 1993). Further classic problems with feminist standpoint theory also attract Clough's attention, and she rehearses the standard critiques. Yet, the similarities between her main criticism of feminist standpoint theory and Longino's critique reveal the thinness of her criticism of Longino. They clearly share the same concern about the tenacity of the problem of scepticism, not only for traditional epistemology but also for feminist epistemology. Both insist on the inadequacy of any particular 'logic of testing' to filter out the social aspects of theory generation.

In defence of standpoint theory, we must consider that a feminist perspective is not automatically privileged: it only tends to be advantageous because it provides alternatives to already available androcentric perspectives. Generating different theories provides for meaningful testing. Processes of discovery, not processes of testing, are the central concern. Ironically, the focus on discovery is best recognised by Longino (1993), who finds this to be a *problem* with standpoint theory. Although she herself undermines the distinction between the context of discovery and the context of testing, she maintains that epistemic normativity can operate only in regard to testing. Prescriptions for understanding cannot be applied to the development of beliefs, but only to sorting out which among contending beliefs are to be preferred. Because the prescriptions of standpoint theorists lie in the context of discovery, they are to Longino merely specious, not effectively prescriptive or normative but at best merely descriptive of feminist practices.

Yet, as post-positivist philosophers of science generally accept, theories cannot be tested by direct comparison to the world, to uninterpreted experience. Rather, their empirical adequacy is tested relative to competing theories. Where no nonsexist theories are generated, sexist theories will survive the most rigorous available scientific assessment (Kathleen Okruhlik, "Birth of A New Physics Or Death of Nature?" in Okruhlik and Harvey, *Women and Reason*, Ann Arbor, 1992). To increase the rigour of testing, and gain epistemic advantage, feminist perspectives on scientific subjects need to be developed, standpoint theorists maintain. Including feminist theories among those being tested provides an advantage – one that we neglect if we haven't developed feminist accounts.

Longino's mistake of assuming that epistemic prescriptions apply only to testing is repeated by Clough in her assumption that all justification, and any evaluation of standards and practices, must

occur in the context of testing. Thus, both philosophers overlook that feminist standpoint theorists attempt an alternative conception of justification as operating in the context of discovery, via recommending certain heuristics, in particular the generation of feminist perspectives. The role of feminist theories in standpoint (and other feminist) epistemology, according to Clough, is the removal of politically undesirable elements of science, providing a politically superior bias (Clough 2003: 98). However, the superiority of any particular set of political values is not the issue for feminist standpoint theorists. Advantage derives from heightened consciousness *about* political values. Such a consciousness can be understood in epistemological as well as political terms, and need not flirt with global scepticism because the value derives from prospects for discovery and from generating scientific theories and problems, rather than from previously generated theories and accepted problems.

Harding, the most prominent and persistent advocate of feminist standpoint theory, insists on ‘starting thought’ from marginalised lives, including women’s lives (e.g. *Whose Science?*). Moreover, just like Clough, her specific criticism of popular ideals of objectivity concerns the obsession with methods of justification understood as testing (“Strong Objectivity: A Response to the New Objectivity Question”, *Synthese*, 1995). Although Harding does try to reformulate a standard of objectivity (‘strong objectivity’) with this in mind, how universal she intends it to be is not clear, so she may not be implicated in the representationalist metaphysics that Clough contests. The clearer difference is that standpoint theorists, such as Harding, deny the easy access to subjugated viewpoints that Clough assumes. Rather, they insist that responsible scientists must aid the development of alternative starting points for science.

Against Clough (in turn, Rorty) and Longino, I wish to press the point that standpoint theorists do not recommend *logically* prior perspectives and do not look for justification to be independent from social context. After all, logical empiricism is not the precursor to feminist standpoint theory; historical materialism is, and *historical priority* provides, the justification for feminist inquiry. Our material circumstances provide the possibility of developing feminist accounts, and our historical circumstances reveal this strategy to be advantageous. Because we live in a sexist society in which only androcentric theories tend to be generated, cognitive advantage accrues from taking the opportunity to develop

competing feminist accounts. This socially-situated contingent priority differs from logical priority that can be iterated indefinitely, and for which adopting the prior perspective would need to be justified by a still prior perspective, and so on (Longino 1993). An infinite regress might arise in principle, yet it does not do so in practice for standpoint theory. That we have feminism and feminist science, and might produce more of both, historically substantiates a material, rather than logically projecting an ideal, feminist standpoint (Hundleby, "Where Standpoint Stands Now", *Women & Politics*, 1997). Feminism isn't in our heads, but in our heritage and in feminism's role in the social world as a resource for inspiring and developing science. Feminist standpoint theorists' claim that feminism provides a cognitive advantage rests on a concretely prior circumstance and our experience with it.

Clough herself appeals to historical situation for her positive project. She argues that history, or past empirical evidence, reveals through inductive inference the error of ignoring feminism: "to ignore the role of women is to get the human story drastically wrong" (Clough 2003: 116). Why defer to history? Feminist standpoint theorists retain the Marxist insight that a perspective derived from oppressed experiences can provide a cognitive advantage. The epistemology itself is to be understood as part of the material context for generating knowledge. So the history of political subjugation and its effects on cognitive practice guide the epistemological theorising.

In particular, the cognitive practices of women reflect women's greater focus on caring for people's bodies, according to standpoint theorists. Yet, this focus is not exclusive to women, Clough aptly points out, but is common to various people in marginalised positions. She further criticises Harding's ambivalence about the significance of gender relative to other axes of oppression. Harding recognises the importance of multiple and intersecting forms of oppression yet she prioritises gender by treating it 'as given' in a way that she rightly refuses to treat race. This inconsistency certainly demands remedy.

Nevertheless, the danger of error in science, according to feminist standpoint theorists, arises not only from ignoring theories that have been generated from a feminist perspective, as Clough recognises: it is from neglecting the need to develop feminist perspectives. This heuristic helps to reveal political values in competing theories, even though (and partly because) it is itself politically self-

conscious. A feminist perspective, like any emancipatory perspective, accounts for its own socio-political circumstance. Standpoint theorists' championing of self-consciousness no longer depends on psychoanalytic accounts of feminine thought, an association that has turned many feminist theorists, including Clough, away from standpoint theory. Holders of standpoints are understood in more localized, sociological terms, especially in recent years, and are less portrayed as women. Instead, they are more clearly understood to be feminists (Hundleby 1997).

The feminist epistemologists Clough addresses may be less committed to representationalism and to viewing justification as testing than she imputes. However, she is correct that the ambiguities in their writings need to be addressed and remedied. Even Harding, who embraces a degree of ambiguity, doesn't seem to mean to include the problems identified by Clough, and the resolution of the inconsistencies might well follow the lines Clough suggests. Likewise, Longino clearly wants to deny that justification filters out social and political values, and is thus distinct from the 'social embeddedness' of theory generation. Nevertheless, her focus on testing standards does occasionally resonate with representationalist metaphysics.

Bringing feminist science studies into dialogue with Davidson and Rorty is certainly long overdue. Yet, as I have argued, Clough's initiative resonates with central aspects of feminist standpoint theory, and so with a form of epistemological justification, though not a form that separates justification from discovery. So, I suggest that the nature of justification still has potential as an issue for science studies – so long as we abandon universalist ambitions.

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Author's Response

By Sharyn Clough

It is extremely gratifying to have such careful and sympathetic attention paid to the arguments I offer in *Beyond Epistemology* and

I want to thank the contributors for their commentaries. I present my response by way of a brief overview of my main arguments, highlighting and clarifying those aspects that the commentators found troublesome, or in need of further discussion.

Beyond Epistemology begins with two conditionals that play a key role in my overall argument. The first conditional is that, if we feminists are concerned about the ways that science contributes to an oppressive *status quo*, then the best place for us to put our energy is in local, empirical examinations of particular scientific studies, rather than in general philosophical examinations of the epistemology of science. The antecedent identifies a very particular audience for my anti-epistemological arguments, namely feminists engaged in science studies who are committed to reducing the negative impact of science and technology on people unjustly marginalised by a variety of intersecting social forces, including sexism, racism and classism.

The second conditional arises out of the very restricted definition I give to the sort of epistemology I want those of us feminists engaged in science studies to avoid. So let me offer a quick recap of that definition here. I am particularly concerned about the sort of epistemology that involves the general specification of, and search for, normative properties that would reliably link our 'inner' beliefs with the 'external' world that those beliefs are supposed to represent (Clough 2003: 12). This epistemological model is recognisably Cartesian, and, to use the language of Davidson, it is 'representationalist' insofar as the model involves an ontological dualism between the internal realm of the mental and the external realm of the physical, and is motivated by the sceptical worry that if we cannot identify the requisite normative links between internal beliefs, and the external world that those beliefs are supposed to represent, then we have no way to tell whether our beliefs are true. A related relativist worry motivated by this representationalist picture is that if we cannot identify the requisite normative links, then we have no objective criteria to decide between competing beliefs. The history of epistemology, as I have identified it, can be read as a failed attempt to alleviate either the sceptical or the relativist worry.

With this definition of epistemology established, the second conditional of my argument comes into play. I argue that, if we feminists engaged in science studies appeal to representationalist epistemology, then we will be confronted with the intractable

problems of scepticism and relativism that can be used against our own reports of the oppressive features of science and our prescriptions for change. I recommend that feminists engaged in science studies stop appealing to the sorts of representationalist epistemology specified in the antecedent.

My negative recommendation is given more bite when accompanied by the neo-pragmatist arguments that I borrow from Davidson and Rorty to show that feminists don't, in fact, need to appeal to representationalist epistemology to make our case about the oppressive features of science and to offer positive recommendations for scientific change. What I take from Davidson and Rorty is their careful and appropriate attention to our language practices, an attention that allows them to conceive of the relationship between knowers and the world in a more naturalistic and ontologically holistic fashion than that envisioned by the representationalist model. Unlike the representationalist model, their alternative pragmatist conception of knowers and the world does not involve an ontological split between the external, physical realm and the internal, private realm of knowers and their beliefs. For Davidson, in particular, knowers and the beliefs they hold are *things in the world*. This is not to say that there is no difference between, say, a rock, and a belief about that rock, but, rather, that to push for a robust ontological difference between the two is to accept some Cartesian metaphysical assumptions we have no good reason to hold. Davidson and Rorty make compelling arguments that, to have beliefs at all, just is to be practically (e.g., linguistically) enmeshed in a physico-social relationship with the world around us, including other knowers. Our beliefs have no content, separate from, or prior to our practical engagement in the world (this latter point is made especially well by Davidson in "Three Varieties of Knowledge" (reprinted in *Subjective, Intersubjective, Objective*, Oxford, 2001). Insofar as knowers, what they know, and the world their knowledge is about, are ontologically continuous, then any and all of these elements are amenable to naturalistic, empirical investigation. In the book I provide more details of this pragmatist model (see, especially Chapters. 6 and 7), but the main point I take from it is that the model frees us from the need to provide a general epistemological specification of, and search for, normative properties that would reliably link the inner, private realm of belief, with the external world those beliefs are about. Insofar as these

realms are ontologically continuous, there is no ontological gap for representationalist epistemology to bridge. If Davidson and Rorty are right, and the content of our beliefs is established through, and continuous with, our practical, empirical engagement in the world, then we have no reason to entertain globally sceptical and relativistic worries about the truth of our beliefs (Clough 2003: 13–14, and Chapter 6).

Of course, local, fallible worries about truth remain, for feminists engaged in science studies, as for everyone. So while I have argued against the utility of general epistemological questions such as *By which normative criteria might we adjudicate between competing knowledge claims?* I have emphasised that we must still answer local questions about the truth, evidential justification or objectivity, of any *particular* knowledge claim or set of claims (Clough 2003: 15). And, as Nancy McHugh argues, we must still answer local questions about the sorts of claims whose truth some of us consistently ignore. In a contemporary U.S. setting, the answers to these questions will likely involve identifying the ways in which racial and sexual privilege buy some knowers the luxury of ignorance.

But, as McHugh and other readers have rightly asked, wouldn't we want to call these more local questions, while not 'representationalist', at least 'epistemological', broadly speaking? In an attempt to avoid confusion, I chose not to call them 'epistemological', instead I referred to these more local questions as 'empirical', where 'empirical' was meant to have a broad referent, including "any investigative project [that] involves a comparison of the knowledge claim in question with our on-going theories and with our experiences of the world" (Clough 2003: 15). I have come to regret this typological decision because it does not seem to have clarified the focus of my concern as I had hoped. Rather, it has led many able readers to construe my criticisms of epistemology too broadly. I think this is at least partly what has happened with McHugh's reading of my work, but she is not alone and I clearly need to rethink how best to make the case. I also need to articulate better what sorts of epistemology I think should remain, once the representationalist elements have been appropriately displaced.

On an historical note, in earlier versions of the book manuscript I made a distinction between 'Epistemology' (as the representationalist epistemology I criticised) and 'epistemology' (as the more

local, empirical projects that might still contain broadly epistemic themes, which I encouraged), but a reviewer rightly found the upper and lower-case distinction too distracting. So a satisfactory solution to the problem of nomenclature remains to be found. I appreciate reading in McHugh's commentary that Dewey, too, struggled with decisions about nomenclature. With McHugh and Dewey, I agree that there are clearly some notions of epistemology that feminists should embrace, in particular, as McHugh describes it, the pragmatist's epistemic focus on "embodied, embedded knowers that transact with a world to generate meanings that have real, consequences that have to be lived with". (And I think that, like the classical pragmatists such as Dewey, the contemporary pragmatists, especially Davidson and Rorty, provide a good framework for discussing exactly these issues.)

A final prescriptive feature that follows from my arguments for a pragmatist, naturalistic conception of knowers and their worlds, and against a representationalist conception, is that, in addition to the ontological holism between knowers, their beliefs, and the world, I advocate a meaning holism between different sorts of beliefs. Following Davidson, I argue that the process by which our beliefs are formed is the same for our beliefs about 'facts' as for our beliefs about 'values' (Clough: 39; and Chapter 6). Building on the pragmatist model, our communication practices give us good reason to support the view that the meaning of beliefs, about both facts *and* values, follows content, and, in the most basic cases, at least, content is established through an external, empirical process of triangulation between speakers and their environment. This focus on communicative practice rightly disrupts the representationalist notion of inner minds clouded with subjective values, filtering the data from the external world to produce more or less objective factual beliefs. Understood holistically, beliefs about both facts and values have empirical content, or must be sufficiently semantically linked to more basic beliefs that do, and it is by comparison between this content and our ongoing experiences that we can, ideally, test the objectivity of any of our beliefs.

This last point is crucial for feminist science studies that highlight the important role that beliefs about values, both good and bad, play in science. Once the role of values is recognised, feminists need to be able to explain how it is that some values (such as feminist values) will lead to more objective science than will others

(such as sexist values). I argue that for those feminist science studies that appeal to representationalist epistemology it is much harder, if not impossible, to articulate how some values could be more objective than others; harder, if not impossible to avoid a self-defeating relativism about the truth of feminist or any other values.

I am particularly impressed with Elizabeth Andersen's examination of values that Elizabeth Potter discusses. But I am still unclear about the utility of using 'emotional experience' as evidence for values, and whether and/or how Andersen thinks that values can in turn function as evidence, but I am very sympathetic to her overall approach.

With respect to my claims about the empirical nature of values, I've recently been working with Hume's description of the empirical realm as 'matters of fact', which he, of course, contrasted with 'relations of ideas'. Recall that matters of fact are beliefs about the world, and that their truth is an empirical, contingent matter, established through fallible processes of induction, that is, by testing the matter of fact against the ever-changing empirical realm. Relations of ideas, in contrast, are necessary truths, such as, for Hume, the rules of arithmetic and geometry.

Very little that once was thought to be a relation of ideas now remains in that category. Matters of fact are what matter. Dewey, Wittgenstein, Quine, and Davidson devoted lifetimes to arguing this point in various ways. Although Hume himself offered a number of contradictory and controversial views about the place of beliefs about values, if the pragmatic analysis I offer is right, then, insofar as beliefs about values express *anything* – that is, insofar as they have meaningful content – then beliefs about values have been acquired through the usual belief-acquisition process of practical engagement with the world through communication with others.

Identifying something using normative, value terms, such as 'good', or 'sexist', or 'liberal', is to classify that thing as belonging to a category, to assign it a property, just as much as it is to identify something as 'blue' or 'hard' or 'wet'. Importantly, we can be right or wrong in assigning various properties, be those properties from the 'factual' or the normative realm. Wanting something to be blue, does not make it blue. Similarly wanting something to be good or liberal does not make it so. Beliefs about values are empirical, then, in the sense that they are matters of fact arrived at through experience in the world. Their truth is contingent, and is

amenable to objective examination. (I was pleased to see that Davidson makes similar claims in his 1995 essay “The Objectivity of Values”, which was not widely available in English at the time I wrote my book, but has since been reprinted in *Subjective, Inter-subjective, Objective*).

The arguments I make about the importance of local, empirical analyses of particular scientific studies – analyses that include the adjudication of values – leads Catherine Hundleby to wonder whether I have not inappropriately focused my discussion on the context of justification, traditionally understood as encompassing empirical processes of testing and adjudication, at the expense of the context of discovery that feminist standpoint theorists have rightly identified as key for discussing values in science. What I had hoped to show was that the distinction between the two contexts is spurious.

Hundleby is correct that I read much of feminist epistemology as maintaining a problematic split between the contexts of discovery and justification (see my discussion of Harding and Longino in my Chapter 8, pp. 129–130). I argued that “by employing a Davidsonian understanding of meaning, belief, and truth, we can collapse the distinction by viewing both discovery and justification as naturalized elements of the equation, which are both, in principle, constrained by beliefs about empirical evidence” (Clough 2003: 129–130).

My positive prescriptions are distinguished from a more traditional or logical positivist model in two ways: First, I argue that we can empirically test *both* our beliefs about values *and* our beliefs about ‘facts’, against the evidence of our experience and our ongoing theories. Second, I argue that, as scientists and science commentators, we can and should carry out this empirical investigation in *both* the contexts of discovery *and* justification, such that any distinction between the two contexts loses significance. (Indeed, I view my attempt to examine the values affecting the projectibility of functional predicates in our evolutionary accounts of menstruation as an example of an empirical investigation that combines both contexts.)

My main difference with Hundleby and the standpoint theory offered by Harding, in particular, is that, while I support the empirical investigation of the values affecting both the generation and the testing of a scientific theory, or comparison between

theories, I disagree with the notion of a standpoint that reduces the role of values, feminist or otherwise, to a ‘non-belief’ conceptual scheme that screens beliefs about data (or affects which data we decide is evidence – a clarification I return to in my response to Potter, below).

This conception of a standpoint as a filtering or organisational scheme, a collection of values that affects our resulting beliefs, but is not *itself* a collection of beliefs, has a number of troublesome effects. It places our values in a separate conceptual category from our beliefs about the world, such that beliefs about the world are available to objective, empirical analysis, while values are not. I think that Harding, Hundleby and I agree with how we want values, especially feminist values, to function in science but, I argue, the conceptual machinery of standpoint theory does not give us what we want.

In particular, the elements of representationalism that I see maintained in the standpoint account of values keeps the account vulnerable to charges of relativism. If we conceive of the feminist values informing our standpoint, not as holistically of a piece with beliefs about the world, available to objective adjudication, as I suggest, but instead as organisational filters for beliefs, then we are vulnerable to criticisms that we feminists use our standpoints to ‘sift’ through collections of scientific beliefs to choose those that suit our political agendas. What we want to be able to say, instead, is that the values that inform the epistemic starting point of a feminist scientist are themselves based on good empirical evidence; that is, these values fit better with our experiences of the world than do sexist values, for example, and therefore, insofar as those values are relevant to the empirical question under scientific investigation, the feminist scientist has an objective advantage. I don’t think that Harding’s standpoint epistemology allows her to say this.

Of course, I don’t think that Harding is alone here, and I argue that a number of classic essays and texts in feminist science studies make use of a version of the very representationalist epistemology that I’ve specified as problematic (Clough: Chapters 3–5). I begin with Bleier’s discussions of sexism in evolutionary theory and sociobiology (1984) and Hubbard’s influential essay “Have Only Men Evolved?” (reprinted in *Discovering Reality*, Dordrecht, 1983), and continue with the early and influential texts by Keller, Harding, and Longino.

Both Potter and Moira Howes argue that some of the feminist analyses of science that I discuss do not appeal to representationalist epistemology in the ways I've described. My process for detecting the presence of representationalism in any given feminist analysis involved asking two questions. Does the feminist analysis construe the facts (the empirical content, the data, the evidence) as separate from, because they are *filtered by*, values (political claims, conceptual schemes and/or worldviews)? And if it does construe values as filters for facts, does this indicate that the feminist analysis is invoking an *ontological* split between our internal, filtered, representations and the external world that is represented (this would be a red flag) or was the filtering language used merely as a metaphor describing the complex relationship between beliefs about values and beliefs about facts, making clear that the two sorts of beliefs are ontologically continuous? This latter usage would be fine, and in fact I think we safely use this sort of filtering language all the time, as when we want to acknowledge the fallibilistic nature of our own beliefs, including those of our value beliefs that act inappropriately as biases affecting the acquisition of new beliefs. I think Howes is right to note that Bleier and many other feminists use this filtering language to acknowledge their own biases, thereby modelling a laudable level of intellectual honesty. The trouble is that Bleier, in particular, mixes this more innocuous discussion of 'biases as filters' with what I think is a more metaphysically dangerous vocabulary that moves her beyond the acknowledgement of the fallibility of any given belief, to a Cartesian scepticism about belief acquisition *simpliciter*.

Indeed, there are a number of occasions where the bias-as-filter language leads feminist theorists to an unnecessary level of scepticism, e.g., Bleier's claim that the only justification for her own feminist criticisms of sociobiology is not that her arguments are better supported by the evidence but that her arguments better disrupt the status quo (1984: 13); and Longino's discussion of archaeology (1990). The bias-as-filter language has also led to an unnecessary level of relativism, e.g. Hubbard's claim that "every theory is a self-fulfilling prophecy that orders experience into the framework it provides" (1983: 47), and that feminist theories are no different, and Harding's claim that all social positions filter experience – there can be no privileged experience from which we will arrive at truth (1991: 149, 185). In these cases I argue that we have moved

from a proper attitude of fallibilism about our beliefs to a more globally sceptical and relativistic position, whereby values (both sexist and feminist) are construed not as beliefs but as filters that organise beliefs – a conceptual distinction that keeps us from treating our feminist values as the well-justified beliefs that they are.

Regarding my discussion of Longino in particular, Potter argues that Longino's analysis is not, in fact, representationalist. According to Potter, for any theory to be diagnosed as representationalist, it has to construe the data from the external world as completely unconceptualised or unanalysed prior to the internal filtering process, and, argues Potter, Longino does not construe 'data' this way.

It's true that I attribute to Longino the view that 'unanalysed' data are filtered through feminist or androcentric frameworks or schemes. But Longino does use this language on occasion, as for example when she claims that the data are 'dumb' and it is our conceptual frameworks that give the data voice as evidence (Longino 1990: 111). However, I think that one of the most important (and problematic) features of the representationalist model is that it views organisational schemes, such as our values, as non-beliefs that are conceptually separate from whatever beliefs the schemes are thought to be organising – whether the schemes are thought to be organising unanalysed beliefs about 'raw' data, about which facts to count as evidence, or about competing hypotheses, is not important. It's the fact that the organising schemes themselves are not viewed as beliefs available to empirical adjudication that is troublesome (and, if Davidson is right, incoherent).

I spent much less time than I should have discussing examples from feminist science studies that exemplify the local, empirical approach to beliefs about both facts and values that I prescribe, but, as I mention (Clough 2003: 17), there are a number of good examples available, including Schiebinger's *The Mind Has No Sex?* (Cambridge, 1989) Fausto-Sterling's *Myths of Gender* (New York, 1985), and a number of Wylie's essays collected in *Thinking from Things* (Berkeley, 2002).

I do try to provide some positive exposition, especially towards the end of the book (Chapter 8) where I discuss a debate within evolutionary biology about the function of menstruation. Here, I identify those features of the debate that could benefit from a pragmatist feminist analysis that is attentive to the social genealogies of the concepts involved (e.g., the values affecting theory generation

and testing) without invoking the relativism that is so often invited by representationalist approaches.

In my study of competing functional accounts of menstruation, I make use of Nelson Goodman's helpful pragmatist discussion of hypothesis confirmation, to describe how, in the competing functional models, different capacities of the female reproductive system are 'entrenched', and relative to these capacities, only certain functional claims about menstruation are 'projectible'. In the traditional evolutionary account of menstruation, the capacity of the female reproductive system for pregnancy is entrenched, and the capacity for sperm-borne pathogen defence is not. I argue that the lack of entrenchment of the capacity for sperm-borne pathogen defence might explain why Profet's model of menstruation as a contributor to pathogen defence is not projectible. I suggested that androcentrism might explain the difference in the entrenchment of these capacities, and the subsequent projectibility of the competing functional accounts. But I could find no feminist literature discussing how this might work (hence my tentativeness). Fortunately, Howes has now provided some of the details we need, by examining how numerous factors, such as disciplinary boundaries, influenced by androcentrism, are likely to affect the recognition of the female capacity for 'sperm-borne pathogen defence' as a complete system and the role of menstruation in that system. I find her discussion compelling and on-target.

There are of course many other insightful details from each of the commentaries that I want to address, but in the space allowed I have highlighted those that pushed me to think more carefully about my arguments. I am fortunate to have had the opportunity to do so, and again, I thank all the contributors for their engaging and generous commentaries.

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