THE EPISTEMOLOGICAL TIES THAT BIND: A PRAGMATIST CASE AGAINST FEMINIST THEORIES OF TRUTH AND KNOWLEDGE AND THE IMPLICATIONS FOR FEMINIST SCIENCE

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ABSTRACT

Feminist claims that scientific activity is intimately involved with the oppression of women, often identify aspects of the epistemology of science, or scientific method, as the primary culprit. In my dissertation I try to persuade my feminist colleagues that despite the important gains we have made through the criticism of science, our fairly recent investment in an epistemological critique is yielding diminishing returns. I begin by examining the epistemological reflections of a number of feminist critics of evolutionary biology, including Ruth Hubbard and Ruth Bleier. I then discuss the more general epistemological approaches of Evelyn Fox Keller, Sandra Harding and Helen Longino.

There are a number of features of feminist epistemology which make it ineffective as a method for adequately addressing the oppression of women by science. For example, feminist epistemological examinations of science have often involved essentialist claims about "women's" experience, and over-general conceptions of science as an institution. These problems are discussed throughout the dissertation. However, the principal focus involves the philosophical details of the feminist epistemologies themselves. Specifically, I focus on their reliance on a questionable model of human psychology, that I refer to as "representationalism."

My anti-representationalist approach is inspired by the work of two neo-pragamatist philosophers, Richard Rorty and Donald Davidson. For the purposes of this dissertation, representationalism is the view that beliefs are the subjective, representational end-product of a sensory process, in which the objects of our world are sensed and then screened through our subjective perceptual frameworks (the filters of our values, worldview, and language). My concern is that the representational conception of beliefs as filtered representations of the world makes coherent a global scepticism about the truth of our beliefs. Since, on this model, beliefs do not arise

from direct access to the world, then they can be radically wrong. All of our representations might be completely inaccurate because they are filtered through our perceptual apparatus, language, cultural worldview and/or theory allegiance. Following Rorty and Davidson, I argue that epistemological debates are premised on the coherence of global scepticism, and that, due to the nature of the representationalist model on which the debates are based, the battle with global scepticism is futile.

Surveying feminist contributions to epistemological debates in science, I show how these contributions invite the coherence of scepticism. In the end, each contribution either accepts scepticism, with relativist resignation, or attempts to defeat it with various claims to objectivity. While the latter response has proven futile, for both feminist and traditional epistemologists alike, the former response is equally problematic. Either way, issues of relativism and scepticism can be used against our well-justified claims that women are being harmed by science.

In the latter half of my dissertation I offer a pragmatic alternative based on Davidson's philosophy of language. Davidson argues that, on a non-representationalist model of language use, global scepticism is not a coherent option. His views undermine the motivation for participating in the epistemological debates that attempt to address scepticism as a coherent and ever-present concern. By undermining the motivation for these philosophical pursuits I hope to encourage my feminist colleagues to return to their important work in science and science criticism with the assurance that our concepts of "error" and "truth" are not always enemies that need feminist epistemological reconfiguration.

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Chapter 1: Introduction

Feminist claims that the oppression of women is systemic in science, often identify the epistemology of science as the primary culprit. Three of the leading theorists who make this case are Keller (1982, 1983 [1978], 1985, 1987, 1992a, 1992b, 1992c), Harding (1986a, 1986b, 1990, 1991, 1993b) and Longino (1987, 1990, 1993a, 1993b). I agree with their claim that scientific inquiry, across disciplines and laboratories, often serves to harm rather than help humankind.[1] Further, they are surely correct that not everyone has an equally likely chance of falling victim to this sort of harm, and that sexism is one of the best explanations of this inequality.

The important work by this "second-wave"[2] of Western feminists, has equipped yet a new generation of feminists with a range of critical diagnostic tools for the investigation of oppression in science. However, the more I investigate, the less certain I have become that an examination of the *epistemology* of science will help us further the goal of eliminating the harm science causes. I will argue this point throughout the balance of my dissertation, directly confronting the works of Harding, Keller, and

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[1] Harding enumerates examples in *The Science Question in Feminism* (1986a, 20 - 22). See also Tavris, *The Mismeasure of Woman* (1992), and Harding's *The 'Racial' Economy of Science* (1993a).

[2] The convention in contemporary Western feminist discourse is to identify the suffragette cause of the nineteenth and early twentieth centuries as the first major "wave" of what we would now call "feminist" political theorising and activism in the West, followed by a "second-wave" beginning in the late fifties and proceeding through to the seventies and eighties. *Hypatia*, the journal of women in philosophy, is featuring a summer 1997 issue on "third-wave" feminism.

Longino as well as a number of other recent feminist writings that link the oppression of women by science to certain conceptions of truth and method.[3]

I see my anti-epistemological thesis as a critique *internal* to work in feminism and science. The focus is not to persuade nonfeminists of the benefits of my feminist predecessors' work in this area. Instead, I will focus on persuading my feminist colleagues that despite the important gains made through the criticisms of science, our fairly recent investment in an *epistemological* critique is yielding diminishing returns.

The huge volume of the feminist science literature necessitates selective sampling in this regard. I begin my dissertation with some epistemological discussions representative of the tradition of Western feminist responses to (and within) evolutionary biology. My choice of *Western* feminist science critiques reflects my own exposure and training and, within the Western feminist tradition, evolutionary theory is one of the most frequent and familiar targets. After addressing the epistemological critiques of evolutionary theory, I then discuss the much more general epistemological criticisms made by Keller, Harding, and Longino. Each of these three theorists suggests that the oppressive aspects of scientific activity are best addressed,

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[3] A sample of the more recent feminist literature making claims of this sort includes many of the essays in Anthony and Witt's edited collection *A Mind of One's Own: Feminist Essays on Reason and Objectivity* (1993), *Feminist Epistemologies*, edited by Alcoff and Potter (1993), and *Feminism/ Postmodernism*, edited by Nicholson (1990). Other examples include essays from *Sex and Scientific Inquiry* (Harding and O'Barr 1987), *Feminism and Methodology* (Harding 1987), *Science, Morality, and Feminist Theory* (Hanen and Nielsen 1987), *Feminism and Science* (Tuana 1989), and *(Dis)covering Reality* (Harding and Hintikka 1983).

and remedied, by constructing new, feminist conceptions of epistemology. Their theories have received wide acceptance by those writing in the field.

In my view, there are a number of features of feminist epistemology which make it ineffective as a method for adequately addressing the oppression of women by science. One is that much of feminist epistemology is focused on the general institution of science and/or scientific method, as exemplified in feminist questions such as "Is the nature of scientific method masculine?" But surely a more urgent issue remains the identification and censure of the *specific* funding agents, scientists, and laboratories responsible for psychologically and physically *harming* people.[4] Feminist epistemologists often, erroneously, presume that "science" names a monolithic, homogenous institution, when, in fact, it includes a multiplicity of disciplines with a wide range of practitioners and normative practices.

A further problematic feature of many feminist epistemologies is their over-general approach to other categories of analysis, such as "women" and "men." These over-generalisations ignore important distinctions *among* women *and* among men, often assuming, for example, that the experiences of a dominant group of women are the experiences of all women. Also obscured are important similarities between women and men who share race or class or some other significant social stratum. Many

[4] My thanks to Edrie Sobstyl for stressing to me the importance of this point.

women and men have a common experience *as oppressors* or as victims of oppression, which cuts across a division by sex.

The final feature of concern I will discuss involves the philosophical details of the feminist epistemologies themselves. Specifically, I am concerned about their reliance on a questionable model of human psychology, that I will refer to as "representationalism." (Details of the model follow shortly.) Although the representationalist model is shared by both feminist *and* traditional epistemological projects, my own feminist political concerns incline me to focus my critical efforts on the former.

1.1 Epistemology defined

Before beginning my diagnosis and criticism of representationalism in feminist epistemology, a clarification of my use of the term "epistemology" is in order. In claiming that epistemology is an ineffective tool for feminists addressing the oppression of women by science, my criticisms are aimed at a set of epistemological *and* ontological questions which concern the justification of belief and the nature of truth, respectively. These two aspects are often collapsed in the feminist literature and referred to simply as "epistemology." This is a convention I will follow, while making the more metaphysical/ontological aspects of the epistemological questions explicit when necessary. For example, to diagnose the representationalist elements within the feminist "epistemology" of science, requires attention to the salience of an *ontological* question within the feminist project, namely, the question *"What is it for a scientific theory to be true?"*

Searching for answers to abstract questions of this type is often a metaphysical search for certainty. It is, at least, an attempt to specify *a priori* the ontological property of theories (or sentences or beliefs) that makes them true, irrespective of the context within which the theories are produced. And this, over and above the question of whether those theories are justified by the available evidence.[5]

The *real* ontological question, then, is *"What property do all true theories (sentences or beliefs) share?"* Historically, the debate about this question has been divided roughly between those who think the answer can be found by appeal to a "permanent, ahistorical matrix or framework" and those who think the answer "must be understood as relative to a specific conceptual scheme, theoretical framework, paradigm, form of life, society or culture" (Bernstein 1983, 8). Bernstein labels these the "objectivist" and "relativist" camps, respectively.[6]

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5 The *a priori* element is retained even by those epistemologists who encourage the *empirical test* of the proposed truth-making properties (e.g., Haack, 1993, *Evidence and Inquiry*, ch. 6). Haack's own "foundherentist" method of inquiry into truth involves both *a priori* and empiricist elements (Haack 1993, 2, 120). My sense is she believes that once the truth-making property is empirically specified (perhaps in some future ideal epistemological world) *then* it can be applied, *a priori*, with good judgement and discretion, of course, as a template to test new cases.

6 Bernstein blends the standard objectivism vs. subjectivism and absolutism vs. relativism distinctions to highlight the epistemological and ontological strengths of the standard distinctions. He argues that few philosophers currently support either absolutism or subjectivism, whereas some forms of objectivism and relativism continue to be debated (Bernstein 1983, 8-12). I will use his objectivist/relativist distinction throughout the dissertation, though I am going to use the distinction for purposes of which he would not generally approve. For example, Bernstein classifies Rorty's work as "relativist" (Bernstein 1983, 9), while I argue that Rorty embraces neither objectivism nor relativism. Rorty sets his pragmatist project against both positions.

Note, that even on Bernstein's construal, there is a close relationship between theories of truth (ontology) and theories of how best to *get at* the truth (epistemology). For example, epistemologically speaking, objectivist appeals to ahistorical, universal methods for identifying truth typically accompany the ontological claim that "truth" names a correspondence relation between a theory and the features of the world described by the theory. And the features of the world are themselves seen as objective, i.e., they are viewed as exisiting independently of our thoughts about them. This latter ontological element of objectivism is often referred to as "realism." Relativists, in contrast, typically identify the truth of a theory not by its correspondence with an objective reality (the nature of objective reality itself being up for debate) but by its "coherence" with other theories. The epistemological choice of which theories are relevant for this comparison is also seen as relative, e.g. relative to the worldview, culture or even gender of the investigator.

The feminist epistemological writings with which I am concerned seldom explicitly express ontological allegiance to one truth-conferring property or another. However they often explicitly prescribe which epistemological attitudes or approaches are best for detecting *the presence* of a truth-conferring property, which implies an allegiance to an ontological position about the nature of truth-conferring properties. For example, in her earlier work, Keller explains that the oppressive nature of scientific activity arises from an epistemological association between the objective detection of truth and masculine cognitive traits, such as

"detachment" and "control" (Keller 1982, pp. 593-594). She suggests that science would be less oppressive, and the objective detection of truth would be improved, if scientists used more feminine characteristics, such as "empathy" and "identification." She supports her *epistemological* prescription through ontological arguments which explicitly criticise relativist or coherence views of truth (*ibid*) and implicitly support a version of the correspondence theory of truth. (The details of Keller's epistemology will be discussed in Chapter 4.)

More generally, in her examination of how best to identify whether a scientific theory is true, Keller shares with feminist, and non-feminist epistemologists, alike, the ontological view that truth is a property of theories (or sentences or beliefs), and that this property might be absent even though the theory in question is justified by scientific standards. In other words, Keller expresses the sceptic's concern that our theories might not have the truth-conferring property, *even though* they fit well with the evidence of our experiences and our ongoing body of theories (Keller's version of scepticism is discussed in section 4.7). This sort of scepticism informs even those epistemologies that search for the property that makes theories "maximally objective" or "less partial" (see, for example, my discussion of Harding in section 5.1).

It is important to note that this global type of scepticism is distinct from the entirely reasonable fallibilist view that any of our currently well-supported theories might turn out to be wrong. Any future judgement of a particular theory would itself be made

on the basis of how well that theory fits with the evidence of our new experiences. But we would still be no closer to grasping that elusive property, independent of this evidential support, that makes the theory true, or false, and so the sceptical worry continues.

When feminist science critics embark on the ontological quest for the properties that make scientific theories true, and the epistemological search for methods to detect those properties, I worry that they open up issues of scepticism that can be used against their well-justified claims that women are being harmed by science. When we construe truth as a property of theories that is independent of the evidential justification of those theories, we (unnecessarily) open up the worry that our *own* well-justified theories about the oppression of women might not have the magical truth-making property.

In response I will argue against the utility of philosophical searches for theories of truth and knowledge, supporting an alternative view whereby scepticism about the presence of truth-making properties, as *distinct from* justification by evidence, is no longer a compelling worry. Relatedly, I will argue that the ontological and epistemological questions motivated by scepticism ("what *is* the right truth-conferring property?" and "how do we know if we've found it?") do not need answers; and that, historically, any attempts to answer them have resulted in confusion for feminists (and traditional philsophers). And, again, following the convention in the feminist literature, I will refer to

my criticisms of both the ontological and epistemological questions simply as criticisms of "epistemology."

1.2 An important distinction remains

But, of course, the question of the evidential justification of a *particular* scientific theory is still an important one that *does* need an answer, and some might call this an epistemological question as well. In my view, an important difference between the two understandings of "epistemology" is revealed when you compare the concrete nature of the question *"Is scientific theory T true?"* with the more abstract tone of the philosophical questions *"What property do all true theories share?"* and *"How do we know if we've found that property?"* Relative to examinations of the latter set of questions, examinations of the former are typically dynamic, and often *ad hoc*, processes of comparing theory T with an ongoing body of theories and with past and present experiences and data. The assignment of truth or falsity is site-specific and is adjusted as new information comes in, or as the relevance of previous information is questioned.[7]

This more specific epistemological assignment is a fallibilistic process to which feminists have made and continue to make valuable contributions, especially in our questioning of the relevance of certain aspects of the ongoing body of theories to which new theories, such as T, are compared. For example, some feminists asked why it was that a psychological theory about the

[7] My articulation of the difference between these two types of epistemological questions is a blending of Ramberg (1989, 9) and Rorty (1995, 148 - 153).

negative effects of stress was being evaluated only in relation to theories about men's psychology. Why, they asked, was it not also compared with theories about the psychology of women?[8] Tavris (1992) documents numerous other cases where feminists have challenged male-only studies of the "human" body and mind.

In order to distinguish, then, between the philosophically innocuous question *"Is scientific theory T true?"* on the one hand, with the more philosophically troublesome questions *"What property do all true theories share?"* and *"How do we know if we've found that property?"* on the other, I will refer to the study of the first question as "epistemology" with a small "e," and to the study of the second set of questions together as "Epistemology," with a capital "e."[9] I will argue that when feminists rely on Epistemological support for their criticisms of various scientific theories, the scepticism such reliance invites dangerously undercuts the persuasiveness of those criticisms.

1.3 The feminist move from epistemology to Epistemology

Why is it that Epistemology has come to be so attractive to feminist science critics? The attraction is a relatively recent phenomenon in the history of feminist science-criticism. Earlier

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[8] After more studies were finally carried out on women, it was found that a number of life events, such as marriage, which had previously been rated positively in the tests on men, actually had *negative* stress impacts on women (Muller 1992).

[9] This isn't terribly original and might seem a little awkward at first, but it will contribute to the clarity of my presentation. When looking for ways to name distinctions that are not typically made, almost any label will seem awkward. However, taking a lesson from the often negative reception of newly coined terms, such as Haack's "foundherentism," I have chosen to stick with using the upper vs lower case of an already familiar term.

feminist scientists were concerned almost exclusively with the *epistemological* question of whether a particular scientific theory was true or false. In this section I offer an explanation of the recent feminist move from epistemology to Epistemology.

In the history of modern Western science, the late 19th and early 20th centuries saw an increased professionalisation among scientists and increased educational opportunites for women (Ainley 1990, 19). Both of these phenomena helped increasing numbers of women to break barriers of sexism and racism to become scientists.[10] Many of them brought feminist concerns to their research (though not all of them identified themselves as feminists), and they often produced important epistemological criticisms of sexist research.

The history of late nineteenth-century evolutionary biology is of interest in this context. In this period, many scientists followed Darwin in claiming that males of most species (including humans) showed greater physiological and mental variability as a group, than did females (Darwin 1981 [1871]; Ellis 1894, 1903; Geddes and Thompson 1890; Cattell 1903). That is, on various physiological scales, such as strength, and various mental scales, such as intelligence, more males than females were thought to rank in the extreme high and low ranges, more females than males in the median ranges.

These sex differences in variability were said to be biologically determined through the cumulative evolutionary

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10 Some of their stories are documented in Ainley 1990; Abir-Am and Outram 1987; Rossiter 1982.

effects of the female selection of mates. Females, though presumed to be relatively passive, were still thought to exercise some choice in mating because it was viewed as a well-established fact that males of any species are more eager to pair up than are the females (Darwin 1981, 272-273). In the case of humans, Darwin conjectured that female mate-choice was probably more prevalent in our early evolutionary history than it was in his own time (Darwin, 367-368). The female act of choosing would, in turn, and over time, result in the evolution of a variety of traits in males from which the females could further choose, cumulatively increasing male variability through biological inheritance (Darwin, 272-273). Despite the highly social nature of humans, the greater variability in the physiology and mental capacity of human males, was seldom given a sociological explanation (assuming the differences in variability could indeed by documented).

In response to these theories, feminist scientists, such as Montague and Hollingworth (1914), designed and conducted painstakingly detailed empirical studies which measured physiological differences and examined the role of social forces (such as the restricted educational opportunities for girls, and the lower expectations for their success outside the domestic sphere) in shaping mental differences between men and women. Empirically testing the epistemological question "Is the biological theory that human males exhibit greater physiological and mental variability *true*?" many feminist scientists answered "No" (e.g., Hollingworth 1914; Montague & Hollingworth 1914; Woolley 1910, 1914; Calkins 1896).

However, as Shields reports, despite the sound epistemological challenges to the variability theory and other questionable theories claiming the biologically-determined mediocrity of females, such theories continued to be held by many evolutionary biologists and psychologists to the present day, especially in the growing field of sociobiology (Shields 1982).[11] It is completely understandable, then, that some feminist scientists have chosen another epistemological tack. If working to distinguish between true and false scientific theories has not always helped the feminist cause, then, some feminist scientists have suspected, perhaps the problem lies with the philosophical conceptions of truth and falsity. The view has arisen that there is something about the nature of truth and method in science that is itself directly related to the ways science harms women.

Among a recent group of feminist biologists who have held this view, and have consequently devoted less of their time to scientific research, are Keller, Hubbard and Bleier. All have written and/or spoken eloquently of the motivations for their career change from feminist scientist to feminist philosopher of science. This change has typically marked a move away from examinations of the innocuous epistemological question *"Is scientific theory T true?"* and toward examinations of the

[11] Kimura's "Sex Differences in the Brain" (1992) is a good example of recent claims about the biologically determined nature of behavioural sex differences. Kimura uses hormonal and evolutionary evidence to support the claim that human males are better at mathematical and spatial abilities than are females. For a thorough epistemological criticism of the evidence she uses see Foss (1996). Foss notes with some discouragement that these same criticisms have been levelled by feminists, and others, many times in the past (Foss 1996, 24), but they have obviously failed to persuade scientists like Kimura.

Epistemologically troublesome questions *"What property do all true theories share?"* and *"How do we know if we've found that property?"* As I have noted (above) with Keller, some feminists answer these Epistemological questions by claiming that both truth and its detection are in some way dependent on the sex or gender of the investigator.

However, while I am sympathetic with this move to philosophy, I am not convinced that reconfiguring Epistemology is the best way for feminists to address the harm caused by science.

I should make clear at this point that while I support a feminist re-focus on the epistemological question of the truth or falsity of particular scientific theories, I do not believe that this examination will map directly on to a study of which particular theories help or harm people. But I do believe that one of the best ways to challenge a theory that harms people is to examine whether or not the theory is true. For example, feminist epistemological examination of evolutionary views about women has not often had immediate positive results, but such examination has never, so far as I know, made the situation worse. However, feminist Epistemological searches for the properties that all true theories share can indeed make things worse, and irredeemably so, if such a search encourages scepticism about the truth of the feminist critiques themselves.[12]

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[12] Haraway's literary criticisms of evolutionary theory and primatology are evidence of another successful approach (Haraway, *Primate Visions* 1989). While Haraway's literary approach does not focus on whether various primatological theories are true, neither does she attempt to construct Epistemological responses to them. Her work has the virtue of analysing evolutionary and primatological theories as texts, in all their historical and cultural specificity, thereby avoiding the Epistemological

In the vocabulary of the feminist debates on this question, I suggest a return to what Harding calls feminist critiques of "bad science" as distinguished from feminist critiques of "science as usual" or "science at its best" (Harding 1986a, Ch. 1). In contrast, Harding supports the latter critiques. Those feminist critiques which characterise science done badly as biased with sexist values and patriarchal ideology are missing the point, says Harding, because, on her view, *all* our science, even that produced within the highest of our current evidential standards is value-laden or ideological (Harding 1986a, beginning on pp. 21 - 24). Harding suggests instead an Epistemological prescription for *new* evidential standards, which she calls "strong objectivity" (e.g., Harding 1991). I will argue that we need to take her Epistemological route *only* if our distinction between values and facts is based on the questionable model of psychology I referred to earlier as representationalism. As I will explain further below, representationalism marks a questionable distinction between the "unadulterated" facts, or empirical content available to human sense organs, and the value-laden schemes, worldviews, or cultural filters through which those facts are perceived, more, or less, successfully.

1.4 Lessons from pragmatism

My concerns with Epistemology and representationalism are inspired by the pragmatist arguments of Rorty, who, in turn, is

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problems I have described above, and providing a good foundation for future epistemological work.

inspired by Davidson's philosophy of language (see Rorty 1979, 1991a). Rorty and Davidson focus on the Epistemological search for the truth-making property of scientific theories (or sentences or beliefs), showing how this search is motivated by the sceptical concern that *all* our theories might be justified by the evidence but still be *completely false*. They argue that due to the nature of the representationalist model on which Epistemology is based, the battle with such global scepticism is futile (Rorty 1979, 1991a; 1991b; Davidson 1990a, 304). And, of course, they suggest that the representationalist model can and should be replaced.

According to the representationalist model, versions of which were articulated as far back as Descartes, if not earlier, beliefs are conceived as representations of their objects. In the most elementary cases these beliefs are said to be the subjective end product of a sensory process whereby the objects in our world are sensed and then screened through our subjective perceptual frameworks (the filters of our values, worldview, and language). Theories are viewed as the combination and systematisation of beliefs/representations. Sometimes the resulting theory is said to feed back into the filtering system, so that our allegiance to the theory affects our ability to accurately perceive new data. Longino argues, for example, that in the anthropological study of human origins, the commitment to either a "man-the-hunter" or a "woman-the-gatherer" theory acts as a filter through which the anthropological evidence about human evolution is perceived (Longino 1990, 103-111).

The representational conception of beliefs as filtered representations of the world explains the scepticism that fuels Epistemology. If beliefs are only copies or representations of the world, then it is possible that somewhere in the copying process an error might have occurred, rendering the copy inaccurate. In other words, all of our representations could be inaccurate because they are filtered through our perceptual apparatus, language, cultural worldview and/or theory allegiance.

The Epistemological search for properties of truth, and their detection, arises as a response to this global scepticism. Objectivists hope to defeat scepticism, while relativists seem resigned to the sceptical view that our beliefs are so hopelessly filtered we can never be sure of an accurate representation. The best we can hope for, in this case, is a "maximally objective" representation.

Rorty and Davidson offer a non-representationalist alternative that "dodges" the sceptical issue, thereby undermining the motivation for maintaining the Epistemological debate about which property (correspondence or coherence, for example) is *really* indicative of truth, maximal objectivity, etc. As I will explain further in section 6.1, Davidson takes a holistic approach to belief, conceiving all our beliefs as inter-connected with each other in terms of their content. Worldviews and political values (including feminist values) are conceived as part of the web of belief rather than as filters through which the evidence for our (empirical) beliefs pass. This conception is an important aid for feminist Epistemologists who have accepted the sceptical view

that their own political values are separate from questions of empirical evidence (see, for example my discussion of Longino in section 5.4).

Davidson also argues that if we view belief, not as a potentially faulty representation of the external world, but as the product of a triangulation between language users and the shared features of their world about which they are communicating, then we must, in fact, have a number of true beliefs (e.g., Davidson 1989a, 164; 1989b; 1991a; 1991b, 195). While he admits that any one of our beliefs may be false, he argues that the detection of false beliefs *requires* that we have a background of true beliefs against which the error of the false beliefs can be measured (Davidson 1984, 196 - 197). This latter claim undercuts the global sceptic who wants to make error a general concern, i.e. who wants to deny or question the existence of norms against which errors can be measured.

While Davidson's account of belief and meaning does not defeat the sceptic on her own terms, he is able to keep the sceptic's case from getting off the ground. According to him, if one knows a language, then one knows many things about one's world (Davidson 1989a; 1990a, sec. III). Davidson does not argue that scepticism can be defeated, that language use guarantees that our beliefs as representations are accurate. Instead he offers a non-representationalist model of belief and meaning, from which scepticism is a non-starter. I find this suggestion highly attractive.

Returning to the larger pragmatist project, I should note that there are a number of features of Rorty's pragmatism which make it highly *unattractive* to a number of people. There is some concern that Rorty does not remain faithful to the pragmatism of his American forebears. There is also the question whether Davidson, or anyone else whose work Rorty appropriates, would agree to be called a "pragmatist" in either the "original" or the "Rortyan" sense. I am not particularly concerned with these debates. There are, however, other more substantive points of contention, such as whether Davidson's project (and derivatively, Rorty's project) is as successful at escaping representationalist Epistemology as it claims, and I will take up these points as they arise in my discussion (see especially section 6.3).

I should also note that some feminist theorists, including Fraser (1989; 1991), Lovibond (1989; 1992), Benhabib (1991), and Butler (1991), have begun to explore Rorty's pragmatic program. The Summer 1993 edition of *Hypatia* was a special issue on feminism and pragmatism, though only two of the articles mentioned Rorty's version of pragmatism. Lovibond's 1992 article is a critical response to Rorty's "Feminism and Pragmatism" (Rorty 1991f). The views expressed in his 1991 article run parallel to my own arguments, though he does not focus on the feminist Epistemological approach to science, as I will.

However, feminist treatment of Rorty's pragmatism is still rare and the feminist theorists cited above tend to focus almost exclusively on the "postmodern bourgeois liberalism" of Rorty's political project. My own thesis focuses, instead, on his and

Davidson's arguments against Epistemology.[13] On those few occasions when feminists *have* examined Rorty's Epistemological criticisms, his views tend to be misinterpreted as Epistemological *support* for coherence or relativism (e.g. Lovibond 1992, 64). I argue against this relativist Epistemological reading of Rorty in 6.3.

For now I will just say that Rorty and Davidson's pragmatic alternative to representationalism provides the most satisfactory method I have yet encountered for clearing a path through the Epistemological undergrowth of feminist science and science-criticism. With a path cleared, it is my hope that my feminist colleagues can return to their critical work in evolutionary biology, and other sciences, comforted that our concepts of "error" and "truth" are not enemies that need feminist reconfiguration; comforted also that our feminist values are themselves beliefs with some relation to empirical content. Indeed, many of our feminist beliefs are well-supported by the empirical evidence and these beliefs, in turn, can be used as evidential support for other claims. Similarly, when we identify anti-feminist or sexist values affecting science we can challenge these beliefs on the basis of their (often) inadequate relationship to the evidence.

Returning to science, with these assurances, means returning to the old enemies; working to eradicate the harmful effects of sexism, racism and other oppressive systems in all aspects of scientific research, laboratory by laboratory, research program by

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[13] Arguments for the viability of a separation between Rorty's Epistemological concerns and his liberal politics will have to be made elsewhere, though Ramberg (1993) has made a great start.

research program. This undertaking is boring, plodding and hard, but relative to examinations of Epistemology it has often (though clearly not yet often enough) proven to be effective.[14]

1.5 Chapter highlights

In Chapter Two I begin my negative thesis by explaining in more detail my concerns with feminist Epistemology, highlighting the role of representationalism and the self-refuting scepticism it fuels. I argue that Epistemological arguments about truth-conferring properties, are a) premised on the coherence of scepticism, but b) consistently fail to defeat it. In support of (a) I outline and augment Davidson's analysis of the representationalist metaphor that guides Epistemological debates. With respect to (b) I present a number of arguments, including Sellars' arguments about the "Myth of the 'Given'," Goodman's "New Riddle of Induction," and Davidson's criticism of correspondence theories.

In Chapter Three I examine the ways in which the introduction of Epistemological theorising has impaired a number of influential feminist criticisms of evolutionary research. I begin with the work of Blackwell, a feminist contemporary of Darwin. I then discuss the writings of more recent feminist theorists such as Shields, Russett, Hubbard, and Bleier. Bleier's work marks the beginnings of the feminist Epistemological movement toward

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[14] Sperling's suggestions in "Baboons with Briefcases: Feminism, Functionalism, and Sociobiology in the Evolution of Primate Gender" (1991) capture the spirit of my prescription for small "e" epistemology in feminist science and science criticism. She notes, too, that the detailed empirical studies she prescribes will involve an "unbearably messy" and "time-consuming" process" (1991, 26).

over-general discussions of science and method. She argues that, historically, the objective method of identifying truth-conferring properties has been gendered masculine, and that a new, feminine method would be an improvement (she stops short of saying it would be *more* objective).

The claim that objectivity is gendered masculine is treated further in Chapter Four, where I focus on the works of Keller (1982, 1983 [1978], 1985, 1987). I trace Keller's arguments from her earliest claims for the increased objectivity associated with feminine gender identity, to her later writings in which increased objectivity is associated with the "not-male." Throughout, Keller relies heavily on over-general accounts of gender and sex categories. I also argue that because her work remains within a representationalist model, it is repeatedly defeated by both scepticism and relativism.

In Chapter Five I analyse Keller's recent *acceptance* of relativism in her 1992 essay collection *Secrets of Life, Secrets of Death*. I also examine the relativism that appears in the highly influential feminist Epistemologies of Harding (1991, 1993b) and Longino (1987, 1990). I trace this feminist acceptance of relativism to a representationalist use of underdetermination theory. I argue that relativism seems a coherent option only if we stay within a representationalist framework, and I begin hinting that we don't have to.

In Chapter Six I adopt a pragmatic interpretation of Davidson's work to argue that not only are Epistemological arguments about truth *flawed*, they are also completely

unnecessary. They are unnecessary because the fear of scepticism that gave rise to their construction is based on an unnecessary commitment to representationalism. I outline Davidson's philosophy of language as an alternative model of belief and meaning that is *not* premised on the coherence of scepticism. Consequently, his model undermines any Epistemological motivation for defeating scepticism.

The implications of this Rortyan reading of Davidson for feminist science and science-criticism are discussed in Chapter Seven. Specifically, I provide a Davidsonian response to the relativist problems with Longino's underdetermination theory, and I include a pragmatised, Goodmanesque account of the utility of sex/gender categories. I suggest that feminist science-critics adopt these pragmatist, non-representationalist attitudes toward their own truth claims, which will help them avoid the self-refutation of scepticism and relativism.

In the final chapter I provide a case study to illustrate what pragmatised science and science criticism might look like. Returning to the evolutionary themes with which I began my feminist Epistemological survey, I examine the philosophical debate between pragmatists and Epistemological objectivists about the nature of functional traits. Focusing on one functional account in particular, I discuss Profet's essay, on the function of menstruation, published recently in the *Quarterly Review of Biology* (Profet 1993). Profet hypothesises that all internally fertilising mammals menstruate, and that menstruation functions, she claims, to clean the reproductive tract of sperm-borne

pathogens. Profet also argues that the reason scientists have never viewed menstruation as particularly functional is because they have never thought to perform an evolutionary analysis of menstruation. I argue that a pragmatist feminist account of the genealogy of biological views about menstruation and reproduction would provide a more complete answer to the question why menstruation has not previously been viewed as functional.

1.6 Concluding remarks:

The pragmatic attitude I prescribe encourages the epistemological process of decision-making in feminist science and science criticism, whereby the truth of our individual beliefs, and scientific theories is assigned locally, and, of course, fallibly. This sort of epistemological assignment is typically *ad hoc* and dynamic—the criteria are always being adjusted as new information comes in. These features are part of what separates a pragmatist epistemological project from a troublesome Epistemological one. The latter involves the attempt to provide general, *a priori* "recipes" or guidelines for truth-making. As Davidson reminds us, however, "truth is beautifully transparent" (1991a, 122). He explains that because of the interrelationship between truth and meaning, and because we typically know what we mean, truth no longer needs to be seen as the sort of thing about which we need an explanatory theory.

More specifically, I want to encourage feminist science-critics and scientists alike in the important epistemological task of

analysing the causal relations between our theories and the world. However, to the extent that we construct *a priori* Epistemologies, to identify which features of the causal stories make them *true* causal accounts, we will continually be chasing after new recalcitrant features. The very scepticism the Epistemological position was constructed to solve will be reintroduced, leaving us with less than we started—less than a pragmatic account reminds us we already have—namely, innumerable well-justified claims about the oppression of women by science.

Chapter 2:
Feminist Epistemology—Problems in Practise and Theory

Feminist critics of science have taken up both the objectivist and relativist positions in the Epistemological debates. Neither side appears to be gaining any ground in a debate that has been alive in one form or another since Plato, if not earlier. If there was nothing else to tell against Epistemology, feminists should be persuaded by the historical point that asking the questions *"What property do all true theories share?"* and *"How do we know if we've found that property?"* have not proven to be fruitful (especially not for those engaged in the urgent political task of reducing the oppressive forces of science). But there are other reasons to question the fruitfulness of feminist engagement in Epistemological debate. Three of these reasons were highlighted briefly in the introduction and I will return to them here.

I first noted that feminist Epistemologists typically focus both their critical and prescriptive efforts on the general institution of science and/or scientific method. Some of the feminist scientists-turned-Epistemologists focus fairly specifically on oppressive forces within their own fields of scientific study. Ruth Hubbard, and Ruth Bleier, for example, focus critical Epistemological attention on their fields of evolutionary biology (discussed in section 3.3). However when we move to the more general Epistemological approaches of Evelyn Fox Keller and Sandra Harding, the focus on a particular aspect of scientific research is often lost, as is the ability to address the instances where actual

women are harmed. In the move away from specific questions such as "Is the evolutionary theory of women's lesser variability true?" and toward more general Epistemological questions such as "Is the nature of scientific truth male or masculine?" we lose our focus on the accountability of individual scientists, researchers and funding agencies. This over-general approach erroneously presumes that "science" names a monolithic, homogenous institution.

Relatedly, I question how the over-general characterisation of science as male or masculine serves to encourage young women to enter science, and whether it helps women who currently work as scientists. It seems that no matter how much we try to avoid it, the implication is that women scientists are, derisively, labelled as "masculine." Georgina Feldberg documented this phenomenon in her study of undergraduates in feminist courses on gender and science (Feldberg 1992). She found that the feminist women students who were not scientists interpreted feminist critiques of science and epistemology as arguments for the irrelevance of science in their lives. They also viewed women scientists as "sell-outs" who had bought into the "system" in order to succeed.[15]

We need to provide the young women and men who are currently discouraged from participating in science with more specific characterisations of the day-to-day sexism, racism and classism they might encounter. We don't want to appear to

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15 My thanks to Jodi Jensen for drawing these concerns and this study to my attention.

support a North American science culture that says only men of a privileged class can or should participate in science.

A second, related, concern is that the feminist focus on over-general conceptions of science has typically been associated with over-general and essentialist accounts of gender and sex categories. Thus, important distinctions *among* women and similarities *between* some women and men are ignored or obscured.[16] While a number of feminists have expressed concerns about this essentialism, few concrete proposals have been formulated to counter it, especially in Western feminist Epistemology. Answers to big Epistemological questions typically require big and often inaccurate generalisations—yet another reason to scale down our projects and focus on the scientific practices of actual men and women.

Following the lead of Diana Fuss (1989) I will not be essentialist about the term "essentialism," recognising that in the examples from feminist Epistemology, essentialism involves at least two types of problems: those of conception and execution. The first of these is the conception that there are essential characteristics that ahistorically fix the terms "women" and "men" (and by implication, fix women and men themselves, as those to whom the terms refer). Such characteristics are said to fix women and men across time, across cultures, across ethnicities, across class, etc. Many of the feminist Epistemological arguments I am concerned with turn on an implicit belief in some "given" constant

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[16] Judith Grant makes similar criticisms of the essentialism in feminist Epistemology in her chapter on the subject (see Grant, *Fundamental Feminism* 1993, ch. 4)

within the multiple characteristics and experiences of women and men, such as women's experience as mothers and daughters. However, in section 4.5 I argue that these beliefs and assumptions are often undermined by the diversity that emerges from historical and cross-cultural data, and even the diversity of experiences of women and men who *share* historical and cultural backgrounds.

While these feminist generalisations about women and men are often mistaken in conception, they are also often flawed in execution—the second problem. Some feminist Epistemologists make the distinction between those features of the category "woman" that are essential and those that are accidental, using criteria that reflect the theorists' own specific, historical experiences, more than they reflect any general, ahistoric experiences of women (though I will have called the obviousness or "naturalness" of these latter experiences into question). In section 4.5, for example, I explain that some white feminist Epistemologists may focus on women's sexual difference from men because sex differences are *their own* primary site of oppression, but this site of difference might not be a primary or even distinctive site of oppression for women of colour, for example.[17]

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[17] Throughout my dissertation I will argue for the importance of recognising a number of divisions within the categories "men" and "women," such as "white women," "European men," "transsexuals," and "women of colour." However I do not mean to give these divisions any more ontologically "natural" or "essential" status than I give to the categories "men" and "women." I recognise that there are often situations in which the categories "women of colour" or "white women" require similar deconstruction, and so I use these categories merely for the negative purpose of illustrating the problems with our current sex/gender categories. I present a pragmatist interpretation of the "reality" of sex/gender categories, and the implications for feminism, in sec. 7.3.

I do not believe that the problems regarding monlithic conceptions of science, and essentialist conceptions of sex/gender categories, are *necessary* features of feminist Epistemology. However, I will argue that they are currently ubiquitous practical problems and I will examine them throughout the balance of the dissertation. A third problem, concerning the role of representationalism and scepticism, *does* seem to be inherent in Epistemological projects, and it is this problem that will become my focus.

As I noted in the introduction, the Epistemological questions *"What property do all true theories share?"* and *"How do we know if we've found that property?"* are premised on the coherence of global scepticism. The Epistemological worry is that our theories might be justified by the evidence, but still not have that special elusive relationship with truth. Again, this is not a healthy "contrite" fallibilism which acknowledges that any one of our current theories might turn out to be wrong. It is a global worry that a) *all* of our knowledge of the external world might be wrong, and, worse, that b) we might never even be aware of it.

Objectivist Epistemologists attempt to answer global scepticism by suggesting that truth, identified as correspondence, *can* be recognised if one is sufficiently objective. Relativists, at the other end of the Epistemological continuum, are critical of objectivist attempts and resign themselves to accepting scepticism in its relativistic guise. For relativists the global worry that all our knowledge of the external world might be wrong becomes the global worry that there is no way to adjudicate between theories

about the external world that are produced from within our own internal frameworks. Coherence *between* a collection of subjective theories is the only criterion left for the relativist. This robs her arguments of any Epistemological bite when she attempts to promote one theory over another (even her theory of relativism!). And, as I will explain further, scepticism remains a motivating factor even for positions such as empiricism or instrumentalism, which fall somewhere between the two polar extremes.

Neither objectivists, instrumentalists nor relativists have succeeded in persuading each other that they have identified the property of truth that would answer their sceptical concerns. Feminist criticisms of science that make use of any of these Epistemological approaches are weakened by the scepticism and relativism these theoretical avenues invite but then leave unanswered. For example, when feminists have argued that instances of bias and abuse in science are innumerable, multifarious and complex, this is not a claim about which we can afford to encourage scepticism. People are being harmed by various scientific practices, theories and methods. Some women and men have been oppressively excluded from practising science on the basis of irrelevant criteria such as sex and race. They have also been described as *subjects* of biased scientific theorising, in ways that justify their continued exclusion from science and from other arenas of "rational inquiry," further restricting their opportunities and freedoms. And, even worse, many have been physically harmed or killed by scientific products that have been

inadequately tested. We cannot afford to encourage scepticism about these claims. Similarly, when we have argued that such bias and abuse must stop, the truth of this argument must not be seen as relative to a subjective feminist worldview. It must be true *simpliciter*.

Richard Rorty's pragmatist project criticises the Epistemological debate by arguing that the scepticism it makes coherent results from a questionable representationalist model of knowers and the world (Rorty 1991a; 1991b). As I explained briefly in the introduction, he uses Donald Davidson's philosophy of language as a non-representational alternative to Epistemology—an alternative that makes scepticism a non-issue. I believe this alternative to be a useful one for feminists. In this next section I examine more closely how Rorty and Davidson view the relationship between Epistemology, representationalism and scepticism.

2.1 Symptom: Scepticism; Diagnosis: Representationalism

So why is it that the Epistemological questions *"What property do all true theories share?"* and *"How do we know if we've found that property?"* assume the coherence of scepticism? Rorty identifies the underlying culprit as the representational metaphor that frames Epistemological debate (e.g., Rorty 1991b, pp. 151-161). Discussants on both sides of the debate, he explains, view beliefs, sentences or theories as representing the world. We are said to acquire these representations through a filtering process. Here, in Davidson's terms, our "language scheme," "worldview" or

culture is described as a medium through which the empirical "content," "sense-data," or "the facts" of the external world, are filtered (Davidson 1984). Invoking this filtering process, the representationalist invokes a metaphysical gap between the subjective end-product of belief and the objective external reality the belief is about, a gap between mental "inner space" and the outside world.[18] If one uses this representational model the task then becomes one of looking for properties of bits of our beliefs, sentences or theories that make them true representations of the bits of the world to which they refer. Typically, the truth-conferring properties are identified as relational properties, such as the property of correspondence. A scientific theory that has the property of correspondence is one that successfully bridges the implied metaphysical gap between our inner subjective beliefs and objective external reality.

The claim that a truth-conferring correspondence relation is needed to bridge the gap between our representations and the bits of the world they describe implies a number of questionable metaphysical commitments. One is the ontologically realist commitment to the view that the world is packaged or objectively "given" as bits of evidence or facts to which, ideally, our truthful theories correspond in the requisite way. Another is the commitment to the view that the inner subjective space from which our representations proceed is a *non-natural* (supernatural?) element, insofar as it is *metaphysically* independent from the external, objective natural order. Finally,

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[18] My thanks to Bjørn Ramberg for suggesting this characterisation.

this metaphysical independence or gap between the inner subjective stuff of mind and the external objective reality makes coherent the worry that the two worlds might not be bridgeable—*all* of our subjective theories about external reality "might be just as they are and yet reality—and so the truth about reality—be very different" (Davidson 1990a, 298). In other words, the independence is such that all of our theories would "float free" of the bits of the world they purported to describe, unless securely anchored via the truth-conferring relational properties. When one conceives of such a gap between representations and the world represented, there is always the possibility of massive error in the representations (i.e., it becomes conceivable that all of our bridgework could be completely undependable). This is the worry of global scepticism, so clearly articulated by the proto-representationalist Decartes in his theory of mind/body dualism.

Because Epistemology has had such a lengthy history, it is impossible to do justice to the details of the myriad responses to scepticism that have been articulated in the literature. However, I will discuss details of the two major trends in the literature, objectivism, and relativism, and a mid-way point, empiricism (or instrumentalism).

The objectivist assurance that scepticism can be defeated arises from the belief that we can delineate *a priori* the criteria for judging whether our representations have the truth-conferring relational property or not (or that we can work empirically toward the ideal end of inquiry where methods of truth detection can *then* be applied *a priori*). In other words, the claim goes, we

can tell if and when the bridgework between our subjective inner space and the objective outer reality is dependable. "Objectivity" names the prescribed approach to the detection of truth. If we are objective, if we stand apart from the filters of all our subjective theories, if we enlist the help of other objective observers similarly placed, then we can open the bridgeway between our sensory receptors and the causal forces of the empirical data. We can tell whether a particular theory is in a correspondence relation with the natural facts given up by the world the theory purports to describe.

C. I. Lewis prescribes a version of objectivism in his book *Mind and the World Order* (1956 [1929]). He explains that "the two elements to be distinguished in knowledge are the concept, which is the product of the activity of thought, [such as the forming of an hypothesis or a theory] and the sensuously [empirically] given, which is independent of such activity" (Lewis 1956 [1929], 37). For Lewis, the "given" of experience is "what remains unaltered, no matter what our interests, no matter how we think or conceive" (Lewis, 52). This is what Davidson calls the "content." Our conceptualisation of the given is the imposing of a filter or "scheme" over the content.

For Lewis, the sense data and our subjective filtering of that data must, *in principle,* be capable of separate analysis. The intuited experience of the given is error free; error only arises from our filtered conceptualisations of the given (Lewis, 121). Objective knowledge of the empirically-given data results when a conceptualisation of the given holds up to empirical test over time,

and the probability of error produced by the subjective filters decreases (Lewis, 37), or, in other words, when the gap between our inner conceptual space and the outer world is successfully bridged.

Instrumentalists and empiricists take these objectivist views to be generally coherent, but false for a specified range of claims. More specifically, they disagree with the traditional objectivist view about the correspondence between the theoretical or unobservable elements of a theory and the external world. For example, Bas van Fraassen's constructive empiricism maintains that we can only have objective knowledge of the truth about the *observable* entities in a theory. He explains that "to accept a theory is (for us [constructive empiricists]) to believe that it is empirically adequate—that what the theory says about *what is observable* (by us) is true" (van Fraassen 1980, 18; italics in the original). When a theory makes reference to unobservable entities we cannot have such knowledge. He argues that theories which contain unobservables can be "empirically adequate," but not true or false as a whole (*ibid*). Theories about micro-particles, for example, cannot be true or false as a whole (1980, 17). More traditional instrumentalists, such as the positivists, deny the existence of unobservables outright, rather than remaining agnostic as van Fraassen does. Notice that instrumentalists and empiricists agree with the more straightforward objectivists that truth-as-correspondence *can* provide objective knowledge, for theories containing only observables, at least.

Those who have moved well away from objectivism, but who stay within the representationalist framework of the debate, end up with a relativism that resigns them to doubt, at a very general level, the existence of any firm causal relationships between their theories and the world. At this relativist end of the continuum we find the tacit claim that if we are critical of the objectivist notion of correspondence properties making certain theories true, then we are left with the position that it is *we* who make them true, that our subjective filters are so opaque that truth, if it can be spoken of at all, can only be said to be relative to *us*, our politics, our worldviews, and *not* to the world. Ruth Hubbard presents a relativist view when she claims that "every theory is a self-fulfilling prophecy that orders experience into the framework it provides" (Hubbard, 1983, 47). According to relativists, the metaphysical gap between our representations and the world represented remains unbridged, or at least any bridgework we construct is irredeemably blocked by the filters of our worldviews and conceptual schemes.

The questionable metaphysics of the representationalist model make scepticism a coherent concern for any number of positions on the Epistemological continuum. In the absence of any consensus that this scepticism has been defeated, feminist critics of science who enter the Epistemological debate invite scepticism with no guarantee that it will not be used against their own critiques. While this concern might be discouragement enough for some, the current philosophy literature attests to the existence of many Epistemologists, feminist or otherwise, who simply accept

their questionable representationalist metaphysics and continue the battle against scepticism anyway. While continuing this battle might seem brave, there are a number of arguments that suggest it is foolhardy. To better motivate my excitement about a non-representationalist, scepticism-free alternative I will next discuss some of the arguments that show why representationalist Epistemology consistently fails to defeat the very scepticism it makes coherent.

2.2 The Epistemological failure to defeat scepticism

I begin with a version of an empiricist attempt to defeat scepticism. Empiricists typically claim that the correspondence bridge between our subjective linguistic entities (theories, sentences, beliefs) and the objective world, can be known to be reliable, at least for those entities that have empirical content. If we are sufficiently objective observers, then we can open the bridgeway between our inner subjective world and the causal forces of the observable data from the external world which impinge on our sensory receptors. The reliability of knowledge about theories with observable content is said to provide a foundation for knowledge about theories with more abstract, unobservable content. That is, we can be fairly sure of the reliability of theories that have less or no observable content if we can link them to the Epistemic foundations provided by more stable observable theories.

One of the most sophisticated versions of empiricism can be found in the early writings of Willard Van Orman Quine (see, for

example, the first two chapters of *Word and Object* [Quine 1960]). The sophistication, by my anti-representationalist criteria, results partly from the fact that Quine does not identify correspondence as a truth-conferring property between two metaphysically distinct entities. He attempts to naturalise the inner "subjective" side of the subject/object gap, making the subject *part of* the external world treated by empirical science, rather than part of some metaphysically separate supernatural order.

Quine also takes a holistic approach to meaning and truth. Unlike the reductionistic views of his positivist colleagues, views to be found in Rudolf Carnap's early work, for example (Carnap 1939), Quine argues that only sentences, not individual terms, can be said to have meaning and that this meaning comes from the sentence's role in the larger theory of which it is a part. He is also much more holistic about the division between sentences with observational content and sentences without, than is, say, van Fraassen. That is, for the most part, he views the division as a continuum. The criterion for whether a sentence is "observational" or "nonobservational" is relative to the placement of that sentence in the web of sentences from which it derives its meaning.

However, Quine argues that the division between sentences with observable content and those without is still an Epistemologically crucial division. For example, in the chapters of *Word and Object* cited above, he argues that what makes a sentence (or theory) true is its relation to sensory stimulation. Here it is clear that he retains an interest in the Epistemological

question *"What property do all true theories share?"* and, as I will argue in the discussion that follows, his sophisticated answer nevertheless fails to defeat scepticism.

Quine calls those sentences which have a close relation to sensory stimulation "observation sentences." Observation sentences such as *There's a cat on the mat* "prompt the assent" of any number of people receiving the same visual stimulus (Quine 1960, 43). Quine writes that observation sentences "suggest the datum sentences of science," that is, the *evidence* (44).[19]

Not all sentences have this close link to sensory stimulation, however. Some sentences will *not* prompt assent from different speakers similarly placed, especially if the sentences contain words that get their meaning from relation to other words, as opposed to acquiring meaning through more-or-less direct ostension. Quine gives as an example "[He is] a bachelor" (45). According to Quine, the sentences of our various theories of the world stand in a web-like relation to each other, with observation sentences ("there's a cat on the mat") at the periphery, well-anchored to the sensory stimulation of the outside world. Nonobservation sentences ("he is a bachelor") are found in the webbing of the centre.

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[19] There are debates about whether it is the public assent to the sensory stimulus, or the reception of the stimulus itself that Quine *ultimately* considers to be the evidence for a sentence's truth (e.g., Davidson 1990b, and Quine's response in the same volume [Quine 1990]) but certainly there are clear passages where Quine gives the evidentiary role to the physical reception and my discussion will focus on these.

Now, to be sure, Quine approaches the distinction holistically; there are no hard and fast boundaries between words which get their meaning from observation and those which get their meaning from their relation to other words. Indeed the holism suggested by the fine grading off between observation and nonobservation sentences makes it look as if the meanings (and, relatedly, the truth) of the sentences of any theory are always, to some extent, *relative* to their place in the web of the "containing theory," rather than objective and definitive. Quine uses the concept of "stimulus meaning" to respond to this relativistic worry. "The stimulus meaning of a sentence for a subject sums up his disposition to assent or to dissent from the sentence in response to present stimulation" (Quine 1960, 34). He continues:

> If a sentence is one that (like 'Red' and 'Rabbit') is inculcated mostly by something like direct ostension, the uniformity [in language learning] will lie at the surface and there will be little variation in stimulus meaning; the sentence will be highly observational. If it is one that (like 'Bachelor') is inculcated through connections with other sentences, linking up thus indirectly with past stimulations of other sorts than those that serve directly to prompt present assent to the sentence, then its stimulus meaning will vary with the speakers' pasts, and the sentence will count as very unobservational (1960, 45).

The stimulus meaning of any sentence can be examined in isolation from the sentence's relation to other sentences. Quine explains how this responds to worries about relativism:

> would be awkward, since, conversely, the individual component sentences offer the only way into the theory. Now the notion of stimulus meaning partially resolves the predicament. It isolates a sort of net empirical import of each of various single sentences without regard to the containing theory, even though without loss of what the sentence owes to that containing theory. It is a device, as far as it goes, for exploring the fabric of interlocking sentences, a sentence at a time (Quine 1960, 34-35).

According to Quine, then, if the stimulus meaning of a sentence varies greatly from speaker to speaker, then the meaning is related less to direct sensory stimulation and more to other sentences. Observation sentences with stimulus meanings that are more uniformly shared by speakers are candidates for objective knowledge because they acquire their meaning through their relation to sensory stimulation from the external world. According to Davidson, this argument shows that for the earlier Quine who wrote *Word and Object*, observation sentences play a justificatory role. In other words, Quine wants "to anchor at least some words or sentences [the observation sentences] to non-verbal rocks" (Davidson 1991a, 126). Davidson describes Quine's view further:

> Whatever there is to meaning must be traced back somehow to experience, the given, or patterns of sensory stimulation, something intermediate between belief and the usual objects our beliefs are about (*ibid*).

Unfortunately, as with most objectivist projects, this "something intermediate" leaves conceptual space for scepticism.

Unfortunately, as with most objectivist projects, this "something intermediate" leaves conceptual space for scepticism. Injecting intermediaries between the meaning of our beliefs and that which would make our beliefs true, encourages scepticism, because we don't know if these intermediaries are supplying us with correct information. How could we ever step outside the process to check? (Davidson 1986). Davidson argues that "no such confrontation makes sense,"

> for of course we can't get outside our skins to find out what is causing the internal happenings of which we are aware. Introducing intermediate steps or entities into the causal chain, like sensations or observations, serves only to make the epistemological problem more obvious. For if the intermediaries are merely causes, they don't justify the beliefs they cause, while if they deliver information, they may be lying (1991a, 125).

Davidson understands why sensations, for example, have been thought to play an Epistemological role. We are aware of our sensations in a way that gives them salience in the causal process of belief acquisition. However, this awareness is simply another belief.

> Emphasis on sensation or perception in matters epistemological springs from the obvious thought: sensations are what connect the world and our beliefs, and they are candidates for justifiers because we often are aware of them. The trouble we have been running into is that the justification seems to depend on the awareness, which is just another belief (1991a, 124).

Here, meaningful understanding of the external world is made a function of an Epistemic intermediary. Of the resulting scepticism Davidson writes: "It is ironical. Trying to make meaning accessible has made truth inaccessible" (1991a, 126).

A further problem results from the fact that there is no way to tell at what point the sensation of the "uninterpreted world" ends and our subjective interpretation or perception begins. This is the representationalist problem of the scheme/content distinction. One of the more well-known examples of Quine's early adherence to the scheme/content distinction, follows:

> We cannot strip away the conceptual trappings sentence by sentence and leave a description of the objective world; *but* we can investigate the world, and man as a part of it, and thus find out what cues he could have of what goes on around him. *Subtracting his cues from his world view, we get man's net contribution as the difference.* This difference marks the extent of man's conceptual sovereignty—the domain within which he can revise the theory while saving the data (Quine 1960, 5, emphasis mine).

Here, Quine still makes the questionable assumption that there is a meaningful Epistemological distinction between unanalysed sensory cues, and one's "worldview" or analysis of those cues, even though his naturalism toward the subject that produces the "worldview" gives him little conceptual apparatus for making the distinction.

Wilfred Sellars's work in *Science Perception and Reality* further criticises the notion of unanalysed sensory cues, or what

he calls, the "myth of the given" of sensory information (Sellars 1963).[20] Providing an historical overview of the use of the "given" in the empiricist tradition, Sellars writes that

> classical sense-datum philosophers have...taken givenness to be a fact which presupposes no learning, no forming of associations, no setting up of stimulus-response connections. In short, they have tended to equate *sensing sense contents* with *being conscious*, as a person who has been hit on the head is *not* conscious, whereas a new-born babe, alive and kicking *is* conscious (Sellars 1963, 131).

Sellars argues against this view, explaining that perceiving even the most "primitive" sense datum requires all kinds of previous abilities, such as the ability to discriminate the one sense datum from others, for example. Therefore knowledge of the particular sensation in question cannot be Epistemologically foundational. Of the knowledge required *before* a particular sensory datum can be perceived, he writes:

> Observational knowledge of any particular fact, e.g., that this is green, presupposes that one knows general facts of the form *X is a reliable symptom of Y.* And to admit this requires an abandonment of the traditional empiricist idea that observational knowledge 'stands on its own feet' (1963, 168).

Presupposing objective criteria for distinguishing between the categories "X" and "Y" also proves to be difficult. Here, Nelson

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[20] In *Science and Subjectivity* (1967) Israel Scheffler provides a critique of C. I. Lewis' formulation of "the given" (see, especially, Scheffler, pp. 34 - 35). However, Scheffler remains uncritical of the larger representationalist model he shares with Lewis.

Goodman casts a critical eye over our certainty about the categories nature objectively "supplies" and to which our true theories "correspond" (Goodman 1955). He begins with the category or predicate "green" which we use, for example, in the theory "All emeralds are green." We assume that every time we observe another green emerald this is empirical evidence in favour of the theory. But what if, Goodman asks, "green" was the wrong category? What if there was another category, "grue"? ("A colour which applies to all things examined before [Jan. 1, 2001] just in case they are green but to other things just in case they are blue" [1955, 74]). If we were observing "green" emeralds *before* Jan. 1, 2001, it might just be that this observation does *not* confirm the theory "All emeralds are green," but *instead*, (or likewise) confirms the theory "All emeralds are grue." No satisfactory objectivist answers to Goodman's question have been forthcoming. It seems that we can never be sure that even our most straightforward sensory reports of nature's "given" categories provide objective evidence for our theories about those categories. Problems with scepticism only seem to multiply for the objectivist.

To review to this point, elements of Quine's earlier writings showed an objectivist attempt to "harness" sensation in the service of Epistemological justification (Ramberg 1989, 13). Quine argued that those beliefs expressed in sentences which arise from direct links to sensations (e.g. observation sentences) provide the foundations for those sentences and beliefs not so directly linked (e.g. Quine 1960, 42). However, Davidson reminds us that sensory

stimulations, while causally related to belief, "cannot, without confusion, be considered to be evidence, or a source of justification, for the stimulated beliefs" (1991a, 132). And Goodman's "riddle" reminds us that we have no objective criteria for choosing some categories as empirical evidence, over others.

So what Epistemological role is left then for objectivist empirical intermediaries? None, according to Davidson and Sellars. Sellars reminds his readers that his analysis still leaves an important though *non-Epistemological* role for empirical sensation of the outside world. He does not have idealist doubts about the existence of an objective reality, for example (Sellars 1963, 161). Paralleling Sellars on this point, Davidson writes: "No doubt meaning and knowledge depend on experience, and experience ultimately on sensation. But this is the 'depend' of causality, not of evidence or justification" (1991a, 127). Davidson concludes:

> The moral is obvious. Since we can't swear intermediaries [such as sensations] to truthfulness, we should allow no intermediaries between our beliefs and their objects in the world. Of course there are causal intermediaries. What we must guard against are epistemic intermediaries (Davidson 1991a, 125).

So, contrary to what many objectivists have argued, detection of empirical entities cannot be put in the Epistemological service of warding off scepticism. Relativists fare no better as they simply use this point to show the futility of attempts to construct a correspondence bridge that is free from the filters of our

subjective conceptual schemes. Better to invoke the coherence of one theory with another, they suggest, rather than trying in vain to bridge the metaphysical gap between subjective scheme-riddled theories and the objective external world. But of course the claim that relativism is itself a true description of our relation between our theories and the world cannot be defended from scepticism. No Epistemological escape from scepticism appears in sight.

However, the arguments of one other cohort of Epistemologists still needs to be addressed. This is the new generation of objectivist Epistemologists who acknowledge this history of failure but who imagine that they will be able to *transcend* their history by finally discovering the right material for building the correspondence bridge.[21] In response to this sort of admirable, but misguided, hope there are further arguments by Davidson and Arthur Fine which question not just the previous attempts to build a correspondence bridge, but the very concept of truth as correspondence itself. I will conclude my discussion with a brief review of these arguments.

2.3 Another look at correspondence

Davidson mounts numerous criticisms of the very idea of truth as correspondence (e.g., in Davidson 1984; 1990a). According to the representational model of truth as correspondence, he explains, a sentence or theory "fits our sensory

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21 Haack's "foundherentist" account of truth is a good example of this sort of project, (Haack 1993).

promptings, successfully faces the tribunal of experience, predicts future experience, or copes with the pattern of our surface irritations, provided it is borne out by the evidence" (Davidson 1984, 193). But of course, comparing a theory to the available evidence is just what we *all* do when we examine whether the theory is true. The Epistemologist responds to this observation by noting that because we know our theories could be wrong in the future, correspondence with *available* evidence is not enough. Truth, she claims, must be correspondence with all *possible* evidence, past, present and future (Davidson 1984, 193). However, she has still not told us anything new about truth, she's just added more of the criteria with which we're already familiar. Future criticism of our current theories will, of course, be made on the basis of new evidence. As Davidson argues, to say that a theory corresponds with the totality of evidence past, present and future, "adds nothing intelligible to the simple concept of being true" (*ibid*). That is, truth-as-correspondence is not a new property against which we can test our theories (Davidson 1984, 194). As one Davidson commentator writes:

> To say that 'x is true' means 'x corresponds to the facts' provides no elucidation of the predicate 'is true'. Correspondence is just another way of talking about truth, and not a way of telling us what truth is (Malpas 1992, 241)

Examining the claim that true theories correspond to the totality of the evidence also shows us that there is nothing "interesting or instructive" to which these theories can be said to

correspond (Davidson 1990a, 303). If all true theories correspond to the totality of evidence then they all correspond to the same thing (*ibid*). Trivialising correspondence in this way removes the individually "given" facts to which our true theories are said to correspond. And if the individually given facts cannot be provided then we lose the notion of our beliefs, sentences or theories as *representations* of those facts because "there is nothing for them to represent" (p. 304). Losing the reified notion of "facts" given up naturally and objectively by the external world, and losing the notion of our theories as subjective, internal representations of those facts, is a major step toward losing our sceptical fear that there is a *metaphysical* gap between our theories and the world those theories describe. (I will discuss this further in section 6.1.)

Arthur Fine is also critical of Epistemological uses of correspondence. He notes that objectivists (he calls them "realists") often argue for their position by appealing to what he calls the "wouldn't it be a miracle" argument (see, for example, Richard Boyd 1984, 49, 59). According to these realists, theories that explain the world so well must correspond truthfully to the world, indeed "it would be a miracle" if they did not (Fine 1989, 81). Fine points out, however, that it is precisely the question of whether explanatory success justifies belief in the truth of the "explanatory story" that is up for debate (82).[22] Newtonian

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22 van Fraassen makes a similar point in his criticism of realism (1980, 20-25). However, as I noted earlier, he recommends an Epistemological variant to realism which he calls "constructive empiricism." I argue that Epistemological variants are never completely effective as corrective measures because they remain within the representationalist model.

mechanics had great explanatory success, but since Einstein's work, we now know that Newton's theories are false, or at least that their truth is restricted.

According to these criticisms, reality is not divided into ontologically-given facts to which our true theories correspond. Our true theories correspond to the totality of evidence as a whole. Further, we cannot rely on detecting correspondence relations to tell us if our theories about the world are true, no matter how holistically we divide "the totality of evidence." This is because detecting correspondence *presupposes* a notion of truth, it doesn't *explain* truth. Davidson and Fine argue that viewing correspondence as a bridge across the representationalist gap is a conceptual confusion. Davidson, in particular, goes on to argue that the confusion is telling—that there *is* no representationalist gap to bridge.

Perhaps in the future a satisfactory Epistemological response to these conceptual criticisms will appear. However, it would seem that the burden of proof for showing the usefuless of continuing the Epistemological debate falls squarely in the Epistemologist's court.

2.4 Summary:

I began this essay by introducing my thesis: Entering into the Epistemological debates about what property makes a scientific theory true and how best to detect the property, is not the most effective way for feminists to challenge the oppressive forces of science. In this chapter I gave three reasons for my claim.

The first reason I discussed is my concern that much feminist Epistemology is focused on the general institution of science and/or scientific method. This focus makes it next to impossible for feminists to pinpoint specific agents responsible for harming people, which remains the most urgent issue. The second reason is that the level of generality of the "science" discussion is often related to an over-general characterisation of sex and gender categories that exaggerates the similarities among women and ignores similarities between women and men. The third reason became the focus of the chapter, namely that the representationalist model underlying Epistemology makes scepticism a problem that has never been (and will never be?) dissolved. Given this failure, I argued against the fruitfulness of feminist engagement in the Epistemological debate, worried primarily that once scepticism is introduced by the debate, there is nothing to keep it from being used against important feminist claims.

I also offered a Rortyan/Davidsonian explanation of the metaphysical mechanisms that relate representationalism, scepticism and Epistemology. I hinted that Rortyan pragmatism provides good reasons to think that choosing representationalism (and engaging in Epistemology) is *optional* because there is a non-representational alternative that makes scepticism a non-issue. If global scepticism is a non-starter, then the Epistemological debate becomes radically unmotivated.

I will introduce this non-representationalist alternative in Chapter Six. In the next three chapters I document the problems

with representationalism that appear in the Epistemological claims of a number of important feminist critics of science. In particular, I will show how these feminist Epistemologists find themselves caught between objectivism and relativism. In the end they are satisfied with neither, but they are persuaded by their representationalist commitments that the Epistemological continuum provides the only options.

Chapter 3:
Feminist Epistemology and Evolutionary Theory

One of the most common targets of Western feminist science criticism has been evolutionary biology; in particular Darwinian theories of sexual selection. Charles Darwin's foray into the world of sex difference began tentatively in *The Origin of Species by Means of Natural Selection* (Darwin 1962 [1859]), but it is in *The Descent of Man and Selection in Relation to Sex* (Darwin 1981 [1871]) that he focused on these differences more explicitly. Darwin postulated his theory of sexual selection to account for the evolution of secondary sexual characteristics—sex-specific characteristics, not directly related to reproduction, such as the "extravagant plumage" of the peacock. The fact that both the peacock and peahen survive and procreate while unequally endowed meant that, strictly speaking, the plumage isn't necessary for survival (Darwin 1981, vol. 1, 258). Expanding his theory of natural selection to include sexual selection, Darwin hypothesised that secondary sexual characteristics do indeed have a biological function because they better enable the individual to attract a mate, fend off competitors for that mate (*ibid*), and/or provide for the care of offspring. A few details of his theory are provided below, followed by some feminist Epistemological responses to it and to the work of contemporary sociobiologists.

3.1 The Darwinian theory of sexual selection

In his examination of the nature of the differences between secondary sexual characteristics in males and females, Darwin observed that it is usually the adult male of any species who is more modified with respect to secondary sexual characteristics (vol. 1, 272). Within any given species, he observed, adult females and the young of both sexes tend to look more similar to each other than do males (*ibid*). In the case of humans, he noted that secondary sexual characteristics tend not to be as spectacular as those of the plumes of the peacock, but, he claimed, both physical and mental sex differences are readily apparent.

Of the physical differences, Darwin identified adult human males as taller, heavier, stronger with more angular shoulders and more plainly pronounced muscles, hairier, especially on the face, and deeper in voice, than adult females and children of either sex (vol. 2, 316). Among Europeans, he noted that women have more "brightly coloured" skin than adult men (*ibid*). Women, generally, were also observed to have rounder faces, and broader pelvises than adult men (vol. 2, 317). While these observations about women might contradict the claim that men are more varied, Darwin noted, instead, that most of these adult female physical traits are shared also by children, male or female (vol. 2, 317), and, he claimed, male and female children more closely resemble each other, even across races (*ibid*). Darwin did allow women something of a distinct identity "intermediate between the child and the man" with respect to certain features such as skull shape (*ibid*).

Darwin believed that these physical differences between the sexes are accompanied by mental differences. He wrote: "Man is more courageous, pugnacious and energetic than woman, and has more inventive genius" (vol. 2, 316). Further,

> The chief distinction in the intellectual powers of the two sexes is shown by man's attaining to a higher eminence, in whatever he takes up, than can woman... whether requiring deep thought, reason, or imagination, or merely the use of the senses and hands (vol. 2, 327).

Some positive mental characteristics were observed to be "more strongly marked" in women, such as intuition, rapid perception, and imitation, but Darwin is quick to point out that "some, at least, of these faculties are characteristic of the lower races, and therefore of a past and lower state of civilization" (vol. 2, 326-327).

Comments about the "lower races" show that Darwin's Victorian sexism was often complicated by racism. He explained that "all the secondary sexual characters of man are highly variable, even within the limits of the same race; and they differ much in the several races" (vol. 2, 320). He seldom wrote of "human" sex differences unless he had first catalogued various instances of the differences within the "races" or "subspecies" of humans. For example, a survey of various "races" allowed him to conclude that "women in all races are less hairy than men" (vol. 2, 319). Often, however, he found he had to qualify such generalisations between races. For example, he noted fewer sex differences in amount of hair and general physique amongst

"American aborigines" than amongst the "negroes" and the "higher races" (vol. 2, 323). He also cited Carl Vogt's observation that sex differences in cranial cavity "increase with the development of the [human] race so that the male European excels much more than the female, than the negro the negress" (Vogt 1864, quoted in Darwin, vol. 2, 329-330, footnote). In a discussion of sex differences in "voice and musical powers" Darwin wrote that "natives of China" do not exhibit the sex differences found in other races (vol. 2, 330). Aside from these scattered observations, Darwin devoted an entire section to the discussion of race differences in sexual selection, entitled "On the Causes Which Prevent or Check the Action of Sexual Selection with Savages" where he gave examples of the lower levels of sexual difference in "primitive" and "barbarian" tribes of his own time (vol. 2, pp 358-367).

Often, then, a woman had to be of "civilised" European ancestry before she would qualify for the particular brand of sexism inherent in Darwinian discussions of sex differences. Otherwise, her psychology and physiology were relegated to discussions of the "lower races," where, it was claimed, "progressive" sex-differentiation was not as evolved.[23]

As I discussed briefly in the Introduction, Darwin ascribed the superiority of male secondary sexual characteristics to "greater male variability" (vol. 1, 275). Across species, more males than females were thought to rank in the extreme high (and extreme

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[23] American evolutionary theorist Edward Drinker Cope is one of the most explicit theorists in this regard (see Cope 1974 [1887], pp. 280 - 290).

low) ranges in measures of physical and mental prowess. Darwin argued that greater male variability was due to the greater sexual "eagerness" of males (vol. 1, 272). He explained that "the female,... with the rarest exceptions, is less eager than the male ... She generally 'requires to be courted'; she is coy, and may often be seen endeavoring for a long time to escape from the male" (vol. 1, 273). Regarding female mate choice, Darwin's characteristically keen powers of observation afforded him the view that a female typically accepts "not the male which is the most attractive to her, but the one which is the least distasteful" (*ibid*). Regardless of the details, he concluded that "the exertion of some choice on the part of the female seems a law almost as general as the eagerness of the male" (*ibid*). Female selection of male mates, would, over time, encourage more variability in males to satisfy choosy female tastes.

In those instances of species where the *female* has the more pronounced secondary sexual characteristics, or where there are no such differences between the two sexes at all, Darwin suggested that perhaps there has been *mutual* selection where the males "have selected the most attractive females, and the latter the more attractive males" (vol. 1, 277) However, in the next passage he discarded this view, writing that "from what we know of the habits of animals, this... is hardly probable, for the male is generally eager to pair with any female" (*ibid*). It is more probable, he continued, that the female traits were acquired by the male and transmitted to and developed in the female, perhaps during periods when there were greater numbers of females than

males which might have encouraged an uncharacteristic choosiness amongst the males (*ibid*).

With respect to humans, in particular, the fact that some variation is inherited by both males and females required special consideration (vol. 2, 328). Making a virtue out of necessity, Darwin concluded that if women did not at least benefit by inheriting some of the sexually selected superiority of men (though only in small amounts if he had to concede any at all) then "it is probable that man would have become as superior in mental endowment to women, as the peacock is in ornamental plumage to the peahen" (vol. 2, 328-329).

For the most part, however, Darwin expressed a great deal of equivocation about whether and how secondary sexual characteristics are inherited. In those cases where he was sure that only males exhibit the trait he offered the following explanation (vol. 1, 280). Employing a pangenetic theory that anticipates modern hormonal theories, Darwin hypothesised that "gemmules" or "undeveloped atoms," derived originally from the male tissue that produced the male secondary sexual characteristic, are passed to offspring of both sexes. However, he argued, these gemmules remain undeveloped until after puberty in the male, and forever undeveloped in the female. After puberty, the adult male body produces newly modifed cells for which the gemmules have an affinity. In the presence of the appropriately modified cells, the gemmules unite and develop, forming new tissue to reproduce the male secondary sexual characteristic. In the case of the less-modified adult females and

the young of either sex, the appropriately modified cells are absent, so the gemmules of the male secondary sexual characteristic don't develop. Darwin admitted, though, that "why certain characters should be inherited by both sexes, and other characters by one sex alone, namely by that sex in which the character first appeared, is in most cases quite unknown" (vol. 1, 285).

Undaunted, Darwin concluded that the superior secondary sexual characteristics of the ("civilised") human male have the biological function of helping the male attract females and subsequently support and maintain a wife and family (vol. 2, 327-328). The time when these superior physical and mental characteristics are most needed and developed is during maturity, and hence these characteristics will be passed on mostly to the male offspring to be manifest at their maturity (vol. 2, 328).

Evolutionary views about the sexes had a number of harmful effects on women. The claim that women varied less on measures of intelligence, for example, led many (male) scientists and scholars of the time to argue against the utility of educating girls and women, especially in the fields of math and science. Granville Stanley Hall made similar arguments in his two volume work *Adolescence* (1904). Hall wrote extensively on the negative influence of women's menstrual cycle on their abilities at school and in life more generally (vol. 1, 490 - 494). He asserted that women are less reliable because they are slaves to the fluctuations of their hormones. Ironically, this lack of reliability was claimed to be indicative of women's more generic (less-

varied) nature. He wrote: "Everyday of the 28 [days in her cycle] she is a different being...[which] reveals her as a more generic creature than man" (494). And "to know one [woman] more involves knowing all" (505). Evolutionary arguments were also used in appeals to restrict women's political influence, in favour of keeping women in the domestic sphere (see, for example, Spencer 1969 [1873], 340 - 342).

As I have discussed in the Introduction, there were a number of women scientists working at the turn of the century who were critical of the evolutionary theories of Darwin and his colleagues. Among these were Mary Calkins (1896), Helen Montague and Leta Stetter Hollingworth (1914), and Helen Thompson Woolley (1910, 1914), who criticised the observations of mental and physical sex differences, and constructed new research designs to test sex differences.[24] Helen Montague and Leta Stetter Hollingworth, in particular, used research designs that included subjects from a wide variety of races and classes (Montague and Hollingworth 1914). When sex differences *were* in evidence these women criticised the biological variability explanation, suggesting social factors instead.

Recall, for example, that the theory of greater male variability predicts that more males than females will be found at *both* extremes of any particular physical or mental scale. Some evolutionary theorists, such as Havelock Ellis (1894) argued that

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[24] A number of turn-of-the-century debates about evolutionary claims of sex and race differences can be found in the pages of *Popular Science Monthly*. Louise Newman has edited a collection of pertinent articles from *Popular Science*, entitled *Men's Ideas/Women's Realities: Popular Science 1870-1915* (Newman 1985).

the variability theory accounted for the higher numbers of males than females in mental institutions. Hollingworth responded by noting first, that the higher number of males is found only for those under the age of sixteen. She argued that "feebleminded" girls under sixteen were more likely to be "absorbed" quietly into the isolation of menial family chores, whereas the "deficient" boy who leads a more public life at school, etc. is more quickly found out and brought to the attention of the medical community. From this research she concluded that the lower numbers of women in institutions did not necessarily prove the biological determinism of the variability hypothesis (Hollingworth 1914, 515).[25]

In these early stages of Western feminist criticism of evolutionary theory most of the work was epistemological in focus, that is they focused on examinations of the truth of theories of women's lesser variability and intelligence, and the harmful implications these theories would have on pedagogical, legal and political reform for women and girls (e.g. Hollingworth 1914, 510-511). Concerns that this epistemological focus was not proving to be effective did not typically arise until the "second wave" of Western feminism in the middle of this century, and so the more troublesome *Epistemological* themes that develop out of this concern were rare in the earlier writings. However the criticisms of evolutionary theory by one early feminist, Antoinette Brown Blackwell, stand out for their prescient introduction of Epistemological issues (interestingly Blackwell was not a scientist).

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[25] My thanks to Meredith Kimball for highlighting Hollingworth's argument.

I will begin my review of the problems of feminist Epistemology with her work.

3.2 Blackwell: The Sexes Throughout Nature

Blackwell was a feminist contemporary of Darwin, and the first woman in the United States to be ordained as a minister (Newman 1985, 8). She responded to Darwin's theory of sexual selection in her book *The Sexes Throughout Nature* (1875). Blackwell agreed with Darwin that there are biologically-based differences in the mental capacities of men and women, but she argued against Darwin's view that the "feminine" instincts and tendencies are inferior to the "masculine." Rather, she viewed the feminine and masculine mental capacities as different but equivalent in quality and importance. And, she eloquently, if somewhat naively, argued that this "different but equal" status can be measured empirically:

> If the special class of feminine instincts and tendencies is a fair offset in every grade of life to corresponding masculine traits, this is a subject of direct scientific investigation. It is a question of pure quantity; of comparing unlike but strictly measurable terms. In time it can be experimentally decided, and settled by rigid mathematical tests. We do not weigh lead and sunbeams in the same balance [but we can still] estimate their equivalent forces (Blackwell 1875, 11).

There is no basis at this point for characterising Blackwell's belief in the empirical accessibility of sex differences as a troublesome Epistemological commitment to the correspondence theory of truth. However, a revealing conflict soon arises between

her belief that these facts can be straightforwardly accessed and her belief that bias *filters* our access. This latter belief suggests a relativism that relies on the representationalist scheme/content distinction I criticised in Chapter Two. Blackwell claimed that Darwin's theory of the inferiority of feminine traits was not based on objective empirical measurement (the empirical content), but was biased by his "extra-empirical" evolutionary commitments (his conceptual scheme). And, she claimed, this relativist problem will arise for any "positive thinker":

> Any positive thinker is compelled to see everything in the light of his own convictions. The more active and dominant one's opinions, the more liable they must be to modify his rendering of related facts—roping them inadvertently in to the undue service of his theories (1875,13).

Here, Blackwell argued, our subjective renderings of the objective facts of the world are "modified" through the filter of our conceptual schemes, ideologies or worldviews.

Immediately, scepticism becomes an issue. Why, in the face of this pervasive filtering of the facts, should we accept Blackwell's own subjective rendering of the facts as true? Blackwell responds to this scepticism by claiming that despite the inherently biased filtering of our representations, *her* particular bias, accrued by virtue of her experience as a woman, bridges the gap between her subjective views and the objective "facts of womanhood." She writes:

> However superior [the men's] powers, their opportunities, their established scientific positions, yet in this field of inquiry *pertaining to the normal powers and functions of woman*, it is they who are at a disadvantage. Whatever else women may not venture to study and explain with authority, on this topic they are more than peers of the wisest men in Christendom. Experience must have more weight than any amount of outside observation. We [women] are clearly entitled, on this subject, to a respectful hearing (1875, 6-7; emphasis in the original).

She makes no attempt to reconcile this objectivist claim about the Epistemic privilege of women's experience of womanhood with her more relativistic claim that worldviews and experience filter and *bias* the facts. On their own, each of these representationalist claims is troublesome, and causes more problems than are solved, but mixed together they produce an inconsistent, uncompelling thesis. This inconsistency weakens the persuasiveness of the important criticisms that make up the balance of Blackwell's book.

There is also the problem of over-generalising with respect to the categories "men," "women," "masculinity" and "femininity." These over-generalisations typically accompany feminist Epistemological claims of the sort Blackwell is making. According to her, there are "facts of womanhood" about which she has the authority to speak. The two problems of essentialism discussed in Chapter Two are in evidence here. First, Blackwell's argument entails the questionable essentialist conception that there are experiences that all women share, *viz.*, those experiences "pertaining to the normal powers and functions of woman." Second, she assumes that *her own* experiences are representative

and that, with respect to claims about women, these experiences provide her with a generic female Epistemic authority over all males. With the perspective provided by late-twentieth century feminist theorising we can see more clearly that her experiences are not generic, rather they are highly specific. She is an educated, American, white woman, she is not the generic "woman." These problems with essentialism will be revisited below.

The main focus of Blackwell's book is her contention that Darwin failed to apply his principles of evolution consistently in the area of sex differences (Blackwell, 1875 16). For example, she noted that if Darwin is right that selective pressure adds only to the *male's* endowment as the human species evolves, the inequality between the sexes, unless met with "a check in some unknown law...," would continue to increase "... to a degree which it is startling to contemplate!" (p. 19). Blackwell concluded that some checks will evolve in the future to prevent "too great an inequality between the sexes, [and therefore] it cannot be too preposterous to suppose that in the past and in the present similar natural checks always have been, and still are, in active operation" (*ibid*).

However, it is important to recall that Darwin acknowledged that in humans most traits are passed to both sexes, especially in the case of the "lower races" where, he claimed, less sex-differentiation had evolved. He did not believe that this process leaves men and women "equivalently endowed," certainly not in the case of Europeans. Unfortunately, Blackwell continues to

discuss the "various structural modifications" that have evolved as checks to maintain "a virtual equivalence of the sexes" (p. 20) without taking into consideration Darwin's differential claims regarding race.

I think this oversight in Blackwell's writing is related to her representationalist commitments. I noted in Chapter Two that over-generalisations about the categories "men" and "women" are not *necessary* aspects of feminist Epistemology, but I do think there are contexts in which they are indicative of the uncritical objectivist approach to categories of analysis that is often part of the representationalist model. Blackwell treats the categories "men" and "women" as naturally "given"—she just believes that Darwin had the facts about these categories wrong. However, Darwin's categories were often more fine-grained than Blackwell acknowledges. Rather than "women" and "men" he often used "European women" or "men of the 'lower races'." His racism and sexism are inextricably intertwined. It seems, then, that within this Darwinian context, Blackwell's writing evinces an uncritical ontological "rigidity" when she continues to impose her own over-generalisations about the categories "men" and "women" on to Darwin's more fine-grained (racist) theories of "barbarian" men and women of the "civilised races". If she has an empirical argument about the equivalence of the races that tells against Darwin's claims about race differences, then this argument needs to be provided, and it would be welcome. As it stands, she invokes the essentialist assumption that women of different races all share the *same* experiences "pertaining to the normal powers

and functions of woman," (namely, *her* own experiences). This essentialism results in the same sort of racism we find in Darwin's work when he assumed that women of different races had *different* experiences. Blackwell's criticism of Darwin's theory of sex differences could have been much stronger if she had shown a greater ontological flexibility about her categories of analysis.

3.3 Second-wave Epistemologists:

One hundred years after the publication of Blackwell's critique of Darwin, two papers by Stephanie Shields continue in this representationalist tradition: "Functionalism, Darwinism and the Psychology of Women" (1975) and "The Variability Hypothesis: The History of a Biological Model of Sex Differences in Intelligence" (1982). In both articles Shields documents the relationship between the scientific facts of variability and the cultural/political context within which those facts were "discovered." As a psychologist she focuses on the evolutionary claims of the greater variability in male intelligence, and the greater mediocrity of female intelligence. Because she assumes the representationalist split between the evidence or facts and the political values through which the evidence is screened, her writings contain conflicts similar to those in Blackwell's book. On the one hand, Shields makes the claim that Darwin's theory of sexual selection and greater male variability was filtered by the sexist ideology of Victorian culture (Shields 1975, 765; 1982, 771). On the other, she makes the claim that some Victorian contemporaries of Darwin, such as Karl Pearson (1897), were able

to remove their filters and get the scientific facts of variability *right* (Shields 1982, pp. 776-777). So again, there is a conflicting appeal to both objectivist and relativist claims about knowers and the world. Shields also leaves unexamined the question whether her own theorising is subject to cultural filters. Relatedly perhaps, is her sustained lack of acknowledgement that the "sexist" variability question in humans was also, for Darwin and his colleagues, a "race" question about the extent to which European men and women were different from men and women of "the lower races."

There are a number of other critiques by the second wave of Western feminist scientists-cum-philosophers that are more self-conscious about the existence of these conflicts, though they are not self-conscious about the representational model that gives rise to the conflicts. The influential essays of Ruth Hubbard, Cynthia Russett, and Ruth Bleier will be discussed in this regard.[26]

Ruth Hubbard's essay "Have Only Men Evolved?" parallels Shields' critique and focuses on both Darwinian and more recent sociobiological evolutionary theories (Hubbard 1983). A certain amount of ontological rigidity about the distinctness of sex categories and race categories appears early in the introduction to her essay when she explains her focus on the androcentrism of Darwinian accounts of sex differences. Here, she writes that "the ethnocentric bias of Darwinism is widely acknowledged" while its

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[26] Harding identifies these conflicts with an Epistemological position she labels "feminist empiricism" (Harding 1986a). But rather than suggesting feminists move beyond Epistemology completely, she prescribes "strong objectivity" as an Epistemological variant to empiricism (Harding 1991). I discuss my concerns with her Epistemological prescriptions in 5.1.

"androcentrism—is rarely mentioned" (p. 52). Hubbard's writing on this point does not take into consideration the fact that the androcentrism of Darwin's theory of sexual selection in humans was often applied differentially among the human "races"—the androcentrism and ethnocentrism are intertwined in his account.

Further representationalist commitments surface as Hubbard invokes a relativist scheme/content distinction in her description of the constructed nature of scientific knowledge, including evolutionary theory (Hubbard 1983, 46). For example, she explains that,

> every theory is a self-fulfilling prophecy that orders experience into the framework it provides. Therefore, it should be no surprise that almost any theory, however absurd it may seem to some, has its supporters (1983, 47).

It might seem from this that she is embracing the relativist side of the Epistemological debate and abandoning the objectivist view that a theory can be judged as true or false on the basis of its correspondence relation with the evidence. Indeed she writes that "[t]here is no such thing as objective, value-free science" (p. 47). However, she then spends the latter half of the paper presenting feminist evidence to counter the "false" Darwinian and sociobiological evidence about women. But why are we to be persuaded by *her* claims? Harking back to the conflicts in Blackwell's writings, Hubbard argues that despite the filtering of biases and worldviews, women scientists, such as herself, presumably, can "recognise an androcentric myth when they see one" and must "think beyond it" and "come up with ways of seeing

the facts and of interpreting them" (p. 66). Further, women scientists "can sift carefully the few available facts by paring away the mythology and getting as close to the raw data as possible" (*ibid*).

Hubbard is conscious of the paradox she invokes here, writing that "paring away the mythology" will be difficult because "women scientists tend to hail from the same socially privileged families and be educated in the same elite universities as our male colleagues," i.e., the biases and worldviews of these women will be similar to those of the men (p. 66). However, she claims that because she and her women scientist colleagues are at least "marginal to the mainstream" this should make it easier to "watch ourselves push the bus in which we are riding" (*ibid*). I am not convinced that merely invoking this quasi-Neurathian metaphor resolves the scepticism that arises when she appeals *both* to objectivism (e.g., in her claim about women's ability to "pare away the mythology") *and* to relativism (in her claim that "every theory... orders experience into the framework it provides"). In my view, Hubbard does not face up to the seriousness of the representationalist conflict, and again, the reader is left with an inconsistency that undercuts the important claims she makes against Darwinian and sociobiological theory.

Cynthia Eagle Russett's book *The Victorian Construction of Womanhood* presents an historical analysis of the early, Western psychology of sex differences (Russett 1989). Unlike the other feminist critiques surveyed, Russett's writing more fully acknowledges the racism of this science, though, like Hubbard, she

tries to separate the race categories from the sex categories and then justifies dealing exclusively with the latter. Russett explains:

> Race was a burning social issue in England and America. Abolitionist movements agitated the issue of black emancipation with increasing stridency. In this atmosphere science became a weapon, its findings useful as they legitimated or discountenanced the claims of black people to political and social equity (1989, 7).

However, according to Russett, the "women's movement" was even more challenging, and she uses this challenge to justify her focus on sexism:

> It even dared broach the subject of equality in personal, and especially matrimonial, relationships. Such assertiveness was more unsettling than the racial threat because it was more intimate and immediate: few white men lived with blacks, but most lived with women. Scientists responded to this unrest with a detailed and sustained examination of the differences between men and women that justified their differing social roles (1989, 10).

What she perhaps meant to say is that few white men lived with black men or women, but most lived with white women. Russett chooses instead to use over-general categories. When she uses the designation "women" instead of the term "white women," it appears that the term "blacks" refers only to black *men*.[27] She also fails to note that the "women's movement" she refers to was

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[27] This particular type of feminist essentialism is so pervasive that it has "inspired" a collection of essays by women of colour entitled *All the Women are White, All the Blacks are Men, But Some of Us are Brave* (Hull, Scott, and Smith 1982).

largely by and for white, middle-class women and the response from science was to highlight the sex differences between white men and white women.

Russett continues her discussion of women and the sex differences between women and men, speaking generically, without making any of these distinctions. For example, she describes the racism and sexism of evolutionists as resulting from a need to maintain some kind of hierarchy *within* the human species given that they no longer believed in a strict hierarchy which placed humans *apart* from other species. She writes that "women and the 'lesser races' served to buffer Victorian gentlemen from a too-threatening intimacy with the brutes" (p. 14). In other words, when evolutionists placed "'women' and the 'lesser races'" midway between "Victorian gentlemen" and apes, this eased the evolutionists' message about the ignoble nature of human origins. However, Russett needs to be more specific in her description here, because it was clearly *white* women and both women *and men* of the "lesser races" that had the buffering effect.

With respect to her methodological criticisms of the scientific research on sex and race differences, Russett begins with a straightforwardly epistemological account of the limitations of Victorian science. She characterises the Victorian science of sex differences as "bad" science that does not live up to the standards of scientific method required for today (p. 11). Further, and more importantly, she explains how, in many ways, it did not even live up to the scientific standards circulating among scientists at the time (pp. 182-188). She writes that eventually, however, "the

Victorian paradigm [of sex differences] erodes" due to improvements in scientific understanding:

> Together, genetics and endocrinology made some of the headier 19th-century theories of sex difference no longer tenable. Woman was not a lesser man. She was not man arrested short of developmental perfection. She did not stand midway between the child and the man (1989, 161).

Russett self-consciously rejects the relativist claim that scientific improvements regarding women's status were exclusively a result of "extra-empirical" circumstances. According to Russett, this more constructionist view of scientific change, described by Hubbard above, leaves insufficient room "for [the effects] of alterations in the scientific evidence, for factual or interpretive disproof, correction, and emendation" (Russett 1989,178).

While this rejection of the relativist elements of representationalism helps her avoid the problems found in the other feminist writings, her rejection, and thus her avoidance of the problems, is only partial. She concludes that although Victorian science was biased and flawed, it is actually not fair to expect anything more because science and ideology "far from being polar opposites, are part and parcel of one another. Scientists cannot help but bring cultural beliefs and interests in to the construction of their theories" (p. 188). Somewhat disengenuously she then argues that despite the filtering effects of these ideological preconceptions,

> good scientific practice, in the late 19th century as now, should not have permitted those preconceptions to distort the scientists' vision, to blinker them in such a way as to predetermine their conclusions... (Russett 1989, 189).

When representationalists like Russett view political concerns as "extra-empirical" filters through which bad science sifts the empirical evidence, then the sorts of inconsistencies and half-hearted claims I have highlighted, above, are bound to result. To foreshadow my positive thesis (see sec. 7.2), I suggest that we view political beliefs as members of the same holistic web of evidence occupied by empirical beliefs, rather than as filters through which the empirical beliefs pass. It is this latter representationalist view of evidence that guides the work of the feminist theorists I have been surveying. The representationalist split between values and empirical facts removes the conceptual framework that would allow feminists to document the empirical basis for our politically-motivated beliefs

Returning to Russett, she claims that there is the objective evidence, facts about "men" and "women" given up by nature, and there is our subjective rendering of the facts. If we follow some *a priori* notion of "good scientific practice" (about which Russett provides little detail) we can lessen the effects of our subjective filters and know whether the gap between the evidence and our theories has been sufficiently bridged. But the "inseparability" of scientific facts from the filters of subjective, value-laden ideology

means we can never *eliminate* the filters, allowing scepticism once again to make an unwelcome appearance.

Scepticism becomes a major factor in Ruth Bleier's arguments in *Science and Gender: A Critique of Biology and its Theories on Women* (Bleier 1984). Her main target is the determinism entailed in evolutionary and sociobiological theories of sex differences in mind and behaviour. In lengthy introductory and concluding essays on feminist methodology and "patriarchal science," she self-consciously addresses the conflicts between objectivism and relativism that I have been highlighting throughout this chapter. She begins with the characteristically relativist claim that scientific facts are constructed by those in power (p. 13). Consistently, at least, she notes that her own critique of sociobiology involves offering "counter facts" that are no less free of values and interests (*ibid*). In the face of the relativism she believes is inherent even in her own work, Bleier argues that the only justificatory criterion she can appeal to is whether or not her criticisms disrupt the status quo. Her explanation is worth quoting at length:

> I would argue that the nature of my own worldview, as it influences my approach in this book, is its own justification. That is, while biological determinists—in the face of overwhelming contradictions—assert the genetic, hormonal, and evolutionary determinism of human nature and our behaviors, it is my aim to describe all those myriad contradictions that make such theories totally inadequate as explanations of behaviors and forms of social relationships. Even if some of the "facts" I cite in support of my arguments are disputable, I will have made the case—and I hope

> convincingly so—that there is no simple "truth" as Sociobiologists and other supporters of the status quo would have us believe (1984, 13)

Here I think Bleier's representationalism has failed her. While I agree that there is no "simple truth" as understood by objectivists—that correspondence doesn't bridge the gap between subjective theory and objective reality like some Epistemologists might have hoped—this doesn't mean that we have to be resigned to Bleier's level of scepticism about the justification of our feminist claims. Rather, it adds fuel to Rorty's contention that we should choose a non-representationalist alternative to the subject/object gap.

In a later chapter "Patriarchal Science, Feminist Visions" Bleier reinforces the relativist version of scepticism introduced above. Again I will quote a passage at length as in it she articulates better than even Rorty or Davidson, how invoking the representationalist metaphor of a gap, filtered by our conceptual schemes, invites scepticism (unlike Rorty or Davidson, Bleier endorses the idea). She writes:

> Scientific ideas and theories represent efforts to describe and explain the natural world; that is, reality. That reality, in the form of our perceptions and interpretations of it, is like the rest of our culture, a product of human thought. Yet it is perceived as objective reality, which becomes incorporated, in its various forms, into our early and developing consciousness. That consciousness is the medium through which we perceive and interpret the "objective realities" of the external world, learn our individual location within it, and form a world view. That consciousness and its

> worldview provide a framework for ordering and interpreting our experiences, *which come to confirm the world view of which they are, in part, the products* (1984, 193; emphasis mine).

Bleier's thesis is now firmly mired in the relativism and scepticism Rorty warns about. How, if her own framework orders her experiences, can she persuade her readers of the truth of her claims? Bleier's response in this regard is a startling one that, in concert with similar themes in the writings of Evelyn Fox Keller, has left a lasting stamp on the field of feminist critiques of science. Bleier does not make Blackwell's claim that women are simply more objective in their studies of women, rather she suggests that truth and objectivity are *themselves* patriarchal concepts that require feminist (or feminine) reconfiguration.

At this point in her thesis she begins the self-conscious feminist examination of the Epistemological questions *"What property do all true theories share?"* and *"How do we know if we've found that property?"* by arguing that our traditional concepts of truth and objective method have been produced by a biased patriarchal discourse. Her discussion exemplifies the over-general approach to science found in many feminist Epistemological projects. The prescriptions for "science" and scientific method that result from her over-general analysis are exceedingly vague and it is difficult to see how they might be applied to an actual scientific setting. I discuss these concerns further in the next section.

3.4 Science, objectivity and masculinity:

Bleier begins her discussion of scientific method by reviewing the evolutionist claim that the "male" mind is objective and detached—the seat of Reason—whereas the "female" mind is subjective, emotional, and inferior. She then notes that the allegiance of science, including evolutionary biology, to objectivity and detachment, means that science "has defined itself" as "*the* expression of the male mind" (p. 196). Scientific "truth or its perception is contingent on being male" (*ibid*).

There are a number of problems with this conception of science as a masculine monolith. First, her claim anthropomorphises science. She writes that "science 'defines itself'," but surely it is individual scientists, text-book authors and their marketing strategists who do the defining. Second, her claim about the "male" nature of science is an over-generalisation of the claims made by the individual scientists involved. Bleier doesn't take into consideration the fact that many of the nineteenth-century evolutionary theories of sexual selection assign the "male" privilege of rational objectivity and detachment only to certain sorts of "male" minds, *viz.* white/European minds. So, if any gender related characterisations of this heterogenous institution of science can be made, then perhaps we might say that science is the expression of the *white* male mind. But then this more specific characterisation flies in the face of centuries of science produced by Asian, African and other non-white cultures. We have no evidence to suggest that the science produced by non-white cultures proceeds with less rationality, less objectivity than

does science produced in the West. We need to be careful that a feminist characterisation of science as "white" and "male" does not reinforce Darwinian evolutionary claims about the lesser rational abilities of non-whites.

Bleier encounters exactly this problem when she qualifies her discussion quite suddenly and explains that we associate science with the "male" mind because a highly specific group of men in "Western industrial class culture" have been trained to think a certain way—they have been trained to be in control. She provides no evidence for this claim, but continues, writing, "to know, to be certain, is part of being in control" (p. 202). She further explains the relationship between knowledge and control by arguing that "it is important to know causes for events and phenomena, for without that 'knowledge' one cannot know how to intervene effectively in order to remain or be in control" (*ibid*).

Women from Western cultures, she claims, have been trained differently. They have been socialised not to gain control but to be attentive to "context," "interaction," "process," and "nondualistic" modes of thinking. Very little in the way of careful description is provided for any of these terms. And then, speaking generically again, she asserts that "for women in general, control has been a non-issue" (pp. 202-203). She explains:

> Just as men were not taught or expected to think about parenting as a relevant issue for their lives or their self-definition before the contemporary women's movement, the question of being in control (of anything or anyone, including their own selves) was never part of women's

> frame of reference for conceptualizing or realizing their own relationships to others or to nature (p. 202).

The three-way, over-generalised association between men from Western industrial class culture, their training to be "in control," and the control required for knowledge in science, is said to explain the association between science and the "male" mind. In section 4.6, I present a contrast to Bleier's over-general, essentialist view, by discussing, for example, some of the research about the control many women, as mothers, have over children, and the control many white women have had and continue to have over black people, both women *and* men.

While the above discussion reveals Bleier's ontological rigidity about the categories "men" and "women," she makes it clear that she does not believe that sex differences regarding control are biologically determined. She invokes the psychoanalytic theories of Hein (1981) to explain that these essential differences between women and men arise from different patterns of *socialisation* (Bleier, p. 202). (Whether psychoanalytic theory escapes its own biological determinism is a question I examine in the next chapter).

Bleier claims that women have been socialised with a female style of thought that is nondualistic (201). Bleier explains that male thought, in contrast, continually invokes dualisms such as "subject-object, culture-nature, thought-feeling, active-passive" (p. 198) and that women find the opposite, "fluid" mode of thought more "easy" because they are attentive to context, interaction, and process (p. 201). She provides no evidence for these over-general

claims, though she occasionally mentions the psychoanalytic "suggestions" of feminist theorists Hein (1981), Fee (1982) and O'Brien (1981).

She argues, further, that the female Epistemic style provides clues for preserving a concept of knowledge and objectivity that is not filtered by the patriarchal worldview (*ibid*). According to Bleier, the dualistic thinking associated with males, obscures the "flux, change, and interaction" of "life and matter" (*ibid*). The male reasoning styles of control, detachment, and dualistic thinking *weaken* what would otherwise be good scientific practice (*ibid*). She argues that using a nondualistic women's reasoning style would improve the situation (*ibid*). She writes:

> While women certainly are,...educable to male-defined rules, they are more attuned to the fluidity of life, and acceptance of change, fusion, and interaction. Such experiencing of life more easily generates a sense of inclusiveness and contextuality as cognitive frameworks and modes of perceiving and understanding the world... (Bleier 1984, 201).

Again, she does not explain how she has arrived at these exceedingly obscure and over-general characterisations of the female Epistemic style. What *is* clear is that she believes a better science would result if scientists modelled the female style, "put aside" preconceived male notions of static dualities, and allowed the flux and change of nature to "speak" to the scientist (p. 206). However, she provides no concrete examples of how such a science would proceed. Her specific criticisms of evolutionary

theory from earlier chapters are unconnected from and superceded by these over-general Epistemological accounts of science, men and women.

In summary, Bleier uses psychoanalytic theories of the difference between female and male minds in order to construct a new Epistemological approach. This new approach is based on a feminine concept of objectivity that will both acknowledge the relativism of our socialised worldviews *and* provide a methodological foundation from which to justify feminist theory and science criticism. It is a very complicated project that I am not convinced will work, largely because it remains firmly in the representationalist mode. However, in order to do justice to the complexity I will now turn from feminist critiques of evolutionary theory, to feminist philosophy of science more generally, because it is in this latter body of work that psychoanalytic theories receive the most attention.

Scientist-turned-philosopher Evelyn Fox Keller has been one of the most influential feminist proponents of using psychoanalytic theory to explain how, in her view, our very understandings of objectivity and truth have been filtered by a masculine worldview (Keller 1982, 1983[1978], 1985). Like Bleier, she attempts a way out of this relativism by conceiving of the existence of a more dynamic, female (or at least nonmale) concept of objectivity to which feminists can then appeal when justifying their own claims. In the next chapter I examine Keller's project in detail, adding to the many published criticisms of her work my own criticism of her continued reliance on a

representationalist understanding of objectivism and relativism. The problems Keller's project encounters are probably some of the best illustrations of why a change from representationalism would be a really good idea for feminists.

Chapter 4:
Keller's Epistemological Reflections on Gender and Science

In one of her earlier essays, Keller discusses and supports what I have characterised as the feminist move from epistemology to Epistemology. (Keller 1982). She writes that feminist criticism of unwarranted inferences from data to theory, sloppy methodology and other epistemological concerns typically affects the truth of theories only in the "'soft' sciences," such as evolutionary biology (1982, 593). Keller is after a more "radical" Epistemological critique that would detect "androcentric bias even in the 'hard' sciences, indeed in scientific ideology itself" (*ibid*). She prescribes a two-fold Epistemological task to this effect.

First, she writes, it is necessary for feminists to "distinguish that which is parochial from that which is universal in the scientific impulse, reclaiming for women what has historically been denied to them" (1982, 593-594). That which is parochial is the association of objectivity with masculinity, domination, and control (594). Women, she argues, have typically been denied any association with objectivity, parochial, or otherwise (*ibid*).

Second, feminists have to "legitimate those elements of scientific culture that have been denied precisely because they are defined as female" (*ibid*). This point is less clear. The male associations with science receive more attention in her essay than do female associations. But she does give brief mention to three "repressed" elements of scientific culture that are associated with

the female and need to be legitimated by feminists. Her descriptions of these "female" elements make use of psychoanalytic concepts that are poorly defined. (I will try to draw out the details of Keller's psychoanalytic project shortly.) The first of these repressed elements is female "subjectivity" (p. 594); another is the cultural association between feminine gender identity and "ego merging" (a psychoanalytic term used in contrast to masculine "ego autonomy"); and, although she doesn't label it "female," the reader is to infer an association between females and the "erotic impulse," another psychoanalytic concept that she associates with "union" or "merging" (p. 598). The relation between "ego-merging," the "erotic-impulse" and scientific *method*, is like-wise sketchy, but she does write that the erotic impulse can be contrasted with the aggressive impulse that we associate, in science, with masculinity, power, and control over the objects of study (*ibid*).

Keller's thesis, then, is that, in science, the "universal" ideal of objectivity has been confused with a "parochial" version that associates objectivity with masculine aggression, and the detachment from and domination over the object of scientific study. Further, science has devalued *alternative* elements of the universal objective ideal that are associated with females, such as subjectivity, and identification and union with the object of study (1982, 593-594). She argues that if we become conscious of the sexist ideology that fuels these mistaken conceptions of objectivity, then we can choose to reject what has been associated with the male and embrace what has been associated with the

female (1982, 598). Feminists must not let objectivity lose its "*intrinsic* meaning" (593, emphasis mine), but must work to "transform" the traditional understandings of objectivity (603) in order to create a truly objective science, that is freed of its patriarchal moorings. The representationalist element in her appeal to this *ideal* notion of objectivity becomes apparent when she argues that straying from the objective ideal would invite relativism—the only other Epistemological option for representationalists (e.g., Keller, 1982, 593). She writes:

> Feminist relativism is just the kind of radical move that transforms the political spectrum into a circle. By rejecting objectivity as a masculine ideal [relativism] simply lends its voice to an enemy chorus and dooms women to residing outside of the Realpolitik modern culture; it exacerbates the very problem it wishes to solve (Keller 1982, 593).

For Keller it is crucial that feminists keep the ideal of objective science *apart from* the sexed associations of males with objectivity and females with subjectivity. Otherwise, she writes, feminist criticism of the association between objectivity and male domination, for example, could mistakenly be construed as a criticism of objectivity *simpliciter*. And as we've seen, Keller believes that a criticism of this sort necessarily involves a move toward relativism, a move she rightly recognises as disasterous for feminism. She argues that we need a non-relativised concept of objective knowledge to give normative force to our important political claims (Keller 1982, 593). But, as I will argue in Chapter Six, we don't need objectivist Epistemology to provide this

normative force. Indeed, as I've attempted to show, adopting *any* position on the Epistemological continuum only makes our way more difficult.

Keller argues that the relativised *male* concept of objectivity is merely an *ideology* of objectivity which is "linked with autonomy and masculinity, and in turn, the goals of science with power and domination" (1982, 594). Her alternative to a relativised male concept of objectivity is *dynamic* objectivity, the universal ideal of objectivity which represents "the quintessentially human effort to understand the world in rational terms" (*ibid*).

Keller's work contains examples of all three of my concerns with feminist entry into the Epistemological debate. First, as we found with Bleier's arguments, Keller's critique moves away from examinations of specific scientific theories and toward a much more general target—the monolithic conception of science as male (mid-way through her essay Keller begins referring to "Western" science [p. 598]). As I have argued, however, this is probably not the most important target for feminists to attack. At the same time I doubt that the target is conceptually coherent. Second, as with all of the feminist Epistemologists discussed in Chapter Three, Keller makes extensive use of over-generalised categories such as "female" and "feminine." She also insists on the explanatory efficacy of these over-generalisations when called to defend them (as I will discuss in section 4.4). This ontological rigidity about her categories of analysis, along with her discussion of ideal objectivity, leads to my third and overarching concern

about the invitation to scepticism that arises from Keller's acceptance of a (feminist version of) representationalist Epistemology. I will highlight all three of these concerns throughout my discussion of Keller's work, beginning with her use of psychoanalytic theory.

4.1 Keller and psychoanalytic object relations theory

Keller writes that the two-fold task of feminist science criticism is aided by a psychoanalytic study of various psychological processes affecting the character of scientists and science itself (Keller 1982, 595). She argues that the sex symbolism of science is explained by a psychoanalytic account of the childhood development of individual scientists.[28] The psychoanalytic view Keller prescribes is called "object relations theory." She refers to the work of psychoanalyst D. Winnicott (1971), and two feminists who make extensive use of the theory, Nancy Chodorow (1978) and Dorothy Dinnerstein (1976).

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28 Keller adds to her psychoanalytic thesis an historical account of the linking of science with objectivity and masculinity, highlighting the scientific writings of Francis Bacon. Her use of Bacon has been effectivley criticised by Edrie Sobstyl who challenges feminist overgeneralisations about the historical relationship between scientific method and masculinity (Sobstyl, "Gender and Science: Some Disquiet About the Masculine Mind of Science," unpublished manuscript). For example, Sobstyl compares Susan Bordo's claims that René Descartes was "fleeing the feminine" in himself (Bordo 1987), with Keller's claims that Bacon was "subduing the feminine" aspects of nature (Keller 1982, 598-599). Given that Descartes and Bacon arrived at completely opposite understandings of what constitutes "scientific method," one championing deduction and rationalism, the other induction and empiricism, how, Sobstyl asks, can we use patriarchal domination to explain both? The only way we can do this, she argues, is by glossing over crucial details of their work and the reception of their work by their peers and later generations of scientists.

I should note that within much North American psychology and psychiatry, the validity of observations made in the case study reports of psychoanalysts has long been questioned. Philosophers too have engaged the validity question (e.g., Popper 1962, 1974 and Grünbaum 1984). Popper, famously, argued that psychoanalytic theory was not a science because its hypotheses could never be falsified: "it [is] practically impossible to describe any human behaviour that might not be claimed to be a verification of these theories" (Popper 1962, 36). Grünbaum agrees that psychoanalytic method is "epistemically flawed" although he disagrees with Popper about its lack of falsifiability (Grünbaum 1984, 124). For the purposes of my dissertation I will leave aside the question of the validity of psychoanalytic observations of differing cognitive styles between individual children, for example. I will confine my criticisms instead to the biological determinism inherent in the psychoanalytic *explanations* for those observations. This determinism produces generalisations about two biological categories, namely, "female" and "male." I argue that there are numerous counter instances to these generalisations.

Briefly, psychoanalytic object relations theory predicts that when females are the primary caregivers, the girls and boys in their care will differ in their patterns of gender, cognitive, and emotional development. These different patterns result from the different ways that children have of *relating* to the *objects* (including people) in their world—hence "object relations."

Feminists who make use of object relations theory retain a number of parallels with Freud's original theory of psycho-sexual development, but typically they focus on the social and relational rather than the instinctual life of developing children. For example, Chodorow argues that from a relational, rather than an instinctual standpoint, pre-Oedipal and Oedipal children need to turn to a father because he is a "non-mother" primary figure (Chodorow 1978, ch. 6). Connection with a non-mother provides relief for their fragile developing egos as they strive to differentiate themselves from the all-encompassing mother (*ibid*). She argues that a boy has a lessened need to turn to the father at this stage because the mother sets the boy apart as an object of her (hetero)sexual desire, allowing the boy to develop a sense of himself *apart* from his mother. Further, his male identity is enhanced by turning *away* from the female mother and the feminine in himself. Girls are not set apart in this way and often feel overwhelmed by their mothers, especially if their mothers identify with their daughters as similar selves (*ibid*).[29]

Like Freud, Chodorow hypothesises that girls, more than boys, will turn to the father as a result of penis envy. For Chodorow, penis envy enters the clinical picture because the boy's independence is embodied in his penis, the site of difference from the mother. The daughter has no such site of difference. Thus the penis is envied as the daughter longs for independence and

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[29] One of the first overgeneralisations required by object-relations theory is the assumption that all mothers have a feminine gender identity and are heterosexual.

liberation from her mother. The penis functions as a symbol of power and independence (Chodorow 1978, 123-125).

Unlike Freud, Chodorow highlights the importance of the girl's continued relationship with her mother, as well as highlighting the social rather than instinctual elements of the girl's motivation. For example, Chodorow suggests the girl's envy of the penis may also be a result of her realisation of the preferential treatment her mother gives to those who have a penis. The girl discovers that her mother desires and prefers those people with penises. The girl "comes to want a penis, then, in order to win her mother's love" (Chodorow 1978, 125).

4.2 Object relations, objectivity and science

These differences in psycho-sexual development are said to have parallels in cognitive development, or "ways of knowing." Recall the claim that male children learn a masculine gender identity by relating to their female mother in behavioural opposition to her and all that is feminine. The mother encourages this by setting the male child apart as an object of her (hetero)sexual desire. This masculine pattern of coming to know the world by distancing, and being distanced, from the primary object in one's world is different from the learning pattern that accompanies a female's feminine gender formation. Female children learn their feminine gender identity by closely modelling their behaviour after their mother, which she encourages. A female child's pattern of coming to know the world involves empathetic understanding, modelling and identification. This

difference in the gender socialisation of children is said to be the first step in the association between males, masculine gender development, and a way of knowing marked as "objective"—a style that puts distance between the knower and the known. Females, in turn, come to be associated with feminine gender development, and a subjective, relational process of knowing.

In the following passage, Keller describes the link that feminist work in object relations theory suggests between cognitive, emotional, and gender development:

> Our cognitive ideals...[are] subject to the same psychological influences as our emotional and gender ideals. Along with autonomy, the very act of separating subject from object—objectivity itself—comes to be associated with masculinity. The combined psychological and cultural pressures lead all three ideals—affective, gender, and cognitive—to a mutually reinforcing process of exaggeration and rigidification. The net result is the entrenchment of an objectivist ideology and a correlative devaluation of (female) subjectivity (Keller 1982, 595-596).

Feminist object relations theorists claim that for some male children, the objective cognitive style can develop from the normal need for autonomy (the differentiation of "self" from "other") to an exaggerated need to dominate "others." This is the same masculine, objectivist ideology that Keller sees in science. She writes:

> I invoke psychoanalytic theory to help illuminate the forms of expression that [the] impulse [to dominate] finds in science as a whole, and its relation to objectification in particular. The same questions I asked about the child I can

> also ask about science. Under what circumstances is scientific knowledge sought for the pleasures of knowing, for the increased competence it grants us... and under what circumstances is it fair to say that science seeks actually to dominate nature? (Keller 1982, 597).

Continuing with this linking between the masculine gender development of male children and the masculine gender development of science, Keller explains that the male child/science comes to define his/its self as "not female" as *different from* mother/nature. Further, the child/science is not immune from the disdain with which the larger social context treats any feminine associations, such as that of empathetic, relational knowing. From Keller's viewpoint, then, it is hardly surprising that a feminine, subjective process of knowing is devalued in science, at the expense of more masculine, objective modes of knowing (1982, 596).

Keller provides a contrast to masculine, objectivist domination with "dynamic objectivity" which involves the more feminine idea of "letting the material speak to you." The endorsement of such a naive view of induction seems odd coming from Keller, given that she makes numerous criticisms of Bacon, himself a champion of a similarly naive inductive approach. Keller sees this more feminine approach in the work of geneticist, Barbara McClintock. Keller writes that McClintock's "major criticism of contemporary research is based on what she sees as an inadequate humility" (Keller 1985, 162). McClintock reports that "much of the work done [by others] is done because one wants to impose an answer

on it," when, for her, the most successful approach is to "just let the material tell you" (McClintock in Keller 1985, 162) Keller despairs that while most scientists *observe* their object of study, they do not "encounter the object as such, in its own fullness" (p. 166). The "feeling for the organism" of study is missing with many scientists (Keller 1982, 599).

In these passages from Keller and McClintock, we are presented with a fairly straightforward, if not naive, version of the representationalist view. The material of study is described as offering itself up, if only the scientist is willing to be truly objective, relinquishing her desire to impose her subjective ideological filters over the otherwise "natural" data, clearing the bridge between the objective world and the inner subjective mind. More free from her dominating impositions, the scientist, it is claimed, can better empathise and identify with the data. While Keller sees this alternative, feminine approach embodied in McClintock—a woman—she tries to de-emphasise the biological determinism of psychoanalytic theory. She argues that McClintock's approach can be, and is, practised by both men and women, because "neither science nor individuals are totally bound by ideology" (1982, 599). In the next section I will argue that Keller's attempt to downplay the biological determinism is inconsistent with the larger psychoanalytic model to which she is committed.

Keller also argues that just as the processes or modes of acquiring knowledge have been gendered, so too have the products—i.e., the theories and models. She explains: "individuals

drawn by a particular ideology will tend to select themes consistent with that ideology" and, conversely, reject themes inconsistent with that ideology (1982, 600). Keller provides an example of the favouring of "master molecule" models over interactionist models, in cellular biology. The former models assume a hierarchical approach, whether it involves claims that the nucleus operates in a top-down fashion over the rest of the cell, or that genes provide information for the rest of the cytoplasm with no two-way interaction between them. Interactionist models, such as the one McClintock used in her research, are more complex and allow for feedback between components of the genetic material in a non-hierarchical fashion. Keller argues that one of the reasons McClintock's work was not initially accepted by her masculine science colleagues was because the favoured master molecule models are more easily aligned with masculine symbolism of domination and control, than are the interactionist models that McClintock used (1982, 601).

There is, in this latter argument of Keller's, the same vagueness and over-general approach to terminology that I noted in the Epistemological writings of Bleier. While Keller's claims that "ideology plays a role in the choice of theory" make sense, especially, I will argue, if we view ideological beliefs as wholistically related to more straightforwardly empirical beliefs, we still need to be precise about our characterisation of the ideology in question. As Popper would remind us, arguing, as Keller does, at this over-general level almost *any* phenomenon can be read as symbolic of "masculine domination and control."

4.3 A mid-point review

Keller's 1982 essay encourages feminist analysis of the psychoanalytic factors at work in the masculine gendering of male scientists and science itself, at the same time that she champions alternative methods and theories that have been historically downplayed because of their association with the feminine gendering of females. For Keller, the utility of analysing the masculine gendering of male scientists and science results from her belief that neither male scientists nor science are slaves to gender. She believes that revealing the unnecessary relationship between the ideology of objectivity, masculine symbolism, domination and control will allow scientists to see that they have a choice to abandon this relationship and embrace feminine modes of knowing, to produce a more universal, less-parochial, dynamic objectivity (1982, 598).

> I will suggest that we might...use feminist thought to illuminate and clarify part of the substructure of science (which may have been historically conditioned into distortion) in order to preserve the things that science has taught us, in order to be more objective (589).

Recall that Keller wants to salvage a dynamic notion of objectivity, in order to avoid the relativism of claims that objectivity *itself* is relative to masculine gender identity. Dynamic objectivity, she claims, does not necessarily have this "parochial" connection with gender—anyone can develop the more feminine, cognitive style—so it can provide a universal, non-relative

foundation for justifying our good science. It would seem from this that Keller, like Bleier, is relying on the important feminist distinction between biological sex and socialised gender roles (despite her almost exclusive use of sex terms in this regard). One of the implications of the sex/gender distinction is that gender is not necessarily determined by sex—one can be male without being masculine, for example. Any scientist, no matter what their sex, can, and should, develop the more feminine dynamic objectivity.

Unfortunately, Keller's use of psychoanalytic theory restricts this case for individual gender flexibility. No matter how much she writes of our individual freedom from "gendered ideology," object relations theory is, at bottom, an account of how the *anatomical* similarity and difference between the child and the child's primary caregiver explains the cognitive capacities of boys and girls, *as grouped by sex*, not as gendered individuals. Boys with female primary caregivers (these mothers are feminine by definition) *will* develop the capacity for the ideologically-charged autonomous objectivism; there is no explanation given for how they might deviate from this. Girls with female primary caregivers *will* develop the capacity for the preferred dynamic objectivity, and it seems impossible that they could develop the more negative autonomous objectivism. According to Keller's use of object relations theory, then, only females are able to develop dynamic objectivity. It is the method used by one half of humanity (at the most) and does not provide Keller with a neutral or "universal, human" foundation to which she can appeal to ward off relativism.

4.4 Accepting biological determinism at what cost?

In later writings (*Reflections on Gender and Science* 1985; "The gender/science system" 1987), Keller downplays these inconsistent claims about the "freedom of association" between sexed individuals and their gender, acknowledging the biological determinism of object relations theory. She accepts that in some ways, masculine gender development is causally reducible to the interplay between the male child and the female mother while feminine gender development is causally reducible to the interplay between the female child and the female mother. For example, in chapter four of *Reflections on Gender and Science*, she notes that the development of objective, autonomous cognitive styles, though "relevant for children of both sexes," will come to be associated with only one sex, if children of both sexes have a female as a primary caregiver (1985, 85). And, relatedly,

> it is important to recognize that, although children of both sexes must learn equally to distinguish self from other and have essentially the same need for autonomy, to the extent that boys rest their sexual identity on an opposition to what is both experienced and defined as feminine, the development of their gender identity is likely to accentuate the processes of separation (1985, 88).

She explains later that "the relevance of gender to science is (a) a socially constructed relevance [brought about by parenting patterns], but (b) *carried* by the sex of its participants" (1987, 43, emphasis in the original).

In this passage she insists on the usefulness of questionable generalisations about human cognitive capacities—generalisations based on membership in the categories "male" or "female." While acknowledging the deterministic relationship between sex and gender increases the consistency between her use of psychoanalytic theory and her writings about the associations between gender and science, the consistency is purchased at the expense of explanatory power. Again, assuming that omnipresent female caregivers are the statistical norm for most of the cultures that have produced modern scientists, Keller is unable to explain the development of her prescribed dynamic objectivity in male scientists, something she clearly wants to do (see, for example, Keller 1985, 175). In the case of Barbara McClintock—Keller's prototypical example of dynamic objectivity in action—the problem of explanation is reversed. Keller acknowledges that although McClintock exhibits dynamic objectivity, McClintock did not have an omnipresent mother, and she has never been a mother herself. Keller attempts to redescribe what looks to be a lack of fit between theory and evidence by explaining that "'however atypical (McClintock) is as a woman, what she is *not*, is a man'—and hence under no obligation to prove her masculinity (i.e., she does not have to enforce her autonomy by separation from her subject of study)" (Keller 1987, 42, emphasis in the original). What Keller is left with, then, is a *modified* version of dynamic objectivity that discourages dominating male objectivism while encouraging non-male (no longer *necessarily* female) relational and nurturing approaches to objectivity. However, even

this modified version fails to defeat relativism because dynamic objectivity is still *relative* to the cognitive capacities of only one category of persons—*non-males*. Because of Keller's insistence on using these over-generalisations about non-males (females?) and males, her representationalist attempts to defend objectivity from relativism continue to be unsuccessful, and her thesis is considerably weakened as a result.

Her insistence on the utility of these over-generalisations is related to representationalism in other ways as well. Her insistence suggests that she views the categories "male" and "female" with a representationalist eye—as if they were uncomplicated, natural categories. In her discussion of the psycho-sexual relationship between cognitive capacity and sex/gender, Keller assumes that sex/gender categories are uncomplicated by race, culture and class, for example. However, it is not obvious that sex/gender categories (even if they were not collapsed together) are isolatable from other well-documented features of human identity, nor is it obvious that they mark the only, or even the primary, difference in cognitive capacity (assuming again that psychoanalysts have indeed documented such differences).

4.5 Complicating sex/gender:

Elizabeth Spelman responds to the questionable essentialism of object relations theory, in her book *Inessential Woman: Problems of Exclusion in Feminist Thought* (Spelman 1988, ch. 4). She notes that Chodorow, for example, while critical of much of

Freud's psychoanalytic theory, still retains his universalist, over-general views about the parent/child relationship. Chodorow portrays women's experiences as mothers and daughters, and men's experiences as fathers and sons as uniform and monolithic. She is, of course, aware of differences *within* the categories "woman" and "man" such as "white," and "Chinese," "working class," and "middle class." She argues, however, that these differences are secondary to the primary similarities between women as mothers and daughters, and men as fathers and sons (e.g. Chodorow 1978, 77, 137, 175). While Chodorow doesn't speak of the essential characteristics of all women and men throughout history, she still sets her arguments within less-than-nuanced historical and cultural contexts (e.g., she often refers to mothering in "industrial late-capitalist society" [1978, 32], or "Western industrial society" [57]).

Spelman hypothesises that for feminists who are white and whose education provides an economic privilege, sex/gender may indeed be their only site of difference from the male oppressor, who is also typically privileged and white. But surely, Spelman argues, it would be a mistake to generalise this particular experience of masculine oppression, to the experience of oppression of all women, even within the modern, industrialised West (Spelman, 97-100). She quotes a chilling passage from Daryl Dance who explains how many black mothers' concern for their children extends beyond a sexist conception of patriarchy:

> The Black mother has a more ominous message for her child and feels more urgently the need to get the message across. The child must know that the white world is dangerous and that if he does not understand its rules it may kill him (Dance 1979, 127, quoted in Spelman, p. 98).

Marilyn Frye makes a similar point in her book *The Politics of Reality*: "Because we white women have been able to think of ourselves as looking just at *women* and *men* when really we were looking at white women and white men, we have generally interpreted our connections with these men solely in terms of gender, sexism and male dominance" (Frye 1983, 124).

What this means, of course, is that *even if* sex/gender is the only site of difference for white, middle-class women, race and class still operate in the oppression these women experience. To have sex/gender as the *only* site of one's oppression, *requires* that one be of a certain race, class, sexual orientation, age, linguistic grouping, etc. (Spelman 104-106). In this way it is difficult, and questionable, to try to conceptually isolate the effects of sex/gender from that of the other prominent features that mark the human experience of oppression. Even acknowledging other experiences of oppression by "adding" them on (for example, by adding the experience of white supremacy to a black woman's experience of patriarchy) still treats these experiences as discrete and isolatable variables of a woman's life. Against this, many black women and other women of colour have spoken of their difficulty in isolating the sexist or racist elements of a particular experience of oppression (e.g. hooks 1981, 12 - 13).

Further complications for sex/gender categories come from writings by and about transsexuals who feel part of neither the female nor the male world and refuse to be classified one way or the other (see Rothblatt 1995; Herdt 1994; Bornstein 1994).

Returning to object relations theory, we have to ask whether it is conceivable that boys and girls relate to their mothers only or even primarily on a discrete sex/gender dimension. It would seem more likely that the sex/gender dimension is complicated by the child's and the parent's sense of where they fit in the larger human context of racism and classism, for example, *as well* as sexism. Boys and girls don't grow up to be "generic" men and women, but as specific men and women (and, for some transsexuals, for example, even this more specific sex categorisation remains ambiguous). Spelman rightly questions how this specificity is accounted for by object relations theory (Spelman 1988, 97).

4.6 Object relations and feminist standpoint theories

Keller's use of object relations theory to articulate the difference between male and nonmale approaches to knowledge is modeled on a Hegelian/Marxist view of Epistemology. Both Hegel and Marx argued that in hierarchical social worlds, one's material position in the hierarchy affects the extent of one's knowledge, or, more radically, affects one's ability to recognise truth itself (see Marx and Engels 1964). A number of feminist theorists have modeled this materialist approach to Epistemology explicitly, using object relations theory, or variants, to describe a feminist or

woman's "standpoint" (e.g., Hartsock 1985, 1987). Hartsock argues that in a social hierarchy divided by sex, what men can know is partial or distorted, and what women or feminists can know is Epistemically superior or more objective (*ibid*).

Unfortunately, as in Keller's work, the conceptual problems with object relations theory can also be found in these feminist standpoint accounts. For example, in her essay "The Feminist Standpoint: Toward a Specifically Feminist Historical Materialism," Hartsock uses object relations theory to focus on women, generally, as mothers and daughters (Hartsock 1985, ch. 10). She explains: "In addressing the institutionalized sexual division of labor, I propose to lay aside the important differences among women and instead search for central commonalities across race and class boundaries" (Hartsock 1985, 233).

Aside from the problem of isolating the effects of race and class, Spelman notes that the decision to "lay aside important differences among women" does not typically involve the general problem of deciding whether to focus on the similarities among or differences between women. Instead, the problem, as she and many others have pointed out, is typically that of conflating, for example, the conditions of white, heterosexual, able-bodied, middle-class women with the conditions of *all* women. What becomes apparent is that any problematic differences between *women* are actually the differences between the first group of women and all others. Spelman explains:

> The focus on women "as women" has addressed only one group of women—namely, white middle-class women of Western industrialised countries. So the solution has not been to talk about what women have in common, as women; it has been to conflate the condition of one group with the condition of all and to treat the differences of white middle-class women from all other women as if they were not differences (Spelman, p. 3).

Relatedly, Hartsock encounters the problems of relativism that I reported in Keller's work. For Hartsock, there is an objective or Epistemically privileged vantage point for gaining the truth about power relations (Hartsock's particular interest), but her account of this objective view becomes *relativised* to the standpoint of women (or feminists). For example, Hartsock writes:

> Whereas Marx relocated power on to the epistemological ground of production, I argue that women's lives provide a related but more adequate epistemological terrain for understanding power. Women's different understanding of power provides suggestive evidence that women's experience of power relations, and thus their understanding, may be importantly and structurally different from the lives and therefore the theories of men. I suggest that, like the lives of the proletarians *vis-à-vis* capital, women's lives make available a particular and privileged vantage point not only of the power relations between men and women but on power relations more generally (Hartsock 1985, 151).

A final concern with those feminist standpoints derived from object relations theory is that often, masculine gender development is equated with dominating and objectifying others, in a way that makes sexism the *model* for other sorts of

oppression (e.g., by "adapting" male domination over females to explain whites dominating blacks, capitalists dominating workers, or science dominating nature) (Spelman, 1988, 85). Sexism is also often described as the *cause* of these other sorts of oppression. Hartsock writes, for example, that using object relations, "one might then turn to the question whether capitalism rests on and is a consequence of male supremacy" (1985, 262). The claim here, says Spelman, is that "if men weren't so insecure about their sense of self vis-à-vis their mothers, they wouldn't need to define anyone else as Other" (Spelman 1988, 85). However, this equation between masculine identity and the development of racism, for example, makes it difficult, if not impossible, to understand the development of racism in women, unless these women have absent mothers and omnipresent fathers. But surely the population of racist women is larger than these unusual parental configurations would allow.

bell hooks also voices concern about the equation of masculine gender development with the development of racism and classism, in her book *Talking Back: Thinking Feminist, Thinking Black* (hooks 1989, ch. 4). She points out that this equation allows women to mask their own role in oppression and domination. She writes: "women can and do participate in politics of domination, as perpetrators as well as victims" (hooks 1989, 20). Women can be racist and homophobic, for example. Similarly, Biddy Martin and Chandra Mohanty (1986) highlight how white, privileged daughters participate in oppressive systems because they *share* race and class with *both* their parents. The

similarity in experience between daughters and fathers is undertheorised in object relations theory (Martin and Mohanty 1986, 204). hooks examines the ways in which women, despite the predictions of object relations theory, participate in oppressive relationships, particularly *as* mothers (e.g., 1989, 20). She writes:

> Even as I speak, women who are ourselves exploited, victimized, are dominating children. It is necessary for us to remember, as we think critically about domination, that we all have the capacity to act in ways that oppress, dominate, wound (hooks, 1989, 21).

Bat-Ami Bar On (1993, 92-93) is also critical of the romanticising of mother/child relationships that necessarily neglects the unequal power dynamic involved.

4.7 Representationalism continued in "The gender/science system"

In the paper "The gender/science system," Keller continues with her representationalist arguments both for the importance of objectivity in science and for sex/gender as a primary, isolatable, natural kind (Keller 1987). There are two features of her arguments in this 1987 paper that exhibit representationalist commitments. The first is that Keller again argues that abandoning objectivism can lead only to relativism. The second is her acceptance of the coherence of scepticism.

With respect to the first feature, Keller argues that science is a "maximally reliable (even if not faithful) representation of nature" (p. 46). Fair enough (assuming that her use of the word "representation" is not enough to make a diagnosis of

representationalism). But, she then reports that *to think otherwise* is to embrace a "post-modern" relativist alternative—the view that what counts as true in science will not depend on nature but will, instead, be relative to the politics of various scientists (p. 48). The representationalist model severely limits the options here—relativism is the only other alternative to an objectivist view of science.

Her argument also involves the second feature of representationalism I mentioned, namely an acceptance of scepticism. Keller adopts the view that there is a gap between the scientist as subjective representer and the objective world as the data represented. As I have argued, this gap injects the unnecessarily sceptical view that while our scientific representations must be faithful to be true, the representations are metaphysically separate from the external world so their fidelity can never be guaranteed. While Keller has a *certain* amount of faith in the accuracy of scientific representations (they can be "maximally reliable," even if not faithful), she continues her argument against relativism by offering the sceptical observation that "nature" is "ultimately unrepresentable" (though, contra the relativist, nature *does* exist) (*ibid*).

It appears that she is aware that no objectivist argument can satisfactorily explain how it is, on the representationalist model, that we can get *completely* outside our subjective skins to accurately represent the outside world of nature. But, for Keller, this is a more satisfactory level of scepticism than that found in the relativist claim that the truth of the outside world is somehow

constructed by us. However, as I will argue in Chapter Six, neither of these representationalist options needs to be satisfactory for feminists, because there is another option available.

In the case of debates about the priority of gender to race and class, Keller criticises the view that gender is "infinitely plastic" (1987, 38). According to Keller, feminists need to acknowledge the important relationship between gender and sex—we can't ignore the "recalcitrance of sex" (1987, 48). She writes that feminist theorists who concentrate on the "proliferation of difference" between women of different races, classes, etc. are humbled in the face of the sameness that is our sex (1987, 48). And again she offers a sceptic's warning: "Neither nature nor sex *can* be named out of existence. Both persist, *beyond theory* as humbling reminders of our mortality" (1987, 48, first emphasis in the original, second emphasis, mine).

In this passage, our mortal sexed natures are viewed as fixed givens beyond which we cannot and should not stretch our feminist theories about the plasticity of gender. Again, Keller is trapped by the representationalist model. While she is critical of the objectivist view that scientific method provides a direct or infallible correspondence between our theories and nature (nature and sex, she says, exist *beyond* theory), she is also concerned about a radical relativism where the truth about nature is relative to our conceptual schemes. In the end she decides to be a disillusioned objectivist and accept scepticism—nature, like sex, must exist out there somehow, but both remain "ultimately unrepresentable." This disillusioned view of the powers of human

investigation fails the feminist project of investigating and eliminating the oppression of women by science. *Pace* Keller, many feminist investigations of oppression in science have been found to be accurate and reliable. There is no mystery to the phenomenon of oppression, only a fallibilist's sense that we still have more to investigate. By imposing Epistemological limitations on the scope of scientific investigation, Keller's criticisms serve to undercut the progress of the very feminist projects she seeks to support.

Keller continues her discussions of gender and science in her 1992 collection of essays *Secrets of Life, Secrets of Death.* Here her defense of the priority of gender over other categories within race and class is not necessarily based on the biologically-determined relationship between gender and "recalcitrant sex" but rather on the fact that in science, at least, everyone is typically white and economically advantaged, so race and class can be "bracketed" from the discussion (e.g., "Gender and Science: An Update," 1992b, note on p. 17). She does note that the conception of gender bracketed in this way is necessarily specific "to a particular subset of Western culture" (*ibid*) which is, of course, also true of her conception of science. However, she then defends the importance of gender categories, generally, to discussions of science, generally, with the following:

> Gender and gender norms come to be seen as silent organizers of the mental and discursive maps of the social and natural worlds we simultaneously inhabit and construct—*even of those worlds that women never enter* [i.e., most of science] (p. 17; emphasis in the original).

But by the same token, these worlds are also not inhabited by certain sorts of *men*, (indeed *most* men) so I remain unconvinced of Keller's arguments for the natural *priority* of sex/gender categories as uncomplicated "givens," especially when discussing as diverse an institution as science.

I will return to this collection of Keller's essays in my next chapter. There I discuss how, in these more recent writings, Keller takes a decided turn away from objectivist Epistemology, but because she remains in a representationalist framework, she ends up embracing the only other alternative, *viz.*, an unneccessarily high level of relativism. She is joined in this troublesome move toward relativism by the arguments of two other highly influential feminist science critics, Harding (1991, 1993b) and Longino (1987, 1990).

Chapter 5

From Objectivism to Relativism in Feminist Epistemology

In the introduction to *Secrets of Life, Secrets of Death*, Keller rejects some of her earlier views about objective method, "non-male" or otherwise (1992a, 4). She no longer believes that objective method is that which distinguishes theories based on "ideology" or myth from theories based on "fact." Her new views

> represent a shift from my earlier preoccupation with the frailties of description, and in one respect at least, a departure from my initial confidence in the possibility of identifying certain beliefs as "myth-like," as distinct from other beliefs that are, by implication, "myth-free." Such a notion now seems to me suspiciously reminiscent of the old demarcation between "truth" and "ideology," or between "good science" and "value-laden science," demarcations that are themselves residues of the copy theory of truth (1992a, 4-5).

In representationalist terms, she no longer believes that objective method involves identifying which theories have the truth-conferring property of correspondence, and which are based merely on ideology. In another essay in the collection, "Critical Silences in Scientific Discourse" she writes of "abandoning the hope for a one-to-one correspondence with the real" (1992c, 73).

However, it is clear throughout these essays that her abandonment of objectivity is based on what she sees as a failure of execution, not of conception. For Keller, correspondence is indeed the sort of relation we need to bridge the metaphysical gap

between us and the world, but we can't construct successful bridges because they are always blocked by the influence of cultural conceptual schemes. She explains: "Since nature is only accessible to us through representations and since representations are necessarily structured by language (and hence, by culture), no representation can ever 'correspond' to reality" (1992a, 5). Again, for the representationalist, this criticism of objectivism inevitably leads to some version of relativism. All our "representations" have been filtered through our subjective language scheme or culture.

How, then, are we to choose between subjective representations if none has the objective truth-conferring property of one-to-one correspondence with the external world? Keller suggests that we choose those representations that facilitate certain "interventions," to use Ian Hacking's phrase (Hacking 1983). Specifically, we should choose those interventions that best suit our political goals. In the following passage Keller explains the options she believes this sort of conceptual relativism leaves for feminists:

> Since it is demonstrably possible to envision different kinds of representations, we need now to ask what different possibilities of change might be entailed by these different kinds of representation? For this, we need to understand the enmeshing of representing and intervening, how particular representations are already committed to particular kinds of interventions. Is there, for instance, a sense in which we might say that the program of modern genetics already has, written into its very structure, a blueprint for eugenics? Or that nuclear weapons are

> prebuilt into the program of nuclear physics? And if so, what kinds of theories of the natural world would enable us to act on the world differently? (Keller 1992, 76).

From the above quotation it is clear that, unlike her earlier warnings against relativism, she now "accepts" the instrumental role of subjective lingusitic filters in our choice of theories. However she is still concerned to acknowledge the role of the "non-linguistic" realm, i.e., the role of the objective reality the theories describe. For example, in her essay "Gender and Science: an Update" Keller writes that for feminist critics who take the objective success of science seriously the new task is to answer the question "How do 'nature' and 'culture' interact in the production of scientific knowledge?" (1992b, 36). But discovering how these two metaphysically distinct realms interact becomes as much of a problem for Keller as it was for Descartes. Keller is in good company when she is unable to provide a compelling answer. Despite her switch from objectivist searches for truth to instrumentalist searches for success, the representationalist elements remain, as does the scepticism and relativism. In the next section, I argue that Sandra Harding and Helen Longino encounter similar problems.

5.1 Harding on Objectivity:

Paralleling Keller's views in her 1992 collection, Sandra Harding's recent work on feminist standpoint theory is critical of the claim that objective method consists in detecting a one-to-one correspondence between true representations and the world

(Harding 1991, 1993b). But, like Keller, Harding does not provide a criticism of representationalism, *per se*; rather she is critical of the clarity of the correspondence relation. Harding argues that certain aspects of culture, namely the social standpoint of the representer, filter the correspondence between any one representation and the world represented. As with Hartsock, this is Harding's version of the Marxist claim that one's lived reality, one's social standpoint, will "organize and set limits" on one's understanding of the world (Harding 1993b, 54).

In "Rethinking Standpoint Epistemology: What is 'Strong Objectivity'?" (1993b), Harding explains her commitment to the general tenets of standpoint theory:

> The starting point of standpoint theory—and its claim that is most often misread—is that in societies stratified by race, ethnicity, class, gender, sexuality, or some other such politics shaping the very structure of a society, the activities of those at the top both organize and set limits on what persons who perform such activities can understand about themselves and the world around them. ... In contrast, the activities of those at the bottom of such social hierarchies can provide starting points for thought—for *everyone's* research and scholarship—from which humans' relations with each other and the natural world can become visible. This is because the experience and lives of marginalized peoples, as they understand them, provide particularly significant *problems to be explained* or research agendas (1993b, 54; emphasis in the original).

Note, here, the move away from an objectivist reliance on the over-general categories of sex/gender that were found in Keller's

arguments. Harding discusses, instead, the complex ways in which many oppressive forces shape the lives of marginalised peoples. Her justification for the value of hearing from marginalised peoples seems also to be free of the inconsistent objectivist claims found in Keller's earlier work. Harding does not rely on claims about the different and/or more objective cognitive *style* of marginalised peoples, rather she makes the less problematic claim that starting scientific inquiry from their lived experience would introduce different, and long-neglected *content* for scientific examination ("problems to be explained").

Unfortunately, Harding soon makes a number of objectivist claims that conflict with those in the above passage. For example, right after the quoted passage about "the lives of those at the bottom of social hierarchies," she returns to a more over-general prescription of starting science from "women's" lives, borrowing from the standpoint theory of Dorothy Smith (see Smith 1987a 1987b). According to Smith, and Harding, *all* women share the common work of "caring for bodies" (Harding 1993b, 55). In a footnote, Harding acknowledges that grouping women together like this might be inappropriate, because wealthy women, for example, don't have to "care for bodies" as much as poor women do. But, she claims, wealthy women still have to do it more than their wealthy brothers. However, a number of poor and/or non-white men *do* participate in "caring for bodies," and it might be that, on this particular labour axis, these men and their impoverished sisters share a standpoint *more closely* than do wealthy women and poor women. Similarly, in many places,

Harding reduces her detailed descriptions of science from "Western, bourgeois, homophobic, white, [and] sexist" to, over-simplified descriptions such as the "male bias" of the "most fundamental categories of scientific thought" (e.g., Harding 1986b, 652).[30]

That Harding's project involves a continuing objectivist commitment to the over-generalised notion that "science" is simply "gendered" or "sexed" and that sex/gender categories are unconstructed or "given" by nature is evidenced in subtle ways, most recently, in her introduction to *The "Racial" Economy of Science* (Harding 1993a). Here, she explains that "racial" is put in scare quotes to denote the constructed, contested nature of race and the racism of science (Harding 1993a, 2[footnote]), which seems right. However, she never uses scare quotes in discussions of sex/gender and the sexed/gendered nature of science (see, for example, her phrase "'race' and gender" [Harding 1993a, 11]). While Harding has no trouble questioning the ontological naturalness or primacy of race categories, sex/gender categories receive no such creative deconstruction.

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30 One wonders, though, whether her more detailed descriptions of science as bourgeois, homophobic, sexist, *white* and *Western*, are accurate, given that science is and historically has been practised in a number of non-Western countries, by non-white peoples. Perhaps the science practised in these countries is not the sort of science Harding is criticising, but then what evidence do we have that these countries are free from bourgeois, homophobic, and sexist science? We need to be careful about explanations of wherein the difference between "Western" and "non-Western" science lies. The familiar problems of over-generalising about "science" are encountered here, but are exacerbated by Western, including Western feminist, tendencies to romanticise and exoticise Eastern cultures. On the Western tendency to exoticise the East see Edward Said *Orientalism* (1978).

Another conflicting objectivist claim arises when Harding writes that "starting off research from women's lives will generate less partial and distorted accounts not only of women's lives *but also of men's lives and of the whole social order*" (Harding 1993b, 56, emphasis mine). That "starting research from women's lives" might produce *different* (more appropriate?) scientific research about *women* is a claim that needs a lot more detail and documentation than she provides, but it is, I think defensible. That starting research from women's lives might produce *increased objectivity*, or decreased distortion in scientific studies about *women* might also be defensible on a case-by-case basis, presumably by showing that certain women scientists have fewer biases about women than do men scientists. However, the claim that research started from women's lives will provide *increased objectivity* about *men's lives and the whole social order* is a much more problematic claim that encounters precisely the same problems found with the standpoint views of Hartsock and the early Keller.

What Harding is claiming is that while different degrees of opacity accrue to different social standpoint filters, not all social standpoints generate equally partial representations or beliefs. The social standpoints of women, or feminists with "maximally liberatory social interests" for example, "have generated less partial and distorted beliefs than others" (Harding 1991, 144, 148).

> The history of science shows that research directed by maximally liberatory social interests and values tends to be better equipped to identify partial claims and distorting assumptions, even though the credibility of the scientists who do it may not be enhanced during the short run. After all, anti-liberatory interests and values are invested in the natural inferority of just the groups of humans who, if given real equal access (not just the formally equal access that is liberalism's goal) to public voice, would most strongly contest claims about their purported natural inferiority. Antiliberatory interests and values silence and destroy the most likely sources of evidence against their own claims. That is what makes them rational for elites (pp. 148-149).

Because she argues that *all* beliefs have a social filter, Harding disavows the claim that the standpoints of women or feminists will produce "true beliefs"—just less partial, less distorted ones than those produced by "antiliberatory interests," for example (1991, 185,149). Paralleling the problems encountered by Hubbard on this point, Harding purchases some consistency by claiming that *all* knowledge is somehow distorted, but this claim robs her of the foundation she then needs to argue that the knowledge produced from some standpoints is less distorted, generally, than that produced from others. Is this latter claim distorted too? She needs to be able to answer "no" but her relativist claims about the filters affecting *all* our knowledge claims preclude this answer.

Also of concern is the over-general nature of the standpoints she invokes. While there are certainly areas about which some people have more objectivity than others, her claim that women,

as a group, have more objectivity about the whole social order than do men as a group is empirically unsupported. In her article "Marginality and Epistemic Privilege" (1993) Bat-Ami Bar On adds some important conceptual criticisms in this regard.

Bar On argues that the existence of multiple systems of marginalisation (e.g., *within* groups of women, and *within* groups of men) problematises claims of the epistemic privileging of any one marginalised group. "Is any one of these groups more epistemically privileged than the other, and if that is not so—if they are equally epistemically privileged—does epistemic privilege matter?" (1993, 89). Clearly she thinks it does not, and I agree. If *every* marginalised group can claim some sort of Epistemic privilege, the claim of any one *individual* group loses its bite. And, in any case, along with the increase in the number of claims to privileged standpoints (labourers, women, lesbians, the disabled, etc.) there is a decreased focus on the relationship between the standpoint and the day-to-day practises of scientists. Again, diagnosing the opression resulting from these day-to-day practises is crucial for feminist critics of science.

For Harding, even with the less partial view provided by the standpoint of women or feminists, *objective, true* knowledge is impossible to attain. This does not mean, however, that objectivity has no role in Harding's project. Just as Keller reconfigured scientific method, Harding reconfigures objectivity, to give it a new role.

In her book *Whose Science? Whose Knowledge? Thinking From Women's Lives*, Harding describes the traditional view of

objectivity as "objectivism," which results in a "semi-science" that "turns away from the task of critically identifying all those broad, historical social desires, interests, and values that have shaped the agendas, contents, and results of the sciences much as they shape the rest of human affairs" (1991, 143). She prescribes, instead, "strong objectivity" which extends the idea of scientific research "to include systematic examination of... powerful background beliefs" thereby "maximising objectivity" (p. 149). Again, following Marx, and sharing Keller's new goal, Harding articulates a representationalist role for "strong objectivity," namely the critical examination of the linguistic or social filters, "the powerful background beliefs," that continually block our knowledge-seeking of the non-linguistic natural realm.

Before I assess this new feminist reconfiguration of objectivity, I want to briefly examine one more example of the trend, in the work of Helen Longino (1987, 1990). Longino argues against some feminist accounts that equate objectivity with value-free scientific method (Longino 1987, 60). Longino suggests, instead, that objective, good science is always biased with "contextual values," that come from our "interpretive frameworks," such as the interpretive frameworks of particle physics that guide "observations" of elementary particles in cloud chambers (54). Much of science, she claims, is guided by these interpretive "contextual values" so diagnoses of bad science as that which is "biased by contextual values" and unobjective, won't fully capture the problem (56). What we need to do is redefine

objectivity as that which allows us to better examine the influence of these interpretive frameworks. She explains:

> We cannot restrict ourselves simply to the elimination of bias, but must expand our scope to include the detection of limiting and interpretive frameworks and the finding or construction of more appropriate frameworks. We need not, indeed should not, wait for such a framework to emerge from the data (1987, 60).

According to Longino, all our theories are underdetermined by the supporting data, so, ultimately, our choices between theories are relative to these interpretive frameworks rather than the data (1987, 61). This is very similar to Keller's and Harding's claims that theories, even "good" ones, are always filtered by social experience, or culture.

5.2 From objectivism to relativism:

Though Longino, Harding and Keller reject the claims that objective method involves impartial (value- social- culture-free) detection of one-to-one correspondence, they all seem to accept the metaphysical gap of representationalism that correspondence sets out to bridge. But, as we have seen with Keller's earlier work, if we accept the metaphysical confines of representationalism, but criticise objectivist attempts to detect correspondence, then our only choice is to move dangerously close to relativism. This seems to be the fate of the arguments provided by all three theorists. Each is left with a "watered down" prescription for feminist scientific method which, they tell us, can only be used to

determine how the filter of culture intervenes between the world and scientific knowledge. All our knowledge is relative to our conceptual filters. Once our new "maximally" objective method has helped us identify the values, culture and politics which comprise the conceptual scheme guiding our theories, the best we can do is pick the theory screened through the most appealing (to feminists) and/or least partial conceptual scheme (though, as I have argued above, Harding's argument for "least-partial" is inconsistent). For Keller this new method results in a resigned scepticism: "nature" is ultimately unrepresentable. Similarly, Harding claims that there can be no such thing as a "true" representation of nature.

This feminist move toward relativism might not seem obvious at first, especially when Harding and Longino label their theories as "objective" accounts. But, on closer representationalist diagnosis, their theories are relativist accounts of which conceptual schemes makes for the best ("objective") filters between us and the world. Why it is that some version of relativism has been prescribed by Keller, Harding, and Longino as the most reasonable Epistemological position for feminist science and feminist science-criticism?

Although Longino is the only one to use the term "underdetermination," the problems with relativism encountered in the writings of Harding, Keller and Longino all seem to stem from a representationalist use of underdetermination theory. Underdetermination theory, often associated with Quine, has both uncontroversial and controversial elements. In its most generic,

uncontroversial form it claims that scientific theories are underdetermined by the evidence brought forward in their support, i.e., 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 (Quine 1981, 28-29). Part of the controversy arises from an apparent corollary, namely, given that some scientific theories *are* chosen over others, these choices must be relative to a political "worldview," "explanatory scheme," or "paradigm," rather than just the evidence (Quine's views on this corollary are hard to pin down, see Bergström [1993] for a review of this point). Aspects of the arguments of Longino, Keller and Harding support this relativist interpretation, which they view as an improvement over the objectivist claims of one-to-one correspondence between theory and evidence.

One example in Longino's work comes from her analysis of competing anthropological theories for interpreting the use of ancient chipped stones (Longino 1990, 103-111). One theory, highlighting the role of the male hunter, interprets the stones as hunting tools. The other, competing theory, includes the role of the female gatherer and interprets the stones as implements for gathering and preparing edible vegetation. According to Longino, the available evidence supports both theories equally well, so the choice of the male-focused model or the more inclusive model must be *relative* to an underlying *political* commitment, namely to androcentrism or feminism, respectively (e.g., Longino 1990, 109).

Here we have an example of the representationalist claim that political values screen or filter the empirical evidence about which we form beliefs, but that the values are not themselves beliefs with empirical content.

Longino and others who use this relativist version of underdetermination theory make a compelling case for the reasonableness of the position. However, in my view, it is still a dangerous use of relativism for feminists to deny that their political values have any relation to empirical evidence. Further, this acceptance of relativism is completely unnecessary. That it is dangerous, is, I hope, obvious. But that it is a danger that can be avoided might not yet be so obvious.

First, it is important to note that the existence of two theories that are supported *equally well* by all the available evidence has proven to be difficult to document (Bergström 1993, Brown 1995). This is not to argue against the relativist problem of underdetermination theory, *in principle*, but to suggest that examples of competing hypotheses are hard to come by in the real world, and I will argue shortly that Longino has not found one in the chipped stones example. Typically, one theory is supported by some aspects of the evidence while the competing theory is supported by other aspects, so the choice between them can then proceed on a non-relativist discussion of, for example, which aspect of the evidence is the most relevant.

But the main reason why the relativist account of underdetermination theory can be avoided is because it is predicated on the unnecessary, and untenable, representationalist

view that "evidence" and "political considerations" emanate from two metaphysically separate spheres—the first from the objective, external world; the second from the subjective, internal mind (or minds, as when political views are said to be "socially constructed"). On Davidson's account, discussed in the next chapter, underdetermination does not have to have the relativist implications of the representationalist model, because his account does not rely on a metaphysical split between "outer" and "inner" worlds.

I now return to Longino's representationalist use of underdetermination theory to illustrate more clearly the development of a dangerous amount of relativism in what is, otherwise, a highly compelling discussion of feminist science.

5.3 Longino and feminist science:

In Longino's 1987 essay "Can There Be a Feminist Science?" she previews the major themes of her book *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry* (1990). One of these themes is the debate over the criteria for what makes feminist science "feminist." As I will discuss further, below, Longino convincingly argues against the feminist claims that feminist science is marked by a "feminine" style of reasoning, or theoretical content that matches the "feminine" propensity for "interactionist" vs "linear" causal explantions. In *Science as Social Knowledge* she articulates alternatives to both these feminist accounts and to the traditional accounts of objectivity in science (Longino 1990).

As an alternative to traditional objectivist accounts, she argues that scientific knowledge is a social product, which sometimes, of course, can weaken objectivity, but often it can actually *increase* objectivity. She writes:

> I will argue that there are standards of rational acceptability that are independent of particular interests and values but that satisfaction of these standards by a theory or hypothesis does not guarantee that the theory or hypothesis in question is value- or interest-free. ... [T]he development of knowledge is a necessarily social rather than individual activity, and it is the social character of scientific knowledge that *both protects it from and renders it vulnerable to* social and political interests and values (1990, 12; emphasis mine).

In other words, objectivity in science can be *increased* by the community exchange of ideas and criticism that balances out the biases of any one individual.[31]

As an alternative to the objectivism of some feminist accounts, Longino argues against the conflation of a "feminist" science with scientific theories that are characterised as "feminine" or "female,"—a conflation I noted in the earlier writings of Keller, and which Longino finds in the work of Bleier :

> Some theorists have written as though a feminist science is one whose theories encode a particular worldview, characterized by complexity, interaction, and wholism. Such a science is said to be feminist because it is the expression

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31 Although Longino presents this as a view of objectivity in science that contrasts with the traditional, logical positivist's approach to objectivity and values, Ernest Nagel makes exactly the same point in his book *The Structure of Science* (1961, 489). Similar remarks about the role of the scientific community can be found in Popper, e.g. his idea of "inter-subjective agreement" (1959).

> and valorization of a female sensibility or cognitive temperament. Alternatively it is claimed that women have certain traits (for example, dispositions to attend to particulars and interactive and cooperative social attitudes and behaviors rather than individualist and controlling ones) that enable them to understand the true character of natural processes (which are complex and interactive) (1990, 187)

Longino is appropriately concerned that this approach to feminist science uncritically embraces gender stereotypes (1987, 53).

Longino criticises the associations of "feminist" with "feminine" or "female" not only in characterisations of certain theories, but in characterisations of scientific method as well, especially as the associations appear in feminist standpoint theories (Longino 1987, 53; 1993b). Here, again, I think these criticisms can be applied to Keller's earlier work. Keller's feminist prescription of dynamic objectivity was based on the association between dynamic objectivity and typically female (or non-male) cognitive traits. While critical of the proposed "feminine" derivation of these standpoints, Longino also criticises the very idea that women share a standpoint, irrespective of its etiology. She writes that "women are too diverse in our experience to generate a single cognitive framework" (*ibid*). While Harding, too, is clearly aware of this diversity, her own work on standpoint theory is inconsistent in this respect, as we have seen. She often makes uncritical use of sex/gender categories as, for example, when she prescribes "start[ing] thought from women's lives, *or the lives of people in other marginalised groups*" (Harding 1991, 157,

emphasis mine). This is the "all the women are white" generalisation identified in section 3.3.

Finally, Longino is also critical of the idea that feminist science aims to "reveal the truth that is hidden by masculine 'bad' science" (1987, 53). This suggests the ideal of a value-free objective science similar to the objectivist prescriptions found in Keller's earlier work. Longino argues that even good, objective science is value-laden. What makes Longino's approach to science feminist, she claims, is not an alignment with research or methodology that is gendered "feminine," but an alignment with research or methodology that explicitly supports feminist political values and goals.

5.4 Longino, underdetermination theory and relativism

Longino outlines two sorts of values that, she claims, are part of even the best science: those values that are constitutive of scientific practice; and those that affect the context in which science is practiced (1987, 54). The constitutive values govern "what constitutes acceptable scientific practice" (*ibid*). The contextual values are the background values, explanatory schemes, or political commitments that each scientist might bring to her laboratory.

Against more traditional philosophers of science, and paralleling, to some extent, the work of Thomas Kuhn (1972), Longino argues that the second set of values—the contextual values—play an active role not only in what some call the context of discovery, but also in "the inner workings of scientific inquiry"

or the context of justification (1987, 54). These contextual values parallel, in many ways, the "paradigms" in Kuhn's writings. Longino discusses the example of the highly theoretical values that guide the otherwise indirect "observations" in particle physics (*ibid*). Contextual values *must* play a role in scientific justification and theory-choice, she argues, because we can't choose theories on the basis of the evidence alone. Given that theories are underdetermined, theory-choice, even "objective" theory-choice is always based on something more than evidence (*ibid*).

Thus, Longino explains, there is "no formal basis for arguing that an inference [from data to theory that is] mediated by contextual values is thereby bad science" (1987, 55). It *could* be bad science, but the presence of contextual values is not the deciding factor. Indeed, she argues, the influence of contextual values in "the inner workings of science" can be part and parcel of good science as usual (p. 56). This opens the door for her to market feminist science, and feminist science criticism—two obvious sites of contextual values at work—as good science as usual. She claims that feminist scientific practice will be *good* science *insofar as* it "admits political considerations as relevant constraints on reasoning, which, through their influence on reasoning and interpretation, shape content" (p. 61).

This is a provocative move on Longino's part, but suggesting that political considerations are relevant *in addition to* considerations of evidence produces a relativism that has a very limited market appeal and even this limited appeal is paid for with an unnecessarily high price. As with Harding and Keller,

Longino provides a sophisticated critique of correspondence theory and the objectivism end of the relativism/objectivism duality. However, by moving closer to the relativism pole she gives away too much. As will become clearer in Ch. 7, I will suggest that we view political considerations as *further elements* of evidential reasoning, rather than as filters that "constrain reasoning." The former account of political considerations removes the relativist implications encountered by Longino.

5.5 Symptom: Relativism; Diagnosis: Representationalism

There are a number of typical representationalist elements in Longino's move toward the relativist corollary of underdetermination theory. Illustrating how the contextual values of our worldview play a role in science, Longino discusses the role of feminist and non-feminist background assumptions in the "woman-the-gatherer" vs. "man-the-hunter" interpretations in anthropology and in the selection of interactionist vs. linear models used to "mediate data" within sex-hormone research (e.g., Longino 1990, chs. 6-7; 1987, 58).

In the latter case, the interactionist model of the influence of sex hormones highlights the two-way interaction between the presence of prenatal hormones and resulting physiological changes at the pre- and postnatal cellular and macro levels. Linear models focus on a more deterministic, one-way relationship where the prenatal presence of hormones are assigned all, or most, of the causal power, with little, or no causal attention given to the feedback from the rest of the system either

pre- or postnatally. This linear model is very similar to the "master molecule" model that Keller described as "masculine" (Keller 1982).

Longino is also critical of the linear model, but not because she identifies it with any particular gender symbolism. Examining the competing hormonal theories, Longino, and her research partner Ruth Doell, found sexism and androcentric bias in many aspects of the linear theory research, though they did not find that sexism or androcentric bias affected the inferences from data to theory in any straightforward way. Instead, they claim that the inferences were affected at a deeper level by *prior* commitments to patriarchal political ideals, which, *in turn*, affected commitments to the linear explanatory model. Inferences from data to theory within the level of the linear explanatory model were found to be sound.

Longino describes the patriarchal contextual values, and the linear explantory model associated with them, as screens or filters in the process of scientific justification. For example, she writes:

> In the conduct of research [explanatory models] serve as background assumptions against which data are ordered, in light of which data are given status as evidence for particular hypotheses and as a context within which studies gain significance (1990, 135).

Longino explains that commitments to the linear hormonal explanatory models were in turn affected by objectionable, patriarchal politics. The scientific result is a one-way, deterministic view of prenatal hormones in control of adult

human behaviour. Longino describes this as a patriarchal, hierarchical view of human behaviour which limits understanding of "human capacities for self-knowledge, self-reflection, [and] self-determination" (Longino 1987, 58). Longino prescribes the non-linear, interactionist model instead, because self-knowledge, self-reflection, and self-determination are part of a feminist political vision or worldview (p. 59).

I agree with Longino when she argues that being a woman or a feminist does not remove the political bias involved in the choice of an interactionist model over a linear one, nor does it, *pace* Harding, make the choice "less partial" or "maximally objective." I also agree when she argues that interactionist models should not be chosen because they are seen as "the expression of 'women's nature'" (1987, 61). However when she writes that an interactionist model should be chosen by feminists, "because of explicitly political considerations" (*ibid*) her representationalism weakens her case. Surely, there is *another* reason for feminists to choose an interactionist model, namely that there is *evidence* that the interactionist model is better than the linear model.[32] The models are not equally well-supported by the same body of evidence. Even Longino writes that the interactionist model "allows not only for the interaction of

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32 Lynn Hankinson Nelson makes the same observation about Longino on this point, but Nelson's diagnosis is from a Quinean rather than a Davidsonian perspective (Nelson, *Who Knows? from Quine to a Feminist Empiricism* 1990, 238-239; see also Nelson 1993). Insofar as Nelson makes use of Quine's nonrepresentationalist moments, I am generally in agreement with her proposals. However she does not discuss Quine's more Epistemological tendencies and I am unsure, as of yet, whether this influences her feminist thesis.

physiological and environmental factors but also for the interaction of these with a continuously self-modifying, self-representational (and self-organizing) central processing system"—something that the linear model *cannot* do (1987, 58). But, says Longino, this is not enough. "Obviously model-choice is also constrained by (what we know of) reality, that is, by the data. But reality (what we know of it) is, I have already argued, inadequate to uniquely determine model choice" (61).

My sense is that Longino's use of the hedge "what we know of reality" is the same sort of scepticism we find in the arguments of Harding and the later Keller. It is the scepticism that results from conceiving of a metaphysical gap between the raw data of the world, out there waiting, and our organising schemes primed to filter the waiting data. The organising filters of feminism or androcentrism block unmediated knowledge of reality, serving as preconceived explanatory frameworks which organise the raw data of sex hormones, for example. In Longino's words, again, explanatory models "serve as background assumptions against which data are ordered, in light of which data are given status as evidence" (1990, 135).

In Longino's description of feminist scientific practice as one that "admits political considerations as relevant constraints on reasoning, which, through their influence on reasoning and interpretation, shape content" (1987, 61) she parallels Keller and Harding who split the "political" (what Keller calls "culture," and Harding calls "social standpoint") from the "evidence." The political serves as a conceptual scheme that filters the evidence, so

we can never be guaranteed against massive error in our theories. For all Longino, Harding and Keller know, all our theories might be "floating free" of the real world because the theories are part of our representational system of explanatory models or political worldviews, which filter reality. If this is the case, then, Longino is right: when choosing between representations that are underdetermined by the evidence, we are free, to some extent, to choose between those that have been screened according to our feminist political inclinations.

But wait. We shouldn't give up on the potentially decisive role of evidence so soon. Just because correspondence doesn't bridge the metaphysical gap, doesn't mean we have to be resigned to this level of relativism. Let's just abandon the Epistemological project completely by jettisoning the representational metaphor of the gap. So say Rorty and Davidson, or so, at least, I have been hinting. It is time, now, to make good on the promise to illustrate a non-representationalist alternative that would free feminists from the Epistemological ties that bind.

Chapter 6:
A Pragmatist, Davidsonian Alternative

As I argued in section 2.1, "representationalism" names the most comprehensive diagnosis of the sceptical symptoms plaguing Epistemology. To review, in the representationalist model, beliefs are conceived as representations of their objects. In the most elementary cases these beliefs are said to be the subjective end product of a sensory process whereby the objects in our world are sensed and then screened through our perceptual frameworks (the filters of our worldview or language). This argument was most apparent in the selections from Quine and also from Bleier. Theories are viewed as the combination or systematisation of beliefs. Sometimes the resulting theory is said to feed back into the filtering system, so that our allegiance to the theory affects our ability to perceive new data. Selections from Hubbard and Bleier supported this view, as did underdetermination theory in the way used by Keller, Harding, and Longino.

The representational conception of beliefs as filtered representations of the world explains the scepticism that fuels Epistemology. If there is a metaphysical distinction between subjective beliefs and the objective world, then some sort of Epistemic bridge is needed to link the two. However, if beliefs do not arise from direct access to the world, if a bridge is needed to link them, then we have injected the possibility of global error. All our representations could be inaccurate because our bridge

might be blocked by the filters of our perceptual apparatus, language, cultural worldview and/or theory allegiance.

Objectivists on the Epistemological continuum argue that (ideally) we can provide objective reasons for thinking our representations are accurate by checking for whether the correspondence relation that would bridge the gap between our representations and the world represented is clear or blocked by conceptual filters. There is some disagreement between more straightforward objectivists and empiricists or instrumentalists about how we check for correspondence with theories containing nonobservational language, but most agree that it can be done for theories containing observational language, at least. Some of Quine's work supports this view, as does van Fraassen's. With respect to observational language, they all agree that the method for detecting correspondence is objective, i.e., it is an external checking method available to all (qualified) participants.

Unfortunately, as I noted, there has been little satisfactory progress within the various objectivist programs. Certainly all the feminists whose work I surveyed came to argue against objectivism as a live Epistemological option. Within the confines of the Epistemological continuum this leaves some version of relativism as the only alternative.

Relativists make the negative point that checking for the clarity of the correspondence relation can never be an objective process. They argue that our subjective conceptual schemes filter this meta-process just as they filter our belief-acquisition process. However, because this relativist point accepts the view that

correspondence is *needed* to bridge the representationalist gap, the relativists' doubts about the ability to objectively detect correspondence apply equally to the relativists' *own* Epistemological claims. This is, indeed, the sceptical worry found in the recent writings of Keller, Harding, Longino and earlier in Bleier.

I have suggested, however, that there are more pursuasive arguments against objectivism (*and* relativism) which tell us that the representationalism on which they are based is an optional model. Davidson, for example, argues for an alternative to representationalism that is *not* premised on the coherence of scepticism. If we are convinced that, on Davidson's model, global scepticism is a non-starter, then we no longer have any motivation for continuing the Epistemological debate about how best to address scepticism. I hope that the previous four chapters have provided support for my claim that "unmotivating" the Epistemological debate would be a liberating move, especially for feminist critics of science. Davidson's role in this liberatory project is explained below.

6.1 "A reason *for a belief that isn't* evidence *for that belief"*

As discussed in the section 2.2, Davidson makes a number of points against the representationalist model that informs Epistemology. For example, he argues against the claim that the objective detection of sensory data can be used to justify or stand as evidence for beliefs that represent those data. Davidson notes that for the justification process to work, we have to be *aware* of

the detection of sense data, and this awareness is simply another belief. His argument undercuts the objectivist attempt to construe awareness of sensory data as an evidential entity that stands *independent from* our beliefs.

It might seem, however, that in revealing the incoherence of harnessing sensations as independent evidence, Davidson has removed any justificatory scheme for our empirical beliefs. This seems to leave us with the scepticism Davidson's non-representationalist model is supposed to avoid. If explanations appealing to the sensory origins of our beliefs don't justify those beliefs, how do we know that we are not globally mistaken about the world? In this section I introduce Davidson's "radical interpreter" as a heuristic device that provides a "*reason* for supposing most of our beliefs are true that is not a form of *evidence*" (Davidson 1991a, 127).

It is important to make clear that the term "most" in the above quotation is not meant as a quantificational claim guaranteeing, for example, that a certain *number* of our beliefs must be true. Rather, Davidson uses the concept of the radical intepreter to support a *philosophical* claim, namely the claim that the detection of false beliefs *requires* that we have a background of true beliefs against which the error of the false beliefs can be measured. This latter claim undercuts the global sceptic who wants to make error a general concern, i.e. who wants to deny or question the existence of norms against which errors can be measured.

The "radical interpreter"—an adult interpreter faced with a completely foreign language—is an idealised concept Davidson borrows from Quine. Quine introduced the character in his explanation of how we would have to proceed to learn a completely foreign language when no "translation manual" is available (e.g. Quine "Ontological Relativity" 1969). I will argue that if we analyse meaning from the perspective of the radical interpreter, a whole host of traditional Epistemological problems can be set aside.

Davidson equips the radical interpreter with the abilities of a competent adult speaker of a language. Parachuted into the midst of a foreign land, she has general expectations about how to proceed. She has a sense of basic logical structure (i.e., she understands the implications of those elements of a language ["and," "if..then," etc.,] that give the sentences that contain them their particular logical form). She also has the ability to discern when the speakers of the foreign language are making assertions, that is, expressing, in the form of sentences, beliefs held true (even though, in the beginning, she has no idea what those sentences mean).

Davidson notes that, in order to make any progress in her new world, the radical interpreter must watch for correlations between types of sounds uttered by the native speakers and the kinds of events in their shared world that caused the utterances. In the beginning this is all she has to go on. She does not have any preconceived notion of the particular semantic role that is played by any particular noises uttered by the native speakers. Rather,

at this early stage, it is the radical interpreter's successful (accurate) identification of the environmental reference that prompted the native speakers' noises, which provides those noises with semantic content in the first place. For example, the interpreter's understanding of the meaning of the native speaker's utterance "There's the bus!" is provided by the shared causal relationship between the arrival of a bus in the visual (or aural) fields of the interpreter and the native speaker, and the native speaker's utterance.

The foreign noises that express basic or simple beliefs, in sentences such as "There's the bus!" are the starting points for the radical interpreter. These basic beliefs are expressed in what Quine called "occasion sentences" (Quine 1960). Occasion sentences are those sentences the truth values of which change depending on precise, salient variables such as the time and place the sentences are uttered and who utters them. The truth of the sentence "There's the bus!" for example, will depend on the presence of a bus at the time the sentence is uttered. For these "basic" beliefs expressed in occasion sentences, it is possible for the radical intepreter to make an educated guess about the truth conditions of the native utterance, because she has such immediate access to the truth values of her guesses.

Quine contrasts these occasion sentences with "standing sentences," such as "There have been some buses." These latter sentences will be true depending on much more general variables, such as the presence of buses at any number of times prior to the occasion "There have been some buses" is uttered. What makes

occasion sentences, as opposed to standing sentences, the basic entry points for the radical interpreter is not the Epistemic simplicity of the terms involved in the sentences, but the relative ease with which a non-native speaker can guess the truth conditions of the native occasion sentences.[33]

The causal triangular relationship between the interpreter, the native speakers' utterances of occasion sentences, and the objects and events in their world, requires that the interpreter assume the natives are speaking about their beliefs truthfully. While the adult language user has the ability to recognise when a native speaker is making an assertion, this recognition does not guarantee that the native speaker's assertion is true. But, says Davidson, at the beginning, the radical intepreter must *assume* that the native speaker's assertions are true. For interpretation to occur she must assume that the same relation between belief and truth holds for those she interprets, as for herself—what Davidson and Quine have called "the principle of charity." In other words, starting with the most simple utterances such as "There's the bus!" the radical interpreter must assume that she and the native speakers agree about what would make those utterances true (e.g., the presence of a bus).

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[33] Quine goes on to distinguish a subclass of occasion sentences, the observation sentences, in order to make a number of empiricist claims (as discussed in section 2.2). Like some of the more traditional foundationalists such as Hempel (1965), Quine tries, at times, to use the empirical simplicity of occasion sentences as an Epistemological grounding for claims about more complex sentences. As I will argue further in the next section, Davidson's discussion of occasion sentences has no such Epistemological implications.

Why is this agreement necessary at the beginning when the interpreter is collecting sentences in the native language, and correlating them with the sorts of environmental conditions that prompted the sentences? It is necessary, says Davidson, because in order to identify her teachers as having *any* beliefs she must assume the beliefs they hold are true. Once she has established an empirical base of correlations between their sentences and hers, *then* she can start to make judgements of inconsistency and falsehood. Before that point, identifying her teachers' beliefs as false would deplete the empirical base from which she needs to begin her interpretative project in the first place. As one Davidson commentator explains, assigning "too much falsity among beliefs undermines the possibility of identifying beliefs at all" (Malpas 1992, 159). Identifying falsehoods and misconceptions, is "parasitic" on an established coordinate of shared meaning.[34] We are getting closer, then, to explaining Davidson's "anti" sceptical claim about the necessity of having true beliefs for the identification of false beliefs.

It might still be unclear, however, why the existence of a "shared coordinate of meaning" between the native speaker and the radical intepreter, guarantees, in Davidson's words, that "it cannot happen that most of our plainest beliefs about what exists in the world are false" (1991b, 195). Just because there must be *agreement* between the radical interpreter and the native speakers' about the truth of basic beliefs, does not guarantee that those beliefs are, *in fact*, true. Davidson responds by examining

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34 My thanks to Bjørn Ramberg for this characterisation.

the concept of truth itself. Where, he asks, do we come up with the concept of objective truth? The answer is in shared language. "Unless a language is shared there is no way to distinguish between using the language correctly and using it incorrectly; only communication with another can supply an objective check" (Davidson 1991c, 157). And communication with another can only start by assuming agreement on what makes utterances true—the principle of charity.

Davidson's apologists note that the principle of charity is unfortunately named, because it does not operate as advice that we could choose to follow or not (e.g. Ramberg 1989; Malpas 1992). Ramberg emphasises this point:

> The principle of charity,...offers no advice to us as interpreters, it yields no interpretational strategy. It is not a heuristic device, nor is it, accordingly, something we could get by without; it is a *condition of the possibility* of interpretation (Ramberg 1989, 74).

Ramberg notes, too, that "just as we have no choice, if we want to make sense of what others say, but to regard them as on the whole speaking the truth, so we have no choice but to regard *ourselves* as largely speakers of truth" (Ramberg 1988, 643). But, again, applying the principle of charity to our own utterances is not to be seen as a cognitive compliment (however deserved). Ramberg continues: "It has, rather, a somewhat deflationary effect on our self-esteem insofar as it implies that we cannot lie even if we try. Cannot lie very much, that is, just as we cannot be

dramatically and romantically mistaken about how things are" (*ibid*).

Again, this last comment should not be taken as a claim about the truth of any *number* of beliefs. If, within a system of beliefs, or theories, we interpret "dramatically mistaken about our beliefs" as "mistaken about a pretty large number of beliefs" it is quite clear that Aristotelian physics, for example, did get the world *dramatically* wrong. But, note that the sceptic's claim that we could be globally mistaken is not proven by showing that Aristotle's particular theories turned out to be false. Stated positively, all we need to diffuse the coherence of the sceptic's claims, is to express the force of "cannot be dramatically mistaken" in pragmatic, behavioural terms. That Aristotle was in possession of true beliefs is illustrated by the fact that he had some objective understanding of the norms against which he measured error, e.g., he was able to move about in his world with roughly the same success that we move about in ours. There is a background of true beliefs that we continue to *share* with Aristotle. This shared background of beliefs includes all those beliefs which enable us to say that Aristotle's views were *about*,or explanatory *of,* features of our shared world, such as the motion of falling bodies, and that his views are *false* with regard to those features.[35]

If the principle of charity is a precursor for successful interpretation, this means that truth must be held primitive for

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[35] My thanks to Norman Swartz for pressing me about the importance of error. The fact that we can detect error is the primary weapon in the arsenal of those who argue against the coherence of scepticism.

words and sentences to be meaningful. This takes us back to the example of the radical interpreter correlating environmental circumstances with basic native utterances, e.g "There's the bus!" The radical interpreter has no initial preconceptions about how to link a native utterance with specific semantic content. Rather, her attention to the correct (true) reference of the native sentence is what provides her with clues to the meaning of the utterance, in the first place. The meaning of an utterance is given by its truth conditions, and not the reverse.[36]

6.2 *Telling the sceptic to "go away"*

Davidson uses these points about the radical intepreter to support his extensionalist claim that in the simplest cases of beliefs, i.e., those expressed in occasion sentences, the events and objects that cause those beliefs (the *extension* of the beliefs) also determine their contents, or meaning (the *intension* of the beliefs) (Davidson 1989a, 164; 1989b; 1991a; 1991b, 195). This means that in the simplest cases, there cannot be wholesale slippage between our understanding the meaning of a sentence, and our understanding of the conditions that would make that sentence true. Davidson describes this approach to meaning further, in the following passage:

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36 Some logical positivists used this verificationist claim to support a reductive Epistemological agenda. See C.J. Misak (1995, pp 144-151) and Ramberg (unpublished manuscript) to distinguish Davidson's (and Quine's) interpretive points about extension and truth from the verificationism associated with the logical positivists.

> As long as we adhere to the basic intuition that in the simplest cases words and thoughts refer to what causes them, it is clear that it cannot happen that most of our plainest beliefs about what exists in the world are false. The reason is that we do not first form concepts and then discover what they apply to; rather, in the basic cases, the application determines the content of the concept (1991b, 195).

Davidson's extensionalist approach to meaning excludes the possibility that the speech of the radical intepreter could be, in priciple, indistinguishable from her teachers *and* idiosyncratic with respect to meaning. In the simplest cases of beliefs expressed in occasion sentences, the meaning of her utterances is determined by their being used correctly in the presence of another speaker and the event in the world that caused the utterance. Taking a holistic approach to build from the simpler cases of beliefs to beliefs expressed in more complex theories, any idiosyncracies in the radical intepreter's meaning are, in principle, available for her correction through a purely extensional examination of how the she has applied her references. Somewhere along the line, any discrepancies can, in principle, be revealed. There is no "subjective" "inside" to her beliefs that is metaphysically separate and inaccessible from the viewpoint of the native speakers in the "objective" "outer" world.

For example, if the radical interpreter has interpreted "She's candid," in the native language, as "She's rude" in her own language, the difference in meaning between the two sentences could, in principle, be revealed to her. The two words "rude" and

"candid" are linked in a web-like fashion to different, simpler concepts, which in turn have different causes. The two utterances are correctly applied on different occasions, *this* is what gives them different meanings.

Using the model of the radical intepreter, Davidson's causal analysis of belief provides us less-than-radical interpreters, with a *presumption* in favour of the truth of any particular belief. However, a presumption is not a guarantee. He cheerfully admits that the truth of each belief is up for grabs, though not all or even most of these beliefs can be up for grabs at once. It is the veridicality of beliefs *generally*, as understood through his causal account, that makes "meaningful disagreement" over *particular* beliefs possible (Davidson 1984, 196 - 197). Our beliefs have no content unless we have established a common convergence between ourselves, another speaker and a shared environmental stimulus. Occasion sentences provide the entry points for this convergence. Once we have established a pattern of successful convergence, a pattern of semantic "firmness," *then* we can say of any particular belief that it is false. You have to be right about a large background of beliefs before you can critically examine the validity of particular ones. Similarly, successful communication with others, indicates that you know many things about your world (Davidson 1989a; 1990a, section III).

We now have a way to explain how, on Davidson's view, scepticism does not arise as a coherent option that needs addressing. Davidson does not show that global scepticism is wrong, he simply argues that on the model of the radical

intepreter, a metaphysical gap between language users and the world is unthinkable. On the representationalist view, beliefs are conceived as an "inner" non-natural, subjective representation of the outer, natural realm. In contrast, Davidson asks us to try viewing belief as the production of a triangular causal relationship between three naturalised entities, namely, ourselves, other speakers, and our shared environment. From the perspective of the radical intepreter, our ability to use language comes from *direct, unmediated, causal contact* with the world, which, in turn, guarantees that we have an established background of true beliefs against which our false beliefs can be measured. As Davidson writes, "communication begins where causes converge" (1991a, 132). If we want to doubt in a wholesale, global fashion the causal etiology of our beliefs, we must also "give up language" (Ramberg 1989, 97).

While Davidson's views have enjoyed a certain amount of sympathy within mainstream philosophy of language, they have also been met with criticism. I cannot begin to do justice to the details of the critical reception of his work, referring readers instead to Ernest LePore's edited collection devoted to the subject (Lepore 1986). However, in the next section I do set out to defend Davidson's project from certain criticisms that arise when he is construed as an Epistemologist—a defense that is potentially damaging to my claim that Davidson provides a good non-Epistemological option for feminist critics of science.

6.3 Evaluating Davidson's escape from Epistemology

On balance, Davidson's causal account of meaning supports neither objectivist nor relativist understandings of truth. Davidson's theory is not of truth but of meaning. This latter claim is a pragmatist interpretive point supported by Rorty (1991a; 1991b) and Ramberg (1989). Though again, Rorty and Ramberg both point out that Davidson's wholistic focus on interpretive *practice* or pragmatics should not be construed as a pragmatic *theory* of truth. Davidson supports this non-Epistemic reading of his work in his "Afterthoughts" to "A Coherence Theory of Truth and Knowledge" (Davidson 1991a), but, aside from the importance of author's intention, there are a number of arguments which can be amassed to defend this reading as the one that is the most comprehensive and rational. I will begin by arguing against the view that Davidson's work supports objectivism (often referred to in the Davidsonian literature as "realism"; see for example, Evnine's view of Davidson [Evnine 1991, ch. 9]).

First, while Davidson's causal theory of meaning provides us with an explanation how it is that we have a background of true beliefs against which false beliefs are assessed, it does not attempt to function as empirical justification for *particular* beliefs. Rather, he claims, the background of true beliefs is necessary before "meaningful disagreement" over particular beliefs is *possible* (Davidson 1984, 196 - 197). Malpas reinforces this point:

> Davidson claims that, as beliefs are in their nature veridical, all beliefs are justified in this sense. This leaves open, as an empirical question, the issue of whether any particular

> belief, or set of beliefs are justified in some particular context. But what is closed off is the question as to whether all our beliefs might be unjustified and unjustifiable. ... Local error is admissible, so long as global truth is preserved (Malpas 1992, 218).

Unlike the objectivist, Davidson argues that the causal connections between our beliefs and the world cannot be used as independent justifications of our beliefs. Empirical evidence in favour of a particular belief, e.g., the feminist claim that physiologists often generalise about human bodies without studying women's bodies, cannot be used as a belief-independent justification for that claim. We have to *perceive* any causal sensations that make up the empirical evidence in support of our claim, and this perception is *itself* another belief. Justifying any particular belief can only be made be made by appeal to other beliefs. It is *between* beliefs, conceived in wholistic, web-like fashion, that the important justification process proceeds, not between beliefs and some non-beliefs we call "the evidence."

To be sure, we can, and do, use some of our beliefs to provide evidential support for other beliefs, but we can't construe this evidence as an Epistemological "non-belief" entity. Davidson refers to the evidential support provided for one belief by other beliefs, as a "reason" for that one belief (as when he promotes his causal account as a "*reason* for supposing most of our beliefs are true that is not a form of *evidence*"). Davidson's position on the causal relationship between our words and the world is Epistemologically benign, then. It does not support any positive

claims about objectivism or realism. Rather, it functions largely as a negative claim against the relativist's scepticism (Davidson 1990a, section II). As Ramberg explains:

> If we think we understand what people say, we must also regard most of our observations about the world we live in as correct. Davidson does not provide metaphysical [or "Epistemological"] assurance of our connection with reality, he simply makes the point that if we try to give up the world, we must also give up language (Ramberg 1989, 47).

Davidson also argues against the empiricist elements in the theories of van Fraassen and the early Quine. Recall that empiricists highlight the causal role of empirical sensations with respect to "observables" only (or sentences containing only observable terms). Nonobservable terms (or sentences containing them), they claim, do not have this same causal relationship with empirical sensations. For Davidson, however, there is no sense in which empirical data play a causal role in the acquisition of *some* beliefs (say those beliefs expressed in observation sentences) but not other beliefs (those expressed in nonobservation sentences). He makes the wholistic point that empirical data plays a causal role in establishing the content of *all* beliefs. Again, however, his view on the role of empirical, causal relations is to be distinguished from the representationalist understanding of beliefs as *made* true by their causal connections to the empirical world. On Davidson's non-representationalist view of belief, the causal connection plays no Epistemological or justificatory role.

Davidson emphasises that there is an important distinction to be maintained between (a) acknowledging that empirical data is the *only* stuff of knowing (a point which he shares with Quine) and (b) giving an empirical theory of which data is going to be the *right* stuff of knowing (Davidson 1991b). Davidson's particular brand of externalism involves sticking with the acknowledgment that empirical data is the only stuff of knowing. At the same time he denies the coherence of the claim that one can use empiricism to provide an Epistemological theory about which data is the right stuff of knowing (that is a theory that beliefs are true, or maximally objective insofar as they are justified by their relation to empirical evidence). As I noted in 2.2, this distinction is sometimes blurred in Quine's empiricism, even in his more naturalist moments, and it is purposefully blurred in more straightforwardly objectivist accounts.

According to Davidson, "empiricism is the view that the subjective [i.e. individual sense experience] is the foundation of objective empirical knowledge. I am suggesting that empirical knowledge has no epistemological foundation and needs none." (1989a, 166). Davidson shares Dewey's pragmatic annoyance with philosophical theories that view truth as "correspondence between thought and reality inaccessible to experimental research and ordinary practice" (Davidson 1990a, 279).

Another more general difference to note between Davidson's account of meaning and Epistemological accounts of truth (such as correspondence) is that, according to Davidson, our successful language use indicates that we must have some working

knowledge how to apply the predicate "true" *before* we can interpret new utterances. His causal theory of meaning cannot be used to explain how we should *apply* the truth predicate (as correspondence theories claim to do). Malpas explains this point below:

> Most of our beliefs must always be true prior to any attempt to compare beliefs or reality with 'the facts'. The attempt to make such comparison presupposes the identification of the beliefs to be compared, and that already presupposes a background of mostly true belief—a broader horizonal setting—against which the identification can be made. So it is not correspondence with the world or the facts that, in general, makes our beliefs true. It is rather the truth of those beliefs in general that makes correspondence itself possible (Malpas 1992, 242).

In other words, we cannot understand an utterance and *then* test whether it truthfully refers to its object. Truth must have been imported much earlier in the process. This is a point against those who, like Fodor (1987), support causal theories of how our words come to refer to their objects, and suggest that we isolate meaning and *then* test for truth. In the following passage Ramberg describes further the question-begging nature of these causal theories:

> While we might be able to formulate a causal theory of reference without using the concept of truth (or some similar predicate) *testing* such a theory presupposes knowledge of the truth-value of sentences, knowledge which we have come by independently of the theory to be tested... If this is true, a causal theory of reference cannot give rise

> to the sorts of empirical predictions that we want a semantic theory to generate; they are always ad hoc explanations of meaning already known. (Ramberg 1989, 27 - 28, emphasis mine).

Rorty explains further that, although the radical interpreter will eventually end up with a number of correspondence or "satisfaction" relations between the native speakers' words and events in the world, "these links will not be the basis for the translation. Rather they will be fallout from the translation" (Rorty 1991a, 137).

The opposite sort of misunderstanding of Davidson's theory, is the argument that his theory supports relativism, rather than objectivism. According to this view, Davidson's criterion of truth for our beliefs is whether they cohere with our other beliefs, which, the critics point out, leaves the external world disturbingly unaccounted for (see, for example, the criticisms by Williams 1991, 230-232, and much of ch. 7; and by Dalmiya 1990). While the title of Davidson's essay "A Coherence Theory of Truth and Knowledge" (1991a) lends *prima facie* support to this interpretation, the essay contains passages such as the following: "It should be clear that I do not hope to define truth *in terms of* coherence and belief" (Davidson 1991a, 122; emphasis mine). Critics who label Davidson a coherence theorist typically fail to appreciate his claims about the direct causal connections between our simplest empirical beliefs and the world those beliefs are

about.[37] In an earlier version of "The Structure and Content of Truth" Davidson responds to these concerns:

> What we hold true, what we believe, determines what we mean, and thus, indirectly, when our sentences are true. Believing doesn't make it so, but it creates a presumption that it is so. This is not because belief creates a world, as coherence theories and various forms of idealism maintain, ... it is because the contents of beliefs are in centrally important ways determined by the causes of those beliefs (Davidson 1988, 7).

Believing that we do not "create" our world does not, for Davidson, entail a metaphysical independence between our thoughts about the world, screened through conceptual schemes, and the world "as it is" in its "natural" "unrepresented" form. This representationalist view involves the conceptual separation between belief, meaning and truth which is anathema to Davidson's extensionalist approach to meaning, and leads to scepticism.[38] On Davidson's non-representationalist view, our explanatory models, values, political commitments or

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[37] Those of Rorty's critics who claim he is a relativist similarly fail to see his allegiance to Davidson on this point. See, for example, the criticisms of Aune (1972), Fisk (1976), Machan (1993), Veatch (1985, especially pg. 318), McCarthy (1990a) and the exchange that follows (Rorty 1990; McCarthy 1990b), Haack (1993, ch. 9, especially pg. 187) and Rorty's response (1995). For Rorty, relativism involves the two-part representationalist claim that: a) for whatever reason, those representations we call true are not constrained by any relation to the world; that it is something within us (our cultural, historical biases, values, etc) and not the world that eventually makes our representations true; and b) therefore, any one representation is as good as any other. Again, while Rorty and Davidson are critical of representationalism and the accompanying notion of correspondence making our beliefs true, they explicitly argue that our true beliefs are *indeed* constrained by the world (so *a fortiori*, any one belief is *not* as good as any other).

[38] See Davidson (1984) for further criticisms of conceptual schemes.

"worldviews" are best conceived, not as conceptual schemes through which the evidence from the "external" world is filtered; not as underdetermined representations of the evidence; but as further strands in our web of belief. The veridical nature of belief tells against the sceptical views that the world could remain "ultimately unrepresentable" by us (assuming that by "unrepresentable" we mean "unknowable") or that "for all we know" the world could turn out to be outrageously different from how we, in fact, conceive it to be.

In one pragmatic swoop, Davidson shows how Epistemological theories such as objectivism are unnecessary (and, so far, failed) responses to scepticism, while allowing room for meaningful disagreement on the truth or falsity of particular beliefs. In other words, he leaves aside objectivism without taking the "for all we know... so anything goes" position of relativism.

Chapter Seven:
Feminist Science and Science Criticism from a Pragmatist Perspective

It is my hope that by removing the sceptic's motivation for Epistemology, the energy of feminist Epistemologists will be freed up to return to the other important *epistemological* projects that remain in our studies of science. The epistemological task of analysing the connections between our scientific beliefs and the causal connections between beliefs and the world, is a political and socio-cultural project to which feminist contribution is crucial. As Harding counsels, we need to identify all the causes for our various beliefs, in all their, often ignoble, specificity (Harding 1991, 147). We will only run into the difficulties I have documented if we attempt an Epistemological, meta-analysis of what features of certain causal connections make them *true* causal accounts (or "less-partial" accounts, in Harding's case).

We must also be careful that our causal analyis does not *metaphysically* bifurcate "political" reasons for scientific beliefs from "evidential" causes for these beliefs—a bifurcation that parallels the traditional Epistemological split between the "discovery" of a belief and the "justification" of that belief. This advice is particularly relevant for addressing the problems that Longino, Harding and Keller encountered when they prescribed a relativist version of underdetermination theory. They argued that the underdetermination of theory by evidence means that, when evidential support is equivocal, we have to choose between

theories on the basis of our political values. However, this construal presumes the representationalist view that "the evidence" and our feminist "political values" emanate from two metaphysically separate spheres—the first from the objective, external world; the second from the subjective, internal mind (or minds). "The evidence" is construed as providing independent (objective) support for a theory, while political values are viewed as dependent and subjective.

7.1 Davidson on underdetermination theory

In response to this representationalist claim about the belief-independence of empirical evidence, Davidson reminds us that when we marshal empirical evidence in support of a belief or theory, we need first to be *aware* of the empirical evidence, and that *awareness* is itself another belief. In the project of marshalling epistemic justification for our individual beliefs there is no independent, "non-belief" entity to which we can appeal. The evidence for a belief must *itself* be a belief. It is also important to see that *both* our political values and our more straightforwardly empirical commitments are beliefs of this evidential sort. On Davidson's model even our (feminist) political beliefs must have some web-like relation to empirical evidence, if they are to have any content.

There are a number of ways in which feminist political values can interact with and support the more straightforwardly empirical commitments that, together, make up our growing web of beliefs about the oppression of women by science. For example,

recall Longino's particular discussion of the role of political values in choosing between competing archaeological intepretations of chipped stones (see sec. 5.2). One theory, highlighting the role of of the male hunter, interprets the stones as hunting tools. The other, competing theory, highlights or includes the role of the female gatherer and interprets the stones as implements for gathering and preparing edible vegetation. According to Longino, the available evidence supports both theories equally well, so the choice between the male-focused model or the more inclusive, female model must be *relative* to an underlying *political* commitment, namely to androcentrism or feminism, respectively (e.g., Longino 1990, 109). I argue, instead, that feminist political values are *themselves* beliefs with empirical content that can in turn provide *good evidential support* to the "woman-the-gatherer" interpretation over the "man-the-hunter" intepretation. Our choice of the "woman-the-gatherer" model does not need to be construed as relative to the "non-evidence" world of feminist politics.

For example, feminist political analysis of past scientific practises has revealed what is by now a well-documented pattern, namely that theories of human bodies and/or behaviour that ignore *women's* bodies and/or behaviour have proven to be innaccurate. The all-male studies of the effects of stress on "humans," referred to in section 1.2, are illustrative here (Muller 1992). The feminist archaeologist who holds to her "woman-the gatherer" theory, in spite of the equivocal evidence provided by the chipped stones, still has *good inductive evidence*, based on her

feminist political views, to support her decision. The "man-the-hunter" theory leaves out the role of women in the human development of technology and culture. The feminist archaeologist who chooses to interpret the chipped stones on the basis of a theory that includes or even highlights the role of female agrarian behaviour is making her choice based on past evidence that to ignore the role of women is to get the "human" story drastically wrong. Her decision is not "merely relative" to feminist politics, it is not based on some "non-evidence" belief entity brought in when all the objective evidence, "independent of belief," is equivocal. Rather it is a decision well-supported by inductively observed instances of past scientific errors. (I will discuss some of the more pragmatised features of inductive support in section 7.3.)[39]

On my view, then, the "man-the hunter" and the "woman-the gatherer" interpretations are not equally-well supported by the evidence. The latter is *better* supported than the former by feminist analyses of past scientific practise. It is not the case that, faced with interpretations equally well-supported by the "belief-independent" empirical evidence, we are forced to the "inner" belief world of politics to make our choice.

On Davidson's model, our empirical beliefs have no better metaphysical links, than do our political beliefs, to the outer,

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39 I was heartened to read Richmond Campbell's essay on feminist empiricism that makes the very same point here (Campbell 1994). He argues that feminist motivation can *contribute* to objective evidence gathering. Objectivity, in the sense of rationally assessing the evidence, should not be equated with neutrality (1994, 100). However, his arguments about the importance of evidence reveal an empiricist motivation to defeat scepticism that keeps me from wholeheartedly supporting his project.

independent objective world, just as our political beliefs are no more closely related, than our more straightforwardly empirical beliefs, to our inner subjective world. But this is because, on Davidson's view, there *is no* inner or outer world, there is no metaphysical bifurcation. There is only one world, an objective view of which can be made meaningful only by the language users who are part of it.

While it is certainly possible that some of the political beliefs that make up our belief "webs" might be more *geographically* remote from the empirical beliefs at the edge of our webs, the wholism of Davidson's model indicates that the political beliefs are still connected, by some threads, to those empirical beliefs. When we examine meaning on the model of the radical intepreter, we see that changes in empirical beliefs can, and must, in principle, affect more theoretical beliefs, even if the effect is only slight. For the radical interpreter, no two theoretical beliefs can both conflict with each other in drastic ways *and* have the same truth conditions.

Of course, even though Longino might not have found one, there still might be cases where we want to say that, from the point of view of us non-radical interpreters, two conflicting theories are equally well-supported by the empirical evidence. Here, if we are careful to construe *both* the "empirical" *and* "political" evidence in support of each theory as *themselves* beliefs, we might say that both types of belief can be epistemically underdetermined by their causal relationship with the external world. But, in principle, the radical intepreter *must*

be able to identify the precise causal history of any individual belief, even if we, less-than-radical-interpeters cannot.

In the sceptic's world, the fear is that the metaphysical separation between us and the world makes coherent the worry that we are, *in principle*, unable to speak with confidence about the causal links between our representations and the world represented. Davidson's point is not to offer comfort to the sceptic that her representations are indeed accurate, but to rethink the "beliefs as representations" model itself. He uses the radical intepreter to give life to an alternate view of the relationship between language users and the world, whereby all we have (and all we need) is an interconnected web of empirical and theoretical/political beliefs, where for any one attribution of error, that potentially false belief must be connected sufficiently firmly to a sufficiently rich background of true beliefs before we can even identify that belief as being false *about* some feature of the world.

7.2 Feminist science without Epistemology

I have argued that feminist science criticism is the most successful when it is kept internal to specific science projects, and because these projects are always in flux, the prescriptions are best kept *ad hoc* and dynamic. The criteria for what constitutes the proper prescription will always be undergoing adjustment as new information comes in, for us, as for the radical interpreter. This is what separates a pragmatist interpretive project from Epistemological theories of truth that attempt to provide *a priori*,

general accounts of truth-making. In the next passage, Ramberg further examines the pragmatist's brand of epistemology:

> Judging the truth-value of sentences is a matter of human inquiry running the gamut from idle speculation... to rigorous, explicitly systematic cognitive procedures. Epistemology, in so far as it is simply such inquiry directed towards itself, is just part of our normative theorizing about how to justify our beliefs; how to make our inquiries as efficient as possible in establishing the truth-value of sentences. Here is where, for instance, theories about the possibility of verification and falsification, ... find their place, but such theories are not the only, perhaps not even the primary, kind of theory that is epistemological in this broad and innocuous sense (Ramberg 1989, 9).

Returning to the distinction with which I began this essay, between the innocuous epistemological question *"Is this theory, or sentence, true?"* and the troublesome Epistemological questions *"What property do all true theories share?"* and *"How do we know if we've found that property?"* Ramberg explains that we will encounter problems only when we think that answers to this latter set of questions will provide answers to the former question (*ibid*). For, as Rorty reminds us, no matter what truth-making property (coherence or correspondence, for example) we identify in a particular theory, we still fallibilistically acknowledge that future evidence might show the theory to be false (Rorty 1995, 149). When feminist Epistemologists, for example, identify the truth-making property as "coheres with evidence and feminist political goals" and then make use of Harding's method of "strong objectivity" to ascertain that a particular feminist science claim

actually *has* the property, this process does not protect that claim from future falsification. But this is precisely the goal that Epistemologists try for and fail to reach. When feminist Epistemologists link the mundane question of the truth of a particular claim ("the Darwinian variability hypothesis is oppressive to women") to Epistemological truth-making and truth-identifying properties, the fact that the claim might *have* those truth properties and *still* turn out to be false introduces a devastating, and unneccesary scepticism to the equation.

If we abandon these meta-searches for theories of truth and knowledge, and the scepticism they engender, we are left, instead, with our every-day epistemological practices of testing our claims against our past experiences and our ongoing bodies of theories. The claim that the Darwinian variability hypothesis is oppressive to women is well-supported by these practises. Of course, this epistemological study remains a fallibilistic project—we might come up with new evidence that falsifies our claims. But scientists and others in the business of producing knowledge claims have never had any more assurance than that provided by the best evidential support available at the time.

Arthur Fine provides a similar reminder of the limitations of Epistemology for science, arguing that Epistemologists "see science as a set of practices in need of an interpretation, and they see themselves as providing just the right interpretation" (Fine 1989, 100; see also Fine 1984, 1991). However, Fine continues, "science is not needy in this way. Its history and current practice constitute a rich and meaningful setting. In that setting questions

of goals or aims or purposes occur spontaneously and *locally*" (1989, 100).[40] Fine's epistemic description is very similar to Davidson's own take on science and truth. Fine continues:

> Truth cannot be "explained" or "given an account of" without circularity. Nor does it require anything of the sort. The concept of truth is open-ended, growing with the growth of science. Particular questions (Is this true? What reason do we have to believe in the truth of that?..) are addressed in well-known ways. The significance of the answers to those questions is rooted in the practices and logic of truth-judging ...but that significance branches out beyond current practice along with the growing concept of truth... There is no saying, in advance, how this will go (Fine 1989, 101).

Fine's point is related to my concern that the feminist move from scientist to Epistemologist is typically accompanied by a shift in focus toward "science" and/or "method" and away from the specific agents responsible for harming women. This feminist approach erroneously presumes that "science" names a monolithic, homogenous institution. Prescribing a local, *ad hoc* approach to the study of specific scientific projects, as is suggested by Davidson and Fine, helps address this group of concerns.[41]

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[40] Fine relies on the view that scientists are not "naturally" inclined to take Epistemological positions such as objectivism or relativism (realism or antirealism) (see also Fine 1991). This has brought him a lot of criticism from philosophers such as Ernan McMullin (1991) and Richard Schlagel (1991) though I haven't seen any from scientists themselves (scientists are probably just not reading the philosophy journals in which Fine speaks on their behalf). I do not commit myself to the view that all scientists are pure from Epistemological leanings.

[41] See John Dupré (1990) for similar points on the pluralism of the sciences.

While I believe that feminist science critics should leave Epistemology and return to specific science projects, I do not mean to encourage the view that scientists, feminist or otherwise, are Epistemologically special, that they have a privileged access to truth-making and truth-identifying properties. I recommend instead, Rorty's view of scientific inquiry, expressed in essays such as "Is Natural Science a Natural Kind?" (Rorty 1991d) and "Science as Solidarity (Rorty 1991c). Here, Rorty identifies the uniqueness of scientific practices not in any new method for establishing the truth of particular scientific claims, but rather in the democratic, moral tenor of (ideal) scientific investigations (Rorty 1991c, 39). His pragmatic view of scientific investigation as a set of democratic values still allows him to speak of the truth of various scientific claims, but, he writes, to say that "truth will win" in the (ideally) open, democratic scientific encounter "is not to make a metaphysical claim about the connection between human reason and the nature of things. It is merely to say that the best way to find out what to believe is to listen to as many suggestions and arguments as you can" (1991c, 39). Again, this is set against his belief that as language users we have and are constrained by direct causal contact with our world, we can't and don't just "make it all up."

Rorty's critique of the traditional representationalist view of scientific method complements much feminist science criticism, insofar as his critique deconstructs the "scientist-as-priest" myth. Rorty describes this as the myth that the world speaks God's language and that scientists have a special method for tuning in to

God's voice. For example, Rorty is critical of basing the science/arts split on something like "rational versus irrational," "hard versus soft," or "objective versus subjective" (Rorty 1991c, 36). Returning to his view of scientific investigation as (ideally) exemplifying democratic virtues, he explains how these virtues can and should be evident in *all* disciplines. "On this construction, to be rational is simply to discuss any topic—religious, literary, or scientific—in a way which eschews dogmatism, defensiveness, and righteous indignation" (Rorty 1991c, 37).

7.3 A Pragmatist view of sex/gender categories

Returning to my criticisms of feminist Epistemology, not only was I concerned that feminists often target a monolithic version of "science," I also expressed concern with the over-generalisations of sex/gender categories that typically accompany feminist Epistemological projects. When feminist theorists choose these over-generalisations over more complex alternatives (and when these choices are accompanied by other Epistemological symptoms) I argued that the choice is evidence of the ontological rigidity often required by representationalist accounts. What if, instead, we applied a pragmatic, non-representationalist approach to the ontological status of our categories of analysis?

Goodman's analysis of the confirmation of scientific hypotheses provides just the sort of alternative we need. Although there have been no satisfactory Epistemological responses to Goodman's "grue" riddle, Goodman himself came up with a reasonable pragmatic solution. According to Goodman, we

decide that a theory or hypothesis is confirmed by its positive instances if the theory contains categories or predicates that our linguistic *practises* have allowed us to habitually "project" (Goodman 1955). Projectible predicates, or hypotheses containing those predicates are those that have been "entrenched," through practise or habit, in language use. To use his famous example, the hypothesis "All emeralds are green" is confirmed by instances of green emeralds, whereas "All emeralds are grue" is not similarly confirmed. "Grue" is not a projectible predicate, by Goodman's linguistic criteria, while "green" is. "Grue" has not had a history of use in inductive generalisations about emeralds. "Green" has.

Paralleling Davidson's wholism, Goodman claims that the legitimacy of any new predicates "has to be decided on the basis of their relationship to older predicates" (1955, 98). Keeping in mind Davidson's causal account of meaning, we should not equate the fact that projectible predicates are those that have been entrenched in our lingusitic practices with the view that "we create our world." Our language use tells us that our predicates or categories arise from firm causal links with our world, though these causal links cannot be used to objectively verify the criteria by which we identify any *particular* category.[42] As one Goodman commentator explains further:

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42 Wittgenstein makes similar points about identifying colour categories in his *Philosophical Investigations* (e.g., 1958 IIxi, pp 226-227). See also Rheinwald (1993, p. 73) for further discussion of the relationship between beliefs about the causal structure of the world and the entrenchment of certain predicates.

> Entrenchment is a lingustic fact, but it's not just a linguistic fact. Entrenchment is neither accidental nor the result of an arbitrary decision. Whether a term is entrenched, or rather can become entrenched, depends essentially on the way the world is... (Rheinwald 1993, 69).

Neurath's boat metaphor is helpful for expanding on this point. Imagine that we are floating in a boat at sea, but we occasionally discover that the hull of our boat is not water-tight. The boat represents our theories about the world, with each plank in the hull representing the different categories or predicates we have projected in our theories. The difference between a "contrite fallibilism" about the sea-worthiness of our boat, and a complete or global scepticism, is that on Neurath's model *our boat is floating*, so most of the planks must be water-tight; most of the categories that we habitually project must successfully refer. And even if we can't immediately specify which planks are leaky, or which will *become* leaky, we can still repair the leaks as we discover them (see, for example, Goodman pp. 98 - 99).

So, then, how, do the categories "women" and "men" fare in our hypotheses and theories? I think that for the most part they fare just as well as do "green" and "blue," but let's examine the case of "green" and "blue" more closely.

Serendipitously, while looking for Elizabeth Woolley's review article on the "Psychology of Sex" (Woolley 1910), I came across a neighboring article in the same volume entitled "The Puzzle of Color Vocabularies" (Woodworth 1910). Here I was delighted to find a discussion of the absence of the colour names "green" and

"blue" amongst *the majority* of human languages, especially ancient languages. Anthropologist Geiger wrote of discovering this phenomenon in the *Bible* and also in the *Vedas*: Of the latter he writes:

> These hymns, consisting of more than 10,000 lines, are nearly all filled with descriptions of the sky. Scarcely any subject is more frequently mentioned; the variety of hues which the sun and dawn display in it, day and night, clouds and lightnings, the atmosphere and the ether, all these are with inexhaustible abundance exhibited to us again and again in all their magnificence; only the fact that the sky is blue could never have been gathered from these poems by any one who did not already know it himself (Geiger [1880] cited in Woodworth 1910, 325).

Geiger provides similar examples regarding "green." Woodworth was careful to point out that, when tested, most adult humans, even those speaking languages that had no names for "blue," for example, can *distinguish* between green, blue, and most other colours on the spectrum, so citing these observations is not meant to support linguistic determinism. Words do not "create our world," for either Woodworth, Goodman, or Davidson. Woodworth hypothesised, instead, that the people speaking these languages simply had no use for the colour names "green" and "blue" (Woodworth, pp. 333-334).[43]

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[43] Woodworth explained his views about the low functional utility of the colour names "green" and "blue" by first addressing the question of the functional utility of colour names generally. He hypothesised that colour categories are introduced to help identify objects and to distinguish them from their backgrounds. But, he argued, other sorts of words often serve the same function. He wrote, for example, that "We speak of a berry as ripe, rather than red, of meat as well done or underdone rather than brown

As I noted in 4.5, there is a growing literature by and about transsexuals who find the categories "women" and "men" similarly useless, searching, perhaps, for some new middle category (e.g., Rothblatt 1995; Herdt 1994; and Bornstein 1994). If we take a pragmatic Goodmanesque approach to our categories of analysis, whether they be categories of colour or sex, we are reminded that the categories can be refined, we can rethink their range of application, and over time, when necessary, we can discard them. A Davidson/Goodman account of the projectibility of our predicates allows for an openness in our hypothesis testing that is beneficial for any rational investigation, whether it is science or feminist science-criticism.

But what about the concern that feminism *requires* some essential difference between the categories "women" and "men," a difference that is endangered by the claims of women of colour, or transsexuals who claim that sex categories fail to capture their own sense of themselves and their oppression? Denise Riley addresses this sort of feminist concern in her book *"Am I That Name?" Feminism and the Category of "Women" in History* (1988). She writes compellingly of the history of Western feminism as a group of movements constantly struggling to both deconstruct and reconstruct the category "woman" depending on the political

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or red. In such cases the color is the mark by which the condition of the thing is known, but what is named is the condition rather than the mark" (1910, 333). There is a lack of function, then, for many colour names, which, he believes "accounts for the widespread poverty of languages in such names" (*ibid*). With respect to "green" and "blue," in, particular, he notes that they are themselves primarily background colours. "Red" and "yellow" are the colours of objects that need to be distinguished from this background, and so are bound to be more functional than "blue" and "green" (*ibid*).

needs of the time (Riley 1988). She writes that "woman" (and even more so, "women") is an "unstable" category; "that this instability has a historical foundation, and that feminism is the site of the systematic fighting-out of that instability" (Riley 1988, 5). I agree with Riley when she writes that the "systematic fighting out" doesn't have to be seen as a problem for feminism, rather this is what feminism is all about (*ibid*).

Similarly, Chandra Talpade Mohanty is critical of the view that the categories "men" and "women" are naturally given and require only that feminists then analyse the role of "women" and "men" in "the third world" or "the workforce" or "the family" (Mohanty 1991) She suggests that we also need to see that the categories "men" and "women" are constructed *within* each of these settings.

> The problem... is [one of assuming] that men and women are already constituted as sexual-political subjects *prior to* their entry into the arena of social relations. Only if we subscribe to this assumption is it possible to undertake analysis which looks at the "effects" of kinship structures, colonialism, organisation of labor, etc., on women, who are identified, *in advance* as a group. The crucial point that is forgotten is that women are produced through these very relations as well as being implicated in forming these relations (Mohanty 1991, 59; emphasis mine).

Within feminist science criticism, the more local and specific the targets, the more flexible and dynamic our approach, the less likely we are to need ontological rigidity in our categories of analysis. This is just one more reason why feminist critics of

science should spend more time with specific, and necessarily messy studies of particular scientific theories and practices. As Mohanty counsels, her arguments are not "*against* generalizations as much as they are *for* careful, historically specific generalizations responsive to complex realities" (Mohanty 1991, 69; emphasis mine).

7.4 Summary:

"Right conduct in belief" to borrow an old pragmatist phrase, has been achieved in the sciences and in feminist science criticism, not through adherence to *a priori* Epistemological accounts of truth, but, however fallibly, through adherence to *ad hoc*, site-specific rules, such as these time-honoured suggestions for laboratory experiments:

> Always repeat an experiment a few times.
> Have a few other folks try to repeat your experiment.

To this list can be added some rules suggested by Longino:

> Create recognised avenues for the criticism of evidence.
> Employ shared standards that critics can invoke.
> Always be responsive to criticism.
> Share intellectual authority equally among qualified practitioners (adapted from Longino 1990, 76).

Unfortunately the persuasiveness of Longino's rules are weakened when she motivates her prescription with a representationalist understanding of "evidence" and "objectivity." Longino suggests these rules because, she argues, they allow for the most

"objective" critical exploration of the "contextual values" at work filtering the scientific evidence. According to Longino, once we see that even our "objective" theories and models are underdetermined by the evidence, critical exploration of contextual values and their influence on "explanatory models" is all we can then make of the concept of objectivity. She comes to prescribe certain scientific theories not necessarily because of their relationship to the evidence, but because they have been filtered through contextual values that best suit feminist politics. As I noted, however, this more relativist analysis weakens the role of evidence and so invites scepticism.

A non-representationalist understanding of contextual values or worldviews would conceive of them, not as filters between our beliefs and some non-belief form of "evidence," but as further important strands in our web of belief. When we justify particularly crucial elements of our feminist worldviews, such as our beliefs about oppression and justice, our appeals to the evidence have been well-documented and are powerfully persuasive as a result. There is no need for us to doubt the evidence for our feminist political values, as long as we conceive of the evidence as that which is provided by other beliefs in our web.

Of course there are ways to use the representational language of "aboutness" without being committed to the representationalist metaphor of beliefs being about some non-belief form of evidence. Rorty gives examples of rings on tree trunks "representing" the age of the tree. But, he explains, in these cases "we use the term

'about' as a way of directing attention to the beliefs which are relevant to the justification of other beliefs, not as a way of directing attention to non-beliefs" (Rorty 1991e, 97). This inter-belief comparison is where all justification happens. In this way, we can make stronger claims than those allowed by Longino (or Harding or Keller). Our scientific theories and our beliefs about oppression and justice are not merely relative to our feminist conceptual schemes, they are *justified* by the evidence and they are *true*.

Chapter Eight:

A Pragmatist Case Study—Back to the Theory of Evolution

In this dissertation I have collected a number of examples of the problems encountered by feminist critics of science when they employ a representationalist model of minds and language. This model forces a questionable metaphysical split between our subjective, political beliefs and the objective, external world of the "evidence," which, in turn, makes global scepticism about our political beliefs a coherent concern. I then offered a pragmatised reading of Davidson's philosophy of language as an alternative that avoids the questionable metaphysics. From Davidson's nonrepresentationalist model scepticism is not a coherent option.

If we are persuaded by this pragmatised Davidsonian model, we can abandon the (never-ending) Epistemological battle with scepticism, and return to our feminist criticisms of science as chemists, biologists, psychologists, and sociologists, etc. In these specific science forums we can continue the project at which feminist theorists have always excelled—that is, the construction of genealogical, pragmatic accounts to diagnosis the oppressive nature of terms, theories or models used in science. And, importantly, we can continue to question how the documenting of an oppressive genealogy bears on the issue of the *truth* of the scientific claim under consideration.

The relationship between what has traditionally been called the "discovery" of a claim (its genealogy; the pragmatic constraints that led to its introduction), and the "justification" of that claim

(its truth; its relationship to "the facts") is by no means straightforward. However, by employing a Davidsonian understanding of meaning, belief and truth, we can collapse the distinction by viewing *both* discovery and justification as naturalised elements of the equation, that are, both, in principle, constrained by evidence. By staying within the traditional, representationalist framework Longino and Harding, in particular, impose a problematic metaphysical split between discovery and justification, as they argue that subjective political values from the discovery context screen or filter evidential testing in the justification context (see, for ex., Longino 1987, 54; Harding 1991, 143-144). When Longino argues that the the non-evidentially constrained realm of politics is always bound to influence the underdetermined, evidential realm of justification, this attempt to show a relationship between the two is mitigated by her conception of each realm as metaphysically distinct. She reifies, at a more general level, the very split between the subjective/political realm of discovery and the objective/evidence realm of justification that she seeks to deconstruct. Reifying this traditional split only damages our own, well-justified, *and* politically-motivated criticisms in and of science. The non-representationalist alternative removes this danger.

However, while a non-representationalist view supports the claim that both the "discovery" and "justification" of a theory are constrained by evidence, it does not support claims of an objectivist "logic" or Epistemological method at either level. If we are no longer motivated to search for the property that all true

theories share, we should likewise abandon our concerns about which method indicates the *presence* of a truth-making property. Searching for evidential relations to guide our scientific examinations, genealogical or otherwise, is largely an *ad hoc* task that proceeds in well-known ways ("have someone else check your results," "use double-blind research designs where possible"). If we attempt to "precipitate out" the features of these well-known methods that make them *objective* methods for identifying *truth*, we will be continually chasing after recalcitrant features. Recall the convoluted changes Keller was forced to make to her account of "dynamic objectivity" in the face of the recalcitrance of McClintock's gender (see section. 4.4).

To further illustrate my non-representationalist prescription for the construction of pragmatic, feminist genealogies in science, I will now return to the evolutionary topics with which I began my dissertation. I will first discuss a pragmatist evolutionary account of biological function, and then apply this account, first to the function of the allegedly superior (European) male physique and intellect; and second, to the function of menstruation. In both cases I will highlight a number of entry points for feminist genealogical criticism of these accounts.

8.1 A pragmatist view of biological function

Earlier, in section 3.1, I favourably discussed Hollingworth's epistemological criticisms of Darwinian functionalist claims about sex differences in intelligence (e.g., Montague & Hollingworth 1914; Hollingworth 1914). Recall that Darwin argued that human

males, especially European males, had superior mental and physical secondary sexual characteristics, and this superiority had a biological function because it helped the males to attract and defend female mates (Darwin 1981 [1871], vol. 2 327-328). Darwin hypothesised that the selection of superior characteristics in males was aided by the fact that males varied more from the "generic human form" than do males of the "lower races," and adult females of any race, on any number of physical and mental measures (Darwin, vol. 1, 275). In turn, he hypothesised that the greater physical and mental variability in males resulted from the fact that the females in our prehistoric past were more choosey about mates than were the males. The highly variable male, especially the "civilised" European male, was viewed as the "engine of evolution" (see Darwin [1981, vol. 2, pp. 358-367] for an explanation of how female mate-choice was frustrated pre-historically in certain "lower races," thereby decreasing the effects of male variability, and subsequently, male superority, in those races).

Hollingworth did not provide any genealogical accounts of Darwinian functionalist claims, choosing to attack the truth of the claims more directly. She chose a research design that would empirically evaluate the claim that males are more *inherently* physically varied as a group than are females; a claim that must be supported by those who believe that superiority (and inferiority!) is found in males, while mediocrity is the rule for females. She and Montague made cranial measures of a large, diverse population of infants (assuming that measurements at

infancy would best reflect inherent, biological causes, rather than social or cultural pressures). They found no significant sex differences in cranial variability; the Darwinian claim was not supported (Montague and Hollingworth 1914). Hollingworth also criticised the claim that greater male variability in *mental* traits (if it is found) is inherent, arguing instead for the value of sociological explanations (Hollingworth 1914).

I would like to expand the target of this epistemological critique to extend not only to the details of the male superiority thesis itself, but also to the larger, philosophical debates about biological functions more generally. Feminists have not yet made significant critical inroads in the mainstream philosophical debates about biological function. The players in the debate fall into two main groups that I will label "etiologists" (who represent the objectivist position from the Epistemological debates) and "pragmatists." The views of the pragmatists in this debate are particularly ripe for feminist appropriation.

Beginning with the views of the etiologists, their main claim is that *nature* provides us with the distinction between functions and nonfunctions in any given organism; and evolutionary theory, objectively applied, is the best way to discover the distinction. The function of the heart, a favourite example in the etiologist literature, is to pump blood, because an evolutionary account reveals that the heart was *selected* to pump blood. The heart was not selected to produce heartbeats, so noise production is a nonfunction, or an "effect." The current version of this etiological position is taken by philosophers such as Larry Wright (1972,

1973), Ruth Millikan (1989), Karen Neander (1991) and Peter Godfrey-Smith (1994). According to this objectivist, etiological approach, a mechanism is functional insofar as it was selected to perform that function in the past. The term "etiological" refers to the historical course of the functional mechanism's evolution by natural selection.[44]

The etiological account of functions is criticised by philosophers such as Robert Cummins (1989), Christopher Boorse (1976), and Elizabeth Prior (1985) who take a more pragmatic approach. These philosophers also focus on the evolutionary course of a functional mechanism, but they do not believe that we arrive at this course by "reading off" the evolutionary facts of a metaphysically separate, objective environment. For example, they agree with the etiologists that the heart functions to pump blood, but they disagree that a natural selection account provides objectivist criteria for this view. They argue that when we categorise certain aspects of our environment as "functions," and use natural selection to account for those functions, our interests in some biological systems, rather than others, provides an influential context for the functional study.

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[44] I have characterised the position of the etiologists in Epistemological terms, believing that most of these philosophers, and some of the scientists who take this position, are committed to some sort of objectivist Epistemology. However, there is much debate over whether Darwin himself was an objectivist, as I've defined the position, especially concerning his views on the "naturalness" of species categories (e.g., Mayr 1994 and Sober 1994), and the objectivity of inductive "fact gathering" (e.g., Hull 1973). On the flip side, the pragmatist rivals in the functional debate do not necessarily conform to the non-representationalism I prescribe—indeed their anti-objectivist critiques often make them sound like Epistemologists of a relativist stripe(!). I will augment (and improve) their otherwise useful anti-objectivist views with non-representationalist insights where necessary.

They note, for example, that our interest in the cirulatory system influences our views about the functions of hearts. We have no such interest in a "noise-making system" so heartbeats are simply "effects." Pragmatists view functions as "those effects of the components of [a] system reference to which provides us with our best account of some high-level capacity of that system" (Prior 1985, 311), and, they argue, our interests influence, to some extent, which systems we examine.

This is not to say that human interests are a subjective filtering device through which the facts of natural selection pass with more or less objectivity. Recall instead, the origins of objectivity, as explained through the radical interpreter. According to Davidson, an "objective" delineation of a category is the product of two or more speakers noticing, that is, taking some interest in, the same features of their environment and grouping those features into the same linguistic category (such as the categories "functions" and "mere effects"). On Davidson's model, human interests are naturalised and made part of the scientific equation. Far from forming some metaphysically separate, subjective viewpoint that filters the external, objective facts about functions, human interests are part and parcel of the objective categorisation of functions..

We can also add Goodman to the equation, using "entrenchment" where Prior, for example, writes of human "interest." When attributing functional status to elements of a physiological system, both the system, and the capacities of that system, can be viewed as predicates that need to be well-

entrenched in the appropriate linguistic sphere before functional hypotheses containing those predicates are themselves projectible.[45]

Within the biological study of the well-entrenched circulatory system, for example, we have a number of components such as the heart, the veins, and the arteries, all of which produce a number of different effects. Once a *capacity* of that system has been similarly entrenched, such as the capacity "circulates nutrients/disposes of wastes," those effects of the components that contribute to our *explanations* of that capacity will be identified as functions, the rest (such as production of heartbeats) as mere effects. In other words, the entrenchment of the capacity "circulates nutrients/disposes of wastes" helps to decide which of any competing functional hypotheses about the components of the circulatory system are confirmed by observations of the operation of the system. For example, the high frequency of the use of the capacity predicate "circulates nutrients" (or a co-extensive term like "pumps blood") will positively influence the projectibility of the functional hypothesis "the heart functions to pump blood."[46] The relatively low frequency of the use of the capacity predicate "produces heartbeats" will negatively influence the projectibility of the hypothesis "the heart functions to produce heartbeats."

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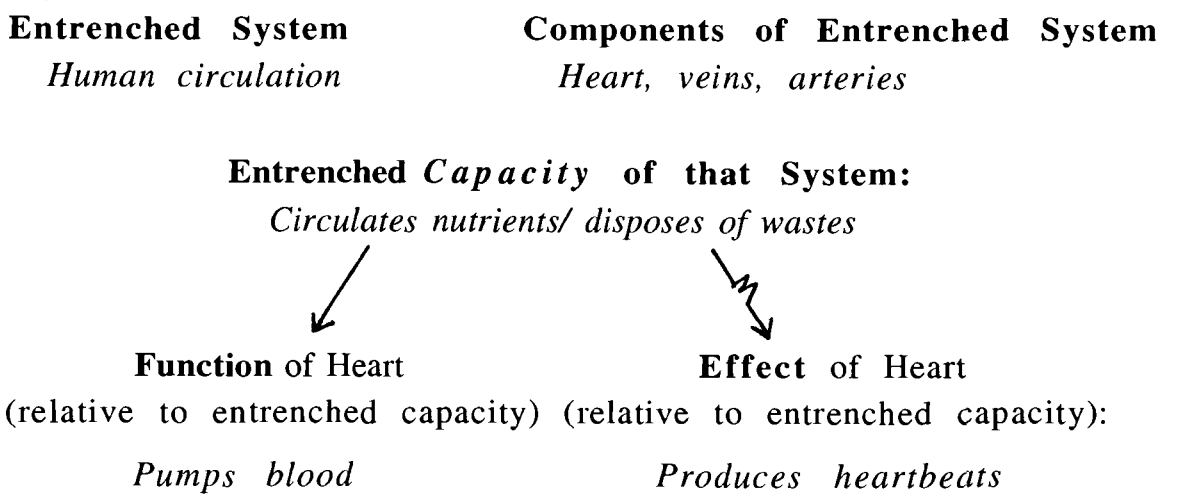
45 Goodman's discussion of predicates similarly extends from simple colour categories like "blue" to more complex capacity categories like "conducts electricity" and "is radioactive" (Goodan 1955, 97).

46 Goodman explains that the co-extensions of a predicate are equally projectible, even if they are not as familiar to us. Projectibility cannot be reduced to mere familiarity—unfamiliar predicates might become projectible (1955, 95-96).

Observations of the working human heart will tend to confirm the first hypothesis, but not the second (see Figure 1).

Figure 1:



At the moment, then, biologists are inclined to say that the hypothesis about pumping blood is true, while the hypothesis about producing heartbeats is false. Articulating the relationship between projectibility and truth is tricky, but it seems clear that projectibility is needed before an hypothesis can become *confirmed* by its positive instances. The obvious representationalist worry that arises from this formulation is that if we define a true hypothesis as that which is well-confirmed, and we believe that confirmation depends on entrenchment in our previous linguistic usage, rather than "the world," then we could be globally mistaken about what is true.

There are a number of ways to respond to this representationalist worry. First, the non-representationalist view of the radical interpreter illustrates that false beliefs, about functional hypotheses, for example, only have content in relation

to a background of true beliefs against which the false beliefs are compared. Global error is incoherent on this model. Beliefs are not potentially erroneous representations about non-belief entities.[47] But of course, individual beliefs, about functions, for example, can indeed be false. Second, Goodman notes that it is only when one *defines* truth, in some Epistemological sense, as "those hypotheses well-confirmed by observations of positive instances" that scepticism about the relationship between "projectibility" and "truth" becomes a problem (Goodman 1955, 99). Goodman, Rorty and Davidson support, instead, the "cautionary" use of true, found, for example, in the the phrase: "this hypothesis is 'well-confirmed' but it might not turn out to be 'true'" (see, for example, Rorty 1995, 149). Problems arise if, in an attempt to defeat some *future* sceptic, we define projectible hypotheses as those that will *turn out* to be true (Goodman 1955, 99). If, instead, we are fallibilistic about our hypotheses, we can acknowledge that "produces heartbeats" *may* become entrenched within biology, and "the heart functions to produce heartbeats" might turn out to be true. Even though the pragmatic model of

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47 Scheffler proposes a different response to concerns about the relationship between human interests, categories, and hypotheses about those categories (Scheffler 1967, pp. 37 - 39). He acknowledges that human (subjective) interests might influence what categories we choose to study, but, he claims, we can still be objective about the testing of our hypotheses *about* those categories. This makes sense to me, as long as we do not construe the objective test of those categories as a comparison between a non-belief entity (the evidence) and a belief entity (our hypothesis about the category).

the radical intepreter makes a *general* scepticism incoherent, we still need to be fallibilists about our *particular* truth claims.

8.2 The function of "(European) male superiority"

Returning to Hollingworth's epistemological critique of variability, we can make use of this pragmatic, Goodmanesque approach to construct a genealogical account of the function of "(European) male superiority." Darwin hypothesised that intellectual and physical superiority helped the "civilised" male to attract mates, a claim supported recently by Kimura (1992). The more pragmatic-minded philosophers such as Prior, might suggest that an examination of the capacities or features of the system to which the component "(European) male superiority" contributes might reveal to us the reasons for the tenacity of such a sexist claim.

"(European) male superiority" is a component of the well-entrenched system of sexual selection. One of the well-entrenched capacities or features of this system is the phenomenon of "female mate-choice" (see Figure 2).

Figure 2:

Entrenched System
Sexual selection

Component of Entrenched System
(European) male superiority

Entrenched *Capacity* of that System
Female mate-choice

↓

Function of (European) male superiority
(relative to entrenched capacity)

Attracts mates

Here it becomes clear that "attracting mates" contributes to our explanations about "female-mate choice." But why, we might ask, is female mate-choice a projectible, that is, entrenched feature of sexual selection? Here is one site where feminists can contribute a crucial genealogical examination.

In our androcentric society, it might seem odd that "female-mate choice" is well-entrenched at all, because the predicate suggests that, at some earlier point in the evolutionary record, human females, for example, exerted some power (of choice) over their male partners. One explanation might be found in an examination of the concept of the "over-sexed" "eager" male. This characterisation of the male is crucial for female mate-choice to be successful and provides a useful genealogical entry point for feminists. Generalisations about the female lack of desire and male eagerness continue from Victorian times to the present, and they can obviously be interpreted in numerous ways. Surprisingly, Havelock Ellis provided a prescient feminist acount of this phenomenon when he explained that a woman's lack of desire may, in part, be due to the fact that her partner has not understood her sexual needs (Ellis 1936 [1897-1928], vol. 3).

Haraway has made some more recent inroads on this question when she argues that "female mate-choice" paradoxically requires "female passivity" (Haraway 1989, 364). Female passivity is, of course, as well-entrenched a predicate as one can find in androcentric evolutionary biology. Haraway explains that Darwin viewed our prehistoric male ancestors as competitors in access "to the means of reproduction," i.e., females. This means that the

female becomes the "limiting resource" (*ibid*). Haraway notes that, unfortunately, the limiting resource "always runs the risk of being nothing but the prize, not a player (agent) in its (her) own right" (*ibid*). The choosey female is reduced to a passive object of desire.

The relationship between hyper male sexuality, female passivity and female mate-choice might help explain why female mate-choice is as entrenched as it is in androcentric biology. It might also help explain why "attracts mates" is the accepted function of one component of sexual selection, *viz* European male "superiority." "Attracts mates" contributes to our explanations of the capacity "female mate-choice." In the next section I examine these pragmatic, genealogical analyses of function in more depth, utilising a recent evolutionary theory of the function of menstruation.

8.3 The function of menstruation

In 1993, biologist Margie Profet captured the attention of the popular press with the publication of her "radical" thesis: menstruation functions as a defense against pathogens transported by sperm (Profet 1993).[48] There has been less response in scientific journals and what little there is has been negative (e.g., Clarke 1994, Finn 1994, Strassmann 1996). Profet

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48 See, for example, the articles in *The New York Times* (Angier 1993), *Time* (Toufexis 1993), *Newsweek* (Seligmann 1993), *People* (Plummer 1993), *Shape* (Amodio 1994), *Glamour* (Glamour editorial staff 1994), *New Scientist* (Mestel 1993) and *Omni* (Rudavsky 1994), all featuring Profet as the "maverick scientist" with the "radical thesis."

hypothesises that all internally fertilising mammals menstruate because, she claims, menstruation cleans the reproductive tract of sperm-borne pathogens, i.e., menstruation has been selected as a pathogen defense. She also argues that scientists have never thought of menstruation as functional because they have never thought to perform an evolutionary analysis of menstruation (Profet 1993, 336). The traditional view of menstruation as a preparation for the implantation of a newly fertilised egg is not a functional thesis, says Profet, rather it views menstruation as a "nonfunctional byproduct of reproductive cycling" (*ibid*).

In presenting her thesis, Profet reviews the microscopy evidence that pathogens are transported by sperm to the uterus (Profet 1993, 341). These pathogens may originate in the vagina, in the cervix, or in the male reproductive system. She then describes an array of female defenses against sperm-transported pathogens in the vagina, cervix and uterus (342-343). Of course, she notes, this defense system must be balanced with the need to make sperm welcome for reproductive purposes. The balance generally involves increased defenses somewhat during, but especially *after*, the female's period of sexual receptivity, that is, during and after exposure to sperm and the accompanying pathogens (342).

For example, during and after sexual receptivity, the walls of the vagina become cornified or scale-like "hindering sexually transmitted pathogens from colonizing vaginal tissue" (342). In the cervix, thick, acidic mucous accumulates to keep sperm and the accompanying pathogens from proceeding to the uterus.

During sexual receptivity, this particular defense must be weak to allow sperm through to fertilize eggs, but before and after sexual receptivity this defense is particularly strong. The uterus and oviducts have similarly well-timed defenses (343 - 344), such as "nonmenstrual forms of normal uterine bleeding" (348). Profet writes: "In sum, the female reproductive organs have a cascade of defenses designed to protect them against sexually-transmitted pathogens. I propose that menstruation is one such defense" (344).

Profet argues that menstruation protects against pathogens both mechanically and immunologically (345). In the mechanistic process, pathogens attached to the endometrium (lining) are expelled as the endometrium is sloughed off. "Pockets of menstrual blood form hematomas at the base of the endometrium, which lift, stretch, and help to shed it" (345). In the immunological process, pathogens are fought with leukocytes. "Menstrual blood delivers large concentrations of leukocytes to bacteria-infested endometrial tissue. Leukocytes directly combat pathogens and also phagocytize [envelope] potentially infected necrotic tissue" (345).

Profet then anticipates and counters a possible problem with her thesis. Pathogens such as bacteria actually *require* iron to survive. It might seem unlikely then, that an iron-rich substance such as menstrual blood has the function of *combatting* bacteria (346). Profet responds with two arguments. The first is that the substance *lactoferrin*, which is found in both menstrual and venous blood, chemically sequesters the iron, making it

unavailable to bacteria. Levels of lactoferrin in venous blood have been shown to increase prior to menstruation (and it is inferred that the levels are high in menstrual blood as well) (346). The second is that iron levels are low in venous blood during menstruation to begin with (again, it is inferred that the levels are low in menstrual blood as well). She also notes that iron levels in menstrual blood might be *less* than those in venous blood, which would mean that the iron-loss during menstruation, calculated by measuring venous levels, is over-estimated (347).

To review, Profet argues that menstruation is one mechanism in a system of pathogen defense. Although the medical and biology journals have not been forthcoming in appraisals of any kind, it would seem that Profet has described the function of menstruation in a way that synthesises a disparate patchwork of empirical research. Such a project seems long overdue. I shall now turn to Profet's far-less compelling support of her thesis that she draws from an objectivist, etiological model of functional explanation.

8.4 "The physiology of menstruation shows adaptive design"

Consistent with the objectivist etiological approach, Profet argues that to explain the presence of a mechanism is to explain why it was naturally selected to perform its function. Specifically, she argues that the presence of menstruation is explained by its selection for the removal of sperm-transported pathogens. She is particularly concerned to argue the functional point because current clinical practice favours treating some uterine infections

by artificially inhibiting menstruation. According to Profet, menstruation actually *combats* such infection, so inhibiting menstruation at these times is contraindicated. Current clinical practice, she says "blames the firemen for the fire."

I argue that her etiological account fails to ground this potentially important functional thesis. I then show how her functional thesis would be strengthened if she first examined some pragmatic issues about the lack of entrenchment of the system she is examining, namely, female defense against sperm-borne pathogens.

Profet cites Williams' (1966) two-part investigation for identifying whether a process is a functional mechanism (i.e., whether it has been naturally selected) or not (Profet, 336). The first part of the investigation involves "identifying the problem that the candidate mechanism was designed to solve." The second part involves "elucidat[ing] design—that is, show[ing] that there is an adaptive fit between the mechanism and the problem that is too close to be merely the product of chance or the by-product of other mechanisms" (*ibid*).

To satisfy the first part, Profet itemises a number of candidate problems, besides her own preferred one, that menstruation may have been designed to solve. One such problem is the build-up of plant toxins in the uterus. Menstruation might remove these toxins. Another is the strain on the cardiovascular system that results from high iron levels. Again, iron loss through menstruation might be thought to keep these levels healthful (337).

She then continues with part two of Williams' investigation by illustrating how menstruation as a defense against pathogens shows an adaptive fit that is not found with menstruation as a removal of plant toxins or as a reduction of iron. She argues that natural selection cannot explain these other competing options. In the case of plant toxins, she points out that they can be removed without endometrial breakdown. As for the selection of iron reduction, she notes that our ancestors "rarely live[d] long enough to suffer degenerative diseases of old age" such as heart disease (337).

In arguing that menstruation as a pathogen defense shows the adaptive fit "too close to be merely the product of chance" Profet points out that menstruation *must* be an adaptation (i.e. it must be a functional mechanism) because it is too costly to have lasted unless it also offered some selective advantage (337). "If menstruation were both costly and functionless, natural selection surely would have eliminated it long ago" (336). According to Profet, menstruation is costly both nutritionally (through iron and tissue loss) and reproductively (through the reduction of the number of reproductive opportunities in any one breeding season). Further, the uterus wall is lined with specialised spiral-shaped arteries "that constrict and dilate in a sequence timed to induce menstruation" (339). Finally, menstrual blood differs from venous blood such that in menstrual blood, clotting is reduced (339). Profet believes that this efficient, precise, and complex system points to adaptation. She writes: "If menstruation were merely a functionless by-product of cyclic hormonal flux [i.e., if

menstruation were not naturally selected], there would be no mechanisms [the spiral arteries] specifically designed to cause it, nor would the constituents of menstrual and venous blood differ significantly [such that venous blood clots but menstrual blood does not]" (338).

8.5 The etiological account: Some concerns

On Profet's account, if menstruation were both costly and functionless, natural selection would have eliminated it long ago. Menstruation *is* costly. Menstruation has not been eliminated. Therefore menstruation has a function (i.e., has been naturally selected). In specifying the *exact* function, Profet argues that pathogen defense is the only functional hypothesis (that is, the only hypothesis that fits with a selection account) that can explain the presence of menstruation. That menstruation has the function of removing sperm-transported pathogens, *explains* the presence of menstruation (and the requisite physiology such as spiral arteries and low levels of coagulant). This formulation follows the standard objectivist, etiological account of functional explanation. Her argument can be reconstructed as follows:

> (1) The members of an existing species *h* which fertilise internally are functioning adequately.
> (2) For the female members of existing species *h* to function adequately, it is necessary that they have naturally selected (i.e., functional) mechanisms to remove sperm-borne pathogens from their internal reproductive organs.

(3) If menstruation as a pathogen defense were a naturally selected mechanism in the females of *h*, then premise (2) would be satisfied.
Therefore:
(4) Menstruation is present in females of *h*.

Of course, as Hempel (1965) noted, conclusions such as (4) do not follow deductively from premises such as (1) through (3). In this case, some other equivalent functional mechanism could satisfy premise (2). As Hempel points out in his example of the function of the heart, an artificial mechanism could be implanted to fulfill the same function as a biological heart. Nagel (1961) argued that the concern with functional equivalents, such as those provided by artificial mechanisms, was spurious because *normally* such artificial equivalents are not present. However, in the case of menstruation, a number of potential equivalents are already present normally, so the problem remains. Indeed, Profet's description of the defense system of the female reproductive tract provides a number of normally existing functional equivalents, e.g. vaginal cornification, mucous plugs in the cervix, and nonmenstrual bleeding in the uterus. Any or all of these could have been further selected to satisfy (2). At most Profet can infer some general sort of defense mechanism, with the built-in redundancy of the functional equivalents, but she cannot infer menstruation in particular.

Like Hempel, Profet wants an objectivist theory to ground what she believes is an ontological, naturally-given distinction between the *mere effects* of a mechanism and the *functions* of a

mechanism. In the heart example, for instance, Hempel rules out "production of heart sounds" as a function, because the production of heart sounds does not contribute to the "proper working order" of creatures with hearts (Hempel 1965). In Profet's vocabulary, the chance/design distinction provides this Epistemological grounding; the production of heart sounds is a product of chance rather than design.

I have some concerns about Profet's and other evolutionary theorists' attempts to use etiology or selection history as objectivist criteria for identifying functions. According to Profet, the etiology of menstruation as removal of plant-toxins or as reduction of iron shows that these are the products of chance, and therefore they are *merely* effects. The etiology of menstruation as defense against pathogens shows that it is a product of design, and therefore it is *the* function. However, initially, *most* phenotypic changes are the result of random genetic occurrences, that is, *chance* occurrences. If we eliminate as functionless all those evolutionary changes in phenotype that resulted from chance genotypic mutations we would end up eliminating a number of phenotypic features which we currently view as functional. A criterion for functional status, other than "designed/selected," is needed by the objectivist.

Some evolutionary theorists might respond by claiming that at the *initial* occurrence of a mechanism or trait, it *has* no function, though it may acquire functional status later in its selection history. Neander makes a similar point in her discussion of "piggyback" traits—those traits that have no selection history

except through their close connection to another trait that *has* been selected (Neander 1991, 179 - 180). According to Neander's etiological account, the "functioning" piggy-back traits have no function, because they have no selection history. One of the problems with taking this route is that the etiologist is left with no objectivist method for deciding when to confer functional status to the piggy-back trait. When in the trait's history should functional status be conferred? After two generations? Three? There are no theoretical accounts available that would answer this question.

Another problem with using "chance vs. design" to provide an objectivist grounding for the effect/function distinction arises for any mechanism that did not persist, that is, was not selected, to perform the function it currently performs. Then one is left with a case where the presence of the mechanism cannot be explained by reference to what it was *initially* selected to do, or if one chooses to explain it with reference to what it was initially selected to do, one cannot account for its current "function."

Prior (1985) and Boorse (1976) provide a number of illustrations of this problem. Prior gives examples cited originally by Darwin in 1859 (see Darwin 1962, 183). These include the swim bladder that was selected as a flotation mechanism in fish but which may have subsequently evolved to function for respiration in higher vertebrate animals (Prior 1985, 315). Boorse gives a fictional example asking how one should respond to the discovery that the male urethra first evolved as a sperm conductor, though now, of course it also evacuates urine (Boorse 1976, 76).

Recently, Godfrey-Smith has articulated an etiological account that makes reference to the *modern* selection history of a functional mechanism (Godfrey-Smith 1994). Because Godfrey-Smith's account restricts the period of focus to the most recently-selected function, it would, I think, answer Prior's and Boorse's concerns about the functional status of mechanisms whose functions have changed over a period of time. However, notice that Godfrey-Smith's criterion "recently-selected" is *relative to* human time-lines. It is not an objectivist response completely independent of human interests. Just as the more pragmatically-minded theorists have noted, future generations of human observers might identify functional mechanisms that are different from those *we* currently identify. Different system predicates and different capacity predicates may become entrenched in the linguistic use of future generations. There is no saying, in advance, how this process will go.

8.6 A pragmatist prescription

To address the problems with using selection history to provide objectivist grounds for the function/effect distinction, Cummins suggests an alternative, more pragmatic approach (Cummins 1989). He argues that, typically, we don't appeal to functions (vs. effects) to explain the presence of the mechanism in question, rather we appeal to functions (vs. effects) in order to explain the capacities of some "containing system," whether that system be an organism, a system of organisms, or a system within an organism (Cummins 1989, 501). Illustrating this suggestion

with the heartbeat example, Cummins provides the following deductively valid argument:

> (1) Vertebrates incorporating a beating heart in the usual way exhibit circulation.
> (2) The vertebrate in question incorporates a beating heart in the usual way.
> Therefore:
> (3) This vertebrate exhibits circulation (499).

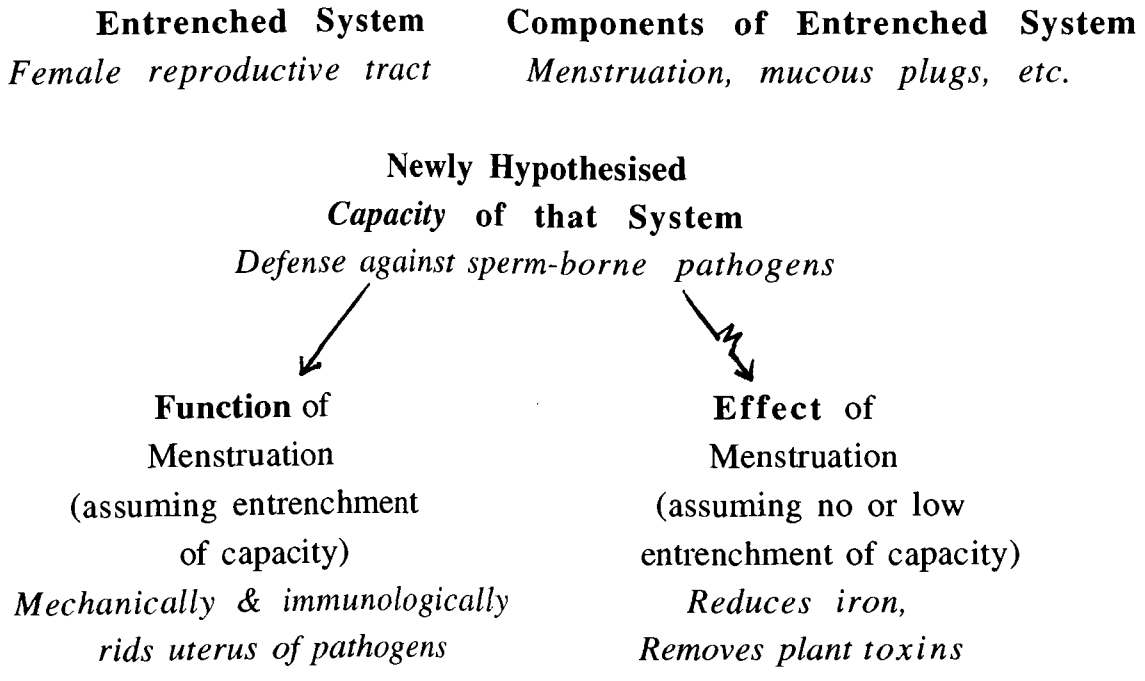
Here, hearts figure in our inference to the best explanation we have of circulation. I should note that neither I nor Cummins is committed to a deductive, nomonological (D-N) model of explanation, however, for purposes of comparison with Profet's initial deductive attempts, I have revamped her appeal to the function of menstruation, below.

> (1) The reproductive tract of female mammals incorporating menstruation, mucous plugs, cornification, and nonmenstrual bleeding in the usual way, exhibits defenses against sperm-borne pathogens.
> (2) Human females, for example, incorporate menstruation, etc. in the usual way.
> Therefore:
> (3) Human females exhibit defenses against sperm-borne pathogens.

I admit that this formulation might sound odd, but note that the oddness is not due to any logical problem. Among other things it is due to the lack of entrenchment of hypotheses about what would count as "incorporating menstruation in the usual way." I will discuss this point further in the next section.

For Cummins, part of the problem with providing objectivist grounding for the distinction between functions and effects is that the distinction can only be made *relative to* an explanation of the capacities of the containing system of *interest* (Cummins, 507). In Goodman's terms, the containing system predicate needs to be entrenched, as do predicates or categories of the system's capacities (see Figure 3). The containing system of Profet's functional account is the female reproductive tract, which is, itself, well-entrenched. But what about Profet's hypothesised *capacity* of that system (the capacity of defense against sperm-borne pathogens)? How well, a feminist might ask, is this capacity entrenched in contemporary evolutionary biology? How often has this capacity been used in making inductive generalisations? My own examination of the literature indicates: not well, and not often.

Figure 3:



One criticism of this pragmatic analysis of the criteria for distinguishing function from effect is that if a mechanism fails to function, for whatever reason, then on some etiologists' reading of these pragmatic accounts, it appears that the mechanism no longer has a function. Millikan (1989) is representative of the etiologists with this concern. She writes that Cummins' analysis of function gives us only the "marks of purposiveness," i.e., how something functions currently *as* something. For example, on the pragmatist view, the heart beat functions *as* a circulatory device, but it can't be said in objectivist or realist metaphysical terms to *be* a circulatory device. Millikan prefers evolutionary accounts because these give the *underlying* structure— "the basic sense of function that hooks function to purpose" (1989, 293). Millikan calls this sense of function the "proper function." Similarly, Neander writes of the normative role played by the notion of proper function which is related not to what any particular mechanism *can* do, as Cummins' might argue, but rather to what that mechanism is *supposed* to do (Neander 1991, 180). Neander argues that because Cummins' account lacks this normative strength, not only is he unable to speak of the function of a malfunctioning heart, for example, but his project can be used to attribute functions to a number of mechanisms that are normally thought to have no proper function at all. Regarding this latter concern she gives the example of tumors that do not play a normative functional role in biological discourse but which satisfy Cummins' criteria for functional status.

First, I will respond to the concern that the pragmatic criteria for differentiating function from effect are unable to differentiate function from malfunction. Here it seems useful to invoke a type/token distinction, whereby functional status accrues to the function as a "type" rather than to any individual "token" of that type. The pragmatist can then distinguish between the function of the type and any particular malfunctioning token. The malfunctioning token is not necessarily representative of the functional type. For example, an individual heart may be malfunctioning, and not pumping blood, i.e., not contributing to the capacities of an individual human's circulatory system. But, we still claim that hearts, in general, have the function of pumping blood, and this functional identification is related to the heart's contribution to the circulatory system.

Second, a response to Neander's concern that Cummins' pragmatic criteria for differentiating functions from effects attributes functional status to mechanisms such as tumors, when normally, "proper" functional status is not attributed to them (Neander 1991, 181). While I agree that tumors are not normally considered to be functional, it seems to me this is a purely contingent fact about what has typically fallen under "normal consideration," i.e, which topics have been "well-entrenched." (Human survival and reproduction, for example, are well-entrenched). However, one can imagination a context in which it is useful for biologists to examine a "cancer system" which has the reproduction of cancer cells as its goal. To claim that biologists typically don't do this, and so tumors, for example, don't have

biologically proper functions, seems a marginal victory. Biologists *might* do this. They might begin forming inductive generalisations about the functions of cancer systems. And when they do, the field will have to pragmatically reassess what counts as a "proper function."

Indeed, in a recent issue of *Discover*, George Williams and Randolph Nesse are interviewed about the new field of "Darwinian medicine" the focus of which is the identification of functions within *disease* organisms, and the way human bodies have evolved with vulnerabilities to these organisms (in Oliwenstein 1995, pp. 111 - 117). Williams and Nesse also note the difficulties they have had in getting the new field "entrenched" (*ibid*, p. 117). Oliwenstein reports that "few physicians or medical researchers have done much serious surveying [of disease organisms] from Darwin's viewpoint" (*ibid*, p. 115).

Relatedly, Prior reminds us that (up until the introduction of Darwinian medicine) *it just happens* that humans have an investment in those capacities that increase our survival and reproduction (Prior 1985). Natural selection is our best account of the existence of capacities that increase survival and reproduction, and so it is bound to figure in our discussions of which "effects" best explain the existence of those capacities, and which are incidental to those capacities. Like Cummins, Prior relativises her account of functions to human research interests. She writes:

> I will argue that the ascription of functions is both theory and interest relative. Typically function statements are the *products* of the method of functional analysis. Functions are those effects which contribute toward the exercise of certain capacities of the containing system (Prior 1985, 310).

The pragmatic implications of this analysis for distinguishing between the heart's function and its effects (to return to the perennial favourite example) are explained in the following:

> The function of the heart is to pump blood because it is that effect of the heart which features in our best account of the organism's capacity to take nutrients into its cells and dispose of wastes from those cells. It is not a function of the heart that it produce heart sounds because the production of heart sounds does not similarly feature in our best account of some interesting capacity of the organism (Prior 1985, 310).

Consistent with her pragmatic sensibilities, Prior does not give any special metaphysical status to the goal or capacity to which the activity in question makes some vital contribution. "Goals are simply those end states which are of special interest to us" (Prior 1985, 312). Human blood circulation, for example, is a crucially interesting goal to us. In Goodman's terms this goal is well-entrenched.

Now we can return to the question of the entrenchment of the distinctively *female* mammal's goal of defending the uterus from *sperm*-borne pathogens. According to these non-etiological, pragmatic considerations, there are three levels of "interest" or "entrenchment" required to get a functional account projected and

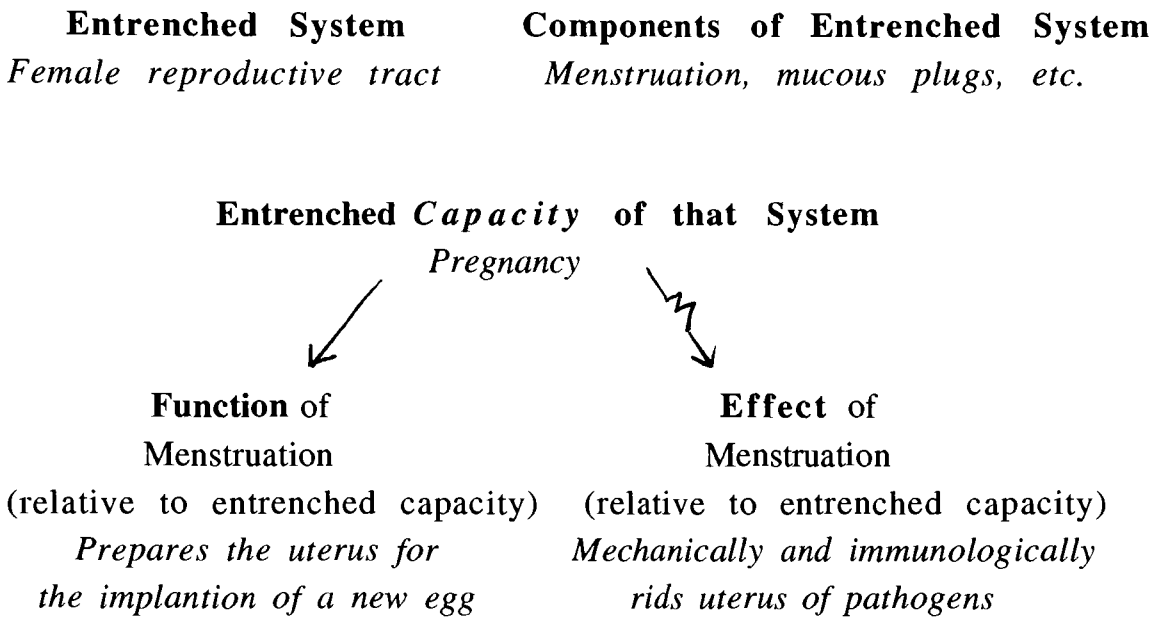
subsequently confirmed, and Profet's account is missing two. The first element is some established research interest in a particular containing system. In Profet's example, this is the system of the female reproductive tract, and this system is well-entrenched. The second is the entrenchment of a *capacity* of the containing system, that is, the capacity, or some extensional equivalent, has to have appeared in a sufficiently large number of hypotheses in evolutionary biology. In Profet's case, this would mean established research interest in the capacity of the female reproductive tract to defend itself against sperm-borne pathogens. The third element is some understanding of how individual components of that system contribute to our explanations of the capacities of that system. This element would be manifest in projectible hypotheses about the role of the components in the functioning of the system. For Profet, this would involve understanding how menstruation fits in with the healthy operation of the sperm-borne pathogen-defense system. These last two elements are currently absent in Profet's case—hypotheses containing the predicates "defends the female against sperm-borne pathogens" are not currently projectible. As Profet laments, there has been little established research interest in the sperm-borne pathogen-defense system of the female reproductive tract, and as a consequence there is little understanding of how the components of that system work together.

Here, then, is a site for feminist genealogical examination of how masculine bias often informs the choice about which scientific subjects are "of special interest to us." The need for the female

reproductive tract to remove sperm-transported pathogens may indeed qualify as one subject that has historically been of little interest to androcentric evolutionary biologists. This historic lack of interest might explain why my reworking of Profet's deductive explanation (above) sounded odd. We don't yet have established scientific criteria for what would count as "incorporating menstruation in the usual way."

It is interesting also to compare the lack of entrenchment of Profet's functional thesis, and of the capacity to which it is hypothesised to contribute, with another more thoroughly entrenched account—*viz.* the view that menstruation prepares the womb for the implantation of a fertilised egg. This latter account might not be as good an account as Profet's, but it is still presented to us as a functional account. And unlike Profet's account, it fits within the well-established, well-researched system of the female reproductive tract. In Goodman's terms, the predicate "prepares the uterus for the implantion of a new egg" is an "old-timer" well-entrenched in the linguistic conventions of physiologists, mostly through its relation to the well-entrenched capacity predicate "pregnancy"—a capacity which we are likely to associate with female reproduction. Profet's own functional account makes use of the predicate "defends the female from sperm-borne pathogens" which is not itself entrenched and has little or no historical relationship to other well-entrenched predicates (see Figure 4).

Figure 4:



Again, feminist theory has a lot to contribute to the question of how the pregnancy predicate came to be of interest, i.e., came to be entrenched in physiology and evolutionary biology, while the sperm-borne pathogen-defense predicate did not. We have good inductive evidence to suggest that the sexism of physiologists and evolutionary biologists has influenced which hypotheses about female reproduction are used and which are subsequently confirmed. It is not surprising that traditionally, scientists have made little use of a predicate that is premised on the suggestion that heterosexual reproduction might be a danger to women. Our feminist studies of sexism in science provide good *prima facie* support for further examination of Profet's functional hypothesis. It is only through an increase in the frequency with which we use the predicate "protects females from sperm-transported pathogens" that Profet's functional hypothesis can become projectible and confirmed by observations of

menstruation. Recall that Profet's account has been met with very little response in the medical and biology journals. We need to get started. Entrenchment is neither an apolitical nor an overnight process.

8.7 Conclusion

If we as feminists keep in mind Goodman's view of the relationship between projectibility, entrenchment and linguistic practice and Davidson's view of the relationship between linguistic practice, belief and truth, then we're in pretty good shape to criticise the objectivist claims of evolutionary biologists that certain systems, and functions within those systems, are "naturally given." At the same time we can avoid the relativist view that sexist scientists just "make-up" functions, or that our own prescriptions are "merely" relative to our feminist politics, (i.e., we can avoid the view that feminist politics are free from evidential constraints). Rorty explains that this pragmatist interpretive approach to science allows us to "switch attention from 'the demands of the object [of study]' to the demands of the purpose which a particular inquiry is supposed to serve. The effect is to modulate philosophical debate from a methodologico-ontological key into an ethico-political key" (Rorty 1991e, 108). The latter effect serves perfectly the demands of feminist science and science-criticism.

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