OBJECTIVITY IN FEMINIST PHILOSOPHY OF SCIENCE

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Thesis submitted to the faculty of the Virginia Polytechnic Institute and State University
in partial fulfillment of the requirements for the degree of

Master of Arts
In
Philosophy

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June 22, 2004
Blacksburg, Virginia

Keywords: feminist, objectivity, Donna Haraway, Lynn Hankinson Nelson, Susan Haack, Sandra Harding, Helen Longino, hormone replacement therapy, breast cancer, gender, philosophy of science

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ABSTRACT

Feminist philosophy of science has long been considered a fringe element of philosophy of science as a whole. A careful consideration of the treatment of the key concept of objectivity by such philosophical heavyweights as Thomas Kuhn and Karl Popper, followed by an analysis of the concept of objectivity with the work of such feminist philosophers of science as Donna Haraway, Lynn Hankinson Nelson, and Sandra Harding, reveals that feminist philosophers of science are not members of some fringe movement of philosophy of science, but rather are doing philosophical work which is both crucial and connected to the work of other, "mainstream" philosophers of science.

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Introduction

We assume that there is something special about scientific knowledge. We also assume that science has some sort of privileged perspective on the world, and that through science we have formed conclusions that are more reliable and closer to the truth than the conclusions formed in other ways. The question is: why? Why does science occupy this special position? How do we justify our confidence that scientific conclusions are sounder than conclusions drawn by non-scientific means?

One approach to responding to the "why" questions is to identify what factor or factors make science distinct from non-sciences. Ideally, only one factor would be needed to distinguish between sciences and non-sciences reliably, and that factor would also answer the question of why scientific knowledge should be considered privileged.

Several philosophers at the turn of the 20th century dedicated themselves to demarcating science from non-science. One approach of the inductivists, such as Carnap, held that the key identifying factor for distinguishing science from non-sciences is the presence and importance of the method of induction. Scientific induction allows one to infer that a generalized conclusion is likely to be true from a particular set of instances, and according to the inductivists, scientists use induction to conclude from repeated test results that they will get the same results in the future. Inductivists hold that it is through the use of scientific induction that science gains its privileged epistemic position.

One philosopher, Karl Popper, was dissatisfied with the inductivist position. He advocated that the key to the special nature of science was not induction, but instead was falsification. When a scientist is pursuing falsification, she sets up tests of the theory in question under the most risky circumstances possible; should the theory survive such "genuine tests", it can be considered scientific.

Ultimately Popper sought a logical analysis of scientific knowledge and became a vocal member of the logical positivist movement, a movement which held supreme the rational nature of science and contended that some sort of rational explanation for science could be developed. Furthermore, the positivists believed that a rational explanation for science would firmly establish scientific knowledge as privileged. At the same time the positivists acknowledged that many decisions within science are merely conventional and therefore also acknowledged that some elements of relativism existed in science.

Several post-positive philosophers spoke out against the positivist movement. Relying on many of the same philosophical groundings as the positivists, the post-positivists focused on the element of relativism within positivism and attempted to show that the relativism within science ultimately cannot be contained – it permeates all of science. The post-positivists argued that a rational explanation for science couldn't be developed because science is not a rational activity. Several post-positivists concluded further that science does not, in fact, occupy any sort of privileged epistemic position.

Many similarities exist between the positivists and the post-positivists; it is primarily in their conclusions that they differ. Both camps held a rigid notion of what it means for a field to be rational, for example. The positivists and the post-positivists both agreed that rational theory choice is algorithmic. The real difference between the two

camps is that the positivists believed it possible to develop such an algorithm, while the post-positivists rejected that possibility (and contended as a consequence that theory-choice is not rational). In spite of these differences in conclusions, post-positivism is merely a natural extension of positivism; post-positivism's advocates were not developing a new school of philosophy of science so much as drawing out the sometimes-overlooked positions of the positivists and bringing them to their logical conclusion.[1]

Positivism and post-positivism were two of the most prominent movements within philosophy of science during the 20th century, but many other movements existed as well. Just as positivism and post-positivism are intimately related, some of these other movements are natural extensions of either positivism or post-positivism as well. At first, it might appear that categorizing a movement as related (or not) to the positivists or the post-positivists is a simple matter of preference and focus. For example, if one desires to emphasize the continuity of philosophy of science, then all movements within philosophy of science can be related somehow or other. One might wonder if, as long as each movement is given appropriate consideration, it matters whether and how the history of philosophy of science is constructed.

Yet it is precisely the matter of "appropriate consideration" which employs the history of philosophy of science. In all spheres from the classroom to the editing offices of journals, choices are made about which historical movements are important. Furthermore, when considering a contemporary issue like objectivity in science, philosophers have to decide whose work is relevant. The works within movements seen

[1] See the first chapter of Larry Laudan's *Beyond Positivism and Relativism* (Westview Press, 1996) for further elaboration on the positivist roots to post-positivism.

as central in philosophy of science are privileged over the works done within movements that are seen as tangential.

Feminist philosophy of science is one movement within philosophy of science whose positioning within the field is in contention. During the latter half of the 20th century, the growing field of feminism engaged in a great deal of work to understand the roles that gender, culture, and power have in shaping individuals' conceptions of the world. Part of this work focused on science and questioned what impact gender has had on scientific progress.

For some feminists, investigation into gender bias within science involved case studies – pointing out that certain theories or experiments relied upon gendered assumptions. For others, this investigation became a critical analysis of gender bias within a whole field. Scientists like Ruth Bleier and Nancy Chodorow each focused on one field - neurophysiology and biology, respectively - in an attempt to show how those fields were affected by gender biases in a number of ways.[2] Their works each draw on different tools - Bleier's work is rooted in feminist theory, while Chodorow uses psychoanalytic tools for her arguments - but both review the current state of the field and argue that important changes in how science is practiced need to be made. As Bleier puts it: "it need not be permitted to claim that science is or even can be objective, transcendent, neutral, and value free."[3] Beginning with critiques of a single case study or a single field, feminists began to argue that science could be criticized as a whole and that

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2 Ruth Bleier's work on neurophysiology can be found in her book *Science and Gender* (1984) Elmsford, NY: Pergamon Press. Nancy Chardorow's work can be found in her book *Reproduction of Mothering* (1978) Berkeley: University of California Press.

3 Bleier 201.

gender bias can only be eliminated through changing the "scientific method" as it has been traditionally conceived.

Around this same time, a number of budding philosophers were being trained in the philosophy of science of Quine, Kuhn, and other post-positivists, or in the works of philosophers such as Marx and Foucault. Writers who called upon their various philosophical training to argue that more rigorous attention to method cannot eliminate gender biases formed a new field of philosophy, called feminist philosophy of science or feminist epistemology. These writers advocated whole new ways to conceive and to practice science. These feminist philosophers, seeing the case studies developed by feminist historians of science and by feminist scientists, had practical grounds for developing their theories of science, but they also had philosophical reasons for doing more – for developing critiques of previous conceptions of matters such as the nature of "objectivity". After developing such critiques, many feminist philosophers of science went on to develop positive philosophical positions on subjects like how the scientific community should conceive of "objectivity", and how to increase the "objectivity" of a given enterprise.

Though the presence of feminist philosophy of science as a field is undeniable, what is under contention is how to describe these philosophers - are they the product of a natural progression within philosophy of science or outsiders who misunderstand the issues and topics on which they are weighing in? And why does their placement matter? How does it affect philosophy of science? I address each of these questions in my thesis, beginning with how the feminist philosophers should be located in relation to philosophy

of science more generally and, once this issue has been explored, concluding with a discussion of why the treatment of the feminist philosophers of science matters.

In my thesis, I argue that feminist philosophy of science, having the same basic philosophical groundings as the positivists and the post-positivists, is intimately related to the rest of philosophy of science and has an important role to play in contemporary issues within philosophy of science as a whole. I argue that on the key issue of objectivity, there is a clear progression of ideas from the work of philosophers such as Kuhn to the later work of philosophers such as Longino and Harding; finally, I argue that the feminist philosophies are not simply a natural extension on traditional philosophy of science, but they also represent an important step forward for the field.

The claims I make in my thesis are certainly contentious. There are many philosophers who believe that feminist philosophy of science is disconnected from philosophy of science at large and that it represents an unfortunate misstep in the progress of the field. These philosophers attempt to discredit the field of feminist philosophy of science as a whole by showing some fundamental mistake the feminists have made. Often opponents of feminist philosophy of science identify this "mistake" as resulting from the fact that the feminists, as outsiders to philosophy of science, misunderstand and misuse some of the concepts traditionally considered within philosophy of science.

Susan Haack is an excellent example of just such an opponent of feminist philosophy of science. In a variety of essays Haack makes clear her position that "radical feminism" - as she frequently calls it - gone awry.[4] Similarly, she argues that recent trends in philosophy of science, trends which include the sociologizing of science and

work in feminist philosophy of science, are the work of misguided outsiders to the field. She makes this point most clearly in her essay "Puzzling Out Science." Here Haack critiques the "New Cynics" of science - the "radical sociologists, radical feminists, radical Afrocentrists, and radical followers of (somewhat dated) Paris fashions in rhetoric and semiology" who advocate that science is the product of social negotiation, and as such should not be considered to have an "objective" or "rational" quality.[5] According to Haack, the New Cynics do not engage in philosophy of science when they criticize science; instead, their work is an attempt to foist off politics as philosophy. Therefore the New Cynics, and their arguments, can be dismissed as irrelevant when engaging in a discussion of a particular question within philosophy of science.

Integral to her arguments is the position that the New Cynics operate in a fundamentally different framework from other philosophers of science. She develops this point by first describing the "Old Deferentialist" position within philosophy of science; the Old Deferentialists (who go unnamed by Haack) believe that science progresses through the accumulation of true theories – that is, theories which are supported by one of a variety of methods including surviving attempts to falsify it, having a great deal of positive evidence to support it, etc. In Haack's description of the history of philosophy of science Quine, Hanson, and Goodman each describe problems for the Old Deferentialist position which are "tough" but also "superable, or avoidable."[6] Indeed, according to

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4 These essays include "Science as Social - Yes and No", "Reflections of an Old Feminist", and "Puzzling Out Science", among others. All of these essays can be found in Haack, 1998, *Manifesto of a Passionate Moderate,* University of Chicago Press, Chicago.

5 Haack 1998, 91.

6 Ibid.

Haack, Kuhn himself "did not intend radically to undermine the pretensions of science to be a rational enterprise," a fact which most readers of Kuhn apparently miss.[7]

Philosophers of science mostly continued (and continue) to believe that the Old Deferentialism is correct in essentials, though they did concede that much more work is needed on the details. It is at this point in her description of philosophy that the New Cynics enter the scene, believing that they have brought a real challenge to the Old Deferentialist program. Yet, according the Haack, rather than offering up a legitimate argument against the Old Deferentialist program, the New Cynics offer only "an astonishing farrago of confusion, non sequitur, rhetoric".[8] Haack makes the claim "it is beyond my power, and your tolerance, to deal comprehensively with all this" but then does go on to suggest a key fallacy that, as she sees it, explains how the New Cynics misunderstand traditional issues within philosophy of science.[9]

Haack identifies the key mistake within the New Cynics' strategy as focusing on acceptance (the agreement of a particular community that a claim should be accepted) over warrant (the quality of the evidence for a particular claim). Consider the example of hormone replacement therapy (HRT) studies. Initially, such studies were purely observational; these studies compared the heart disease rates for women who were currently on HRT with those for women who were not. Researchers believed that HRT worked as an effective preventative measure against heart disease in women, and possibly against certain cancers as well, and advised that menopausal women be placed on hormone replacement therapy. However, when an organization focused on women's

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7 Ibid.
8 Haack 1998, 92.
9 Ibid.

health issues conducted their own study, this time randomly assigning women either to receive the therapy or to take a placebo, they discovered that hormone replacement therapy actually increases the risk of heart disease and certain forms of cancer. The illusory effect of a decrease in heart disease rate was a product not of the fact that the women were on hormone replacement therapy, but rather of the fact that the women who tended to be on hormone replacement therapy tended to have better health practices overall.

While many of the "New Cynics" might use such an example to show that the initial group of researchers was biased in some way that prohibited them from recognizing fundamental flaws in their study, Haack would break down this example in terms of acceptance and warrant. For Haack, the warrant for a particular scientific claim is far more stable (though perhaps less accessible) than the acceptance for that claim. The warrant for a claim is a function of the "quality of evidence with respect to that claim at that time."[10] Since definitions of what "counts" as evidence and what counts as "good" evidence are both the result of a particular scientific community's reflections at a particular time, ultimately under Haack's definition the warrant for a scientific claim is relative. However, since it is the experiences of a rather large scientific community that is determining the merit of accepting a particular claim, the size of the community should provide some stability in the definition of what makes a claim warranted. Alternately, the acceptance of a claim is the decision of an individual, or perhaps a small subcommunity. Thus Haack has set up a relatively stable background measurement, the warrant, against which the acceptance of a claim can be judged. In the case of the HRT studies, the initial researchers accepted the results, but they did not have warrant for the results. Ultimately,

in claiming that the New Cynics focus on acceptance over warrant, Haack is also arguing that her distinction between warrant and acceptance can maintained as meaningful.

Haack ponders why such a shift in focus from warrant to acceptance could have occurred - perhaps coming from backgrounds in areas such as literary theory and sociology these New Cynics are more prone to attend to the social elements of science rather than considering the role warrant plays. Regardless of their reasons for focusing on acceptance over warrant, Haack argues that in making such a move the New Cynics are too quick to assume that scientific claims are simply accepted as definitely true or rejected as definitely false. Instead, "evidence may be better or worse, warrant stronger or weaker, and the acceptance status of a claim can, and should, vary accordingly."[11] Should action upon a theory be required – for instance, should a person decide to build a rocket ship to the moon – Haack holds that the scientist or engineer can act "as if the theory were true." Therefore the individual in question is not accepting a theory as true, she is simply accepting the theory as useful. Real acceptance, for Haack, is a rare thing in so far as acceptance is read as total acceptance. Haack contends that another of the key errors of the New Cynics is the incorrect attribution of a true/false dichotomy within science.

Haack's stance on the warrant/acceptance distinction assumes that there is a distinction - that the warrant of a particular claim is somehow more objective and distinct from the acceptance of that same claim. Yet the notion that one can draw a meaningful line between warrant and acceptance is a claim that the New Cynics, or at least the feminist philosophers of science among them, question. Furthermore, the feminist

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[10] Haack, Susan. *Defending Science – Within Reason*. (Amherst: Prometheus Books, 2003), 60.
[11] Haack, 1998, 110.

philosophers of science have not come to question the relationship between acceptance and objectivity due to some ignorance concerning philosophy of science; when closely examining some of the principles common among various feminist philosophers of science work on objectivity, the connections - rather than the disconnections - between the feminist philosophers of science and previous philosophers of science such as Kuhn come to light.

In investigating the treatment of objectivity as a social concept in feminist philosophy of science, I consider the work of several leading writers. These philosophers - Helen Longino, Lynn Hankinson Nelson, Sandra Harding, and Donna Haraway - have been associated with different "camps" within feminist philosophy of science. Longino and Nelson are viewed as (and call themselves) "feminist empiricists", Harding is identified with "standpoint feminism", and Haraway is frequently called a "feminist postmodernist". These camps - outlined by Harding in the mid 80's - no longer appropriately classify all of the work done in feminist philosophy of science today, yet they do represent three very different programs and prescriptions for philosophy of science. After exploring each of these programs individually, I show that beneath their differences is a common treatment of objectivity as a social concept.

Furthermore, turning to the work of Kuhn, I demonstrate that the feminist philosophers' treatments of objectivity is fundamentally rooted in the same concerns about the social nature of science which extend back into the post-positivists and indeed into the positivist movement in philosophy of science. However, the feminist philosophers of science move beyond Kuhn and previous philosophers' descriptive accounts of science to give prescriptive accounts of how science should conduct itself.

Ultimately, far from being a group of cynics discussing philosophy they don't fully understand, the feminist philosophers of science represent a productive and related movement within philosophy of science. After having discussed the feminist philosophers of science individually, having uncovered a similar treatment of objectivity within their work, and having connected that treatment to historical figures within philosophy of science, I then return to Susan Haack's description of the feminist philosophers of science in order to show that her attitude of excluding the feminist philosophers of science from philosophy of science is completely misguided and misrepresents those philosophers. Only after dismissing Haack's description in favor of a new treatment of feminist philosophy of science do I explore the structure of the field of philosophy of science, so that I can consider not simply why the feminist philosophy of science could be understood as important philosophy of science, but why it should be.

Chapter One:

A Review of Feminist Philosophy of Science

Currently, the body of feminist philosophy of science includes writings from Marxist postmodernists, scientists, historians of philosophy, philosophers of science, and a diverse range of other authors. Rather than engaging in the impossible task of summarizing feminist philosophy of science in all of its diversity and imposing a definition upon the field that would arbitrarily delimit its boundaries, I instead focus on several leading philosophers within feminist philosophy of science. In selecting these philosophers, I kept in mind the traditional dissection of feminist philosophy of science into three camps – feminist empiricism, feminist standpoint theory, and feminist postmodernism. While these camps no longer adequately incorporate all writers working within feminist philosophy of science, I use them as a guide to ensure this study is not limited to one particular facet of feminist philosophy of science, but instead extends through the work of a diverse set of philosophers in the field, each working from a quite different perspective.

In order to illustrate best the differences between the various philosophers' views on science, I return to the example of hormone replacement therapy (HRT) and consider how each feminist philosopher of science would describe the example using the language of her particular brand of feminist philosophy of science. In order to provide a relatively neutral perspective from which the feminists can be compared with each other, first I describe the HRT example in greater detail. Once the basic facts of the HRT example have been outlined, I return to my descriptions of various philosophical positions within

feminist philosophy of science, using my own analysis of how each philosopher would respond to the HRT example to elucidate the differences between their philosophies.

Hormone Replacement Therapy – A Brief Review

In 1945, combined estrogen and progestin therapy were approved to treat menopausal symptoms including hot flashes, mood swings, and fatigue. Women who had undergone a hysterectomy could use HRT with estrogen alone, but estrogen-only HRT was believed to be risky for women who still had a uterus, as it appeared to increase the risk of cancer of the uterus. By the 1980s, researchers believed that they had data suggesting that long-term use of combined HRT might help prevent heart disease, as well as perhaps certain cancers in women.[12] In as late as 1992, researchers were still convinced that they had sufficient evidence to show that HRT reduced the risk of heart disease, but by this point many researchers were beginning to see evidence that HRT might increase the risk for endometrial cancer and may be associated with a small increase in risk for breast cancer. However, researchers believed that ultimately the benefits for HRT for women in certain classes - for those who had had a hysterectomy and for those with coronary heart disease or at high risk for coronary heart disease – sufficiently outweighed the potential risks to recommend HRT on a limited basis. Enthusiasm for whole-heartedly recommending HRT was tempered by the recognition by researchers that more study needed to be done on HRT to understand the differences between combined HRT (estrogen and progestogen) and estrogen-only HRT, as well as to understand how HRT might affect a woman's risk for other diseases.[13]

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12 Smith, Michael, "Women, Doctors Still Confused by HRT" in Medline Plus http://www.nlm.nih.gov/medlineplus/news/fullstory_16077.html.

13 Grady D, Rubin SM, Petitti DB, Fox CS, Black D, Ettinger B, Ernster VL, Cummings SR, Hormone therapy to prevent disease and prolong life in postmenopausal women. Ann-Intern-Med. 1992 Dec 15;

All of these results were from observations, not experiments. Researchers used data collected from women who were currently on estrogen-only HRT and combined HRT to conclude that the use of HRT helps prevent heart disease. Yet, as Dr. Marcia Stefanick noted in a 2002 NewsHour interview, "We know that the estrogen users were by and large a healthier group of women, and so we have what we call the healthy user bias effect, and there are a number of other kinds of biases that have led to this, now we know, incorrect belief that estrogen would prevent heart disease or estrogen plus progestin."[14]

But the biases recognized through hindsight were largely unapparent to researchers in the early 1990s, who continued to work under the assumption that HRT prevents heart disease and possibly other diseases in post-menopausal women. When the National Institutes of Health established the Women's Health Initiative, a multi-million dollar effort to study the health of post-menopausal women, it did so with the belief that the WHI would enable doctors and researchers to better understand the exact benefits of HRT; it was assumed that HRT would, in fact, have benefits. The WHI emphasized the use of clinical trials in which women were randomly assigned to receive either the procedure or a placebo. Researchers hoped that, through the use of these experiments, the WHI would clear up discrepancies in some of the observational studies.

Researchers were shocked by the results they found. Among the women taking the combined HRT, researchers found "a 41 percent increase in strokes, a 29 percent increase in heart attacks, twice as many blood clots in the lungs and legs, a 22 percent increase in total cardiovascular disease and a 26 percent increase in breast cancer

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117(12): 1016-37.
14 Stefanick, Marcia, http://www.pbs.org/newshour/bb/health/july-dec02/hormone_7-9.html

compared to women taking a placebo."[15] The effects found were so pronounced that the researchers decided to stop the study in 2002, three years early.

In spite of these dramatic results, many doctors and patients still remain convinced that the benefits to HRT outweigh the risks. For example, while doctors acknowledged that the WHI study disproved that combined-HRT lowers menopausal women's risk of heart disease, many still felt that estrogen-only HRT should still be proscribed for women who had had a hysterectomy.[16] Patients still desired both forms of HRT, believing that it contained benefits ranging from younger-looking skin to improved organ health.

Dr. Susan Love, in a 2003 interview on WebMD, described the culture that led to the development of HRT, and which still lead many to believe HRT must ultimately be beneficial to menopausal women. As she describes the medical community, they "thought that menopause was a disease -- estrogen deficiency disease." Love claims that the equation of menopause with disease now seems "a little crazy" since menopause is "normal", but contends that nonetheless at the time the medical community thought of menopause as a disease and, since it was thought of as such, ended up "thinking [they] need to treat it, or replace what's missing. And that led us to the hormone replacement (HRT) idea."[17]

We can break the historical example down into three major phases. The first phase addresses the initial use of HRT – the exploration into the possible benefits of hormones for menopausal women. The second phase involves the initial use of

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15 "Questions and answers about hormone replacement therapy" from http://www.st-joseph.org/news/HRTrisky.htm

16 In March 2004 the WHI had to stop their estrogen-only HRT study, citing results similar to the earlier combined-HRT study.

observational data that, it was thought, confirmed that HRT could be quite beneficial for women in certain categories. The third phase begins with the WHI study, and includes both the results of that study and the general reaction to its findings. Let us now turn to how feminist philosophers of science might analyze these different phases.

Feminist Empiricism

Let us consider first the feminist empiricists. Sandra Harding, a leading feminist philosopher, traces feminist empiricism as arising from the 'spontaneous consciousness' of feminist researchers. Recognizing that a historical review of science generates significant examples of biased research, these researchers sought to 'cleanse' science by advocating a more rigorous and careful attention to existing methods of research.[18] In their view, those biased results were obtained through 'bad science' – scientists not properly adhering to scientific rigor and detachment – and therefore researchers should focus their efforts on maintaining scientific standards.

As Harding herself acknowledges, the views of more recent feminist empiricist philosophers are not captured in any detailed fashion by the above description. Helen E. Longino, classified by Harding as a feminist empiricist, contends that few feminist scholars have conformed to Harding's profile.[19] Feminist empiricists do exist, but they are mostly self-defined and as a result no overarching definition of feminist empiricism exists. Two such self-defined scholars are Helen Longino and Lynn Hankinson Nelson.

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17 Love, Susan . "Hormone Therapy after Menapause," 2003, from http://my.webmd.com/content/article/62/71736?src=Inktomi&condition=Menopause

18 Harding, Sandra. "Rethinking Standpoint Epistemology: What is 'Strong Objectivity?" in Evelyn Fox Keller and Helen E. Longino (eds.) *Feminism & Science* (Oxford: Oxford University Press, 1999), 237.

19 Longino, Helen. "Subjects, Power, and Knowledge: Description and Prescription in Feminist Philosophies of Science" in Janet A. Kourany (ed) *The Gender of Science* (Upper Saddle River, New Jersey: Prentice Hall, 2002), 312.

Helen Longino retains at the heart of her position the view that the world really exists, and that we can obtain knowledge about the world "by the traditional methods of the natural sciences."[20] Scientists cannot and should not abandon their current methods of inquiry, but Longino does not believe a methodological critique or increased attention to scientific rigor would provide the scientist with sufficient tools for analyzing troublesome research programs.

One reason she denies the power of the methodological critique is that she believes the scientific "method" is not an absolute concept; it exists only within the community that agreed upon and developed that method. In addition, research might be troublesome not due to any methodological issues, but because of the underlying assumptions and models of a particular researcher.[21] A methodological critique alone does not include a critique of the underlying assumptions both within the community and within the individual, and therefore is insufficiently strong.

Rather than relying on methodological critiques to root out gender, race, and class biases within science, Longino instead proposes that we begin by considering the ultimate function and goals of science. Scientific knowledge, Longino argues, is not constructed by one individual independently applying methods to her pursuits, but instead is the result of many individuals working in interaction with each other. Scientific communities are productive to the degree to which they successfully criticize and transform individual scientific inquiries. To that end, the communities need not be 'scientific' in nature – in the sense that they include only scientists – but must instead

20 Longino 310.

21 Longino stresses this point through example: "a critique of research on the influence of prenatal gonadal hormones on behavioral sex differences that is limited to methodological critique of the data fails to bring out the role of the explanatory model that both generates and gives evidential relevance to that data." (312)

focus on being dialogic. In presenting her case for community diversity, Longino points out that acceptance of background assumptions is often an unspoken prerequisite for membership within a community. Therefore, criticism within that community never reaches the foundational level, and those communities who narrowly define themselves (say, as 'scientific') are destined to be less productive than a more inclusive counterpart.

Longino specifies several criteria necessary for productive dialogic communities: there must be publicly recognized forums for criticism; the community must not simply tolerate dissent, but respond to it in a reflective manner; the community must draw up its own publicly recognized standards against which scientific endeavors are evaluated; and there must be equality of intellectual authority within the community. Longino argues that the last standard is not calling for relativity. Instead, it requires that the community regard all criticisms and arguments from its members as useful and worthy of serious consideration.

As Longino herself acknowledges, a dilemma arises if one both advocates pluralism within a community as a means to scientific knowledge and at the same time maintains the traditional scientific standard: scientists obtain knowledge through the pursuit of community consensus. Her solution to this dilemma is to reject the traditional standard and to "detach scientific knowledge from consensus." Since communities often include sub-communities, each interested in different relationships or focusing on different aspects of certain relationships, it is to be expected that these sub-communities (with different goals in mind) will come into conflict over how to model a particular part of the world. Longino argues that in such a circumstance both communities might be

correct in a certain sense, insofar as each model allows its researchers to direct their "interactions and interventions" effectively.

Implicit in Longino's solution to the pluralism dilemma is the position that scientific knowledge does not exist in isolation from individuals interacting with their environment; indeed, it is within the dynamic and (hopefully) increasingly successful interactions between individuals and the world that scientific knowledge can be found. Since a theory cannot be considered 'right' absolutely, talking about knowledge only makes sense if we locate that knowledge within a community.

When analyzing the HRT case study using Longino's brand of feminist empiricism, the inability of the scientists to recognize their biases in the first phase of HRT use is the result working within an insufficiently dialogic community. The researchers each assumed something– like menopause is a disease that can be "cured" by HRT – that caused them to accept without question an interpretation of the observational data they were receiving. The individual assumptions were similar enough in kind so that the researchers were unable to check each other – blinded by their own biases, each scientist was unable to recognize *other* scientists as being similarly hampered.

In the second phase of the HRT case study, in which scientists gathered observational data that seemed to support their earlier conclusions about HRT, a danger existed that scientists would never evaluate the possibility that they were drawing biased conclusions, simply because they were gathering research with an eye towards a particular result (supportive of HRT). As Longino might phrase it, it was lucky that the researchers eventually designed a study that gave them such conclusive results against HRT, and therefore made them reevaluate their rationale for their initial support. It was

especially lucky considering that the researchers originally designed the study to evaluate the *positive* effects of HRT.

In order to systematically limit the potential for bias, the researchers should have pursued the inclusion of a diverse range of opinions on subjects such as menopause, the appropriate use of medicine and medication, and the nature of diseases. That group would have been more likely to identify the HRT research, based on observational data, as depending upon certain assumptions about the female body. One might argue against Longino that diversity was unnecessary in this case; during the third phase of the case study, the scientists discovered their error through what could be considered routine scientific channels. Yet in that phase, scientists uncovered their errors not through some sort of methodological critique – recognizing the inherent flaws in observational studies and seeking to reevaluate systematically the assumptions which made that observational data make sense – but instead through chance. The study was not, after all, designed neutrally; it was designed to investigate the positive effects of HRT. Longino believes that increasing diversity will increase the propensity for scientific biases to be uncovered, so while there is no *guarantee* that increasing the diversity would have resulted in the scientists uncovering the biases in their research, there is a (theoretical) account for how increasing the diversity should have increased the propensity.

Ultimately, the HRT case study cannot directly support Longino's arguments regarding diversity, although certainly she can use her feminist empiricism to describe what had "gone wrong" in the scientific community to allow such wide-spread acceptance, based on observational data, of a hypothesis which was ultimately shown to be incorrect. In addition, we can use the HRT study to illustrate Longino's key point

about scientific knowledge – it was not that individual researchers knew that HRT does or does not help prevent heart disease, but that knowledge about HRT was located within the scientific community as a whole. Scientists working within their (limited) community did not recognize the methodological problems with concluding from observational studies that HRT is beneficial for women because the scientists were not sufficiently diverse in their assumptions. Therefore, the best venue for the community to decrease the chance that scientists fall prey to a sort of "group think" is for the scientific community as a whole to increase its diversity.

Lynn Hankinson Nelson, another feminist empiricist, would have a slightly different take on why scientists in general initially accepted that HRT was beneficial for women's health. Like Longino, Nelson criticizes the idea that scientific investigation will eventually result in definitive evidence for one theory or viewpoint of the world, and argues that scientists conduct their investigations within a context of theories, practices, and commonsense knowledge. However, Nelson focuses far more than Longino on the social nature of the evidence collected within the scientific community. In addition, many philosophers find another distinction between their works: [22] Longino discusses individuals having knowledge within a community, while Nelson holds that "communities, not individuals, are the primary loci of knowledge."[23] Before we discuss the possible distinction between Nelson and Longino, and the resultant distinction

22 Linda Alcoff and Elizabeth Potter, editors of *Feminist Epistemologies* (New York: Routledge, 1993), mark out a distinction between Longino and Nelson: Longino analyzes epistemic agency "in terms of a single knower" in a social situation, whereas Nelson holds that such an endeavor is wrongheaded (Alcoff 8).

23 Nelson, Lynn Hankinson. "Epistemological Communities" in Janet A. Kourany (ed) *The Gender of Science* (Upper Saddle River, New Jersey: Prentice Hall, 2002), 323.

between their treatments of the HRT case study, let us consider Nelson's arguments separately.

W.V.O. Quine heavily influenced Nelson's work. She begins her case for communal knowledge with a reference to his underdetermination thesis: "our theories are and will forever remain underdetermined by all the evidence we have or will ever have for them."[24] In other words, we cannot simply use deduction from our experiments to decide which theories of the world we should adopt. We must make a choice, and that choice is not going to be justifiable by reference to some result; instead, just as there are an indefinite number of curves which can fit a set of data points, so too are there an indefinite number of possible theories which could fit our data about the world. There is, no one *true* theory of the world (although one theory may be commonly preferred over all others).

Our theories of the world do not exist independently from each other; changing a theory in physics might change a theory in chemistry, for example. For Quine, every theory in a world-view relates to every other theory. We can imagine the theories as individual points, each connected to the other points to which it at all relates. Taken all together, all of these theories form a sort of web, with changes in one point reverberating throughout the web. While there is no one true theory, there are theories whose insertion into the web would disturb our web less; changing a theory which has a great number of neighboring theories, for example, would require more adjustments than saving that theory and making adjustments to a more "fringe" theory. Quine argues that individuals work to preserve their "web of belief" in as stable a state as possible.

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24 Nelson 324

These descriptions of Quine's views alone do not immediately bring us to Nelson's view of knowledge as communal; they simply point out that knowledge is constructed. From this starting point, Nelson adds that evidence is the result of working within a system of theories and practices. Nelson works with the example of the discovery of proton structure in one of her essays. Such a discovery required theories ranging from "that the actions of subatomic particles underwrite events at the macroscopic level" to basic laws of nature. It also required practices such as standards of evidence and methodologies. These theories and practices formed the "system" that underwrote that discovery, and similarly there is a system underwriting every current discovery.

This system of knowledge could be understood as the web of belief, with one important qualification: no individual created this system. It exists only as the product of a community. Nelson's example elucidates her position that no one person has created the huge structure of belief within which modern scientific discoveries are made; that structure of belief is, in many ways, inherited as a product of previous generations' refinements. Furthermore, according to Nelson no one individual *holds* that system of belief, in the sense that they have complete control over it. She contends that it is nonsense to talk about an individual possessing knowledge. Knowledge about any particular theory cannot be abstracted from the community's system; to attempt to make such an abstraction is to attribute too much control to the individual.

Nelson does qualify her statements about the communal nature of knowledge. She does not wish to be considered a relativist or an advocator of coherent theories of knowledge. She retains the empiricist position that evidence will come to bear in some

meaningful way on theory choice. She states that "an attempt to suspend one's belief in the existence of something akin to gravity" will point out that not all theories are equally compatible with experience. Nelson diverges from Quine on this point.[25] Quine believed that any theory could be accommodated or discarded in accord with evidence, but certain theory choices would be more difficult to make since they would require more adjustments to the web of belief. Nelson emphases the latter argument, and concludes that certain theories would require such difficult adjustments to the community's system of belief that those theories could never be considered practically acceptable. Thus evidence can limit our freedom of theory choice, although it still ultimately underdetermines our selection.

Nelson holds a particular view of evidence: it is not "experienced" by individuals, but instead is fundamentally social. As mentioned earlier, the same social context that makes the scientific investigation possible also makes evidence collection possible. Additionally, the system constitutes "*part of the evidence for [the evidence]*."[26] Just as an individual cannot be abstracted from the community and said to possess knowledge of a theory, so too an individual cannot be said to possess knowledge of a piece of evidence.

Let us return to Nelson's own example of the discovery of proton structure to understand her position on evidence. She holds that "experiences of protons were *not*

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25 It is worth noting that, while I have a presented a simplified version of Quine here, his actual formulation of the underdetermination thesis differs significantly between some of his works. As Laudan points out, Quine's underdetermination thesis can actually be separated out into two different versions (Laudan 33). The first version is the "egalitarian thesis", while the second version is the "nonuniqueness thesis." Under the first version, Quine proposes that any statement can be held true come what may. Under the second, more modest version, Quine proposes instead that for any theory, there is an indefinite number of rival theories that are as well supported by the evidence. I have been working with the egalitarian thesis here, but even if Quine's underdetermination thesis is taken in the more modest form of the nonuniqueness thesis, it still differs significantly from Nelson's conclusion that evidence will severely limit theory-choice.

26 Nelson 327, italics in original.

possible until relatively recently", and that these experiences became possible not simply as a result of technological changes but as a result of changes in the community's system.[27] We can investigate proton structure only because our community's system makes that investigation possible, and therefore our very experiences themselves are located within the community, not the individual.

Most "bad science", Nelson contends, is the result of a failure to take into account all of the possible evidence. She gives the man-the-hunter reconstruction of human evolution as an example. Such a theory emphasizes the importance of the role of the male hunter to the eventual evolution of humanity. However, many researchers have since criticized the "man-the-hunter" theory, pointing out that "woman-the-gatherer" probably played at least as important a role in human evolution. Nelson holds that one of the pieces of "evidence" which was supporting the "man-the-hunter" theory was an organizing principle that "leads to partial and distorted accounts of social dynamics."[28] Nelson suggests that if this organizing principle were properly evaluated *as evidence,* scientists would realize that it cannot be used, and therefore the "man-the-hunter" theory would not appear preferable to the "woman-the-gatherer" theory.

To return to the HRT case study, rather than emphasizing a lack of diversity in the community, Nelson would focus on the use of assumptions like "menopause is a disease" as evidence. In particular, she could ask the question, "What causal explanation could researchers have offered for their observations that HRT lowered women's risk of heart disease?" Within those causal explanations would likely be the assumptions that were

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27 Changes in technology, Nelson argues, are driven by our theories and so cannot account for our ability to see something now that we did not see before. We now have the technology to investigate proton structure because we designed it, and we designed it within our system of theories. (Nelson 328).
28 Nelson 333.

serving, perhaps in a subtle way, as part of the evidence for recommending HRT. When we understand the scientists in phase one of the case study as using their untested assumptions as evidence, their support for HRT becomes a natural consequence.

The scientists needed to ask themselves those questions regarding causality, and then needed to recognize that their organizing principles, like "HRT corrects some sort of deficiency within women's bodies", lacked critical evaluation. Nelson would not emphasize diversity in the same manner as Longino, except perhaps to note that scientists need to have the ability to suggest alternatives to their organizing principles, and those alternatives are more likely to be suggested by individuals with a markedly different perspective on the scientific question at issue. Ultimately, however, Nelson would argue that any "knowledge" about HRT belongs to the community as a whole; no one individual could have developed radically different views on HRT. Instead, the community as a whole must have gradually moved from accepting HRT to rejecting it.

At first it might appear that Nelson's position cannot account for theory change. Since all knowledge is located within the community, no one individual can hold a scientific position that radically differs from that commonly accepted. However, Nelson does allow for individuals to have slightly different access to the communal web. What is important to note is that an individual cannot have *unique* access to the web. In other words, no one individual is uniquely positioned to "discover" something; all discoveries are seen as natural consequences of the current web formation, and furthermore are seen as consequences that anyone could have proposed. Theory change can occur within Nelson's description of science. What cannot occur are radically new positions, or radical scientific figures. Einstein's theory of relativity must be understood as a

reasonable development from the current theories of his time. Furthermore, Einstein himself is simply the individual who came up with relativity theory first; anyone *could* have developed it.

Longino and Nelson's descriptions of science contain important differences in where the philosophers identify knowledge as being located, yet they also contain striking similarities. Both emphasize the importance of evidence, believing that evidence constrains scientific theories. Both emphasize that science is a communal process; for Longino, scientific knowledge comes about through a process of community evaluation of an individual's theories, whereas for Nelson scientific knowledge comes about through community acceptance of a certain theory or other. But both believe that through describing how science really works, they can identify key ways in which science can protect itself against bias. For Longino, science can improve itself through attention to the membership in the dialogic community - which should be diverse and all members should be valued as potential contributors. For Nelson, science can improve itself through more rigorous attention to and evaluation of all of the evidence supporting a theory.

Longino and Nelson's positions each are subject to different criticisms. Longino's emphasis on community diversity seems at odds with the common notion that science requires consensus. Nelson's position does not seem to allow for those figures commonly viewed as revolutionary to be anything other than "first"; they cannot be understood as epistemicly privileged over any other community member. It might be possible to develop a feminist empiricist position that avoids one or both of these criticisms.

There is, however, one common critique of Longino and Nelson, which seems to attack the very fundamental beliefs underwriting feminist empiricism. Feminist empiricism, in both the forms we have seen and almost certainly more generally, emphasizes the importance of evidence. This position stands in contrast with another popular feminist empiricist position: science is conducted within scientific communities and those communities in some way determine how evidence is evaluated and used. It could be argued that giving scientific communities a great deal of control over the formation or use of evidence is tantamount to advocating relativism. After all, scientists within one community might develop a certain set of standards and might have a certain series of background assumptions, and would, according to the feminist empiricists, develop theories in accordance with those standards and assumptions. A different community might develop different theories, but those theories might be in accordance with *their* standards and assumptions. Feminist empiricism appears to give the scientist no tools for theory comparison between communities; each theory might be "right" for its community. And so feminist empiricism seems to advocate a certain relativism not entirely unlike cultural relativism; the "culture" is simply taken to be that of the scientific community.

At first glance, standpoint theorists such as Sandra Harding appear to advocate a line similar to Nelson's or Longino's – objectivity can be achieved through diversity within the scientific community. Yet standpoint theorists believe that diversity alone is not enough; only diversity with an emphasis on the minority will truly give marginalized lives a voice in scientific inquiry. Through investigating further standpoint theory as

described by one of its leading advocates, Harding, the difference between standpoint theory and feminist epistemology will become clear.

Feminist Standpoint Theory

Sandra Harding directly questions the very notion of "objectivity." She notes four difficulties with defining objectivity. One problem is that the capacity for objectivity has historically been attributed only to particular groups. Statements such as "women (or feminists, Marxists, environmentalists, Blacks, welfare recipients, patients, etc) are more emotional, less impartial" deny that all people are equally capable of being objective.[29] Another problem is that objectivity is frequently attributed to knowledge claims in such a manner as to suggest that objectivity should be considered evidence for the claim in question. Yet it is unclear how exactly objectivity could function as evidence. Additionally, objectivity is attributed to methods or procedures. These procedures are considered to be more objective the more that they maximize some quality (such as standardization or impersonality) associated with fairness or with the likelihood of correcting error. Finally, objectivity is attributed to certain kinds of communities: communities that consciously form themselves (including or excluding members) in an attempt to refine their knowledge seeking.

Harding uses these examples to underscore her point that objectivity is not a single idea, but instead is a term that has been utilized by different groups for their own purposes. She references Peter Novick and Robert Proctor's works on the history of objectivity, repeating their point that objectivity, as well as relativity, has been used at

29 Harding, Sandra. "Strong Objectivity: A Response to the New Objectivity Question" in Janet A. Kourany (ed) *The Gender of Science* (Upper Saddle River, New Jersey: Prentice Hall, 2002), 340.

times "to block social justice and at other times to advance it."[30] Considering the problematic nature of objectivity, Harding questions what sorts of procedures should be used to maximize objectivity. She believes that the current standards for good procedures to maximize objectivity are too weak to identify possible biases that are culture-wide. Harding holds that maximizing objectivity is not the same thing as maximizing neutrality, not in the least because neutrality is another highly contested concept. Instead, she advocates turning to standpoint epistemologies: epistemologies that reject neutrality in favor of investigating diverse minority positions.

To support her position against neutrality, Harding turns to questioning the politics in science. Many scientists acknowledge that politics play a role in science, yet hold that the role that politics plays is an external one; politics might dictate what programs get money or attention, or might specify which scientist gets to take the lead on a certain project. Yet scientists, it is assumed, are supposed to fight against politics within science. Politics as an external force might be viewed as a necessary evil – after all, funding must be chosen in some manner – but politics as an internal force must be avoided. Indeed, science should be depoliticized as much as possible; concepts in science should be reduced to the most politically neutral explanation possible.

Harding rejects the notion that full depoliticization of our sciences is even possible. She points out that in attempting to find "neutral" causes for problems in science, scientists retain many of their biases; they simply design the neutral causes in such a way as to also support their own biases. One example Harding gives of an attempt to depoliticize science is Nazi science: the scientists depoliticized societal problems by reducing them to surgical or medical terms, and it just happened that those terms could be

30 Harding 341.

used to defend the proposition that poverty, for example, was due to some deficiency in the poor. For Harding, politics is not intruding upon a "pure science"; science is already through and through political.

Due to the thoroughly political nature of science, rather than combating biases "neutrality" actually serves to hide them. An ideal of objectivity that attaches itself to neutrality is doomed to be insufficiently powerful to identify or to avoid political or social assumptions within science. Furthermore, objectivity that is wedded to method is doomed to come into play too late, since method can only be applied to scientific justification. Method is used only after "a problem is identified as a scientific one, after central concepts, a hypothesis and research design have already been selected."[31] Harding contends that it is in the context of discovery that biases and assumptions get formally embedded within the scientific process, and methods chosen that fail to evaluate the earlier context of discovery cannot hold any significant claim to objectivity.

Harding proposes standpoint epistemology as a method for achieving a sort of objectivity. Standpoint epistemology rejects the notion that all positions for epistemological inquiry are equal. Instead it proposes that all human thought is limited by its particular historical location.[32] Yet not all human thought is limited in the same way. A key notion in standpoint epistemology is that "marginal lives" are actually epistemologically superior to "ruling lives." This notion comes out of the Marxist notion that, while the master only knows his own life, the slave knows both his life and the life of the master. Marginal lives are those lives that are able to grasp not only the concepts

31 Harding 2002, 344.
32 Harding 2002, 346.

which rule the lives of the ruling class, but that also stand outside those concepts and so are able to recognize them as mere conventions.

Starting off from a marginal standpoint is not supposed to direct research solely to the benefit of that standpoint, but is instead supposed to create research from the marginalized position. Research begins from these positions, but is supposed to explain not only those lives "but also the rest of the micro and macro social order."[33] Yet it is important to note that in practice these standpoints are the positions of individuals. That is to say, standpoint theory does not advocate attempting to generate one "women's standpoint", but instead advocates occupying a variety of possible different individual standpoints.

When considering the HRT case study, Harding would likely point out that a key problem with the scientists' conduct during phase one was that they were not assuming a minority position when formulating their conclusions about HRT. From a male perspective, the marked drop in hormone levels signifying menopause also might appear to signify the onset of some sort of "disease" or "condition". From the perspective of middle-aged individual, the state that the body is in during the "prime" of life is the best state, hormone levels included. Scientists working from these perspectives would assume that any "correctable" alternation in this optimal state *should* be corrected. Assuming the standpoint of an elderly woman, however, the post-menopausal body is a perfectly natural condition, which is not associated with an overall decline in health. Furthermore, assuming the standpoint of a lower-income woman, any regiment of treatment which would require as extensive a program of regular medication as HRT should be critically compared with other possible programs with more limited medication regiments (such as

one which focuses only on hot flashes), or which would not require medication at all (such as programs which focus on receiving estrogen from plants).

In considering possible responses Harding could make to the HRT scenario, a certain level of assumptions about the concerns that an individual in a certain standpoint would have is necessary. Though Harding wishes to deny that standpoint theory fundamentally depends upon stereotypes and generalizations about how "certain people" live (people from marginal standpoints), it is difficult to understand how one could employ standpoint theory without similarly employing such generalizations. One school of feminist philosophy of science that critiques feminist standpoint theory on this charge is feminist postmodernism.

Feminist Postmodernism

One key feminist postmodernist, who criticizes both the feminist standpoint and the feminist empiricist projects, is Donna Haraway. She agrees that objectivity is a highly contested concept, but believes that "feminists have to insist on a better account of the world; it is not enough to show the radical historical contingency and modes of construction for everything."[34] Haraway believes that not only feminism but also Marxism offer powerful tools for critiquing the ideal of objectivity within science. In fact, she recasts the feminist empiricist and the feminist standpoint projects as compatible with Marxist philosophy. Like Marxism, an important aspect of feminist philosophy of science should be the prescription for a better future - for a new science.

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33 Harding 2002, 347.

34 Haraway, Donna. "Situated Knowledges: The Science Question in Feminism and the Privilege of Partial Perspective" in Evelyn Fox Keller and Helen E. Longino (eds.) *Feminism & Science* (Oxford: Oxford University Press, 1999), 251.

Haraway identifies the "problem" facing feminist philosophers of science as that of how they can have an account of the "radical historical contingency" of knowledge claims while at the same time maintaining a commitment to "faithful accounts of a 'real' world."[35] Haraway rejects the notion that feminist philosophy of science is doomed to result in a sort of relativism, or at least she rejects the notion that the relativism it faces is troublesome. She believes that the solution to the problem facing feminist philosophy of science lies in recognizing that feminist objectivity is simply a sort of "situated knowledge."[36]

At first it might appear that Haraway is arguing for standpoint feminism. Yet Haraway is critical of standpoint feminism for two reasons. As mentioned above, she questions how standpoint theorists can "assume" alternate standpoints that are any better than mere caricatures of minority positions. Moreover, Haraway does not believe that "minority" viewpoints necessarily have a better "view" on the biases affecting science. Instead of advocating that individuals working in science assume minority viewpoints, then, Haraway simply recommends that all individuals recognize that objectivity is about limited location and situated knowledge for *everyone*. Individuals cannot transcend their problematic relationships with their objects of study to assume a god's vantage point. Instead, everyone must "become answerable for how we learn to see."[37]

Haraway believes that feminist accounts of objectivity within science require "a deceptively simple manoeuvre".[38] The object of study must be regarded just as much of an agent as the scientist; in other words, the object can change and affect the scientist just

35 Haraway 252.
36 Haraway 253.
37 Haraway 254.
38 Haraway 259.

as much as she can affect it. The world, for Haraway, is not a passive object waiting to be "decoded", and therefore scientists must recognize that they are involved in a conversation of sorts with their subject matter. This does not mean that relativism rules; Haraway is firm on her stance that a simple relativism will not help one to sort out the complex interaction between the scientist and the world. Instead, acknowledging the agency of the world means recognizing that the world can produce drastically different results than we might expect, and realizing the imposition of scientific laws or rules has no effect on how the world will conduct itself. Ultimately, then, Haraway is arguing for a notion of objectivity which would strip it of its permanence and which resists crowning one particular group as having privileged access to it.

It is difficult at first to understand the practical applications of Haraway's post-modernism. Perhaps she would argue that the HRT researchers in the first phase were assuming some sort of control over the natural world, or were assuming that there was a kind of "error" within the natural course of women's bodies that science could correct. Haraway could cast the scientists' discovery of their error in assuming that HRT would be beneficial as also a discovery of their own hubris, hopefully resulting in the realization by the scientists that their interactions with the world are far more complicated than a mere agent/object relationship.

Each of the feminist philosophers of science proposes distinct philosophies, and therefore each philosopher would both describe and proscribe scientific activity differently. However a common treatment of objectivity can be found within each of the feminist philosophers' works, although since some treat objectivity more directly than others it takes a bit of work to bring out this common treatment. Therefore, having

briefly outlined each philosopher's main tenets in this chapter and having focused mainly on the distinctions between the philosophers, in the next chapter I consider the similarities in their treatments of objectivity. After considering several attempts by leading feminist philosophers of science both to define how objectivity has been used within philosophy of science and to reject those treatments, I work to show how all of the feminist philosophers of science treated rely upon a common foundation for their inferences about objectivity. Once this foundation has been unearthed, I use the remainder of the chapter to investigate the potential philosophical connections between this foundation and the work of the positivists and post-positivists.

Chapter Two:

Objectivity

In the previous chapter, I explore each of the feminist philosophers' programs individually in order to give the reader a rudimentary understanding of some of the diverse philosophies captured by the label "feminist philosophy". Though each philosopher has different descriptions of science, conclusions about how "bad science" occurs, and prescriptions for how science should conduct itself, running through these diverse programs is a common treatment of the concept of objectivity.

Traditionally, objectivity has been contrasted with subjectivity. Whereas the label of "subjective" has been attached to individual, or possibly even group, interpretations of the world, the term "objective" indicates an absolute, perspective-neutral, view of the world. An objective statement is the "fact of the matter", whether or not it is or can ever be recognized as such. Under this definition of objectivity, a statement or hypothesis is more objective the more it corresponds to the real state of affairs in the world – regardless of whether or not that state of affairs can ever be directly accessed. In other words, we might not ever be absolutely certain about the connection between HRT and heart disease, but there is a fact of the matter concerning that connection, and that fact is the objective truth.

Sandra Harding and Helen Longino both provide philosophers with an analysis of how objectivity has historically been used within philosophy of science. According to Longino, objectivity has been treated in two key ways: as a part of a product account of science, and as a part of a process account of science. Philosophers of science who hold

to a product view of the objectivity of science believe that science is objective because it, at its best, presents an accurate view of the world. The hypotheses about the world that science gives us "corresponds accurately" to the way things really are.[39] Implicit in the product view of science is the avocation of scientific realism – that the entities (atoms, DNA, etc) which science purports to have discovered really exist. Also consistent with a product view of science is the intuition that an individual scientist can be objective – that an individual scientist can uncover, on his or her own, truths about the world.

The process view, on the other hand, holds that science is objective in the sense that it is an objective mode of inquiry. For the process view, it matters far less whether or not the facts of science correspond to the facts of the world (in fact, many advocators of the process view of science might argue that one can never know to what degree the two sets of facts are in alignment). Instead, what matters as far as objectivity is concerned is the extent that the particular processes that make up the scientific enterprise have, as a part of their most fundamental makeup, a share in moving scientific claims from the realm of the subjective to a more objective state.

If adhering to the product view of science, one would seek to increase the objectivity of science through attention to its findings – to the degree to which those findings correspond to the real world. However, if advocating the process view of science, one would attend to the process of scientific inquiry, increasing the overall objectivity of science by making an effort to ensure that the processes within science help to move individual scientific endeavors from the subjective realm towards the objective.

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[39] Longino, Helen. *Science as Social Knowledge*. (Princeton: Princeton University Press, 1990) 62.

Objectivity, in its various formulations, has played an important role within philosophy of science as an anchor for the concept of truth. There are two main ways in which objectivity has come to bear in philosophy of science. In product accounts of science, objectivity it the ultimate goal because it gives the scientist the facts of the matter. The positivist notion that science is a rational activity which produces truths about the world falls in line with such a product view of science. In process accounts of science, objectivity is not a final state, but rather is a desired, but impossible to reach, way of conducting oneself. Without objectivity as a goal for the processes by which science is carried out, science would lapse into the realm of intuitions and opinions. It is the use of more and more objective methodology which strips the subjective elements from the scientific process, and which yields findings more likely to stand up to repeated examination. To return to an earlier discussion, the scientific community refines its notion of what processes are more or less objective, and it is from this understanding of the level of objectivity present in a discovery that the community establishes the warrant for a claim. This warrant is not dependant upon acceptance; the scientific community might have accepted the hypothesis that HRT helps prevent heart disease, but their acceptance did not render the hypothesis true, nor did it even render the hypothesis more likely to be true. Only warrant can render a hypothesis more likely, and that warrant can only be established through the use of objective methods.

Philosophers who accept the above description of objectivity can describe what went wrong in the HRT case study in their own terms. Some philosophers of science such as Susan Haack might argue that the HRT case study shows scientists acting without objectivity – the scientists accepted claims that didn't have enough warrant because they

wanted to accept them, and did not evaluate the objectivity of their methodology. According to such a diagnosis, the future progression of science depends upon the elimination of bias and of a conflation of acceptance with warrant; in short, the progression of science depends not upon redefining objectivity to acknowledge elements of subjectivity, but instead upon maintaining a rigorous account of objectivity which continues to attach it to neutrality and rationality.

The feminist philosophers of science offer an alternate prescription for scientific progress: progress can be achieved through the rejection of the traditional notion of objectivity as a neutral perspective to which scientists are supposed to aspire. Although each feminist philosopher builds up her own very different program, the similar treatment of objectivity within the programs exists as a common foundation for feminist philosophy of science as a whole. Furthermore, the feminist philosophers' treatment of objectivity is a natural progression from earlier treatments by the philosopher Karl Popper and the post-positivist philosopher Thomas Kuhn. The feminist philosophers of science are not exploring some radically different subject matter, nor are they located in some position isolated from the remainder of philosophy of science; in their intellectual history are the same major figures who factor into the history of the rest of philosophy of science. However, the feminist philosophers of science are not simply reiterating Kuhn and Popper; they move beyond the work of those earlier philosophers to give radically prescriptive accounts of how science should conduct itself, and it is because of those prescriptive accounts that the feminist movement in philosophy of science should be regarded as an interesting and vital movement within philosophy of science as a whole.

This chapter's focus on objectivity allows it to address an issue that provides the foundation for a great deal of feminist philosophy of science, as well as philosophy of science in general. Objectivity is not the only concept commonly treated by the feminist philosophers of science and earlier philosophical movements, but it is from worries about objectivity that many feminist philosophers of science begin their work, and so an established connection between treatments of objectivity runs to the very heart of the rest of the various programs the feminist philosophers are advocating.

Objectivity within Feminist Philosophy of Science

Some feminist philosophers of science deal with objectivity very openly, whereas others consider it only indirectly or by way of other concepts like knowledge. But all of the feminist philosophers of science considered earlier do deal with objectivity, most likely because it is such an essential concept to the development of a program within philosophy of science. Beginning with the feminist philosophers of science like Harding and Haraway, this chapter expands upon their treatments with a consideration of Longino and Nelson, in order to reach an understanding about the common treatment of objectivity within feminist philosophy of science.

Sandra Harding uses her worry about objectivity as the impetus for her work in feminist standpoint theory. She rejects the idea that objectivity is a simple, absolute, or stagnant concept, and instead advocates understanding objectivity as a changing notion connected with our ideas about neutrality and, if it can be considered a single concept at all, as an ideal that is never achieved. In explanation for the source of her doubts about objectivity as an absolute concept, Harding cites the work of Peter Novick and Robert Proctor, who both have done work in mapping the collection of meanings associated with

objectivity. According to Harding both Novick and Proctor, "point out that asserting objectivity sometimes has been used to advance and sometimes to retard the growth of knowledge, and the same can be said for assertions of relativism."[40] In other words, both relativism and objectivity are, to Harding, labels that various figures have used to marginalize or to normalize particular theories.

Harding's worry about objectivity goes beyond a fear that, historically, individuals have designated a particular individual or experiment as "objective" for political reasons to a concern that objectivity can never remove itself from the political - that it will always necessarily be a sort of blue ribbon awarded by the group currently dominating the intellectual world. At the time when scientists were debating whether or not to advise women to take HRT, for example, it was up to the scientific community to decide whether or not the observational data on HRT was sufficiently objective to be valid. The actions of the doctors who prescribed HRT on the basis of that data collection suggest that the doctors did, in fact, believe the observational data to be sufficiently objective. Thus one hypothesis is privileged over another competing hypothesis – in this case, that HRT is more damaging to women's health than beneficial – through the assignment of objectivity to its supporting research.

Beginning with this worry about objectivity being political, Harding uses analysis from "feminist, postcolonial, environmental, and other movements for social justice" to consider the implications of an ideal in science which is so essentially political.[41] Such an ideal doubtlessly would serve as a powerful tool for silencing or marginalizing groups, opinions, or sciences that are unpopular. But Harding does not wish to reject the concept

[40] Harding 2002, 341.
[41] Ibid.

of objectivity entirely. Instead she retains some notion of objectivity as an ideal to be strived for within science; her objectivity is renamed "strong objectivity", and is the result of the use of standpoint epistemologies.

Though Harding does not specifically acknowledge the exact nature of "objectivity" within her strong objectivity, it appears to have at its heart the idea that something is objectively true if it would be considered true from a variety of different perspectives. When maximizing objectivity means maximizing the chance that a given individual, from any of a number of backgrounds, will consider a claim to be true, it is unsurprising that the resultant philosophy emphasizes the importance of considering minority viewpoints. Ultimately for Harding what matters for any scientists studying HRT is that they attempt to maximize their objectivity through considering how minorities – in terms of age, class, and race – might study the problem. Far from locating themselves within some absolute, neutral perspective, scientists achieve objectivity by first assuming a number of subjective positions, and then recognizing the truths common to all of those positions.

Other feminist philosophers begin from a similar worry about objectivity to Harding's and come to a dramatically different prescription for science. Like Harding, Donna Haraway acknowledges that objectivity is a highly contested concept, and that the rhetoric of science has used it to establish scientific hypotheses as the truths about the world. And also like Harding, Haraway retains an insistence upon objectivity as "some enforceable, reliable accounts of things not reducible to power moves..."[42] Yet Haraway believes that "only partial perspective promises objective vision."[43] Far from maximizing

[42] Haraway 252.
[43] Haraway 254.

objectivity through maximizing the number of perspectives involved, Haraway holds that if objectivity is ever maximized, it is through the identification of the factors that have limited that objectivity. In other words, rather than pursue minority standpoints to maximize objectivity, Haraway would see scientists focus on their own positions, and on understanding the perspective every scientist is located within as being just that – a perspective. The HRT researcher shouldn't attempt to put herself into a minority perspective; instead she should attempt to recognize her relationship with her material as a dynamic one full of potential biases.

Common to the treatments of objectivity within both Harding and Haraway is the identification of objectivity as a highly problematic, yet still desirable, ideal. Similarly, though less directly, the feminist empiricists hold that some version of objectivity is essential to science. They too believe that objectivity is a problematic notion, but rather than discuss objectivity directly the feminist empiricists focus on questions related to objectivity – what counts as good evidence or good reasoning.

Longino, for example, worries about the status of rationality. She believes that reasoning is a social activity; that is to say, an individual can engage in a limited form of reasoning – calculation – but cannot engage in reasoning without some community standards regarding how information can be combined and brought to bear on judgments. Longino breaks down reason into two parts – constructive reasoning and justificatory reasoning. The first can be engaged in by individuals – individuals can combine ideas to produce new ideas. The community might influence how ideas are combined, or to what ends, but the actual combining could be done without a community presence. This constructive reasoning does not need an appeal to objectivity to be successful; it is the

equivalent of the "logic of discovery", and as such need not meet community standards. Justificatory reasoning, however, requires a community. It is reasoning which can stand up to challenges and responses, and therefore it is reasoning which must meet community standards. As Longino puts it "what counts as an appropriate consideration, as a reason, is determined and stabilized through discursive interactions."[44] Justification is a practice of meeting objections, and as such justification relies heavily upon community assumptions for its status. The ultimate status for justificatory reasoning is that it has reached objective conclusion, but judgments about the objective nature of a claim are, as well, the product of social interactions.

The objectivity of a claim is critically tied to the productivity level of the community that has produced that claim. The more productive a community is, the more likely it is to transform the scientific inquiries within it. In other words, more productive communities have more internal interaction and the beliefs generated by the community are shaped by the entire community, rather than by a subset. For example, if the doctors and researchers who were making decisions regarding the relationship between women's postmenopausal health and HRT were engaging in a lively and spirited debate within which all perspectives, regardless of credentials, were considered, then that scientific community was productive. However, history suggests that a lack of productivity, of critical examination from diverse perspectives, rendered the scientific community too biased to recognize that their data supporting HRT was sketchy at best. Diversity, for Longino, becomes tied with objectivity because diversity increases community

[44] Longino, Helen. *The Fate Of Knowledge* (Princeton: Princeton University Press, 2002), 104.

productivity, yet the diversity she requires is a diversity that would increase community discourse and debate.

Within Longino's philosophy is the same rejection of objectivity as an absolute ideal that exists within Harding and Haraway. Longino might not focus on objectivity per se, but her concerns regarding the state of rationality are underpinned by the notion that rationality is subjective, and subsequently there is no place within her philosophy for an objectivity which can be rationally attained by an individual in isolation.

Similarly, Nelson does not directly address objectivity, yet her concerns about the nature of evidence can be seen as critically linked to other feminists' worries about the status of objectivity. Nelson identifies the key source for bias within science as being the inappropriate acceptance of certain evidence for theories – like the acceptance of biases about the nature of menopause as evidence for HRT. Yet the identification of which pieces of evidence count as good evidence and which are mere biases can be difficult when researchers fail to identify all of the evidence contributing, however subtly, to the support of a particular theory. Furthermore, if evidence about the world as a whole is formed into a web of belief which we cannot stand outside off, then no "objective" position exists from which some evidence can be judged "better" than others. Instead, every individual must view scientific practices from their particular perspective, and must evaluate the soundness of evidence while recognizing that the evaluation is occurring from a partial perspective.

Cashing out Nelson in this way, her concerns about objectivity are quite similar to the concerns of Haraway, and like Haraway at the root of her philosophy is the seemingly contrary position that "objectivity as absolute" does not exist, and yet objectivity as a

concept should not be abandoned entirely. Indeed, this complex treatment of objectivity is common to all of the feminist philosophers studied, and contains commitments both to the social nature of knowledge and to the idea that there can be some science that is, in some sense, better than other science.

Thus far I have shown how the feminist philosophers of science reject the notion that one individual can achieve an "objective" perspective, if by objective we mean absolute or neutral. Yet this rejection stands in a complicated relationship with another common move in feminist philosophy of science – the argument that objectivity is an ideal that should be attempted. The feminists resolve this seeming contradiction in their philosophy by firmly locating objectivity as an ideal, and by arguing that contrary to our intuitions, objectivity is increased through interactions with subjectivity. In other words, although objectivity is the goal, it is striven after through the assumption and acknowledgement of one or many subjective positions.

The above description of the feminist philosophers of science might be taken to present the feminist philosophers of science as a purely reactive movement that simply defines itself and its concepts through criticism and rejection of previous movements. Indeed, much of their philosophy could be read as a reaction against other movements in philosophy of science. However, such a reading disregards the extent to which the concept of objectivity within feminist philosophy of science, like feminist philosophy of science as a whole, is the product of a gradual evolution of concepts and ideas from previous movements such as positivism and post-positivism. The feminist philosophers are not simply rejecting their predecessors or co-opting their terms while misunderstanding the meanings behind those terms, but instead have assumed their

inheritance from previous philosophers and have carefully evaluated the merit and implications of that inheritance. Furthermore, the feminist philosophers of science move beyond their positivist and post-positivist roots to not simply describe the state of objectivity within science, but to proscribe how scientists should treat objectivity in order to further the progress of science as a whole.

Before evaluating that final move of the feminist philosophers, and the philosophical consequences therein, it is important to investigate precisely what their inheritance from Popper and Kuhn entails as well as how the feminist philosophers of science evaluate that inheritance. Only after the feminists' initial decisions regarding objectivity have been recognized as firmly rooted in the same concerns as some of the founding fathers of the philosophy of science can it be determined whether the feminists third move is radical and divergent enough to divorce them from philosophy of science as a whole, or whether the final move of the feminists concerning objectivity actually places them at the forefront of philosophy of science as a useful and promising movement.

Popper and Objectivity

Sir Karl Popper rejected what he saw as a tradition in philosophy, dating from Descartes, of viewing knowledge as subjective. He believed that previous philosophers were mistaken in locating all knowledge as existing within a particular knower, and instead proposed that subjective knowledge – knowledge available only to one knower – can become objective knowledge. Popper's division between objective and subjective knowledge is both intimately related and rather foreign to the common use of those terms today.

Popper demarcates between subjective and objective knowledge by examining whether or not any individual needs to be included in the epistemic story. For Popper, subjective knowledge is knowledge that is inextricably tied to a particular knower; it is knowledge that resides within a knowing subject. Objective knowledge, on the other hand, is knowledge that consists of "linguistically formulated expectations submitted to critical discussion".[45] Objective knowledge is subjective knowledge that has been formalized in some way, and in the process no longer is contained within a particular knower. Whereas subjective knowledge cannot be subjected to criticism or debate, objective knowledge is knowledge that can grow and change as a result of the subjection of that knowledge to rational scrutiny.

Popper's demarcation between objective and subjective knowledge can still be seen as falling within modern epistemology, yet Popper does not link "objective" necessarily with "true" or "absolute". Instead, objective knowledge is preferred over subjective knowledge simply because it can potentially be criticized; objective knowledge is not necessarily true, but objectifying knowledge and removing it from the subjective gives that knowledge more truth-potential. The objective knowledge *might* be true knowledge, but that is not what recommends it. In the game of science, where the ultimate goal is obtaining theories closer to the truth, knowledge that can be subjected to criticism will certainly be more useful than knowledge that is permanently locked within one knower, and it is for that reason at Popper prefers objective knowledge claims.

According to the Popperian model, the human endeavor is to understand the world, science is the field that best points us to the more truth-like theories, and objective

[45] Popper, Karl, *Objective Knowledge: An Evolutionary Approach* (Oxford: Oxford University Press, 1972), 66.

statements are the components from which theories are made. So, in a roundabout way, the human endeavor is made up of pursuing objective statements. Yet not every potential objective statement can be investigated. Instead, we make observations and formalize our intuitions as a result of some need. Something causes us to notice everything from the leaves changing color to the interaction (or lack thereof) between oil and water. Even simple observations like the above examples had to be made, and they were made because they in some way impacted human lives. When an individual's observational data leads her to believe a certain result should occur, or when she doesn't have enough information to hypothesize what result should occur, then she faces a problem. Say, for example, that she observes that her plants perk up when given water, and the proceeds to soak them every day. When the plants start to die, she suddenly is faced with a problem with her observation that plants respond well to watering. After she has made her observation objective, changing from something like "I saw that the plants improved after watering" to something like "watering always leads to better plants," it is science, Popper would argue, that best gives her the tools she needs to analyze the truth-likeness of her objective statement.

Since scientists are "problem-solvers," they do not stand in a completely neutral relationship with the world they are investigating. They are not seeking all truths, but the truths they need to solve some problem in their lives. The process of science might have a certain degree of neutrality to it, but the questions the scientists are employing science to answer are anything but neutral; they are not only very specifically designed around what the scientists believe they need to know about the world, but also are created in an active relationship with the information the scientist already believes she knows. To

return to the simple example, the woman investigating why her plants are dying would question certain objective statements, like "watering always leads to better plants", before others, like "what comes out of my faucet is water."

The scientist forms a hypothesis in response to the problem, and runs an experiment to investigate whether or not that hypothesis has any merit. Yet if the experiment produces results validating the hypothesis, those positive results do not mean the hypothesis is true. It simply means that it is more likely that the hypothesis is true than that it is false. Popper argues that the scientist can shore up the truth-likeness of her hypothesis not through pursuing additional supporting data, but instead through attempting to prove her hypothesis wrong. If the hypothesis withstands the various tests she devises to falsify it, then it improves its truth-like standing.

It is the process of falsification that Popper most intimately connects with the scientific process. While others before him sought to prove that it is induction, or better yet some "inductive method", which gives science is credibility and enables it to pick out truth-like hypotheses, Popper believes that it is through engaging in attempts at falsification that science has increased our understanding of the world (or, at least, our understanding of what the world is not).

Indeed, Popper identifies falsification as the key factor that separated science from pseudo-science, and consequently believes that understanding the role falsification does and should play within science is the best avenue for philosophers to establish scientific knowledge as privileged. Since it is surviving attempts at falsification that improve a hypothesis's truth-status, rather than the overall collection of supporting evidence, theories that cannot be falsified are unscientific. Furthermore, fields made up

of theories that cannot be falsified are, as a whole field, unscientific or pseudo-scientific. So astrology is unscientific, not because it has been proven wrong but in fact because of the opposite – astrology makes predictions such that no experiment can be designed which would definitely disprove the field, or theses within the field. The more ways in which a theory's predictions have been tested, the more scientific that theory becomes. At no point, however, is the theory accepted as true; it is simply more scientific and, possibly, more likely to be true.

However, judging whether or not falsification has occurred is not a simple process, as Popper acknowledges. A single instance of getting damaging results does not necessarily mean the theory is to blame; it is possible that the scientist miscalculated, for example, or that the experimental findings differed from those predicted because of a flaw in some *other* theory involved in the experiment. Furthermore, Popper recognizes that scientific theories have historically often been retained in the face of not just one, but a number of apparently falsifying pieces of evidence. Falsification is not, then, an entirely uncomplicated or free from subjective elements. It does not lead to necessary acceptances or rejections of theories; rather, it indicates which theories are more or less truth-like.

Overall, science within the Popperian picture does not produce truths which are absolute, or even which might be labeled by a modern audience as "objective". Instead, it produces truth-like statements which exist independently of a particular knower, and which are strengthened through their subjugation to further criticism and attempts at falsification. Furthermore, each truth-like statement is simply one of many which could have been investigated; it has been formulated in a particular manner as a result of a

variety of factors including human interests and the background knowledge of its author. Science is not objective in the sense of asking unbiased questions or of making neutral observations. Indeed, according to Popper, "*we approach everything in the light of a preconceived theory*…As a consequence one is liable to pick out those things which one either likes or dislikes or which one wants for other reasons to find."[46] Science is objective, but it is objective because it deals with objective statements that are subject to criticism and that one could attempt to falsify. The objectivity within science is purely a product of the sort of knowledge with which it interacts – falsifiable knowledge that has been formalized and isolated from an individual knower. Though his notion of objectivity within science may not grant scientific claims the sense of surety or correctness which some might desire, Popper retains the notion that scientific claims are an improvement over non-scientific claims – after all, they are more "truth-like."

Kuhn and Objectivity

Thomas Kuhn has been considered by many to argue directly against Karl Popper. Though Kuhn sees himself as merely reinterpreting much of what Popper says – as seeing a rabbit where Popper saw a duck – Popper adamantly opposes Kuhn's "interpretations" of his work. Before addressing possible similarities between their treatments of science, and notably of objectivity within science, I consider Kuhn's work independently of Popper's.

At the center of Kuhn's philosophy of science is a distinction between normal science and revolutionary science. Normal science is puzzle-solving science. Scientists engaging in normal science are working within a particular paradigm made up of

[46] Popper, Karl, "Normal Science and its Dangers" in Imre Lakatos and Alan Musgrave (eds.) *Criticism and the Growth of Knowledge*, (New York : Cambridge University Press, 1974), 52.

accepted methodologies, goals, and theories that are assumed to be correct. Normal scientists do not question the framework of the paradigm to which they are members; if a normal scientist was working within a paradigm that utilized Newtonian theories to explain interactions in physics and obtained a particularly discrediting observation (say, of the perihelion of Mercury), then the normal scientist would develop hypotheses and investigate them while remaining within her paradigm. She would not question Newtonian mechanics; instead, she would work to develop a theory that is compatible both with her paradigm and with her observation. If she fails in her attempt, she would discredit the observation before she would discredit any part of her paradigm.

After a good number of abnormalities that challenge the dominant paradigm have built up, revolutionary science can occur. Revolutionary scientists are scientists who have found themselves rejecting some fundamental assumption, practice, or other grounding the principle within the paradigm of which they were members. These scientists, after standing in limbo outside of any paradigm, also work to form a new paradigm. If this new paradigm should be established – such as a paradigm that hold the laws of Einstein, rather than those of Newton, central – then the revolutionary scientists locate themselves within that new paradigm and become normal scientists again. And so now two paradigms existed where before there was only one; through having a sort of conversion experience a scientist can flip from being a member of one paradigm to another, but this paradigm shift does not happen as a result of logic or of the careful consideration of the merits of each of the two paradigms. Since the normal scientist is always firmly located within one paradigm, she cannot be convinced through logic to change her position nor can she stand in some separate position and consider her options.

Somehow, she simply "converts" from one paradigm to the other, and although she might develop a series of arguments as to why her new paradigm is better, those arguments are arguments which appeal only to those also located within the new paradigm. In other words, her arguments could not have been the reason she actually changed paradigms, nor could they convince anyone from her old paradigm to follow her to the new paradigm. Rationality may exist, and a rational explanation for her shift could be developed, but only relative to the perspective of one similarly located within a paradigm.

Kuhn does seem to use a number of Popper's ideas about science in the development of his paradigm system, yet there is enough relativism within it to make Popper's rejection of Kuhn understandable. Kuhn begins with the notion that scientists do challenge their beliefs, and do work on puzzles (or, perhaps rephrased as Popperian "problems"). Yet Kuhn not only recognizes that scientists frequently retain theories in the face of falsifying evidence, he makes that observation the basis for his philosophy. Scientists might engage in falsification, but they are not attempting to falsify everything, and many theories must be accepted as true in order for one theory to be isolated and tested. If all theories were "up for grabs", so to speak, Kuhn would contend that no one theory could ever be identified as the false one. Scientists need a structure within which they can work, and furthermore, since science is done by scientists working together and challenging each other, the structure within which one scientist does her work must be held in common by the other scientists with whom she is interacting.

All of the above might have been accepted by Popper if it weren't for two implications: that scientists are ultimately irrational in their acceptance of their paradigms, and that the majority of scientists are puzzle-solvers who exist with blinders

to how their work might challenge the dominant theories in their respective field. For all his acknowledgement of the somewhat capricious nature of identifying one theory as falsified, Popper still wishes to argue that science is a rational activity. Furthermore, although Popper might accept that scientists do develop their new theories within the context of certain background theories, he wishes to reject that those background theories must remain in the background, unquestioned by most scientists.

The Kuhnian picture of science might have a great deal of similarities with the Popperian picture, at least as far as the initial observations about science go, yet it can be and has been argued that they are worlds apart. It remains to be investigated in this thesis whether Popper and Kuhn are as divided on the concept of objectivity as they are on rational status of scientists. Kuhn does not explicitly lay out the objective/subjective distinction as Popper does; certainly, though, Kuhn does hold that an essential part of normal science is the interaction between scientists of a given paradigm, and in order to interact one can assume that their intuitions must have been formalized somehow into theories that can be criticized. Kuhn also explicitly criticizes the notion that sensory experience is, or possibly could ever be, objective in some absolute sense. Scientists do not collect "the given" results of experience, "but rather, the 'collected with difficulty.'" A scientist collects the data that she collects in large part because she is looking for it; she is focused on one particular outcome of an experiment, and so her research is automatically biased towards that outcome.

Kuhn's philosophy retains the notion from Popper that the knowledge of science must be open to criticism, yet he also points out that the "objective" knowledge – which is open to criticism – is purely a product of the paradigm within which is has been

developed; it has been formulated as a result of data collected for a certain purpose and with a certain intention, and it can only be properly criticized by other individuals located within the paradigm. Furthermore, should the theory attain overall acceptance with the community at the level of paradigm acceptance, regardless of how "objectively" it has been phrased it will not face rational criticism: individuals within the paradigm would not think to criticize it, and individuals external to the paradigm could not develop arguments against the theory which would be convincing to anyone within the paradigm.

Kuhn acknowledges that "the epistemological viewpoint that has most often guided Western philosophy for three centuries dictates" that sensory experience be judged as fixed and neutral. Western philosophy has desired an objectivity which is absolute and permanent, and although Kuhn is sympathetic to that desire he believes that sort of conception of objectivity "no longer functions effectively, and the attempts to make it do so through the introduction of a neutral language of observations now seem to me hopeless."[47] Objectivity is, in many ways, now subjectivity. The formalization of an intuition and the exposure of that now objective statement to criticism might exist, but only within a paradigm which has been accepted on non-rational terms. The objective statements must be located within their respective paradigms in order to be understood, and so Kuhn has taken objectivity as Popper conceived of it, and although he has allowed that statement to exist independently of a particular knower, he has tied it inextricably to a community of knowers.

Having examined earlier treatments of objectivity within philosophy of science, it is clear that there are a number of points of similarity between the feminist philosophers'

[47] Kuhn, Thomas. *The Structure of Scientific Revolutions*, (Chicago: University of Chicago Press, 1970), 126.

treatments of objectivity and earlier treatments of objectivity. Once these points have been considered, a new definition of objectivity for the feminist philosophers of science can be composed which accounts for the positive aspects of their program, instead of focusing exclusively on the negative.

Objectivity – A Positive Definition

Feminist philosophers of science follow Popper in intimately connecting "objective" with "able to be criticized". Though the feminists do not label particular knowledge claims as objective as Popper does, they do express the sentiment that a statement becomes more objective the more criticism it is exposed to. However, the feminist philosophers of science go one step further than Popper: a statement is not objective simply if it can be criticized, but instead it becomes more objective as it is exposed to and survives more criticism. Furthermore, the feminist philosophers of science recognize that divorcing objectivity from neutrality does not eradicate the potential for politicizing objectivity; it prevents those seeking to use the term "objective" to legitimate certain scientific claims from extolling the neutrality of those claims, but it leaves open the question of what counts as legitimate criticism, and of who should be valued as a potential contributor to the critical community. Some critics will be recognized while others are marginalized, and so instead of certain individuals being labeled as more "neutral" they will be labeled as more "knowledgeable" about the topic under discussion. Having pointed out that trading "neutral" for "able to be criticized" does not remove the political elements of objectivity, the feminist philosophers each develop a unique prescription for how science should best address its inherit political elements.

The feminist philosophers of science work from a very similar starting point as Popper, as well as from similar concerns. Like Popper, the feminist philosophers hold that scientific statements must be open to criticism, and one of the major goals of scientists is to pursue falsification – perhaps rephrased as "to subject statements to criticism." However, the feminist philosophers of science question one key element of Popper's philosophy, and their question could be phrased several ways: how is one individual capable of pursuing falsification? How can one person recognize his or her own biases? And finally, how can science as a whole best ensure that its hypotheses are falsifiable and criticizable? While this line of questioning is largely alien to Popper's philosophy, the feminist philosophers and their philosophy can still be viewed as growing out of Popper and Popperian views about science, rather than as existing in complete isolation from earlier philosophy of science.

Popper is not the only major philosopher of science to whom the feminist philosophers relate. Many of the ideas of the feminist philosophers of science regarding the community can be understood as generated by the same concerns that trouble Kuhn. Like Kuhn, the feminist philosophers of science recognize that science is done within communities (or paradigms) with accepted standards, methodologies, and goals. It does not make sense to talk about one individual "doing science;" instead we must regard the fact that scientists are community members as a fundamental fact of their existence as scientists. According to Kuhn scientists cannot be disembodied, or thought of in the abstract. Following this line of reasoning, any objective statement is always going to be subjective in so far as it can only be judged within a particular community. However, the feminist philosophers of science do not follow Kuhn's particular merging of objectivity

with subjectivity; instead, they entangle the two in a slightly different way. Rather than describing science as containing multiple paradigms existing at the same time, the feminist philosophers of science typically address the scientific community as just that – a single community. This difference in numbers of communities impacts the sort of relativism within the resultant philosophy, and as a result the relativism that the feminist philosophers of science describe as existing within science is very different from Kuhn's relativism.

For Kuhn, when a statement's objectivity is relativized to a particular paradigm, it is relativized because the statement only makes sense within that paradigm. However, for the feminist philosophers of science, working with a single scientific community, the statement is relativized because it is only as objective as the criticism to which it has been exposed, and therefore its potential for having its objectivity increased depends entirely on the propensity of the community to generate criticism or to recognize biases. A statement could be understood as objective relative to how diverse or how dialogic the scientific community is on the particular issue that the statement addresses.

The feminist philosophers of science are not reactionaries who wholeheartedly reject all previous philosophy of science and who cling to some absolute relativism; instead, they are descendents of Popper and Kuhn who have evaluated and selectively retained aspects of those earlier philosophers works. The feminist philosophers share a propensity for connecting objectivity with criticism, for considering the community to be an essential part of science, and for elements of relativism with Popper and Kuhn. Where the feminist philosophers could be understood as diverging from those earlier

philosophers is in their line of questioning, in sorts of questions and issues feminism brings to the intellectual forefront.

Rather than defining objectivity within feminist philosophy of science negatively, as not absolute, for example, I am now in a position to define it positively. There are three main components to objectivity within feminist philosophy of science. The first, and most primary, is that the objectivity of a statement is a function of the degree of investigation and criticism to which a statement has been subjected. The second component is the investigation into and the pursuit of a theoretically "ideally" objective statement – a statement that has been subjected to all possible criticism. This ideal provides the feminist philosophers with a goal for objectivity, as well as for science, and in turn that goal becomes the third main component of their understanding of objectivity. Science should be made up of the most objective statements possible, and therefore science should work to increase the potential for criticism or for the recognition of biases within the scientific community.

It is within this final component of feminist philosophers' treatment of objectivity, setting up objectivity as a goal, that their feminist concerns fully emerge. Certainly within the prescriptive elements of feminist philosophy of science feminism's focus on the importance of diversity, on the marginalization of minority voices, and on the degree to which gender, race, and class affect our views, understandings, and concerns about the world can be seen, but awareness of feminist concerns also can be seen as coloring the line of questioning and criticism to which the feminist philosophers subject Kuhn and Popper.

Yet while the "feminist" in "feminist philosophers of science" can be viewed as a pointer to how that group of philosophers expands upon previous philosophy, and it should not be understood as a complete description of the entirety of their project. Considering only one of the concepts feminist philosophers draw upon – objectivity – feminist philosophy of science emerges as an expanding and growing branch of philosophy of science, yet its intimate ties with earlier philosophy of science also emerge. The marginalization of feminist philosophy of science, the dismissal of the feminist philosophers wholesale, or the decision to consider their work only as fringe movement must be generated from concerns about the feminist philosophers' expansions upon Kuhn and Popper, and a defense of the feminist philosophers of science as potentially quite fruitful must also focus on the novel aspects of their philosophy.

Chapter Three

A Defense of Feminist Philosophy of Science

The previous chapter presents the feminist philosophers of science as philosophers, following in the tradition of Kuhn and Popper, who have brought feminist concerns to the field of philosophy of science. This presentation stands in stark contrast with that of Susan Haack, who considers the feminist philosophers of science to be just one contingent within a relativist movement seeking to undermine the status of science in order to achieve political goals. After showing that Susan Haack's description of the feminist philosophers of science is at best based on a superficial understanding of the feminist philosophers and at worst a complete misreading of their work, I shall then assert that the feminist philosophers of science can and should be viewed as philosophers who could be of great worth to the field of philosophy of science. They are not feminists seeking to use philosophy to further their aims as feminists, but philosophers who use feminist tools to further their philosophical understandings of science and human knowledge. Only once this reading of the feminist philosophers of science has been accepted can the great potential value of their work be recognized, and can philosophy of science as a whole truly engage all its available resources.

A Return to Susan Haack

In the introduction to this thesis, I consider the description Susan Haack gives of the feminist philosophers of science. These philosophers belong to the "New Cynics," a mixed bag of Afrocentrists, feminists, relativists, and members of several other movements who are collected under one name due to, as Haack describes it, a common

assertion that "not only does science have no particular epistemic authority and no uniquely rational method, it is really, like all purported 'inquiry,' just politics."[48] Furthermore, these New Cynics do not approach philosophy of science from the perspective of a philosopher of science, but instead are grounded in "sociology, anthropology, literary theory, or feminist philosophy."[49] In other words, their philosophical efforts come out of their desires to further their political, or perhaps ideological, goals. As Haack presents the New Cynics, these "philosophers" are merely adopting that label to promulgate their own ends, and the resultant philosophy fails to make any real connection to or criticism of philosophy of science.

Susan Haack identifies the key failure of the New Cynics as conflating warrant with acceptance. Warrant, as Haack defines it, is a normative judgment of the merit or the evidence for a particular scientific claim, whereas acceptance is the standing of that claim in a particular relevant community.[50] The New Cynics, according to Haack, are "predisposed to focus on the social or the rhetorical, and may not be well equipped to attend to the often formidable technicalities involved in questions about the warrant of scientific claims."[51] Consequently, the New Cynics erroneously claim that warrant does not exist separately from acceptance. Having inextricably tied warrant to acceptance, the New Cynics next argue that acceptance is a mere social or political construct, and therefore warrant is entirely social or political as well. At least, this is how Susan Haack reconstructs the core of the New Cynics' philosophy, and by including the feminist philosophers of science in that category, this is how Haack describes the feminist

[48] Haack 1998, 92.
[49] Ibid.
[50] Ibid.
[51] Ibid.

philosophers of science as well. Furthermore, using her generalizations about the New Cynics Susan Haack creates a straw feminist philosopher to attack, a feminist philosopher who neither matches any of the leading feminist philosophers I discuss in this paper, nor who is consistent with my discussion of the feminist philosophy of science movement in general.

Susan Haack's dismissal of the feminist philosophers of science relies on an argument created in three major steps, each of which is remarkable in its misreading of the feminist philosophers of science. Her first move is to group the feminist philosophers of science with a number of other individuals from very different backgrounds, in order to form the group called the New Cynics. This group is supposedly aligned around the use of philosophy to achieve political, and specifically liberal, goals. Though it is true that the feminist philosophers generally have leftist leanings, that fact alone does not show that the motivations feminists are guided by and the goals they are aiming towards are political, rather than philosophical.

Having lumped the feminist philosophers of science together with this disparate group, Haack's second move is to assault their credibility as philosophers by attacking the New Cynics as a whole, claiming that the group misunderstands the philosophy of science with which they are engaging. A reader following Haack in this move must not only follow her in her description of their philosophy as relativist and divergent from Kuhn, but also must overlook the philosophical sophistication with which the feminist philosophers of science debate. Haack is claiming that the feminist philosophers of science can and should be divorced from philosophy of science as a whole, and in the process she is requiring also that her audience ignore the fundamental connections

between the feminist philosophers' work and that of earlier philosophers of science and that her audience dismiss the feminists' potential criticisms of other philosophers of science.

Finally, Haack offers up one feature of all the New Cynics – the conflation of warrant with acceptance – as the key feature of the feminist philosophers of science as well. It is this "conflation" which Haack considers to be the fatal flaw within feminist philosophy of science and which she uses to justify her claim that feminist philosophy of science is based on a core misunderstanding about science and philosophy of science. On each point of Haack's discrediting of feminist philosophy of science she demonstrates that she fails to adequately engage their philosophical positions and insights, but on this point especially her rendering of feminist philosophy of science does not do justice either to the philosophical insights the feminist philosophers have had or to their resultant philosophy.

1. Feminist philosophers of science are just more New Cynics

In her first move, Haack justifies her grouping by claiming that the feminist philosophers of science, like the rest of the New Cynics, hold that science is just politics. Certainly, all of the feminist philosophers of science I address in this thesis refer to the social nature of science, and each prescribes a certain social dynamic as necessary to achieve longer-enduring scientific claims, but discussions of the social nature of science do not necessarily relegate the entirety of science to politics. While it is true that some feminist philosophers, such as Sandra Harding, do discuss the political nature of science, within their works the feminist philosophers also emphasize that science is not simply mere politics. Harding herself emphasizes the development of a "New Objectivity," a

sort of objectivity which will help science to achieve its epistemological – and not just political – goals.

Furthermore, if the feminist philosophers of science regarded science as a merely political institution, then one would also expect that their suggested changes for science would be political for political reasons. Science should be democratized, for example, because for some political reason democracy is the best political institution. Yet in their discussions of changes science must undergo if it is to improve itself, the feminist philosophers of science appeal to epistemic, rather than political, rationales. Instead of advocating the inclusion of minorities because of some political ideology, for example, Longino proposes that minority voices would help recognize biases within science – an epistemic concern. In this – the focus on the recognition of biases – Longino and Haack even seem to agree. Haack, in criticizing the feminist philosophers of science, states:

> these observations about the epistemic interdependence of knowing subjects and the social character of scientific inquiry, rather than suggesting the conclusion that knowledge is inherently political, suggest, on the contrary, that one of the (of course fallible and imperfect) mechanisms by means of which bias gets detected and corrected – in scientific theorizing, and, though less systematically, in inquiry generally – is by means of competition between supporters of rival theories or approaches.[52]

Haack's recognition that it is the competition between rival theories that generates scientific progress in the form of bias recognition is strikingly similar to Longino's starting point – that the justificatory reasoning which forms the backbone of science depends upon community interaction and criticism. These two philosophers differ in the prescriptive portions of the account – in Longino's emphasis on diversity, and Haack's on science as usual. Longino's primary concern is just as philosophically motivated as

[52] Haack 1998, 146-147.

Haack's concern mentioned above; accordingly, the category of "New Cynic" is as awkward, or as fitting, on Longino as it would be on Susan Haack.

The essential concern for each feminist philosopher's prescriptive plan for philosophy of science is the link between a statement's objectivity and its survival of criticism, not some political end point. Understanding the feminist philosophers of science as engaging in philosophy of science for mere political motivations fails to recognize the real philosophical work the feminists have done and the real philosophical motivations driving them. Though the prescriptive plans they arrive at out of their initial investigations might be influenced by their politics, critics of these prescriptive approaches must address the feminist philosophers of science in general as philosophers and point out how the political influence on their prescriptive plans undermines their philosophy. Such a strategy would be the appropriate manner for criticizing the feminist philosophers of science, not haphazardly lumping them into the category of New Cynics.

2. Feminist philosophers of science do not properly understand philosophy of science.

Haack's second claim, that all of the New Cynics fail to understand the philosophy with which they are engaged adequately, stands in direct contrast with my presentation of the feminist philosophers of science as careful evaluators of historically loaded concepts within philosophy of science. Far from misunderstanding the field of which they claim to be members, the feminist philosophers of science engage in some of the most interesting and thoughtful evaluations of previous philosophy of science. Consider, for example, the feminist philosophers' consideration and evaluation of Kuhn. Haack contends that the feminist philosophers of science, like the rest of the New Cynics,

fail to recognize that Kuhn himself did not wish to undermine the authority of science. In fact, Haack implies as much in "Puzzling Out Science," when she states:

> Kuhn himself, evidently, did not intend radically to undermine the pretensions of science to be a rational enterprise; but most readers of *The Structure of Scientific Revolutions*, missing many subtleties and many ambiguities, heard only: science progresses, or "progresses," not by the accumulation of well-confirmed truths, but by revolutionary upheavals; there are no paradigm-neutral standards of evidence, only the different standards of incommensurable paradigms; a scientific revolution, like a political revolution, depends upon propaganda and control of resources; a scientist's shift in allegiance to a new paradigm is like a religious conversion, a conversion after which things look so different that he may be said to 'live in a different world.'[53]

Therefore, any relativism that has its roots in Kuhn, Haack implies, is misguided.

In this criticism of the feminist philosophers of science, Haack makes several broad claims, each of which is wrong. First of all, Haack assumes that the feminist philosophers of science actually do wish to undermine the authority of science, perhaps by emphasizing its relativist character. I have already addressed this charge against the feminist philosophers of science in part in the previous section, responding to attacks that the feminist philosophers of science wish to reduce science to politics, but this charge widens the criticism to a more general accusation of relativism. Secondly, Haack claims that if there is a dramatic shift between the feminist philosophers' end-points and Kuhn's, then philosophers should consider that shift as a mark against the feminist philosophers. Yet this attitude of Haack's runs counter to the very spirit of the philosophical endeavor.

Under Haack's first assumption, she asserts first that relativism will necessarily undermine science, and second that the feminist philosophers of science are all relativists. There are indeed some feminist philosophers of science who seem to embrace relativism, yet they do so with the intention of modifying relativism in such a way so that is won't

undermine science. Lynn Nelson, for example, fully recognizes that a great deal of her work serves to bring to light the relativist elements in science, yet she also contends that ultimately our pictures of the world and constructions of evidence will be constrained by the fundamental laws of rationality, in the process of this contention attempts to save science from being described with an "anything goes" and/or "anything can be saved" approach.

Furthermore, the work of other feminist philosophers of science, such as Donna Haraway, hardly seems to be captured adequately by the label "relativist" at all. She does, after all, call for a "no-nonsense commitment to faithful accounts of a 'real' world," and the focus of a great deal of her work comes from reconciling that commitment with recognizing the contingent nature of knowledge.[54] This focus exhibits a resistance to abandoning science to relativism, not an acceptance or a support of such a move.

Haack's second assumption is that the feminist philosophers of science reached different conclusions than Kuhn because of some misinterpretation of his philosophy. She offers as evidence that Kuhn never intended his philosophy to undermine science, but the New Cynics seem to be using it to do just that. Yet rereading the work of historical philosophers and showing their work has different implication than those philosophers might have agreed with is a common practice in philosophy, not something to be distained or considered unusual. Kuhn himself felt that he was reinterpreting or re-viewing much of the work of Popper, that their disagreements lay in seeing a rabbit where the other saw a duck. Using the work of a previous philosopher to achieve your own ends is hardly the mark of bad philosophy; in fact, it might be considered the

[53] Haack 1998, 91.
[54] Haraway 252.

foundational move for any good philosopher who seeks to take philosophy in new directions.

The feminist philosophers of science hardly fit with Haack's picture of a group of radicals piggybacking off the work of Kuhn, misinterpreting it to suit their relativist ends. Not only is it problematic to label all feminist philosophers of science as having relativist ends, but also presenting them as feminists who dabble in philosophy undercuts any attempt to improve philosophy of science. Exploring the readings and interpretations of philosophy of science in feminist philosophy of science as legitimate work within philosophy, rather than dismissing it all outright as wrongheaded, is not only the method for justifiably criticizing the feminist philosophers of science, but it is also the only response to their work which would derive benefits – either in the recognition of errors or in the examination of what they have gotten right – for philosophy of science.

3. Feminist philosophers of science do not understand the important difference between warrant and acceptance.

Finally, the essential error which Haack attributes to feminist philosophy of science – the error of conflating warrant with acceptance – is one which would only be suggested by a reader who fails to understand in any sophisticated way both the choices the feminist philosophers are making and the rationale behind their choices. First, the generalization that the feminist philosophers do not understand the important epistemological distinction between warrant and acceptance ignores the work of feminist philosophers of science such as Lynn Nelson, who relies on the concept of warrant to differentiate between science and non-science. While it is true that Nelson has reconceived of warrant as tied to community acceptance of values and methodologies,

this reconception hardly means that warrant is simply a sort of mere acceptance, as a review of Nelson will show.

To begin, Nelson contemplates a question not unfamiliar to other philosophers of science – how to draw a line between crystal ball gazing and "science". To answer this question, Nelson turns to "warrant", as defined by her feminist empiricism. Nelson holds that the "standards of evidence and methodologies are 'of our own making'", and the warrant for a claim is the result of community consensus upon certain standards and methodologies (against which, presumably, the crystal ball gazing would not measure up well).[55] But like Haack, Nelson is concerned with establishing that there are rules in science; her rules are simply ones with which we all have (perhaps unknowingly) agreed. The scientific community excludes the crystal ball gazer not because there is something wrong, in an absolute sense, with her practices, but because her claims do not have enough warrant according to science. Even if a few scientists decided to accept the crystal ball gazer's claims as "scientific", they would not be changing the warrant for that claim. The warrant is located within community standards that cannot be easily manipulated by individuals, but which can only be changed by alterations within that community as a whole.

Nelson's distinction between warrant and acceptance differs from Haack's, but this difference does not lie in Nelson's "error" of failing to recognize the objective nature of warrant. Nelson works to retain objectivity, of a sort, to which warrant is connected, but she has reconsidered what that objectivity consists of. Similarly, we can recognize the other feminist philosophers of science as retaining a place for warrant separate from

[55] Nelson 330.

mere acceptance within their philosophical programs, but connected instead to their understanding of objectivity (or perhaps, New Objectivity).

Haack and the feminist philosophers of science do not differ over their recognition that science is a special form of knowledge-collection, nor do they differ in their wish to understand what it is that makes science special. Certainly, they do not differ in their status as philosophers, and it is as philosophers that they disagree over how objectivity, and the warrant tied to that objectivity, should be understood within science. When feminist philosophers of science do connect warrant to some form of (most likely communal or social) acceptance, they make this move not because of some conflation of the two concepts, but because of a deliberate consideration of previous notions of objectivity, followed by an acceptance of objectivity as linked with surviving criticism, rather than as linked with neutrality. Haack's notion of warrant depends upon a conception of objectivity which markedly differs from that within feminist philosophy of science, and if she is to identify the feminists as making an error in their dismissal of warrant as absolute, the point at which she needs to engage their philosophy would be at the grounding conception of objectivity. Doubtlessly such a debate over the concept of objectivity would serve both the feminist philosophers and Haack, as each side refines and improves their position in response to the other, yet unfortunately a debate such as that requires first that each side credit the other with membership in the philosophical community, something that Haack is unwilling to grant the feminist philosophers of science.

The Usefulness of Feminist Philosophy of Science

Philosophy is, like science, concerned with the pursuit of truth, and also like science the truth with which it concerns itself is a contested ideal. Yet philosophy, be it philosophy of science, ontology, or any other of a number of subfields, is equally concerned with the engagement of ideas. With sometimes little or no "evidence", as judged by scientific standards, philosophers must instead offer up the advantages or disadvantages to a particular system of thought – it is coherent, or it matches with our intuitions, or perhaps it solves puzzles by which other systems find themselves helplessly ensnared. Through some rational debate, systems can be pitted against each other in the quest to find the optimal view of ethics, epistemology, or any other subset of philosophy, with the ultimate goal of obtaining the "best" picture of the world as a whole.

To a field such as philosophy two components are useful, though perhaps not essential: criticism and openness. One individual can develop a set of ideas independently of critics and perhaps can even evaluate the extent to which that system is advantageous when compared with other systems. Yet with the aid of criticism that individual can refine his or her system and improve upon it. It is through criticism that ideas are tempered, and that the advances one individual has made can come to impact and advance the work of another. Similarly, the philosophical community's openness to the inclusion of individuals in its discussions serves to help the community to invigorate itself with new thinking, new ideas, and new criticism. Indeed, some degree of openness is necessary for any criticism to take place; a philosopher must actually engage in or assume the positions of some other philosopher in many cases to make any use out of that philosopher's criticism.

When we narrow our focus from philosophy at large to academic philosophy, we can see that a few additional proverbs are enforced: it is unacceptable (now) to discredit an individual's criticisms on the basis of sex, race, or class. It is moderately acceptable (though perhaps not optimal) to dismiss an individual on the basis of their membership to some intellectual community. The difference between the two treatments could be that there is no common error that could be attributed to all Asian philosophers, whereas some flaw in reasoning could be considered common to all moral relativists, for example. Indeed, the technique of identifying a common error is precisely what Haack appeals to in dismissing feminist philosophy of science.

Taking all of the above into consideration in determining whether or not the feminist philosophers of science have something to offer philosophy of science as a whole, what comes to be at issue is whether the feminists potentially offer any criticism for philosophy of science, whether they offer any new ideas, and finally whether they could justifiably be excluded on the basis of some common error. I believe it is clear even to their detractors that the feminist philosophers of science have a number of points of contention with other movements within philosophy of science; instead, what has been debated is the degree to which those criticisms are grounded in any sophisticated understanding of the philosophical concepts they employ. Furthermore, in giving a number of very different prescriptive plans for science, the feminist philosophers of science offer a plethora of new conceptions of science to be explored, mined for their insights, and refined or rejected. In short, feminists philosophers of science are bringing their feminist training and concentration on the social elements of science to the table, which includes both new criticism for philosophy of science as well as new material.

Finally, in excluding the feminists as a group, or the marginalizing their work, philosophers would only serve the interests of philosophy of science as a whole if in fact a common error could be found within feminist philosophy of science, which would class each of their stances as individuals as misguided. Yet even this exclusion could only be legitimate if it involved an evaluation and elimination of the feminist philosophers of science as philosophers, which requires a degree of charity not present in previous dismissals of their work.

Real World Implications of Accepting Feminist Philosophy of Science

The acceptance of feminist philosophers of science as philosophers with feminist concerns, rather than with feminist motivations, has a number of practical implications, but I will review only a few. Within literature, this acceptance would show up in the inclusion of feminist philosophy of science within philosophy of science collections as simply one more group of philosophers. In other words, if the rest of the collection presents various positions within philosophy of science with a series of articles (by scientific realists, for example), then the feminists would be treated in a similar manner, rather than being excluded or "covered" by a summary article. Within the classroom, this acceptance would not simply give the feminist philosophers of science discussion time, but would incorporate their ideas and criticisms into discussions of other philosophers of science. And finally, this acceptance would render it no longer permissible to dismiss the feminist philosophers of science as feminists, "New Cynics", relativists, or any other label, so long as that dismissal did not also include as careful an evaluation of their work as I hope I have given here.

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