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RATIONALIZING EPISTEMOLOGY: AN ARGUMENT AGAINST NATURALISM IN FEMINIST PHILOSOPHY OF SCIENCE

by

MAUREEN LINKER

A dissertation submitted to the Graduate Faculty in Philosophy in partial fulfillment of the requirements for the degree of Doctor of Philosophy, The City University of New York

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Abstract

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by

Maureen Linker

Adviser: Professor Jerrold J. Katz

The dissertation involves an examination of recent work in Social Epistemology. In particular, I am concerned with the question of how one's social position could affect judgments regarding evidence and confirmation. To answer this question I undertake an investigation of feminist epistemology and philosophy of science. Feminist epistemologists have raised criticisms of the traditional analysis of knowledge by arguing against the primacy of the individual and for a more thorough-going analysis of the community in accounts of knowledge. This shift, in my view, is an important contribution toward a more complete theory of knowledge. Yet feminist epistemology is a controversial contributor to the area of social epistemology generally construed. Thus, in the first chapter I offer a defense of the contributions of feminist epistemology to analytic philosophy, against its various critics.

In Chapters Two and Three I examine feminists' reliance

on Naturalistic models of theory-choice such as Quinean Holism or the later Wittgenstein's Social Constructivism. I argue that the reliance on these models is a tactical mistake for feminists since these models are based on problematic accounts of meaning and truth. Alternatively, I advocate the adoption of a realist meaning theory along the lines proposed by Katz, as well as a *rationalized* epistemology in the service of explaining deductive and inductive inference. My argument involves showing how ralism and rationalism entail the kind of strong program of justification and rational censure which is required if feminists want to successfully condemn existing biased practices.

Consequently, I propose a reformulation of the distinction between a context of discovery and context of justification. My reformulation differs from the past positivist distinction in that "discovery" rather than being wholly random and subjective, is instead reflective of patterns of social experience. "Justification" is conceived of as more sensitive to the epistemic privilege of varying social standpoints. However, a separation is still maintained between how one comes to arrive at a belief and the methods for justifying that belief. My conclusion is that a partnership between rationalized epistemology and feminist theory can provide rich possibilities for both disciplines.

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I would also like to thank my father and brothers for their love and support during my years as a graduate student.

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Finally, my thanks and love to Peter Amato who has been a constant source of philosophical and personal inspiration.

I would like to dedicate this project to the memory of my Mother and my sister.

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INTRODUCTION

To say scientists are in the business of generating theories that help people to understand reality, seems uncontroversial. Less certain is the question of how this business operates. In some of the recent literature in the philosophy of science and epistemology there has been a focus on the fact makers themselves. Who or what is responsible for the knowledge deemed "scientific"? Is it the result of individual or group enterprise, or a complex combination of both? When we refer to a "scientific community" what are the institutionalized sanctions that demarcate members of this group? And following this line of questioning; Who or what sets the conditions for proper procedure and accredited facts?

In this dissertation I will explore some of the theories that have been proposed to answer the above questions. My interest specifically will be with those philosophical views that are concerned with the role of social and political factors in the formation of scientific knowledge. These views will be drawn from work in the areas of feminist epistemology and social epistemology. The inquiry will yield the following results. First, that the criticisms by feminist and social epistemologists, as to the dominance of the individualistic perspective in analyses of

science and knowledge along with the neglect of social and political factors, are well-grounded. Secondly, that institutionalized science and scientific methodology are resistant to interpretations which claim that scientific knowledge is a reflection of the social and cultural experiences of individual scientists. Thirdly, in accepting that a relationship exists between scientific knowledge and social factors, one should not be led directly to a naturalized epistemology and metaphysics. In my view, this is the mistake made by feminist and social epistemologists. Alternatively, I will propose an epistemological and metaphysical analysis which is realist in the sense that mind and language-independent truths are countenanced. I will argue that such an analysis offers a more philosophically adequate explanation of the matter than the explanatory models advanced by feminist and social epistemologists. In effect, what I am arguing for is the rightness of claims as to the overlooked nature of social factors in the pursuit and production of knowledge. I share the view with feminist and social epistemologists that there is significant, unexamined data left unanalyzed in traditional theories of knowledge. Yet while I agree on the matter of data, I disagree as to how it should be interpreted. Critics of more traditional epistemology interpret this data by adopting a framework of "naturalized" foundations. The central tenet of naturalized epistemology

is the view that belief and justification are a function of a knower's psychological and sociological causal history. I will argue that such a naturalized framework relies on problematic theories of meaning as well as questionable accounts of deductive and inductive inference patterns. For these reasons I suggest the adoption of a rationalist foundation for the explanation of knowledge acquisition and theory-choice. Hence, the three themes that will unfold throughout the course of the argument will be the question of whether patterns of social marginalization effect patterns of inference, the limits of naturalized epistemology, and the need for rational foundations in the explanation of social epistemology.

My argument will be structured along the following lines. Chapter One will begin with a consideration of objections to the social dimensions of epistemology focusing specifically on feminist epistemology and philosophy of science. Feminist philosophy of science has been described as a "source of inspiration" for social epistemology since the 1980's.[1] Generally, feminist philosophers of science and epistemologists have argued that social interests are at work in the methods of truth seeking typically employed in the sciences. As such, they urge an epistemic research program which is committed to the idea that our accounts of knowledge must undergo an "excavation" in which social factors and inequities are brought to light and understood.

Social epistemology has been characterized as "dividing into three branches"[2]. These are: the role of social factors in individual knowledge; the organization of the cognitive labor of individuals and groups; and the nature of collective knowledge. Because feminist philosophers are concerned with all three of these elements their work provides a good case in point for discussion. In addition to this reason there is also the fact that currently in philosophy "feminist epistemology" is a more questionable enterprise than "social epistemology" generally construed.[3] If doubts can be settled regarding this more controversial contributor to social epistemology, then in my view, it will have settling effects for the field as a whole.

Therefore, in Chapter One I will consider what I see to be three general objections to the enterprise of feminist epistemology. The result will be that the objections raised provide no real challenge to the area of scholarship described as feminist epistemology and its related role in social epistemology. Having cleared the way for the possibility of feminist epistemology, I will then move toward a discussion of the particular epistemic models proposed in feminist and social explanations.

Chapters Two and Three will involve an examination of the analyses offered in the literature of feminist-social epistemology and philosophy of science with a focus

specifically on the issue of theory-choice. I divide the majority of these analyses into two naturalized categories. Chapter Two deals with a general Holistic Model and in Chapter Three, a Social Constructivist Model.

The Holistic Model is advanced by philosophers such as Lynn Hankinson Nelson (1990), Louise Antony (1993) and Hilary Kornblith (1994). These philosophers offer views that can be used to explain the causal claims of socially guided research by way of Quinean arguments, including arguments against the analytic-synthetic distinction. One of the consequences of Quine's attack on the distinction is that decisions about modifying scientific claims must be treated as pragmatic and not logical decisions. Further, in deciding where to modify our theoretical structure in the face of disconfirming evidence, we are not precluded from modifying the propositions of logic and mathematics as well as empirical claims. Hence Quine advocates a naturalized epistemology that is integrated with work in psychology. As part of psychology, philosophical claims about how we can have knowledge of the world and how science can produce knowledge are just as open to revision as any other part of science.

Epistemologists influenced by Quine note that empiricist accounts of evidence have been forever changed by Quine's arguments. These changes can accommodate the data of interest to feminist and social epistemologists without

appealing to foundationalist and individualist epistemologies. From a Quinean backdrop, these epistemologists step forward with explanations of discovery and justification that take knowledge to be a product of a communal theorizing. What is insisted upon is the fact of social and political experiences, including fundamental experiences of gender, race and class, as playing a role in the overall theoretical schema. How much of a role these factors play, and in what particular ways, is a divisive issue prompting a variety of answers. What these views share however, is the recognition that the boundary between scientific values and social and political values does not hold steady. By understanding how this boundary is crossed, proponents of the Holistic Model hope to uncover how objectivity and cognitive authority in science have been used to distort and manipulate institutionally sanctioned ways of knowing.

My argument will be that the acceptance of Quinean holism in the development of this theoretical model presumes a view of language and knowledge that rests on faulty assumptions. By adopting the Quinean perspective, these epistemologists have inherited a model that is not up to the task of explaining the connection between knowledge and social factors. Moreover, the social emancipation and epistemic normativity that is envisioned by making this

connection, envisioned particularly by feminist epistemologists, is untenable given this view. I will argue instead that Quine's view of meaning should be replaced with "linguistic realism" a view developed in the work of J.J. Katz (1981, 1990), as it has significant advantages over the Quinean view. In addition I will present arguments against the dual theses of "centrality" and "revisability" advanced by Quine and Hilary Putnam (1975). My argument will involve showing the internal inconsistency of these notions and their inadequacy in explaining the truths of logic, language and mathematics. The results of these arguments will prove favorable for a rationalist explanation of the social character of knowledge.

However, holism is not the only naturalistic model relied upon to explain social factors in the generation of knowledge. There are also those who advocate a Social Constructivist Model along the lines of Wittgenstein's later philosophy. In Chapter Three I will turn to this proposal and the views of philosophers such as Helen Longino (1989, 1990, 1994a, 1994b), Naomi Scheman (1983) and Elizabeth Potter (1993). Constructivists adopt from Wittgenstein the notion of a socially constructed language and epistemology which is limitless and continually expanding. In line with Wittgenstein's arguments against traditional normativity and essence, these philosophers argue that the best explanation of the relationship between

knowledge and social factors will rest on a view of science and objectivity that is fundamentally a matter of social consensus. Applying the paradigm of a "language-game" constructivists argue that the rules of the game with regard to the language of theories is starting to change. The change in the game is attributed to the change in the gender, race, and social status of the players.

In addition, social constructivists argue that there is no absolute right way to do science or go about discovering the Truth. Sound judgement and valid reasoning are grounded in an ultimately practical kind of common knowledge. Embracing the idea of a "community of interdependent knowers", these philosophers have rejected realism, epistemological individualism and absolute standards of normative justification.

Though both holists and social constructivists view language and epistemology in a naturalistic and anti-foundationalist light, they differ in that on the Social Constructivist Model there is no adherence to the positivism of sensory experience. Unlike Quine, Wittgenstein did not give a priority to the physicalistic conceptual scheme over all others. In addition, the inclusion of non-scientific values into science is also more easily explained on the Social Constructivist picture since social norms constitute truth rather than uncover truth.

In Chapter Three I will argue again that the view of

meaning on which the Constructivist Model rests, in this case as developed by Wittgenstein, contains problematic arguments and assumptions. These problems carry over to social constructivist analyses of knowledge and metaphysics. Again I will suggest a replacement with linguistic realism and a more rationalist epistemology and metaphysics with the result being a more theoretically adequate explanation of the connection between epistemic inquiry and social factors.

Having set out these arguments and promissory suggestions, I will proceed in Chapter Four to outline an alternative epistemic model. My explanation of the connection between epistemic inquiry and social circumstances will rely on the combined effects of a realist meaning theory and a resulting realist epistemology and metaphysics, along with an objective analysis of inference, belief and judgement. The objective analysis will involve arguments that probablistic facts regarding an individual's social group as well as their personal identity effect belief but ultimately do not affect the evaluation of inference patterns. Therefore the analysis supports a more rationalist approach in epistemology. For these arguments I will draw upon elements in the work of Jonathan Adler (1993), and Richard Feldman and Earl Conee (1985).

My argument will involve the claim that a consciousness of social marginalization creates a cognitive sensitivity to

particular kinds of evidence among individuals and communities. In this way social realities enter scientific reasoning. However, I will go on to show how this sensitivity to evidence is limited to beliefs and does not extend to the preference for patterns of judgment which are more truth productive. Hence, I will show that the correct evaluation of inference must be grounded in objective, reliable, truth productive patterns of inference independent of the person or community that implements these patterns.

In Chapter Five, I will sum up the linguistic and epistemic arguments thus far and then resurrect a revised "context of discovery" and "context of justification". This revised version of the distinction will differ from past positivist accounts insofar as the context of discovery will not be construed as a wholly unpatterned, purely subjective feature of an individual's psychology. What I will show is that in accepting that a relationship exists between science and patterns of social experience, one is not committed to the absence of agent-neutral standards of evaluation and judgment operative in the justification of knowledge.

One element that factors into my analysis is a focus on researchers who see a connection between their methods of inquiry and their identity as a member in a social group. A consciousness of marginalization will therefore contribute to understanding how knowledge intersects with social factors. In this way I see group identified researchers as

providing epistemologists with a unique perspective from which to explore the question of social factors and knowledge as they have a social self-consciousness atypical for science. However, as I will make clear in the arguments that follow, I do not agree with those who claim that the position of self-recognized social marginalization offers an individual or group with a "superior" epistemic standpoint from which to know. Rather, as I will try to show, what these perspectives provide is an insight into how social factors are involved in epistemic inquiry despite the fact that many individuals have failed to see them. In this way a consciousness of social marginalization does not provide one with a perspective which is uniquely superior but rather, uniquely informative. The methodology employed in deciding which information will prove useful for the acquisition of true beliefs is, I will argue, independent of the matter of social perspective.

As I will try to show, the informativeness derived from a marginalized perspective is due in large part to the fact that the majority of people who have been involved in the most socially sanctioned pursuits of knowledge, have also been identified with the most socially preferred groups. As a result, the intersection of their social and cultural experience with their scientific approach was not anomalous. Since the majority of people in science proper have been part of roughly the same social groupings (i.e.

white, male, American, European, middle-class) they have not necessarily experienced gender, race or class as categories affecting their systematic organization of knowledge. In addition, there is the fact that in developing the right attitude in the institutionalized pursuit of knowledge, one must accept the conventional ways of knowing and the disciplinary perspectives presented as the norm. We may call these attitudes "right" or "natural" but the essentialist assumptions they carry can benefit significantly from critical examination.

I will maintain that if we are interested in the question of how social realities factor into scientific knowledge, it will be best to start with the experiences of individuals who consciously recognize a system of social hierarchy in science and seek to address it. As Terri Elliott has said "Aspects of the social order are conspicuous for marginalized people because they are unusable for them. Its unusability is not discovered however, by abstractly attempting to establish its properties, but rather by understanding the effects of the dealings in which it is used."[4] This unusability, and what it tells us about the organization of scientific methodology, has thus far been understood only vaguely in epistemological terms.

Therefore, the combined results of this investigation will be that social perspectives do interestingly factor

into the pursuit of knowledge and yet do not factor into the correct evaluation of language and inference. I end Chapter Five with the conclusion that a naturalistic position is a tactical error for social epistemology given that an agent-neutral standard of evaluation provides both a theoretically better account of knowledge as well as the only possibility for the condemnation of practices which are biased and discriminatory. Hence, social epistemology is best understood in rationalist terms.

In the conclusion I offer some suggestions regarding results which might follow given my view. I offer the possibility that the partnership between feminist criticism of science and a *rationalized* epistemology will provide important benefits for both schools of thought. By advocating a strong program of justification, rationalists need not be blind to the reality of social factors in epistemic matters. At the same time, in recognizing the harmful affects of epistemic overconfidence in the past, feminists need not give up the benefits of standards of justification. The remainder of the chapter will devoted to summarizing these outcomes.

There are two characteristics to the project overall that require special mention. The first is why a concern with knowledge in a general sense, is analyzed from the point of view of the construction of scientific knowledge and the process of theory-choice.

Science is seen at this point and time in our history as a highly respected and successful enterprise. While critics of science exist, their role is circumscribed to pointing at misapplications of the uses of science and scientific methodology. For example, questions are raised as to whether we ought to invest in research for space exploration or AIDS. Questions of this kind assume the likely possibility of scientific success and are not proposals for acquiring true beliefs in some other way. Proposals are not made because it is generally believed that the methodology of science is judged overall to be uniquely sensitive to the question of evidence. Hence the closer an inquiry comes to the methods of science, the stronger the inclination for societal institutions to consider it true.

Therefore in keeping in line with much of the work in epistemology, scientific knowledge is used here as the exemplary case of knowledge in general. So that whatever holds true in circumstances of justification and evidence in science, will hold true for less exemplary cases of knowledge.

The aspect of scientific methodology that will be the focus in this work is the situation of a researcher's having to decide among alternate competing theories by judging what the best account of evidence is in the event of explaining data.

The second characteristic of the project overall is the

predominance of issues from the philosophy of language. Just as epistemology is concerned with giving a coherent reconstruction of the methodologies of science, analytic philosophy generally construed is concerned with analyses that start from the perspective of language. This century has brought about a revolution in scientific progress and technology as well as a linguistic turn in philosophical thought. As such, many of the accounts of knowledge developed focus on the question of propositional meaning. In the models discussed in Chapters Two and Three, an understanding of the epistemic issues requires an understanding of the meaning theory assumed by each model. In addition, in the alternative epistemic explanation proposed in Chapter Four, an underlying meaning theory is required to frame the epistemic, metaphysical and psychological claims that are advanced. Consequently, the variety of approaches to knowledge - scientific, linguistic, historical, social, psychological and metaphysical - will not be treated here as independent of one another. As I will try to show, an adequate picture of knowledge will only emerge with the integration of all these different approaches. Undoubtedly, a completed version of such a picture is a still a long way off. However, in outlining some recent attempts currently being pursued and the controversies they entail, my intent is to show why a rationalist epistemological model requires consideration

even in the present climate of naturalized epistemology.

NOTES

1. Schmitt, Frederick. 1994.
2. Ibid., p.4
3. See *The Monist*, Volume 77. No 4, October 1994
4. Elliot, Teri. 1994. p.248.

CHAPTER ONE

Scientific Inquiry and Social Epistemology

I. The Marginal Perspective in Science

> The difficulty with success stories is that they tend to obscure the impact of oppression while focusing on individual strengths. You start to believe that with sufficient determination anything can be accomplished. If conflicts and obstacles exist, they are "internal".

The above quote comes from Evelyn Fox Keller's article "The Anomaly of a Woman in Physics".[1] In that article, Keller describes her experience as a female graduate student in the Physics department at Harvard in the early 1960's. It is a hard story for Keller to tell, as she says, in part because of the conflict of being a scientist who also feels it important to recount her personal experiences. She explains,

> It may be difficult for those removed from the mores of the scientific community to understand the enormous reticence with which anyone, especially a woman, would make public his or her personal impressions and experiences, particularly if they reflect negatively on the community. To do so is not only considered unprofessional, it jeopardizes one's professional image of disinterest and objectivity. Women, who work so hard to establish this image are not likely to take such risks.[2]

Keller's description of her experiences recounts a climate in which the ideals of hard work and "good" scientific methodology were considered to be independent of social and political realities. The scientific establishment of which

she was a part, encouraged a "minimizing" of differences based on one's sex. As a result, while Keller noticed herself to be a minority in the department, she understood this to be a result of her being "special" in that she was able to transcend the outdated, stereotyped roles felt by most women. The fact that she had made it into the physics department of the most prestigious university in the United States was evidence that discrimination was not a problem for her. If other women experienced obstacles in the pursuit of their interests at this point in time, it was because they were not sufficiently motivated and determined. The opportunities were there if women wanted them.

It was within the context of this way of thinking that Keller began to experience real difficulty and alienation. Because she was considered to be special, in that she was a woman who was able to meet the standards required in this "equal opportunity" offer, she was alone. Her identification with women and with the political and social realties that women faced, were ignored in the all male environment of Harvard's physics department. Her most difficult time came when she felt the full force of a clash of identities. On the one hand she had been socialized as a woman, replete with all the characteristics and stereotypes that came with that process. On the other hand, to succeed as a theoretical physicist, she felt she must adopt a new identity and persona so as to be successful among men. The

pressure she felt was to choose one over the other. To reject the stereotypical feminine role, meant giving up aspects of herself which she took pride in. Her emotional responsiveness, her compassion toward others, her reluctance to work competitively as opposed to cooperatively, and her desire to be a mother and family person. Yet, to give up her identity as a physicist meant rejecting her curiosity, her sharp analytic abilities and her love of physical theory. What was most frustrating for Keller was that the men in the department did not seem to have to make the same choices. While certainly no man came to the discipline without having to conform in some ways to the subject, most did not seem to struggle with the fundamental questions of identity that faced Keller. As she saw it, the dynamics in the physics department were significantly more hospitable to the facts of male socialization. Once Keller started to struggle more publicly with these issues, she experienced real discrimination and hostility from other members of the department. She was seen as erratic, unduly emotional and overly concerned with gender issues. She was advised to leave the physics department and was given a transfer into the Biology Department where she completed her Ph.D..

Keller's story provides a starting place for understanding the conflict felt by many people who struggle to become part of a community which characterizes itself as non-discriminatory and non-hierarchical and yet seems to

privilege a particular kind of socialization and system of values. Science is a particularly interesting example of such a community because of its apparently natural evolution toward minimizing subjective differences and prejudicial factors and its reinforcement of the virtues of objectivity and disinterestedness. The problem arises when it is felt that this natural development serves as a pretext for maintaining a social order that favors a certain select group.

If the effects of social stratification and inequality are independent of the methods of science, then to introduce such an analysis within the confines of a scientific inquiry is to be "unscientific". Hence science becomes an activity resistant to social analysis. It is not surprising then, that scientists like Keller feel an enormous reluctance in bringing up gender based criticisms of the field. In doing so an individual shows just how "unscientific" she can be.

Keller's experience is interestingly consistent with the findings of a large scale study done by the American Association for the Advancement of Science (AAAS) entitled "The Double Bind: The Price of Being a Minority Woman in Science."[3] The study was initially prompted by the fact that only 11.5% of professional scientists in the United States were women and of these, less than 5% were non-white. The study organizers decided to interview Native-American, Puerto-Rican, Mexican-American and African-American girls

and women working at all levels in the natural sciences. The respondents ranged from high school girls attending specialized science schools to women working as professional scientists.

Among the results noted were that female students in the sciences perceived their teachers to have lower expectations of them than for male students. They also experienced a sense of "social-isolation" and a lack of same-sex, same-race peer companionship.[4] The majority of the girls interviewed (61%) felt that they were headed for a career as a primary or secondary school science teacher. The remainder saw themselves as physicians. Many talked of the difficulty in forming "informal study groups" and found it hard to ask for individualized help from teachers.[5]

Among those women interviewed in graduate school, their most pressing concern was a lack of contact with major advisors. They knew of career rewards and professional decisions being determined "over lunch" or "over cocktails"[6] in intimate social settings with an older male professor and a younger male protegee. All agreed (98%) that the white males they knew in graduate school had an advantage in having accessed an informal system of "contacts" for entry and advancement in the professional world. Moreover, the further along they progressed in graduate school, minority women experienced an increase in loneliness, pressure to choose a more traditional career and the desire to return to

their original community.[7]

Most graduate and professional women interviewed felt strongly that they had to adopt an uncomfortable personality style in order to advance their professional status. They felt their own cultural experience made it such that this professional style was even more extreme for them to take on than for the white women they knew in their respective fields.

And finally, many professional women felt that a strong involvement in activities that would promote race and gender concerns in science, and would address some of the difficulties they experienced in their rise through the ranks, would be professionally damaging to pursue compared with general civic-interest activities that are not gender and race specific. As one African-American chemist described it "I wouldn't want to become known as the one always focusing on black issues or women's issues. I think it would make my non-black male colleagues uncomfortable and less inclined to include me on projects."[8]

In a 1982 National Science Foundation[9] study of women scientists and engineers, women ranging in age from twenty-five to ninety-two were interviewed. An unusual result of this study was the fact that younger female scientists seemed more than their older counterparts to think that the idea of a woman scientist went against the popular notion of what a woman was supposed to be. When older women were

interviewed they said they rarely felt a gender bias in their experience due probably to the fact that "I was the only woman around".[10] Some said that perhaps if there had been a few more women, together they could have made sense of their outnumbering. But in their singular experience as female scientists they were encouraged for their "rugged individualism".[11] However, many of these older women added looking back on it, how isolated they really were and in the light of the feminist movement, how discrimination was operative in their field.

The fact that younger women scientists and engineers perceive a more rigidly limiting stereotyped notion of women was found surprising by the study's organizers. When they explored this fact in interviews with the women, they discovered that mass media images contributed strongly to this perception. The young women talked of the ways in which mass media typically depicted women, especially in relation to science and technology, in inane ways. The effects of such imagery, particularly on women who were eager to have role models in their development as scientists, was to minimize the dignity and achievements of women. Some of the images recalled by the women interviewed were "the busty, blonde, short-skirted nurse" or "the somber, prudish, matronly, lab worker"[12]. These were contrasted with the popular images of male scientists as "all-american astronaut hero", "good natured, absent-minded

genius" (popularized by America's admiration for Einstein) or "serious, respected man of science".[13]

The study organizers devote a substantial amount of time to the issue of media influence and end their comments by saying "The negative effects of this mass media imagery on the general perceptions of young women cannot be overemphasized"[14].

One final study, completed in 1995 by Gerhard Sonnert and Gerald Holton and sponsored by the National Science Foundation[15], found results consistent with the earlier studies described above. Using two explanatory models, a *deficit model*, based on structural facts and the formal and informal mechanisms used in determining appropriate hiring practices along with a *difference model*, which posits the existence of differences in behavior, outlook and goals between men and women, Sonnert and Holton found an interaction between structural impediments and behavioral-attitudinal issues resulting in a gender disparity in the sciences.[16]

Sonnert and Holton's study of male and female scientists is thus far, the largest of its kind based on 700 initial interviews with 461 (362 men, 99 women) former awardees of a National Science Foundation (NSF) postdoctoral research fellowship and 239 (147 men, 92 women)former recipients of a National Research Council (NRC) postdoctoral associateship. The scientists interviewed ranged from

awardees at the start of these programs in 1959 through recent awardees up until 1986. Every woman who has ever received an NSF or an NRC fellowship was interviewed as well as a control group of men. In addition to the initial questioning, Sonnert and Holton conducted 200 personal interviews with 92 men and 108 women lasting two to three hours[17]. What is unique about this study is the focus on women who remained in science long enough to receive a prestigious postdoctoral fellowship. By studying this group, Sonnert and Holton hoped to shed some light on the questions of barriers, real or imagined.

Among the study's results it was found that 29 percent of women were represented at the top 15 percent of national research universities compared with 27 percent of men. However women as a group "paid" for prestigious affiliation with notable disadvantages in rank, whereas men did not experience such a trade-off[18]. In the physical sciences, mathematics and engineering, even among this elite sampling, women's average academic status was almost one full rank below men's. Despite legal prohibitions that are apparently now in place, 72.8 percent of women reported discrimination whereas among the men, 12.9 percent reported reverse discrimination[19]. While some of the discrimination described took the overt form of a denial of job or tenure to a woman apparently well qualified, there were more subtle accounts of exclusion and marginalization. In particular,

in the area of scientific collaboration, women experienced a statistically significant exclusion from collaborative projects at the equal or senior partner level. Also statistically significant, more women said that their postdoctoral advisors ignored them or treated them explicitly as subordinates.

Sonnert and Holton asked the interviewees if men and women do science differently. The answer from the majority was "yes". Somewhat more women, 60.8 percent versus 49.4 percent of men, said that they believed in the existence of gender differences in the work of scientists in general. The biggest difference described by the scientists interviewed is the tendency for men to be more "entrepreneurial" aiming for research projects with higher visibility and more potential for career advancement. This difference translated into the predominance of men entering into the most competitive projects in their respective fields. On the other hand, women were noted, by both men and women, to select problems according to a "niche approach", creating their own area of expertise. A majority of the women interviewed expressed the pleasure of working on problems that were not within the domain of competitive investigators and researchers. Moreover, it was noted that women tend to work on individual projects longer and take on projects that are broader in scope than do men[20]. A result of this tendency was used to explain the fact that the women

in the group, on average published less overall than their male counterparts. (2.8 percent per males per year, 2.0 percent per females per year). However, Sonnert and Holton noted that within this sampling of scientists, the women received significantly more citations per article, on average than did men - 24.4 for women versus 14.4 for men[21]. It was noted that this result could provide evidence for a shift toward a more qualitative approach in the scientific reward system rather than publication productivity.

Sonnert and Holton end the study with the summary statement:

> "Do women scientists have equal access to science, and are there obstacles that keep them from gaining equal access? The collective outcome suggests a larger accumulation of disadvantages than of advantages....The accumulation of subtle structural disadvantages offered by the deficit model together with the attitudinal and behavioral model, may afford a partial explanation of the glass ceiling where it exists."[22]

What these studies have in common is the fact that women in science feel themselves to be entering a ready-made structure that is more conducive to the virtues emphasized in white, male socialization; virtues like competitiveness, self-assertiveness, self-importance, and public accomplishment. These qualities pose a double standard in that they are considered socially positive in men and negative in women.

In investigating the literature of women in science and the accounts that have influenced feminist and social

epistemologists, certain things coalesce. Girls it seems, grow up discovering that women are valued far more highly by the society for how much they please others physically and emotionally and for how responsive to others they can be, than for how much they themselves are satisfied intellectually and emotionally. In addition, girls and women are not included among history's most professionally competitive males, including scientists, athletes, the majority of powerful figures in the world's political and religious organizations. Essentially, women are missing from the history of honored individuals who are credited with shaping major world changes. Given their marked absence, girls must wrestle with a social pressure to not identify with these roles while at the same time acknowledging the need to equalize power relations between the sexes. Factors such as these are part of the complex socialization processes that effect individual and group identity formation. One result of this process is that many girls and women noticeably feel the effects of gender differences in their attempt to take on the virtues of science. Moreover, in trying to articulate what feels like a one-sidedness to the activity of science, women talk of a dismissive attitude on the part of their more prominent male colleagues which in turn undermines their own ability to analyze the experience.

With the development of feminist theories, many women

have been able to articulate the tensions and conflicts they experience and analyze them in social and political terms. In 1879, Maria Mitchell, the first American female astronomer and the first president of the Association for the Advancement of Women, wrote that as she grew older having led an independent life, she came in contact with many intelligent political women, particularly some of the early suffragettes, who convinced her of the necessity to become a "woman's rights woman"[23]. The perspective that feminism offered, she says, gave her the confidence to publicly pursue a place for women in science and education.

Almost one hundred years later, Keller says that "if I had had an awareness of the social and political realities of women while a graduate student, it might have saved me from persisting in a search for acceptance and validation as a woman scientist where it would not and could not be given."[24]

Ruth Hubbard, a professor of biology at Harvard, writes that "feminism affords women with a framework from which to explain their experiences as women in a culture that does not encourage race, class or gender analysis".[25] As feminists in science, women develop ideological and political reservations about fitting into what they perceive as a hierarchical structure. Hubbard questions the notion of "equal access". She argues that women are reluctant to

push for access to professions that are not really equally open to them. The inequality, Hubbard claims, is a result of the fact that women must jockey for positions in a field that they have had no part in building and in which they are acutely uncomfortable. Because the virtues of a scientist generally, are so consistent with male socialization, Hubbard argues that women are being subtly discouraged from developing their own intellectual potential and seeking positions of influence in the production of knowledge. The result as she sees it is a sifting out of all but the most determined minority of women.[26]

When feminist philosophers argue that there is something problematic with such a system, they are criticized by many (including many women) for reinforcing old stereotypes and dualisms, in this case the dualism between science and women. They are reprimanded for concluding that science and knowledge are in some fundamental ways, "masculine" endeavors, too difficult and challenging for most women. However, unlike some of the dualisms of the past, (such as reason and emotion, man and nature or mind and body), feminist critics argue not that there is something inferior about women, rather they argue that something is wrong with science and something is wrong with the way scientists relate to the natural world and each other. Hence, the alternative to changing the virtues of women's experience is exposing the hierarchy in science and

changing *its* methods and virtues. This possibility suggests the development of new ways of thinking about experience and knowledge.

Many feminists working in epistemology and philosophy of science have claimed that women and other socially marginalized groups, because of their different socialization and cultural experience, have different conceptions and assumptions about their relationship to the natural world. It has been argued that a feminist perspective has developed which entails different views on the importance and connectedness of other people, distinct ways of relating moral responsibility to intellectual pursuits, and alternative conceptions of the epistemic and valuative notions of "success" and "failure". Hence the inclusion and to some extent replacement, of these notions in the more traditional picture of science is viewed by feminist epistemologists as representing a more complete material and intellectual understanding of experience - a truer knowledge.

Yet for critics of feminist epistemology there is at the very least a skepticism and at the most, a complete denial of anything like "feminist ways of knowing". The very name "feminist epistemology" is thought to share the same uneasy alliance with other such misnomers as "Jewish physics".

In a 1994 issue of *The Monist* entitled "Feminist

Epistemology: For and Against"[27], several authors voice their objections to the pursuit of epistemology from a feminist perspective. These include Harriet Baber, Barry R. Gross, Iddo Landau, and Alan Soble. Aside from the writers included in this volume there are other outspoken critics of feminist epistemology among them Susan Haack[28] and Cassandra Pinnick[29].

What these critics have in common is the belief that feminism is an ethical or political position which can offer nothing normative to the study of knowledge. The experiences of women in science like the ones described above are understood, when they are considered at all, to be the result of sexist or racist *scientists* not sexist or racist science. This general view is then recast into a variety of criticisms. I will divide the criticisms into three different types, looking at each type in turn and offering reasons for why ultimately, they should be rejected.

II. Three Types of Criticism

A. The Detriment Argument

The first type of criticism of feminist epistemology to be considered will be referred to as the "Detriment Argument". The basic idea behind this criticism is that feminist epistemology is detrimental and in fact at

odds with the egalitarian interests of women and likewise, all marginalized groups. This criticism is typified in the article by Harriet Baber, "The Market for Feminist Epistemology".[30]

Baber reasons that a way of doing philosophy and epistemology described as "feminist" paves the way for "pink ghettos" - small, undervalued areas circumscribed for women's work in philosophy.[31] Aside from a distaste for ghettoization, Baber also offers an argument for why women qua women should not be understood to have a unique way of knowing or experiencing reality.

Baber argues that feminist theorists draw on work in sociology, psychology, political science and the natural sciences hoping to build on a "woman's perspective", something along the lines of the "different voice" described by Carol Gilligan in her work.[32] Baber claims that if Gilligan is to serve as an example feminist theorists will soon be out of a job because Gilligan's work is so questionable. Baber cites Carol Tarvis who argues that Gilligan's ideas lack empirical support.[33] Tarvis' conclusion is based on the results of several follow-up studies done after Gilligan.[34] Baber explains that behaviors linked to gender have very little to do with one's sex and much more to do with what a person does for a living and their situation in life. The myth of a different voice is likened by Baber to the myth of mother-infant bonding

debunked by Diane Eyer[35]. These so called "scientific" myths are supported, according to Baber, by popular beliefs about men and women as well as the institutional goals of the society. They are appealing in that they reaffirm folk wisdom regarding gender differences which "many people have traditionally found intuitive"[36].

Baber refers to an example in Tarvis' study of a law professor who presented Gilligan's material to her class and received "vociferous resistance" from both male and female students[37]. As the professor describes it "...many of the women in class plan to be litigators and they don't consider themselves naturally soft or pliable or less capable of justice based forms of moral reasoning.[38]" Hence Baber concludes that Gilligan's study and any subsequent study of a "female voice" or perspective is doomed to failure as there is no consistent set of facts that capture all women's experiences. Moreover, any attempts that say there is something consistent or patterned in women's experience will be dangerously limiting for those women who do not share the perspective. This last concern of Baber's will be readdressed later in my response to the third type of criticism of feminist epistemology.

To begin, Baber's reliance on Tarvis to undermine Gilligan's view and feminist epistemology along with it, fails to take some obvious considerations into account. For instance, the issue of whether gender differences are

biologically determined, psychologically determined or culturally enforced remains an open question. Yet, Baber characterizes Gilligan as offering a view of difference that depends on "deeply entrenched biological and developmental differences which are difficult to alter".[39] Gilligan is explicit about not attempting to settle the issue in *A Different Voice* She explains:

> The different voice I describe is characterized not by gender but theme. It is primarily through women's voices that I shape its development. But this association is not absolute....No claims are made about the origins of the differences described or their distribution in a wider population, across cultures, or through time. Clearly, these differences arise in a social context where factors of social status and power combine with reproductive biology to shape the experience and thought of males and females.[40]

Having misconstrued the scope of Gilligan's analysis to be a rigid study of biological and psychological sex differences rather than as an exploration into an alternative and misunderstood moral perspective; Baber goes on to say that a more accurate criterion of "psychological and behavioral differences which do exist are better explained...by one's current situation in life. What one does and needs to do"[41]. This criterion is being offered up by Baber as a contender to a gender based explanation. However, where one is situated in life is significantly determined by one's gender. In the SEEDS series[42], a study of global labor divisions organized by The Population Council, Martha Chen writes:

> Gender is one of the most significant determiners in how a person will be effected by socioeconomic and demographic trends. Moreover, there are continuing aspects of the traditional systems - discriminatory customs and norms regarding the sexual division of work, marriage, family and the inheritance of property which contribute to unequal gender distribution around the world.[43]

By shifting the focus to one's situation in life, Baber begs the question of difference, given that a crucial factor in the division of labor and social status is determined by gender. Regardless of whether there are essential biological-sex traits, men and women are socialized differently and occupy different roles within a society. One of the empirical studies Baber relies upon to dismiss Gilligan's work ends with the statement "conditions of employment, not qualities of the individual determine what people value about their work...when men and women hold the same prestigious jobs, their values and behaviors are similar."[44]

However, men and women are a long way off from "holding the same prestigious jobs". Therefore, the differences noted by Gilligan can reasonably be attributed to the above mentioned economic and labor divisions along with obvious facts of different experiences of socialization. But the point is that neither Gilligan nor those subsequently interested in her work who identify themselves as feminist epistemologists, attribute the differences noted in the study to be the result of what Baber calls "virtually

ineradicable" qualities in men and women.[45] The fact is that Gilligan "traced" the difference through the voices of women and subsequently an overwhelming number of women and men saw the model as a springboard for articulating alternative, feminist based research programs.

It would be truer to Gilligan's work, and the work of feminist epistemologists who have found the study compelling, to see it as a reevaluation of a mode of thinking and acting that has historically been undervalued, considered irrational, and stereotypically been attributed to women. The theoretical communities that have acknowledged Gilligan as something of an influence do not stand or fall with the question of the accuracy of her empirical findings. Rather, the study is taken to be more useful by many because it has provided a framework for investigating normative questions of moral reasoning rather than facts of biological sex-trait differences.[46] Gilligan's approach has given a sense to the kind of conflicts of interest many feminists (and some non-feminists) have experienced in their theoretical challenge to accepted models of knowledge and value.

As far as Baber's reference to the law class in which Gilligan's work was seen as offensive to women, the fact could more likely be attributed to the inadequate way in which the material was presented as opposed to what actually appears in the work. Gilligan suggests that girls and women

in her study interpreted moral questions from the perspective of a "care" oriented approach. She stresses several times that this perspective employs a logic and mode of reasoning that assumes responsibility to others and the maintaining of connection rather than a logic of rights and individual autonomy. Nowhere does she imply that this perspective is "soft" or "pliable". In fact in the abortion study, Gilligan goes to great lengths to emphasize the level of complexity and sophisticated deliberation employed by women when taking into account the needs of all persons involved in the dilemma.[47]

Baber's final point is that even if Gilligan has gotten hold of something, and there are some patterned differences in male and female approaches to certain kinds of problem solving, it should still be downplayed because it is an idea that could be too easily abused and used to justify the prevention of women from reaching equal status in the society. Clearly, the fact that an idea can be misused and misinterpreted is not reason enough to prevent the exploration and understanding of that idea. If Baber's point is just an enlightened warning, then feminist epistemologists can acknowledge it and continue on with their work. However, Baber seems to have something more in mind.

Feminist epistemology and philosophy represent the "pink ghettos" of women's work that Baber finds abhorrent.

Ironically, her reaction perpetuates the undervaluation of "female" identified labor. She sees little worth in countenancing an area of scholarship described as "feminist epistemology" and denies the opportunity it could provide for those philosophers who seek to analyze institutionalized systems of knowledge, value and power. Moreover, she sees danger in accounts that rely on gender divisions to explain how knowledge is defined and produced.

As an example, Baber cites Ellen Swallow Richards, the first woman to attend MIT, a chemistry major, and the founder of "home economics" in the 1880's, as an example of a pink ghetto builder similar to feminist epistemologists of today. Richards, as Baber describes her, was happy to use science to formalize the domestic duties associated with women rather than pursue the more equalizing route taken by feminists of the time who sought entrance into male dominated fields. Richards is portrayed by Baber as a naive conspirator, aiding sexists of the day to keep women confined to less worthy roles in academic and social life.

Yet, Baber fails to mention the ways in which Richards was not such a sexual segregationist. In 1878, soon after MIT began admitting women directly, a lab was set up to study sanitation. The lab was the first of its kind in the United States. Richards, who had earned her degree in chemistry, was hired as an instructor of sanitary chemistry and held the position until her death.[48] Swallows worked

with MIT professors analyzing water samples. This experience led to her interest in the composition of food and groceries, safe-drinking water and low-cost diets for the poor.

In 1889, Richards started the "New England Kitchen" where she and several of her female students from MIT prepared nutritious soups for the city's poor. It was this experience, she was quoted as saying, which convinced her that nutrition science could provide the opportunity to understand and help solve social problems.[49] This conviction was the theme of her address to The Association of Collegiate Women in 1890. Richards lectured on the need not only for women to do science, but the need for science to embrace the unique perspectives women could offer.

Richard's work could be interpreted as radical in that she struggled to have science be shaped and directed by women and what they knew, rather than have women conform to a field they did not have a hand in organizing and which was in conflict with their social experience and sense of responsibility. The discipline of Home Economics that Richards originated, arose from a sphere traditionally assigned to women, yet it challenged that sphere's assigned boundaries and used its sources of strength.

The fact is that Richards and women like her, who sought to enlarge the opportunities available to women and who took seriously so called "feminine" work, experienced

ghettoization not because they saw their work as separate and less valuable, but because others did. Critics like Baber who feel as she says that they must "actively distance themselves from such enterprises remaining aloof from women's organizations in the profession even denying that they are feminists"[50] relegate feminist epistemology to the ghetto not because they have cogent reasons and valid philosophical differences, but because they fear that by condoning the study of feminist issues in philosophy, they will incur guilt by association and lose the ground they have struggled to earn. Hence, the "Detriment Argument" comes down not to an argument, but to the fact that feminist epistemology is too closely identified with women and as such, it has the potential of becoming the "home economics" of philosophy. Rather than risk this possibility and have talented professional women go into such a segregated and undervalued area, proponents of the argument wish to do away with the field altogether.

B. The Unimaginability Argument

The second type of criticism, I will call the "Unimaginablity Argument". This criticism, typified in Barry Gross' article "What Could A Feminist Science Be?"[51] makes the claim that science, and the epistemological reconstruction of scientific methodology, is a gender neutral enterprise involving such non-social concepts as

"evidence", "justification" and "confirmation". Gross' central concern is illustrated in the example he gives of a murder. Jones is standing over Smith who lies dead on the floor. Jones is holding a smoking gun. This is good, albeit not conclusive confirming evidence that Jones killed Smith. Then, as Gross asks in frustrated astonishment; How would this fact be understood any differently in a world that incorporated a feminist view of science?[52]

Gross' inability to imagine something like "feminist science" and therefore a feminist epistemology that describes the methodology of this enterprise, is due mostly to what he describes as the "very large and ambitious nature of the project. It is so large a project that one is hard-pressed to believe that anyone even thinks it could be carried out in some stepwise fashion."[53] The excessive magnitude of the project, as Gross sees it, stems from the fact that feminist science entails an unimaginable "reinventing" of traditional science. While Gross grants that the tools of science may have been used in oppressive ways, the essential and time-tested methods and techniques of science seem themselves to be neutral with regard to social and political issues. So therefore, to reinvent science in the light of political concerns would do nothing imaginable to change the essential characteristics of scientific methodology.

Gross makes a comparison with a transportation system.

If a racist, sexist society used a transportation system, and that society experienced a political revolution and restructuring such that it was no longer racist and sexist, that would not change the essential features of the transportation system; it would still be essentially a system whereby goods and people were carried by some mechanical means over distance in a reasonable time. As Gross says, a "feminist transportation system" is an absurd notion in the same way that "feminist science" is.[54]

The first problem with the way the question is posed by Gross is his uneasy talk of "a" scientific methodology. Compare these two quotes from his article:

> I) Parenthetically, it hardly seems to make sense to talk of science or method as a global phenomena. There are different sciences and different branches and different subfields and specialties within them.[55]
>
> II) One can no more have a science that eschews *all* the time-tested methods and techniques of the natural sciences as we know them - than one can have the transportation system just described.[Emphasis Mine][56]

Comparing these two quotes, a tension can be felt in Gross' wish to admit the variety and complexity of science, while at the same time being committed to some kind of an essentialist characterization. However, the features that make up the essential methodology of science are left by Gross, to the imagination. He gives a few examples of medical achievements throughout history to show the positive results of traditional science, (a reminder to the "anti-

authoritarian" feminists who he thinks seem to forget the "miracles of medicine"[57]) but he never offers the kind of clear picture of the essential elements of science that he does for a transportation system.

Now this could be chalked up to the fact that it is difficult to elucidate something like the essential features of the complex set of systems known as "natural science". However, if this is true, Gross should show some restraint in making an argument on the basis of unimaginability. If there exists a complex system whose foundations are difficult to characterize, and a proposal is on the table for changing some basic tenets of this system, then the ramifications of the change could be vast and intricate enough that to simply imagine them would be difficult. It would seem more correct to understand Gross's question, not as a matter of a priori analysis or unimaginablility but rather as a matter for a posteriori investigation within a knotty problem in the philosophy of science.

Putting aside the issue of simple unimaginability, a more interesting concern raised by Gross is the question of "particulars". How would the particular aspects of science, elements like `evidence', `justification' and `confirmation' be altered by a feminist perspective in epistemology and science.

If we return to Gross' original example, how would a feminist perspective alter the judgement of evidence we now

make as to the fact of Smith's being a likely suspect in the slaying of Jones? The simple answer to Gross' question is that it would not alter *that* judgement in any way. The question of evidence that Gross presents contains none of the controversial features that are in dispute for feminist epistemologists. It is not a point of contention for feminist epistemologists to argue that *every* human inference involves a bias toward the undervaluation of women and non-white males, the continuation of a discriminatory social structure and the reverence for white western male virtues. Contrary to straw-man stereotyping, feminist theorists do not categorically see biased inferences being drawn everywhere. Such a characterization provides critics with a reason not to take feminist arguments seriously. The thinking goes: if you see discrimination everywhere then you are not seeing it anywhere specifically and hence you have an exaggerated and useless analysis. As a result of this way of thinking, the actual "particulars" of feminist criticism are lost.

To be more accurate, feminists do see bias in many places, even in more places than some might imagine, but it should be from within the range of cases criticized that the debate is framed, not in examples like the one Gross gives which is irrelevant to the discussion. The example is irrelevant because Gross seeks to establish a point about the role of evidence in science and the case he gives could

only be settled in a court of law. The methodology and practice of science and law differ; hence the burden is on Gross to explain why he connects them.

Separating out the error in Gross's conflation of the question of evidence, we can look at legal theory and science in turn. Feminists working in law have marked-off some important cases which can help to shed light on the question. If we exchange Gross' murder case with one of these, the question of evidence is no longer so simple to answer. For example, historically in legal theory the notion of "social equality" has meant that equals should be treated equally. This has meant that if men and women are equals, they should be treated according to law, in the same way. However, some feminist legal theorists have argued that this sense of "equality" has unfair implications for women since it ignores the differences in the social realities facing the two genders.[58] In a 1986 case, *Rabidue vs. Osceola Refining Company*[59] a three member committee of federal circuit court judges was appointed to the case to determine whether or not the plaintiffs counsel had demonstrated legitimate evidence of sexual harassment. The evidence in question included displays of posters of nude or partially clad women and vulgar and obscene language. The plaintiffs maintained that the evidence in question resulted in a "hostile and intimidating work environment"[60]. Two out of the three members of the special committee ruled in favor

of the defense. As to the obscenities, the court wrote: "...although annoying, they were not so startling as to have affected seriously the psyches of the plaintiff or other female employees."[61] Further, the poster displays were ruled to have a "de minimus effect on the plaintiff's work environment when considered within the context of a society that condones written and pictorial erotica at the newsstands, on prime-time television and at the cinema."[62]

In a scathing dissent which has raised considerable support from feminists; Justice Damon Keith, the one judge who ruled in favor of the plaintiff, blasted the majority ruling for "failing to see the overall circumstances".[63] In his review of the original court record, Judge Keith found that one of the supervisors in question used anti-female obscenities, including references to women as "whores" and "bitches". In addition female workers were discouraged from lunching with male customers while male workers lunched regularly with the firm's female customers.[64] Keith argued that the supervisors had a "primitive" view of women and an "anti-female animus."[65]

This extremely disparate view of the facts and findings of sexual harassment in *Rabidue* indicates that the majority and the dissent operated from different underlying assumptions about evidence of sexual harassment and sexual hostility. The majority assumed that men and women were equal and used a "reasonable person" standard to decide that

women should not have judged the workplace to be "hostile".

The dissent on the other hand utilized a "reasonable woman's" standard because Judge Keith found the reasonable person's perspective "failed to account for the wide divergence between most women's views of appropriate sexual conduct and those of men."[66] He went on to write "Unless the outlook of "reasonable woman" is adopted in cases of this nature, the defendants as well as the courts are permitted to sustain ingrained notions of reasonableness and evidence fashioned by the offenders."[67]

Now if we compare this case with Gross' case of Smith and Jones, we can see how in the Gross case a smoking gun is an instance of evidence that is given no new light by a feminist perspective. However, in cases of harassment, the question of evidence and its relationship to feminist theory raises significant issues.

To move from law to science, a feminist perspective on the question of evidence also applies in a range of relevant cases. Consider the following examples, the first is from the field of obstetrical science. In the United States today 97.4% of all births in hospitals are conducted with the laboring woman lying on her back in a bed or delivery table[68]. The most common position, known as the "lithotomy position", has the woman flat on her back with her legs up in the air in stirrups. In Harry Oxhorn and William Foote's 1980 text entitled *Human Labor and Birth*, it is explained

that the theory behind the decision to use the lithotomy position is a result of the following evidential advantages. First, that it "provides more complete freedom from pathogenic organisms, easier for hospital personnel to monitor the fetal heartbeat without having to ask the woman to sometimes change position, easier for hospital personnel to administer drugs, easier for the doctor to see the birth, good position for the use of forceps."[69]

The evidential disadvantages include "risk of supine hypotensive syndrome, sacroiliac or lumbarosacracal strain, possible thrombosis in the veins of the legs, possible nerve damage, and the danger of aspiration of vomitus".[70] The advantages are highlighted and shown to be consistent with a wider range of obstetric theories thus risking the possible disadvantages. Alternative modes of birthing such as the squatting position which are explained as "enlarging the pelvic outlet, and enabling the laboring woman to use her expulsive forces to a greater degree and reduce the risk of hypotensive syndrome"[71] are rejected as reasons to adopt the position because "it presents problems for the accoucheur to control the birth and to manage complications as well as making it impossible to administer drugs."[72]

In this case, the question of relevant evidence depends upon a framework which is committed to the preservation and maintenance of "control" for the medical personnel and sacrifices the physiological needs and comfort of the woman.

Proponents of midwifery[73] look at the same data and see evidence for a different approach to be taken toward childbirth. Assuming a framework in which the goal is not only the avoidance of death and ease for medical personnel but also the quality of care and the satisfaction experienced by the birthing woman, those who practice the "art" of midwifery rely on evidence for their practices which depend upon a range of complex criteria incommensurable with obstetric science. The evidence in favor of midwifery and the arguments by feminist theorists against various obstetrical theories has recently resulted in the replacement of some obstetric and gynecological practices in European and North American hospitals with a model involving a choice of birthing positions, a reduction in drugs and surgical procedures and the inclusion of loved ones in the birthing room.[74] These results prompt a rethinking of what "good" scientific practice involves regarding judgements of evidence in the field of obstetrics and gynecology.

Shifting to the area of cell biology, in a recent paper by The Biology and Gender Study Group entitled "The Importance of Feminist Critique for Contemporary Cell Biology"[75] the authors explain that questions of gender bias should be posed at the outset of any scientific research program so as to provide critical rigor and avoid possible errors. The focus of the group is on the ways in which

cultural norms and gender inequalities have led to the formation of particular interpretations in biology that excluded or ignored available valid evidence.

One example the study cites involves a well entrenched theory considered to be a "textbook" explanation of fertilization[76], which purports the ovum to be a passive participant in the act of fertilization and the sperm as "the active agent that must move and penetrate the ovum. The egg passively awaits the sperm which contributes the activating agent..."[77]

However, in recent investigations by Gerald and Heide Schatten[78] using electron microscopy, they were able to show that when the sperm contacts the egg, it does not "burrow through". Rather the egg directs the growth of microvilli - small finger like projections of the cell surface - to clasp the sperm and slowly draw it into the cell. The phenomena of microvilli extending to the sperm has been known since 1895 when E.B. Wilson published the first photographs of sea urchin fertilization. But as the study group says "this evidence has been largely ignored until recent studies, and its new role remains controversial in the field."[79] The Schatten's in their writing, attribute a rethinking of standard sex-role stereotyping as an influence in their investigation of microvilli. The members of The Gender and Biology Study Group credit this kind of rethinking to the increase in feminist critiques of cell and molecular

biology. They refer to the work of biologists Ruth Hubbard and Marian Lowe and their research for the Committee for Responsible Genetics and the research of Eva Eicher and Linda Washburn of the Jackson Laboratory on genetics and sex determination.[80] The Group sums up their report with the comment: "A feminist critique of molecular and cell biology involves being open to different interpretations of one's data than is traditionally taught and having the ability to ask questions that would not have occurred within the traditional context."[81]

These examples show that the concept of evidence and its extension, as well as the related concepts and extensions of justification and confirmation, are not only possible to explore from the perspective of gender, but may actually be more adequately understood by the normative controls this perspective provides when gathering evidence and analyzing data. Hence Gross' inability to see a relationship between gender and evidence is a result of his failure to consider relevant cases.

C. The Perspective Argument

The last criticism of feminist epistemology I will address concerns the question of a woman's perspective or standpoint and is raised by authors such as Alan Soble[82] and Susan Haack[83] in their writing. The criticism maintains

that there is something paradoxical in the view that the perspective of women is essential for science since there does not seem to be something like "the perspective of women". A characterization of women's essential perspective can be challenged by any woman who argues that it fails to capture *her* experience. Moreover, if the view is reformulated to mean only a "feminist woman's perspective" then there is no reason to include non-feminist women in science; unless the point is that all perspectives should be included in science. However, assuming this is the case we will easily be led into a degenerate pluralism whereby a feminist perspective offers nothing unique to science. What is needed is some argument that will give us identity conditions for a person's "social location" and how these conditions contribute to something novel in the production of scientific knowledge. Hence, we need to get a handle on the notion of "perspective" otherwise the identification of perspectives will have no predictive force. So in essence the criticism comes down to a challenge to make clear what a "feminist perspective" is and how it could uniquely contribute to science and epistemology. I will call this objection "The Perspective Argument".

Both Alan Soble and Susan Haack are proponents of The Perspective Argument. Soble, in investigating the writing of Evelyn Fox Keller, finds that Keller's arguments regarding a "feminist style of thinking" are groundless. The

result for Soble is a general skepticism regarding any account of the role of gender in scientific knowledge. Haack's argues that most of the accounts feminists have put forward are significantly defective for two reasons. One, the accounts fail to take into consideration the work of mainstream philosophers of science; specifically Carnap, Hempel, Popper, or Feyerabend. Haack notes an occasional reference to Kuhn, but not enough to merit the omission of these other important theorists in the philosophy of science.[84] Secondly, feminist philosophy of science and epistemology attempt no serious analysis of the concepts of rationality, objectivity or value-ladeness, which are crucial for their arguments. Haack, like Soble, concentrates on Keller since in her view, "Keller is the most sophisticated and thoughtful representative of the feminist critics of science."[85] Other representatives of feminist epistemology and philosophy of science such as Rose, Haraway, Weinrich-Haste and Easlea[86] are rejected for various reasons by Haack. Rose's work is rejected because it is "so clotted with Marxist jargon as to be unreadable", Haraway's because Haack had to struggle with "convoluted banalities of recent French philosophy" and Weinrich-Haste's because it was, in Haack's words "titled in such a repulsively cute way that I was tempted to not read it at all." Finally with Easlea's article, Haack says that it had such a "broad streak of wooly romanticism, I probably missed

its substantial points out of sheer irritation."[87]

Haack's admitted frustrations and biases are not reassuring if we look to her work hoping for good philosophical reasoning. Her argument is a clear case of an ad hominem abusive attack; thus offering no insight into the worth of the arguments put foward by the philosophers she criticizes. Haack's ultimate assessment of feminist epistemology and philosophy of science, based on her reading of Keller, is that if it is the case that girls and women are brought up in such a way that they employ methods of reasoning and objectivity in a way that is different from what is required by science, then girls should be brought up differently. What should not follow is that science and epistemology be required to account for this deficiency in the upbringing of women. However this is not to say that Haack rejects looking at science from a feminist perspective. She writes:

> Some no doubt, would regard the whole project of feminist science and epistemology as absurd. I think they would be wrong; for looking at science from this perspective one encounters, from a promisingly unfamiliar angle, a whole host of good, hard questions: about the internal organization of science, about the role of science in society, about the character and status of scientific methods, about scientific language and metaphor....But it would be equally wrong to imagine that a feminist approach can give simple, easy answers to these good, hard questions. In the work that I have been discussing, regrettably, the soggy and self-indulgent predominates over the detailed and discriminating.[88]

I will not take issue with the accounts that Haack attacks

because they are in my view, easy targets. I say this because these philosophers are intentionally challenging the paradigm of analysis typical in most philosophical writing and as such are striving to evoke a more personal, literary style. It is not surprising therefore that Haack, an analytic philosopher and logician, fails to see "good, hard work" in these articles. I think more difficult for Haack to dismiss as "soggy" or "self-indulgent" are the theorists that I will discuss in what follows; philosophers such as Longino, Antony and Nelson whose work is very much in the tradition of analysis.

I want to note a conclusion of Haack's, namely, that if girls and women reason in a way that is different from what is required by science, then this difference need not be of interest to theorists of science. Given this view, I am puzzled as to why Haack thinks the questions a feminist perspective raises are "good" ones. In what way could the results of such an investigation of difference yield relevant information for philosophy of science and epistemology if, on Haack's view, science and epistemology are justified in their present construals of reason and objectivity?

I will leave this question to turn to the general problem raised by both Soble and Haack, the problem of trying to account for a "feminist perspective."

Feminist accounts of perspective appear most often in

the work of "feminist standpoint theorists". This is the view shared by philosophers such as Dorothy Smith, Nancy Hartsock, Alison Jagger and Hillary Rose[89]. Generally speaking, standpoint theory entails the claim that women, because of certain aspects of their experience, possess a privileged position which provides them with a unique perspective. Feminist standpoint epistemologies take a number of different forms, but common to all of them is an appeal to women's unique association with what has been judged as undervalued labor and irrelevant activity by post-Enlightenment science and philosophy.

In "Hand, Brain and Heart"[90], Rose looks to the craft-organized areas of inquiry for a model of a feminist science that can counter the dualisms of Enlightenment thought. Her vision of a transformed, feminist science involves what she calls the "unity of hand, brain and heart" in the process of scientific inquiry. In a later paper[91] Rose broadens this argument to include a model of a feminist epistemology. Following two basic Marxist principles - -one, that a materialist human knowledge must come from practice, and two, that oppressed social positions offer a privileged epistemological access - - Rose argues that women's devalued labor offers them a distinctive knowledge of the social and natural worlds.

Hartsock, who also argues for a feminist materialism, asserts that we must move beyond both Enlightenment and

Marxist epistemologies. She claims that women's social role, relegated to relational and contextual perspectives, allows them to understand aspects of nature and social life not available to Enlightenment epistemologies. In addition, Hartsock transcends Marx in that she sees women's labor as a more fundamental category than proletarian labor because women's labor is more closely tied to the basic physical and emotional needs of all human beings. Hartsock, like Rose, wants to offer a successor science based on this unique feminist perspective.

Feminist standpoint epistemologies are appealing to feminists who are searching for an alternative to institutionalized knowledge. They combine a critical attitude with an emphasis on the strengths and qualities that have been associated with the feminine and, consequently been devalued. However, as I will show, this position as it is argued for currently, is not the answer to defining a feminist science and epistemology.

The initial problem with standpoint epistemology is that it fails to clearly demarcate those aspects of the social order that are influential in the formation of a "feminist perspective". "Women's labor" is given as the most significant determiner of a women's perspective. However this criterion ignores the influences of other significant factors such as socialization and the variety of social ideals, moral judgement, and education.

Secondly, even if it is possible to get a handle on the relevant factors that contribute to feminist epistemological perspectives, we would still not have a normative philosophy. Standpoint theories could provide a starting place for understanding the origin of a feminist and social epistemology but they would not provide "grounds" for this knowledge. Yet standpoint theorists in more extreme versions[92] want to do more, claiming that marginalized perspectives should be a starting place for scientific theory formation because of the "superior" perspective this position affords. In more moderate versions, standpoint theory argues for the suspect or incomplete quality of theories which result from exclusion and marginalization. This claim seems not only reasonable but somewhat obvious. Hence, what is needed is the crucial addition of an interpretation of neglected perspectives such that a critical argument regarding the limitations of traditional epistemology can be made. Assuming this to be the case, present feminist standpoint theories provide only a necessary - not a sufficient - condition for defining feminist epistemology. The Perspective Argument serves to remind us that a)"perspective" has not been adequately defined and; b)perspective alone will not offer a well formulated criticism of traditional epistemology.

Given the difficulty of trying to characterize both the factors that contribute to a "feminist perspective" and a

normative epistemic principle, is there still a chance for defining the project of feminist epistemology? In my view, the answer is yes. Following a proposal by Terri Elliot in her paper "Making Strange What Had Appeared Familiar"[93], feminist epistemology can be understood firstly as a reconstruction of a *pattern* of marginalizing experience. This is not to say that the experiences of persons who are socially marginalized are the same. However, it is also not to deny that an analyzable pattern emerges in the investigation of the experiences of members of social groups. The emerging pattern serves as the object of investigation in a feminist or social epistemology.

Elliot's account advocates a more moderate version of standpoint theory and provides an epistemically useful analysis of perspective. Her view resurrects Heidegger's notions of "readiness-to-hand" and "presence-to-hand" to shed light on the question of perspective.[94] For some, engagement with the world involves circumstances and objects which are ready-to-hand in that they are easy to use and present no noticeable obstacles. So for someone who is healthy and able to walk, a flight of stairs presents no noticeable challenge and therefore is ready-to-hand. However, for others, the same situation can result in a presence-at-hand, in that it presents noticeable obstacles and a *conspicuousness* regarding the particulars of the situation. So for someone who is bound to a wheelchair, the

flight of stairs become present-to-hand and noticeably unusable.

Elliot's point in bringing up the distinction is to show how aspects of the social order are conspicuous for marginalized persons in that they are unusable for them. Given that gender is the earliest and most pervasive determiner of social role and expected behavior, and gender inequalities exist, women are in a position to notice how certain aspects of institutional knowledge are merely "present-at-hand" for them and hence problematic. These experiences can arise in a range of circumstances - from the way in which labor is divided to how scientific methodology is understood.

So, for example, the pervasive use of sexual metaphors in the description of scientific theory construction provides insight into the question of unusability. Nobel Prize winning physicist Richard Feynman in his *Lectures on Physics*[95], describes his love for a theory as similar to "falling in love with a woman. But as in love, with time her faults start to become apparent and then the theory, like an old lady, has very little that is attractive about her. The best we can then say about her is that perhaps she is a good mother and gives birth to a beautiful new theory that will have all the men's hearts pounding."[96] Paul Feyerabend, in explaining why his theory regarding the rational reconstruction of science is preferable to Popper's

says "Such a development, far from being undesirable, changes science from a stern and demanding mistress into an attractive and yielding courtesan who tries to anticipate every wish of her lover."[97]

The sexual connotations rife in metaphors of this kind, including examples of "hard" or "soft" science, "rigor" versus "softness", or the "seminal" quality of an idea, have been described extensively in the literature of feminism.[98] My purpose in bringing them up here is to provide one example of how our public picture of science can present women with a problematic situation. Encountering such descriptions of science may not present a woman with an entirely unusable situation, but it requires her having to work around an obstruction in deriving the meaning of the metaphor. On the other hand, for a man, the same description can be an invitation to join in a collective enterprise, presenting no noticeable obstacles to his sense of self.

Now as Elliot makes clear, this does not mean that unusability will necessarily be the experience of all woman. For some, they may be able to be engaged, interpret circumstances as ready-to-hand and not feel hindered by the predominance of male references. How can we understand this?

Elliot explains that a perspective on the unusability of certain aspects of social experience is something earned,

it is not a birthright. The emergence of an individual perspective on unusability may then be joined by others to reflect a repeated pattern. When this occurs several things can follow. For those who did not see the unusability previously, the fact of its exposure may result in a dawning cognizance; a realization first, that one has been working to overcome obstacles and second, that these obstacles do not have to be there. In this way women, who share some characteristics in virtue of their gender, become engaged with a network of others who voice certain critical aspects of their experience. For those women who do not perceive the unusability in *their* experience, there is no obligation to share the perspective, only to recognize how limiting aspects of the world are for others. Moreover, contrary to theorists like Hartsock or Harding, to describe a pattern of marginalizing experience is not to point to a perspective which is epistemically privileged. Rather, what we find is a perspective that is uniquely sensitive to circumstances which assume value-neutrality but contain elements of exclusivity and marginalization. This information is significant as it can expose the apparent implementation of objective methods of reason and justification, which may be far less than reasonable or objective. But the perspective does not address directly, the question of how to evaluate these procedures assuming their inherent reasonableness or objectivity. As such, they are informative and potentially

corrective but not epistemically superior.

Yet the feminist perspective can have normative force on two fronts. The first comes from the demand put upon societal institutions to acknowledge the reality of systematically exclusive and unusable circumstances. The second involves the development of methodological principles that can be used to uncover the exclusive nature inherent in background assumptions and evidential criteria. The kind of models that will best meet these requirements will need to capture what is unusable, from the point of view of gender, and suggest alternative conceptions that will lead to greater useability and consequently, more complete knowledge. Such models will be significantly strengthened if they can provide an analysis that poses only a minimal challenge to the fundamental logic of research as it is understood in various fields and to the logic of explanation as is understood in the mainstreams of epistemology. By addressing the limitations of the prevailing conceptions of method and explanation, and showing how these conceptions constrain and distort the results of research, feminist epistemology can be understood as a call for greater rigor in the analysis of knowledge. This direction would make it possible for many people to grasp the importance of feminist accounts of knowledge while they are using the various methods and norms of their respective research traditions. Recently, philosophers such as Helen Longino Lynn Nelson,

and Louise Antony, all of whom will be discussed in the chapters that follow, have developed promising proposals very much along these lines.

Returning to the Perspective Argument, we can respond to the concerns it raises by recognizing that what should underlie an adequate feminist epistemology, or any socially motivated account of knowledge, is not a monolithic group perspective but rather a pattern of repeated unusability that has been overlooked or suppressed in more traditional accounts of knowledge.

III. Conclusion

Having considered and rejected the various skeptical challenges to feminist epistemology, it is time to move to the next step in the argument. Before doing so, I want to "take stock" by summarizing where the preceding discussion leaves off. First, the possibility exists for an account of knowledge and scientific inquiry that is informed by a feminist perspective. Secondly, such an account should involve both a descriptive element, reflecting existing patterns of marginalization (or what I referred to earlier as "unusability") and a normative or prescriptive element regarding the appropriate criteria for the acceptance of evidence, justification and confirmation. Lastly, the most promising accounts will adopt some form of "conservativism"

whereby prevailing models of logic and methodology are not only made consistent with, but improved by a social analysis.

In Chapters Two and Three, I will look at two types of such proposals and ultimately reject the naturalized assumptions they carry. An alternative epistemic account is argued for in Chapter Four. However, before I move to these arguments I want to address one brief point regarding feminist epistemology and philosophy of science.

It is conceivable that someone could accept all of the reasons I have given above for the legitimacy of looking at epistemology and philosophy of science from this wider perspective but not accept that this work is particularly "feminist". I would argue against this claim on the grounds that the link between knowledge and social and political factors builds directly on the critiques of science and individualism made by feminists in the early literature of feminist philosophy of science. Moreover, in none of the more mainstream accounts of society and knowledge, including the work of philosophers such as Wittgenstein, Quine, Kuhn or van Frassen, is there a recognition of gender and power in describing social relations. This contribution seems to me to have uniquely come out of the work that is described as feminist.

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CHAPTER TWO

The Holistic Model

I. The Problem

In the feminist research discussed in Chapter One, what we find is the development and defense of what could be called "group-ideal consistent theories". That is, theories that are broadly speaking, consistent with and supportive of the general aspirations, assumptions, intuitions and even political aims of the social group. In other words, these are not simply theories about women, but theories of a particular type about women. Specifically, they are theories which exhibit a consistently emancipatory implication for women's perspective and position in society.

Research of this type is "problematic" in the Kuhnian[1] sense in that it presents an anomaly which fails to accord with the expectations of normal science. Normal science has struggled to establish a tradition of "purity" in which external non-scientific values are considered antithetical if they enter into the objective standards of methodology. So, researchers who identify data and characterize methodology as socially and politically emancipatory, are incorporating standards of justification into science that are guided by personal belief and ideology. Given this, the view that science needs to explain "experience" in group relativized, social terms is bound to present a problem for

those who view science as essentially providing us with objective knowledge or truths about the real, physical, psychological or social world independent of political ideology and social bias.

Hence, any explanations of the role of social factors in the acquisition of knowledge have generally challenged traditionally epistemology and been subsumed under naturalized epistemology, a branch of psychology. Within the area of social epistemology known as "feminist epistemology" accounts of the categories of "gender" and "social group" have been analyzed using a variety of naturalized models ranging from postmodern critiques of reason to recent empiricist philosophy of science. From within these accounts, I will focus specifically on those models that draw upon the resources of the Anglo-American tradition and attempt to transform the apolitical discourse into one useful for explaining the significance of gender issues and inequalities in epistemic theories. The model I will consider first is the Holistic Model.

II. The Influence of Quine

The first epistemological model to be discussed is the Holistic Model. This model borrows heavily from the writings of C. S. Peirce (1940) and more prominently W. V. O. Quine (1963) and is exemplified in the recent writings of philosophers Lynn H. Nelson (1990, 1994) and Louise Antony

(1992,1993).

Quine's development of the Holistic Model was a response to the failures of previous positivist philosophy of science. The positivist program involved showing how any empirically meaningful claim could be reduced, by the application of semantic and logical rules, to statements purely about sensory experience. Such a "rational reconstruction" if possible, would show how all theoretical disagreements could be resolved by an appeal to the neutral domain of empirical experience. Essential to this project was that a viable distinction could be drawn between statements whose truth depended upon empirically contingent matters of fact and statements that were true "by convention" and dependent upon the logical and semantic structures of the theory. In addition, it would have to be shown that the reduction of empirically meaningful claims to the specific claims of sensory experience could be carried out in some clear fashion.

Quine was among the most decisive critics of this project, arguing that statements do not have any specific consequences if understood individually or in isolation. If follows that no single statement of experience or observation can refute any theoretical claim or resolve a theoretical dispute and therefore only a conjunction of hypotheses could do this. Moreover, Quine maintained that there was no way to distinguish between statements that were

true as a matter of contingent empirical fact and those that were true in virute of logic or semantical conventions. Hence, Quine proposes a Holistic Model.

The distinctive feature of the Holistic Model is that it uses a coherence criterion for theory-choice in relation to a sufficiently comprehensive and coherent body of beliefs. Quine explains the way the model works in the following way:

> Total science is like a field of force whose boundary conditions are experience. A conflict with experience at the periphery occasions readjustment in the interior of the field... But the total field is so underdetermined by its boundary conditions, experience, that there is much latitude of choice as to what statements to reevaluate in the light of any single contrary experience. No particular experiences are linked with any particular statements in the interior of the field, except indirectly through considerations of equilibrium affecting the field as a whole.[2]

The affect of this model on our conception of theories and justification is to make them part empirical content/part value judgements. As Quine says in the following passage:

> If this view is right, it is misleading to speak of the empirical content of an individual statement...Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system. Even a statement very close to the periphery can be held true in the face of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called logical laws.[3]

As a result, the empirical content and the pragmatic value of adopting a given statement cannot be sharply differentiated. Coherence is the criterion that guides our

choice among theories, and what the theory coheres to is generally the more central, rather than the more peripheral parts of our web of belief. Even so, as Quine explains it, those statements at the periphery may be held true if we make the necessary adjustments elsewhere in the web.

The center of the web of belief will contain those statements held more in common by more theories, those statements which will require the greatest amount of adjustments throughout the web if they themselves are altered. The justification of theory-choice thus involves on this model, not the mere linking up of individual empirical claims to similarly limited though more general empirical claims, as on the old positivist model, but involves to various degrees of significance, coherent relations of deduction, explanation and probability with our currently held theories.

Traditional positivist empiricism viewed epistemology, as "first science"; a theory that described the "extrascientific" foundation and provided the "prescientific" justification for science. But Quine recognizes that no account of theorizing, no framework for explaining our theorizing, can provide such a foundation or play such a role. Given the necessity of theory for our coherent sensory experience and the understanding that, in the end, whatever evidence we have for science is sensory evidence, there can be no such foundation for science. For

these and other reasons, Quine maintains that epistemology is firmly within science. Thus, the role of epistemology on this view is explaining how we have gone about constructing our theories. It does not justify these theories. However, as Quine explicates it, we may still say of a belief or theory that it is warranted, and what it means to say that not all beliefs and theories are equally warranted. The criterion of coherence with other beliefs allows us to make these judgements. We are warranted in so far as the belief or theory in question accords with our experience in general. However, these judgements can only be made for Quine, from within the perspective of an already accepted framework of understanding. In a number of places Quine draws on a metaphor of Neurath's to describe both the relationship between a theoretical framework that is accepted and new questions, and to underscore the lack of extra-scientific vantage points.

> Neurath has likened science to a boat, which if we are to rebuild it, we must rebuild plank by plank while staying afloat in it.[4]

> Our boat stays afloat because at each alteration we keep the bulk of it intact as a going concern. Our words continue to make passable sense because of continuity of change of theory; we warp usage gradually enough to avoid rupture...We are limited in how we can start even if not in where we may end up. To vary Neurath's figure with Wittgenstein's, we may kick away our ladder only after we have climbed it.[5]

Given this view, the task of epistemology, according to Quine, is discovering how the beliefs we have, have been

acquired. This ultimately, is a task for empirical psychology. Hence, Quine advocates a "naturalized epistemology". His description of science is as a "single fabric". The "whole of science" for Quine, incorporates more than scientific theories; as he states it, the fabric includes "everything we ever say about the world"[6]. This system of sentences is to be viewed as both empirically determined and meaningful in virtue of empirical reality.

This claim marks a clear break with positivist and foundationalist views. Within those traditions the meaningfulness of mathematical and logical sentences is insured, despite the fact that these sentences are interpreted as non-empirical, by understanding them as analytic, that is, true in virtue of their meaning without reference to empirical facts. But on Quine's view the proposed distinction between analytic sentences without empirical content, and synthetic sentences with empirical content, is a false distinction. Quine's view depends on two related arguments. The first, that there are no "things" that are meanings and second, that translation is indeterminate.

The first argument appears in "Two Dogmas of Empiricism". Here Quine maintains that the traditional notion of analyticity rests on a notion of synonymy, and in his view it is not possible to make objective sense out of the concept of synonymy. As a result there are ultimately

no meanings. Quine explains that in science, one cannot claim that an object of study exists without having conditions for type identifying such objects. For example, physics can't claim that atoms exist without having conditions for type identifying atoms. With regard to meanings, the identity condition assumed has been the notion of synonymy or sameness of meaning. So, if there are meanings there must be a clear and objective notion of synonymy. Quine examines three possibilities in which a clear notion of synonymy might be found.

The first is, "Definition". According to Quine, there are three sorts of definitional activity. They include paraphrase or lexicography, explication and abbreviation. With regard to paraphrase, Quine maintains that this form of definition reports and compiles pre-existing synonymies. As a result this form of definition presupposes rather than provides an explanation of synonymy. With explication, Quine says that this form of definition does not attempt to improve upon or clarify pre-existing synonymies. However, explication makes improvements on definitions in that they provide new defining explanations which are synonymous with the term to be defined specifying a certain context. Yet this too presupposes rather than provides an explanation of synonymy. Finally, with abbreviation, Quine maintains that this form of definition merely creates synonymies by an arbitrary decree; that is it merely gives examples of

synonym pairs consisting of a word and an abbreviation. Quine argues that this is just stipulation not an explanation of synonymy.

In all three cases of definition the important thing Quine wants to show is that the class of definitions are a circle. Each attempt at a definition leads us back to other members of the circle. However, what we need for clarity and illumination is to break out of the circle.

Quine next considers a second possibility for an objective notion of synonymy; logic or semantical rules. Carnap attempted to define analytic truths as those truths which are derivable from laws of logic and semantical rules.[7] Semantical rules, on Carnap's view, are a species of logical law, a species which formalize the inferential features of extralogical vocabulary.

Quine argues against Carnap's proposal on the following grounds. First, Carnap does not provide semantical rules which define words in more than one natural language. Thus semantical rules do not by themselves generate translations between various natural languages. Rather, translation between natural languages requires the establishment of bridge principles between semantical rules in different natural languages. This account of translation is a shortcoming in Carnap's account of analyticity in that it provides no general account of meaning, that is, no account of meaning for variable sentences and languages. And Carnap

requires a general account of meaning to base an argument that there is a clear and objective notion of synonymy and analyticity. So, Carnap's attempt amounts to defining analyticity in terms of semantical rules, an understanding of which is not independent of an understanding of analyticity. Once again the definition of analyticity is circular and thus uninformative.

Finally, Quine considers linguistics or interchangability as a source for an objective notion of synonymy. If synonyms are interchangeable in statements preserving truth, we could propose a definition of synonymy based on interchangability. But the question arises: Is interchangeability preserving truth a sufficient condition for synonymy? Apparently not, as Quine explains it, for non-synonymous words with the same extension such as "creature with a heart" and "creature with a kidney" can be interchanged in extensional contexts and still preserve truth. As a result, a definition of synonymy based on interchangeability must involve not the preservation of truth, but of necessary truth or analyticity. For only in this way could we separate out of the interchange non-synonymous words in extensional contexts. However, a definition based on the preservation of necessary truth or analyticity assumes a prior knowledge of the necessary truth of analyticity. If we were to say that synonym pairs are necessarily related we create an intensional context, and

are thus no longer providing a criteria of interchangability in extensional contexts. A definition of synonymy by way of interchangability in intensional contexts would be question begging.

Hence, Quine concludes that there are no "objects", either abstract objects or mental entities which are meanings. Quine's other argument, for the indeterminacy of meaning, takes as a premise this conclusion from "Two Dogmas of Empiricism".

The indeterminacy thesis arises from Quine's recognition that the identity of meaning would have to be determined on the basis of verbal behavior. And verbal behavior would provide an "indeterminate" translation in the sense that:

> manuals for translating one language into another can be set up in divergent ways, all compatible with the totality of speech dispositions, yet incompatible with one another. In countless places they will diverge in giving, as their respective translations of a sentence of one language, sentences of the other language which stand to each other in no plausible sort of equivalence however loose.[8]

Quine's conclusion that the totality of linguistic evidence cannot eliminate incompatible translations is developed in the situation he calls "radical translation".[9] He illustrates the situation with a story of a field linguist in a jungle who is trying to choose among various translations for the expression "gavagai" in the language spoken by the jungle inhabitants. Quine argues that the

speaker's disposition to respond in such a translation situation is "incapable of deciding among 'rabbit', 'rabbit stage', and various other terms as translations of 'gavagai'".[10] The idea here is that one cannot refer to a rabbit without referring to a rabbit stage or undetached rabbit part, nor on hypotheses which might enable the linguist to choose among extensionally equivalent translation options. Thus, Quine's argument leaves sense and meaning with no grounds to resist radical translation.

It is for these reasons that Quine advanced the view that all sentences both organize and share some empirical content. All, in varying degrees, share in both aspects of what previously had been understood to be the separate domains of analytic and synthetic sentences. The collapse of the analytic/synthetic distinction, and the resulting view that all meaningful sentences have some empirical content, has several consequences. One, is that all sentences, including logical and mathematical truths, are on this view, subject to revision.

> It is misleading to speak of the empirical content of an individual statement-especially [as in the case of logical statements] if it is a statement at all remote from the periphery of the field.
>
> Furthermore it becomes folly to seek a boundary between synthetic statements, which hold contingently on experience, and analytic statements, which hold true come what may. Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system...by the same token no statement is immune to revision.[11]

On Quine's view, not all sentences are equally likely to be revised. Some sentences in the network of our going theories are less likely to be revised because they are more "embedded" as in the case of logical and mathematical truths. There are two aspects to Quine's notion of embeddedness. Logical and mathematical truths are embedded in one sense in that they are less likely to be challenged by recalcitrant experience. This is for the reason that they are far from sensory experience and don't really have any particular sensory experiences associated with them. Secondly, these sentences play a role throughout all our theories and serve as a structural feature upon which these theories are built.

The embeddedness of logical and mathematical statements gives them a special, yet still empirical, status. Revising them would reverberate through the network, and this, together with their distance from sensory experience, makes it less likely that we will do so.

Returning to the general issue of the Holistic Model, those who hold this as a model of epistemological explanation do not see the collapse of the analytic/synthetic distinction and the resulting view of logic and mathematics, as entailing a skepticism about truth. Rather what is understood to be entailed is a "pragmatism" that is not conceived of as skeptical. Quinean pragmatism stems from three related views. The first, that

science is a bridge of our own making which connects our sensory stimulations. Secondly, that there are no extratheoretical standpoints from which we experience the world. And lastly, that truth is "immanent".[12] This three part pragmatism is best summed up in Quine's words in "Things and Their Place in Theories":

> The scientific system, ontology and all, is a conceptual bridge of our own making, linking sensory stimulation to sensory stimulation.
>
> But I also have an unswerving belief in external things-people, nerve endings, sticks, stones...Now how is all this robust realism to be reconciled with the barren scene that I have just been depicting? The answer is naturalism: the recognition that it is within science itself, and not in some prior philosophy, that reality is to be identified and described.
>
> ...it is a confusion to suppose that we can stand aloof and recognize all the alternative ontologies as true in their several ways...It is a confusion of truth with evidential support. Truth is immanent, and there is no higher. We can only speak from within a theory, albeit any one of a various.[13]

With the shift to a holistic view of knowledge gathering and the collapse of the analytic/synthetic distinction, Quine argues that the illusion of epistemology as "first philosophy" must be given up and epistemology recognized as part of science. The break with positivism and Popper's work in general is clear.[14] While Popper relegated questions about "discovery" - how it is we come to posit objects and form hypotheses-to empirical psychology, Popper also specifically distinguished that project from the work in epistemology of science. On his view, this was the study

of the logic of "justification" of theories.

Hempel and Nagel also specifically rule out as relevant to an explanation of science, questions about the connection between sensory experience and theory construction, questions Quine sees as in the domain of epistemology[15]. Quine does not want to separate these questions from questions concerning the justification of a theory. Given the Quinean picture, it is not clear how one might draw a distinction between the two "contexts" of science that foundationalist and positivist philosophers marked off.

The pervasive aspect of Quine's work that I want to draw out in this sketch is his consistent interest in "rubbing out boundaries". As he states it, "This rubbing out of boundaries could contribute to progress, it seems to me, in philosophically interesting inquiries of a scientific nature".[16] In the broadest sense, Quine denies that we can distinguish "the objects we talk about" from "the ways we talk about them". And so, he rejects the alleged distinctions between metaphysics and science, between theoretical sentences and observation sentences, between metaphysics and epistemology.

This view of science without boundaries, as inclusive of almost all our efforts to organize and understand our experiences, is the most important and far-reaching effect of the differences between Quine's view and traditional empiricism. In particular, its effects are evident in the

work of those feminist epistemologists who advocate a Holistic model as the correct model of theory-choice.

III. Holism From a Feminist Perspective

Given the above outline of Quinean holism, a model of Holistic theory-choice emerges. On this model theories are viewed as sets of sentences, whose component features include sensory experience as well as judgements with regard to the coherency of the experience in light of other beliefs. The Holistic Model is not hierarchical in the sense that we cannot mandate what a theory must cohere to other than to distinguish the more central from the more peripheral parts of our web of belief. As was stated earlier, the more central or embedded beliefs are the ones we are least likely to revise in the face of recalcitrant experience as they would require radical readjustment throughout the entire web. So, adopting a particular conclusion, does not require a foundational consistency, only a coherence with currently held beliefs.

Yet, the Holistic Model, as I have sketched it, claims to avoid radical or absolute relativism because, first, it admits that experiential anomalies must be accounted for somehow, even if only by hallucination, and thus eliminating the possibility that we are totally unconstrained in what we believe. And secondly, it purports that the probability is

very high, given our degree of success in prediction, that at least some of our beliefs within the web are true even if we can't say at any given time exactly which ones those are.

The crucial point of intersection between Quinean holism and feminist epistemology is the critical stance taken toward the view that science is autonomous. The autonomy at issue is the detachment of science from common-sense experience and theory. For Quine, this has included our beliefs and observation, the ways theories are adopted, the relationship between standards of evidence and going theories, and the fact that standards of evidence, theories and "truth" are constrained by our current conceptual framework. The need for a more expansive and realistic account of the epistemology of science that philosophers like Kuhn have endorsed is reflected in Quine's attention to science as an evolving process rather than a series of finished products. In addition, Quine notes his refusal to separate what had been bracketed by positivists like Hempel, Nagel and Popper as a "context of discovery". For Quine, the only "product" of the scientific process is a "conceptual bridge" of our own making which is bound by experience.

However, in the spirit of Quine's holism, feminist philosophers who advocate a Holistic Model of theory-choice go a step beyond Quine to blur a last boundary. These philosophers take a view of holism that they see implied in

Quine's work but not outright articulated. The last boundary to be collapsed on their view is the boundary between science and social, moral and political values.

In a chapter entitled "A Remaining Boundary", Lynn Nelson in her book[17], argues that Quine separates science from these "non-scientific" values, and that this separation is unnecessary and unjustified given his views. Nelson notes that in earlier work Quine was inclined to say that he upheld a distinction between science and values as strongly as he questioned every other boundary separating science from other things. Consider the following two quotes from *The Roots of Reference:*

> Scientific theory stands proudly and notoriously aloof from value judgements.[18]

> I think that what sets morals apart from scientific theory is a substantive point of modern scientific theory itself: a scientific doctrine as to the origins and basis of morality. Science sees the moral law no longer as coeval with the cosmos, but as the work of society. Therefore science addresses itself to the origins of the moral law, among other things, but does not incorporate its content. This divorce of science from moral values is a sophisticated manifestation, reflecting no significant quirk in language learning.[19]

In later work, particularly the 1981 article "On the Nature of Moral Values" Quine clarifies the point that science does get into the business of explaining the origins of moral values. While values are social, they are only derivatively so[20], because they are most likely grounded in natural selection and inherited by individuals. Traits like "fellow

feeling" and "altruism" are favored by natural selection because they are favorable to the survival of society. An evolutionary explanation is a satisfactory one for Quine, as it insures that the values a society incorporates are "sound" in the sense that they evolved with the purpose of continuing our survival. Because he views societies as sharing similar interests and concerns, Quine maintains that moral conflicts are not frequent and the lack of a way of resolving them is not a pressing matter.[21]

Yet while Quine sees natural selection as providing an explanation for the origin of social values he still consistently sees scientific claims as being quite distinct from value claims. For Quine, there is no adjudicating values, no way to judge them in the way that scientific claims can be judged and adjudicated according to empirical evidence. As Nelson reads him, Quine's skepticism is the view that values, with the exception of the discussion of natural selection, are unable to be grounded ontologically. Thus, Quine concludes that moral claims have no claim comparable to the claims available to science, since values are not subject to "empirical controls".

On Nelson's view, contrary to what Quine maintains in the passages quoted above, his appeal to natural selection, in fact any use of empirical science, to explain the origins of values does not keep science out of the business of values. For Nelson, it results precisely in the

incorporation of values into the content of science. While Nelson supports Quine's general picture of science and knowledge gathering, she argues that this project must involve science far more deeply in the business of values than Quine acknowledges and that involving ourselves in this kind of business is something we should undertake with a robust self-consciousness. The use of natural selection, as Quine uses it to explain values, would justify those moral values that are consistent with an evolutionary explanation. This use of science to underwrite values by Quine, suggests that values are "objective" in a way that elsewhere Quine says values cannot be.

The moral of the story for Nelson is that as long as we see science as standing "aloof" from values, as long as we advocate, praise, or adopt values without reference to what we come to know about how things are, then there is nothing "objective" about them. If moral, social and political beliefs are viewed as separate they will still become incorporated into the content of science, but according to Nelson "ideology will have a field day-particularly when we continue to declare that they are not so incorporated."[22]

While Quine maintains that our theories are generally without boundaries, he does not address the possibility that issues in the larger social community could have a bearing on scientific theories. The network of theories is structured in such a way, on his account, that scientific

theories are more embedded than ones that systematize social or political experience. But, according to Nelson, this implicit hierarchy should not follow on an epistemological model that relates common sense, science and philosophy as all theoretical, interdependent and evolving. The balance of her book is devoted to undermining the alleged boundary upheld by Quine. She disagrees that there is nothing "objective" about values and questions whether the monolithic "we" that Quine talks of when he claims that "...there is little clash of interests as *we* pursue our separate ways. Our scales of values blend in social harmony."[23] really means everyone.

Quine serves as the springboard for feminists who advocate the Holistic Model. But the fact that Quine's epistemology does not recognize categories like race, gender or political experience in theorizing is seen as an inadequacy. Feminist empiricists argue that the direction of scientific research and the content of theories may importantly reflect the scientists' experiences of race, gender or politics. Reconciling feminism with holism requires, for philosophers like Nelson, revising some aspects of Quine and abandoning others. In particular, the empiricist commitment to "individualism" in the sense that the acquirers of knowledge are individuals, is, for feminist epistemologists, artificial and ultimately untenable. Quine's holism provides the tools to construct a criticism

against this kind of individualism, even if Quine did not flesh this out himself. As Nelson says:

> By virtue of our membership in a number of epistemic communities, as well as by virtue of our experiences as individuals, we can each contribute, and uniquely, to the knowledge generated by our various communities. However singular an experience may be, what we know on the basis of that experience has been made possible and is compatible with the standards and knowledge of one or more communities of which we are members.[24]

Furthermore, as mentioned above, an attractive feature of the Holistic Model for feminist epistemologists is that it provides a way of explaining the blur between contexts of discovery and justification, without falling into radical relativism. There are no extra-theoretic standpoints or experiences, on Quine's view, by which we might judge whether our beliefs and theories are true. Without this standpoint, a global skepticism is incoherent. What is possible is more "local" skepticism that would arise during the progression and development of our theory making.

IV. Points of Intersection and Criticism

In this next section I would like to address the problems that arise at the particular points of intersection between Quinean holism and feminist epistemologies which advocate a Holistic Model of theory-choice. The criticisms I will raise stem from a recognition of the incompatibility

between the goals for feminist epistemology and the implications of Quinean naturalized epistemology. In her essay, "Quine as Feminist: The Radical Import of Naturalized Epistemology" Louise Antony remarks:

> Naturalistic epistemology has the great advantage over epistemological frameworks outside the analytic tradition in that it permits an appropriately realist conception of truth. Without appealing to at least this minimally realist notion of truth, I see no way to even state the distinctions we must ultimately articulate and defend... Surely one of the goals of feminism is to tell the truth about women's experience.
> Is institutionally supported discrimination not a fact? Is misogynist violence not a fact?[25]

Because I agree that a realist notion of truth is necessary to explain and defend worthy from unworthy epistemic and social practices, I do not agree that naturalized epistemology and Quinean holism provide the "great advantage" for achieving this goal. Eventually, in Chapter Four, I will argue that a realist notion of meaning and truth has significant advantages over holism for articulating and defending the goals of a social and feminist epistemology. I base this conclusion on the problems and inconsistencies that I find to inhere in the Quinean model.

Drawing upon work in the philosophy of language by J.J. Katz, I will argue that Quine's attack on the analytic/synthetic distinction does not work. Secondly, I will discuss what I see as internal inconsistencies in

Quine's view of the embeddedness of logic and mathematics. This internal inconsistency along with unconvincing arguments for the collapse of the analytic/synthetic distinction show that epistemic holism, as it is developed by Quine, is a flawed model and as such does not provide an adequate explanation of knowledge. Ultimately, contrary to the views of some feminist epistemologists, I will disagree that Quine's naturalized epistemology provides even a minimally satisfying realist notion of truth. As a result, I suggest that those feminist epistemologists seeking a viable model of theory-choice should forego the adoption and expansion of Holism.

My criticisms start with a return to Quine's arguments against the analytic/synthetic distinction and the indeterminacy of translation. Quine's claim that actual translation is indeterminate, that is, that there is in principle nothing which allows determinate translation, is a metaphysical claim. The metaphysical claim is that translation is indeterminate because there is no fact of the matter with regard to what theories of translation are about. This is for the reason that there are no meanings which a theory of translation takes as its objects. And this metaphysical claim is not backed up by the epistemological arguments provided in *Word and Object*. The arguments offered there only prove that we do not have sufficient evidence to know the properties of meanings; but

this only indicates that theories of meaning are underdetermined by the available evidence. And underdetermination is not a special problem for theories of meaning. Rather, it is a problem for all scientific theories. Thus as far as Quine's epistemological arguments go, theories of meaning are on a par with scientific theories generally.

What needs to be shown is that Quine's arguments do not rule out meaning in general only meaning as construed by the three possibilities considered in "Two Dogma's of Empiricism". Katz, in his work, offers such an argument[26]. He begins by maintaining that Quine assumes the method of substitution criterion as the proper method of defining concepts in the theory of meaning. This method holds that some feature, like truth, of the context in which substitutions are made, remains invariant and this feature must not be intensional otherwise the account is circular.

Yet Katz argues that there is a better method of defining concepts in the theory of meaning. This better method entails defining concepts on the basis of axiomatic specification of the relations between concepts much in the same way that concepts are defined in logic or mathematics. This is the notion of theoretical definition. On Katz's view, there is a fundamental pool of semantic primitives and these have necessary connections and relations. These axioms are justified because from them, stems a consistent

system in which the facts of our language are explained. Quine's arguments could be construed, according to Katz, as a reductio of the idea that the identity conditions in a meaning theory should be defined by way of substitution criteria. Hence, on Katz's view, the only legitimate charge that Quine can make against meaning theories is that they are undetermined but not indeterminate. Quine has not shown that meanings do not exist.

In Katz's view, a reason why Quine's arguments against linguistics have taken hold, is that many philosophers are unfamiliar with more recent issues in linguistic theory. In Quine's writing he assumes that substitution criteria are the proper way to clarify concepts in linguistics. Quine notes that substitution criteria "in one form or another played central roles in modern grammar."[27] However, Katz argues that substitution criteria held a central place in linguistics during the Bloomfieldian period, roughly 1930 until 1950. In Bloomfield's approach to language, he sought to characterize the behavioristic features of language - the noises a speaker makes - as opposed to "impalpable ideas" a speaker may have.[28]

Yet, as Katz points out, there are strong reasons, apriori as well as historical, against the assumption that substitution criteria are the proper way to clarify concepts in linguistics.[29] As sketched out above, Katz provides the model of theoretical definition along the lines of

theoretical definition in logic and mathematics, as a preferred form of defining concepts in linguistics. With theoretical definitions, we are not prevented from using concepts belonging to the same family as the concept to be defined. As Katz explains it:

> Effective theoretical definitions explain a concept relative to the other concepts by representing the structure of the primitives in the entire family of concepts. The degree of relatedness exhibited among the concepts in the family is thus a measure not of circularity but of the systematizing power of the explanation.[30]

Hence, Katz concludes that what Quine has shown is not a knock-down argument against meaning, only an argument which shows that the paradigm of substitution criteria is inappropriate to impose upon a theory of meaning.

The historical reason Katz cites against Quine's take on linguistics, is the "Chomskyan revolution" in the 1960's. One feature of Chomsky's approach to language was to shift from substitution criteria definition to theoretical definition in generative grammars.[31] As Katz explains, Chomsky modeled his conception of a generative grammar and theoretical definition on formal systems in logic. And later research in semantics, most notably by Katz in the 1960's and 1970's, developed the Chomskyan paradigm of theoretical definition within a generative grammar.[32] The outgrowth of this research was the evolution of "decompositional semantic representation"[33] whereby the symbols in formal representations typify component senses

and sense relations at every level of grammar. With the development of a theory of semantic representation, Katz notes that it is possible to formulate a notion of analyticity which is not dependent on thought processes and can include more than subject-predicate statements.[34] Moreover, as Katz maintains, a theoretical definition of analyticity avoids both of Quine's criticisms against Carnap's explication of analyticity. First, decompositional analyticity, unlike analyticity construed by way of meaning postulates, holds for variable languages. That is, the notion of analyticity is a meta-theoretic concept which is true of some sentences in an object language. To say of a statement that it is analytic, is to offer a logical interpretation in which the structural relations internal to the senses of the statement are unfalsifiable and in some cases, necessarily true.[35] This interpretation is not relativized to a particular natural language since senses are built out of abstract semantic simples that can be represented in all natural languages. The form of representation may vary from language to language but the propositions expressed by the sentences of a language will remain consistent in that they will not falsify the truth of an analytic proposition.

Secondly, on Katz's view, the theoretical definition of analyticity also answers Quine's request for a definition of analyticity that tells us what property holds for a sentence

that is marked as analytic.[36] The property of being analytic is that of having a "redundant predication".[37] So, for example in the sentence "mothers are female parents" the sense of the predicate gives us no new information not already contained within the sense of the subject.

As Katz has pointed out in his work, the idea that there is a right and wrong translation for sentences, even if the options from which to choose are vast, is a belief held in our common-knowledge view of language. Quine has posed a skeptical challenge to this view and as such he incurs a burden of proof. Yet his arguments do not supply the evidence necessary for discharging that burden. The reason is that Quine has overlooked the possibility of accounting for sense along the lines of decompositional semantics. The consequence of this failure is explained by Katz in the following passage:

> The immediate consequence is that we are free to presume the existence of meanings in the same spirit with which scientists beginning the study of any new field are free to presume there are facts and laws to be discovered. Without a reason to think that the theory of meaning suffers from anything worse than underdetermination, we may pursue the parallel with well-developed sciences. We may take the view that there are meanings. As with other sciences, trying to construct a theory will, in the long run, show whether the initial optimistic presumption was correct.[38]

As was shown earlier, Quine's argument for the indeterminacy of language depended on the argument that there are no objects of investigation countenanced in

linguistic theory. But with the possibility of meaning developed on the theory of decompositional semantics, the data derived from a speaker's intuitions about the actual senses of their expressions can serve to constrain an interpretation. As in other sciences, linguists will form hypotheses to explain the linguistic evidence. A linguistic explanation of linguistic data, unlike causal explanations, will explain the properties of words, sentences and senses by positing principles or rules that the language follows. Hence a hypothesis' ability to provide the simplest, most predictably powerful explanation will be a justification for its rightness.

Katz's arguments provide a way of overcoming Quine's skepticism about meaning. Yet, thus far, in employing these arguments, linguistics is merely on a par with other sciences in the Quinean framework. And while Quine is clearly skeptical of the existence of "meanings" he is not an obvious realist with regard to the existence of the objects of natural science. For Quine, there are no "absolute" ontological decisions. We do not, on his view, discover objects and then theorize about them. Ontologies are proposed and evaluated within theories. Hence even if we can show that analytic truths "exist" in the same way that the objects of natural science exist, this does not confer upon them a necessity or unrevisablity. On Quine's epistemological view, no statement is immune from revision.

This includes logical and mathematical truths as well as statements closest to the "periphery" of sensory stimulation that we are less likely to get wrong. So the "absolute" character of analytic, logical and mathematical statements can still be explained by their centrality or "embeddedness".

Yet how viable are the dual notions of "embeddedness" and "revisablity"? Do they provide a clear explanation of the role of analytic, logical and mathematical truths within our theories? Along with Katz's argument for the possibility of meaning, I want to argue further that there are also reasons to question Quine's centrality and revisablity thesis.

In *From A Logical Point of View* Quine writes:

> ...no statement is immune to revision. Revision even of the logical law of the excluded middle has been proposed as a means of simplifying quantum mechanics...[39]

However, Quine also explains that it is rational to keep revisions in our body of knowledge to a minimum. To revise or abandon central logical principles would have consequences we can hardly begin to envisage. In *Methods of Logic* he writes:

> Our system of statements has such a thick cushion of indeterminacy, in relation to experience, that vast domains of law can easily be held immune to revision in principle. We can always turn to other quarters of the system when revisions are called for by unexpected experiences. Mathematics and logic, central as they are to our conceptual

> scheme, tend to be accorded such immunity, in view of our conservative preference for revisions which disturb the system least...[40]

How then are we to understand the revisablity thesis? Quine himself seems to hold both a radical and conservative interpretation of revisability. On the one hand, Quine argues that there are many statements of logic and mathematics for which we feel certain, and quite reasonably, that their truth value will never come into question. This is not because our knowledge of them has a special source which guarantees their truth, since we know that some statements which had this status have lost it as our body of knowledge progressed. Rather, the source of our certainty is explained by the systematic role that such statements have within the body of our beliefs. These statements are in some sense "remote" from experience but they are not totally immune from future experiential evidence.

However, Quine also explains that it is rational to keep revisions in our body of knowledge to a minimum. To revise or abandon central logical principles would have consequences we can hardly begin to envisage. While Quine argues for epistemological holism he is not prevented from seeing how mathematics and logic differ from other sciences. It may be possible to conceive of possible states of affairs which contravene the laws of physics or psychology, but the laws of logic seem to be operative in all states of affairs. The necessity we feel in mathematics and logic is explained

by the fact that they are so deeply embedded in the framework of beliefs. Hence, it is unlikely that any evidence could force us to undertake the enormous revisions that would be required if we were to abandon one of them. It follows that it may be absolutely rational to hold these principles immune from falsification in the light of any new evidence.

Consequently, wholesale revisability is in principle a possibility. However, no foreseeable circumstances would ever arise that might lead us to revise the embedded statements of logic and mathematics. One could feel very well justified, in light of what Quine has said, in believing that the only rationally admissible conceptual schemes are those which do not refute the central laws of logic and mathematics.

Philosophers who principally accept Quine's picture, have raised concerns over his characterization of the revisability thesis. In particular Putnam, in his 1983 article "There is at least one a priori truth" and Azzouni in a 1992 article "A priori truth" have expanded upon and clarified the notion of revisability.

Putnam takes the revisability thesis seriously, but only up to a point. He maintains that our discussions of revisability and conceptual change require a stable notion of rationality. The manner in which we define this notion will rely upon some unrevisable truth and that truth is the

principle of non contradiction. However, Putnam also wants to claim that with this in mind we must still take a moderate view regarding revisabiltiy and a prioricity. What Putnam proceeds to develop is an account that both acknowledges the correctness of the revisability thesis while still accounting for the intuition that certain propositions we hold true seem to be unrevisable in character. Quine's attempt at explaining this feature of our knowledge is the notion of the "centrality" of certain concepts within the web of our beliefs. But Putnam argues that the centrality thesis is not enough. We need to recognize the fact that within a particular rational conceptual scheme certain pre-theoretic "maxims" or general principles are held to be a priori and unrevisable. However, for Putnam it is possible that we may come to withdraw our claim that these pre-theoretic principles, like the principle of non contradiction, are a priori and unrevisable since we can never predict what future rationally admissible theories will commit us to. Hence, we wind up with a quasi kind of post-Quinean local unrevisability.

The notion of revisability Azzouni relies on is one adopted from Putnam. A statement may be revised in the following two ways. We may either a) Redistribute the truth value of the statement. Or b) Reject the statement within a theory. The relevant notion of revisability that Azzouni is

concerned with is the latter. Perhaps it would be best in a particular conceptual scheme to restrict our capacity to express certain concepts "on pain of paradox"[41] and one of the things we might not express is the principle of non contradiction.

Azzouni explains that within some deviant logics the principle of non contradiction is unexpressible. These deviant logics are attempts at constructing self-referential languages. On the Tarskian model, languages are not self-referential because if they were, we would face paradoxes. Hence, an object language and a meta-language distinction was postulated whereby object language propositions cannot refer to their own truth or falsity. However, in a deviant logic, we may restrict the domain over which our truth predicate ranges and avoid paradoxes while still winding up with a self-referential language. Azzouni explains that when we try to characterize the language from the "inside", that is without an object language, meta-language distinction, then we "must leave one or another aspect of the language uncharacterizable."[42] The result is that within a deviant logic some statements are not expressible. Specifically, the principle of non contradiction is unexpressible within that theory.

While Azzouni claims that Putnam's arguments for the unrevisability of the principle of non-contradiction are the best, they are not enough and Quine's radical revisability

thesis still holds. Yet, on Azzouni's view, this is not to say that we may revise the truth value of those propositions which rely on the principle of non-contradiction, rather we may revise them in the sense of "revising them out of a conceptual scheme altogether: that is they are inexpressible there."[43] Such is the case in the example of the deviant logic.

For Azzouni, those truths which seem intuitively unrevisable to most of us come down to those propositions whose incorrigibility results from the best methodology in *our* conceptual scheme. What Azzouni argues, and Putnam hints at, is that we then make the mistake of thinking that our methodology is applicable to every possible future conceptual scheme. Not holding contradictory statements seems to be a matter of our methodology. Azzouni argues further that *unimaginability* is not a good enough argument for impossibility. What we cannot imagine in our theory might still be a possibility for some future theory.

Yet another aspect to the Azzouni argument is explaining how the truths of mathematics are not incorrigible. The most plausible account for the seeming unrevisability of mathematics claims that it is a theory whose results are independent of the results in the empirical sciences. The picture of mathematics, and ultimately of logic and semantics is the following: We first set up axioms and inference rules. From these we

generate truths. We use mathematical and logical principles in generating these truths. If algorithms are corrigible then truths generated from these algorithms are also corrigible. Necessary, unrevisable truths would have to depend on two considerations. First, that we correctly analyze our methods for gathering evidence and two, such methods force the existence of true statements. Azzouni argues that while it is true that the general procedure is independent of the empirical sciences there is still a quasi-empirical character to any particular proof procedure. Proofs require that our memory works properly, that we in fact know how to do mathematics, that we wrote things down correctly. Since we could be mistaken with regard to these quasi-empirical procedures our proofs could be wrong. That is, nothing we do in terms of axioms and inference rules is incorrigible. Hence, the truths we generated, including those which seem unrevisable, are not beyond revision.

We wind up with two arguments on Azzouni's view. The first states that some rationally admissible conceptual schemes might not express the principle of non-contradiction. In that case, not every rationally admissible conceptual scheme has the resources to express seemingly unrevisable truths in other rationally admissible conceptual schemes. The second argument claims that relative to particular conceptual schemes there are empirical truths and quasi-empirical truths which have to do

with the very framework of the conceptual scheme. These quasi-empirical truths are corrigible but in a different sense from standard empirical truths. Our proof procedures depend on our correctly applying mathematical and logical principles, and since we could always make a mistake our results could always be mistaken. Hence, seemingly unrevisable truths are actually facts that are corrigible, revisable and relative to our conceptual scheme.

Putnam is reluctant to buy into wholesale revisability, yet he is also hesitant to push too hard for the absolute unrevisability of even the principle of non-contradiction. Clearly, he is uncomfortable with the view and as such his efforts to clarify Quine raise additional problems. Azzouni accepts the radical interpretation of Quinean revisability across the board. He argues that there is not "at least one a priori truth" as Putnam has struggled to claim. For Azzouni, there are no a priori truths at all. His argument amounts to refuting the a prioricity of the principle of non-contradiction and tidying up both Quine's and Putnam's account of the apparent necessity of logical, mathematical and certain semantic propositions.

In my understanding of the first argument of Azzouni's, namely that unexpressiblity undermines necessity and unrevisability, certain difficulties emerge. The deviant logic described by Azzouni is understood to be a rationally admissible conceptual scheme in which the principle of non-

contradiction does not apply. Taking the picture a step further, the argument seems to imply that there are possible conceptual schemes which would be rational to adopt and in which analytic truths remain in the background useless and unused. Hence, seemingly unrevisable truths do not function as stability points in conceptual shift. However, to correctly capture our intuitions, unrevisability should be construed as a property of some truths which could never be *falsified* in any rationally admissible conceptual scheme. The scheme cited by Azzouni does not falsify an unrevisable truth it merely fails to express it. There is something stipulative in arguing that because a proposition is not expressed in a conceptual scheme its truth has somehow been called into question. To change our beliefs in the unrevisablity of certain propositions requires showing that there is a model in which these propositions are false, not they are left unexpressed.

Moreover, there is no argument given by Azzouni for why it might be rational to shift to a particular conceptual scheme which has less expressibility than a scheme we already hold. It is not at all clear that the more rational move to make in conceptual change is to avoid paradoxes at the cost of losing expressibility. I am not claiming here that such an argument is impossible, only that it is not given in Azzouni's account. At the very least, this would require some further argumentation.

Azzouni's second argument concludes that mathematical, logical and semantic truths are arrived at through corrigible, quasi-empirical proof procedures. Since our methods are fallible our results may be fallible. There are two problems with this view. The first turns on the distinction between the methods we use to arrive at a proof and the fact of the matter that the proof is intended to capture. If we have made an error in arriving at a result in our proof, that does not show that the truth we are after is corrigible. So, while we can agree with Azzouni that our methods for deriving a priori truths are falliable, we are still need not grant that a priori truths are corrigible. The second problem with the view concerns the fact that the a priori propositions that we are concerned with are those contained within the "center" of our web of beliefs. Propositions like; "2+2=4" and "If all A's are B's and All B's are C's then all A's are C's" and "If Janet persuaded Matt to do X then Matt intends to do X". The question to ask here is what is the algorithmically generated proof procedure that is used in arriving at these truths? Traditionally, the truth of propositions of this kind were considered "clear and distinct" stemming from Descartes' analysis in *The Discourse* and *The Meditations.* We reflect upon our beliefs, with the aim of distinguishing those which are actually knowledge from prejudice and error. In the course of this reflection, we formulate skeptical

challenges: evidence of delusion could suggest that we were wrong about what seems most certain. To defeat the skeptical challenge we may try to employ reasoning or argumentation. At this point we recognize that either our reasoning requires further reasoning or that the truth of the proposition is somehow self-evident and does not require a proof. When we accept that no proof is required we come to see that there are beliefs we have which are secure in such a way that the manipulations of a demon or evil genius will not shake our confidence in their truth.

With his notions of "centrality" and "embeddedness" Quine hoped to capture these truths. In his paper, Azzouni attempts to clarify why these embedded propositions are in principle revisable. Yet his notion relies on there being a proof procedure which establishes our certainty in these statements. However, with regard to the above propositions of mathematics, logic and semantics, the source of our intuitions in their certainty is not dependent on proof procedures. Our estimation of the truth of these propositions is based on pure conceptual reflection which is independent of quasi-empirical methods.[44] For Azzouni's argument to be effective, all seemingly unrevisable truths must rely upon some kind of proof procedure. However, simple mathematical statements and logical and semantic entailments are immune to this kind of criticism. There is no memory limitation or mistake that could be made in

writing down the resulting truths, since our recognition of their truth is a matter of immediate conceptual apprehension.

Within this discussion of the related notions of "centrality" and "revisability" I have tried to show how Quine's own view is inconsistent and unclear. Putnam makes an effort to clarify and settle some of the problematic aspects that inhere in Quine's account but he ultimately raises additional problems. Finally, Azzouni offers the clearest and most well developed account of revisability but it ultimately fails in providing convincing evidence as to how certain fundamental truths may be in principle revisable. What these failures show is the need for an epistemologically relevant ordering of truth productive procedures. Rather than assuming a web of interconnected, revisable principles, what is needed is a hierarchical model that preserves the necessity of the truths of logic and language.

We can now trace the consequences of the arguments against indeterminacy and the revisability thesis for feminist epistemology. Feminists who rely on Quine seem to do so for several reasons. First, on Quine's view of science, there are no boundaries between science, metaphysics, methodology, and epistemology. This is appealing to feminist epistemologists because one of the criticisms raised against science is that values are

incorporated into scientific theorizing without being understood or explained. Socially marginalized theorizers who admit to incorporating their values and beliefs into science are seen as anomalous. So one aspect to the work of feminist epistemologists is exploiting Quine's notion of blurring boundaries to account for this alternative research. However, given the arguments above raised against Quine, not all boundaries have been rubbed out. There is still a reason to hold fundamental principles of logic, mathematics and language as true, meaningful and beyond revisability. So an account of the incorporation of values into science cannot rely upon an epistemological model which unsuccessfully denies the existence of necessary truth.

A second reason for the endorsement of Quine by feminist epistemologists, is that on Quine's view we can make sense of the notion of epistemological interdependence. All those things we countenance as true are determined by our shared talk within a theory. So criticisms raised by socially marginalized theorists do not lead to epistemolgical chasms between themselves and more mainstream theorists. In other words, feminist criticism of scientific investigations can serve as a corrective for our current practices since these criticisms are internal to the large-scale shared theory which constitutes our web of belief. However, this aspect of Quine's view is only available to adopt if holism is characterized in a consistent and

coherent way. The laws of logic and mathematics and language, which unify our theories, are explained by Quine as being centrally embedded in our beliefs but still revisable. However, given the above arguments, Quine's explanation is problematic and unconvincing.

Thirdly, feminists are attracted to the holistic conception of truth because it apparently does not lead to a radical epistemological relativism. However this desirable feature depends on an overall framework in which good theories are clearly distinguishable from bad theories. Judgments of this kind can only be made by relating a well developed model of correspondence with our existing web of belief. But given the arguments above, this model, particularly with regard to the notions of meaning, centrality and embeddedness, lacks the necessary coherence to make it an ideal candidate. Further, the strong program of justification needed to condemn practices which are discriminatory is unavailable on the kind of naturalized empiricist model proposed by Quine.

I will argue in Chapter Four that a model developed along the lines of epistemic rationalism can account for the features that feminists want to explain and furthermore, will avoid the problems raised here for the Holistic model. However, before that argument is given I want to first consider another model of theory choice popular in the literature of feminist epistemology. This model I will

refer to as the "Social Constructivist Model".

NOTES

1. See Kuhn, T.S. 1970. Kuhn's work is given a much more detailed analysis in my Chapter Five.

2. Quine, W.V.O. 1963. pp.42-43.

3. Ibid., p.43

4. Quine, W.V.O. 1960. p.3.

5. Ibid., p.4

6. Quine, W.V.O. "Two Dogmas of Empiricism" in Quine 1963. p. 42.

7. See Carnap, Rudolph. 1967. "Empiricism, Semantics and Ontology" in *The Linguistic Turn*. edited by R. Rorty. Chicago, Ill. University of Chicago Press.

8. Quine. 1960. p.27

9. Ibid., p.70

10. Ibid., p.71

11. Quine. 1963. p.43

12. Quine, W.V.O. 1981. "Things and Their Place in Theories" in *Theories and Things*. Cambridge, Mass. Harvard University Press. pp.21-22

13. Ibid.

14. Popper, Karl. 1959. *The Logic of Discovery*. New York, NY. Harper & Row.

15. Hempel, Carl. 1965. *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*. New York, NY. Free Press

Nagel, Ernst. 1961. *The Structure of Science: Problems in the Logic of Scientific Explanation*. New York, NY. Harcourt, Brace & World.

Both Hempel and Nagel's work will be discussed in more detail in Chapter Five.

16. Quine, W.V.O. 1969. "Epistemology Naturalized" in *Ontological Relativity and Other Essays*. New York, NY. Colombia University Press.

17. Nelson, Lynn Hankinson. 1990.

18. Quine, W.V.O. *1974.*

19. Ibid., pp.51-52.

20. Quine, W.V.O. 1981. "On The Nature of Moral Values" in *Theories and Things.* p.63.

21. Ibid., p.63.

22. Nelson. 1990. p.134.

23. Quine. 1981. p.51. Emphasis is mine.

24. Nelson. 1990. p.150.

25. Antony, Louise. 1993. "Quine as Feminist: The Radical Import of Naturalized Epistemology." In Antony & Witt 1993. p. 190.

26. Katz, Jerrold J. 1990.

27. Quine, W.V.O. 1963. p.56.

28. Bloomfield, Leonard. 1936. "Language or Ideas" in *Language 12: 89-95.*

29. Katz. 1990. p. 188.

30. Ibid., p.189.

31. Chomsky, Noam. 1965.

32. Katz. 1990. p.190.

33. Ibid.

34. Ibid., p.192.

35. Katz, Jerrold J. 1981.

36. Katz. 1990. p.192.

37. Ibid.

38. Ibid., p.193.

39. Quine. 1963. p.43.

40. Quine. 1972. p. xiii.

39. Quine. 1963. p.43.

40. Quine. 1972. p. xiii.

41. Azzouni, Jody. 1992."A priori Truth".In *Erkenntnis*, 37, 327-46.

42. Ibid.

43. Ibid.

44. For a more detailed discussion see J.J. Katz. *Cogitations*

CHAPTER THREE

The Social Constructivist Model

I. Outline of the Model

The second model of theory-choice to be considered is the Social Constructivist Model. The theoretical underpinning for this model is exemplified in the later writings of Ludwig Wittgenstein (1953, 1969) and more recently in the feminist writings of Helen Longino (1990), Naomi Scheman (1983) and Elizabeth Potter (1993).

The Social Constructivist Model, like the Holistic Model, relies on coherence as the criterion in theory-choice but differs from the Holistic Model in that language and social practices set the prior background conditions necessary to determine whether or not a statement or set of statements coheres. As we saw with the Holistic Model, there was a correspondence relation assumed between knowledge-claims and an empirical reality. Justification was accorded to those statements which captured the quality of our sensory experiences while cohering with our overall web of belief. Although coherence can involve the particularities of the individual knower or scientist, the ultimate arbiter of truth for a statement is empirical reality. It is for this reason that feminist epistemologists who rely on the Holistic Model, have taken

pains to show how non-scientific values may still be incorporated into the epistemic process and at the same time, not abandon a commitment to empiricism.

The Constructivist Model operates with a different conception of justification and truth. Justification for the constructivist, operates at the level of implicit and imperceptible meanings and explicit behaviours shared by a community of language users. Truth is a property that holds of some bedrock statements within a "language-game", to use Wittgenstein's term, and such statements are meaningful in that they cohere with the rules of the language-game. In this way the constructivist's notions of justification and truth are contingent and historical. The bridge between the knower and what is known is never entirely free from subjective elements. Instead, these notions are the product of a process which involves observation, social practices, theory-construction and communication. This process of knowing constitutes what is true, so that truth is a construction, rather than something external and "discovered" intact. As a result the framework out of which our notion of truth emerges, can change. New ways of talking get introduced, new ways of behaving become integrated into our common social practices and former ways are rejected. Thus on the Constructivist Model, especially in its more recent forms, the combined effects of power, politics and human interest direct the course of developing

language-games. Consistent with this idea, is the fact that values are not epistemically distorting. Instead, the standards and practices of the community, including what is of social or moral value, serve to guide us in our way of talking and behaving. "Knowing" for Social Constructivists, is knowing how to participate in a language-game.

These last comments, while they are consistent with the later Wittgenstein, go beyond his actual work. More contemporary Social Constructivists, in particular certain feminist epistemologists, see themselves as drawing upon the general Wittgensteinian model of meaning and knowledge, and extending it further. Before looking at these more recent construals, I want to develop a general picture of Wittgenstein's views with an emphasis on those aspects which have influenced feminist Social Constructivists.

The remainder of this Chapter will proceed in the following way. I will give a general account of Wittgenstein's view of knowledge and justification. This account will draw mainly from passages in *On Certainty*. This will then be followed by a somewhat more detailed look at three integral components to Wittgenstein's philosophy; the notion of a language-game, the impossibility of private language and rule following in a langugage-game. For this section I will rely on the *Philosophical Investigations, On Certainty,* and *The Blue and the Brown Books*. I will then connect this discussion of Wittgenstein to the work of those

feminist epistemologists who advocate a Social Constructivist Model of theory-choice. After joining the particular aspects of the Wittgensteinian picture to the model of Social Constructivism employed by those feminist philosophers, I will show how the picture is both internally flawed and limited in its scope for the purposes of any epistemology which sees as its goal social or political emancipation. In the final section of this chapter I will chart the direction I argue should be taken by feminist epistemologists, given what I have shown as problematic with the Holistic and the Social Constructivist Models.

II. Wittgenstein On Certainty

For the later Wittgenstein, there are no independent, objective points of support, outside human thought and speech, that serve as foundations for our knowledge. In section 105 he writes:

> All testing, all confirmation and disconfirmation of a hypothesis takes place already within a system. And this system is not a more or less arbitrary and doubtful point of departure for all our arguments; no, it belongs to the essence of what we call an argument. The system is not so much the point of departure, as the element in which arguments have their life.[1]

The system that Wittgenstein is referring to here, is the system of our linguistic and behavioral practices. This system determines the way we interpret the incoming world of

experience. Meaning, knowledge and truth are maintained only in the linguistic practices which embody them. We talk and act. Our acting affects the way we talk, and our talking affects the way we act. Over a long period of time, successful action is the strongest evidence we have that our language has a relationship with reality. The system remains stable and constant only because our system of practices follow rules. However, even these rules do not provide an absolute or fixed point of reference, because they can be subject to a variety of interpretations. What really seems to give our system of practices their stability is that we generally agree on our interpretations of the rules. This is not a fortunate fact, or even a matter of logical consistency, rather it is undertood as being the result of natural human experience. For Wittgenstein, we cannot really ask how this situation came to be. In our present system of practices, the internal fit of language and the world is simply given in successful cases of communication. He writes:

> I want to regard man here as an animal; as a primitive being to which one grants instinct but not ratiocination. As a creature in a primitive state. Any logic good enough for a primitive means of communication needs no apology from us. Language did not emerge from some ratiocination.[2]

Thus, the system with which one's beliefs, language and behavior must cohere is a system of interrelated and communal linguistic and social practices. This system is

acquired and engaged in long before persons are explicitely and critically aware of it. We are not "taught" this system in the sense of learning rules and reasons for speaking and acting. Rather implicit and imperceptible common meanings are shared as we become members of a community of language-users. Wittgenstein describes it in the following way in section 279:

> It is quite sure that motor cars don't grow out of the earth. We feel that if someone could believe the contrary he could believe *everything* we say is untrue, and could question everything that we hold to be sure.
> But how does this *one* belief hang together with all the rest? We should like to say that someone who could believe that does not accept our whole system of verification.[3]
> This system is something that a human being acquires by means of observation and instruction. I intentionally do not say "learns".

When a person gets to the stage of being capable of critical reflection of the system, they are already entrenched in the very practices they question. In this way, critical questioning or theorizing about language, is an extension or an imitation of the actual language-game. To engage in this kind of questioning does not lead us to a deeper understanding of linguistic concepts. It is the mistake of philosophy to suppose that there is a hidden essence or true meaning behind the words we use. For example, suppose I ask the philosophical question, "What is freedom?". On Wittgenstein's analysis, the fact that I have asked this question is evidence that I have already become familiar with the use of the word 'freedom' in the basic

language-game I participate in. In my language-game 'freedom' is used in the the following sorts of ways: a) All people should have the freedom to pursue their interests provided they do not cause serious undeserved pain to others. b) Comfortable cotton clothing gives one lots of freedom to move. c) He tries to give people the freedom to express their point of view.

Cataloguing the ways in which 'freedom' is used shows the extent of meaning for the term. When I move to the question of what the term means apart from its use I lose my grounds of evidence and justification. There is no meaning over and above the practices of the system. Wittgenstein says:

> There is always the danger of wanting to find an expression's meaning by contemplating the expression itself, and the frame of mind in which one uses it, instead of always thinking of the practice. That is why one repeats the expression to oneself so often, because it is as if one must see what one is looking for in the expression and in the feeling it gives one.[4]

In this way, Wittgenstein is showing how real questions of justification and evidence can only be asked of the standard practices in the language-game, despite the fact that we feel we want to seek some transcendent thing beyond this. It would be legitimate to ask, "Is the word "freedom" in English ever used to refer to a happy, carefree mood?", because we would have an actual context, a language-game which sets the conditions for determining rightness or

wrongness of meaning. Wittgenstein says: "In order to make a mistake, a man must already judge in conformity with mankind."[5]

A related point that Wittgenstein raises regarding evidence and justification, is the fact that much of what we say and do rests on a level of accepted beliefs and practices which function as a kind of "bedrock" for our language-games. The fact that we share a common language depends on an unreflexive and natural set of shared propensities to speak and behave in certain ways. These propensities operate at a level below reflection and justification. They are rooted in our words and actions in such a way that we do not even seek evidence for their truth. In fact, when situations arise which could confirm these beliefs we do not even register it as a justificatory opportunity. Instead, Wittgenstein explains that these bedrock beliefs make the language-game possible, in that they provide a framework for understanding. He writes:

> I have a telephone conversation with New York My friend tells me that his young trees have buds of such and such a kind. I am now convinced that his tree is....Am I also convinced that the earth exists?[6]

In this case my friend, who is a reliable source, tells me that, for example, his magnolia trees are starting to bud. I have reason to now believe that my friend has magnolia trees. But, Wittgenstein asks, do I now have further confirmation of the earth's existence? After all, the

earth's existence is necessary for the existence of the trees. Yet given the above situation, it would never occur to us to believe we were just given additional reasons to believe in the earth's existence. This is one of the accepted "ends" or "stopping points" that Wittgenstein refers to in *On Certainty*, a ground that requires no further grounds.[7] To shift to a position of seeking evidence for the existence of the earth is to shift to a different language-game, which would have alternative "ends" or "stopping points". In our language-game we speak and act as if the earth exists. Not because we continually find good evidence for this belief, but because by accepting it we allow for successful action and communication.

While there is a stability and constancy to our language-games, Wittgenstein does not deny that language-games can change.

> On the other hand a language-game does change with time.[8]

> If we imagine the facts otherwise than as they are, certain language-games lose some of their importance, while others become important. And in this way there is an alteration - a gradual one - in the use of the vocabulary of a language.[9]

Given that Wittgenstein denies the idea of a fixed essence or an essential nature of a thing, essence can shift with shifting interests. There is no independent source for establishing real truth or meaning in the traditional foundationalist way. These notions, for Wittgenstein, are

relativized to language-games. Judgement and valid reasoning are grounded in linguistic and social practices that are arrived at by explicit and implicit consensus. Justification is not determined by a rational irrebuttable endpoint like physical description and conformity to sensory evidence, as in the case of the Holistic Model, rather it is relativized to a context and the expectations of a community of language users. If the interests and practices of the community change, so too do their methods of justification and their language-game.

However, missing from Wittgenstein's picture is an explanation of the dynamics of language change. What forces shape the gradual shifting interests he alludes to in the passage above? How is it that human practices can alter direction in such a way that we accept a different set of assumptions? Given that there is no perspective external to a particular language-game from which to make judgements, it is difficult to understand why language-games do not just remain constant and unchanging. Further on in this chapter, in the discussion of recent feminist epistemology, we will see how these authors attempt a more detailed description of the forces affecting language change, while at the same time maintaining that language-games are not radically relativistic. Since Wittgenstein really says nothing about conflict, incommensurable discourse, or social and political divisions, I will leave it to the discussion later to

reintroduce this topic.

Given the above general discussion of Wittgenstein's views in *On Certainty*, I now want to focus in on some related topics as they appear in his other works.

III. Language-Games, Private Languages, and Rule Following

Wittgenstein introduces the notion of a language-game in the *Blue Book*:

> These are ways of using signs simpler than those in which we use the signs of our highly complicated everyday language. Language-games are the forms of language with which a child begins to make use of words. The study of language-games is the study of primitive forms of language or primitive languages. If we want to study the problems of truth and falsehood, of agreement and disagreement of propostions with reality, of the nature of assertion, assumption and question, we shall with great advantage look at primitive forms of language in which these forms of thinking appear without the confusing background of highly complicated processes of thought. When we look at such simple forms of language the mental mist which seems to enshroud our ordinary use of language disappears. We see activities, reactions, which are clear-cut and transparent.[10]

Hence, language-games are the uses of language in which the meanings of words are clearly understood. By "primitive", Wittgenstein is not referring here to the anthropological sense of the early stages of human culture, but to our early or original stages of development. Primitive situations are those circumstances which prompt a child to form an utterance and to act in accordance with a standard way of

speaking. As children we are told that the thing we are drinking out of is called "cup". Those around us say "cup" each time the object appears. When we eventually get to the stage of uttering "cup" when we want that object, or answering "cup" when someone asks us "What is this called?" then we show that we know how to participate in this aspect of the language-game. We know more than just the name of the object, we have acquired the skill to use the name correctly by estimating what is an appropriate situation for its utterance. Primitive circumstances like this secure us in the activity of our language-game. Wittgenstein is suggesting that it is here that we should look to answer questions about meaning, truth, and knowledge since these situations form the framework out of which we interpret our reality.

What is important to note here, is Wittgenstein's intent to show language-games as containing the resources to understand our standard ways of talking and acting. He writes in *The Investigations*:

> I shall call the whole, consisting of language and the actions with which it is interwoven, the "language-game".[11]

The fact that meaning, truth, and knowledge are grounded in the practices of our linguistic communities makes them part of the public domain. This view is at odds with the idealist or psychologist claim that thought is independent of language or that language is an expression of independent

thoughts. On Wittgenstein's view of language-games, our mental states are socially constructed through the public mechanisms of expression and attribution. This is where Wittgenstein's well known argument for the impossibility of private language, sections 243-315 of *The Investigations*, comes into effect.

Language-games and the variety of modes of behaviour they involve, are the arbiters of semantic judgements. Hence, the domain for semantic evidence is the public domain. As a result, private language is impossible in much the same way that a private game would be impossible. By this Wittgenstein is not referring to games that a person may play alone or games in which some of the moves are only in the thoughts of the individual player. For Wittgenstein, these kinds of games are subsets or parasitic versions of a true game which is public and meaningful in virtue of actually engaging in play. Similarly, language is meaningful in virtue of public use. To generate our intuitions regarding the meaninglessness of a non-public game, Wittgenstein asks us to picture a "ball game in which the other player throws the ball to me and I throw it back to him in our imagination."[12] It is necessary for the play to make sense, that we know what happens to the ball, where it lands, whether or not it was caught. In the same way language requires a public reality. Without the agreement produced by common action and assertion, language ceases to

make sense on this view. A private language would lack the semantic regularity necessary to corroborate a speaker's reactions and to ground her memories in the consistent use of a term. It follows that any real language must refer only to things that can be publically verified so as to establish some pattern of regularity. Hence, in a private language there would be no way of distinguishing the actual rule-following of a language from apparent cases of rule-following. Just as in the case of the private game, there would be no means for determing whether or not I was playing the game by the rules if no one helped to regulate and verify my decisions. In section 202 of *The Investigations*, Wittgenstein states:

> and hence...`obeying a rule' is a practice. And to think one is obeying a rule is not to obey a rule. Hence it is not possible to obey a rule `privately'...

The question of how we might determine whether or not a person is following a rule does not amount to the question of whether or not the person has some particular representational formulation in her mind when she follows the rule. Rather, what Wittgenstein is concerned with is investigating the necessity of the broader behavioural and linguistic contexts in which rule-following takes place. This is for the reason that an internal image or picture is just a sign that requires a further interpretation. An internal picture is no different from a word in this sense.

Wittgenstein uses the example of a "cube"[13]. When we recognize the utterance of the word "cube", Wittgenstein argues that it does not help to imagine that an image comes before our minds to allow us to interpret the utterance. First, because the range of uses or "method of projection" for a term cannot be understood through an image, and secondly, by supposing the existence of a mental image, we have transferred the question to an internal context without explaining the mechanism of interpretation. As Wittgenstein says: "it is still absolutely inessential for the picture to exist in his imagination rather than as a drawing or model in front of him"[14].

Being able to see into someone's head will not reveal an answer to the question of what guides their behaviour in a particular circumstance. Consider another case in which we might actually wonder whether or not someone was following a rule. Suppose I ask someone to not smoke near me, and they in fact put out their cigarette. We might wonder whether they did it because they were following my instructions or for their own different reason. It could be the case that they had another cause for not smoking at that moment, their cigarette suddenly tasted stale for example. How in principle could we determine which is the case? Wittgenstein's answer is that it would not help matters if we had a means for looking into the person's mental and neurophysiological processers. This would only show the

internal object of their thought, not the specific interpretation for their behaviour. A better place to look is the broader linguistic and social context which could tell us whether or not the person has been brought up in such a way that they respect reasonable requests. Likewise we could discover whether they have been taught similar practices and observed similar customs. The best analysis will come by seeking the common framework of modes of behaviour which we and this person share.

Hence, it is the wider horizon of rule-related activity which is needed to answer questions of language use and rule following. This wider horizon is the language-game. It is only against the backdrop of a particular language-game that questions of rule following can meaningfully be asked. We are trained to master our language-game and play according to its rules. Wittgenstein maintains that we can be trained without formally being taught the rules. He makes this point in Section 31 of *The Investigations* referring to a game of chess:

> One can...imagine someone's having learned the game without ever learning or formulating the rules.

Wittgenstein is making the point that the training which takes place within a language-game is not propositional or formal in nature. It is the experience of participating in common practices of speaking and performing that constitutes our ability to follow the rules of the language-game.

Hence, in comparison with other philosophers, Wittgenstein's account of human knowledege is fundamentally social. The arguments against the possibility of private language, as well as the interweave of language-games and rule following with human social life, means that Wittgenstein does not present the problems of philosophy as though an individual arrived at forms of knowledge through singular moments of epistemic certainty. This overriding social aspect to Wittgenstein's philosophy serves as the springboard for feminst epistemologists who adopt a Social Constructivist model of theory-choice.

IV. Wittgenstein and Social Constructivism

In her article "Gender and Epistemic Negotiation", Elizabeth Potter writes:

> If Wittgenstein is right and the individual is not linguistically prior to the community, then the individual cannot be epistemically prior either. And it follows that the epistemic community cannot be comprised of a set of epistemically independent individuals; we must therefore, begin to view the community as comprised of epistemically interdependent individuals. Any adequate epistemology must analyze knowledge first in terms of the community and only then attend to the individual. Idealized models of epistemic agency proceeding as though the individual were, if not the source, then certainly the principle agent of knowledge, are at worst mistaken and at best put the epistemological cart before the horse.[15]

Potter represents a position that is common among feminist

epistemologists such as Longino and Scheman who advocate a Social Constructivist model of theory-choice[16]. On this view, communities are the primary epistemic agents, not the individual subject supposed by traditional epistemology. The move from the individual to the community as the primary epistemic agent stems from two related views. The first is an acceptance of arguments against a correspondence theory of truth. Given the view that our epistemology is one theory within a larger network of going theories which are all interconnected linguistically, there can be no extra-theoretic standpoint from which to determine anything like a correspondence with extra-linguistic reality. "Facts" on this view, are constitued and organized by our language use, they are not discovered "pure" and intact. Secondly, given the rejection of a correspondence theory of truth, justification now turns on an association with other people and the public methods for arriving at consensus. Therefore, it is the properties that arise communally which become the primary focus for a constructivist epistemology.

Like Wittgenstein, social constructivist philosophers view experience itself as fundamentally social, not something ascribed to individuals. Sensory experience, which provides important evidence for scientific claims, is viewed as dependent on the public discourses and practices of a culture.

Potter goes on in her article to argue that the production of knowledge intersects with social and political facts at the point where competing beliefs in a community must be decided upon. She recounts actual decision making processes by sociologists at the Salk Institute in which "micro-negotiations" and interchanges take place in deciding what constitutes a "legitimate quantity of peptides". Involved in the paradigmatic discussion were issues involving seminars and conferences, journal articles, as well as church, political and Institute business meetings. These micro-negotiations play a role, according to Potter, in larger "macro-negotiations" where negotiation shifts from a few individuals to hundreds of interested people who negotiate over months or years. Potter's observations of micro-negotiations leads her to the conclusion that what constitutes evidence for a claim is not determined by individuals, but by the standards a community accepts concomitantly while constructing and refining existing theories. These public standards, on Potter's analysis, constrain what an individual can know.

Potter also recounts the development of Boyle's Ideal Gas Law, which rested on the beliefs that the air has pressure and that the idea of a vacuum was possible, to show how the macro-negotiations which led to the Law's acceptance, were influenced by not only public standards of evidence, but also class and gender biases. Potter's claim

starts with the fact that Boyle was opposed politically to Franiscus Linus, a Jesuit who advanced the view of a "funiculus", an invisible cord which arose in a sealed tube because of nature's abhorrence of a vacuum. Linus' view was consistent with the Aristotelian paradigm and relied upon a well accepted set of Aristotelian explanations and empirical evidence.

Thus, Linus' explanation was an integral part of a religious and political outlook to which Boyle objected. By mounting strong scientific arguments against Roman Catholic religion and politics, Boyle helped to undercut their influence. Moreover, Potter goes on to suggest that Boyle was also waging a separate attack on Leveller women who were fighting for political change during the English Civil War (or Puritan Revolution) and an end to bourgeois liberal ideology. During this time, Boyle wrote his essays on women including "Letter to Fidelia", "Letter to Mrs. Drury" and "The Duty of a Mother's Being a Nurse". In these letters, Boyle criticized the radical change in the social position of women and in the ideology of equality. Potter sees this as consistent with Boyle's rejection of sectarian politics, grounded in a natural philosophy which postulated that matter was everywhere alive, and which put an emphasis on the individual soul and the idea that all people are created equally. Defeating the natural philosophy by showing matter to be inert, Boyle, and other mechanists, repressed much of

the political writing of the radical movement. When Charles II was restored to the throne in 1660 many of the radicals were imprisoned and often tortured for their views. Boyle meanwhile was a well respected member of the Royal Academy.

What Potter intends to show by these facts, is the role that social and political commitments play in the selection and choice of theories. While from our modern post-mechanistic perspective, Linus' theory of "funiculi" seems naive and somewhat silly, at the time it was supported by an existing body of credible evidence. While during this same period, outside of the laboratory, radicals advocating a natural philosophy, were threatening the existence of middle and upper-middle class Englishmen. Boyle's mechanistic and "anti-organicist" ideology, on Potter's view, prompted him to justifiy the claim that matter is innert and to have it accepted by the ruling classes whose power was in jeopardy. At the end of her article, Potter claims that in no way does she see the development of Boyle's Law as "bad science". She writes:

> I take the conclusion that the intersection of politics with the content of scientific theories is irrational, to be absurd. Instead, it seems quite reasonable, when the data do not uniquely select one theory, to select the theory that coheres with one's world view.[17]

Given the arguments for underdetermination, epistemologists like Potter, advance reasons to believe that social and political factors can be influential in deciding among competing theories. This is also consistent with

Kuhn's analysis of scientific development as being historically determined and contingent, and Putnam's view that science is a process that involves discovering statements which are true and "relevant" to a wide set of interests and values[18]. What is argued for by Potter and others, is not a simple reduction of epistemology to social and political issues, but rather the need for an adequate epistemological model that attends to the complex ways in which social values influence negotiation, justification and decision making procedures. However, feminist social constructivists are not only seeking to clarify the history of decision making for purely intellectual purposes, they are also seeking to reach an emancipatory goal consistent with the beliefs of the researchers discussed earlier. For this reason, the working model adopted from the later Wittgenstein needs to restrict the possibility of radical relativism.

The Constructivist Model, unlike the Holistic Model inspired by Quine, provides a way of conceptualizing the integration of values into the epistemic realm without struggling to blur a boundary between science and politics. This is for the reason that on the constructivist view, values are constituents in the construction of truth. They serve as one factor among many leading to the "normative" quality of language and social practice. On the Holistic view, as discussed earlier, it was difficult to relate

values to scientific theory-choice because of Quine's insistence on the lack of empirical controls for values. Nelson tried to counteract this claim by "rubbing out" Quine's ad hoc last boundary. As I have tried to show in Chapter Two however, the project of naturalized epistemology and Quinean holism is itself unable to meet the task of explaining the integration of politically guided research withing science, particularly when that research has as its goal an emancipatory end.

On the Constructivist Model, social and political experience becomes something we create and are trained to use, rather than something transcendently normative. So, the problem of biased and unfair science becomes then, the question of what processes are involved by which members of social groups are constructed as subjects and understood? What are the conditions that contribute to the material, economic, and interpersonal relations that constitute these social categories? And more importantly, how can the processess which determine "truth" be made more just? Answers to these questions will involve an evaluation of scientific practice which for the constructivist, depends upon our linguistic and social practices. Yet how do we reconcile the ideas of a truer and more just social structure with claims made by Wittgenstein like the following from *On Certainty*:

> Suppose we met people who did not regard the propositions of physics as a telling reason.

> Now how do we imagine this? Instead of the physicist, they consult an oracle. (And for this we consider them primitive.) Is it wrong for them to consult an oracle and be guided by it? If we call this "wrong" aren't we using our language-game as a base from which to combat theirs?[19]

Wittgenstein seems to recognize here that ultimately some form of persuasion would be needed to convince these "primitive" people that our criteria for truth are better. The fact that our language is grounded in common practice gives an objectivity to our discourse so that for Wittgenstein, we can judge what is true or correct. Yet how might we analyze a situation, like the one above, where there is a conflict or incommensurability in discourses? It is not clear on Wittgenstein's view how we might interpret such a situation in which practical consensus breaks down and there is epistemic conflict because Wittgenstein has consistently argued against the notion of a transcendent, truly "real" reality. Justification is not determined by a rational, irrebuttable endpoint and therefore as Wittgenstein claims, it must instead be relative to the expectations and practices of a community. Yet what guides rational action when disputes arise and the "community of interdependent knowers" clashes? This question is especially important for philosophers like Potter, Longino and Scheman, who see alternative scientific research as offering emancipatory results for socially marginalized and oppressed people. Given the dispute with current scientific

and epistemic practices which exclude or ignore the categories of race, class and gender, those feminist epistemologists who advocate a Social Constructivist Model need to offer an analysis of their challenge that is in line with the Wittgensteinian view of meaning, but does not slide into a radical relativism.

Longino, in her paper "The Fate of Knowledge in Social Theories of Science"[20], addresses the question of radical relativism from the perspective of a socially constructed epistemology, using the example of a theoretical conflict between creationism and darwinism. Longino argues that what is required to explain why one theory is "better" than another is an analysis of factors like public forums for critical interaction, public standards, equality of intellectual authority among diverse perspectives and how criticism on a public level, is managed. For Longino, the status of observation and evidence plays only a minimal role in the dynamics of theory-choice. This is for the reason that data are never interpreted without social negotiation and critical discussion among group members. These social factors constitute the publicly validated goals that will decide in matters of theory-choice. On Longino's view, given a commitment to free and open criticism within the context of commmunally acknowledged goals, obviously wrong-headed views would not meet the warranting conditions necessary to count as knowledge. Creationism would not

withstand the critical scrutiny and challenges that would be mounted against it during the above procedures. Furthermore, it would require a public commitment to salvation that would be at odds with the other goals of an open, inquiring community. Longino sums up her position as "construction with constraint"[21] in that theories are built up out of social needs and practices while at the same time their selection is conditioned by discursive interactions that meet public criteria for effective community criticism. All aspects of this process would be continually open to reflection and reformulation in the light of changing factors. The only illegitimate move we could make is to advance context independent rules of inquiry that must hold for all communities. Hence for Longino, a socially constructed view of knowledge does not need to lead to a radical relativism.

Having put in place what I see as a reasonable interpretation of the Wittgensteinian influenced model of social constructivism, I would now like to turn to some criticisms of this model. As in the last chapter, I will argue first that the model relies upon a view of language, and more specifically meaning, that is unduly skeptical and limited. And secondly, by replacing this view of meaning with a more robust semantic realism, we will wind up with an improved way to explain methodology that has as its goal social and political emancipation.

V. Problems with Social Constructivism

I will adress my criticisms against the general position of social constructivism by first returning to Wittgenstein's view of the fundamentally social character of languaguage and knowledge.

To begin, I want to reconsider Wittgenstein's argument against the possibility of private language and rule following. As in the case of Quine's arguments against meaning, I will argue that Wittgenstein's view fails to account for a theory of meaning along the lines of a realist semantics as argued for by Katz. By outlining Katz's position against Wittgenstein, I will show how the social character of language and knowledge can be better understood via a theory of abstract senses which explains meaning in such a way as to avoid the problems of psychologistic and socially constructed theories of meaning. I will then argue further that along with these linguistic considerations, there are reasons both epistemic and metaphysical to question Wittgenstein's socially constructed reality. Moreover, given these considerations, feminist and social epistemologists will not escape the problem of radical relativism by building a model-theoretic explanation of theory-choice atop a Wittgensteinain epistemology.

Wittgenstein, and later social constructivists influenced by his views of meaning and knowledge, argue that

there is no way to make sense of a hidden or underlying semantic reality in virtue of which our public, collective communication and judgement makes sense. Philosophers who seek some transcendent norm only confuse the issue in that they must explain how we have knowledge of some realm which we cannot be in causal contact with. Hence, Wittgenstein advances the private language argument and the paradox of following the rule to show how no norms beyond the convention of use, exist to guide our linguistic and epistemic practices.

Yet by accepting a view of meaning as use, important linguistic facts are inadequately explained. Specifically facts like synonomy, ambiguity, analyticity, necessity, literalness, non-literalness and redundancy. Katz, in his work, attempts an explanation of these facts which entails positing the existence of abstract objects which provide norms for linguistic behaviour. As I will try to show here, given the arguments for the existence of these abstract, non-psychological relations, Wittgenstein's reasons for thinking that there can be no objective norms outside of social contexts, should be rejected.

On the realist account of linguistics, as suggested by Katz, language is understood as a set of abstract objects. Like the psychologist or conceptualist, the realist recognizes a distinction between "knowledge" of a language and the language itself. What this difference amounts to

for the realist is that the set of abstract objects which make up the language has an ontological status independent of a speaker/hearer's knowledge of this set.

In Chomsky's work[22], the difference between the language and the language user is explanined in psychological terms as a "Competence/Performance" distinction. Competence refers to a person's knowledge of her language, the system of rules which she has mastered so that she is able to understand an indefinite number of sentences and to recognize grammatical mistakes and ambiguities. Competence can be understood as the idealized representation of language, which is seen as distinct from performance.

Performance is the actual process by which speech is produced by native speakers. Actual utterances will contain features that are irrelevant to the phonological, syntactic and lexical rules which reflect the native speaker's competence. Lapses of memory or biological limitations are features of performance which will be discounted by the linguist constructing a grammar.

The linguistic realist differs from the conceptualist in accounting for the system of rules which constitute grammar. The conceptualist takes the system of rules to be an idealization of the speaker's language faculty. However, the realist argues that the language, the object picked out by the grammar, has a status apart from a speaker's

knowledge of it. The language is a set of abstract objects which includes infinitely many senses and sentence types. Generally, it is agreed by most linguists that there are an infinite number of possible grammatical sentences that could be constructed in any natural language. This data is interpreted by the realist in the following way. Each sentence, if it were uttered in English, for example, would be a token of an abstract linguistic type. So if I said "Looking for an affordable apartment is a tiring chore", my utterance would correspond to the abstract sentence type `Looking for an affordable apartment is a tiring chore'. The token utterance gets its meaning in virtue of the sense of the type. The sense of a sentence type arises as a result of the compositional relationship of the senses of the word types. This relationship constitutes the literal meaning of the token. The meaning of a particular token utterance is a function of the literal meaning of the type of which it is a token, and various pragmatic or contextual facts about the utterance environment. In Katz's view, the pragmatic account suggested by Grice[23] can be used to fill in the necessary contextual factors to correctly interpret the utterance.

Grice's idea is that the pragmatic considerations involved in a speaker meaning something, has a structure which is determined by specific internal and external constraints. The internal constraint is a reflection of the

speaker's intention to mean something, and the external constraint involves the fact of communication being a cooperative enterprise.[24] For Grice, the internal constraint is the intention for the speaker to bring about a certain state of mind in the hearer of the utterance in virtue of the hearer's recognition of the speaker's intention. So in effect, there is an ends-means quality to conversation in that the end is the intended effect the communication will bring about in the mind of the hearer. The means are the cooperative processes of those involved who are committed to the communication being successful.

Grice then suggests principles that guide this process by which communication is advanced toward understanding. These principles, which include maxims like "Contributions should be as informative as required, no more" and "Contributions should be true and supported as well as is reasonable in the circumstances" frame the expectations of people engaged in communication. This analysis of literal meaning coupled with pragmatic meaning explains how in particular circumstances tokens of the same type can result in different recognitions of intent. For example, in looking for an apartment with a friend as we descend the fifth flight of stairs in the last apartment we will look at for the day, when I utter "Looking for an affordable apartment is a tiring chore" she responds with a weary nod of the head, I see she has understood my utterance

literally. However, if on a different day we are both picked up by a realtor in an air-conditioned limousine and served champagne as we sit comfortably back, when I utter the same token sentence, my friend's laugh is evidence of her having recognized the irony in my statement. By construing meaning as a function of the literal meaning of sentence types and pragmatic considerations which are involved in token utterances, Katz offers an analysis that goes directly to the question of the application of an utterance, which was a concern for Wittgenstein. Wittgenstein argued that a mental image of a cube fails to determine the range of application for the use of the word "cube". Remember that it was for the reason that a mental picture was just an "internal" version of the concrete written or spoken word and what we were after was an interpretation of that object. On Katz's view, there is a source other than use, to set semantic norms involved in judging appropriate linguistic behaviour. In virtue of being competent speakers of English, fluent speakers have the ability to grasp the literal sense of the word "cube" as `regular solid of six equal square sides' and to reason pragmatically so as to communicate and realize intentions in actual conversation.

Katz argues further that the whole use of the word is not grasped in the native speaker's apprehension but only the literal use. Hence Wittgenstein is right to think it

"queer that the whole use of the word comes before the mind"[25]. On Katz's view the non-literal pragmatic sense of the word cannot be known a priori. Katz says:

> In the case of literal application, what comes before our mind in the act of grasping a sense is nothing more than what, qua speakers of English, we can get from our knowledge of the grammar of the types in our language and the principles for assigning tokens to types. Thus, extrasemantic features of the word "cube" cannot be supposed to come before the mind.[26]

As Katz argues, our recognition of linguistic facts like "non-literalness" and "irony" depend for their starting point on grasping the literal meaning of types so that pragmatic reasoning may be successfully carried out. This grasping is not a kind of psychological introspection but rather the intuition (in the mathematical and logical sense) of an abstract grammatical object which sets the literal range of the extension for a token of the word type.

The most significant reason advanced by Katz, for arguing that the norms of linguistic behaviour are set by the existence of sense relations, is the necessity of certain truths. By explaining linguistic facts in terms of mind-independent entities, our meaning theory can explain our intuitions of necessary entailments, linguistic contradictions, and meaninglessness through the existence of sense relationships. By supposing the structure contained within our well-formed utterances to correspond with a realm of abstract senses and sentence types rather than with the

particular facts of individual psychology, we can say why it is so counter-intuitive to imagine the sense of "murder" ever entailing something other than the sense of "kill". In addition, sentences that are intuitively grammatical, but are too long to be uttered by any individual, like "My father's father's....was a man.", where "father's" occurs 10,000 times; can still be explained as meaningful since meaningfulness is not essentially determined by use.

Katz offers the option of understanding the normative quality of language in terms of abstract senses and types. As such, it presents a preferable choice to the Wittgensteinian move of locating linguistic normativity in the practices of a language community. As Katz writes:

> Our linguistic realism provides an alternative to Wittgenstein's view on either the "conventionalistic" or "individualistic" interpretation. The former is championed by philosophers like Winch and Kripke, and the latter by philosophers like Stroud and McGinn. We have no need to take a stand on the exegetical issue, since both interpretations portray Wittgenstein's late philosophy as grounding linguistic normativity in facts about the natural world, in particular, facts about human behaviour. Linguistic realism presents an alternative to the late philosophy in that neither the linguist nor the philosopher is describing natural facts in describing the criteria for correctness in the use of a natural language.[27]

This matter of linguistic normativity is behind the paradox of following the rule, and it is also behind the argument against a private language. This is for the reason that a language that exists only in the mind of an individual

speaker, lacks the necessary controls to determine consistent and correct usage. So, does accepting the option of linguistic realism entail that a private language is possible? In my view the answer to this question is no. In fact, I would argue that being a member of a linguistic community is one natural fact that is ingredient in developing the necessary intuitions to be a speaker of a language. This idea is consistent with the view of linguists that interaction with other persons is psychologically necessary to learn language.[28] Assuming our best anthropological science, human beings have always lived in communities. The focus on the community that is urged by feminist and social constructivist epistemologists is well placed in my view. The problem however, is in the move by philosophers like Potter and Longino who seek to also locate normativity in those same quarters. Linguistic, epistemic and metaphysical realism, does not have to lead to an advocation of staunch individualism. That is, we can admit the necessity and variety of multiple perspectives for arriving at reliable knowledge, without ignoring the independence of what is known. The point being that exposure to the particularities of social experience can promote the kind of conditions necessary for understanding reality. The mistake, is to slide from the limited social perspective of the knower to a dismissal of the norms for knowledge. So, in the same way, interaction with our

community of knowers provides us with the conditions for realizing the abstract relations inherent in language and knowledge. The mistake is to locate these constraints in the limited practices of a language-game.

Feminist and social constructivist epistemologists concerned with explaining the overlooked social and political character of knowledge, draw upon Wittgensteinian arguments for the fundamentally social character of language. However, these arguments fail to capture significant linguistic facts and in my view, are less scientifically true to the data of language. Moreover, extending the Wittgensteinian move of locating the normativity of language in the varying practices of language communities, to the normativity of knowledge located in those same varying practices, results in the serious problem of relativism. This problem is especially important for feminists who will need a strong normative basis from which to make their claims of injustice and inequity. Regarding this point, I am very much in agreement with Phillip Kitcher when he writes:

> Mindful of the harm that has been done by treating some standards as objective, some conclusions as established, and some positions as superior, critics of traditional epistemology propose that we rethink our reigning metaphors. While I believe that these considerations are powerful and significant, and that they call for detailed exploration of the ways in which the growth of human knowledge has effected human well-being, the fault may not lie with the epistemological notions of objectivity, truth, and epistemic superiority but rather with the ways in

> which these notions have been too hastily applied to support the prejudices and further the interests of a dominant group. Concern for objectivity seems to me to be potentially liberating...[29]

In my view, it is right to argue that epistemology has suffered from ignoring the social factors that play a role in human knowledge. However, I disagree that by accepting this position we also accept a straightfoward move to a naturalistic approach to language and knowledge. Granted, a realist conception of language and epistemology ushers in the problem of how we, as spatio-temporal beings, could have knowledge of abstract objects. This problem, most notably raised in philosophy of mathematics by Paul Benacerraf[30], claims that knowledge of abstract objects requires that we, as natural beings would have to have some kind of causal contact with abstract entities. Since abstract objects cannot be reached causally, it would be impossible for us to have knowledge of them. Since we do have knowledge of mathematics, for example, our knowledge must be of something other than abstract objects.

However, this objection rests on the acceptance of a causal theory of knowledge across the board. In his paper, "What Mathematical Knowledge Could Be"[31]. Katz argues that there is no reason to think that a causal theory of knowledge generalizes to all cases of knowledge. Katz's argument turns on showing how formal knowledge of numbers, propositions and expressions like "Seventeen is a prime

number" or "No proposition is both true and false", cannot rest on reference to objects or facts in the natural world. Rather, the necessary truth of these cases provides counter-examples to a causal condition on knowledge generally. Hence, the criticism of realism based on the view that empirical knowledge cannot interact with formal knowledge is correct. However, this does not show that knowledge of abstract objects is impossible, only that it is non-empirical. The alternative proposed by Katz, is to understand formal knowledge as what we know by intuition and "the light of reason."[32]

The acceptance of this formal component of knowledge explains the truths of language, logic and mathematics in a more thorough way than the naturalist models considered in this chapter and the last. Hence, as Quine has stated, we must countenance the existence of those objects to which the bound variables of our theories refer to in order that these statements may be true.[33] If these entities include sets, propositions and senses then it follows that we have a commitment to them.

Finally, because I see the need for a more inclusive epistemology that is communally centered and sensitive to social and political concerns, relativism should be carefully avoided. A realist conception of language and knowledge provides us with a route that is secure.

Having given what I see as reasons for rejecting both

the Holistic and Constructivist Models of theory-choice, I would now like to turn to epistemological questions of objectivity, inference and justification. I will propose a Realist inspired model that incorporates the concerns just mentioned.

NOTES

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3. Ibid., Section 279.

4. Ibid., Section 601.

5. Ibid., Section 156.

6. Ibid., Section 208.

7. Ibid., Section 204.

8. Ibid., Section 256.

9. Ibid., Section 63.

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13. Wittgenstein. 1953. Section 139-140.

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23. Grice, H.P. 1975. "Logic and Conversation". In *Syntax and Semantics, Vol. III*. P. Cole & J.L. Morgan, eds. Academic Press. New York, NY. pp. 41-58.

24. Katz, Jerrold J. 1990. *The Metaphysics of Meaning*. MIT Press. Cambridge, Mass. p. 147.

25. Wittgenstein. 1953. Section 197.

26. Katz. 1990. p. 149.

27. Katz. 1990. p.125-126.

28. See for example Burge, Tyler. 1989. "Wherein is Language Social". In *Reflections on Chomsky*. Basil Blackwell. Oxford. UK. pp. 175-191. Also Chomsky, Noam. 1986. *Knowledge of Language: Its Nature, Origin, and Use*. Praeger. New York, NY. p. 18.

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30. Benacerraf, Paul. 1973. "Mathematical Truth". *The Journal of Philosophy*, Volume 70. pp. 661-79.

31. Katz, J.J. 1995. "What Mathematical Knowledge Could Be". In *Mind*, Volume 104. 415. July 1995. pp. 491-522.

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CHAPTER FOUR

Social Communities and Objectivity

I. Introduction

In the epistemological models considered in Chapters Two and Three, the central naturalistic component was the view that the psychological and sociological factors that contribute to a person's belief also contribute to that belief's warrantability. What this leads to, for naturalized epistemologists, is the evaluation of belief and the related notions of evidence and justification according to relevant psychological and sociological factors along with sensory experience. Holists advocate a more central place in their models for sensory experience, cognition and psychological processes in general. Constructivists, on the other hand, advocate the replacement of traditional logical and empiricist foundations in epistemology with sociology and political theory. Both groups agree that the measure of a belief's warrantability is significantly linked to the question of what factors caused the belief as well as factors pertaining to the social location and position of the believer. Moreover, both groups agree that the question of when to overhaul beliefs is answered not by a rational procedure for more truth, but instead on the basis of previously held beliefs and/or prejudices. In a sense, what we operate from are "rules of thumb", in which we are

constrained by a general principle of non-contradiction and a concession to as modest revisions possible.

However, social-feminist epistemologists like the ones discussed in the previous chapters, also want to criticize past epistemic accounts for their exclusivity and ignorance of the perspective of gender. Patterns of inference and reasoning, according to philosophers like Nelson and Longino, have been advocated and adopted, praised or blamed, in light of background biased assumptions. What is missing in present mainstream epistemology, is an analysis of these biases from the perspective of gender. Consider for example, a hypothesis tested in behavioral endocrinology to determine what the effects of prenatal hormones have on spatial and mathematical ability.[1] The hypothesis sought to test the apparent sex differences in human cognitive ability, specifically the apparent superiority of male mathematical and spatial ability. To confirm this hypothesis, researchers injected male and female rats with large doses of androgen, a male hormone. As the hypothesis predicted, rats negotiated mazes with greater success after having the injection.

The experiment was taken to establish that male hormones increase spatial ability in rats. However, in assuming this relationship, there are a host of unconsidered background assumptions. These include a model that relates hormones directly to human cognitive ability, the assumption

that there are sex differences in cognitive abilities and that these are explained biologically, and the assumption that negotiating mazes on the part of rodents has a connection to human mathematical ability. Taking these assumptions together, it is then reasonable to conclude that the experiment provides an explanation for alleged male mathematical superiority.

One of the norms used in judging whether an experiment confirms a hypothesis is "explanatory power". What this norm amounts to is determining that the evidence in question confirms the hypothesis only if no credible alternative hypothesis explains the evidence equally well.[2] The researchers in the experiment described above assumed there was no such alternative hypothesis. Following these experiments, feminist biologists in the field offered and tested a broader hypothesis: that hormones generally, not specifically male hormones, will improve a rat's spatial ability, or more appropriately, a rat's ability to negotiate a maze. This hypothesis was tested by injecting male and female rats with large doses of estrogen, the female hormone. The rats performed just as they did in the earlier experiment.[3]

How would the above case be analyzed by feminist epistemologists and philosophers of science? First, Nelson and Longino would presumably argue that given the background assumptions and hypotheses of the first group of

researchers, including their common sense and scientific beliefs, the conclusion reached was reasonable.[4] Secondly, they would deny that the above research was flawed because it was underwritten by assumptions, research and theories in a wider scope. The fact that this first group of scientists sought to synthesize their body of beliefs is a sign of good science.[5] When the second group of scientists challenged this conclusion it was based on criticisms of the set of assumptions and broader theories that informed the work of the first group, assumptions as to the biological basis for sex-differentiated performance in mathematics and sex-differences in cognitive ability. It seems reasonable to assume that the first group of researchers would view their initial results as unconfirmed given the second group's results.[6] Hence, the role of social and feminist epistemology and philosophy of science is to foster the articulation and development of mechanisms for understanding the nature of evidence relations and their connection with background assumptions and biases. Along these lines, it is also a goal to foster an awareness on the part of scientists who inevitably bring such assumptions into the foreground of their scientific reasoning.

However, as I have argued in Chapters Two and Three, the feminists discussed thus far, embrace a naturalistic interpretation of knowledge and the result is that they are hard-pressed to come up with an agent-neutral or community-

neutral standard of evaluation to condemn the background assumptions they have found in epistemology and science.

In the chapter that follows, I will draw out the problem that inheres in these feminist accounts of knowledge, the problem of arguing for both the partiality of the concept of objectivity while presupposing the very concept under attack. In other words, if we accept arguments that objectivity and supposed value-neutral standards of evidence, justification and confirmation are laden with social and political assumptions, then what will ground a critique of those practices that is not itself subject to the same criticism? After all, feminists do not want to argue that the "right" epistemic account will only be determined by an appeal to power. For this reason, a notion of truth must be upheld that has some prescriptive force and is also acceptable to collective human enterprises. What underwrites this notion is some version of objectivity. However, these very notions are under attack for their partiality.

This problem is referred to as the "Paradox of Bias", by Louise Antony in her article "Quine as Feminist: The Radical Import of Naturalized Epistemology"[7]. A paradox is generally understood as a situation in which a set of statements that are mutually exclusive of each other, all appear to be true. In this case we have one claim which says that impartiality is the wrong strategy to adopt in

epistemic investigation since it serves as a pretense for maintaining biased beliefs. The second claim is that partiality is the wrong strategy to adopt in epistemic investigation since it results in biased beliefs. Hence, feminists cannot both condemn impartiality and simultaneously condemn partiality as strategies in epistemic investigation without posing a dilemma.

Antony points to this problem in feminist epistemology while recognizing the importance of feminist critiques of knowledge. With regard to the broad strokes, I find Antony's view right on target. However, when it comes to her solution to the paradox, her argument runs astray. In the next section, I will summarize her view and point to the places at which her commitment to naturalism undermines the very problem she sets out to solve. I believe Antony's argument to be one of the most concise, well-formulated versions of feminist naturalism in the literature. Hence, I use it in this chapter as a sounding board, since it provides a clear opportunity to show why naturalized epistemology will not lead feminist epistemologists out of the paradox of bias.

II. Antony's Historical Account

Antony begins her argument by acknowledging a legitimacy of feminist contributions to the theory of

knowledge. She sees feminist work as offering an "awareness of experience in which specific questions and problems arise that any adequate epistemology must accommodate."[8] Antony is referring to the way in which the history of philosophy and science has been committed to an ideology of objectivity that either ignored or suppressed evidence which ran contrary to the special interests of a privileged group. Antony, like Nelson, sees the project of exposing the limits of this ideology as one that can be carried out in the mainstreams of analytic philosophy, in particular on the model of naturalized epistemology originally put forward by Quine.

Antony differs from Nelson however, in that she finds most of the present feminist descriptions of traditional modern philosophy to be "cartoonish characterizations" of the actual problems and theories that have developed and shaped the analytic tradition.[9] Antony goes back to the early modern distinction between rationalism and empiricism to show how the insights of Descartes and Hume and their influence on twentieth century Anglo-American thought, have been grossly misunderstood by feminist theorists in epistemology. While there are some points throughout her description of contemporary feminist epistemology that are themselves sometimes "cartoonish" and oversimplified, Antony's historical picture seems to me to be right. I will give a summary view of her discussion of Descartes and Hume

and supplement this discussion with an account of Kant, an account I see as contributing to the kind of interpretation that Antony argues has been lost in most of the present feminist accounts of these philosophers.

Briefly put, Antony argues that Descartes was not an advocate of disinterested, disembodied, radical solipsism as he has often been characterized by many feminists. His method of systematic doubt was not inconsistent with the view that experience is important and engagement with the world can contribute to finding the truth. Descartes expressed contempt for "cloistered teachers" who were unable to form a just conception of the world by ignoring the variety of human experience.[10] One important influence that Descartes' brand of rationalism has had on succeeding generations has been the belief that the mind is natively structured and as such it sets limits on the concepts and hypotheses that can be formed in response to experience.

In Hume's case, Antony points out that one of his significant contributions to subsequent Anglo-American thought was introducing the "Problem of Induction". Belief in induction, Hume concluded, was a custom or tendency of our mental structure being ingrained by nature. It is a species of natural instinct which no reasoning processes are able to either produce or prevent.[11] Like it or not, Hume argued, we are saddled with our belief in induction since we are constitutionally unable of doubting it and conceptually

unable to justify it. Therefore our only hope is *explaining* it. Hume's insight set the stage for approaches in epistemology which sought to figure out how we as limited cognizers can produce such an expansive system of knowledge enabling us to flourish and survive. Hume's skepticism and Locke's "copy theory" both posit knowledge as derivative of experiences, though their notions of "experience" differ from rationalists. After arguing that all knowledge is derived from empirically observable, discreet and unconnected data, Hume was led to wonder: What is it that unites our impressions and ideas and makes us experience them as connected?[12] Unable to really answer this question and apparently unwilling to deviate from his original theory, Hume concluded that it was social custom or convention which made coincidental experiences appear to be related to each other in a cause and effect sequence.[13]

Antony argues that both Descartes and Hume agreed that the mind was natively structured and that structure partially determined the shape of what we call "knowledge". Their disagreement was about the level of specific constraints imposed by this mental structure. However, both schools of thought were committed to the question of the possibility of providing a rational justification for the processes by which human beings arrive at theories of reality. What is lost in feminist accounts of "traditional philosophy" and this early modern period, is the enormous

controversy between rationalists and empiricists regarding the extent to which the structure of the mind might constrain the development of knowledge.

To further Antony's account I want to add Kant to her list, as his views are often characterized by many feminists as advancing the idea that all knowledge comes from reason and the attainment of knowledge is an essentially solitary pursuit that has no experiential preconditions.[14]

In his critique of the Humean system, Kant declared that it was not in reality an actual theory of knowledge precisely because it could not explain the process of synthesis which had troubled Hume. Knowledge, on Kant's view was more than the accumulation of facts and experiences, it was the ordering and understanding of what one had observed. Hence, the actual epistemological issue for Kant was how is it possible that we can differentiate "experiences" from "non-experiences". If all knowledge was, as Hume said, derived from empirical observation, then how can we explain the presence of the intuitive category "not experience". That is, if all knowledge comes to us through experience, how could we even have a category in our minds which deals with things not experienced? Kant's conclusion was that our very ability to formulate this notion constituted a proof of the objective reality of an "intuition" responsible for ordering reality. In addressing the problem of induction Kant wrote:

> "I could not only prove the objective reality of the concept of cause with reference to objects of experience but also deduce it as an a priori concept because of the necessity of the connection it implies. That is, I could show its possibility from pure understanding without any empirical sources...If anything is lacking, it is the conditions for the applications of these categories, and especially that of causality, to objects. This condition is intuition, and, when it is lacking, this application for the purpose of theoretical *knowledge* of the object as noumenon is rendered impossible."[15]

In a sense, simply knowing the meaning of the words "cause" and "effect", one can deduce the relation between the two notions without ever having an empirical illustration. Kant is drawing attention to our ability to recognize and distinguish a cause from an effect in the first place.

The point I want to bring out here is that for Kant, attention to raw experiences is not knowledge at all. A theory of knowledge will be required to account for the way that experiences are interpreted by the knowing subject. We can talk about different experiences leading to different conclusions about life, but this is only to speak about experiences as "information" that leads to knowledge. For Kant, the ability to interpret the information of daily life is a universal feature of the human mind.

Kant's insight is that in order to have knowledge, the mind must act on lived reality, and must be able to order it according to principles which are themselves rational and universal rather than empirical and particular. The Kantian project was not therefore, committed to the self-sufficiency

of reason or the inessential nature of experience. To characterize it as such is to lose the insight.

Returning to Antony's historical account, she maintains that the rationalist proposal to answer the mystery of knowledge was the positing of innate structures of the human mind. Because sensory experience by itself is insufficient to account for the extent of human knowledge, innate structures must be natively present as part of the human essence. Therefore, the regularities that we acknowledge in our experiences with the world, are in large measure, regularities that we are "built" to perceive. However, by solving one problem with innateness, rationalists inherited another problem, namely; How can we be sure that the ideas that result from our innate structures and help to form our theory of reality, are actually getting us to the right theory? As Antony puts it "Innate ideas lead us somewhere, but do they take us where we want to go?"[16]

The empiricist project in the early modern era, influenced by Hume, abandoned the project of seeking an external justification for our knowledge. Empiricists argued that we can never manage to offer a justification for epistemic norms without somehow presupposing the very norms we wish to justify.[17] In other words, to argue that the methods and principles I use to justify my knowledge, such things as observation, probability or deduction, are correct, is to assume the legitimacy of the very principles

in question. I can only justify these principles by an appeal to further observations, probabilities or deductions. For example, I consider myself to know that Brooklyn is one of the five boroughs of New York City. I believe myself to be justified in believing this claim on the basis of observation and reliable testimony from others. I justify these observations and testimony on the basis of past reliable observations and reports, which is itself an employment of an inductive principle. Hence, my justificatory account is circular. Therefore, according to empiricists, we must accept the problem of induction and recognize that the future of epistemology will involve only characterizing our given cognitive natures rather than providing a justification for them.

The distinction between rationalism and empiricism that Antony alludes to, paved the way for one segment of the logical positivist project aimed at improving upon Hume by taking all the material that was needed to legitimize scientific practice, but was not explained through experience, and making it a matter of linguistic conventions. The success of this particular positivist project depended upon there being a viable distinction between synthetic and analytic statements along with the possibility of reducing statements with empirical content to claims about sensory experience. But as Antony explains, this project came under considerable attack in the 1950's by

Quine along with Hempel, Goodman, Putnam and others.[18]

Quine's general epistemic picture has already been laid out in Chapter Two. My presentation is very much in line with the account of Quine that Antony gives.[19] However, Antony and I diverge at the point where she uses Quine and naturalized epistemology to solve the paradox of bias we both agree inheres in feminist epistemology.

III. Antony's (Unsuccessful) Solution to the Paradox

Antony begins her solution to the problem of bias by explaining that Quine's initial arguments for naturalized epistemology were at odds with his a prioristic commitment to behaviorism. Psychological behaviorism was shown to be severely limited, particularly with regard to language, by Chomsky in his review of B.F. Skinnner's *Verbal Behavior*.[20] As Antony characterizes him, Chomsky viewed the best explanation of the processes of language acquisition and the knowledge obtained through language, to be the result of a set of innate biases limiting the kinds of linguistic hypotheses possible. The result for Antony, is that Chomsky, rather than Quine, should be viewed as "the naturalized epistemologist *par excellence*."[21] in that he has provided an empirical vindication of rationalism. Subsequent work adopting nativism along with the import of rationalism in a naturalized scheme, provide the tools for

Antony to solve the paradox of bias.

The element of rationalism that Antony sees as having been imported into naturalized epistemology is the value rationalists have upheld for a limiting conception of the human mind. If the human mind was "completely open" as the early empiricists thought, then we could never construct the rich systems of knowledge we construct in the short time we have. Assuming the correctness of the thesis of underdetermination, an unbiased mind would have the potential to generate an infinite number of logically possible theories and be unable to decide between those that actually are generated in our finite experience. Paring down the alternatives is according to Antony, "the defining feature of the human epistemic condition"[22]. So, one limiting condition of our experience is native conceptual structure. This result provides Antony with an empirical ground for rejecting pure neutrality as an epistemic ideal, and for valuing biases that help to cut down our acquisition of knowledge to manageable proportions. However, Antony's assumption rests on conceiving nativism in its least controversial form that is, simply as a system of innate concepts. Yet, in present debates between rationalists and empiricists in the philosophy of language, the character of a native conceptual structure is a controversial matter.[23] I will say more about this problem in the next section. Suffice it say at this point, that Antony assumes some

generalized notion of native conceptual structure as responsible for setting limits on the kind of psychological procedures which make knowledge possible. Given this, one element of the paradox of bias (namely, explaining our partiality), can be understood by the fact that our disposition to believe what we believe, arises from our innate conceptual structure. However to get out of the paradox, Antony needs to show why certain kinds of partiality are bad.

Antony claims that the evaluation of bias is an empirical, as opposed to an a priori question and that a naturalistic study of knowledge can show us when bias does and does not facilitate the gathering of truth and knowledge. This is done, on her view, by holistically evaluating the overall theories and background assumptions in which the biases operate. This suggests that Antony is advocating a coherence model of justification in which good evidence is distinguished from bad evidence by evaluating the statement's consistent coherence with a wider set of beliefs. However, Antony is not committed to a "pure" coherence in that she advocates a "confirmational hierarchy" in situations of scientific investigation.[24] Emotions, desires, and social values should be understood, on her view, as epistemically distorting the discovery of empirical facts and therefore should be set aside when an investigator pursues data. This ideal objective method will

have priority over alternative methods since the divestiture of personal goals, moral values, or hunches will result in more reliable scientific theories. Upholding an ideal of objectivity in science is not, for Antony, inconsistent with the idea that we might never fully realize this ideal as researchers. She writes:

> I have no doubt that it is a fact - that no one can fully rid oneself of prejudices, neurotic impulses selfish desires, and other psychological detritus. But this does not impugn the moral or cognitive value of attempting to do so. The real test of the adequacy of a norm is not whether it can be realized, but whether we get closer to what we want if we try to realize it.[25]

Hence, the naturalized approach as it is employed by Antony, removes the warrant for an epistemic ideal of pure impartiality and objectivity and replaces it with a recognition that bias is inherent in human epistemic endeavors but the evaluation of good bias from bad bias is possible to engage in through agreed upon methodological norms of objectivity based on pragmatic considerations of utility.

In addition to these considerations we can add the thesis of underdetermination advocated by holism, to more fully explain how enquiry invokes partiality. Empirical beliefs and social and political beliefs are interdependent on this model and are justified through a coherentist theory of justification. All beliefs in our system are logically interdependent, since their justification depends upon their

coherence with the rest of the system. However, for a holist like Antony, some hierarchical ordering is methodologically useful for trimming our epistemic job down to manageable proportions. Thus, the naturalized approach to knowledge advocated by Antony sees biases as both natively present and acquired. We then need to develop methods for sorting these biases into the categories "good" and "bad" based on how they operate in overall theories. That is, if particular biases are at odds with good methodological biases in science, then they should be discounted. The process of discounting certain beliefs can be normatively regulated by publicly available data emanating from sources out in the world. As Quine describes it "The edge of the system must be squared with experience"[26] Hence, the equilibrium and self-critical practice that follows on this model means that no beliefs will in principle have priority over any others. However, we can constrain our choices by a reliance on publicly determined experiences along with our pragmatic considerations. Therefore, we can maintain that our preference for one particular bias is justified over another if it promotes a more useful, coherent body of beliefs. We recognize our predilection for certain assumptions and biases but that does not exclude us from legitimately criticizing some biases rather than others. Thus, the paradox of bias is solved for Antony.

I agree with Antony that Chomsky's contributions to linguistic and epistemic theory have been liberating for traditional empiricism. However, I disagree that even this much epistemic room provides us with the kind of justifiable constraints to engage in the evaluation of good bias from bad bias or good inferences from bad ones. This kind of evaluation is necessary if Antony is to have a way out of the paradox of bias. But a reliance on naturalistic processes to ground normative judgements as to the reliability of patterns of reasoning, will lead us to incoherent results. I will try to show these limitations of naturalism by readdressing the questions regarding the concepts of objectivity, explanation and inference.

IV. Objectivity and the Evaluation of Bias

The notion of objectivity is central to the question of how to effectively distinguish worthy beliefs from unworthy beliefs, and worthy biases from unworthy biases. Given that epistemologists want more than criteria based on wishful thinking or political force to guide criticisms of certain reasoning practices, some notion of objective belief and knowledge is necessary. Generally, in the literature of epistemology and philosophy of science, there are two notions of "objective" at play. The first is bound up with questions about the truth and referential quality of

scientific theories. This is the notion consistent with "scientific realism", the view that science is concerned with the independent existence of the objects of our experience. The second sense has to do with patterns of inquiry. This is the notion consistent with a "scientific method". On this view there are correct methods of inquiry which filter out extraneous, subjective predispositions which can interfere with the production of true knowledge. Commonly, it is argued that if science is objective in the first sense, it is a result of inquiry in the second sense.[27] This is for the reason that what we propose to be the case about the objective nature of reality depends upon testing that reality through methods that transcend wishful thinking or subjective predispositions.

Hence, objectivity has both a metaphysical and an epistemological component. Objectivity in the metaphysical sense, has to do with the objects of theories, descriptions and perspectives. Realism and realist assumptions involve a (minimal or maximal) commitment to a world which exists independently of thought, action and language. Objectivity in the epistemological sense, has to do with knowing the world through methods which are non-arbitrary and non-subjective thereby allowing us to test hypotheses and theories to determine the features of this existing reality.

The feminist and social epistemologists discussed thus

far advocate objectivity in the second sense but ground it in naturalist domains. Holists like Antony and Nelson, find objectivity to inhere in the coherent network of our cognitive and perceptually based beliefs while social constructivists, like Longino (described in Chapter Two above), locate it in the social character of inquiry. Both varieties of naturalism view "background beliefs" as operative in an individual's or a society's "web of belief" or "language-game". Further, they claim that these background beliefs play a role in directing the knower toward a bias for a particular theoretical explanation, thus setting a limit whereby knowledge is made possible. Let me make this clearer by briefly summarizing the general naturalist position regarding value-ladeness and bias in belief and theory choice.

First, both holists and social constructivists make use of the 'underdetermination thesis' whereby a theory is underdetermined by data, in order to argue that values of some kind are always involved in the formation and evaluation of hypotheses and evidence. Secondly, it follows from this that there is no way to eliminate a priori, the inclusion of values into the formation and evaluation of hypotheses and evidence. Third, there are both internal scientific values, like "simplicity, consistency, predictability" and external, contingent social values like "promotes public health, or advances our arms build-up"

which are simultaneously operative in the pursuit of scientific knowledge. Hence, science's invocation of values per se, is not evidence of "bad" science or non-objective science. Rather, it is when the values invoked are at odds with norms that are taken to be "correct" by the public that the science and hence the values it is based on, are judged "bad" and non-objective by feminists. These norms would include such principles as the inherent equality of persons and the injustice of discrimination based on gender, race, class or sexual preference. Thus, these norms are not a priori formal principles, but instead, moral and political commitments which stem from a free and equal society. The purpose of this argument then, is to show how "good" science can invoke "good" biases and objective, publicly agreed upon norms, while "bad" science invokes biases that are non-objective in that they are contrary to the relevant socio-political aims of the culture.

In this summary of the naturalized character of feminist epistemology and philosophy of science, I am understanding scientific reasoning in the broad sense of theory construction and theory-choice. This is because only when it comes down to matters of competing hypothesis and theory-choice, that the issues of value-ladeness and background assumptions have their most prominent role. As Longino describes it:

> I am not arguing that *all* scientific reasoning involves value-laden assumptions. Sometimes auxiliary

> assumptions will be supported by very mundane inductive reasoning... If, however, as I have argued, there is no a priori way to eliminate value-laden assumptions from evidential reasoning generally, then there is no way on a formal basis for arguing that an inference mediated by prior values is thereby bad science.[28]

Hence, if there is no formal basis for ruling out "bad" science, we must conclude on the naturalist account, that our basis will have to be presumably, an "objective" social, moral or political one.

Questions regarding reasoning and judgment, and the objective character of knowledge are essential to theoretical development in epistemology and philosophy of science. However, I want to argue that these questions involve matters that are not resolvable through naturalist means. The discussion thus far concerns questions about the relationship between hypotheses and evidence and how this relationship might be mediated by background assumptions or biases, some of which are justified via social norms. Settling these questions requires raising issues that are metaphysical or conceptual in nature. They depend upon both an account of the meanings for the concepts "evidence", "justification" and "inference" that is not imprecise or vague, and a theory as to what kind of things are understood to fall within the domain or extension of the various concepts. In this way the question of evidence presupposes the resolution of issues which are meta-theoretic. Because they are meta-theoretic issues, they need to be resolved

with no prior ontological assumptions. What we are after in raising these issues then, is an analysis of evidential relevance which will constitute some standard of rationality that is independent of any particular research program or scientific theory. I will try to show that if we begin our search for such an analysis with no prior ontological assumptions we will be led not to naturalism, but rather to rationalism and metaphysical realism.

V. Inference

I want to begin the first step of this search by considering the notions "inference" and "inference pattern". I begin here because it is common ground between the philosophers described above and myself. It is common ground between us for three reasons. First, we are in agreement that epistemology has as one of its goals the acquisition and promotion of true beliefs. While there may be disagreement between us as to how to understand the notion of "truth", there is consensus with regard to the goal of acquiring it. Secondly, the feminist philosophers described above are concerned with a method for evaluating good patterns of belief and inference from bad patterns of belief and inference and this too is a goal I share. Third, we see as a goal for epistemology, the ability to criticize belief systems and theories which perpetuate the

discrimination of marginalized social groups and favor the circumstances for those in a preferred social position. In line with this third goal is the idea that there is some objective criterion available to ground such criticism independent of any individual person's psychology or causal history. The feminists discussed thus far take this objectivity to be found within the community of interdependent knowers, and hence construe it naturalistically. However, in recognizing that a metaphysical commitment assumes a prior meta-theoretic assumption, I will leave open the question as to what the ontological status is for the concept of objectivity and the practice of inference evaluation. What I intend to show however, is that such notions are ultimately unavailable on the basis of naturalized models. The reason is naturalism, as it has been discussed thus far, is committed to the view that the primary subject of epistemology is a social group. My view will be that a neutral standard of evaluation cannot be constructed on the basis of group-relative ratings.

Returning to the issue of inference and inference patterns, I will take these notions to refer to logical entities, schemas or prescriptions, composed of a set of premises or grounds and a conclusion.[29] For example, consider instances of the readjustment to prior probability based on new evidence in the case of Bayesian inferences or

statistical assignments of confidence to hypotheses based on calculations involving relative likelihood.[30] I will assume that when people implement these patterns they rely upon causal psychological and physiological processes. Examples of these kind of processes in clear, mechanical terms include things like physical implementations of Bayesian or statistical procedures in the brain. Hence, we can think of an inference as having two components analogous to the "Competence/Performance" distinction in linguistics. The competence component is the logical and idealized form of the actual inference just as the competence component in linguistics is the idealized form of the grammar. The performance component is its psychophysiological implementation in which the grounds for the inference are interpreted as leading to a conclusion based on some causal process in the mind/brain of an individual just as the performance component in linguistics is the actual psychophysiological implementation of the rules of the grammar by the speaker.

There is one sense in which the distinction between competence and performance as I am using it here, harks back to the logical empiricist distinction between a context of discovery and a context of justification.[31] Proponents of the distinction supposed that a context of discovery for a given hypothesis was the result of the circumstances surrounding the hypothesis' initial formulation - for

instance, its genesis in imagination, guesses, the emotional experiences of the scientist. However, these psychological factors were seen purely in individualistic, psychologistic terms.[32] Therefore, these factors were treated as random and had no systematic relationship to the culture, social organization or political and economic interests of the scientist. Within the context of justification, these factors were disregarded and just the hypothesis itself was considered in relation to empirical, observed reality.

In appealing to a competence/performance distinction, I am not upholding such a rigorous divide between an individual's psychology and factors external to her psychology. Within Chomsky's own account, competence is understood as being a concern for psychology much like a theoretical posit in physics is a concern for scientists of nature.[33] In this vein, I will suppose that there is a level at which inference, even in its idealized form has a basis in psychology and may relate to the performance component in a complex way. So, contrary to the traditional positivist view, I am not drawing a sharp and rigid distinction between psychological factors and non-psychological factors. What is reminiscent about my use of the distinction here and the positivist distinction, is that I am separating out the rule of inference itself from the causal and sociological factors that may lead one to implement the rule; similar to the separation of the

hypothesis itself from the factors involved in formulating the hypothesis. I will say more about the distinction between a context of discovery and a context of justification in Chapter Five.

Thus, if we think of inference patterns in these terms, as having a competence component and a performance component, we can raise two questions. The first is the question of how to determine whether a conclusion is justified given a subset of grounds, which are presumed justifiable. This is a question of the heritability of warrant among a set of beliefs in the same way that validity is a question of heritability of truth among a set of propositions.

The second question concerns the production of beliefs and how adjustments to the beliefs of an individual are systematically influenced by psychological and sociological causal factors that impact on the individual. For example, how might an individual come to hold a theory in which women are considered to be intellectually inferior to men. This kind of question is different from the first question in that the first question is concerned with what the correct practice of justification and confirmation is while the second question presupposes these notions and asks how incorrect reasoning occurs.

The second question is an empirical one since it requires systematic descriptions of contingent matters of

fact whereby some beliefs and not others, come into and out of existence within a particular population of believers. As an empirical question it is not purely a logical question. On the naturalistic models considered previously, the answer to the logical question depends on an answer to the empirical question. However, on a non-naturalist model these questions are viewed as independent. I will argue that the non-naturalist claim that these two matters are independent is correct and that contrary to the view of naturalists, the logical issue does not depend on the nature of the relationship of the content of an individual's belief and facts about her causal history. Instead, I will try to show that the issue depends on the merit of the best pattern of inference of which the belief and its grounds are instances.

When one infers that some explanation is better than competing explanations, one is making an inference to the best explanation. In his article "Knowledge, Inference and Explanation"[34], Gilbert Harman points out that *all* inductive inference takes the form of inference to the best explanation. Even in cases involving probability and degree of confirmation, Harman provides convincing examples to show that inference to the best explanation is ultimately what warrants one's belief.[35] Following Harman's line, if we accept that inferring correctly is inferring to the best explanation, then we need to get clear on what the best

explanation would involve and how we might evaluate it in light of the logical and empirical questions raised above.

"Explanation" can be understood in two senses.[36] In one sense, it is the practice of making something intelligible by describing its structures and processes. In another sense it is showing how an event under description, occurred. Which sense of explanation one intends, depends upon the domain in which the explanation is given. For example in the field of medicine, when one talks of "explanation" it is understood, generally in the second sense, as a matter of causal sufficiency. If we want to explain why some person has heart trouble we look to the causal factors that led to the present condition of their heart. However, in fields like logic, mathematics or linguistics, explanation is understood in the second sense. Consider the task of linguists who are concerned with developing a science of language to explain the structures and characteristics of sentences. For example if we consider the sentences:

(1) Who does Bill know of Kate's belief that is a jerk?
(2) Who does Bill know that Kate believes is a jerk?

The linguist might be concerned to explain why (1) is an ungrammatical sentence of English while (2) clearly seems to be a grammatical one. According to present linguistic theory[37], (1) is explained in terms of a principle called "subjacency". Rather than being a causal explanation, the explanation in this case is based on positing properties

that these sentences have and imagining a law that works over these properties.[38] Given this law, we can explain how other sentences that share the form of (1) are ungrammatical while those that share the form of (2) are grammatical. Assuming that this way of characterizing what is going on with these sentences meets methodological considerations like simplicity, being non-question begging and extensionally correct, the "subjacency principle" is the best explanation. In this case, the best explanation is not a causal explanation.

Assuming these senses of "explanation" we can return to the logical question to determine what a reliable inference to the best explanation might entail. My argument will be that if we consider this question alone, we will find that no reference to the empirical question need arise and moreover, no questions concerning facts about the epistemic agent or community need arise either. An inference to the best explanation will posit the existence of reliable patterns of inference which operate in a lawlike way independent of who or what implements these patterns.

To begin, I will stipulate that epistemic evaluation is based on independent inference patterns. Now suppose an epistemic community C makes a set of observations O and concludes B. Since, as I have stipulated, evaluation connects to inferences patterns, we can settle whether C's inference to B is warranted by considering all possible

schemas of inference passing from O to B and ask whether any of these are reliable and truth productive. C's inference receives epistemic approval in proportion to the approval given to the most reliable patterns of inference of which C's inference is an instance. Thus the logical question is concerned only with the epistemic value attributable to C in a derivative sense. The primary subjects of approval are the patterns of inference from O to B. As a result, it does not make sense to ask questions about the origin and generation of these patterns of inference as beliefs for C. In this way the inductive question is parallel to the deductive question. If I believe that *p* and also if *p* then *q*, then I am deductively justified in believing *q*, not because my psychological processes of generating *q* are somehow better than other processes I could have used, but because modus ponens is a valid form of inference. In fact, If I believed that if *p* and also if *p* then *q* and then generated *q* purely out of a hope for *q* to be true, I would still have a reason for *q* even if I did not see that I did. My failure to recognize that I have reasons does not diminish the validity of my inference. Hence, the epistemic value of an inductive inference does not depend on the process used to generate that conclusion and the two issues are therefore independent.

Prima facie, holists and social constructivists might view this argument as too easy. Most likely they would

take issue with my initial stipulation, namely that epistemic evaluation is connected to inference patterns. Yet, as I presumed above, holists and social constructivists are in agreement with me that one central goal of epistemology is to develop explanations of rational evaluation. If we suppose the epistemic agent or community is the primary subject of epistemic evaluation we will be led into some pretty unusual inconsistencies.

For example, consider the situation in a deductive case. Lets suppose that community S believes that Wittgenstein was born in Vienna. S also believes that if Wittgenstein was born in Vienna then he was born in Austria and finally S believes that Wittgenstein was born in Austria. Now there are a diverse set of instances of the familiar processes of reasoning by which S could arrive at their beliefs and thus underwrite this substitution instance of modus ponens. S may employ a number of processes by which to arrive at the belief that Wittgenstein was born in Austria. They could on the one hand, collectively be implementing a mechanical proof algorithm that conforms to modus ponens or they may imagine that Wittgenstein was born in Austria, find it a communally appealing thought and opt to believe it independently of the initial two premises. If S adopted the latter process, then I would presume that in evaluating the situation, we would judge S and derivatively modus ponens to score low on our epistemic rating system.

On the other hand, if S implemented the first process, they and derivatively modus ponens would receive a high epistemic rating. The point here is that as soon as we require some consistent criterion for rating inference patterns, we have made a requirement for the inference patterns to be separated from the subjects implementing these patterns. This is for the reason that we can not use evaluations based on communities or psychological and sociological processes to recover a consistent set of evaluations for inference. By abandoning the logical framework out of which to evaluate inference patterns, we will be left without resources to evaluate consistently.

In addition to consistency, there are other reasons to assume that epistemic evaluation of inference is determined independently of the subject or community that implements the inference. In their article "Evidentialism"[39], Richard Feldman and Earl Conee defend an evidentialist version of epistemic evaluation against views which rely upon naturalist considerations such as the voluntary nature of a belief for evaluation, the cognitive limits of the believer, or the reliability of the psychological processes which cause the belief. I will mention their argument against the voluntary view here, a bit further along in the discussion I will bring up their argument again, against the cognitive limitations view and the reliabilist view.

Feldman and Conee explain that an inference need not be

constrained by any voluntary control for that belief to be suitable for epistemic evaluation.[40] They give as an example, two cases, one of justified belief and the other of unjustified belief. In both cases the beliefs were involuntary on the part of the believer. In the first case, a person enters a lit room and spontaneously believes the lights are on as a result of seeing the lights actually on. The person in this case, is justified in their belief regardless of the fact that they cannot voluntarily adjust their belief, modify it, or change the cognitive factors that led to the belief. In the case of an unjustified belief, the authors give an example of a paranoid person who involuntarily believes that he is being spied on as a result of an unconscious psychological need for attention. Assuming there is no spying going on, this person is unjustified in his belief, regardless of the fact that it is involuntary. It is unjustified because there is no evidential relationship between his belief and the facts.

Feldman and Conee point out that an analysis of epistemic evaluation in terms of a person believing voluntarily is confusing an assessment of the belief with an assessment of the believer.[41] We attribute epistemic merit to a person if they voluntarily believe some proposition *q* as a result of seeing that *q* follows from some evidence *e*. If they voluntarily refuse to see *q*'s relationship to *e* we do not attribute epistemic merit to them because they are

failing to see the merit of the inference pattern from *e* to *q*. In circumstances where a person's beliefs are involuntary, we judge them, not the relationship between evidence and belief, to be beyond the bounds of epistemic responsibility. Hence, the voluntary nature of a belief is relevant, but only in evaluating a person's epistemic record. It is not relevant in evaluating the relationship between evidence and that belief.

If we bring this case back to the general discussion of the evaluation of inference in light of a community's practices, the same point about voluntarism can be made. A community can have both justified and unjustified beliefs, and make justified or unjustified inferences, regardless of the factors that led to their having those beliefs. For example, if The Food Co-op Committee involuntarily believes that a delivery of hazelnuts just came in upon watching the delivery of the new shipment, that belief is justified regardless of the fact that it was an involuntary belief on their part. Similarly, if the Food Co-op Committee is prone to collective paranoia and believes that their organic apples have been poisoned by Newt Gingrich, their belief is unjustified (assuming that the apples have not been poisoned) regardless of the fact that they arrived at their belief in an involuntary manner. These cases show that to evaluate the epistemic worth of a belief or inference, it is necessary to move beyond matters of the believers collective

psychological circumstances and on to facts about the belief pattern's epistemic merit.

I recognize that many philosophers have found the naturalistic program to be appealing because thus far, realist and rationalist epistemologies have not answered skeptical challenges such as "evil demons" who could be manipulating our reasoning processes. However, by accepting naturalism, we exchange one skeptical problem for another. Empiricists must face the problem of Goodman's "New Riddle of Induction"[42], which asks how might we distinguish between lawlike inductive hypotheses such as "Emeralds are green" from accidental generalizations like "Emeralds are grue". "Grue" is a predicate that applies to all things which are green before time *t*, (where *t* is some time in the future), and blue after *t*. Given this definition of grue, the hypothesis that all emeralds are grue is just as well confirmed as the hypothesis that all emeralds are green. Katz, in a recent article[43] points out this problem arguing that "from the perspective of philosophical skepticism, empiricists and rationalists are in the same boat. If for no other reason than that empiricists, too, rely on logical principles to infer general empirical laws and theories."[44] Given that principles are employed from both a rationalist and empiricist standpoint, the philosophical skeptic poses a challenge across the board. However, following the line proposed by Katz, we can distinguish the broad,

philosophical problem of skepticism from the question of accordance with prevailing standards within an epistemic domain. Given that we are concerned here with the evaluation of patterns of inference; a realist distinction between a logical and empirical component in relation to inference, does not incur a unique problem of philosophical skepticism and is in line with standards for explaining evaluation. The naturalist position on the other hand, does not help to explain the questions of evaluation in light of reliable, truth productive procedures. All that naturalism yields is perhaps the ability to rate agents or communities via naturalistic models of causality and coherence. But this does not get us closer to answering the justification question or the question of what relationship exists between the agent and the correct pattern of inference. What is still needed is an account by which we can refer to objective principles of reasoning independent of persons and communities. Realism provides such an account. Naturalism can provide only the additional resources for rating subjects which may allow us to discover incorrect applications of these principles, but it will not give us justificatory criteria.

Feminist epistemologists, like Antony, who advocate holism, recognize the need for such objective criteria but resist seeing how the best explanation of this criteria leads us away from the material aspects of communal

experience. As a result of this resistance, Antony works hard to ground objectivity and epistemic evaluation in a "hierarchy of confirmation" within a web of belief. However, the status of this hierarchy still rests upon our common language and experience of the natural world. What is needed to explain instances of "good" reasoning and evidence from "bad", is a separation of the normative, logical question from the empirical and sociological one.

Consider the problem from the perspective of social constructivism. In *Science as Social Knowledge*, Helen Longino develops some sophisticated arguments for fixing the objective nature of science in the social character of inquiry. However, Longino's arguments depend upon a notion of social agreement based upon the fact that "...we have a common language which we use to describe our experience and within which we reason and that the objects of experience which we describe and about which we reason are purported to exist independently of our seeing and thinking about them."[45] For Longino, these facts guarantee a "logical publicity"[46] to reason and science. But the assumption in Longino's argument is that the "objects of experience" are solely the referents of natural objects including causal history, sociology and psychology. However, when we consider the consistency and necessity required to explain the justification of deductive logic or mathematics or even

language, an explanation in terms of human construction does not get us very far.

If we are open to the possibility that an inference to the best explanation regarding meaningfulness, validity and warrantability can correspond to an objective reality independent of the minds and languages of communities, then we can explain their publicity, objectivity and necessity in a simpler, more consistent way. Among social epistemologists, feminist epistemologists in particular should be concerned with a strong program for countenancing an independent, objective realm of justification. This is because feminists need a basis for dispensing criticism on grounds neutral to a subject's gender or socioeconomic origin. Without this capacity, feminists cannot escape the paradox of bias.

The following example will serve to illustrate this point. Consider a community that believes most women, especially the "feminist" ones are self-serving, manipulative, erratic and less rational than most men. This community is made up of middle-class, suburban married men and woman living in North America. They are political conservatives. Aside from a few women they have read about or seen in the movies, they find most females, including the ones within the community, to be petty and selfish. The women in this community feel they have worked hard to overcome these tendencies in themselves but they are

nevertheless reasonably content to accept their secondary status. Since women in this community believe that most women are untrustworthy, they are comfortable with men occupying the serious leadership roles in religion, politics, economics and education.

Clearly this community's beliefs are antithetical to the arguments of most feminists. Now on what basis can feminists demonstrate to members of this community that their conclusions employ the wrong kind of bias? If we assume a naturalized framework for epistemology, feminists would have to direct their critique at the community itself, its causal history and the origin and network of their judgments. Yet this community contains both men and women who have grown up and been educated within a societal framework that has helped to secure them in their judgments. The people in this community have had material evidence that contributed to their present system of beliefs. Their "web of belief" and their "language game" predominantly warrants their views. For individuals within this community to think otherwise they would have had to counter their causal history or have developed some antidote to it. While we might understand how it is that individuals within this community came to have the beliefs they presently have on the naturalist model, reasons independent of causes will still need to be advanced so as to make the judgement that those beliefs were unfounded and unfair. Philosophers like

Antony recognize that more needs to be said to objectively distinguish the above community's bad bias from feminists' good bias. However, the use of naturalism does not adequately meet this need.

An insight into the difference between norms and strong psychological tendencies, s offered in Jonathan Adler's article "Reasonableness, Bias and the Untapped Power of Procedure"[47]. Adler points out that reasonableness is an ideal that while widely avowed, is nevertheless not widespread. One of Adler's explanations for the lack of objective reasonableness is the fact that many persons are prone to be convinced by factors like their own desire to understand rather than more rigorous procedural constraints. He gives as an example, the process of interviewing a candidate for a faculty position. Generally, in this situation, it is a commonplace for interviewers to rely heavily on the personal interview for confirming evidence of the candidate's worth. However this procedure is not the most reliable indicator of the candidate's potential as a teacher and scholar. More "objective" criteria exist for measuring these qualifications, but they are impersonal and do not give the interviewers the sense of being in control or decisive in the process. These psychological considerations are standardly accepted because, Adler argues, a desire to "understand" the situation is common in people and so the procedure goes unchallenged. He writes:

"Normally, for good epistemic agents, those cognitive interests - truth, fit with our beliefs, confidence in one's judgement based on those beliefs - go together. But they can pull apart...so that a sense of understanding is gained at the expense of the objective side of understanding."[48] In such cases, the material, causal, sociological factors that lead to a "feeling of understanding" while easy to rely on and socially reinforced, are still possible to criticize from the standpoint of objective standards of correct evaluation independent of psychological tendencies.

If we go back to the case of the sexist community above, considering the factors that have contributed to their beliefs we could evaluate it as a "weak" case of epistemic warrant. The case is weak in the sense that members of this community did not look beyond their own experience and employ more rigorous evidential standards. The cognitive, historical, sociological explanation for their weak reasoning may help us to understand their perspective, but it does not address the logical question raised above with regard to the practice of "good" reasoning and the correct evaluation of inference patterns. This question rather than being supported by naturalism, is actually given no assistance. What is needed is an epistemology in the service of the logical evaluation of inference patterns and the correct prediction of logical

properties and relations. Only an epistemology of this type can ground the feminist critique.

Thus far, my argument rests on the assumption that it is possible to evaluate patterns of reasoning independent of the psychological and causal factors affecting subjects who implement these patterns. However, In his introduction to *Naturalizing Epistemology* and his article in the same volume, Hilary Kornblith argues against this assumption, advocating a naturalized method of evaluation.[49] Kornblith characterizes the view he is criticizing as the "arguments-on-paper thesis", an account whereby "being justified in believing a proposition is not psychological...questions about justification amount to nothing more than questions about the quality of various sorts of arguments."[50] Kornblith offers an argument as to why the arguments-on-paper thesis is false. He asks us to consider Alfred who justifiably believes that *p*, justifiably believes that if *p* then *q*, and believes that *q* as a result of wishful thinking. According to the arguments-on-paper thesis Alfred is in fact justified in believing that *q* since there is a good argument that can be given for *q* on the basis of propositions Alfred is already justified in believing. However, for Kornblith, Alfred is not justified in believing that *q* because Alfred's conclusion is not based on the valid grounds given by the argument. According to Kornblith, for Alfred to be justified in believing that *q*, that belief must depend *in a*

causal way on his respective affirmations of *p* and *if p then q*. Thus, questions of justification of beliefs must be intimately tied to questions about the sorts of processes responsible for the presence of those beliefs. Therefore, if Kornblith's argument is right, there is a significant relationship between psychological processes and justification and the arguments-on-paper thesis is wrong.

Above I have tried to argue that if we expect to *consistently* evaluate inference patterns we are required to separate out the psychological processes of the subject from the form of the inference. Further, if we consider the processes involved in Kornblith's example, what we find out about Alfred is what mechanisms are involved in forming beliefs in his psychology. But in evaluating inference patterns we are seeking a ranking according to reliability. Reliability of inferences is not a matter of mechanism or process. Like validity, reliability is a semantic relation between content and ontological truth. Evaluation of a psychological process, on the other hand, is a different matter. A description of a causal process relating two beliefs can be given on purely syntactic grounds. What is needed in addition to the syntactic component of the inference is the dimension of truth, and that is supplied by the semantic component of the logical analysis. These two components need not be construed as inconsistent, just as syntax and semantics are not inconsistent but instead two

descriptions of the same phenomena. The logical question and the empirical question are not incompatible, but to answer one, is not to answer the other.

Kornblith's argument is relevant in that it reminds us how descriptions of the logical evaluation of inference patterns do not capture the facts of psychological processes that are involved when actual human agents either recognize or fail to recognize that they have reasons to believe. As a result, what subjects ought to do epistemologically is perhaps not always what they can do. Naturalized epistemic accounts like Kornblith's, capture what agents can do and therefore uphold a principle of "ought implies can". But naturalized epistemic accounts, particularly when they are adopted by feminist epistemologists should not serve as an excuse for human inadequacies. In the context of feminist critique, epistemology has to serve as a corrective, setting out imperatives that will show why inference pattern *A* is more reliable than *B* and why *B* is more reliable than *C*. If *A* is more reliable and I am employing *B*, then I am well advised to correct my pattern of inferring and adopt the one that is more reliable. Epistemology requires a place for the ranking of truth productive patterns, and naturalized accounts while not in principle incompatible with this task, cannot replace it.

Further, if we return once again to Feldman and Conee's paper mentioned above, a person or community's ability to

implement a reliable pattern of inference does not address the question of whether or not that inference is justified. Kornblith has argued that epistemic justification is "simply doing the best one can in light of the innate endowment one starts from...".[51] Feldman and Conee argue that even on occasions where the inference that would best fit the evidence was beyond human cognitive capacity, the inference would still be *justified* by the evidence. If a person with normal human cognitive abilities could not infer what was shown by the evidence, then that person would be in the adverse position of being unable to infer what was justifiable. This limitation in human ability is not a limitation for an account of justification. The point stands even if we shift the limitations to communal limitations rather than individual, cognitive ones.

Given the arguments above, I want to return to the paradox of bias. Recall that the paradox involved the claim that impartiality served as a pretense in scientific inquiry and was therefore illegitimate. This claim was joined with a second claim which asserted that partiality in scientific inquiry would lead one to be biased in their investigations and was therefore illegitimate. Both impartiality and partiality are thus deemed illegitimate and the two claims pose a dilemma.

Antony's solution to the paradox involved relying on a naturalized framework and a confirmational hierarchy within

our web of belief. We are partial in our investigations since we have a cognitive predisposition to certain ways of describing experience. At the same time, we can impose investigative norms of impartiality which are justified by the utility and coherence they promote for our overall framework of belief. All of these norms are in principle revisable and commit us only to a naturalized metaphysics. However, I argued that her solution conflates the logical question with the empirical question. I have tried to show that to evaluate "good bias" from "bad bias" we will need to focus on the particular patterns of inference independently of the subjects who implement them, even if no subject implements them perfectly. In so doing, we are left with the job of evaluating schemas not social or psychophysiological processes. Proceeding via inference to the best explanation, we will need to posit properties and structural relations to these inference patterns that entail objects of experience independent of human psychology. Such objects will serve as norms of reasoning which can be utilized in judgments of "good" and "bad" instances of bias. Thus, we can grant that human beings are always partial in their judgements of evidence, warrants and grounds. However, we can also uphold the view that some predispositions or biases are better than others in that they more closely approximate ideals of truth productivity. Therefore, the more that feminists are able to show how

scientific methods and processes have been compromised because they failed to live up to their own ideals of reason, the more possible it is to show how bad science is *false* science. The result is a way out of the paradox of bias that is not dependent upon accepting the attitudes and biases that favor one set of communal interests over another. The way out transcends motivation and interest in that whatever my psychological processes, conscious or not, voluntary or not, some patterns of inference are nonetheless reliable. The question of reliability can be answered only by an investigation into the relationship of the content of my premises and conclusions and facts about the properties of the objects of experience. In the chapter that follows, I will argue that this separation between the logical question and the empirical question motivates a reconsideration of the distinction between a context of discovery and a context of justification.

NOTES

1. Rosser, S. 1982. "Genetic Androgyny and Sociobiology". In *The International Journal of Women in Science.*

2. See Giere, Ronald. 1984.

3. Rosser, S. 1986.

4. See Longino. 1990. pp. 128-132.

5. See Nelson & Nelson. 1994. "No Rush to Judgement". In *The Monist,* Volume 77. No.4. pp. 486-508. for an endorsement of a case very much like this one.

6. Blier, Ruth. 1984.

7. Antony, Louise. 1993. pp. 185-226.

8. Ibid., p.187.

9. Ibid., p.195.

10. See for example, *The Essential Descartes* ed. by Margaret Wilson. New York, NY. Mentor Press. 1969.

11. Hume, David. 1977. 30.

12. Ibid., pp. 296-297.

13. Kant, Immanuel. 1956. p.54.

14. For example see Jagger, A. 1983. *Feminist Politics and Human Nature.* Rowman & Allenheld. Totowa, NJ. And Flax, J. "Postmodernism and Gender Relations in Feminist Theory". In *Signs* 12, 4, Summer 1987: 624.

15. Kant. 1956. pp.55-56.

16. Antony. 1993. p.199.

17. See for example Hume's discussion in *Treatise of Human Nature* and *Inquiry into Human Understanding* in the Sections entitled "Of Skepticism with Regard to the Senses". Hume argues that induction is not a truth of reason since its denial is not a contradiction. But neither can it be justified by experience since any attempt to do so would be circular as the practice of using past experience as evidence is only warranted if one accepts the principle of

induction.

18. Antony. 1993. p.201.

19. Ibid., pp.201-203.

20. Chomsky, Noam. 1959. "Review of Skinner's *Verbal Behavior*". In *Language* 35,1, pp. 53-68.

21. Antony. 1993. p.203.

22. Ibid., p.211.

23. For example consider Fodor's version of nativism in Fodor, J. 1980. "Fixation of Belief and Concept Acquisition" in *Language and Learning: The Debate between Jean Piaget and Noam Chomsky*. Harvard University Press. Cambridge, Mass. pp.143-9. Compare this view with Katz, J.J. 1995 above, particularly footnote 5, pp.501-502.

24. Ibid., p.208.

25. Antony. 1993. p.209.

26. Quine, W.V.O. 1980. p. 45.

27. See Longino. 1990. p.63.

28. Longino. 1989. p.207. Emphasis is mine.

29. For a defense of a similar view of logical inference see Mariam Thalos, 1994, *The Monist*, October, Volume 74.

30. For example, consider a case in which someone knows that a die has six sides and then infers that there is a one in six chance of number "five" on a roll - given the relative frequency.

31. This distinction, originally introduced by Reichenbach. 1938. in *Explanation and Prediction*, will be discussed at length in Chapter Five.

32. See Longino. 1990. pp.63-65, for a feminist analysis of the distinction.

33. See Chomsky. 1965. p.4.

34. Harman, Gilbert. 1968. "Knowledge, Inference, and Explanation." *American Philosophical Quarterly*, 5.3. pp. 164-173.

35. Ibid., p.169.

36. Discussions with Virginia Held and Russell Dale helped in formulating this section.

37. See Lasnik H. and J. Uriagereka. 1988. *A Course in GB Syntax.* Cambridge, Mass. MIT Press.

38. Sentence (1) has the property of having two "bounding nodes" that are not separated by a COMP position. For a detailed discussion of the "subjacency principle" see Lasnik and Uriagereka above - pp.20-22.

39. Feldman, R. & Conee, E. 1985. "Evidentialism". *Philosophical Studies*, pp.15-34.

40. Ibid., p.17.

41. Ibid., p.17.

42. Goodman, N. 1954. Chapter III. Section 4.

43. Katz, J.J. 1994. "What Mathematical Knowledge Could Be". *Mind*, Volume 104. 415. July 1995. pp.491-522.

44. Ibid., p.515.

45. Longino. 1990. p.70.

46. Ibid.

47. Adler, Jonathan. 1993. "Reasonableness, Bias, and the Untapped Power of Procedure". *Synthese* 94: 105-125.

48. Ibid., p.121.

49. Kornblith, Hilary. 1985.

50. Ibid., p.117.

51. Kornblith, H. 1983. "Justified Belief and Epistemically Responsible Action." *The Philosophical Review* 92. pp.33-46. The quotation is from p.46.

CHAPTER FIVE

A Reformulation of the Distinction Between the Context of Discovery and the Context of Justification

I. Introduction

The arguments of the preceding chapters have sought to first, establish the possibility of a feminist contribution to the theory of knowledge. Second, to show the limits of a Quinean based model of such a contribution. Third, to show the limits of a Wittgensteinian inspired model of the socially constructed nature of knowledge; and lastly, to argue against a naturalized epistemic model of objectivity and inference evaluation. Hence, the requirements on an account of feminist epistemology that follow from these arguments will involve a consistent evaluation of meaning and inference independent of the beliefs and practices of communities and individuals.

I have suggested a replacement of the naturalist theories of meaning offered by Quine and Wittgenstein and the adoption of a realist meaning theory along the lines proposed by Katz. Further, I have tried to show that in addition to questions about meaning, there are also reasons to adopt a realist view in epistemology, particularly with regard to the evaluation of deductive and inductive patterns of reasoning. My aim therefore, has been to show why a realist account will do the work of explaining the data of

interest to feminist epistemologists in a more satisfactory way than the naturalist accounts currently relied upon.

The final step in my argument will involve outlining a reformulated distinction between a context of discovery and context of justification. This distinction is reformulated in the sense that it involves a difference in the distinction as it was originally introduced by Reichenbach, and then expanded upon by Hempel, and Popper[1]. The view will be that a revised version of this distinction, rather than being antithetical to the purposes of feminist epistemology, will actually be in its service. In laying out my version, I will consider the various reasons why feminists have been resistant to adopting any model that harks back to a division between the contexts of "discovery" and "justification". I will show why ultimately, these concerns are unfounded.

II. The "Old" Distinction

In its most general form, the context of discovery and the context of justification proposed by Reichenbach and embraced by later empiricists like Hempel and Popper; viewed human psychology and sociology as an empirical domain distinct from scientific justification, which was viewed as a normative domain. "Discovery" concerned questions about how hypotheses and theories are generated and

"justification" concerned the testing of those hypotheses and theories in relation to observable consequences. In the first section of his book *Experience and Prediction*[2], Reichenbach emphasizes the difference between the descriptive task of sociology, which aims at explaining the psychological and historical factors involved in the formation of hypotheses, with the task of epistemology of science, which is concerned with the internal structure and content of scientific knowledge. He writes:

> What epistemology intends is to construct thinking processes in a way in which they ought to occur if they are to be arranged in a consistent system; or to construct justifiable sets of operations which can be calculcated between the starting-point and the issue of thought-processes, replacing the real intermediate links. Epistemology thus considers a logical substitute rather than a real process.[3]

Thus, according to Reichenbach, epistemology is concerned only with the "rational reconstruction" of the processes whereby a scientist goes from a hypothesis to the successful confirmation of that hypothesis employing experimental methods. This rational reconstruction requires filtering out matters of a purely empirical and generative nature, and instead orders and analyzes ideal, normative logical procedures. To mark this distinction Reichenbach introduced the phrases "context of discovery" and "context of justification".[4]

Following Reichenbach, the logical positivists were committed to an account of science in which direct evidence

provided by the senses through "observation" was taken to be the foundation of hypotheses, laws, and theories of science. Induction was an account of the connection between theories and factual evidence. Given that theories were understood to be sets of sentences, positivists set out to show how these could be simple constructions from sense data. Explaining the connection between theories and sense data required, for the positivists, that science be rid of anything that could not be traced to direct sensory evidence and the use of induction. Thus, they were motivated to draw distinctions between science and metaphysics, and between science and values. Since science could be understood as distinct from both metaphysics and values the general distinction advocated by positivists was a sense/nonsense distinction.

The "verification theory of meaning" developed by positivism provided the criterion necessary for distinguishing between sense and nonsense. The meaning of a sentence according to this view, was its method of verification. So, if the empirical conditions that would verify a sentence could not be specified, the sentence was "meaningless". An ethical or metaphysical proposition could therefore be banished from science. However, mathematical and logical propositions were saved by drawing a distinction between statements whose truth or falsity was a matter of the meaning of their terms, and hence analytic; from matters

of fact which were understood to be synthetic. As analytic statements, logical and mathematical statements were not empirical, but they could still be meaningful.

Thus the logical positivist program required several elements. First, that constructions from sense data to hypotheses and theories could be shown. Second, that the empirical conditions which would verify an individual sentence could be specified. And finally, a requirement to uphold the distinctions between science/metaphysics and science/values.

Influenced by the early logical positivists, Hempel replaced the problematic verification theory of meaning with its strict sense datum language, and instead employed a "criterion of cognitive significance"[5]. This criterion allowed only those sentences that can in principle be tested by empirical evidence, along with mathematical and logical sentences, into the body of scientific knowledge. Like the verification theory, the criterion preserved a distinction between sense and nonsense thus allowing for the exclusion of metaphysics and values from science.

The problem that Ayer and Carnap had with developing an inductivist model to explain the generation of hypotheses[6] led Hempel to locate the connection between sensory evidence and science in a context of justification. Discovery, on the other hand, was understood to be made up out of random, non-patterned elements which while they promoted novel,

creative guesses, were in fact very subjective. Given that these elements were so highly subjective, no general account could be given since each individual scientist would have a different set of factors influencing his or her hypothesis formation. Moreover, even an exhaustive study of scientists' lives could not determine a "method" which was relevant to the formation of their hypotheses. We would have no way of determining for example, whether a marine biologist who was frustrated by the condition of the lake at summer camp was later influenced by this fact in his hypotheses and theories of lake water; or if it was the fact that as a child, he listened intently to the stories of a magical, crystal clear lake told to him by a much loved grandparent. Such factors, while interesting to a psychologist or biographer, were inessential to a philosopher concerned with the explanation of scientifc justification. Hence, for Hempel, the "objectivity" of science needed to be grounded in the manner in which theories are tested, and the context of discovery needed to be shown to be irrelevant to the *content* of science - and therefore of no epistemological consequence.

Within the context of justification the generative questions of discovery would be disregarded, and the hypothesis put foward by the scientist would be considered only in relation to its empirical consequences, which would then determine its acceptability. Hempel, in outlining this

procedure puts forth three related views. One, that science is primarily concerned with explanation. Two, that specification of explanation - what will count as an explanation and what explanation is - involves a specification of the logical relationships between sentences. And three, that the connection between laws, theories, or generalizations of science and "the world" is through sentences that have specifiable and testable "empirical content". Hempel writes:

> Let us now abstract some general characteristics of scientific explanation. We divide an explanation into two major constituents, the *explanandum* and the *explanans*. By the explanandum, we understand the sentence describing the phenomenon to be explained; by the explanans, the class of those sentences which are adduced to account for the phenomenon.
>
> The explanandum must be a logical consequence of (must be logically deducible from) the explanans...
>
> The explanans must contain general laws, and these must be required for the derivation of the explanandum..
>
> *The explanans must have empirical content; i.e. it must be capable, at least in principle, of test by by experiment or observation...*
>
> Those sentences constituting the explanans must be true.[7]

So, for those in the tradition of later positivism, understanding science involved the exploration and formalization of the logical relationships between sentences, which are distinct to science and constitute the body of its theories. In addition, understanding science involved specifying the logical relationship of those sentences to factual evidence - specifying what Hempel

called the "empirical basis" of science.

The emphasis on this view, was on the logical character of science and the development of a canonical form of science. Ultimately it is in logical relationships that those in the tradition of later positivism attempt to establish the connection between science and sensory evidence. Breaking with the earlier positivist tradition, Hempel maintains that "scientific knowledge" is not in fact, "the product of induction"[8]. Rather, it is the product of the empirical testing of hypotheses. "Scientific knowledge is not arrived at by applying such an inductive inference procedure to antecedently collected data...[but] by inventing hypotheses as tentative answers to a problem under a study, and then subjecting these to empirical test."[9]

Hence, the central task in providing an account of how scientific knowledge is developed - that is, specifying the logic of justification - involves providing a "theory of confirmation". Hempel argues that confirming hypotheses by observation insures that those hypotheses admitted into the body of scientific knowledge are connected to evidence.

I will consider briefly some of the general features of Hempel's account of the relation of confirmation to evidence and compare it with the account developed by Popper.[10]

In his account of the relation of confirmation, Hempel characterizes observation sentences as sentences that "describe evidence". The problem of relating "observation"

to the "sentences of science" is overcome for Hempel by construing the relationship as a relation between sentences. Hempel explains this move:

> (As construed by Nicod) confirmation was conceived of as a semantical relation obtaining between certain extra-linguistic objects on one hand and certain sentences on the other. It is possible, however, to construe confirmation in an alternative fashion as a relation between two sentences, one describing the evidence, the other expressing the hypothesis.[11]

As Hempel conceives it, observation sentences are like the public version of individual sensory experience. However, rather than being private and subjective they are public and intersubjectively verifiable. Yet one of the things required for the sentences and the logic of confirmation is an "ideal" language of science. Ideal, in the sense that the sentences describe only those possible outcomes of accepted observational techniques in a "clearly delimited observational vocabulary".[12] The focus on "agreed upon" techniques and observations reflects Hempel's concern with distinguishing observation sentences from subjective experience. The detail given to the account of observation sentences in Hempel's writing attests to the fact that he recognizes the increasing body of literature challenging the distinction between "observation sentences" and "theoretical sentences", by philosophers like Hanson, Kuhn and Feyerabend.[13]

Like those in the later positivist tradition, Popper (who apparently did not characterize himself as a

positivist[14]) argues that how theories are generated is irrelevant to the epistemology of science. But there is a basic difference in his account of justification and Hempel's. Popper emphasizes the "falsification" of a generalization as a test for a theory, rather than confirmation. On this view, a generalization can never be proven by a single observation (or any finite number of observations), but it can be "falsified" by a single observation.[15] Popper uses the criterion of falsifiability for demarcating "scientific" theories from metaphysics and subsequently, "pseudo-scientific" theories because like Hempel, he is seeking to clarify the relation of science to factual evidence.

Though there is a difference in their focus, Popper shares with Hempel the motivation to provide a "logical analysis of scientific knowledge" by providing an account of the relation of science to evidence. But both Hempel and Popper's accounts of the logic of knowledge suffer from the problem of explicating "observation" in such a way that it is clearly demarcated from theory.

III. Beyond Logical Empiricism: The Historical Analysis of Science

The difficulties that arose in the later positivist tradition were emphasized in the more historical analyses of science advanced by philosophers such as Hanson, Kuhn and

Feyerabend.[16] What these philosophers shared was a rejection of the logical empiricist approach in the work of philosophers like Hempel and Popper. Instead, their interest was in the fact that the history of science repeatedly showed that apparently inconsistent theories seemed to nevertheless support the data they were designed to explain. So for example, the Ptolemaic and the Copernican theories which held rival cosmolgical accounts and the medieval impetus theory and Newtonian physics, which held rival theories of motion, were attempting to explain and were evidentially supported by roughly the same data. What this shows is that a model of scientific knowledge as an accumulation of increasing truths, based on a more accurate understanding of the evidence, was inadequate to describe all scientific change.

Philosphers like Hanson, Kuhn and Feyerabend, rejected the later positivist's assumption of the independent nature of observation sentences from theory. This rejection was based on the fact that science did not show a linear progression from experience to a theory that accounts for the experience but rather the periodic acceptance and then abandonment of "paradigms"[17] or large-scale conceptual systems in which scientific work is done. These paradigms were often in conflict with each other as in the case of the Ptolemaic system and the Copernican system, yet they could be applied to basically the same observational phenomena.

This was explained through the notions of "theory-ladeness" and "incommensurability".

Theory-ladeness is a concept understood in relation to both meaning and observation. Meaning is theory-laden in the sense that the meaning of a term is determined by the theory within which the term occurs. One consequence of this view is that a word or term used in different theories has variant meanings. So for example, the term "mass" has one meaning as understood on a Newtonian model of the physical world and another on an Einsteinian model. This view of the sense of terms being determined within a theory is the doctrine of "meaning holism".

With regard to observation, it is theory-laden in the sense that we see the world and experience it in such a way according to the theoretical categories we are committed to. All observation and confirmation is done within an already held theory since the theory gives these factors their content. This result, that the elements of a theory and its supporting data can only be understood, confirmed, justified and evaluated within the context of the whole, gave rise to the doctrine of "confirmation holism"

Though there are differences in the accounts proposed by philosophers like Hanson, Kuhn and Feyerabend, they all recognize that value-ladeness results in incommensurabilty. Kuhn writes:

> The interpretation [of science], closely associated with early logical positivism

> and not categorically rejected by its successors, would restrict the range and meaning of an accepted theory so that it could not possibly conflict with any later theory that made predictions about some of the same natural phenomena. The best-known and strongest case for this restricted conception of a scientific theory emerges in discussions of Einsteinian dynamics and older dynamical equations that descend from Newton's *Principia*. From the viewpoint of this essay these two theories are fundamentally incompatible.[18]

Feyerabend explains:

> Knowledge so conceived is not a series of self-consistent theories that converges towards an ideal view; it is not a gradual approach to the truth. It is rather an ever increasing ocean of mutually incompatible, and perhaps even incommensurable alternatives.[19]

Incommensurability can be understood as the result of the theory-ladeness of meaning and observation. Since there is no way to translate the language of one theory into another competing theory and because there are no neutral or independent set of data that can settle questions between competing theories, the theories are therefore incommensurable. Assuming this to be the case, a theory is then accepted or rejected not because of rational deliberation concerning the evidential support for the theory but because of psychological, sociological and political factors that impact on a community of people. This conclusion however is controversial, in part because Kuhn himself often appears to say different things regarding the factors that lead to scientific change.[20] For now, I will assume only

the more modest historicist claim whereby theory-choice is importantly affected by social and political factors. Relying upon Israel Scheffler's account in *Science and Subjectivity*[21], I will return to the question of how to appropriately understand Kuhn's view.

Consequently, the work of the historicist philosophers who have challenged the positivist account of science, has resulted in the project of taking seriously aspects of the context of discovery. Specifically, the project of understanding the kinds of social arrangements and practices that characterize scientific communities. The establishment of a historical turn in the tradition of philosophy of science marks the time at which feminist philosophers of science and epistemology such as Nelson and Longino, enter the dialogue.

IV. Feminist Analyses of Theory-Choice

i. The Holist Account

As was discussed in Chapter Two, Nelson's work is representative of the school of thought in feminist empiricism described as "holism". Nelson adopts much of the framework of Quine's naturalized epistemology but extends Quine's talk of "boundary blurring" to include social and political values within the context of science. Nelson is also sympathetic to Kuhn and other historicist's accounts of scientific paradigms and their criticisms of the logical

empiricist distinction between discovery and justification. However, in the spirit of Quine, Nelson is unsatisfied with Kuhn's rejection of the possibility that a choice between competing theories or paradigms could be settled by "observation". Kuhn's insistence that observation could not provide a clear decision between two individual theories is, on Nelson's view, a left-over legacy of positivism.[22] Nelson argues that accepting the theory-ladeness of observation does not mean that we cannot decide between competing theories. As Kuhn describes it, because of theory-ladeness there is no "higher authority" to appeal to for a rational decision when deciding between two competing theories.[23] For Nelson, this view is mistaken and its mistake is based on the fact that Kuhn has understood scientific theories as "closed world systems"[24]. In Nelson's reading of Kuhn, his account of the situation of incommensurability requires that we are comparing two theories in isolation from all others. In other words, the competing theories at issue are unconnected to any other going theories so that no theoretical frameworks can be brought to bear on deciding between the two. Nelson's point is that Kuhn's thesis of incommensurability requires that scientific communites and the theories that develop from them, are in some sense self-contained, and as a result, the whole world is up for grabs when two theories are in competition. Nelson argues that this view follows only if

we assume either that, (as the logical empiricists argued) individual theories face experience in isolation and that this is all we consider when deciding between two theories; or two, we assume that two groups within a science community disagree about every meaning and every observation - again because we are assuming that these communities are in some sense isolated. Nelson explains that given one or the other of these assumptions, and the ruling out of decisive observations or logical relatioships that can settle things, it would appear that nothing could be brought to bear on the situation to enable a rational decision. Hence, one theory only eventually supercedes the other because of various social pressures.

Nelson's rejection of this view depends upon her use of Quine to explore the assumptions in Kuhn. She begins with the consequences of the theory/observation relationship because a specific understanding of those consequences and an insistence on the autonomy of science are what underwrite Kuhn's analysis and lead to his conclusions. For Nelson, Quine's view suggests an alternative take on our current situation and of the evidence that will be used to decide between competing theories.

Quine, as was discussed earlier in Chapter Two, also insists that observation is theory-laden. In fact, Quine maintains that only within a theory is observation actually possible. For instance, in our current conceptual

framework, observation and coherent experience are possible only after the mechanics of object ontology and individuation have been mastered.[25] So, Quine maintains, there can be no extrascientific grounding for science in observation or sense data. But he also argues that observation is both connected to sensory stimulation and constrained by experience. For a relevant community, observation sentences are at the "periphery" of our conceptual network and more than other sentences within the network, do face experience. The communal status of observation sentences is therefore both a consequence of their theory-ladeness and an indication that such sentences need not be understood essentially as theoretical sentences.

The general point is that such sentences can serve as observation sentences for the community *and* have the specific empirical content they do because of an existing theory the community shares. Quine's understanding of the dual constraint of experience and other going theories is evident in this account of observation sentences, and they are the basis for his claim that it is as a whole that theories explain, and as a whole they are constrained by experience.[26] For Nelson, Quine's position suggests that Kuhn's conclusions regarding the consequences of the theory-ladeness of observation for our ability to bring evidence to bear on a theory, are unwarranted.

Given holism, whatever theories we are comparing will bear various relationships to a number of other going theories and, ultimately, to all the theories that make up our most inclusive view of the world. As examples, Nelson cites "hunter/gatherer" theories versus alternative feminist accounts in anthropology; and "master molecule theories" versus feminist criticism in biology regarding genetic determination of traits and behavior based on protein synthesis.[27] Indeed, Nelson maintains, it is only because these disagreements can and do occur that feminists can understand and criticize the "master molecule" theory and its commitment to a linear and hierarchical model of gene action, and that feminists and "man the hunter" theorists can disagree about the use of tools in early hominid activity. So, although feminist scientists and science critics disagree with nonfeminist accounts about specific theories - including some standards of evidence - these communities and subcommunites of our larger community, do not disagree about everything. They share, for Nelson, theories about molecules, macroscopic objects, continents, societies, gravity and so on.

Thus, on Nelson's view, the autonomy and the insulation of scientific communities that Kuhn's thesis of incommensurabilty requires, is untenable. The feminist criticism of science that she considers indicates that our "common-sense" beliefs and experiences, including those of

sex/gender and politics, are inextricably related to our theorizing in science communities. Many common-sense beliefs are therefore shared by feminists and their nonfeminist colleagues alike and can be relied upon when a case of theory conflict arises.

Ultimately, there is then nothing like the "epistemological chasm" that Kuhn's thesis of incommensurabilty would require. It is not the case for Nelson, that two groups share nothing that can be brought to bear as evidence on those things about which they disagree or that there is nothing that is relevant. While she does think Kuhn is correct in that we cannot decide between competing theories without references to the current state of science in the broadest sense, he is wrong in thinking that the state of science *itself* is on the line - no matter how broad the disagreement might be. Even in the situation between feminist and nonfeminist accounts of science, there remains for Nelson a large number of beliefs and theories, particularly in physical object theory, that are not in question and constitute the "higher authority" that Kuhn claims does not exist. On Nelson's picture of science, we need to study the situation between two competing theories by seeing what is going on in both scientific communities and in our larger community - in terms of changes in common-sense and science as well as in the contexts of social and political arrangements.

Hence on the feminist holist view, evidence is not "self-announcing" or uniquely determined by the logical implications of a hypothesis. It is tied to the current state of science in the broadest sense of beliefs, practices and common-sense experience, all of which are subject to empirical controls. Holism and a coherence account of evidence are, in Nelson's view, sound approaches to understanding the situation of theory conflict in science.

What this account accepts from the historical perspective of science is the influence of seemingly non-scientific factors in the choice between competing theories. Where the feminist holist account goes beyond the historical account, is in its commitment to the view that theory conflict does not involve radical "revolution" between isolated theories. Instead, the holist depends upon a variety of beliefs and practices, including social and political beliefs, which are themselves, subject to empirical controls based on coherence.

ii. The Social Constructivist Account

Like the holist account, the social constructivist account is in disagreement with Kuhn's assesment of the insulated nature of scientific theorizing. But unlike holists, social constructivists maintain that those beliefs shaped by social and political contexts and which impact on scientific theorizing, are not themselves subject to

critical evaluation or constrained by evidence. Regarding a conflict between competing theories of evolution, Longino writes:

> The issue here...is whether there is direct evidence for either of the interpretive frameworks within which the data acquire status as evidential support for hypotheses regarding the dietary and social behavior of early hominids. Not only do we not now have such evidence; we cannot have it. What the study of contemporary hunting and gathering societies should teach us is that any speculation regarding the behavior and social organization of early humans remains just that. This leaves framework choice subject to the speculator's preconceived and culturally determined ideas of what human beings are. The distance between evidence and hypothesis cannot be closed by anatomical and physiological knowledge, by principles from the theory of evolution, or by common-sensical assumptions. It remains an invitation to further theorizing, or as some would have it, story-telling.[28]

For Longino, the incommensurable gap between competing theories can only be filled in by "preconceived ideas" or what she also calls "background assumptions"[29], not by further empirical evidence. A situation where we are faced with two actually competing frameworks is not a situation where we need to seek out more data. Rather, on Longino's view, we need to look to the frameworks of interpretation that theorists committed to either theory adopt. In the case of competing feminist and nonfeminist accounts in science, the data are "dumb". What the data require are assumptions in order to function as evidence. The assumptions in the anthropological case Longino considers above, involve ways of seeing and being in the world which

lead two communities to assign different value to male and female activity. These assumptions spring from culturally embedded beliefs and practices which then lend a particular logic and interpretation to empirical experience.

In accepting the relevance of social and political commitments into our scientific practice, Longino is not implying a simple or crude imposition of those ideas onto the study of the natural world or a radical relativism with regard to theory-choice. Rather, she suggests that if we recognize how knowledge is affected by the assumptions, values, and interests of a culture, and that these factors, within limits, can be a matter of choice, then it is clear, scientists have a choice between competing theories. For example, in the case of competing feminist and nonfeminist accounts in anthropology, scientists can continue to use the language and rhetoric of past sexist and racist practice or instead alter their intellectual allegiances. This would mean remaining committed to a goal of understanding experience but the experience would be shaped by whom we choose to be accountable to, socially and politically, in the pursuit of that goal.

By focussing on accountability in theory-choice, Longino's view differs from a feminist empiricist account like Nelson's which proceeds from natural cognitive processes coupled with social experience, to form a coherent theory of reality. Longino's view is not dictated by the

cognitive and social understanding of "data". Rather, an understanding of the data provides only a minimal constraint on theory-choice and theory adoption. The maximal factors are those which involve political and sociological factors; such as, who is funding the research, who is permitted to perform the research, why the research was begun initially and how successful results will be rewarded. From the beliefs, attitudes and practices that arise in answer to these questions, one theory or model will clearly be "more correct", according to Longino.[30] This is because social and political factors *constrain* reasoning and through this influence on reasoning they influence the shape and content of theories and our choices among competing theories.

Hence, the view of science and theory-choice that emerges on the social constructivist picture is one with a high level of interaction between sociology, politics, science, ethics and philosophy. A community's political perspective however, will ultimately be the final arbiter in theory adoption, whether this fact is conscious or not for members of the community. For Longino, a political commitment is justified so long as it is answering those voices which name the social problems. Since women and non-white men have historically been excluded from having their voices heard, the relevant problems were defined and addressed predominatly by white males. With the increase of women and non-white males in the mainstreams of power, a

change has occured in the kinds of questions demanding answers and hence, the kinds of theories that are being proposed. For Longino, this means that while we cannot seek out a single truth or a "higher authority" in Kuhn's sense, we can nevertheless rank theories according to their worthiness as bases for collective action to solve pressing social problems. This ability to rank theories can also be coupled with a final methodological consideration which urges that the the product of the most inclusive scientific community will result in the better theory, other things being equal, than those which are the product of the most exclusive. It is better, for Longino, not because it is measured against some independently accessible empirical reality, but better as measured against the goals of a genuinely democratic community.[31]

Longino's view is a "contextual" empiricism.[32] A very modest and pared down empiricism that advocates a view of epistemology whereby knowledge is socially constructed. In a sense then, Longino, unlike Nelson, shares Kuhn's skepticism about the possibility of deciding between two competing theories based on *evidence*. One or the other theory will be chosen on the basis of social and political factors.

What Nelson and Longino share as feminists, as naturalists, and as philosophers of science influenced by the historical perspective, is a rejection of the

distinction between psychological or sociological matters of discovery and logical, non-empirical matters of justification. For both Nelson and Longino, justification is either a matter of reflective equilibrium and coherence, with any belief in principle, revisable; or as a matter of political momentum and social concern in which theory-choice is determined by one's cultural framework. Both philosophers go beyond the historicist tradition in science to argue that theory-choice is not fundamentally, a matter of incommensurability, but instead a matter of thematizing the role of social and political factors into science. For Nelson, this means developing an overall coherent system whereby physical theory *and* political values, both constrained by empirical evidence, fall into a situation of equilibrium. For Longino, it means ranking theories according to their ability to address the most pressing public concerns within a society committed to democratic ideals. But in keeping in line with the historicist tradition, both Nelson and Longino reject a context of discovery and justification because of its commitment to the separation of logic and social and historical experience.

V. The Distinction Between the Context of Discovery and Justification Reformulated

In Chapter Four, I argued that justification involves both a logical and an empirical component. In criticizing

Antony's account of the "Paradox of Bias", I argued that she conflated these two components. I tried to show that in evaluating "good bias" from "bad bias" we will need to separate out the particular patterns of reasoning from the subjects and communities who implement these patterns, even if no subject implements them perfectly. In doing this, I argued that epistemologists are left with the job of evaluating schemas and patterns of inference not psychological and sociological processses. My way out of the "Paradox of Bias" transcended motivation and political interest in that whatever one's psychological processes or political commitments, some patterns of inference are nonetheless reliable in virtue of the relationship that exists between the content of the premises and conclusions, and facts about the properties and objects of experience.

In this section I would like to put together the epistemological view in Chapter Four with the realist criticisms of naturalized meaning discussed in Chapters Two and Three, thus leading to a reformulation of the distinction between the contexts of discovery and justification.

To begin, the feminist naturalists discussed earlier are skeptical with regard to the possibility of there being objective, bias free enquirers who employ objective, value-neutral methods of investigation leading to true theories. The most predominant reason for this skepticism is that

there can be no such thing as value-neutral enquiry. The assumption behind this reason is that whenever values, bias, or interests are shown to be influencing an enquiry or affecting a method, then the enquiry and method cannot be objective in the traditional sense because the facts are not directing the course of the investigation. Since feminists argue that the facts can never be determined alone, they conclude that traditional notions of objectivity are untenable. Following this train of thought, feminists resist embracing any non-naturalist notion of objectivity since it carries with it an indifference to the role of values, bias and interest within enquiry.

Originally, in the accounts of justification proposed by logical empiricists like Hempel, objectivity was a characteristic of the context of justification and hence, of a justified theory, because objectivity was supposed to provide accurate explanations of how and why things happened as they did. Accuracy and truth were valuable because they could be relied upon. Consequently, the value of the objectivity of justification was in its connection with truth. Therefore truth preservation served as a regulatory principle in theoretical enterprises.

This relationship of objectivity to truth explains why some ideal of objectivity needs to be incorporated into feminist theory and practice. If an argument is needed for this I think it can be found in the fact that the varied

forms of feminist criticism considered thus far, do understand themselves to be committed to social and epistemic change. They aim at producing theories and social arrangements which are different from those already in existence. This fact gives rise to the significant difference between feminists and other naturalized epistemologists. Feminist epistemologists are not in the business of merely describing "the relation of meager input and torrential output" to use Quine's phrase, but in addition they are concerned with advocating alternatives to existing practices which are unfairly exclusive and particularly harmful. To argue for such a change, a responsibility arises for giving reasons why change is required and also a need to make clear how the change conforms to some standard for judging existing theories and social arrangements. This project is only possible if it is based on reliable theories of how things are and how they might be; as well as a critical understanding of the criteria used for accepting some theories and rejecting others. Taking on this responsibility leads to a requirement to endorse robust metaphysical facts about the world which can be used as evidence for the claim that current theories and social arrangements are limited and oppressive. These premises will function as reasons for recommending change.

So for example, consider a case in neuropsychology.

Hemispheric specialists working on lateralization studies have claimed that men are "right brain dominant" and as such they are more adept at mathematical and scientific creativity. This finding is used to explain male predominance in fields like architecture, art, music, and engineering.[33] Women, on this account show less hemispheric specialization and are instead considered "bilateral". This result is explained by the existence of a thicker corpus callosum in more women than men.[34]

However, feminist neuropsychologists and philosophers of science[35] have challenged these claims by appealing to new, more sophisticated biological models of the brain which do not support differences based on innate structure. These researchers claim that the distinctions as they are currently being drawn, are fraught with ideological commitments that undermine the intelligibility of the distinction. And finally, they argue that the model of thinking as information processing which underlies lateralization research, confuses thinking with internal processing strategies.[36]

In challenging the lateralization view and calling for a reconsideration of neurological sex differences, feminist researchers are employing all of the strategies discussed above. Their challenge turns on a critical understanding of the criteria used in accepting or rejecting neuropsychological theories. Further, they are committed to

an ontology of physical and methodological facts which warrants their prescription for abandoning hemispheric difference theories based on sex.

What this picture shows is how a commitment to theoretical change entails the need for a very strong program of justification. If we consider the feminist criticisms of science in their naturalized forms, do they encompass such a strong program of objectivity and justification?

As was shown in the preceding section, on the Holist model, "coherence" is the condition and measure of objectivity and justification. On the social constructivist model the condition is "consensus". I want to argue that these conditions are not sufficient for the kind of objectivity and justification that will be required for feminist criticsm of science and knowledge. My argument is based on two concerns. The first has to do with the relationship between conflict and objectivity that follows on a coherence or consensus model. The second has to do with the need for a norm which will guide our evaluation and response to criticism.

First off, if there is too much coherence and consensus regarding values and beliefs among members of an epistemic community, then there will be no challenges issued to those beliefs and values. That is to say, their existence and operation within a conceptual framework will be invisible.

This was the point originally brought up in feminist criticisms of philosophy and science. Because so many "common-sense" assumptions went unchallenged, their underlying role within our systems of knowledge were not seen. The bias and interest contained within these assumptions are only now becoming visible as a result of criticism. Hence, if we accept the arguments made by feminist theorists while also accepting either a model of coherence or consensus, then we will have to wait for objectivity to come only after there have been challenges from some members of the community. If no such challenges come, then coherence and consensus cannot guarantee objectivity.

The second problem rests with the fact that it is not clear how we should judge an occasion as an opportunity to change our widely held community values and beliefs in response to a criticism of them. It seems to be the case that rarely does a challenge prove to a community of interdependent knowers that a certain belief should be abandoned. If a belief is widely held and is therefore a matter of "common-sense", then a challenge can always be accomodated in a variety of ways. Consider the example of the history of women in science and education, whose intelligence was not thought to provide disconfirming evidence of the supposed irrationality of women, but who were instead seen as "exceptional". Circumstances like this

show that normative procedures independent of coherence and consensus need to be in place so as to filter out potential psychological and sociological influences which result in the stubborn continuation of false and unfair conclusions.

Assuming the legitimacy of these problems, the need arises for some model of objectivity and justification which transcends basic psychological or sociological coherence and consensus. In other words, we need some way to separate out matters of causal, historical, empirical fact from matters of normative, objective, truth productive procedure. The need for this separation motivates a reconsideration of the distinction between a context of discovery and context of justification.

Feminist naturalists like Nelson and Longino reject a distinction between these contexts based on their acceptance of Kuhn's account of scientific change. However, following a line of argument proposed by Scheffler[37], Kuhn's account can be seen as posing no threat to the distinction. Scheffler's claim rests on three related theses within Kuhn's own writing. The first involves the fact that Kuhn denies the cumulativeness of theories, but he also argues for a predictive criterion as relevant to competing theories. Secondly, Kuhn opposes notions of falsification as settling issues in paradigm conflict, yet he also introduces the notions of anomaly and crises which serve a

parallel function. Third, Kuhn downgrades the relevance of deliberation with regard to paradigm change, but he also allows the importance of claims that the new paradigm will solve the problems that led the old one into crises.[38] Scheffler concludes that Kuhn reinstates the very distinction he intends to discredit. Assuming Scheffler's conclusion, we can remove one significant element of the historicist hold on feminist epistemology and philosophy of science.

In addition, Katz's realist meaning theory discussed in Chapters Two and Three, can be used to account for the change in paradigms without collapsing into meaning invariance. In *The Metaphysics of Meaning*, Katz explains: "I have argued that the significant alternative to the Kuhn-Feyerabend view that words obtain their meaning within a theoretical system is not that words mean something in isolation, but that their meaning derives from the semantics of natural language in general, which is common to rival theoretical systems."[39]

Meaning invariance may still occur across theories because on the realist model all the concepts available to science and everyday explanation are contained within the space of possible senses. The notion of all concepts being contained in such a theoretical space is similar to the idea of an infinity of theorems contained within a finite set of axioms. Our native conceptual structure includes semantic

principles which entail a finite set of primitive senses thus providing us with the stock of elementary concepts and also operations for compounding primitive senses to form a complex concept. Katz distinguishes this semantic account of *concepts* with an account of *conceptions*.[40]

Conceptions are accounts of the nature of a thing and they are arrived at by identifying the extension of concepts. They therefore operate as theoretical definitions, intending to capture the reality of experience. Thus conceptions can either be true or false since they can either describe reality accurately or inaccurately. Examples of conceptions include definitions like "force is a vector quantity which produces an acceleration of a body in the direction of its application".

Concepts on the other hand involve the sense of words or expressions and are neither true or false. They are purely lexical items reflecting the space of possible meanings. Thus concepts operate like lexical definitions. Concepts on this account are neither true or false since there is no assertion being made about the nature of reality. Examples of concepts include expressions like "fathers are male parents".

Conceptions in science express synthetic identity claims within a theory. But the combinations that result in the construction of conceptions depends on the stock of primitive concepts. It follows that conceptual change then

is not merely change in meaning for the realist. Change of meaning is only the altering of a previous relation between senses and expressions in the semantic repertoire of the language. Conceptual change on the other hand, involves the development of new and different scientific conceptions. The combinatorial procedures for arriving at these conceptions are not in principle unknowable for someone within a different conceptual scheme since any speaker of a language has available in their native structure, the elementary stock of concepts and combinatorial procedures. Thus, Kuhn and Feyerabend's account of meaning variance across theories reflects variance in conceptions but not variance of concepts.

Given these reasons to reject both incommensurability and meaning variance, the possibility is reopened for a distinction between the contexts of discovery and justification. However, in opening up this possibility we should not ignore feminist arguments for the relevance of social experience in scientific and epistemic matters. Hence, a characterization of "discovery" as a wholly non-patterned, random domain completely independent of justification, should be rejected. Instead, "discovery" should be reflective of systematic social factors. What "systematic social factors" amount to, requires some further explanation.

In Chapter One, I argued against the "Perspective

Argument" by claiming that what emerges from feminist criticism of science is not a single uniform "woman's perspective" but rather a pattern of marginalizing experience. This pattern of social marginalization arises out of the contingent features of human experience; one's social location, gender, race, class, education, and group-identity. Because these factors are empirical and contingent, they will be viewed as falling within the domain of the "context of discovery".

However, at the same time, there are also theories which have arisen from the perspective of a pattern of marginalization, specifically feminist theories, and many of them, like the example above in neuropsychology, are currently serving as "correctives" in the mainstreams of science. The fact that a corrective potential can arise from patterned social criticism is, in my view, a matter that needs to be taken up in some way by an account of discovery. That is to say, patterned social facts which arise within the context of discovery can play a role in the development of new ways of thinking about the importance of a variety of perspectives. Specifically, it should be a matter for an emerging account of discovery to recognize how patterns of marginalization can lead to critical perspectives, and also to recognize that these critical perspectives confer upon a theory, the ability to explain some aspects of the world more clearly. The plausibility of

this claim rests with showing what exactly is supposed to be involved in such a critical perspective.

The argument I defended in Chapter One involved the claim that marginalized or oppressed groups do not have a patterned perspective automatically as a result of their marginalization. Rather, as I tried to show, a critical, patterned perspective is achieved only after a group works out a "critique of unusablity" regarding the existing beliefs and social arrangements they are being excluded from. This "critique of unusability" reveals inconsistencies in presently held theories and the picture of the world these theories support. A critique will show not just that our present theories are inconsistent on their own terms, but also *how* they are inconsistent and therefore open to alternative explanations and hypotheses. Any theory that arises from this procedure will then itself be subject to critique for the reason that theoretical privilege, in principle, will always be subject to a specific set of social arrangements.

What I am advocating is a recognition within the context of discovery for the sensitivity to evidence that arises from social location. However, this recognition of discovery does not lead to differences for justification. If we consider the case of the lateralization theory discussed earlier, it is methodologically important to subject that theory to any relevant competing theories and

this would include the theories of feminist neuropsychologists. This principle of justification tests the theory further empirically, and creates the opportunity to make explicit any possible underlying assumptions operative in the theory. The different epistemic lens afforded by a feminist theory can be used to test the relative worth of the theory in a wider context. However, such a test is strictly a matter of justification. Yet, in accepting that a socially motivated theory can be a relevant theory to use in justificatory tests is not to suppose that all social characteristics and theories will be equally relevant in all subject-matters. The authority given to a critical perspective will need to be established along with its relevance to a subject-matter before such a principle of justification would be employed.

Hence, what these considerations suggest is an ideal of justification which recognizes the authority of critical perspectives based in social experience but ultimately rests on a commitment to objective principles of rationality. What is similar about my view and the positivist view, is that justification is finally, a domain distinct from discovery. It is based in formal principles derived from reason and ammended by reason in the service of truth. However, my view differs from the positivist account in that the logical principles of justification can best be realized by considering relevant theories to include hypotheses and

theories which have resulted from a critique based on social inequalities. Feminist epistemology has offered important reasons to believe that each of us occupies a standpoint, and that these standpoints make a difference epistemically. Thus, what is required for the correct implementation of the ideals of justification, is the interaction among social groups which are often critical of each other but which have epistemological privilege in their own areas. This interaction will be necessary to arrive at the most complete account of justification possible.

Such an account of justification takes on what has been an important contribution of feminist theory. Namely, that it is possible to accept that non-epistemic factors and values can be operative in decision procedures within a theoretical practice without giving up on the idea that these procedures and practices need to be understood in objective terms. At the same time, the strong program of justification which follows on the account I am suggesting, can reduce the threat of relativism always lurking behind naturalistic models of coherence and consensus and answer the problems above regarding the possible absence of conflict and the need for a normative epistemology. Nelson and Longino in their arguments for what should guide theory-choice, have managed to rule out personal and idiosyncratic factors by making theory-choice a public matter. But this move to the community over the individual does not show how

to establish decision procedures by which conflicting points of view within a practice can be assesed and the conflict resolved by more than an appeal to social allegiances. An objective set of justificatory procedures subjecting theories to the authority of social criticism, will provide knowledge that "transcends" the limited loyalties which can arise within a practice.

Hence, my version of the contexts of discovery and justification, conceives discovery as more systematic and patterned than on the traditional account. It also construes justification as in the service of eliminating the particularly limiting features of a preferred social position. Yet like the traditional account, justification is still being upheld as the domain of principles which are arrived at through rational procedures preserving objectivity, reliability and truth productivity. The realist view of meaning endorsed in Chapters Two and Three, and the realist account of epistemic patterns of inference proposed in Chapter Four, allow for the separation of justification from discovery based on the existence of mind and language independent senses, properties and logical schemas. These senses, properties and schemas, and their contingent and necessary relations, are discoverable through scientific investigation. Yet, as feminist critics of science have shown, investigations of reality can, and have been, obscured by the limited perspective of social

position. By placing a demand upon our justifactory procedures to be more inclusive, we can eradicate the limitations of social location and not forsake the goal of truth seeking.

By relying upon naturalized theories of meaning and accepting the end of the distinction between analytic and synthetic truth, feminists have inherited models of language and logic which are merely descriptions of human social and psychological practice. Yet the commitment to social change and fair practice that is the hallmark of feminism cannot be prescribed or justified given this naturalism. I have tried to show that the arguments against necessity, essentialism and the independence of logic are flawed, and as such the possibility is open to feminist epistemologists to endorse a strong rationalist program. I have suggested that such an endorsement would provide feminist theorists with a model of meaning and knowledge in the service of explaining the "corrective" quality of feminist research. A strong program of justification, as I have suggested, does not have to be blind to the social experiences of marginalized groups. The development of procedures to prevent false conclusions is one of the aims of rational methodology. The contributions of feminist scientists and epistemologists can be found in the critical stance taken toward exisitng paradigms and in the novel and informative reinterpretation of data. However, these contributions do not provide new arguments

for accepting a naturalized epistemology. Instead, these contributions have been analyzed in the light of naturalized models which are merely assumed. My argument has been that feminists have not shown that the ideals of reason have been compromised based on their findings. What they have shown is that to realize the ideals of reason more completely, we cannot ignore the significance of social arrangements.

NOTES

1. Reichenbach, Hans. 1938. Section I.
   Popper, Karl. 1962. pp. 3-18.
   Hempel, Carl Gustav. 1966. pp. 42-59.

2. Reichenbach. 1938. p.4-5.

3. Ibid., p.5.

4. Ibid., p.6-7.

5. Hempel. 1966. pp.101-102.

6. Ayer, 1950,1955,1959, Schillp 1963, Carnap 1962

7. Hempel, Carl Gustav. 1965. p. 245. Emphasis mine.

8. Hempel. 1966. pp.14-15

9. Hempel. 1966. p. 17

10. Popper, Karl. 1959. Chapters 1, 2, and 5.

11. Hempel. 1965. p.21

12. Ibid. p.25

13. See Hanson, N.R. 1958.
    Kuhn, Thomas. 1970.
    Popper. 1959. especially Chapter 5

14. See Longino. 1990. p.22

15. Popper. 1959. For example see Chapters 1,2,5

16. See Hanson, N.R. 1958.
    Kuhn, Thomas. 1970.
    Feyerabend, Paul. 1975.

17. Kuhn. 1970. p.23-24

18. Kuhn, Thomas. 1970. Chapter IX.

19. Feyerabend. 1975. p.30.

20. For example see Scheffler's, 1982. *Science and Subjectivity*. Chapter 4 for the varied interpretations of Kuhn.

21. Ibid.

22. Nelson. 1990. p.237

23. Kuhn. 1970. p.93

24. Ibid., pp.163-164

25. See Quine's discussion of physical-object theory in Quine. 1960. pp.7-10

26. Quine notes in "Two Dogmas..." 1963. that the analytic/synthetic distinction and the view that individual sentences have their own specificable empirical content in isolation from a going body of theory are at bottom one view.

27. Nelson. 1990. pp.232-34

28. Longino. 1990. pp.109-110.

29. Ibid., pp.109-123.

30. Ibid., pp.53-55.

31. Ibid., p.214

32. Ibid., p.215.

33. Bryden. 1979.

34. Buffrey, Anthony and Jeffery A. Gray. 1972. "Sex differences in the development of spatial and linguistic skills." In *Gender Differences: Their ontongeny and significancd*. ed. Chrisotpher Ounsted and David D. Taylor. Edinburgh, Scotland. Churchill & Livingstone. pp. 123-157.

35. For a detailed discussion of this debate see "Women and the Mismeasure of Thought", 1989, by Judith Genova. In *Feminism and Science*. ed. by Nancy Tuana. pp.211-224.

36. Ibid., p.214-215.

37. Scheffler. 1982. pp.82-89.

38. Ibid., p.89.

39. Katz, J.J. 1990. p. 309.

40. Ibid., p.243.

CONCLUSION

The view of scientific knowledge and reasoning that I have been trying to develop throughout the course of this work is realist. It is however, a realism that is not indifferent to social realities. As such, it provides a powerful program for the analysis of evaluative claims in epistemic inquiry. In this last section I want to summarize the preceding arguments for realism and suggest some possibilities given my view.

Feminist epistemology, like other social epistemologies, begins at the point of rejecting individualistic accounts of knowledge. Traditionally, ascription of knowledge to individuals depended on recognizing that they have beliefs with the "right" properties.[1] The right properties include for example, that the beliefs are true and are "properly grounded" or "justified" or "warranted" given the evidence. This view gave rise to the traditional formula, "X knows that *p* just in case *p* and X believes that *p* and X's belief that *p* was formed by a process that is justifiable or reliable (in the appropriate sense[2])." For the feminist epistemologists discussed above, the point at which epistemology becomes social is when it becomes apparent that the possibility of justification turns on the properties of other people or of the group to which the subject belongs. For this reason,

feminists have sought to analyze the processes whereby groups develop standards for gathering and justifying hypotheses and reaching social consensus. Involved in such an analysis, are proposals for how to distinguish justified consensus from unjustifed consensus. Therefore, some standards of justification are proposed by feminists so as to decide how group consensus should properly be formed. We have looked at those proposals throughout the course of this work and have seen that they are advanced from the perspective of a *naturalized* epistemology.

The version of realism that I have been advocating, maintains that there are relationships between language and entities like physical objects, senses, logical schemas and properties, which are independent of the persons who use language. These relationships, together with the state of reality, combine to determine the truth values of our statements. The feminists I have been considering have resisted this realism in favor of a notion of truth based on some account combining value-ladeness with communal acceptance.

The way in which one represents the world will undoubtedly have a connection to causal history and social experience. But, what I have tried to show in previous chapters is that an appreciation for the social situatedness of knowers is not to discount the independent reality of what is known.

The feminist epistemologists considered throughout the course of this work, have offered important insights with regard to social situations that may make an epistemic difference. However, by admitting the significance of factors like social standing in epistemological accounts is not to dismiss the ideals of objectivity or realism. Instead, as I have tried to show, different standpoints make available more or less epistemically relevant evidence for arriving at true beliefs. In the example of the test for the hypothesis concerning male spatial superiority described in Chapter Five, mainstream scientists considered biology as the proper locus for explanations of sex differences. Feminist endocrinologists were skeptical with regard to this assumption and assumed that sociology might better explain the differences. The different perspective offered by feminists, in this case, provided more epistemically relevant evidence for arriving at the truth. The differences in these perspectives are reflective of how differences in social location can affect members of epistemic communities. However, as I've argued in Chapter Five, while perspectives relative to social arrangements can matter to questions of patterned epistemic discovery, they will not matter when it comes to how to evaluate the accuracy or reliability of a hypothesis. Thus, a distribution of standpoints can fruitfully contribute to the goal of a more complete theory of knowledge. At the same

time, a realist and rationalist epistemology need not be compromised by accepting the significance of a distribution of standpoints. The result I have argued for is therefore a combination of realism and social epistemology which avoids the risk of radical relativism that threatens naturalized accounts.

The central motivation throughout the development of my argument has been a hope for a more constructive dialogue between *rationalized* epistemology and feminist epistemologists. Feminists are right, I believe, to charge traditional epistemology with neglecting the consequences of epistemological overconfidence for human lives and understanding these consequences from the perspective of history and politics. At the same time however, in their rejection of foundational epistemologies feminists have been too quick to rely on arguments that discount the benefits of strong epistemic standards. To recognize human fallability and oppressiveness in the implementation of epistemic standards, is not reason to abandon the benefits of standards. Rather, it is reason to develop the means for correcting our procedures so that they can be utilized more objectively. Realizing this, there is reason to see feminism and rationalized epistemology as potentially important philosophical partners.

Feminist epistemologists have offered new ways of conceptualizing the project of epistemology and its relation

to politics and society. My argument has attempted to show that this renewed conception, rather than being detrimental to either epistemology rationalized or feminist theory generally, actually fosters the goals of both. This point involves two related theses. First, because a rational epistemology aims at the correct evaluation of procedures for discovering truth, feminist arguments have helped to show how one's social position can obscure or reveal reality and therefore, affect evaluation. With the development of these arguments, feminism has sought to fashion new ways of thinking about objectivity truer to the goals of science.

Second, because feminism aims at ending discriminatory social practices and the undervaluation of women's experience, a realist epistemology and a strong program of rational justification can be in the service of providing the critical stance necessary for advocating social change. Hence, a close association between epistemology and feminist theory can challenge unjustified, discriminatory practices at the roots, while furthering the aims of both disciplines.

By looking at philosophy from "the outside in", where the "outside" is the larger world of stereotypes, social arrangements, and politics, the partnership of a rationalized epistemology and feminist philosophy of science can lead us to uncover biased assumptions which may inhere in epistemic investigations. Thus in attempting to dislodge

firmly held beliefs from the underside of an argument, this partnership opens up the possibility for weakening biases and unjustified beliefs and strengthening the conviction for justified beliefs.

It is worth remembering that feminist philosophy is a relatively new area and still has significant ground to cover in its analysis of the past 2500 years of theory construction. As such, it is not surprising that there is only a modicum of consensus about what exactly is relevant, obvious, and far from obvious in feminist critiques of science. However, as I have tried to show above, throughout the past two decades there have been enough cases emerging from feminist epistemology and philosophy of science to warrant consideration of the critique. Feminists are concerned with developing mechanisms to foster an awareness of how assumptions may bear on the work of scientists. The presence of feminists therefore, is a means for making our theories of knowledge more rational. I think ultimately, the question remains open as to whether this presence marks the start of a new area of philosophical scholarship with long range staying power or a temporary adjunct to the philosophy of science in the service of correcting hidden errors. Because of the open question, feminism can benefit by remaining in a true dialogue with the tradition of philosophy, engaging with its methods and mastering its procedures so as to best understand its future potential.

At the same time, philosophers should be open to how the particular approaches taken by feminists can in fact enrich philosophy generally by highlighting the gaps and exclusions in the tradition.

Throughout the course of this dissertation I have advocated one particular approach in philosophy that I believe is importantly compatible with feminism. The two models of theory-choice considered here, the Holist Model and the Social Constructivist Model, have not solved problems like "The Paradox of Bias" or the problem of relativism. I have tried to show that a realist epistemology and metaphysics, combined with a realist theory of meaning and logic, should be considered by feminist epistemologists. My claim has turned on the arguments that show how realism and rationalism are clearly the kind of philosophical approaches which advance the brand of rational censure necessary for feminist and other social epistemologists to successfully attain their political and philosophical goals.

NOTES

1. Carnap 1958, Quine 1969a
2. See the extensive literature that followed Gettier 1963.

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