Distribution Agreement

In presenting this thesis or dissertation as a partial fulfillment of the requirements for an advanced degree from Emory University, I hereby grant to Emory University and its agents the non-exclusive license to archive, make accessible, and display my thesis or dissertation in whole or in part in all forms of media, now or hereafter known, including display on the world wide web. I understand that I may select some access restrictions as part of the online submission of this thesis or dissertation. I also retain the right to use in future works (such as articles or books) all or part of this thesis or dissertation.

Signature

_____ _____

Stephanie R. Solomon   Date

Through a Glass Darkly:
The Hidden Perspectives that Challenge and Redeem
Science's Self-Conception

By

Stephanie Solomon
Doctor of Philosophy

Philosophy
The Graduate School

_____
Dr. Mark Risjord
Advisor

_____
Dr. Michael Sullivan
Committee Member

_____
Dr. Cynthia Willett
Committee Member

_____
Dr. Alison Wylie
Committee Member

_____
Dr. Robert McCauley
Committee Member

Accepted:

_____
Lisa A. Tedesco, Ph.D.
Dean of the Graduate School

_____
Date

Through a Glass Darkly:
The Hidden Perspectives that Challenge and Redeem
Science's Self-Conception

By

Stephanie Solomon
B.A. The Ohio State University, 2000
M.A. Emory University, 2006

An Abstract of
A dissertation submitted to the Faculty of the Graduate School of
Emory University in partial fulfillment of
the requirements for the degree of
Doctor of Philosophy

Philosophy

2008

Abstract

Through a Glass Darkly:
The Hidden Perspectives that Challenge and Redeem
Science's Self-Conception

By Stephanie Solomon

This dissertation examines the compatibility of the social dynamics of scientific research with the scientific ideal of a self-critical empirical enterprise. In order to avoid either using the social dynamics of science to undermine its goals or to alternately lend credence to views that endorse isolating scientific methods from the social dynamics of their practice, I return to a major origin of these discussions in Thomas Kuhn's work. In the first chapter, I argue that a major insight brought out by Kuhn's work has been overlooked: the limiting role professional hierarchies and shared social assumptions have on the location and nature of criticism in scientific practice.

In the second and third chapters, I explore several possible remedies to this problem as articulated by three prominent feminist philosophers, Lynn Hankinson Nelson, Alison Wylie, and Helen Longino. I derive many positive contributions from these thinkers, but I argue that they ultimately are unable to rescue empiricism from the challenge brought to it by the shared assumptions and power hierarchies of scientific communities.

In the fourth and fifth chapters, I illustrate these social dynamics of scientific communities in the case of early AIDS research. I demonstrate both how scientific criticisms were delayed and ignored due to certain social dynamics, and also how these problems were ultimately remedied through social changes as well.

In the sixth chapter, I argue that in order to maintain the scientific ideal of self-criticism, scientific practice must be guided by social norms, not merely theoretical and methodological norms. Specifically, a social norm is required to (1) recognize that nonscientific communities often acquire expertise relevant to challenge scientific assumptions, (2) acknowledge that social hierarchies often hinder these types of expertise from being recognized, and (3) seek out and develop these types of expertise in order for scientific communities to achieve their own epistemic goals. I conclude by indicating how this social norm is loosely reflected in the practice of Community-based participatory research, but is also useful to provide a more specific and epistemically-argued grounding for this current practice.

"Through a Glass Darkly":
The Hidden Perspectives that Challenge and Redeem
Science's Self-Conception

By

Stephanie R. Solomon
B.A. Ohio State University, 2001
M.A. Emory University, 2004

Advisor: Mark Risjord, Ph.D

A dissertation submitted to the Faculty of the Graduate School of
Emory University in partial fulfillment of
the requirements for the degree of
Doctor of Philosophy

Philosophy
2008

Acknowledgements

TABLE OF CONTENTS

Introduction

Not everything that counts can be counted, and not everything that can be counted counts. (Sign hanging in the office of Albert Einstein at *The Institute of Advanced Study*, Princeton, New Jersey)

As a result of the various underdetermination arguments set out in the second half of the 20 [th] century, the ideal scientific method that was supposed to guarantee unbiased and justified science were found necessarily insufficient without a further engagement with the conditions that make this method possible. These underdetermination arguments, set out most famously by Thomas Kuhn but also many others [1], provided the insight that logic and direct observation are (and must be) insufficient to determine which scientific hypotheses and theories should be endorsed by scientific communities. The inferences between observations and theory are made as the result of presupposed metaphysical beliefs about and models of the world which determine the significant ontological categories at work and the relevant causal relationships between them.

Not only are these presupposed beliefs necessary, but they often crucially involve the influence of social values. In this way, the traditional separation between the values that inform the "context of discovery" but are eliminated from the "context of justification" is undermined. Only in the context of the background of broader meta-empirical beliefs, many of which are social, can "scientific method", narrowly conceived as justifying theories by evidence, proceed. But while the means of criticizing measurement and logic are relatively clear, the means and even possibility of criticizing the metaphysical and methodological beliefs behind them are much more problematic. If

---

[1] Many others also set out similar arguments, such as W.V.O. Quine {Quine, 1969 #40}{Quine, 1960 #209}, Feyerabend{Feyerabend, 1975 #65}, Duhem{Duhem, 1976 #127}, Hanson{Hanson, 1967 #202}, {Hanson, 1969 #200}and Mary Hesse{Hesse, 1966 #175}, {Hesse, 1980 #178}to name a few.

the possibility of a significant aspect of scientific self-criticism is put in jeopardy, the value and rigor of the scientific enterprise is likewise threatened.

In a distinct but simultaneous historical trajectory, feminist theorists emerged who critiqued scientific methods as well. As Helen Longino points out, feminist theorists who engage with epistemology and science are primarily attempting to "reveal or prevent the disappearing of the experience and activities of women and/or that they reveal or prevent the disappearing of gender ."[2] Traditional philosophy and science have been criticized by feminists for excluding the voices and experiences of women and for substantively explaining both the physical and social worlds in androcentric and sexist ways.[3] While some of the androcentrism and sexism has been conscious and intentional, a great majority of instances are systemic and unconsciously performed and perpetuated. Thus, the phrase "disappearing of gender" is intentionally ambiguous as both an active and a passive characterization of the phenomenon.

Historically and sociologically, feminist theorists have attributed the disappearance of gender to the fact that those who have practiced science and those who are able to criticize it have traditionally belonged to elite social, economic, and political strata of society, and consequently scientific accounts of reality correspond to the experiences and assumptions that characterize that social world. Those who do not share those experiences and assumptions have been seen as illegitimate voices of protest if they were heard as voices at all. Further, feminist theorists have argued that these scientific

[2] Helen Longino, "In Search of Feminist Epistemology," *Monist* 77, no. 4 (1994)., 479
[3] Although the distinction between these two terms is disputable, Longino contrasts androcentrism as referring to a perception of social life from a male point of view that consequently fails to accurately perceive and describe the activities of women. Sexism, on the other hand, refers to points of view that presuppose, assert, or imply the inferiority of women. Helen E. Longino, *Science as Social Knowledge : Values and Objectivity in Scientific Inquiry* (Princeton, N.J.: Princeton University Press, 1990)., 129

biases are reflections and products of ubiquitous injustices and imbalances (in education, access, recognition, resources, etc.) in the larger societies in which they exist.

The historical phenomenon of the "disappearance of gender" is intimately connected to the underdetermination argument in two ways. First, background assumptions about gender (gender dichotomies, definitions of gender categories, etc.) in science are prime examples of both the existence of metaphysical presuppositions that can guide scientific inferences, as well as the unrecognized and unchallenged persistence of these assumptions prior to feminist analysis. This is why feminists have brought to light and argued against the metaphysical models as they function in sociobiology, endocrinology, archaeology, and many other fields to illustrate both the incorporation and the criticism of metaphysical views in science.

Second, the conditions necessary for feminist theorists to bring these assumptions to light and assess them are also conditions for the criticism of these beliefs. Feminists were and are able to criticize the androcentric assumptions in science not only because they found empirical, conceptual, and even political flaws in the method and content of science, but because finally feminists, to a greater or lesser degree, achieved the epistemological or cognitive authority in society and in scientific communities to have those criticisms heard and heeded. Before those voices were both politically and epistemically recognized, not only were criticisms of metaphysical views often not heard, but the mere existence and role of the androcentric frameworks themselves went unnoticed.

While philosophers and historians of science have focused on whether the content of scientific metaphysics infused with social values can be *rationally* criticized (by appeal

to what kind of arguments, conceptual, empirical, political?), feminist critique as a phenomenon itself problematizes the *social* possibility of criticizing scientific metaphysics (who is socially allowed to make appeals? How do social conditions affect the awareness of metaphysical presuppositions in the first place?). It is this collection of questions that will be the focus of this dissertation.

In the first chapter, I will take the discussion of the implications of underdetermination back to one of its major sources, Thomas Kuhn's work *The Structure of Scientific Revolutions*. While Kuhn's work is well-trodden territory in discussions about "progress" in science and incommensurability between scientific worldviews, his insights and observations about the social dynamics that hinder scientific criticism have not been adequately examined. I will draw out and define two distinct social dynamics from his insights. Taking these as my point of departure, I will redirect the trajectory of conversation away from the more abstract issues of incommensurability and paradigms and toward a more concrete discussion of the ways in which the social dynamics and background beliefs that inform science can both help and inhibit its endeavors.

These two particular social dynamics are not only underexamined in the philosophical literature, they are also the key to understanding the "disappearance of gender" as well as other epistemological embarrassments in the history of science. The first I call epistemic blindness, and the other social entrenchment. While neither of the terms are new, I am giving them specific definitions in the context of this dissertation. Epistemic blindness is the phenomenon whereby the beliefs that inform scientific methods, categories, and practices are assumed and unrecognized by at least a majority of the scientific community. Social entrenchment is the phenomenon where the social

hierarchies within scientific communities, and between scientific and nonscientific

communities, serve to entrench these beliefs and assumptions and render them either

immune to or strongly resistant to criticism. I will further articulate and illustrate these

phenomena in Chapter 1, and how well normative accounts of science can deal with these

phenomena will serve as a test for their adequacy throughout the dissertation.

In Chapters 2 and 3, I will further redirect the trajectory of the Kuhnian inspired

discussions about science by engaging the discussions of three important feminist

theorists: Lynn Hankinson Nelson, Alison Wylie, and Helen Longino. Each of these

thinkers engages Kuhnian insights with an eye toward conceptualizing a social *and* self-

critical scientific enterprise that places them beyond the commonplace division between

those who believe that the infusion of sociological assessments of science undermines its

ability to be a special epistemic enterprise, and those who believe that social dynamics

can still be separated from rigorous scientific practice. Further, as *feminist* theorists,

these three thinkers also directly engage the ways that collective biases and power

dynamics have affected scientific research. I will utilize the feminist theorists' attempts

to take the discussion well beyond the resources and capabilities of Kuhn himself and

provide many crucial conceptual and theoretical insights. Using these insights as well as

lessons learned from the limits of their account, I will draw conclusions for ways in

which scientific research should be structured so that it can address the social challenges

to rational criticism posed by epistemic blindness and social entrenchment.

I will then, in chapters 4 and 5, present a case study demonstrating the on-the-

ground workings of epistemic blindness and social entrenchment in the case of AIDS

research in its early years. I specifically utilize the early science and politics of the AIDS

movement as a case study to ascertain the ways in which the views of certain scientists and those of specific nonscientific communities were both recognized and ignored by the AIDS science establishment. I follow these dynamics through to the positive effects of AIDS activists to develop alternative research practices that in turn led to more effective and applicable results, as well as the persistent marginalization of other AIDS-effected populations and the gaps in research and application that resulted from it.

This case study will serve two important functions. First, it will show that my epistemic worries about scientific social dynamics inhibiting scientific criticism and innovation leads not merely to philosophical questions but also to very urgent practical problems. Second, it will show various ways that these practical and epistemic challenges were ultimately addressed. While historically this was largely a process of trial, error, and political exigencies, I want to glean from these ad-hoc solutions some guidance toward a more general and programmatic set of solutions.

Finally, in Chapter 6, I articulate a general 3-part articulation of this solution. First, I will utilize sociologists of science and feminist epistemologists to better understand the social dynamics of, and conditions for, expertise, and how understanding these conditions in a general way can provide crucial tools with which to identify the relevant collaborators in scientific research programs. Next, I will bring out the lessons from throughout the dissertation regarding the power dynamics of scientific communities to illustrate the hindrances to recognizing less powerful experts that must be overcome. Finally, I will draw from these first two moments a social norm for scientific practice that would be capable of overcoming both epistemic blindness and social entrenchment in ways that previous social epistemologies have not. I conclude with the normative point

that the scientific community, once it acknowledges that relevant expertise is often held

by nonscientific communities on particular subjects, should be committed to developing a

methodological sensitivity to the power dynamics and cultural intricacies of the situation.

This sensitivity is necessary in order to transcend the problems of epistemic blindness

and social entrenchment because those outside of the scientific community that

potentially hold this expertise are also usually less likely to be recognized, and have also

been historically resistant to being involved as a result of their experiences.

In the conclusion, I will provide an example of this social norm in practice as well

as future challenges that need to be addressed. It is by now widely recognized outside of

feminist circles that the results of scientific research have often been unbalanced,

reflecting and benefiting certain populations and ignoring and neglecting others.

Feminist criticisms have been joined by those provided by theorists of color and

nonwestern theorists to focus on solutions to these problems. Researchers and

practitioners alike have articulated possible remedies. Generally, the proposed solutions

have called for increased attention to the complex issues that comprise and compromise

the lives of people living in marginalized communities, for more integration of research

and practice, and for greater community involvement and control. In other words, they

have argued for more collaborative methods that incorporate nonscientific communities

as agents into the practice of research. This often takes the form of community-based and

participatory research.

In spite of this recognition and response, these approaches are continually

challenged by questions raised regarding their validity, reliability, and objectivity.

Allegiances to traditional scientific methodologies make it difficult to convince academic

colleagues, potential partners, and funders of the value and quality of research that collaborates with its subjects. My dissertation attempts to utilize the lessons brought out by the early chapters to justify the integration of contributions from marginalized communities into research by appealing to their positive ramifications for fundamental scientific ideals and goals.

I believe that the arguments and analyses in my thesis can be utilized to improve the efficacy and support of community-based approaches to research. While these methods are increasingly being funded and attempted throughout the world, they continue to face persistent challenges from scientific and governmental institutions. Moreover, the recognition of scientific responsibility to help develop potential expert communities into a community of experts can provide an important rationale for community-based researchers' commitment to empowerment. And the ability to enable and incorporate relevant expertise to a given question or problem can easily be understood to increase the validity, reliability, and objectivity of scientific research. In the future, I want to examine in detail the ways in which different CBPR programs develop the skills and conceptual capacities of community members, and use these examples to better understand the process of developing expertise as both a theoretical and practical capacity.

I hope that this solution will provide both theoretical and practical resources to address the problem of the "disappearing of gender" as well as the "disappearing" of various other social, psychological, and methodological possibilities that will not only allow scientific communities to be more reflexive about their own practices, but to also weaken some of its conservatism that according to Kuhn leads to science's *Essential Tension*. In this changing and globalizing world, being able to systematically respond to

problems and change in response to new experiences and voices is key to maintaining

scientific institutions as an Enlightenment ideal of progress, as opposed to the

postmodern and ultra-religious fear of totalizing and Westernizing control.

Enjoy.

Chapter 1: Kuhn Revisited

Thomas Kuhn and Paradigms

The challenge to science

Kuhn began his academic career as a theoretical physicist, but upon taking an experimental science course for non-scientists he was exposed to accounts in textbooks and scholarly work that radically conflicted with his hands-on experiences of how science was actually practiced. The misalignment between the conceptions and standards of science he was being taught and the reality of science as it had been practiced compelled him to abandon physics for a career in the history and philosophy of science. There he could examine the historical dynamics of science and connect them to his philosophical concerns about the impact of their prior neglect upon the image and debates of science in his day. The most influential fruit of this labor was the little book, *The Structure of Scientific Revolutions*, which was published in 1962 and by 1987 was reported to be the most frequently cited 20th century book in the period 1976-1983. The *Times Literary Supplement* included it in its list of "The Hundred Most Influential Books Since the Second World War."

What Kuhn found that so powerfully impacted scholarly work about the sciences was the difficulty of dismissing obsolete theories of nature, like Aristotelian dynamics, Newtonian physics, or Ptolemaic astronomy, as simply erroneous or bad science that was later replaced by theories with obviously better evidence, solely through the use of "scientific method". He found that early scientific theories were as much the result of scientific method, i.e. the use of empirical observation, theoretical hypotheses, and logical connections to test claims about the world, as those theories that challenged and

ultimately replaced them. Overall, scientists in the past were no more biased, or

idiosyncratic, or faulty in their measurements and logic than scientists are today.   Kuhn

concluded from his analysis of cases such as the Copernican Revolution and Lavoisier

and Priestley that "the act of judgment that leads scientists to reject a previously accepted

theory is always based upon *more than* a comparison of that theory with the world." [4]

The general insight that logic and evidence cannot by themselves justify particular

scientific hypotheses or theories did not originate with Kuhn, nor was it unique to him.

More generally, it comes under the family of insights about underdetermination that have

emerged from various quarters, arguably beginning with Pierre Duhem's assertions in

The Aim and Structure of Physical Theory, initially published in French in 1906. The

"Underdetermination thesis", which argues at its most general level that theories are

underdetermined by the empirical data brought to support them, was also set out in

varying forms by W.V.O. Quine, Paul Feyerabend, Mary Hesse, Norwood Russell

Hanson, and various others.

Lumping these different thinkers together is itself illustrative that there is no one

straightforward implication of underdetermination. While all of the proposals of

underdetermination circle around similar challenges to the logical positivist tradition,

their proposers have often been problematically invoked as presenting a monolithic

theoretical argument. Although they all engaged with similar general insights, these

thinkers' ultimate philosophical commitments greatly diverged. Quine remained until the

end a physicalist, a behaviorist, and an empiricist, while Feyerabend and Hanson at the

other extreme are often implicitly, and sometimes explicitly, idealist, irrationalist, and

---

[4] Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 3rd ed. (Chicago, IL: University of Chicago Press, 1996)., 77. italics mine

relativists. While Quine focused more abstractly on the *semantic* situation that the

significance and interpretation of statements always faces the tribunal of sense experience

as a whole, Duhem, like Kuhn, focused on the more concrete assertion about *science* that,

"The only thing the experiment teaches us is that among the propositions used to predict

the phenomenon and to establish whether it would be produced, there is at least one error;

but where this error lies is just what it does not tell us." Kuhn himself, also concerned

more with science than philosophical semantics, theorized the history of scientific

development and, crucially for my purposes, included accounts of the role of the social

dynamics of science in this development.

There are two interconnected reasons to begin with an engagement with Kuhn's

thought. First, Kuhn's account of paradigms and incommensurability largely set the

stage for a majority of the discussions about the role of social relations and social values

for epistemological accounts of science in the 20th century. As a result, many of the

terms of art and dynamics that I wish to engage are best understood by beginning at their

source.

Second, I believe that the scholarly preoccupation with incommensurability has

led attention away from aspects of Kuhn's work that need to be engaged, namely his

account of the ways that scientific communities are *socially* resistant to criticism. While

many other thinkers engage underdetermination as a dynamic of theories and

propositions, Kuhn understands paradigms, not merely as background theories, but also

as background social practices (like training, tools, etc.) that guide scientific research.

Kuhn recognized that science is a practice that requires both theoretical *and* social

---

5 Pierre Duhem, "[Selections from] the Aim and Structure of Physical Theory," in *Can Theories Be Refuted?*, ed. Sandra Harding (Dordrecht: Reidel, 1976)., 6.

engagements, a crucial insight that is only lately coming to light in discussions of social epistemology. I will address these two points in turn.

Kuhn and the Science Wars

Kuhn's work has been adopted, dismissed, co-opted, uncritically endorsed, and continuously debated by many academic fields including, but in no way limited to, the philosophers and historians of science to which it was initially addressed. After its publication, Kuhn's account was taken up largely by thinkers preoccupied with what Andrew Ross coined the "Science Wars," where diverse political and cultural aims galvanized around the issue of whether science represents the greatest hope for the progress of knowledge and improved world situations or is to blame for global misunderstandings and social woes and oppressions. Ian Hacking summarizes the situation in *The Social Construction of What?*: "The science wars, as I see them, combine irreverent metaphysics and the rage against reason, on one side, and scientific metaphysics, and an Enlightenment faith in reason, on the other." [6]

As a result, invocations of Kuhn's work have focused primarily on the issue of the metaphysical implications of paradigms for realism/antirealism regarding the objects of science and the implications of this for the idea of the "progress" of science. These arguments have continued to polarize between sociologists of science and postmodernist thinkers on the one side, and philosophers of science and scientists on the other. It [7] seems like the possibility of civil communication between these two sides is nearly impossible.

---

[6] Ian Hacking, *The Social Construction of What?* (Cambridge, Mass.: Harvard University Press, 1999)., 62.
[7] These arguments also play out in the public domain between anti-science religious groups and anti-religious science groups.

On one extreme, those involved in the sociology of science, especially the Edinburgh "Strong Programme" beginning in the early 1970's, took the underdetermination thesis generally, and Kuhn specifically, to undermine the adequacy of philosophical explanations of science that depend on appeals to the rationality or truth of scientific beliefs as an explanans. Instead of rational reconstructions, they advocated a "Principle of Symmetry", whereby both true and false scientific beliefs are equally in need of sociological explanation, and should be explained with reference to the same types of factors as used to explain the belief systems of non-Western modes of thought. M.D. King made an early articulation of this position in his essay "Reason, Tradition, and the Progressiveness of Science."

> What I would advocate is a kind of "epistemological agnosticism". . .which would give sociologists the opportunity of developing the kind of approach that serves more to illuminate actual historical processes of change in the patterns of thought, mode of practice, and social situation of scientists, than to meet the demands of epistemology.[8]

Those who advocated positions on the more conservative end of this program espoused merely the methodological use of ontological skepticism and relativism to better understand the role of social factors in scientific decision-making. On the more radical end, this methodological move was often ambivalently conjoined with the meta-ontological claim that no ontology *is* better than any other, because the way the world is does not constrain what we believe about it. Underdetermination, combined with Kuhn's account of paradigms, was taken to establish that social factors not only play a necessary role in scientific choices, but ultimately determine these choices without any empirical repercussions.

---

[8]{King, 1971 #212},31{Laudan, 1990 #214}.

As Harry Collins famously or infamously asserted, "the natural world in no way constrains what is believed to be."[9] While Collins' more radical point can also be found in other sociologies of science, including Knorr-Cetina: "Epistemic relativism . . .is only committed to the idea that what we make of physical resistances and of meter signals is itself grounded in human assumptions and selections. . .specific to a historical place and time."[10] others did not endorse this more radical thesis.

While this more extreme position is often identified with the Strong Programme itself, Barry Barnes, one of its founders, rejected this move from the very beginning. "It is important not to lose sight of the connection which does exist between knowledge and the real world. . . Knowledge arises out of encounters with reality and is continually subject to feedback-correction from these encounters."[11] In fact, three years earlier he acknowledged and repudiated the temptation to move from methodology to ontology.

> Occasionally, existing work leaves the feeling that reality has *nothing* to do with what is socially constructed or negotiated to count as natural knowledge, but we may safely assume that this impression is an accidental by-product of over-enthusiastic sociological analysis, and that sociologists as a whole would acknowledge that the world in some way constrains what it is believed to be.[12]

Pickering and Latour, two other sociologists who are often associated with the more radical position, have also explicitly distanced themselves from these views, at least since the mid-1990's.

---

[9] Harry M. Collins, "An Empirical Relativist Programme in the Sociology of Scientific Knowledge," in *Science Observed: Perspectives on the Social Study of Science*, ed. K Knorr-Cetina and M. J. Mulkay (London; Beverly Hills: Sage Publications, 1983)., 91.

[10] Karin Knorr-Cetina, "The Constructivist Programme in the Sociology of Science: Retreats or Advances?," *Social Studies of Science* 12 (1982)., 321.

[11] Barry Barnes, *Interests and the Growth of Knowledge* (London: Routledge & Kegan Paul, 1977)., 10.

[12] Barry Barnes, *Scientific Knowledge and Sociological Theory* (London: Routledge & Regan Paul, 1974)., vii.

In spite of these contrast cases, it is the more radical position, either in appearance or reality, which has led to the frequent antagonisms between philosophers of science and sociologists of science, even those whose different methodological stances do not themselves make the two projects at odds. In the midst of this melee, the Kuhnian position that social dynamics, historical contingency, and conservative puzzle-solving play a necessary role in scientific research has not surprisingly been a hot button. It is this perceived antagonism that has also distracted discussions away from the other contributions that Kuhn's work can provide for the analysis of scientific knowledge. Rather than joining a "conversation" that I already believe has gone too far, I would like to return to a place of both common sense and common ground between the two camps.

Kuhn's account: Back to the source

Kuhn's work was one of the first descriptive accounts utilized to challenge the exclusive power of scientific method and to emphasize the necessary role of social dynamics in scientific practice. While it is relatively simple and widely accepted that underdetermination generally requires that the scientific acceptance and rejection of hypotheses and theories must rest on something more than appeals to observation and logical connections (scientific method), what exactly constitutes this "more" has been widely debated throughout the 20th century.

The reason for this is twofold. First, the foundation of the objectivity, criticizability, and universal qualities of scientific claims had previously depended upon their foundation in empirical evidence and logic. By undermining this ground of justification, the autonomy of science from subjective, ideological, and contingent factors is brought into question, and the threat of circularity of justification looms. We saw the

larger impact of this threat in the last section. Thus, much is at stake in delineating these other factors and how scientific claims are thereby justified and justifiable.

Second, the "more" is very complicated. To consider science as a human practice, rather than a purely methodological procedure, is to introduce all of the difficulties and complexities of human dynamics into the discussion of scientific decision-making. Kuhn himself struggles to explain these background dynamics under the rubric of the concept "paradigm" (later more broadly as a "disciplinary matrix") throughout his corpus. Bringing a set of intertwining conditioning commitments for science under a single concept, though, did not enable Kuhn to provide a simple account. One reader calculated that in the *Structure of Scientific Revolutions* he used the word "paradigm" in at least twenty-two different ways. [13]

It is important to note in defense of Kuhn that he believed that the determination of theory choice is achieved by maneuvering among several significant scientific commitments that all interact with and affect each other. As such, a hard and fast separation between those factors that are conventionally understood to determine theory and hypothesis choice and those that have been excluded from it is no longer possible. Due to this, in his later work Kuhn begins to refer to a "disciplinary matrix" which includes theories, paradigms more specifically understood as exemplars, and the other factors I will discuss below. [14] As a result, Kuhn accepts without contradiction that the most binding allegiance when deciding to accept a particular theory is how well it fits with already accepted general scientific laws, concepts, and theories. For example,

[13] Kuhn cites a reader of his named Masterman in Kuhn, *The Structure of Scientific Revolutions.*, 181, cf. Margaret Masterman, "The Nature of a Paradigm," in *Criticism and the Growth of Knowledge* ed. Imre Lakatos and Alan Musgrave (Cambridge; New York: Cambridge University Press, 1974).
[14] Kuhn discusses this shift in language in the 1969 Postscript to *The Structure of Scientific Revolutions*, p.182.

Newton's Laws set puzzles and limited acceptable solutions for physical scientists in the 18th and 19th centuries. In this category he includes symbolic generalizations, like F = ma, what were once the laws of fixed and definite proportions in chemistry, and Maxwell's equations.

So far, the content of Kuhnian paradigms is consistent with the traditional positivist understanding of what guided scientific research before Kuhn. Kuhn then argues that a paradigm also determines the type of education and training that provides status and legitimacy to someone as a scientist. Different paradigms recognize as legitimate different types of people, engaged in different types of practices. While in his initial essay, Kuhn is a bit brief with this category, it ironically becomes the only one with the rightful name of "paradigm" in his postscript. There, he specifies that he means "the concrete problem-solutions that students encounter from the start of their scientific education, whether in laboratories, on examinations, or at the ends of chapters in science texts." [15] These "paradigms", through education and habituation, shape how scientists experience the world, which aspects of it they attend to, and how they, as a community, tend to connect symbolic generalizations to specific circumstances which they only imperfectly reflect. It is also this aspect which inducts scientists into paradigmatic "ways of seeing" the world, a turn of phrase that has earned Kuhn constant criticism since its formulation.

His next example of the influence of paradigms is that they shape the preferred scientific instruments and their employment. Kuhn discusses how the different instruments perceived as relevant to particular modes of research serve to condition the types of data gathered, in what form it is perceived, and how it constitutes evidence for

[15] Kuhn, *The Structure of Scientific Revolutions*., 187.

and against particular theories.   Conversely, choice of instrumentation negatively

determines which aspects of the natural world will fade into the background invisibly.   [16]

The final characteristics of science determined by particular paradigms are what

Kuhn labels "quasi-metaphysical commitments."   [17] He sees these commitments as both

metaphysical and methodological, in that they determine what the basic ontological

entities of the universe are (atoms, phlogiston, ether, matter and force, fields, etc.) as well

as the form of the ultimate laws and fundamental explanations which must result from its

method. Also methodologically, these commitments often determine what the basic

research problems in their domain must be. While the Aristotelian paradigm contained a

teleological world where one must inquire about final causes and forms, the mechanistic

paradigm of the 17th century only saw bodies in motion. In his postscript, Kuhn expands

this category to include heuristic as well as ontological models that supply scientists with

preferred or permissible analogies and metaphors.   [18]

Part of the reason that engagements with Kuhnian paradigms have caused so

much controversy and frustration is that his characterizations of them both overlap and

span different categories of explanation. While the commitment to endorsing new

theories that are consistent with already held theories, laws, and concepts concerns the

content of the theories under evaluation, the appeal to the education and training of

scientists appeals to the social *acquisition* of these theories in a specific cultural context.

Further, the "quasi-metaphysical" commitments of paradigms concern the

---

[16] This point is made by feminist theorists as well, most notably by Donna Haraway in her article " Situated
Knowledges": "There is no ummediated photograph or passive camera obscura in scientific accounts of
bodies and machines; there are only highly specific visual possibilities, each with a wonderfully detailed,
active, partial way of organizing worlds." Donna Haraway, "Situated Knowledges: The Science Question
in Feminism and the Privilege of Partial Perspective," *Feminist Studies* 14, no. 3 (Autumn1988)., 583.
[17] Kuhn, *The Structure of Scientific Revolutions*., 41
[18] Ibid., 184.

methodological and ontological *presuppositions* of theories, and these often determine

the appropriate instruments and their use, which he refers to as a different category. The

feminist theorists and the case studies that I utilize in the remainder of this dissertation

will hopefully help tease out a more cogent picture of how these different levels of

explanation interact with one another.

But no matter how diverse the characterizations and examples of paradigms in

Kuhn's work, the important upshot is that logic and direct empirical evidence are by

themselves *insufficient* to either describe or prescribe theory choice, and various other

factors, *many of them social*, are necessary for the actual choices that are made in

scientific communities.   So far, this is merely a descriptive claim. Whether the social

factors also justify these choices is a question that will be addressed by this dissertation.

In the *Structure of Scientific Revolutions*, Kuhn is also famously ambivalent in his

use and explanation of the *incommensurability* between paradigms. The interpretation

that Kuhn sees paradigms as incommensurable and thus undermines the empirical

accountability of science can find direct roots in his text. Kuhn makes the claim that

there is no sense of comparing paradigms in terms of their ultimate verification and

falsification by the facts ("all historically significant theories have agreed with the facts,

but only more or less."   )[19]Beyond the empirical facts which by themselves are not

decisive, Kuhn provides three general reasons for incommensurability between

paradigms. First, scientists within different paradigms will disagree about the list of

problems that science is aiming to solve, which in turn depends (at least in part) upon the

types of ontological entities and relations presumed to exist and be in need of

explanation.

[19]Ibid., 147.

Second, in new paradigms, "old terms, concepts, and experiments fall into new relationships one with the other ."[20] Kuhn concludes this from the fact that at least part of what constitutes a scientific concept is connected to that concept's explanation, and by challenging the explanation of certain important concepts, like saying space is capable of curvature or the earth is capable of movement, challenges the underlying meaning of the concepts of space and earth in the first place. As a result, "[c]ommunication across the revolutionary divide is inevitably partial."[21]

These two aspects of paradigms lead Kuhn to his most famous references to incommensurability.

> In a sense I am unable to explicate further, the proponents of competing paradigms practice their trades in different worlds. . .Both are looking at the world, and what they look at has not changed. But in some areas they see different things, and they see them in different relations one to the other. . .Just because it is a transition between incommensurables, the transition between competing paradigms cannot be made a step at a time, forced by logic and neutral experience. Like a gestalt switch, it must occur all at once (though not necessarily in an instant) or not at all[22].

All this would seem to lend credence to the more radical and irrationalist interpretations of Kuhn. The first main exegetical point about Kuhn that I want to make is that Kuhn's position, at least in its literal state, asserts no more than that traditional empirical testing is not sufficient to determine unique substantive verifications or falsifications of scientific theories and hypotheses, for these require the guidance of paradigms. The professed goal of his work, in spite of the popular uses to which it is often put, was not to reject philosophical discussions of what scientists "should" do nor undermine the scientific endeavor itself, but rather to make the traditional image of

---

[20] Ibid., 149.
[21] Ibid., 149.
[22] Ibid., 150.

science, both philosophical and commonplace, more compatible with what scientists

"actually" do.

Kuhn, both here and elsewhere, qualifies his incommensurability statements, both

by talking about "partial" incommensurability, and also by referring to "internal values"

like consistency, simplicity, accuracy, fruitfulness, etc. that have served as shared norms

for theory-choice across paradigms.   [23] He points out that although these overlaps and

shared norms are not sufficient to determine which theory is unequivocally better, they do

constrain choice in the realm of the rational.

While this account does introduce non-empirical factors in theory choice, it

importantly does not argue that they displace or are incompatible with empirical factors.

Kuhn does believe that there is a sense in which scientists can compare paradigms against

each other *and* the facts. Acknowledging that social structures can affect both the

questions and answers provided by scientific communities can appear to open the door to

those who want to undermine the objectivity of the scientific enterprise. How Kuhn and

others shed light on this issue is crucial to my aim in this dissertation, where I hope to

establish that a consistent account of scientific practice is possible that both accepts that

social beliefs and dynamics affect the questions and answers of science, and that these

questions and answers can remain empirically and conceptually criticizable.

What this account consists of, though, Kuhn leaves significantly vague. In the

next few chapters, I will undertake an extensive examination of the hindrances to

criticism that face science, understood in light of Kuhn's insights about

underdetermination and paradigms. Several feminist theorists analyze these dynamics

with an eye to defending the empirical and self-critical character of science, and I will

[23] I will discuss these values in more depth later in the chapter.

specifically engage important representatives of three powerful ways of doing this in the

next two chapters: Lynn Hankinson Nelson, Alison Wylie, and Helen Longino. These

thinkers will provide both arguments and tools to conceptualize how paradigms and the

empirical accountability of scientific theories can coexist. As all three thinkers whom I

will examine are committed to this possibility, I will label them "feminist empiricists."

The feminist empiricists, as well as many other theorists, have worried that the Kuhnian

paradigms can serve to undermine the status of empirical appeals in scientific research,

and thus challenge the criticizability of scientific claims and the rationality of science

altogether. As Helen Longino interprets Kuhn,

> . . .the paradigm so determines the context of assessment that one's perception of the world changes with the theories one adopts in such a way that one sees it as confirming the theory. This creates a bond between evidence and hypothesis impossible to break and even destroys, ultimately, the concept of evidence as something to which one can appeal in defending a hypothesis.[24]

Lynn Hankinson Nelson interprets Kuhn similarly.

> In the end, neither the "logic" of science, nor an attempt to test "hypotheses" by observation, nor the world generates and shapes scientific knowledge or determines the outcome of the revolutions. . .There are no standards of evidence to which one can appeal that are not paradigm bound and community relative.[25]

Alison Wylie, too, interprets the implications of a strong Kuhnian view as incompatible with empiricism in the context of her examinations of archaeological theory.

> if paradigms are sufficiently all-encompassing that testing is unavoidably paradigm-dependent—"locked in"—then empirical evidence can only be used to refute the commitments of one paradigm when interpreted in the terms afforded by an alternative paradigm. On the strong form of Kuhnian contextualism that Binford and Sabloff affirm, evidence, qua interpreted experience, cannot provide a neutral, extra-paradigmatic standpoint from which to judge the adequacy of competing sets of presuppositions about "the way the world is }.[26]

---

[24] Longino, *Science as Social Knowledge : Values and Objectivity in Scientific Inquiry*., 57.

[25] Lynn Hankinson Nelson, *Who Knows : From Quine to a Feminist Empiricism* (Philadelphia, Pa.: Temple University Press, 1990)., 76

[26] Alison Wylie, *Thinking from Things : Essays in the Philosophy of Archaeology* (Berkeley: University of California Press, 2002)., 121.

I will specifically engage these three thinkers in this dissertation because instead of choosing one horn of the Science Wars dilemma from a Kuhnian starting point, they all attempt to provide an account of how the scientific dynamics brought out by Kuhn are consistent with an empirically answerable scientific community. Whether or not these thinkers are right about Kuhn, their interpretations of his thought propelled them to explicitly demonstrate how paradigms do *not* have the power to determine evidence. Ultimately, I believe that their accounts have the capacity to explain how paradigms can be understood to be consistent with empirical appeals. As we will see in the next chapter, the feminist empiricists that I will engage argue in various ways *for* the criticizability of the categories and preconceptions brought to the empirical practice of science. They are therefore outside of the "Science Wars" uptake of Kuhn that dominated his literature for some time, and delayed a more fruitful engagement with his insights.

Another reason for engaging feminist empiricists and not other epistemologists, science studies, etc. is that these thinkers also have an explicit awareness of, and the conceptual tools with which to engage Kuhn's most innovative insights regarding the social hindrances to scientific criticism, which I believe have been largely neglected in other fields. Kuhn's account of incommensurability is not the only, and not even the most important, of Kuhn's arguments that science is resistant to criticism and change. The Science Wars, the semantic focus, and many other historical and theoretical circumstances have caused a preoccupation with the problem of incommensurability, and Kuhn's other arguments are often neglected. In addition to his account of paradigms and their implications for incommensurability Kuhn sets out the numerous ways that he finds

that scientific *communities* resist criticism which are also acknowledged in various ways by the feminist thinkers that I will engage.

My ultimate thesis is that although these feminist thinkers largely deal with the challenges which incommensurability pose to science's criticizability, the specific ways that scientific communities can socially block criticism have yet to be adequately engaged. Until this engagement is made, the objectivity of the scientific endeavor, seen as a social activity, is still at risk. I will now turn to these neglected insights.

Active Social Resistance: Social Entrenchment

According to Kuhn, scientific communities within paradigms are often both actively and passively resistant to challenge. In his later work Kuhn invokes this ambivalent dynamic whereby paradigms both make scientific research, seen as a critical enterprise, possible but also ultimately limits its ability to criticize itself as science's "essential tension." But even earlier, in *The Structure of Scientific Revolutions*, Kuhn brings up the conservatism of science several times. In the "Introduction", he refers to the activities of science within paradigms as an "attempt to force nature into the conceptual boxes supplied by professional education", the education which I have indicated is responsible for the exemplars, laws, rules, and methods of accepted scientific research. As a result of this dynamic, Kuhn argues controversially that even the success of the scientific enterprise derives from the scientific community's willingness to suppress "fundamental novelties because they are necessarily subversive of its basic commitments." [27]

Although Kuhn refers to and provides numerous historical examples of the resistance of scientists within a paradigm to novel hypotheses and theories that would

---

[27] Kuhn, *The Structure of Scientific Revolutions*., 5.

undermine that paradigm, he does little to schematize or analyze these types of resistance.

A paper written by Bernard Barber that was published by the journal *Science* at almost

the same time as Kuhn's *Structure* does attempt this analysis. In his article, "Resistance

by Scientists to Scientific Discovery", Barber defines his project as "an investigation of

the elements within science which limit the norm and practice of 'open-mindedness.'" [28]

Barber points out that when scientific resistance to criticism has been noted in the

past, it is often attributed either to the "human" aspect of scientists (as if somehow being

human and being open-minded are mutually exclusive) or else attributed to merely

psychological characteristics of human personalities. Kuhn, for example, appeals to a

psychological experiment with playing cards that indicates that conditioned expectations

can provide resistance to the awareness and acceptance of novelty. [29] While not

necessarily drawing a strong connection between the psychological experiment and

scientific practice, Kuhn does argue that "[i]n science, as in the playing card experiment,

novelty emerges only with difficulty, manifested by resistance, against a background

provided by expectation." [30] While Barber concedes a psychological dimension, he wants

to include in his explanation the social and cultural characteristics of both the scientific

community and the larger communities in which it belongs.

Likewise, although Kuhn invokes a psychological explanation for resistance, he

also urges understanding science in terms of a community of inquirers and finds

community paradigms responsible for how novelties and challenges are addressed (or

---

[28] Bernard Barber, "Resistance by Scientists to Scientific Discovery," *Science* 134, no. 3479 (1961)., 596. The limitation of open-mindedness in the social structure of scientific communities will become directly relevant in the discussion of Longino in Chapter 3.

[29] Kuhn, *The Structure of Scientific Revolutions*., 62-3. These insights were also discussed earlier by Norwood Russell Hanson in Norwood Russell Hanson, "An Anatomy of Discovery," *The Journal of Philosophy* 64, no. 11 (1967).

[30] Kuhn, *The Structure of Scientific Revolutions*., 64.

not). He argues that "[t]he very existence of science depends upon vesting the power to choose between paradigms in the members of a special kind of community." It is as quintessentially a community practice that scientific solutions, to be acceptable, must not only satisfy an individual scientist but must be accepted as a solution by at least a majority of the community.

Many of Barber's types of resistance are reflected in, and consistent with, Kuhn's account. In fact, while Barber's article came out one year before *The Structure of Scientific Revolutions*, he appeals to Kuhn's previous work on Copernicus to define the first source of scientific resistance. Kuhn argues in *The Copernican Revolution*, as well as later in *Structure*, that there was much resistance within the astronomical community during Copernicus' time to abandoning the substantive concepts and scientific worldview based upon the stability of the earth. Just as Kuhn argued that the substantive theories, laws, and concepts of paradigms set puzzles and limit acceptable solutions to scientific inquiry, these same aspects have been responsible for the resistance of scientists to alternative or significantly novel observations, hypotheses, and theories.

Also reflecting Kuhn, Barber indicates that shared methodological conceptions, another dimension of a Kuhnian shared paradigm, are responsible for resistance to scientific discoveries. Barber uses the example of the Baconian antitheoretical tradition in science. As a result of this tradition of methodological proclivities, discoveries based upon highly mathematical procedures, like Mendel's and Einstein's, were deemed suspect. The converse bias against direct observation and intuition has also prevailed in science at times during its history.

In addition to these Kuhnian insights about scientific resistance, Barber also argues that patterns of social interaction among scientists, which usually serve to advance inquiry in terms of precision and puzzle-solving, also are capable of producing resistance to robust novelty and discovery. Barber indicates two major sources of this resistance, professional standing and professional specialization. As one category of this phenomenon, he notes how professional standing in scientific communities, which is usually a reflection of skill, experience, and expertise, can be used to dismiss discoveries by those with lower social standing. Professional authority is capable of providing means to bypass careful consideration and justified rejection of hypotheses. Novelties can be dismissed based not on their merits or demerits, but rather due to either the status of their espousers or the motivations of those with authority to prevent anyone from treading on their heels.

Sometimes it appears to be just an intrinsic quality of age itself that people become more resistant to change. Lavoisier, made famous for his discovery of oxygen, referred to this dynamic at the end of his memoir.

> I do not expect my ideas to be adopted all at once. The human mind gets creased into a way of seeing things. Those who have envisaged nature according to a certain point of view during much of their career, rise only with difficulty to new ideas. . .Meanwhile, I observe that young people are beginning to study the science without prejudice.[31]

Barber provides many examples of these phenomena, from theories being rejected due to their suggestion by those outside the scientific community, like Mendel, or those merely low on the scientific totem pole, like Huxley and the 19th century mathematician Niels Henrik Abel.

---

[31] Lavoisier, *Reflections on Phlogiston*, 1785; cited by Barber, "Resistance by Scientists to Scientific Discovery.", 601.

Professional specialization can lead to resistance to discoveries when innovative "outsiders" to a field of specialization are viewed with skepticism and sometimes even disdain for attempting to affect fields that are not "their own." Hemholz, as a medical scientist, was thus treated when he initially set forth his theory of the conservation of energy to the physics community. Conversely, Pasteur's germ theory met with resistance because he was *not* a medical specialist. This type of resistance appears to rest on the opposition by experts to epistemically recognize those they do not consider experts. [32]

Kuhn, too, observes that challenges to paradigms most often come from young scientists and outsiders. He observes that "[a]lmost always the men (sic) who achieve these fundamental inventions of a new paradigm have been either very young or very new to the field whose paradigm they change."   He[33] later supports this claim with examples. He notes that Galileo was able to perceive a swinging stone as a pendulum, and not as a restrained falling body, due to the fact that he was not raised completely as an Aristotelian. [34] He also utilizes the example of John Dalton's revolutionary theory of chemistry. Kuhn argues that Dalton was able to reconceive chemistry because he came at it, not from within the accepted chemistry paradigm at his time, but rather as a meteorologist examining a physical phenomenon, and it was due to this different paradigm background that he found a fundamental problem with and alternative to the research models of chemistry. [35]

If the discoveries generated by scientific communities can often arise from those precise locations where those same communities are most resistant to finding and

[32] We will find more examples of this in Chapter 4, where the dispute over who has expertise about the new AIDS virus led to the uncritical insulation of various views. (reword)
[33] Kuhn, *The Structure of Scientific Revolutions*., 90.
[34] ibid, 119.
[35] Ibid, 133.

recognizing anomalies, ie. from "outsiders" and those low in social esteem, it appears that there is an "essential tension", not just between the conservative and innovative dimensions of science, but also in the social dynamics of scientific communities. This tension cannot be theorized away as part of scientific "bias" or a concession to the "humanity" of scientists, nor relegated to a "context of discovery" because the aspects responsible, like scientific authority and specialization, are also intrinsic parts of what constitutes science as a unique and effective practice.

Kuhn appears to be aware of this tension, and he responds to it, not entirely plausibly, by arguing that it is specifically those aspects of scientific practice, which he collectively calls "normal science" which are both responsible for exclusions and resistances to novelty, but also enable anomalies to be found.

> That professionalization leads, on the one hand, to an immense restriction of the vision and to a considerable resistance to paradigm change. . .On the other hand, within those areas to which the paradigm directs the attention of the group, normal science leads to a detail of information and to a precision of the observation-theory match that could be achieved no other way. Without the special apparatus that is constructed mainly for anticipated functions, the results that lead ultimately to novelty could not occur. And even when the apparatus exists, novelty ordinarily emerges only for the man who, knowing *with precision* what he should expect, is able to recognize that something has gone wrong.[36]

Not only does Kuhn argue that the precision and restrictions of normal science are a condition for anomalies to be found, he argues that the resistance to novelty itself has a positive relationship to scientific discoveries. He believes that this guarantees that scientists are not "lightly distracted" and that the anomalies that are recognized will thus be all the more significant.   [37]

Kuhn's argument that more precise expectations allow scientists to notice what has gone wrong is a bit surprising in the context of his earlier references to the inverse

---

[36] Kuhn, *The Structure of Scientific Revolutions*., 64-5.
[37] Ibid, 65.

relationship between expectations and the ability to perceive anomalies. In addition, his correlation of the qualities of immersion in normal science, such as narrowed vision and strongly directed attention, appear to conflict as well with his observation that it is often the young and the outsider who are responsible for noticing and proposing novel scientific ideas. Young scientists and outsiders are ostensibly those who are the *least* steeped in the detail, precision, and narrow focus that comes from years of experience within one paradigm-guided research program. This suggests that while the qualities of normal science may have many positive effects for science in general, they seem to have an almost entirely negative influence on the possibility and success of criticizing paradigms, in spite of Kuhn's protestations.

Tacit Knowledge and Passive Resistance: Epistemic blindness

If we reject Kuhn's response to the scientific community's social resistance to change, what other responses are available for us? Before responding to this question, there is one more important component of Kuhn's account that has direct bearing on the problem of recognizing and responding to claims that may undermine or challenge the content of existing paradigms. This is the problem of what Kuhn calls "tacit knowledge." [38] I call this a problem, while Kuhn does not, because I am interested in this phenomenon as it specifically bears upon the ability of scientists to recognize and respond to challenges to their preconceptions and categories. And insofar as tacit knowledge relates to this ability, it is, I will argue, a problem.

Kuhn brings up tacit knowledge in *The Structure of Scientific Revolutions* in the context of arguing for a distinction between the paradigm-guided activities of normal

[38]Kuhn takes this term from Michael Polanyi's discussion in chapters v and vi in Michael Polanyi, *Personal Knowledge* (Chicago: University of Chicago Press, 1958).

science and rule-governed activities such as games. Invoking Wittgenstein's arguments

in the *Philosophical Investigations*, Kuhn argues that the research problems and

techniques that constitute normal science under a paradigm are not related by a set of

explicit rules and assumptions, but rather are connected by family resemblance, shared

models, and shared practices. When asked, scientists most often cannot articulate the

basic presuppositions under which they are working, although they are able to employ

them on a regular basis. Thus, for Kuhn "[p]aradigms may be prior to, more binding, and

more complete than any set of rules for research that could be unequivocally abstracted

from them." [39]

In the Preface to *The Essential Tension*, Kuhn further explicates this point by

addressing a common criticism brought against his account of normal science in

Structure. There he argued that normal science presupposes a consensus among

members of a scientific community. But when, as a historian, he attempted to specify

those paradigmatic elements upon which members of a community were in agreement,

Kuhn found that those elements, such as concept definitions and categories "were seldom

taught and that occasional attempts to produce them often evoked pronounced

disagreement." [40]

Kuhn did not respond to this problem by abandoning his notion of normal science,

but rather realized that scientists were not taught concepts and their relations directly.

Rather, they are taught problem-solving practices and examplars in which those concepts

figured and functioned in specific ways. He compares this exemplar-baased process to

the process of language acquisition, where children are initially taught to conjugate

[39] Kuhn, *The Structure of Scientific Revolutions*., 46.
[40] Thomas S. Kuhn, *The Essential Tension : Selected Studies in Scientific Tradition and Change* (Chicago: University of Chicago Press, 1977)., xvii-xix.

exemplary verbs, and then apply that standard form in order to form other verb declensions that they are not directly taught.

Of particular note is Kuhn's observation that the implicit knowledge that makes normal research possible is never problematic until times of crisis in a science. "To the extent that normal research work can be conducted by using the paradigm as a model, rules and assumptions need not be made explicit." [41] Kuhn characterizes "crises" as periods of pronounced professional insecurity, where the puzzle-solving techniques that characterize the paradigm of normal science are consistently failing to adequately address anomalies. He later refers to it as "the common [scientific] awareness that something has gone wrong." [42]

In this way, Kuhn integrates tacit knowledge into his account as a necessary aspect of scientific practices without thereby seeing its dynamic as threatening to it. The paradigm becomes explicit when it needs to be explicit, namely in periods where the background commitments of science are brought into question and challenged. But like Kuhn's attempt to utilize the extant social qualities of normal science to maintain the possibility of change in the face of social entrenchment above, he makes a similarly problematic conservative move in the face of epistemic blindness.

Kuhn claims that it is only when normal science enters a period of "crisis" that science becomes reflective and attempts to formulate and criticize the presuppositions that guide its engagement with the world. If not for these times of crisis, much of the web of commitments that bind a community together would remain unconscious and unrecognized.

[41] Kuhn, *The Structure of Scientific Revolutions*., 88.
[42] Ibid., 181.

But Kuhn also admits that the puzzle-solving techniques of a paradigm never (or at least rarely) unequivocally "fail" to address an anomaly, for the underdetermination argument tells us that there are always multiple ways to make theories fit the world, and scientists within a paradigm will "devise numerous articulations and *ad hoc* modifications of their theory in order to eliminate any apparent conflict." [43] Kuhn gives several examples in the history of science in which anomalies that ultimately will be the basis for a new paradigm are successfully integrated into the old one for long periods of time.

According to Kuhn, the notable exceptions to the incorporation of anomalies are cases of "gross failure, or repeated failure by the most brilliant professionals." [44] The latter case significantly invokes the issue of professionalization brought up in the last section. It is not trivial that the repeated failures that avoid ad hoc revisions to the existing paradigm are connected to the "most brilliant professionals", because if less respected or esteemed members of the scientific community encountered repeated failure, it would more easily be attributed to their lack of skill or experience, not to a conflict within the accepted paradigm. That Kuhn admits to the possibility of gross failure, on the other hand, is noteworthy given the prevalence of later critiques that often accuse Kuhn of denying that empirical data can ever serve to undermine a paradigm. While he does not seem to find this case likely, it is, obviously, a possibility for him.

Thus, it is rarely, if ever, that the mere existence of anomalies can undermine a paradigm. Kuhn argues that in order for anomalies to cause a crisis in a paradigm, they must point the way to a new paradigm, a new set of background commitments,

---

[43] Ibid., 78.
[44]Kuhn, *The Essential Tension : Selected Studies in Scientific Tradition and Change*., 273.

techniques, and puzzles. "If, therefore, these epistemological counterinstances are to constitute more than a minor irritant, that will be because they help to permit the emergence of a new and different analysis of science within which they are no longer a source of trouble." [45]   In other words, crisis-causing anomalies must be perceived simultaneously as intrinsic problems with the old paradigm and as potentially a part of a different set of models, assumptions, and puzzles about the world.

For scientists within a paradigm to recognize these two moments of crisis appears on the above account to be nearly impossible, for quite a few reasons. In the last section, the discussion of social entrenchment brought out the intrinsic resistance which normal science communities have towards recognizing anomalies in the first place due to the conservative nature of social hierarchies. Challenges, often arising from below, are often overlooked, ignored, or interpreted as trivial to the existing paradigm rather than even entertained as a possible threat. As a result, crisis-instigating anomalies are impeded by the social structures of scientific communities.

With the observation that much paradigmatic content is implicit, we now have a further problem. If much of the content of paradigms is known only implicitly, then it seems like a paradigm crisis both presupposes *and* is a condition for the recognition of the implicit content of paradigms. It is only *after* normal science is in crisis that the implicit beliefs of paradigms are made explicit, and yet a crisis doesn't occur until there is a perceived relationship between the existing paradigm and the anomalies found in research. In order to imagine anomalies pointing to an alternative paradigm, one must have at least some awareness of some deep aspects of the current way of doing things that

[45] Kuhn, *The Structure of Scientific Revolutions.*, 78.

are contingent and debatable, and this presupposes that these deep aspects cannot be taken for granted or employed unconsciously.

So, according to Kuhn's view, it appears that paradigms are only seen as problematic after they are seen as problematic, or that a crisis is the condition for a crisis. As he notes in the Postscript, although it is possible for scientific revolutions to occur without a preceding crisis, a crisis is still a necessary notion, "supplying. . .a self-correcting mechanism which ensures that the rigidity of normal science will not forever go unchallenged." [46] If my argument adequately threatens Kuhn's notion of a crisis, then the self-correcting mechanism of science is also threatened.

It is clear that paradigms *are* in fact challenged in science, and broad shifts in techniques, models, and ontology do occur. But from this discussion, it appears that the normal practice of science, with its social resistance to novelty and discovery, combined with the implicit nature of many of its broader commitments, cannot easily account for these types of criticism and innovation solely appealing to the internal mechanisms of science, seen as the implementation of scientific method. If this is the case, then Kuhn is ultimately committed to an appeal to dynamics external to scientific method to account for the most significant scientific changes in history, and more significantly for the possibility of science's self-correction.

Now I do not expect that these arguments are sufficient to establish whether or not Kuhn *believed* that his account entail that scientific paradigms were ultimately uncriticizable. Kuhn's account of paradigms was too vague, and often inconsistent, to pin him down on such a philosophical commitment as necessity. Whether or not he is committed to the criticizability of paradigms, from the above discussion Kuhn's historical

[46] Ibid.

account of paradigm-driven science implies challenges to the criticizability of science in three important ways.

First, his talk of incommensurability between paradigms and "ways of seeing" determined by them leads to the threat of circular justification in any attempt to justify paradigms, or just the theories in them, with appeal to evidence. If all data becomes evidence through the paradigms of which they are a part, neither broad internal, nor significant external criticism is possible by appealing to empirical data. Thus, the empirical nature of science, one of its defining properties, appears to be undermined. The feminist theorists that I analyze in the next chapters all start out by acknowledging underdetermination and proceed to provide a much more detailed account of how "paradigms", or things that function like them, play a role in scientific communities and that are simultaneously amenable to scientific criticism.

The sociological aspects of scientific communities indicated by Kuhn and explicated by Barber point to two further problems. If the social structure of scientific communities leads to both active conservatism against significant challenges and passive resistance to perceiving significant challenges to paradigms in the first place, then criticizability is challenged once again. Kuhn's account, in spite of his analysis, implies that the best remaining avenues for criticism of paradigm commitments come from *outside* of the authoritative scientific community, but the scientific values and methods have traditionally insulated it from precisely these types of criticism.

Kuhn makes intimations in this direction in his book by discussing "values", both those he considers internal to science and those that are external to science. Kuhn explicates these internal values in his lecture, "Objectivity, Judgment, and Theory

Choice." He lists several of these values, such as simplicity, accuracy, predictability, and others that are traditionally held by the scientific community to be virtues of adequate theories. He considers these particular values internal to science because they are established by the consensus of scientific communities throughout their history (and significantly across paradigms). The failure to meet these values is considered adequate to bring about crises in science.

In spite of this, Kuhn still recognizes that social values and other historical factors, such as philosophical trends and political dynamics play a role in scientific change. But while Kuhn mentions these as playing a role in scientific change, he finds that they are "principally significant in determining the timing of breakdown, the ease with which it can be recognized, and the area in which, because it is given particular attention, the breakdown first occurs." [47] External values are given these inessential powers because Kuhn still holds that a crisis is ultimately the result of the internal recognition of problems from within a scientific community based on the values internal to scientific inquiry.

Not only does Kuhn deny socio-political values an essential (**later I will term this relationship constitutive**) role in bringing about crises in science, but he argues in *The Essential Tension* that the important distinction between revolutionary science and normal science is rooted in the corresponding amount of influence between these two phases and social needs and values.

Early in the development of a new field. . .social needs and values are a major determinant of the problems on which its practitioners concentrate. Also during this period, the concepts they deploy in solving problems are extensively conditioned by contemporary common sense, by a prevailing philosophical tradition, or by the most prestigious contemporary sciences.[48]

[47] Ibid., 69.
[48] Kuhn, *The Essential Tension : Selected Studies in Scientific Tradition and Change*., 119.

Kuhn contrasts this interpenetration of society and science in the revolutionary stages

with the autonomy of normal science from these influences.

> The practitioners of mature science are men (sic) trained in a sophisticated body of
> traditional theory and of instrument, mathematical, and verbal technique. As a result,
> they constitute a special subculture, one whose members are the exclusive audience for,
> and judges of, each other's work. The problems on which such specialists work are no
> longer presented by the external society but by an internal challenge to increase the scope
> and precision of the fit between existing theory and nature. . . In short, compared with
> other professional and creative pursuits, the practitioners of a mature science are
> effectively insulated from the cultural milieu in which they live their extraprofessional
> lives.[49]

This contrast is relevant to the later discussion as well, since the contrast between

immature and mature sciences for Kuhn parallel the conventional hierarchies of science,

with the physical sciences at the top, the human sciences at the bottom, and

biology/medicine occupying an embattled category in the middle. In this way, Kuhn was

still an inheritor of the logical positivist view that the best functioning sciences were

value-free (or at least free of social values). It is this presupposed boundary between a

value-free science and its surrounding societies that will be most challenged, both by the

feminist empiricists and ultimately by my account.

Summary

Philip Kitcher, in his article "A Plea for Science Studies", sums up the generally

accepted insights generated by Kuhn. He delineates these points in terms of four major

claims.

1) Science is done by human beings, that is, by cognitively limited beings who live
   in social groups with complicated structures and long histories.
2) No scientist ever comes to the laboratory or the field without categories and
   preconceptions that have been shaped by the prior history of the group to which
   he or she belongs.

[49] Ibid.

3) The social structures present within science affect the ways in which research is transmitted and received, and this can have an impact on intratheoretical debates

4) The social structures in which science is embedded affect the kinds of questions that are taken to be the most significant and, sometimes, the answers that are proposed and accepted. [50]

Kitcher argues that these points are "generally accepted" within an edited book of people particularly dismissive of social constructivists [51], so this implies that even a conservative audience should not take issue with them. Limited to these four points, I believe that Kuhn still has important insights to provide. I will limit my starting point to these four "uncontroversial" claims, hopefully in order to avoid joining sides in an argument that is beside the point of this dissertation.

Kuhn (1) saw scientific decisions as always preconditioned by background commitments that included metaphysical beliefs, problem-solving techniques, examplars, and research standards (Kitcher point 2). He brought all of these commitments together under the novel, and now well-known, term "paradigm." With the suggestion of the crucial role that paradigms play in scientific research, Kuhn also (2) opened the epistemic examination of science to the social analysis of science as a practice of scientific communities. Paradigms do not condition individual methodologies, but are defined as achieved by, and later influencing, scientists *as a community*.

> Traditional discussions of scientific method have sought a set of rules that would permit any *individual* who followed them to produce sound knowledge. I have tried to insist, instead, hat, though science is practiced by individuals, scientific knowledge is intrinsically a *group* product and that neither its peculiar efficacy nor the manner in which it develops will be understood without reference to the special nature of the groups that produce it.[52]

---

[50] Philip Kitcher, "A Plea for Science Studies," in *A House Built on Sand: Exposing Postmodernist Myths About Science*, ed. Noretta Koertge (New York: Oxford University Press, 1998)., 36.

[51] Ibid.

[52] Kuhn, *The Essential Tension : Selected Studies in Scientific Tradition and Change*., xx.

It is Kitcher's fourth point regarding the influence of social structures on the kinds of questions and answers provided by science that has led to so much controversy in the Science Wars. If social structures can affect the realm of possible questions and answers in science, then this invites the crucial challenge of whether this claim is compatible with the practice of a rigorous and self-critical scientific endeavor. My aim is to argue from this basis in Kuhn's account that the role of social structures in guiding science is compatible with rigorous and self-critical science, but only if governed by certain social values that are conducive to these scientific goals and interests. This is so, even if these values may seem to fly in the face of many traditional conceptions of how science is, and should be practiced.

Chapter 2: Feminist Empiricists Contribute____

Part I: Nelson and Wylie

Why feminist epistemologists?

In light of the relatively unexplored areas of social analysis I found nascent in Kuhn's thought in the last chapter, the reason why I find feminist theorists to be the best responders to Kuhnian problematics is that they have been the first, and still remain the most prominent, analysts of how social and cultural values play a role in scientific decision-making. Kuhn remains in the positivist tradition insofar as he characterizes mature science as value-free (that is, free of "external values" while having "internal values"). I identified some of the tensions inherent in this position invites in the last chapter. The feminist theorists that I will discuss in this chapter all attempt to explain how social values from the broader community in which the scientific community is located have played a role in the sciences, and further how these values are often incorporated into the "paradigms" that shape scientific practice.

There is an interesting commonality in the engagements of Longino, Nelson, and Wylie with Kuhn's accounts. While all three argue that paradigms do not entail incommensurability or circularities of justification, they have yet to engage the full implications of the social hierarchies and tacit assumptions for the criticism of scientific claims. While this is not a unique flaw among the contextualist or postpositivist epistemologies of science that followed Kuhn, what does single out these feminist theorists is that their backgrounds in feminist thought enabled and motivated them to recognize and argue that social values and assumptions have played direct roles in scientific communities. In spite of this background, I will argue that because they have

been mired in arguments to establish the rigor of feminist critiques in the face of philosophical and scientific resistance, their arguments are still not fleshed out sufficiently to deal with epistemic blindness and social entrenchment. This is an interesting fact, considering that addressing these two dynamics, especially as they have led to androcentric and sexist science practices, has been the distinguishing project of feminist theorizing about the sciences. Further, without further addressing these dynamics, the criticizability of science, in spite of all of their work on evidence, is still undermined. Thus, the ability to incorporate these insights into their accounts would be a crucial measure of how successfully they provide an account of both the reality and the appropriate norms of science.

Lynn Hankinson Nelson, Alison Wylie, and Helen Longino, as epistemologists, attempt to provide an account of scientific communities and practices that take seriously the underdetermination arguments; they all accept that evidence, as it has traditionally been construed, cannot determine or fully justify theory choice. Nelson is a direct inheritor of Quine's underdetermination arguments and holism, and both Wylie and Longino invoke underdeterminism arguments in their accounts. As feminist theorists, they also presuppose that the social dynamics and values of scientific communities and the larger society are intrinsic to the production of scientific knowledge, instead of seeing science as merely the abstract and individualistic application of a method independent of those who apply it.

Nelson, Wylie, and Longino represent three different ways that feminist epistemologists attempt to make these insights consistent with the empirical and normative standards of science. These thinkers take up the complex project of providing

an account of the relationship between social values and beliefs and the behavior of scientific communities that Kuhn subsumes under the concept of a paradigm. But unlike Kuhn's account, these thinkers engage the project with an eye to establishing how value-infused scientific commitments remain capable of criticism by evidence.

They each do this in different ways. Nelson, inspired by a Quinian holism, expands the notion of evidence to include both value-laden experiences and the broader metaphysical and methodological theories that inform scientific practices. Wylie attempts to address the value-laden nature of scientific practice by incorporating the social dynamics between different disciplines and research approaches to check the values that inform separate empirical approaches. Longino goes further to incorporate specific social norms to guide scientific communities so that they can engage questions about their background beliefs. In spite of these differences, they all share the goal of providing a normative account of how science can achieve its empirical ideals while remaining loyal to descriptive accounts of how values actually play a role in scientific practice. Due to this shared goal, I will label all three of these theorists as feminist empiricists. [53]

Nelson, Wylie, and Longino also challenge the Kuhnian-inspired view that there is a barrier between scientific communities and the social world in which those communities exist. Each of these thinkers has been informed by and is a participant in feminist critiques of science, but in quite different fields. These critiques have brought

---

[53] In her book *The Science Question in Feminism*, Sandra Harding divided feminist theorists into three categories: feminist empiricists, feminist standpoint theorists, and feminist postmodernists. While this tripartite view has been heuristically helpful in some ways, her definition of feminist empiricists fails to account for the dialogue that they have had, and continue to pursue, with the insights of the other types of feminists, and vice versa. As such, when I use the term "feminist empiricist", it only refers to the commitment to general empirical answerability. As we shall see, the complexity and diversity of these three thinkers belie any further easy equations or contrasts. Sandra G. Harding, *The Science Question in Feminism* (Ithaca: Cornell University Press, 1986).

out the multifaceted ways in which social assumptions about gender (among other social assumptions) that are a part of the larger societies of which science is a part have found their way into the content and methods of scientific research.

These challenges to the boundary between science and politics are a direct challenge to the Kuhnian belief that mature scientific communities, while cultural and political themselves, are insulated from the social, political, economic, and cultural contexts outside of them. [54] While Kuhn urged that we examine science as a practice, he believed that this practice, at least in its quintessential form, was insulated from the values of its cultural milieu. Feminist theorists, on the other hand, are attuned to both the detrimental and facilitating role that these "contextual" values play in the "constitutive" structure and practice of science throughout history. While feminist epistemologists take important steps beyond Kuhn with both their broader socializing move and their analyses of how paradigms are empirically criticizable, I will argue that their empirical accounts thusfar are unable to address the two final problems with which Kuhn was left in the last chapter: epistemic blindness and social entrenchment. They are therefore not able to ultimately reconcile the claim that evidence matters with the implications of the sociological dimensions of their analyses. Ironically, I believe that the move towards incorporating social values as constitutive of science will ultimately provide the avenue through which these problems can be addressed.

The hindrances to criticism posed by epistemic blindness and social entrenchment, as shown in the last chapter, go beyond the paradigm as an abstract set of

---

[54] As we saw in the last chapter, Kuhn does acknowledge interpenetration in the case of revolutionary sciences, but denies its necessity in the case of the mature sciences/natural sciences. Feminist critiques have found cultural values and beliefs to play an intrinsic role in the natural sciences as well, most famously in biology.

commitments and bring out the role and effects of the paradigm upon the activities and interactions of a community of scientists. Evidence that challenges paradigms is often excluded not only because it doesn't agree with the commitments of a paradigm, but also as a result of the social status of the individuals espousing the data vis-à-vis the scientific community, or else the unconscious level of the paradigms themselves. In other words, the problem is both the possibility of consciously recognizing recalcitrant evidence and socially recognizing recalcitrant persons.

These problems have been highlighted by those with feminist scruples like the feminist empiricists, since the social exclusion of women and prevalence of social assumptions in science have been primary issues in feminist analyses, but they have not connected it directly to the possibility of self-criticism in scientific research. In other words, while feminist theorists are successful at making the implicit *content* of paradigms criticizable by bringing to light the social exclusions and androcentric assumptions in science, their accounts are not ultimately successful at theorizing the process by which these criticisms are heard and heeded.

After demonstrating how the feminist empiricist suggestions are helpful but ultimately insufficient in this chapter and the next, I will argue that by taking their insights and accounts further by looking at the social dynamics *between* scientific communities and lay communities that the feminist could ultimately resolve these intractable problems.

Lynn Hankinson Nelson

 Nelson and the broader evidence of a broader epistemic community

Nelson incorporates both the Kuhnian sociological insights and the feminist political insights into a holistic account of science that she claims incorporates its value-ladenness while remaining empirically answerable. [55] Nelson defends the compatibility of these two insights by making two conceptual moves that she believes can deal with the many tensions that exist between these two positions. Like Kuhn and the other feminist empiricists, Nelson has to address how social values and the metaphysical models, methods and theories informed by them are related to the evidential reasoning that marks scientific method, because ostensibly these views cannot be directly checked "against the world."

Nelson first argues for a reconceptualization of evidence in light of the underdetermination arguments of Quine and the socio-political insights brought out by feminist philosophers of science. Instead of the positivist view that evidence is composed of individual observation sentences directly surmised from the world and independent of our values and other theories about it, she denies this possibility and replaces it with two alternative species of evidence.

The first, following Quine, consists of observation sentences, but this time they are considered pre-structured by existing scientific theories and standards of measurement and observation. Quine's version of underdetermination argued that an observation sentence only makes sense in the context of the entire body of theory or theories that inform it. Nelson goes on to argue that not only scientific theories, but also our societal beliefs and values, inform our observations of the world. Sometimes she

[55] {Nelson, 1990 #93}Lynn Hankinson Nelson, "Empiricism without Dogmas," in *Feminism, Science, and the Philosophy of Science* ed. Lynn Hankinson Nelson and Jack Nelson (Dordrecht ; Boston: Kluwer Academic Publishers, 1996).

refers to these metaphysical and methodological theories informed by social beliefs and values as those that broadly constitute a "theory of nature."

The second, and more controversial, type of evidence *is* the body of accepted methods, standards, and theories and the values that inform scientific observations. [56] This type of evidence also includes the range of beliefs, like normative convictions, they have arisen from outside of science. Nelson is making the point that observation sentences are not only *meaningful* in light of broader theories and claims in science, but they are also *justified* the more they accord with them. The content of this second type of evidence is analogous to those aspects of scientific practice Kuhn subsumes under a "paradigm." But while Kuhn saw these elements as *presupposed* by questions of evidence, Nelson wants to analyze these elements also *as* evidence.

This is both a creative and conservative way of dealing with the problems posed by paradigms, for if the beliefs that loosely correspond to Kuhn's paradigms can be considered evidence themselves alongside value-laden observation statements then they all can be understood as of a piece and are open to criticism through the same avenues. This would efficiently solve the problems posed by underdetermination arguments and the social influences in science without causing major interruptions in the standards and ideals of science as it is already practiced.

Nelson contrasts her view with those that construe evidence narrowly as observation sentences and relegating other theories and broad methodological and metaphysical commitments to "background" or "auxiliary" status. As we shall see in the next sections, the use of "background beliefs" is found in Helen Longino's work, while Alison Wylie makes appeal to auxiliary hypotheses in her account. Nelson believes that

[56] From now on, I will refer to this as "Type 2 evidence".

these foregrounding and backgrounding moves are unnecessary because her broader account of evidence is able to assess and compare competing research programs and theories directly. Thus, I will evaluate Nelson's account, and its avoidance of the conceptual moves that Longino and Wylie make, on these terms. But before this evaluation can be made, Nelson's other major innovation must be understood.

In addition to broadening and deepening the category of evidence, Nelson's account requires a shift in the locus of evaluation of scientific practices. As Nelson puts it, ". . .one question this account of evidence immediately raises is how broad a body of theory and results needs to be considered in the assessment of a specific research program or theory." [57]Nelson recognizes that science is not a homogenous system, and different fields and disciplines are informed by different theories and methodologies (and social values).

To answer this question, Nelson urges that the scope of relevant theories and results be delineated by the scope of the scientific community. "The appropriate loci of philosophical analysis of science are science communities, with the standards, theories, and practices of such communities the appropriate loci of philosophical explanations and evaluations of scientific practices." [58] Instead of taking the claims of scientists *qua* individuals as the locus for analysis, Nelson argues that the standards held collectively by scientific communities should hold a place of epistemological primacy. Thus, the scope of relevant theories and social beliefs with which to assess a given research program or hypotheses is coextensive with those held by the relevant scientific communities

[57] Nelson, "Empiricism without Dogmas.", 101.
[58] ibid

involved. Determining which scientific communities are relevant, according to Nelson, is

a local matter depending on the specific issue.

Nelson argues that since, on a Quinian analysis, metaphysical commitments are

continuous with empirical commitments, and on her own further analysis, the evaluation

and criticism of both can be prompted and include social beliefs and values, then

"feminist criticism of current metaphysical commitments must be *within* science, broadly

construed." [59]While Nelson italicizes the fact that feminist analysis is "within" science,

the crucial novelty in Nelson's approach is that science is "broadly construed." For

Nelson, all of our beliefs are interlaced, and a "scientific community" is characterized by

its activity of "constructing theories and naming objects, organizing experiences,

discovering relationships between our ways of organizing and explaining experience,

subjecting beliefs and claims to critical examination, and paying attention to evidence." [60]

Insofar as feminist theorists are participants in this type of activity, as so are,

loosely, many of our commonsensical ways of dealing with the world, then they are all

"science", and part of an overarching "scientific community." [61] They are thus all in

principle able to criticize each other, and the metaphysical and theoretical commitments

involved are all, more or less, subject to different types of criticism within this

overarching community. Nelson's philosophy of science can be interpreted as an

epistemic democracy where one need only be self-conscious and self-critical about the

empirical adequacy of one's cognitive commitments to be a scientific citizen with an

epistemic vote.

---

[59] Nelson, *Who Knows : From Quine to a Feminist Empiricism.*, 311.
[60] Ibid., 315.

Nelson attempts to demonstrate this possibility in her article "Empiricism Without Dogmas" by applying it to a case study. She considers Norman Geschwind and Peter Behan's 1982 study which concluded that the source of correlations between left-handedness, immune-system disorders, and mathematical abilities is exposure to testosterone *in utero*. Testosterone exposure was claimed to be responsible for brain lateralization in human males, which in turn is responsible for the correlations.

Geschwind and Behan empirically supported their conclusions from various different directions. They appealed to several studies that found left-handedness, immune-system disorders, and mathematical abilities much more predominately in men and boys. Additionally, they referenced studies of assymetries in human fetal brains, as well as a study that reported that male rat brains are assymetrical (favoring the right side) while female rats do not share this trait. This particular study was supported by the ability to reverse the symmetry in female rat brains by removing their ovaries at birth.

By reconceptualizing evidence and shifting the locus of epistemic analysis, Nelson believes that her account provides the theoretical possibility for value-laden research programs and theories to be assessed on the basis of their evidential warrant. If her account is successful, she will have shown how critiques of the value-laden standards, methods, and theories behind scientific observations (like those behind Geschwind and Behan's study) are susceptible to trenchant criticisms, such as those provided by feminist theorists. This would be a great stride forward. Nelson will have shown how value-laden research programs, like the studies of brain lateralization, are rationally criticizable, ie. by appeals to refuting evidence, and also how value-laden critiques, like feminist analyses,

do not thereby lose their credibility because it is ultimately the evidence, not the politics, that lends credence to their claims.

In the 1980's, feminist biologists intensely criticized Geschwind and Behan's hypotheses linking brain lateralization and mathematical abilities to testosterone. Nelson's account of the status of the feminist critiques of this experiment is significant because according to her account these criticisms must be "within science", meaning that their arguments against the research program must be based upon traditional epistemic norms like evidential warrant and explanatory power. Otherwise, these criticisms would lose their epistemic status vis-à-vis the scientific community, and there would be no good account for why the rest of the scientific community needed to heed them. Further, it would undermine Nelson's claim that her account can make sense of the possibility of a self-critical and empirical, yet value-laden science.

Like Kuhn, Nelson's account of value-laden science must grapple with the problems of social blindness and social entrenchment, for both of these have a bearing on the empirical criticizability of scientific claims. Taken at face value, Nelson's case study would seem to demonstrate that these two issues were not problematic in the process of the feminist biologists' critiques of the study. Some feminist critiques of Geschwind and Behan's study reflect traditional empirical ways of challenging scientific claims. Feminists pointed out that the position set out by Geschwind and Behan was not able to identify causal mechanisms connecting testosterone to left-hemisphere development, as well as many particular criticisms of illegitimate inferences from observations in the studies cited by Geschwind and Behan. As such, it is straightforwardly "bad science" that these scientists did not respond immediately to the feminists' empirical challenges to

their program, and this case need not indicate anything more trenchant about the

standards and norms of scientific research. While these criticisms can be seen as

straightforward challenges to the sufficiency of evidence and the explanatory power of

the theories, several other challenges provided by feminist biologists are not so easily

subsumed under these categories.

In addition to the evidential criticisms which attack the observations and

inferences of the study, feminist biologists also challenge the aspects of the study that fall

under Nelson's Type 2 evidence; the larger metaphysical and methodological

commitments, often informed by social values, that lent support to the results of the

study. I will quote Nelson's account of these critiques at length and underline those

terms that indicate the commitments at issue.

> . . .feminist biologists criticized the <u>emphasis</u> on the organizing effects of androgens and challenged various hypotheses concerning their effects, pointing to continuous conversions of some forms of sex hormones to others as presenting difficulties for both. They also challenged the linear explanatory <u>model</u> organizer hypothesis <u>presumed</u>, and in particular the extrapolation of the model to humans, citing experimental results indicating complex and often non-linear interactions between cells. . .[62]

> Many questioned the <u>rationale for looking</u> for a biological foundation for sex differences alleged given that a <u>substantial</u> body of research documents <u>significant</u> differences in relevant socialization, that differences among males and among females appear to be more <u>significant</u> than the differences between these groups, and that studies claiming to establish such differences typically <u>assume</u> that gender is a <u>sufficient</u> variable and use <u>criteria</u> for cognitive abilities that are themselves controversial.[63]

As my underlining demonstrates, these feminist arguments disputed the larger

standards of significance, models, and research questions, all aspects of the community-

endorsed "theory of nature" aspect of evidence. The emphasis on researching sex-

identified factors, the significance of sex differences over other differences, and other

---

[62] Nelson, "Empiricism without Dogmas.", 105.
[63] Ibid

methodological choices reflect certain social values and interests that while necessary to

shape the research project are not values intrinsic to research in general.

Nelson points out that many different research traditions shared the assumptions,

emphases, and models that support Geschwind and Behan's study, including

neuroendocrinology, reproductive endocrinology, empirical psychology, and

neuroanatomy. Thus, in spite of the feminist critiques of the social beliefs presupposed

by the research, Nelson argues that in the first few years after it was published, the study

was evidentially justified.

> It might not be reasonable to expect endocrinologists in the 1970's and early 1980's to
> know and consider sociological studies suggesting alternative explanations for the sex
> differences alleged, to know of the critiques offered by psychologists of research into sex
> differences in cognitive abilities and lateralization, or even to know of the several levels
> of critiques offered by feminist colleagues in biology.[64]

But time marches on, and Nelson argues that although it was not "reasonable" to

expect uptake of feminist criticisms in the 1970's and early 1980's, "relevant studies and

critiques were sufficiently publicized by the late 1980's to make it reasonable to expect

those pursuing biological explanations for sex differences to show why such explanations

constituted or were likely to constitute better explanations than [those provided by

feminist biologists]."   [65] From this understanding of changes in the scientific climate,

Nelson concludes that while Geschwind and Behan's study was "good science" when it

came out, informed by social values yet evidentially warranted, by the late 1980's those

who still held it in esteem without further justification were practicing "bad science."

Taking into account both the fact that the feminist biologists were arguing against

the Type 2 evidence of the study and the fact that Nelson is committed to Type 2

[64] ibid, 109
[65] ibid

evidence as *justifying* the research that presupposes it, Nelson is faced with a problem.

Feminist biologists had to struggle for some time to have their criticisms heeded. At the

times when their criticisms first began to be articulated, they were in tension with the

Type 2 evidence at the time, namely the scientific community's accepted body of

research, standards, and theories, and as a result, lacked *justification* in terms of them.

She therefore commits to a time-indexed factor of evidential warrant (my term, not hers).

The standards accepted by scientific communities evolve over time, and at the time when

the study was published, the "evidence" (type 2) that the feminists challenged was in

accord with the rest of the community, and thus the study was warranted. Only as these

background theories and social values changed did the feminist critiques simultaneously

become justified challenges to the study.

Nelson's tensions and lessons

Although Nelson provides an account of science that expands the notion of

evidence to incorporate social values and also expands the idea of science from an

individual to a communal practice, her lack of distinction between background beliefs

and explicit evidence, and the absence of an account of the institutional relationship

between scientists and feminists prevent her from resolving the same problems that Kuhn

faced. Those problems I have defined as social blindness, or the scientific resistance to

subjecting unacknowledged metaphysical/methodological presuppositions to criticism,

and social entrenchment, or the resistance of the scientific community to criticisms from

beneath or outside its boundaries.

Nelson's account cannot adequately grapple with social blindness because her

understanding of Type 2 evidence treats explicit and tacit scientific commitments in

exactly the same way. Feminist analyses not only criticized the social values and methodological/metaphysical beliefs informing Geschwind and Behan's research like the emphasis on research sex-identified factors and the significance of sex differences over other differences, they brought many of those implicit beliefs to light. Before their analyses, widely assumed beliefs that biological accounts of behavior are primary or that sexual dimorphism is automatically significant are considered evidence on Nelson's account because these claims strengthened and sometimes even filled in the inferential chains from observations to conclusions in Geschwing and Behan's research.

One problem with understanding corroboration between disciplines as another line of evidence is that it does not account for the possibility that agreement is not the result of independent verification, but rather the result of shared, unidentified, and entrenched assumptions. Nelson's account provides no tools to distinguish independently verified "evidence" from "evidence" uncritically appropriated by various disciplines from the larger sociopolitical contexts that they share, like androcentric ones. While Nelson considered Geschwind and Behan's study "good science" when it came out due to its "independent" agreement with other scientific disciplines, the feminist critiques (at the time unheeded) demonstrated that this agreement lent them a legitimacy that was not established.

The fact that Type 2 evidence is shared between disciplines often reflects epistemic blindness in the scientific community to the existence of these beliefs and provides them with a sense of validation (since "we all agree") that they have not rightfully earned.   The "time indexed" aspect of Nelson's account of evidence disguises

the fact that many of the targets of the feminist critiques were unquestioned premises, and this is partially why these critiques were resisted.

In other words, while considering Type 2 beliefs as evidence makes sense as a rational reconstruction, it requires a complete abstraction from the circumstances and the commonsense use of the word "evidence" to see many of these views as playing a evidential role in any actual research. In order to be considered evidence, not only the *content* of beliefs must be examined, but also the *role* that these beliefs play (or more specifically do not play) in the explicit reasoning in the research. On Nelson's own account, many types of Type 2 evidence, like social values and broader methodological orientations are not directly established through other evidence or theories, and the main argument for not directly establishing them is that they are also presupposed by other scientific disciplines. Whether understood as something that conveys proof or something that is visible or obvious ("to make evident"), beliefs that are unconscious or unacknowledged fail to satisfy either of these conditions.

Under the circumstances of this study, Nelson's Type 2 evidence most often played a role in the research implicitly and unconsciously (until the feminist critiques), and cannot be considered evidence without doing considerable violence to the term. By considering "theories of nature" as just another evidential source, Nelson elides the difference between the mutually reinforcing values among shared presuppositions that exist between "independent" scientific disciplines on the one side and the values behind the criticisms of those presuppositions set forth by the feminist scientists on the other.

Nelson's account also does not adequately deal with the role that social entrenchment played in the delay of the uptake of feminist critiques. That feminist

critiques of Geschwind and Behan's study took so long to be incorporated into the literature was not merely the result of the quality of their arguments, but also due to the socio-political dynamics at play in the late 70's and 80's. Nelson argued that these arguments were "sufficiently publicized" by the late 80's to then required recognition and reconciliation with Geschwin and Behan's work. Achieving sufficient publicity is not just a simple state of affairs of accumulating more evidence, but is also the result of the successes of the feminist movement itself, along with the social changes that had affected the biological community specifically. During those years, feminists were able to gain access, and subsequently recognition and clout, within scientific communities, and only *then* were their arguments available to be heeded by those responsible for gatekeeping in the scientific literature. Like we saw in the Kuhnian analysis of the last chapter, "outsiders" (in this case feminists) lacked recognition and were often dismissed by the higher eschelons of the scientific community until changes in the social dynamics occurred.

Before these changes, feminist analyses were often dismissed as politically motivated, and therefore automatically in violation of scientific standards of neutrality and objectivity. They were therefore considered unscientific at the outset because set forward by "feminists", and this dismissal came prior to the evaluation of the evidential warrant or conceptual integrity of their arguments. Furthermore, as "political", traditional assumptions about the value-neutrality of science presupposed that any project explicit about its political stake, like feminist analysis, was disqualified from evaluation.

Nelson invokes the direct comparisons of competing research programs (including the methodological, metaphysical, etc. commitments that inform them)

without theorizing the social changes necessary to make these comparisons possible. Without recognizing the role that these dynamics played in the states of affairs crucial to her account (most significantly the delineation of scientifically recognized arguments and voices), her account cannot adequately provide for the possibility of scientific criticism in these types of cases.

In sum, Nelson crucially argues for broadening our understanding of knowing, first from individuals to communities, and next from scientific communities to the epistemic activities of nonscientific communities as well. She wants to consider feminist critiques as part of science since they participate in the activity of "constructing theories and naming objects, organizing experiences, discovering relationships between our ways of organizing and explaining experience, subjecting beliefs and claims to critical examination, and paying attention to evidence." [66] While participation in this activity makes them "scientists" for Nelson, she does not adequately address the tension between the theoretical practice and the social recognition of "being a scientist." It is not only time and insufficient evidence that has delayed feminist biologists from having their arguments heard. Nelson does not integrate the political dynamics at play in both preventing and enabling the uptake of feminist critiques within the scientific communities. For Nelson, what propelled the shift from considering Geschwin and Behan's study evidentially warranted to evidentially unwarranted was which social values and metaphysical theories were represented within the larger scientific communities and literature. But which views and values are represented depends directly upon whose arguments are allowed into the literature, pass peer-review, and are published in reputable journals. It was shown in the last chapter that these practices are

[66] Nelson, *Who Knows : From Quine to a Feminist Empiricism.*, 315.

thoroughly imbued with issues of social hierarchies, professional resistance to challenge, and other aspects of social entrenchment, in addition to the actual quality of the arguments and evidence at stake, and I will further demonstrate these dynamics in Chapter 5.

Even more problematically, Nelson leaves the relevant scientific communities that bear on a specific research program to be determined in local situations. Since it is the "theories of nature" shared by these communities that justify the approach and models of research, whose theories are dominant is a vital question. She also argues that the boundary between traditional scientific communities and the larger communities outside of it is a false one. ". . . an 'epistemological community', a community in which knowledge is constructed and shared, and experiences are possible and shaped, our largest community is, in every sense, a science community." [67] While I will ultimately incorporate this insight into my ultimate suggestions, at this stage it is problematically vague. The issues of epistemic blindness and social entrenchment within scientific communities motivates a more hands-on approach to drawing (and criticizing) the delineation of legitimate voices on given scientific questions.

Alison Wylie

Wylie and linking principles

Alison Wylie also takes seriously the insights of Kuhn, particularly in the context of archaeological research. Utilizing insights from Glymour's notion of bootstrapping evidence by using other hypotheses under a general theory, along with direct evidence to support a specific hypothesis in question, she articulates "linking principles" upon which the identification of archaeological data, as well as their constitution as evidence,

[67] Ibid., 314

depend. [68]   Linking principles include "source-side or background knowledge, middle-

range theory, or mediating interpretive principles."   Wylie [69] identifies the challenge of

capturing the range of interlinked considerations that bear upon the interpretation of

archaeological data. While not providing a definitive list, Wylie mines the examples of

many philosophical commentators and archaeologists for principles of linking data to

hypotheses. Some of these include; analogical reasoning, the fit of new hypotheses with

a conceptual core of established and background knowledge, and pragmatic

considerations in constructing typologies and other tools of analysis.

Whatever their specific nature in the local scientific context, Wylie denies that the

existence and power of linking principles in research leads to a vicious circularity

between archaeological theories and the data that supports them, which she labels the

"interpretive dilemma." Alison Wylie argues in various ways that the activity of "fitting

of experience into conceptual boxes" that Kuhn ascribes to paradigms, is indeed a

process, and an interactive one at that. By examining the details of the interaction

between the concept- and relationship-constituting activities of paradigms with each

other, and between paradigms and the world, it is possible to find several ways in which

the seemingly circular process is amenable to criticism and feedback. She finds these

ways by arguing that in practice, and for her interests in archaeological practice, there are

several strategies for limiting the paradigm dependence of evidence.

Wylie argues that circularity can be avoided through two major avenues. First,

she discusses the "security" of the linking principles. Linking principles can be

understood to be more or less secure for two different reasons. They can be secure in

[68] Clark Glymour, "Relevant Evidence " *Journal of Philosophy* 72 (1975).
[69] Wylie, *Thinking from Things : Essays in the Philosophy of Archaeology*., 174.

terms of their indubitability or "entrenchment" in the research fields from which they are drawn, which is dependent upon both the respective authority of the research fields and the degree of consensus regarding the principles within those fields. They can also be seen as secure in terms of the nature of the linking principles; how strong of a linkage is established by the principles, in terms of determinacy, universality, or uniqueness. Finally, linking principles gain security in terms of the number and complexity of principles required in order to connect the data to the theories. The simpler and more direct the linkages (ie. the less interpretation necessary), the more security is allotted to the principles. The possibility of error, and therefore doubt, increases with the number of inferential links required to connect the data to the theory.

The second avenue for avoiding circularity is the independence of the linking principles. Linking principles can be independent in three ways. The first way is what Wylie labels "vertical independence." Wylie argues that there is a possible independence in archaeology if there is a disruption in the vertical chain that runs through the "elements of a given data base, via claims about how these data may or must have been produced, to conclusions about their significance as evidence of some aspect of the cultural past."[70] The theories and assumptions that condition the collected data may be different from those that determine how the data was produced, which can likewise be different from those that evaluate the significance of the data for the answers to certain archaeological or anthropological questions.

[70] Ibid., 176.

Second, linking principles can be horizontally independent.   [71]   Rather than an

independence between certain types of data and their respective explanations, this

strategy involves the fact that research on the same data can involve several narrowly

circumscribed "paradigms" that interpret the data, rather than one overarching one. For

example, archaeological hypotheses regarding the time period ascribed to certain

collected data are justified both by theories about human cultural behavior at certain

historical times *and* by chemical dating techniques. While both the historical

anthropology theories and the chemical dating theories rest on background assumptions

as well, those background assumptions are different in the two different fields, and thus

the convergence of the theories on the same conclusions about the data serves to enforce

the archaeological inferences from the evidence to the hypotheses in a non-circular way.

Wylie gleans this point from her analysis of Binford's Nunamiut study, as well as

arguments made by Hacking, and concludes,

> Such independence serves to underwrite a localized miracle argument to the effect that it
> would be highly implausible that independent means of detection should converge if the
> body or structure under observation did not exist.[72]

This appeal to miraculous convergence emerges prevalently as an argument for scientific

realism and will come up again later, when we discuss another possible reason for this

phenomenon that undermines, rather than reinforces, the justification of the claims that

converge. As a result, distinguishing between the two possible sources of the same

phenomenon will be a key epistemological challenge facing us.

The third type of independence is the direct (although relative) independence of

linking principles from the data that they interpret. Wylie proceeds to argue that even

---

[71] With her account of horizontal dependence, Wylie expands her account of linking principles beyond the
limits of Glymour's bootstrapping, which presuppose the justification process under a single paradigm.
[72] Wylie, *Thinking from Things : Essays in the Philosophy of Archaeology.*, 176.

when there is only one paradigm providing the assumptions under which certain data is interpreted, the paradigm still does not have the power to explain away all recalcitrant data. She points to instances where anomalies are so prevalent that even though there was no way to interpret these anomalies within the existing framework, they could not be explained away either.

> Even in the areas covered [by the assumptions of a single paradigm], paradigms are often not so tightly determining of how we make sense of experience, and experience itself is not so plastic, that observation can be counted on to deliver all and only what the dominant paradigm dictates.[73]

We could inquire at this point how these points relate to the Kuhnian account presented in the last chapter. Wylie's final point contains echoes of the Kuhnian notion of crisis, where there comes a point in normal science in which gross and repeated failure can no longer be denied.[74] The other types of independence, as well as the issue of the security of linking principles, though, are either not mentioned or not perceived as significant in the Kuhnian account. Although Kuhn acknowledges that there are multiple paradigms at work simultaneously in the different sciences, and not one overarching one, he does not utilize this insight to mitigate the determining power of paradigms nor to allow them to serve as indirect influences on each other. Wylie, along with the archaeologists whom she invokes, does so, and thus demonstrates that although linking principles may not be directly criticized, they are not immune to scientific checks. Wylie admits that neither of these two strategies provide a single, comprehensive foundation for interpretation, but that the apparently circular justification of paradigms can be broken on a localized basis.

[73] Ibid., 124-5.
[74] Kuhn, *The Essential Tension : Selected Studies in Scientific Tradition and Change*., 273.

Ultimately, Wylie attributes a "mitigated objectivism" to the sciences. In a move that finds echoes in both Nelson and Longino's accounts, the response to the threats against the possibility of criticism brought about by a Kuhnian understanding of science as more than a pure methodology is resolved ironically by some of its key non-methodological aspects. For Wylie, "what makes it possible for archaeological evidence to 'resist theoretical appropriation' and thereby serve as a measure of 'relative' or 'particular and contingent' objectivity is precisely the *disunity* of the sciences.", ie. the sociological structure of science as broken into different and relatively independent disciplines.

Wylie's tensions

Although this is not the extent of Wylie's account, at this stage I want to address two important challenges that emerge from her account thusfar. As we saw in Nelson's account, Wylie here faces similar problems with relying solely on a more complex account of evidence to address the threat of circularity between theories and evidential support. First, several of the examples of linking principles, like the examples of paradigms that Kuhn presents, are not accessible to direct empirical justification or criticism. Specifically, some of what Kuhn would call the "quasi-metaphysical" linking principles serve as general models and analogies for research that are both widespread common sense assumptions and are also largely implicit and vague. Wylie herself gestures toward some of these issues in an extensive footnote where she refers to discussions of "commonsense conventions about cultural phenomena. . .which have never been clearly articulated, much less systematically assessed, but which seem highly

questionable when made explicit." [75] She cites anthropologist Kluckhohn (1940) as

providing examples of such assumptions as "cultural stability; the mechanics of

diffusion; relations of race, language, and culture; poly- and monogenesis; and the

like". [76]

The important question is how to distinguish between these types of entrenched

commonsense views and those that manifest the virtue of "security" on Wylie's account.

She defined security above in terms of both the authority of the scientific field that

endorses the linking principles and the consensus within those fields in regard to those

principles. As we saw in Barber's account in the last chapter, scientific authority within

fields, and the authority of certain scientific fields over others, is often the result of power

structures and conservative commitments, rather than the result of better-justified science.

Thus, if the security of linking principles is based upon the authority of the fields that

endorse them, and this authority is based upon certain methodological or sociological

biases, then security serves to insulate underexamined views, rather than legitimate better

supported ones. I am not arguing that this is always the case, but the fact that there are no

theoretical resources to prevent it and also several historical cases that manifest this

dynamic, I am arguing that it is a problem to be dealt with.   [77]

This leads to the second problem, which is that the convergence of independent

evidential paths may be a necessary condition for establishing that the justification of

---

[75] Wylie, *Thinking from Things : Essays in the Philosophy of Archaeology*.
[76] Ibid.
[77] Wylie articulates two strategies with which to respond to this problem. First, she argues that source-side critiques, like those of feminists or race critical theorists, can point out implicit linking principles. Second, she urges that scientists pay careful attention to those situations where horizontal lines of evidence fail to converge. The first suffers from the same insufficiency as Nelson's account of feminist critiques in biology, namely an account of how these voices become recognized and taken up by scientific communities. The second is helpful, but is often elided due to the "miraculous convergence" that I will discuss momentarily.

evidence isn't viciously circular, but it is not sufficient to rule it out. In order to be sufficient, there must exist a safeguard that the "miraculous convergence" of evidence on a certain hypothesis or theory is not the product of shared assumptions in different fields guiding the selection and emphasis of evidence. As Nelson's example above illustrated, several "independent" fields, like neuroendocrinology, reproductive endocrinology, empirical psychology, and neuroanatomy, shared significant assumptions like sexual dimorphism, which served to insulate and support the studies that perpetuated theories presupposing those assumptions. In an analogous dynamic to the problem with Nelson, the problem of epistemic blindness remains untouched by the independence criterion that Wylie provides. This does not mean that these resources are not fruitful, but that she needs a further account for how this dynamic can be prevented from undermining her account of science as a fundamentally empirically answerable enterprise.

While I agree with Wylie's defense of a more nuanced view of the workings of evidence in the "disunified" sciences, her account also does not protect against a larger circularity when assumptions are shared and assumed across different disciplines. In the same way as Nelson's nuanced account of evidence that included values and broader networks of commitments could not protect against social entrenchment of views in high standing in the scientific community, neither does Wylie's.

Like Nelson, in addition to her challenges, Wylie also introduces what I believe to be a fruitful pathway from which to address the problem of social entrenchment, but she does not take it far enough. Nelson and Wylie's accounts were intended to open a space for scientific and philosophical consideration of the role of social and historical values in science. As such, they conservatively articulated their arguments in terms of evidence

and current scientific practices. In her work on feminist critiques of archaeology, Wylie engages the historical fact that many archaeological background commitments and linking principles had never been clearly articulated, and they did not become articulated until feminist critiques articulated them. The emphasis of Wylie's argument is to indicate the feminist critiques and archaeological projects, although politically informed and value-laden, are nevertheless also supported and challengeable by evidence. Her account of linking principles and the disunity of the sciences serves to support her view that there is nothing intrinsically incompatible between underdetermination arguments and science as an empirical (and socio-political) enterprise. Thus, like Nelson, feminist critiques were both justified *and heeded* by scientific communities due to their empirical nature.

While Wylie is right to argue that, once articulated, these "commonsense conventions" and the feminist alternatives are both value-laden and susceptible to evidential debate, I want to focus on the conditions for these conventions to be articulated in the first place. In other words, even if there was not found to be adequate evidence to undermine the "man-the-hunter" model in archaeology, the project to provide adequate evidence for it was not possible until it was exposed as an implicit presupposition as opposed to a justified premise.

In her book, Wylie points out the various ways in which feminist critiques exposed the bias of archaeological data.

Erasure: choice of research problem or determination of significance systematically directs attention away from certain kinds of subjects—namely those that might challenge the tenets of a dominant ideology or might be particularly relevant to the self-understanding of subordinate and oppressed groups.

Distortions: systematic, manifestly interested (standpoint-specific) distortion in how various archaeological subjects are understood

Political Resonance: patterns of congruence b/w the interests of large-scale geopolitical elites and entrenched archaeological research programs

Politics of objectivism: political "neutrality" of science sustains and legitimates existing order

Explanatory critiques: how the internal conditions of scientific practice -the micropolitics of scientific communities—shape the direction and results of inquiry[78]

While Wylie does acknowledge the importance of the feminists for the possibility of these types of critiques, the successes and challenges of feminist critiques also have implications for the standards and practice of science. Like Nelson, Wylie appeals to and analyses the content of feminist critiques of science as empirically valid, in this case in revolutionizing archaeology. Presuming these critiques are empirically valid, or at the very least potentially so, I want to emphasize and analyze the socio-political circumstances necessary in order for those feminist archaeologists to have their critiques heard in the first place.

Wylie approaches this type of analysis in her paper "Gender Theory and the Archaeological Record: Why is there no Archaeology of Gender?"[79] There she asks why feminist critiques of archaeology arrived so late on the scene in comparison to the analogous feminist critiques that emerged in the other social sciences (like anthropology and history). After dismissing the possible excuse that archaeological methodology is less amenable to feminist critique, Wylie begins to engage the effects of the women's movements of the 60's and 70's. These movements had two important, and interconnected, effects. First, they empowered women and affected hiring and

---

[78] Wylie, *Thinking from Things : Essays in the Philosophy of Archaeology*.
[79] Alison Wylie, "Gender Theory and the Archaeological Record: Why Is There No Archaeology of Gender?," in *Engendering Archaeology: Women and Prehistory*, ed. Joan M. Gero and Margaret W. Conkey (Oxford: Blackwell, 1991).

educational practices in such a way that increased the number of women in the social sciences.

Second, Wylie points out that "the crucial condition for the initial development of feminist critiques in fields like anthropology and history is not that women are directly accessible as subjects, but rather that the researcher is prepared to see them *as* subjects." [80] This did not require a developed gender theory, but rather "a conceptual framework that raises the relevant questions, directing attention to gender and providing the impetus to study women's activities and experiences." [81] This process of "adding women", both as scientists and to archaeological investigations, showed to be insufficient to alter the more general analyses coming out of the archeology while it did cause change in these other fields. But why?

Wylie shows how the influx of politicized women in the social sciences advocated this shift in focus and archeology as a specific field increased in gender diversity, but a significant recognition and incorporation of these insights was significantly delayed. Much like her more abstract analysis in *Thinking from Things*, Wylie points to the role of implicit and entrenched linking principles that served to insulate archeology from the content of feminist critiques.

> . . .[G]ender structures that are common in the West (or in a select range of enthnographic contexts) are unreflectively projected onto archaeological subjects as stable, uncontroversial features of the cultural environment in the context of reconstructive and explanatory arguments about the cultural past.[82]

Since it was presumed that gender roles were biological and stable, gender was seen as irrelevant to cultural changes through time. This is a case in point of the erasure of a

---

[80] Ibid., 32.
[81] Ibid.
[82] Wylie, "Gender Theory and the Archaeological Record: Why Is There No Archaeology of Gender?.", 34.

critique on the basis of background beliefs. Since Wylie argued above that feminist

critiques specifically focus on the erasure, distortions, political resonance, etc. of linking

principles, the fact that these same dynamics are used to account for the inefficacy of

feminist critiques is significant. Wylie recognizes that these entrenched assumptions can

often be immune to feminist empirical critique.

> Although feminist critiques have demonstrated unequivocally that such 'essentialist'
> assumptions about gender - hallmarks of contemporary sex role ideology - are
> unsustainable empirically, often on the very evidence collected by those most intent on
> affirming them, they continue to influence even those social and life sciences in which
> feminist critiques have a strong presence.[83]

This indicates that the focus on the empirical adequacy of feminist critiques, while

perhaps responding to the historical charges that they are only political, is often

insufficient to justify them in the light of the scientific community. In addition to the role

of strong assumptions about gender, broader framework assumptions about research

paradigms in archaeology direct explanatory attention toward system-level processes and

away from internal, local, structuring principles like gender. Wylie points out that it

should be an "empirical question" whether a given society is primarily determined by

systemic or internal variables.

There are two important things to note from Wylie's account of why archaeology

has not responded to feminist critiques in a timely fashion. First, as the last part of her

discussion shows, these assumptions are made and perpetuated *a priori*, so that whether

or not they are empirically assessable (or justified as secure linking principles) is not even

on the table. Only if this were the case could they be maintained in the face of

recalcitrant evidence.

[83]Ibid., 34.

Second, it is important to notice that while Wylie is inquiring into what makes the field of archaeology persistently resistant to feminist critique, she answers the question with appeals to implicit assumptions and methodological frameworks that are *not* unique to archaeology. She specifically refers to anthropology as another field where empirically grounded feminist critiques fail to undermine the influence of these assumptions. This connects to my critique of the sufficiency of the "independence" criterion above. Since the static nature of gender has been presupposed (unreflectively) throughout the social and life sciences, it is difficult to see how this could not be considered a "miraculous convergence" as opposed to the reinforcing bias that it is. Difficult to see, that is, unless feminist critiques have an epistemic status distinguishable from the other avenues through which linking principles and assumptions about gender are challenged and perpetuated.

In an ironic twist, the solution to Wylie's problem can be found at the outset of her argument. It is not the empirical access to information about women as subjects, but rather "that the researcher is prepared to see them *as* subjects" that is the problem. For [84] archaeologists to see women as subjects requires them to remedy both the epistemic blindness (collective assumption that they cannot be relevant objects of knowledge) and social entrenchment (authoritative scientists and authoritative frameworks that presume that gender is irrelevant to the determination of the subjects of knowledge).

Wylie and Feminist Standpoint theory

Wylie explains this dynamic by engaging feminist standpoint theory. As she points out in her article, "Why Standpoint Matters," "[s]tandpoint theory offers a framework for explaining how it is that, far from automatically compromising the knowledge produced

[84] Ibid., 32.

by a research enterprise, objectivity may be substantially improved with certain kinds of situated non-neutrality on the part of its practitioners."   [85]   Objectivity, notes Wylie, is often used equivocally between several meanings, but in this context she is arguing that it is a property of knowledge claims, designating a family of epistemic virtues like empirical depth, empirical breadth, internal coherence, etc. Traditionally, the neutrality of the epistemic agent has been taken to be a measure of the objectivity of that agent's claims, but standpoint theory argues, and Wylie agrees, that this connection is tenuous at best. For Wylie, "under some conditions, for some purposes" observer neutrality is conducive to objectivity, while under others an interested standpoint may be more conducive.

Arising from traditional Marxism, the prominent epistemic insight of standpoint theory is that social positions of power (including the power bestowed upon the scientific community vis-à-vis the layworld and the power bestowed upon those with scientific status within the scientific community) tend to provide a limited, and as a result distorted, knowledge of the world. [86] Sandra Harding, a prominent (if controversially representative) feminist standpoint theorist, defines the central tenets of this approach to knowledge.

1) Knowledge and power are internally linked: power relations play a constitutive role in producing knowledge, and knowledge plays a constitutive role in power relations.

2) Any body of systematic knowledge is always internally linked to a distinctive body of systematic ignorance: systematizing and categorizing, by selecting and focusing upon certain elements and relations, simultaneously causes other elements and relations to be hidden from view (not merely excluded).

---

[85] Alison Wylie, "Why Standpoint Matters," in *The Feminist Standpoint Theory Reader : Intellectual and Political Controversies*, ed. Sandra G. Harding (New York: Routledge, 2004)., 348.
[86] While Dorothy Smith's standpoint argument from sociology which came out first {Smith, 2004 #56}, it was Nancy Hartsock's Marxist articulation of standpoint theory that was initially widely publicized.{Hartsock, 1983 #18}

3) Standards of objectivity, rationality, and good method should be strengthened so that they are competent to distinguish those values and interests that contribute to systematic ignorance and those that contribute to advancing the objectivity of knowledge.

4) In order to strengthen the standards of objectivity, we must "Start out thought from everyday lives of people in oppressed groups in order to identify. . .otherwise obscured features of dominant institutions, their cultures, and their practices"[87]

It is important to notice the status of each of these claims. The first two are descriptive claims. The second directly parallels the discussions of the feminist empiricists about underdetermination and emphasizes the issue of implicit background assumptions (systematic ignorance). The first claim reflects the challenge that Nelson faced, and Longino will be seen to face in the next chapter; namely, the dynamic whereby power differences (in scientific hierarchies and between the scientific community and society) play a direct role in determining whose claims are recognized and considered "knowledge." The third and fourth claims are normative. The third is somewhat vague, but I take it to be the point that scientific values should be responsive to the dynamics of scientific communities insofar as they affect scientific objectivity in terms of the criticizability of claims and the perpetuation or elimination of bias.

The final point is the one that often invites standpoint theories' harshest criticisms. In an inversion of traditional epistemology, this claim urges a methodological directive that privileges the epistemic subject conditioned by the social experience of oppression. As many people have pointed out, the directive to "start out thought" from the lives of those in oppressed positions is highly problematic for several reasons. Those

[87] Sandra Harding, "Rethinking Standpoint Epistemology: What Is "Strong Objectivity"?," in *The Feminist Standpoint Theory Reader : Intellectual and Political Controversies*, ed. Sandra G. Harding (New York: Routledge, 2004). Harding came under scrutiny by other feminist standpoint theorists for articulating standpoint theory as a general epistemological claim about knowledge, rather than as a directive regarding how to approach research.

who take it as an epistemological requirement, as opposed to a contingent and useful

approach, face the charge of establishing in a general way the epistemic advantage of this

norm. More broadly, even as a directive Helen Longino argues that it faces the problem

of identifying social positions in order to privilege them. But women, among other

oppressed people, usually occupy several social positions in a racially and economically

stratified society. Feminist standpoint theorists have been accused of homogenizing and

essentializing women in order to establish a singular social position to privilege.

Wylie points out that another problem standpoint theory must avoid is conferring

an automatic epistemic privilege upon people. Even if oppressed positions could be

isolated, there is the challenge that the beliefs of those people in the "same" social

position are often not the same. The automatic identification of a critical edge with

oppressed social locations needs to address the fact that many oppressed people not only

do not engage critically with their environment, but many also embrace it.

In spite of these challenges, the crucial insight in relation to my dissertation that

standpoint theorists acknowledge is that the epistemic challenges of scientific

communities cannot be remedied without engaging the interaction between power

hierarchies and scientific discourse.

In her further account, Wylie wants to hold *both* that the situated knowledge and the

political commitments of these feminist archaeologists made their critiques possible, *and*

that their critiques themselves were evidentially answerable and evidentially superior to

the accounts that they ought to replace. I agree with her on both counts, but I do not

believe that she adequately incorporates the full implications of standpoint theory that she

engages for her commitments to empiricism. The role of a marginalized standpoint (in

this case that of feminized women), was not only locally helpful for formulating alternative views about society, but more globally necessary to bring assumptions to light, and thus making empirical evaluation possible.

Wylie argues as much in her article by saying, ". . .standpoint theory may make a difference to what we know and how well we know it. These include access to evidence (sometimes including background and collateral evidence); inferential heuristics that confer particular skill in disembedding empirical patterns; an expanded range of interpretive and explanatory hypotheses for making sense of evidence; and, often a condition for the rest, critical dissociation from the taken-for-granteds that underpin authoritative forms of knowledge." [88]

This is where Wylie's invocation of standpoint theory can serve her more than she seems to recognize. What makes the feminist critiques and analyses of archaeological data distinctive is that they are *both* empirically grounded *and* derive from a standpoint outside of the sciences. It is this outside/marginalized standpoint that enables a "critical dissociation from the taken-for-granteds that underpin authoritative forms of knowledge." [89] This epistemic privilege that marginalized standpoints have due to their ability to enable the possibility of empirical criticism, when juxtaposed with the epistemic bias (or what Miranda Fricker calls an "epistemic injustice") against precisely those same perspectives, leads us to ask what implications this should have for our scientific norms of objectivity.

This may not be a disagreement with Wylie but rather a shift in emphasis. For Wylie:

[88]Wylie, "Why Standpoint Matters.", 346.
[89] ibid.

> My thesis is that although such examples make it clear that the standpoint of practitioners affects every aspect of inquiry—the formation of questions, the (re)definition of categories of analysis, the kinds of material treated as (potential) evidence, the bodies of background knowledge engaged in interpreting archaeological data as evidence, the range of explanatory and reconstructive hypotheses considered plausible, the array of presuppositions held open to systematic examination—they also demonstrate that standpoint does not, in any strict sense, determine the outcomes of inquiry." [90]

Wylie is interested in *defending* standpoint theory against the charges of evidential circularity. On the other hand, my thesis is that some form of standpoint theory is *necessary* in order to prevent evidential circularity in science.

While neither Nelson nor Wylie have ultimately provided an account of how value-laden and sociologically inflected science (ala Kuhn) can address epistemic blindness and social entrenchment, their analyses have shown us where we need to look. The possibility to criticize background beliefs requires both the critical content cultivated by marginalized standpoints (like feminism) to combat epistemic blindness and the institutionalized social recognition of those standpoints to combat social entrenchment. Nelson's account showed us that we need to understand scientific knowledge more broadly in terms of the social values that inform it, and this often requires looking outside of the "scientific community" for the source of these values. Wylie introduced the possibility of using standpoint theory to delineate and justify the voices relevant to scientific criticisms. The remaining gap in the account is how to resolve the problems faced by standpoint theory. How can we delineate which standpoints are relevant without reifying groups or automatically privileging them? I believe that the tools to answer this question can ironically be found by engaging a feminist empiricist who is sharply critical of standpoint theory: Helen Longino.

[90] Alison Wylie, "The Engendering of Archaeology: Refiguring Feminist Science Studies," *Osiris* 12, no. Women, Gender, and Science: New Directions (1997)., 96.

Chapter 3: Feminist empiricists contribute: Part II

Helen Longino

From paradigms to background beliefs

Like Nelson and Wylie, Helen Longino endorses the epistemic arguments for

underdetermination and theory-laden evidence as well as the feminist insights that values

have and must play an intrinsic role in the sciences. Her accounts begin by urging a shift

from the Kuhnian language of "paradigms" to the language of "background beliefs." To

a certain point, these two terms are coextensive. Longino's arguments for the necessity

of background beliefs are almost identical to those of Kuhn. She begins from the logical

gap between data and hypotheses established by the underdetermination arguments. How

one determines the evidential relevance of data to hypotheses in science, she continues,

depends upon one's other beliefs about the world that determine which principles of

inference to apply to any given situation. Without a commitment to these other beliefs,

"a given state of affairs can be taken as evidence for the same hypothesis in light of

differing background beliefs, and it can be taken as evidence for quite different and even

conflicting hypotheses given appropriately conflicting background beliefs." [91] At first

blush, this sounds like the Kuhnian claim from Chapter 1 that paradigms cause different

scientists to see the same experiment (state of affairs) in completely different ways

(evidence for different hypotheses/theories).

Further, Longino appeals to some of the same historical instances to contrast the

role of different sets of background beliefs constituting evidence as Kuhn used to contrast

competing paradigms. In *Science as Social Knowledge*, Longino speaks of how

---

[91] Longino, *Science as Social Knowledge : Values and Objectivity in Scientific Inquiry.*, 43.

Lavoisier and Priestley, examining the same experimental information, derive different observations and conclusions. While Kuhn referred to this example as a situation of a scientific revolution of paradigms, Longino understands it as an example of researchers with differing background beliefs. So if Longino and Kuhn utilize the same examples and arguments to establish the necessity of these background commitments, what makes "background beliefs" different from "paradigms"?

The difference immediately becomes clear when Longino asks, "Upon what is the acceptance of the background beliefs operative in the contexts discussed based? What sorts of criteria are relevant to deciding between different or competing (sets of) background beliefs?" [92] If, as Longino points out, the same states of affairs can be evidence for different and sometimes conflicting theories within different paradigms, it may appear that there is no possible answer to these questions. But like Nelson and Wylie, Longino has a stake, both feminist and philosophical, in establishing that the Kuhnian insights about science do not fully undermine its rationality and objectivity.

Background beliefs do not determine what exists in the world, but rather which features of it are important, relevant, and worthy of attendance. Rather than metaphors of gestalt switches, Longino prefers metaphors of foregrounding and backgrounding aspects of overlapping experiences that determine under what terms the world is understood. There must be overlap between terms and experiences for several reasons. Longino points to the paradoxical situation, in arguments paralleled by Mary Hesse and Donald Davidson, in which two theories can both be mutually incommensurable and comparable in any way. [93] Evaluation of consistency and inconsistency, compatibility and

---

[92] Ibid., 48.
[93] {Hesse, 1980 #178}{Davidson, 1984 #14}{Davidson, 1984 #15}

incompatibility, or even that theories are discussing the same general phenomena, require a common ground of intelligibility from which to make judgments of difference and sameness. If two paradigms are incomparable, then the assessment of their relationship or lack of relationship is impossible. In other words, Kuhn's account must presuppose an overlap of experience between paradigms in order to distinguish paradigms from each other. [94]

Sets of background beliefs therefore cannot be seen as self-contained and contrasted with other self-contained sets of assumptions, which is what Kuhn's talk of paradigms implies. Rather, background beliefs must be interacting and interlocking presuppositions, whose content can both reinforce and contradict that of other background beliefs. While all data must be informed by a context of background beliefs in order to be evidence for scientific hypotheses, and all hypotheses must be informed by a context of background beliefs in order to be considered meaningful inquiries applicable to the world, these contexts neither uniquely determine data and hypotheses, nor are themselves immune to being contrasted to other sets of background beliefs. Longino concludes that the limitations on incommensurability "restore meaningfulness to the concept of evidence, although evidential relations must be understood relative to some context of assumptions." [95] And, as I will argue below, they must also be understood relative to some context of social practices.

---

[94] As we saw in the first chapter, Kuhn is at least ambiguous on this point. The importance of the arguments of Davidson, Hesse, and Longino about incommensurability is that Kuhn's account cannot (no matter what his intentional position) show that paradigms are incommensurable with each other, on pain of contradiction.

[95] Longino, *Science as Social Knowledge : Values and Objectivity in Scientific Inquiry*., 57.

Longino's contributions

The role of social values

After establishing that there is nothing in principle that insulates background beliefs from criticism, Longino observes two interrelated qualities about the role of background beliefs in scientific research that I will examine in detail. First, background beliefs are both infused by, and endorsed partly because of, social and cultural values that scientists derive from their broader social contexts. Second, background beliefs are logically prior to empirical scientific research by determining what questions are important, which correlations are meaningful, and the general direction of causal relations among those correlations. This point is important because it stands in stark contrast to Nelson's picture where these "theories of nature" are considered in the same evidential plane, so to speak, as (value-laden) observational evidence. Although these models can be derived to a certain extent from empirical findings, they are ultimately established on other grounds. I will examine her first point in this section and her second point in the next section.

Longino argues that background beliefs are often linked to the needs and interests of the socio-economic-cultural context in which scientists live because science must be rooted in what we want to know about nature, what questions we are asking of it, and not just what it independently tells us. As a result, contextual values (what is socially, economically, and culturally important to people) play a pivotal role in determining what science considers important, relevant, and worthy of attendance. While Kuhn argued[96]

---

[96] Insofar as these interests must arise from people's experiences and interactions with the world, they are also empirical. But unlike empirical questions about how the world *is*, interests most often make claims about how the world *ought to be*. These claims, as Hume and Habermas both argue, cannot be directly justified our contradicted by empirical assessments of any current state of affairs.

that these contextual values played a role only in the more immature (and less insulated)

sciences, Longino argues that they play a role in the background beliefs of all of the

sciences.

Longino argues that background beliefs can function either globally, as general,

framework-like models that determine the character of explanation or specifically to

facilitate inference in particular fields. The general models are most illustrative for our

purposes, and Longino uses the examples of the mechanistic, hermetic, or interactionist

models of nature. [97] Longino discusses how the general background belief in 16th and 17th

century science that the natural world is properly understood "as a machine or a

mechanistically organized collection of machines decomposable into quantitatively

describable and manipulable parts" not only determined the typology of objects inquired

after (inert matter whose only real properties are shape, weight, and motion), but also the

relationship between man and nature (nature was an entity to be predicted and

manipulated for human use). Longino, along with several other feminist theorists, has

pointed out the congruence between the mechanistic model of nature and the social and

political needs of European societies at that time.

> What the social and economic developments associated with the rise of the emerging
> bourgeoisie did was to provide an environment in which could thrive a science that
> rationalized and extended the powers of craftspersons and artisans, whose products were
> necessary to that class. The idea of the world as a machine need not have been derived
> from or inspired by or otherwise caused by economic factors in order that we should see
> these as playing a major role in the eventual triumph of the mechanistic way of thought.[98]

While the mechanistic model is one historical example of background beliefs

informed by social interests, feminist theorists have often pointed to the current

---

[97] These models are akin to Ian Hacking's "styles of reasoning," yet as we shall see, Longino articulates
ways that they are amenable to social criticism in ways that Hacking does not.

[98] Longino, *Science as Social Knowledge : Values and Objectivity in Scientific Inquiry*., 96-7. For another
great example of the relation between scientific models and social interests, see Elizabeth Potter, *Gender
and Boyle's Law of Gases*, *Race, Gender, and Science* (Bloomington: Indiana University Press, 2001).

presumption of a sex- and gender-dimorphic division in most living creatures as a current

global model. Independently of the justification of particular instances of sexual

dimorphism, this model determines what kinds of biological and social dynamics are

fundamentally significant to find and account for, and therefore how research projects

and questions are framed. Longino uses the example of hormone research.

> The analogy between human behaviors and the stereotyped non-human behavioral
> dimorphisms seems obvious if one expects sexual dimorphism and classifies behavior as
> masculine and feminine. Without this expectation or the assumption that behavior is so
> gendered, however, the behaviors of the children seem more various and classifiable
> under different schemas. Hand-eye coordination, for example, cuts across indoor and
> outdoor, feminine and masculine behavioral classifications. The assumption of
> dimorphism makes certain features of the behaviors—for example, level of expenditure
> of physical energy—more salient than others, and thus makes the behaviors appear
> suitable as evidence for the hormonal hypothesis.[99]

Longino wants to avoid one frequent response to these types of discussions,

which is to reduce scientific theories to their social explanations. She points out that

worries about the justification and criticism of science are not in tension with the role of

values in science per se. The argument that background beliefs necessarily play a role in

scientific reasoning, and the further argument that background beliefs almost always

incorporate social, cultural, political, economic, and even individual values is not

sufficient to deny that science is an epistemic enterprise. This would require the further

argument that background beliefs, because they are often infused with social values, are

not epistemically criticizable. Longino denies this argument, just as Nelson and Wylie

did. But she diverges in an important sense from the empiricisms we examined in the last

chapter, since she attempts to address the problem of criticizing background beliefs, not

---

[99] Longino, *Science as Social Knowledge : Values and Objectivity in Scientific Inquiry.*, 121.

by expanding or examining the notion of empiricism, but rather by expanding the notion

of criticism beyond empirical criticism.

Conceptual criticism

> Because the relation between hypotheses and evidence is mediated by background
> assumptions that themselves may not be subject to empirical confirmation or
> disconfirmation, and that may be infused with metaphysical or normative considerations,
> it would be a mistake to identify the objectivity of scientific methods with their empirical
> features alone.[100]

In the fourth chapter of *Science as Social Knowledge*, Longino expands the types

of criticism found in scientific discourse. The first, and most analyzed, type of criticism

is evidential criticism that proceeds on the basis of experimental and observational

concerns. These criticisms address "the degree to which a given hypothesis is supported

by the evidence adduced for it, questions the accuracy, extent, and conditions of

performance of the experiments and observations serving as evidence, and questions their

analysis and reporting.." [101]

While the necessity of evidential criticism remains unquestioned, its sufficiency is

brought under suspicion once one acknowledges that there are necessarily shared

presuppositions in a scientific community [however delineated] that determine what

constitutes evidence. The challenge of criticizing these shared presumptions was the

problem that arose in both Nelson and Wylie's accounts. Longino points out that this

situation is avoidable if states of affairs, hypotheses, and background beliefs are all

independently specifiable. This is not a return to a positivist account where states of

affairs are capable of description independent of other hypotheses and background

beliefs, but rather that background beliefs are capable of support by means other than the

---

[100] Ibid., 75.
[101] Ibid., 72.

data upon which they confer evidential relevance. This was the avenue taken by Alison Wylie in the last chapter.

While appealing to independent sources of evidence can avoid many of the circularity traps, it is insufficient to avoid the underdetermination at the root of the problem. "As long as the content of theoretical statements does not consist of generalizations of data or the content of observational statements is not identified with theoretical claims, then there is a gap between hypothesis and data. . ." [102] The connection between evidence and hypotheses requires not only an inductive move, but also semantic and/or methodological ones. The latter are the most significant content of background beliefs, and they are problematic because these meta-level influences of background beliefs are not directly supportable by evidence. Thus, their existence is the most important challenge to the problem of justification.

For example, hypotheses are often accepted because they are "relevant to the explanation of the phenomena comprehended by the theory." [103] Longino points out that most debates about relevance center not upon data but "the assumptions in light of which the data are interpreted." [104] As we saw in the analysis of Nelson's arguments, relevance, like significance and emphasis, is not itself directly determined by evidence, but is rather a decision that must be made for experience to count as evidence for a hypothesis. While one likely move at this point is to argue that these are the parts of science that are immune to criticism, and thus threaten the criticizability of the enterprise, Longino takes another route.

---

[102] Helen E. Longino, *The Fate of Knowledge* (Princeton, N.J.: Princeton University Press, 2002)., 126.
[103] Longino, *Science as Social Knowledge : Values and Objectivity in Scientific Inquiry*., 51.
[104] Ibid., 72.

Longino appeals to another major category of scientific criticism in what she labels "conceptual criticism." Longino divides this category of criticism into three major types. One can challenge the conceptual soundness of a hypothesis, or the consistency between hypotheses and accepted theories. Finally, one can challenge the relevance of evidence used to support hypotheses, and it is this third type of conceptual criticism to which background beliefs are most often germane.

The relevance, importance, or worthiness of background beliefs often cannot be empirically tested because the more methodological and metaphysical ones are second-order claims regarding the research program itself, rather than the particular findings of that program. The goal of science is not only to be empirical and comprehensive, but also to answer particular questions about the world. While the traditional conception of science urges an ideal of disinterested description and explanation, not all questions can be asked and certain phenomena must be foregrounded and backgrounded, considered relevant and irrelevant, in order for science to proceed.

> The background assumptions that fill that gap, then include substantive and methodological hypotheses that, from one point of view, form the framework or proximate intellectual context within which inquiry is pursued and, from another, structure the domain within which inquiry is pursued.[105]

---

[105] Longino, *The Fate of Knowledge*., 127. A good example of this is the contrast between ethnological programs and ethnographic programs in anthropology. As Mark Risjord shows, the context of the former was to ask, "Why are human groups so *different* rather than being the same?" while the latter inquires "Why are the tribes and nations of the world different, and how have the present differences developed?". The difference between these two programs is connected to a fundamental difference in background beliefs. The former presupposed the existence of a broad historical development of civilization, and inquired into how it manifested differently in different places. This assumption wss consistent with enlightenment political theory and romantic speculative philosophy. The latter asked local questions about why people in particular cultures think and act as they do, presuming that cultures are incomparable and of equal value. This was consistent with egalitarian political theory.Mark W. Risjord, *Woodcutters and Witchcraft : Rationality and Interpretive Change in the Social Sciences*, *Suny Series in the Philosophy of the Social Sciences* (Albany: State University of New York Press, 2000).

Not only do background beliefs shape large-scale research problems, but they are also necessary for the uptake of evidence.

Observational data consist in observation reports that are ordered and organized. This ordering rests on a consensus as to the centrality of certain categories (the speed of a reaction versus the color of its product), the boundaries of concepts and classes (just what counts as an acid), the ontological and organizational commitments of a model or theory, and so on. Observation is not simple sense perception (whatever that might be) but an organized sensory encounter that registers what is perceived in relations to categories, concepts, and classes that are socially produced.[106]

In addition to the extra-empirical assessment of background beliefs, they are frequently

unrecognized and implicit as well. Conceptual criticism thereby requires two steps.

First, the concepts and relationships defined by background beliefs must be brought to

light, and only then can they be conceptually assessed. Although Longino is never

explicit about these two stages, she acknowledges (once) that there is a distinction

between implicit and explicit background commitments in science.

Although I shall use these terms interchangeably, it is appropriate to speak of beliefs when these statements are more or less explicitly adopted as tenets and of assumptions when their necessity to a bit of evidential reasoning is not explicitly acknowledged.[107]

The fact that Longino uses beliefs and assumptions interchangeably indicates that

she does not find the difference significant for her account. As we shall see, the efficacy

of her social norms for the criticizability of background *beliefs* is crucially different than

their efficacy when applied to background *assumptions*. It is the latter, especially in the

situations where those assumptions are held in common by a majority of the scientific

community, that leads to epistemic blindness and the social entrenchment of these beliefs.

I will revisit these issues later.

---

[106] Longino, *The Fate of Knowledge*., 100.
[107] Longino, *Science as Social Knowledge : Values and Objectivity in Scientific Inquiry*., 59.

In spite of the challenge of background assumptions, Longino's account makes criticism possible once again, not by isolating science from the influence of value-laden background beliefs, but by making scientific background beliefs available for conceptual debate. So what enables this conceptual criticism to occur? Longino distinguishes herself from the sociologists of knowledge by drawing a connection between the necessity and nature of background beliefs in science, the possibility of their criticism, and the sociality of science. Instead of starting with the sociological description of science as a social enterprise and then challenging the rationality of science in this light, Longino begins with the epistemological necessity of background assumptions on one side and the epistemological requirement that criticism is possible on the other. Longino then *concludes* that it is the sociality of science that enables the criticism of background beliefs. "It is the interaction of subjects exercising their cognitive capacities to observe and reason that stabilizes these processes in a way that permits inquiry or investigation to continue." [108] Not only *is* science a social practice, but it *ought* to be a social practice, for only as a social practice is the traditional ideal of self-criticism attainable by science. "[Science's] objectivity consists not just in the inclusion of intersubjective criticism but in the degree to which both its procedures and its results are responsive to the kinds of criticism described. [109]

Longino's objectivity requires the possibility of what she coins "transformative criticism." Transformative criticism is specifically the type of criticism that challenges the background beliefs of a scientific program. Here we can see that Longino's account fills a gap in the Kuhnian one. Whereas Kuhn's account led to a circular problem where

[108] Longino, *The Fate of Knowledge*., 108.
[109] Longino, *Science as Social Knowledge : Values and Objectivity in Scientific Inquiry*., 76

crises in science (times when the paradigms are challenged) seemed to presupposed the crises themselves, Longino attempts to provide an account of what makes these scientific crises possible by describing the criticism of background beliefs.

Longino argues that the criticism of background beliefs requires "effective critical interaction" between different points of view. "As long as background beliefs can be articulated and subjected to criticism from the scientific community, they can be defended, modified, or abandoned in response to such criticism." [110] She invokes peer review as a prime example of this social process of justification and criticism. The important upshot for Longino is that criticizing background beliefs is an inherently social procedure because the influence of individual preferences, especially in conceptual matters, can only be checked by the constraint and criticism of other members of the scientific community.

For Longino, transformative criticism in scientific communities is only possible when certain social norms guide scientific practice. These fall under the four categories of (1) recognized communal avenues for criticism, (2) shared public standards of criticism within the community, (3) community responsiveness (in its theories and metaphysics) to criticism, and finally (4) equality of intellectual authority. Longino requires critical venues in order to counterbalance the current marginalization of critical discourses in the sciences. She points to limits of space, the economics of scientific production, and privatization as social and institutional forces that tend to curb the critical components of scientific discourse. While there have been political arguments against the economic model of science, this social norm, and its relationship to the possibility of

[110] Ibid.. 73-4.

robust criticism of scientific data and theories, provides an epistemic arguments against this trend.

The responsiveness requirement, which she reconceptualizes as <u>uptake</u> in *The Fate of Knowledge*, requires that the scientific community not only tolerates significant dissent, but recognizes and responds to it. Longino particularly emphasizes the relevance of this requirement to background assumptions in that "the assumptions that govern [scientific] group activities remain logically sensitive to it."[111] The issues of epistemic blindness and social hierarchies, which both have an impact upon which criticisms are recognized, clearly have a bearing on this criterion, and I will return to this issue shortly.

The third social norm Longino advocates is that the scientific community must have <u>shared public standards</u> according to which content and practices are evaluated. This criterion is necessary for a few reasons. First, as she argued when discussing the impossibility of incommensurability, comparison and thus criticism is impossible without interlocutors sharing some common ground.[112] Second, the fact that these standards are shared helps mitigate against individual whims playing a biasing role in scientific discourse. Third, that these standards are public means that the community can be held accountable to them and criticism can be performed by anyone from either inside *or outside the community.*

An important aspect of this criterion is that although Longino requires that the norms are "public", this does not necessarily entail that they are publicly acknowledged. "The point of requiring standards that are public is that by explicitly *or implicitly*

---

[111] Longino, *The Fate of Knowledge*., 130.

[112] Longino points out that this common ground need not always be the same ground, since not only the content of scientific claims, but the standards themselves, must be available for criticism. The point is just that certain standards must be shared and held temporarily constant in order for social criticism to take place.

professing adherence to those standards individuals and communities adopt criteria of

adequacy by which they may be unarbitrarily evaluated." [113] The possibility of implicitly

shared standards is the precise problem of epistemic blindness, and that this blindness can

have a bearing on the possibility of transformative criticism is an issue that Longino does

not here acknowledge.

Finally, Longino argues that scientific communities must adhere to the criterion of

equality of intellectual authority. Longino's final requirement strives to establish

authority based on relevant points of view. These are determined by "logic and critical

discussion" that recognize that "every member of the community be regarded as capable

of contributing to its constructive and critical dialogue." [114] These requirements are aimed

to prevent the suppression or privileging of certain points of view for political reasons

like social position and economic power. Longino blames this dynamic for the insulation

of certain scientific assumptions, such as racist or sexist ones, in scientific inquiry.

Longino further qualifies *which* points of view are relevant in her later work *The*

Fate of Knowledge. There, she adjusts "equality" to "tempered equality" in order to

avoid that she "require that each individual, no matter what their past record or state of

training, should be granted equal authority on every matter." She refers to this state of

affairs as a "cacophony", and it is arguably what would result from any practical

manifestation of Nelson's picture of a scientific community "broadly construed." [115] To

delineate the "relevant voices," Longino appeals to the restrictions that result from the

"shared standards" of the scientific community, as well as the relevant voices implied by

the shared goals of science.

[113] Longino, *The Fate of Knowledge*., 130. italics mine
[114] Ibid., 132.
[115] For this discussion, see Ibid., 131-2.

Longino's tensions

Status of social norms: constitutive or contextual?

One problem with Longino's social norms is that she is unclear exactly as to their status vis-à-vis the other norms of scientific inquiry. In *Science as Social Knowledge*, Longino defines "constitutive values", as "those that are the source of the rules determining what constitutes acceptable scientific practice." [116] As examples of these types of values, Longino takes from Kuhn a list including simplicity, accuracy, predictability, and others that are traditionally held by the scientific community to be virtues of adequate theories. As we saw in Chapter 1, these are the values that Kuhn refers to in his lecture, "Objectivity, Judgement, and Theory Choice" as internal to science because they are established by the consensus of scientific communities throughout their history.

Longino contrasts these constitutive values with "contextual values", those that reflect "the personal, social, and cultural values, those group or individual preferences about what ought to be." [117] She calls these values contextual (while Kuhn calls them external) because they derive from science's social and cultural context. Longino attempts to maintain this distinction in her later discussions while arguing that contextual values play a much stronger (and more necessary) role in science than previously thought. She delineates five distinct types of interactions between contextual values and the epistemic processes of research.

    1) Contextual values can affect the practices that bear on the epistemic integrity of science.

---

[116] Longino, *Science as Social Knowledge : Values and Objectivity in Scientific Inquiry.*, 4.
[117] Ibid., 4.

2) Contextual values can determine which questions are asked and which ignored about a given phenomenon.

3) Contextual values can affect the description of data.

4) Contextual values can be expressed in or motivate background assumptions facilitating inferences in specific areas of inquiry.

5) Contextual values can be expressed in or motivate the acceptance of global, frameworklike assumptions that determine the character of research in an entire field. [118]

While these types of interactions put her account in direct contrast to the strong internal/external divide in Kuhn (and the positivist tradition), her initial definition of contextual values also conflicts with her own account of the intrinsically social nature of scientific inquiry and the role of social values (her four social norms, as well as her definition of the role of contextual values in background beliefs) as bearing directly on the epistemic integrity and the content of science. This is why she further blurs this distinction in *The Fate of Knowledge*, which came out 12 years later. In spite of this later shift, I believe that this early confusion and distinction can be illustrative in ways that are obscured if the distinction between constitutive and contextual values are abandoned entirely.

Longino, like Kuhn, was inheriting a traditional epistemic dichotomy between constitutive values in science on the one hand and contextual values in science on the other. Consistent with this dichotomy, those values presupposed by the goals of science, which uncontroversially include those that enhance empirical adequacy and are conducive to independence from the bias and preferences of the researchers (subjectivity), are constitutive of science, while any values that are not derived from the

[118] Ibid, 86

goals of science are contextual values. This contrast is also consistent with the theoretical and normative picture of the practice of science that Longino develops.

She is simultaneously inheriting the tendency to automatically conjoin constitutive with cognitive values on the one hand and contextual with social values on the other. In the traditional positivist view, cognitive values are evidential or justifying reasons for scientific judgment. Social values, on the other hand, adhere to the "nonevidential (ideological, professional) considerations or on social interactions among the members of a community rather than on evidential reasons in accounting for scientific judgment." [119] Put another way, cognitive values are those that adhere to scientific judgments about theories (like evidential adequacy, consistency, etc.) while social values adhere to judgments about people and communities (like egalitarian, democratic, hierarchical, etc.) Especially in *The Fate of Knowledge*, Longino takes great strides in integrating these cognitive and social values in science.

The equivocation between these two contrasts in her early work reflects an equivocation in the broader literature about social values in science as well. Traditionally, the only values that were derived from the goals of science were those that were independent of all social values, and thus the mapping of the two dichotomories on each other was appropriate. Due to Longino's argument that social values play an integral role in science through background beliefs, and that certain social norms are *required* for the possibility of empirical criticism as well as the elimination of bias (in the form of value-laden background assumptions), social as well as cognitive values become required to achieve scientific goals, and are thus constitutive of scientific practice.

---

[119] {Longino, 2002 #1}, 2

Longino's 1990 definition of "cognitive" as opposed to "epistemic"can help clarify this terminological morass. Those social values (those that qualify social entities) which are epistemic (derived from the goals of science) are constitutive, along with cognitive values (those that qualify scientific theories). Longino's four social norms of scientific discourse fall under this category. Those social values that are non-epistemic (not derived from scientific goals but from broader society) may be constitutive (when they constitute background beliefs, which in turn are necessary to like hypotheses to data). Mark Risjord provides a clear example of non-epistemic constitutive values in his discussion of Boas' moral egalitarianism as determining the relevance class in the erotetic model, or Longino's ecample of mechanistic politics shaping physics models in the 17th century. Finally, social values may be non-epistemic and contextual, such as the political values that determine scientific funding, technological applications, and so on, or the racist and sexist values that have played a strong role in the historical practice of science.

By comparing Longino's conceptual distinction between constitutive and contextual values of science with her theoretical argument about the necessary role of social norms for the objectivity of science, an important and enlightening tension comes to light. The traditional scientific prerequisites for objectivity such as empirical adequacy and the exclusion of subjective preferences, which were understood to be values internal to scientific inquiry, must be augmented with social norms that make these virtues possible. By seeing science as a social enterprise Longino is able to theorize certain social values as *epistemic* values o inquiry as well. Social norms guide social procedures, and thus the responsiveness of the scientific community to important types of criticism require these social norms.

Criticizing idiosyncratic versus collective bias

Longino pits her defense of objectivity against the incursion of "subjective" or "idiosyncratic" biases into the scientific community. "Objectivity in the sense under discussion requires a way to block the influence of subjective preference at the level of background beliefs" [120] This target reflects the trends in the philosophy and sociology of science in the post-Kuhnian climate.

But as many feminist critics have demonstrated, collective biases are just as problematic as, and often more insidious than, individual biases. Androcentric assumptions in particular are examples of this type of collective bias, and they are able to infiltrate science precisely *because* they have been shared among large portions of scientific communities. Longino makes reference to this dynamic several times.

> When. . .background assumptions are shared by all members of a community, they acquire an invisibility that renders them unavailable for criticism. They do not become visible until individuals who do not share the communty's assumptions can provide alternative explanations of the phenomena without those assumptions.[121]

> . . . values can enter into theory-constructive reasoning in two major ways—through an individual's values or through community values. . .Because community values and assumptions determine whether a given bit of reasoning will pass or survive criticism and thus be acceptable, individual values as such will only rarely be at issue in these analyses.[122]

> . . .the invisibility of many background assumptions to a community means that a closed community will not be able to exhibit those assumptions for critical scrutiny. In addition, the degree of intersubjective invariance of a set of observations will be limited by the degree of perspectival variation.[123]

> . . .even though a community may operate with effective structures that block the spread of idiosyncratic assumptions, those assumptions that are shared by all members of a community will not only be shielded from criticism, but, because they persist in the face of effective structures, may even be reinforced.[124]

---

[120] Longino, *Science as Social Knowledge : Values and Objectivity in Scientific Inquiry.*, 73.
[121]Ibid, 80
[122] Ibid, 81
[123] Longino, *The Fate of Knowledge.*, 107
[124] Ibid, 135

The third quote is found in *The Fate of Knowledge* just a few sentences after Longino reiterates that the participation of multiple points of view is "to insure that the hypotheses accepted by the community do not represent someone's idiosyncratic interpretation of observational or experimental data." [125] Nowhere is the tension between the feminist target of collective bias and the framing in terms of individual bias more obvious.

This problem is analogous to Wylie's "security" criterion. Communally shared and recognized scientific standards are of course necessary in a world where transcendent foundational norms of justification to not exist. On the other hand, views are shared due to their ability to withstand criticism as well as their ability to match up to "commonsense" views of those with authority, and thus slip under the critical radar. These can appear supported by "independent" sources when in fact they are uncritically presupposed by them. This yields a circular picture where those commitments in the center of the Quinian web, so to speak, are unquestionable due to their extremely verified status or due to their extremely unquestioned status. To split this circle apart, there must be a way to distinguish these two possibilities.

Longino invokes the of "equality of intellectual authority" norm to advocate a greater level of diversity within the scientific community, and thereby enable a dialogue in which the shared assumptions of the scientific community can be criticized. In this way, she attempts to incorporate the "outsider's" alternative point of view into the internal social mechanisms of the scientific process. This scientific diversity in practice is what Longino attributes to the ability of feminist scientists to challenge the androcentric assumptions in fields such as biology, anthropology, and endocrinology.

[125] Ibid., 106

While I do not argue the factual point that it has been those scientists with feminist scruples who have challenged many of the collective background assumptions of their sciences, I do not believe that Longino's four social norms are adequate to either account for their impact, nor to facilitate this sort of critical engagement in the future.

The problem with all of Longino's social norms is that they do not sufficiently acknowledge the role of power dynamics both within scientific communities and between scientific communities and communities outside of them. The persistence of sexist and racist assumptions throughout the history of science is not simply the product of the absence of women and non-Caucasian scientists that can be remedied by a more diverse scientific population. This would serve to alleviate a symptom of the epistemic problem, not the cause. It is not only the homogenous demographics of scientific communities, but also the cultural power of the shared norms that it endorses, which insulate its assumptions from criticism.

Ideas to take from Longino

The first lesson that can be brought out from Longino's account and its tensions is that any picture that considers social factors ipso facto contextual presupposes the traditional "context of discovery"/ "context of justification" distinction whereby qualities of socio-political or psychological domains cannot legitimately bear upon the objective methodologies of science. Challenging this contrast, as any social account of science like Longino's must, need not automatically imply that all socio-political and psychological dynamics are within the realm of justified scientific social practice. Rather, it means that the positive *and* negative role of social and personal dynamics must be examined, rather than a priori excluded, from the context of justification.

While Longino takes strides in this direction with her advocacy of social norms as conditions for objectivity, she has yet to recognize the full ramifications of her socialization of science. This problem is manifest in her equivocation between "social values" as a term that refers to group or individual subjective preferences (those things that social beings, like scientists, value), and "social values" in terms of the norms that guide social processes (those values that adhere to social entities, like scientific communities.) In essence, her argument is that we need "social values" (her social norms for the scientific community) in order to prevent certain "social values" (collective biases like androcentrism) from inhibiting the criticial enterprise of scientific research.

I do not believe that the problematic use of the constitutive/contextual distinction in Longino's account undermines either her arguments for social norms or those social norms themselves. If anything, seeing the connection between her community level criteria and the constitutive scientific ideal of self-criticism strengthens the import and rhetorical punch of those criteria. It is more revolutionary than Longino realizes to expand the values constitutive of science beyond those that apply to good theories, like empirical adequacy and constituency, to those that also apply to good scientific communities, such as those that manifest Longino's social norms as much as possible.

While is not the first to argue that good social and political norms are necessary for good science, her account is one of the few that rest that argument on epistemic, rather than political grounds. Further, the epistemic grounds that she invokes, namely that certain social and political arrangements are conditions for the possibility of criticizing background beliefs, which in turn are necessary for the connection of theories with data, and thus empiricism itself, enables her to ground certain social values on the

most traditional epistemic values of all, self-correction and empiricism. It is this

connection that I find most fruitful in Longino's account, but before endorsing her

picture, it is important to ascertain whether the social norms that she endorses accomplish

the goals she sets out for them; creating the social possibility of criticizing background

beliefs. In the context of my overall worries, I am primarily concerned about whether

these social norms can enable the criticism of those background beliefs that are implicit

(contributing to epistemic blindness) and held by those with scientific authority

(manifesting social entrenchment).

While Longino's aim is to establish how science is a social enterprise, she does

not seem to incorporate the fact that membership in a scientific community is also a

social location. In her socialization of science, Longino discusses how individual

knowers ought to be seen as conditioned by aspects of their social location. As a social

location, membership in a scientific community also conditions its members, along with

their other social locations in terms of race, gender, nationality, sexuality, etc. As

Longino points out in her article, "Subjects, Power, and Knowledge":

> Because background assumptions can be and most frequently are invisible to the
> members of the scientific community for which they are background and because
> unreflective acceptance of such assumptions can come to *define what it is to be a member
> of such a community* (thus making criticism impossible), effective criticism of
> background assumptions requires the presence and expression of alternative points of
> view.[126]

Membership, and even more often, status in scientific communities often requires an

acceptance of certain assumptions, and education into those communities serves to impart

these beliefs even to those who did not initially share them. There are therefore two

---

[126] Helen E. Longino, "Subjects, Power, and Knowledge: Description and Prescription in Feminist
Philosophies of Science," in *Feminist Epistemologies: Thinking Gender*, ed. Linda Alcoff and Elizabeth
Potter (New York: Routledge, 1993)., 112. italics mine

significant social advantages and corresponding disadvantages that play a crucial role in both the persistence and possibility of critiquing background assumptions.

The first epistemically relevant power dynamic is between those scientists who have built reputations and gained status and power within the scientific community, and those scientists who are either new to that field, or new scientists, or else are experienced scientists who have not gained status in their careers. While the correspondence between scientific clout and scientific credentials and experience is a crucial (and most often appropriate) dynamic within the community, it can also serve to entrench hegemonic scientific positions against credible and fruitful criticisms, as we saw in Barber's account in Chapter 1.

The second relevant power dynamic is between the epistemic authority of the scientific community on a given subject matter and the epistemic disadvantage of laypeople on the same subject matter. Once again, the recognition that scientific training is significant and superior in important ways to the unconditioned beliefs of laypeople is valid, but this dynamic can also serve to insulate scientific assumptions from relevant criticisms from outside its borders.

Both of these dynamics were manifested in the recent historical case of AIDS research, which I will discuss at length in the next two chapters. This case will provide resources for addressing the issues raised by Longino's account of social norms, as well as provide crucial tools with which to formulate a social norm (in addition to her four) which can adequately ensure the possibility of criticism, which was one of Longino's major goals. Venues for criticism, like peer review, served in the case of AIDS research to entrench the hypotheses and theories (and their corresponding background

assumptions) of those with power and to censor and suppress the hypotheses that challenged and criticized those assumptions. Whether the dominant theories were ultimately justified or not is not so much the issue as the historical fact that these theories were often endorsed and applied before this justification was ascertained, and likewise challenging evidence and theories (often from scientists with less power and prestige) were not appropriately engaged, whether they would ultimately turn out to be justified or not.

Shared public standards also served to insulate background assumptions from criticism in the AIDS case. We will find examples of this in the unquestioned commitment to placebo clinical trials, which was a standard method of research shared by the scientific community, as well as the shared assumptions that acceptable explanations of disease must ascertain a single fundamental cause and focus primarily on biological rather than environmental causes. These shared scientific standards acted to block the challenges of both minority scientists and many AIDS patients who argued that other methodologies and explanatory tactics would be more fruitful and effective.

Due to social entrenchments in the scientific communities, uptake/responsiveness to criticism from "inferiors" and "outsiders" was often delayed, if not stymied altogether. It is possible that these challenges could be addressed with Longino's final requirement of "equality of intellectual authority, but this requirement is much too vague, even with her later amendment of "tempered authority." As Longino herself brings up, the relationship of epistemic authority to scientific training, experience, and social location are all relevant to this requirement.

In order to "equalize" authority, it is necessary to ensure that the scientific analysis of a problem allocates appropriate authority to those with appropriate expertise regarding that problem. This also requires a recognition that the reality of scientific hierarchies, while bestowing authority on particular types of expertise, tend to elide and ignore the voices of others with relevant voices. Thus, to "equalize" authority requires a type of counterbalancing, where those in disadvantaged epistemic positions are often also in the precise positions from which relevant criticisms can arise. But what would this social norm look like?

Longino ends her discussion with several questions that remain to be addressed. A couple of them are directly relevant to my discussion. She asks, "What bearing should greater cognitive authority have on the attribution of intellectual authority? [127] The former she defines in terms of the amount of domain-specific knowledge one holds, while the former she understands as the more general skills of "observation, synthesis, or analysis." [128]

Perhaps one way to characterize the problematic issue of social hierarchies in science is that those at the top of the hierarchies do not hold a monopoly on cognitive authority (experience, extensive knowledge of a field.) This is where Longino's account can benefit from the insights brought out by Wylie. Wylie's appeal to feminist standpoint theorists pointed the way to acknowledging power dynamics in science and society in order to advocate heeding those with experiences in less powerful social locations. The crucial help that Longino's account provides are the resources to more specifically articulate the status of this directive (as a social norm) and the content of that

---

[127] Longino, *The Fate of Knowledge*., 133.
[128] Ibid., 133.

directive in relation to the background assumptions that function in science. Shared

standards, recognized critical venues, and specific determinations of why people have

cognitive authority are necessary. I will add to these contributions my own insights

regarding epistemic blindness and social entrenchment in order to articulate a social norm

that can address them. Only then can the voices (both scientific and nonscientific)

relevant to those background assumptions be determined in a way that avoids

essentializing and automatically granting privilege. This understanding can also be

helpful to answer Longino's other question, that of community membership.

> [The tempered equality condition] requires both that scientific communities be inclusive
> of relevant subgroups within the society supporting those communities and that
> communities attend to criticism originating from "outsiders." It makes us ask who
> constitutes the "we" for any given group, and what the criteria are for providing the
> answer.[129]

This question echoes those that also surrounded Nelson's account of a scientific

community "broadly construed", and is at the heart of my project. Philosophically

discussing feminist epistemologies has provided many tools with which to begin to

articulate an answer, but as the problem facing us is both theoretical and practical, so are

the clues to its solution. As a result, I need to bring this discussion closer to the ground.

In the next two chapters I will directly engage the complex case study of the early years

of AIDs research, most specifically through the analysis in the book *Impure Science:*

AIDS, Activism, and the Politics of Knowledge by Steven Epstein. Through examining

the dynamics of this case, I will more carefully distinguish the several ways that

epistemic blindness and social entrenchment interact with the practice of science, and the

way that social values ought to be engaged in order to enhance science's ability to

address these issues.

[129] Ibid., 133-4.

Chapter 4: AIDS case study: Part I

AIDS etiology research

. . .even though a community may operate with effective structures that block the spread of idiosyncratic assumptions, those assumptions that are shared by all members of a community will not only be shielded from criticism, but, because they persist in the face of effective structures, may even be reinforced.[130]   ---Helen Longino

Introduction

Ironically, Longino's quote above indicates the failing I found in the otherwise innovative and groundbreaking accounts of Nelson, Wylie, and Longino. In the last chapter, I found that Longino's social norms address the problematic gap in the (expanded) empiricisms of Nelson and Wylie by recognizing that the constitutive values of science need to include social and not just theoretical norms. This was in response to the recognition that the social dynamics and values of scientific communities play a role in both the criticism and the insulation of these background beliefs. Not only does Longino argue that social norms are needed to make criticism possible, but she argues *which* social norms are needed. This is where I found the flaws in her account, since her social norms are predominately aimed at weeding out individual biases in background beliefs but are insufficient to enable the criticism of background beliefs shared by a majority of (or the top echelons of) scientific communities. This is due to the two intertwined dynamics that also plagued the epistemic accounts of Nelson and Wylie: epistemic blindness and social entrenchment.

In this chapter and the next, I will follow these abstract dynamics through in the concrete case of the first fifteen years of AIDS research in the United States. I will

---

[130] Ibid., 135.

predominately utilize the sociological analysis provided by Steven Epstein in his book,

Impure Science: AIDS, Activism, and the Politics of Knowledge, along with other sources.

The first reason for utilizing this case is that the clash between different sets of

background beliefs, and the role that power dynamics played in their invisibility and/or

entrenchment, plays out in many obvious ways in the AIDS movement. Both in the

process of scientifically conceptualizing the disease and the process of clinically

determining its treatment, the framing of the debate by certain sets of background beliefs

is evident.

A second reason for using the case of AIDS is that reason for using this case study

is that it manifests a historical example of the standpoint theorists' goal of shifting

epistemic credibility to the experiences of the "oppressed." In this situation, the

oppressed (those with less relative epistemic and social authority) can be seen as those

people directly suffering or close to those who were directly suffering from the HIV virus

and/or AIDS, with a subset consisting of AIDS activists. It may be tempting to assume

that since AIDS activists did ultimately have their voices heeded within scientific

institutions, then this was a manifestation of Longino's norm of shared intellectual

authority. I will argue that the actual dynamics of how AIDS activists acquired scientific

credibility and clout sheds light on the differences between the standpoint theorists and

Longino's norms, and will hopefully point the way to a social norm that acknowledges

the actual power dynamics that manifest on the ground.

The last and most important reason for utilizing this case study is that I can

illustrate with it the functioning (and malfunctioning) of all of the social norms that

Longino advocates, as well as the roles of other types of values to enhance and inhibit the

scientific process. I will use these specific instances to both elucidate the criticisms of

Longino's norms from the last chapter and to point the way forward to ways in which

scientific social norms could ground and encourage the dynamics that enhanced

criticizability and novelty. I will articulate this potential solution in the final chapter.

In spite of these analogies, I must make some important disclaimers. First, I will

necessarily simplify and exclude important factors in the historical dynamics at play in

the development of AIDS research. There are too many key players, debates, dynamics,

and information involved to do so. I am focusing specifically on the aspects of the

debates that directly manifest either the problems of the invisibility or entrenchment of

background assumptions, their relationship to the productive or counterproductive

enactment of Longino's social norms, and clues to how the standpoint theorists' insight to

"starting out thought from the lives of the oppressed" can be concretely worked out in

this context.

Another disclaimer is that although AIDS research is a complex situation with

many similarities to other epistemic situations in the natural and social sciences, it is only

one case. While it would be inadvisable to infer from the lessons of one case study to

rules for the sciences in general (Hume would be mad at me), I will use the normative

implications of this case study more as a model than as a generalizable set of rules.

Insofar as other scientific situations reflect similar power dynamics, they should impose

similar norms. Even within this case study, different suggested norms emerge based on

the different dynamics in AIDS etiology research as opposed to AIDS treatment research.

How to apply the lessons of this case study to scientific research that diverges in

significant ways from AIDS research must be determined in a more case by case manner.

A final disclaimer is that I will mostly analyze the epistemic relationship between AIDS researchers and AIDS *activists* in these chapters. AIDS activists, while a subset of the population with experiences of AIDS or HIV and often overlapping with clinical trial participants, cannot automatically be identified with either one of these other groups. In reality, there are many interesting questions of representation involved, including whether gay AIDS activists have the *right* to represent the AIDS population, whether they *accurately* represent them (activists are often more radical and political, and also most often gay while the AIDS affected population had many other demographics), and whether they represent them *better* than others who would claim to do so, like researchers and health professionals who were either gay or HIV positive. While all important questions, I will set them aside for the sake of brevity and focus, and for the time being consider the role that AIDS activists had in shaping AIDS research.

Organization of the case study

At the end of the last chapter, I spoke of two different scientific interactions where these dynamics are manifested. The first is between those who hold dominant scientific status and those who hold lesser scientific status. This is a power dynamic internal to scientific communities, and like the examples shown by Barber and Kuhn in the first chapter, this is a dynamic that is problematic in both the natural and the social sciences.

The second social interaction is between those who share the epistemic status of the scientific community and the weaker credibility found among the lay population outside of it. This interaction has epistemic importance in cases where it can be argued that lay background beliefs, as well as the claims informed by them, are critically relevant

to scientific research on a subject matter. These cases more often (but not exclusively) occur in the social sciences where the "objects" of science and the subjects of science very often coincide, in which case it is plausible to believe that the objects of knowledge would have relevant expertise about themselves and their lives crucial to scientific understanding. [131]

Medical research is a useful case in this regard because it straddles the natural and social science divide. While discussions of the role of values in background beliefs tend to focus either on the natural sciences (Kuhn, Barber, Longino) or on the social sciences (Wylie, Smith, Hacking), the problem extends to both, although important distinctions exist. Medical research cannot easily be placed in either category, but certain aspects of it have more affinity to natural or social sciences. Reflecting this dual aspect of medicine, epistemic blindness and social entrenchment (and their possible remedies) manifest in different ways in different parts of medical research.

Epstein is attuned to these different aspects, and usefully divides his analysis of AIDS research into two parts that correspond to them. The first half of the book analyzes research into the "causation" of AIDS and the second analyzes research into the "treatment" of AIDS. While closely related, the controversies regarding AIDS causation were predominantly between the views of different members (and disciplines) within the scientific community while the controversies over AIDS treatment took place between scientists' and their beliefs and interests and AIDS activists and their beliefs and interests. The division thereby also reflects two distinct ways in which social dynamics can cause epistemic blindness and social entrenchment in scientific research. I will reflect this division by analyzing the manifestations of epistemic blindness and social entrenchment

---

[131] This notion of non-scientific expertise will be elaborated in Chapter 6.

in AIDS etiology research in this chapter, and I will follow their manifestations in AIDS

treatment research in the next chapter.

Epistemic blindness and causal hypotheses

Immune overload hypothesis

The initial notice of what would later be known as AIDS occurred in 1981 when

the CDC began to report the first few cases of people dying of a form of pneumonia

usually easily held at bay by the immune system. The initially accepted scientific

hypothesis for understanding the burgeoning epidemic was alternately called the

"immune overload" or "antigen overload" hypothesis, which postulated a significant link

between the "excesses" (both sexual and drug-related) of the "homosexual lifestyle" and

the overwhelming and ultimate breakdown of the immune system. While provided as an

explanation of the cause of AIDS, this hypothesis clearly was also making certain

evaluative claims about the gay lifestyle. This hypothesis was to have a dominant and

entrenched role in the early explanations of the AIDS epidemic, and Epstein points out

that although value-laden, this hypothesis was plausibly consistent with available

evidence at that time.

> . . .the reasoning behind the immune overload hypothesis was not irrational, and the
> hypothesis was not absurd: after all, the epidemic *was* being observed mainly among gay
> men; many of these men *did* have many sexual partners; many sexually active gay men
> *were* known to contract sexually transmitted diseases, as well as poppers and other
> drugs.[132]

Like Nelson, Wylie, and Longino, Epstein does not argue that values, in this case an

initial scientific preoccupation with the "hazards" of a gay lifestyle, necessarily lead to a

disregard for evidence for the sake of an agenda. Social values and evidential

---

[132][132] Steven Epstein, *Impure Science : Aids, Activism, and the Politics of Knowledge* (Berkeley: University of California Press, 1996). , 51.

accountability are compatible, and it is not in their relationship that the epistemic problem

arises. Rather, the practical problem here, manifesting Longino's theoretical account, is

that evidential relations presuppose certain background beliefs that determine the

significance of some data over others, license some inferences over others, and model

explanations in certain ways that are often unacknowledged and implicit.

> But the strength of the resulting hypothesis depended on a long chain of implicit assumptions—that the syndrome was in essence linked to homosexuals (and the cases among heterosexuals could be explained away); that the link to gay men meant that the epidemic was related to gay men's *sexuality*; that if gay men (by this view) were 'promiscuous,' then the illness must be a consequence of their promiscuity; and crucially, that repeated exposure to sexually transmitted pathogens (and to drugs) was actually capable of causing the immune system damage being reported."[133]

The lack of criticism of these initial assumptions illustrates both epistemic

blindness and social entrenchment. This is not because the links or choices of

significance are ultimately untrue, but rather that no immediate attempt was made to

justify them in the face of recalcitrant data or the links that lacked scientific explanations.

The "plausibility" of these assumptions in light of several background assumptions was

sufficient to keep the overload hypothesis as a mainstream scientific theory and to

disregard the inconsistent or lacking data as insignificant for quite a period of time.

And the inconsistent data existed. For example, in New York City at the time of

the discovery of the syndrome, there were at least as many cases of pneumocystic

pneumonia among injected drug users as among gay men. An initial report made by

some Los Angeles clinicians recorded finding the syndrome in two exclusively

heterosexual men. More broadly, when the CDC's task force initially published its

---

[133] Ibid., 51-2.

statistical findings, 8 percent of the 159 cases were among heterosexuals, one of whom was a woman.

Not only was the dismissal of the drug-user cases done implicitly and without acknowledgement (blindness), but societal power dynamics played a direct role in the fact that the gay cases were considered while drug users largely were not. Gay men, who were often affluent and well-educated, were seen by private doctors and prominent teaching hospitals, and thus their "unique" medical situation came to the fore and was engaged by the medical community. Drug users, on the other hand, tended to get sick and die under the radar, either on the street or at overtaxed public hospitals. Even when their cases did begin to be reported, the understanding of the syndrome as the "gay disease" tended to channel these cases out of the analytic limelight. This focus is exemplified by the fact that the original CDC case definition of AIDS, formulated in 1987, excluded conditions that were more prevalent in drug users, such as pulmonary tuberculosis, endocarditis, sepsis, and bacterial pneumonias. [134]

The relationship between background assumptions and the cases of women with AIDS is also illuminating. Still focused on the disease as one that afflicts gay men, the CDC's original 1987 definition of AIDS excluded those symptoms that appeared exclusively in women, such as pelvic inflammatory disease and recurrent vulvovaginal

---

[134] In the face of many reports that criticized the original case definition of AIDS, the CDC revised its 1987 definition of AIDS in 1991. Congress commissioned the OTA (Office of Technology Assessment) to write a background paper examining the epidemiological evidence used by the CDC to revise its definition and the implications of the new definition. Notably, while the CDC did respond to criticisms by altering the definition, it did not add the diseases associated with these other populations. Rather, it added the condition of a CD4 lymphocyte count below 200. While this is a more general marker of the disease, testing for it is quite expensive, and the paper argues that as a result this condition still may not enable health professionals to track the disease in more impoverished populations. govinfo.library.unt.edu/ota/Ota_1/DATA/1992/9206.PDF Office of Technology Assessment, "The Cdc's Case Definition of Aids: Implications of Proposed Revisions," in *The Definition of AIDS: Epidemiological, Clinical, and Policy Implications* (Washington D.C.: 1992).

cadidiasis. [135]   When activists challenged this oversight, the CDC responded that the

absence of these symptoms was due to an absence of data connecting them to HIV

infection. Epstein points out that this argument was viciously circular because

> . . .the necessary data *couldn't be generated*, because "women of childbearing potential"
> had largely been excluded from clinical trials (putatively out of concern for their potential
> fetuses), and when they were included, no pelvic exams were performed. This meant not
> only that we failed to learn about the effects of HIV in women, but also that women were
> denied access to experimental treatments that might have helped keep them alive[136]

I will examine the epistemic ramifications of clinical trials in the next section, but for

now the important point is that the initial (and in some cases perpetuating) exclusion of

women, hemophiliacs, heterosexuals, drug users, Haitians, Africans, etc. from initial

AIDS research was at least partially the result of the dominant hypothesis that AIDS was

a disease of "gay men", and thus understanding its cause must focus on qualities specific

to the lifestyle of "gay men", and not qualities that adhere to these other social groups.

This is a prime example of epistemic blindness.

While at the time these were admittedly a small minority of the cases and in no

way *dis*proved that the syndrome was significantly linked to homosexuality, this

hypothesis would seem to require some important arguments to establish it as acceptable,

and not merely plausible. First, it would require an explanation of what distinguishes gay

men from sexually promiscuous heterosexuals, from heterosexual drug users, from

hemophiliacs, and from women (straight or gay). Or, it would require an explanation of

why this syndrome is new while homosexual lifestyles are quite obviously not. Or, it

would need an account of the actual physiological processes connecting "risky" lifestyles

with the destruction of the immune system. None of these arguments were provided, yet

---

[135] ibid
[136] Epstein, *Impure Science : Aids, Activism, and the Politics of Knowledge*., 288-9.

the ease with which the hypothesis was initially accepted reflected none of this uncertainty. Ultimately these explanatory gaps would prove fatal to the hypothesis, but not until a scientist with great authority came along to challenge it.

An irony of the "immune overload hypothesis" is that the background beliefs that gave it strength actually were in direct opposition to other prevalent background beliefs in the scientific community. Epstein points out that ever since the bacteriological revolution of the late nineteenth century, two separate assumptions have held sway over biomedical research, and are often conflated with each other. First is the assumption that most illnesses have one necessary and sufficient cause as opposed to several necessary and co-sufficient causes. This view is called "specific etiology."

Second is the methodological assumption that the cause of an illness should focus initially on a microbial explanation, and only after that on lifestyle issues, and finally on qualities of the environment and social organization of society. [137] Thus the early preoccupation of the medical community with the gay lifestyle as the primary explanation for AIDS, over a viral or otherwise physiological explanation, is noteworthy. [138] It illustrates that all background assumptions are not consistent, and can sometimes even serve to elide *each other*.

Monocausal viral hypothesis

The background beliefs about homosexuality that foregrounded the various behaviors perceived as the "gay lifestyle" in scientific research as opposed to monocausal

---

[137] Ibid. , 57.
[138] Epstein attributes some of the facility with which the lifestyle model of causation was adopted to the fact that homosexual behavior has historically already been medicalized as a psychological illness and medically deviant behavior. The medicalization of homosexuality has been widely discussed, most famously by Michel Foucault. As a result, the "gay lifestyle" was more easily assimilated into the explanatory inclinations of the biomedical establishment than would be other lifestyles.

and/or microbial explanations was interestingly inverted later in AIDS research. The

famous (infamous) NIH researcher Robert Gallo, an expert in retroviruses, was skeptical

of the immune overload hypothesis from the outset. In light of my previous discussion,

one important quality of his skepticism was that he charged it with *both* empirical *and*

conceptual problems. Empirically, he noted that it failed to account for the manifestation

of the disease in all of its risk groups (which was simultaneously a debate over the

significance of data), and he also argued for the background beliefs of the scientific

community noted above: namely, that specific etiology was the adequate model with

which to understand illnesses. "Whereas some complex diseases. . .are believed to

involve different steps and sometimes different factors, most human disease (even some

cancers) can be thought of as involving a primary causal factor." [139]

In spite of the biasing assumptions toward framing AIDS as a disease connected

to the gay lifestyle, the increasing number of risk groups that kept emerging, as well as

the scientific clout that Gallo wielded as an eminent virologist, ultimately led to the shift

toward a viral explanation of the disease. Why make appeal to his clout in addition to the

evidence? Because the monocausal viral explanation of AIDS, like the immune overload

hypothesis before it, became entrenched long before it was adequately tested and

contained a certainty unlicensed by the evidence. As we shall see, this led to the

exclusion of alternative hypotheses for quite some time (including those compatible with

the same evidence.)

While Gallo's (and simultaneously Montagnier in France's) work to isolate the

virus responsible for the development of AIDS addressed empirical problems with the

---

[139] Gallo, *Virus Hunting* 148-9 as cited in Epstein, *Impure Science : Aids, Activism, and the Politics of Knowledge*., 69.

immune overload hypothesis, epistemic blindness and social entrenchment later served to

insulate Gallo's viral explanation from criticism in almost exactly the same way. The

immune overload hypothesis had maintained status (deterring criticism) partly due to its

reinforcement of the implicit background beliefs of society (specifically the parts of

society overly represented in the scientific community) regarding the health hazards of a

homosexual lifestyle, and partly due to the social status of gay white men (as objects of

scientific study) over third world people, women, and intravenous drug users. Similarly,

Gallo's monocausal viral hypothesis, that "a virus (later determined to be HIV) causes

AIDS in all infected people by directly killing T cells and that the only co-factor is the

passage of time" [140] became entrenched without eliminating the possibility of cofactors

(due to the prevalence of the assumption of specific etiology) and without accruing

adequate evidence according to widely accepted scientific standards.

To demonstrate the problematic process of acceptance of Gallo's HIV causation

hypothesis, Epstein points to Gallo's initial publication of the causation hypothesis in the

May 1984 edition of *Science*, in which Gallo had an impressive four interconnected

articles about AIDS, as the catalyst. Epstein begins by assessing the conclusiveness of

the causal hypothesis on the articles' merits alone. Epstein conservatively makes appeal

to the causation criteria presented in a recent epidemiology textbook. He then argues that

Gallo's proof that a virus causes AIDS did not adequately satisfy any of them.

> First, there is the strength of the association, which they describe as the "ratio of disease
> rates for those with and without the hypothesized causal factor": here Gallo's evidence is
> compelling but far from perfect, since he was able to isolate the virus only in fewer than
> half of the samples from people actually diagnosed with AIDS. Second, the "dose-
> response relationship": does a higher dose of the causal factor result in higher rates of
> disease expression? Gallo had no data on this point. Third, the consistency of the
> association across different studies: clearly this was yet to be determined. Finally, is the

[140] Ibid., 145.

association a "temporally correct" one, meaning that the cause precedes the expression with a sufficient "induction period" or "latency period"? With the exception of the one virus-positive, clinically healthy gay man who developed AIDS within six months, Gallo had no data to report. [141]

While there *was* evidence that HIV played a key role in the etiology of AIDS, as with the immune overload hypothesis certain implicit background beliefs led to a more specific, and certain, theory than the evidence itself would warrant. In the early years of AIDS research, the hypothesis that a virus was *the* cause of AIDS became largely immune to criticisms before (1) the necessity of the virus was backed by conclusive evidence (according to communal standards) or (2) the sufficiency of the virus to cause AIDS was established at all.

With this background of the initial issues in AIDS causation research, I want to examine how Longino's four social norms: social venues for criticism, shared community standards, equality (or tempered) intellectual authority, and social uptake of criticism, performed in this context. Specifically, I want to show how the first three of Longino's norms were unable to mitigate the problems of the epistemic blindness toward the prevailing background beliefs and the social entrenchment of certain authoritative scientists, and as a result were insufficient to enable the social uptake of criticism.

Social entrenchment

Social venues (peer-review journals) and cognitive authority

After Gallo published his four articles in *Science* in May 1984, a process began in peer-reviewed literature of entrenching the monocausal viral hypothesis that the human immunodeficiency virus, and only HIV, causes AIDS. Epistemically, hypotheses should become more and more entrenched the more evidence accrues supporting them. From

[141] Ibid., 75.

Epstein's analysis, it is clear that the entrenchment of Gallo's hypothesis within the

scientific literature did not correspond to the accumulation of evidence. Epstein

demonstrates this claim initially by doing a content analysis of the scientific articles

citing Gallo's initial hypothesis and its evidence in seven major peer-reviewed journals

from 1984-1986 and examining how many of these articles claimed an increased level of

certainty without an increased level of evidence than Gallo had in May 1984.

Epstein made several striking findings. First, he found that while only 3% of the

articles citing Gallo's research increased the level of certainty without further evidence in

the rest of 1984, this percentage jumped to 25% in 1985 and to 62% in 1986. [142] He also

found that "[e]xpressions of doubt or skepticism—let alone support for other

hypotheses—were extraordinarily rare throughout this period from 1984-1986." [143]

Finally, his analysis lent credence to the charge, made by discourse analyst and cultural

critic Paula Treichler, that "By repeatedly citing each other's work, a small group of

scientists quickly established a dense citation network, thus gaining early (if ultimately

only partial) control over nomenclature, publication, invitation to conferences, and

history." [144] While Treichler did not support her accusation by specific charges or

evidence, Epstein's analysis did discover evidence of circular citation patterns, situations

where one article citied another for evidence, and the latter article cited the first back

again. [145]

---

[142] Ibid., 81.

[143] Ibid., 83.

[144] Paula A. Treichler, "Aids, Hiv, and the Cultural Construction of Reality," in *The Time of Aids: Social Analysis, Theory, and Method*, ed. Gilbert Herdt and Shirley Lindenbaum (Newbury Park, CA: Sage 1992)., 76 as cited in Epstein, *Impure Science : Aids, Activism, and the Politics of Knowledge.*, 80-81.

[145] Peer review has been shown to have institutional (Garfunkel 1994), national (Joyce 1998; Link 1998), methodological (Jadad 1998; Joyce 1998), gender (Dickersin 1998) and outcome biases (Dickersin 1990; Callaham 1998; Misakian 1998). Bias,obviously, runs counter to the value-neutral goal of **research.**

In addition to these social reinforcing dynamics in journals, which are at least partly attributable to Gallo's status in the scientific community rather than merely the power of Gallo's evidence, social exclusionary peer-review behavior can be connected to scientists' corresponding *lack* of reputation. A notable early critic in this regard was a young virologist named Shyh-Ching Lo. In 1986 Lo reported finding a new virus taken from AIDS patients and was able to isolate the agent from six HIV-negative patients who had AIDS-like symptoms. While the *New York Times* gave his discovery a front-page article, the powers that be in the scientific community worked to block him. Over half a dozen journals rejected his work before it was accepted to the *American Journal of Tropical Medicine and Hygiene* in 1989 (three years later!). Ironically, Gallo personally accused Lo of failing to adequately justify his claims. Lo's work did not receive any more scientific attention (either to further prove or disprove it) until Montagnier (once again, a scientist with high social status) independently started examining the role of the virus (which turned out to be another primitive organism called a mycoplasma) in 1990.

If anyone had the legitimate intellectual authority to have his criticisms heeded, it would be Luc Montagnier of France's Pasteur Institute. Other than Gallo, there was no scientist at the time more reputed for AIDS research than Montagnier, whose team arguably discovered the HIV virus (which Gallo may or may not have stolen credit for). In spite of this accomplishment, Montagnier received similar trouble from the scientific community for voicing a cofactor hypothesis at the 1990 International Conference on AIDS. At the conference, Montagnier presented research that showed, much like Shyh-Ching Lo had in 1986, the presence of mycoplasma in a significant percentage of AIDS patients. From this data he suggested that mycoplasma may be a cofactor that, along with

HIV, was necessary to cause AIDS. While not returning to the "immune overload hypothesis", this view suggested that a necessary interaction with other factors, such as lifestyle, environment, or other microbes, had not yet been ruled out by the research up to that point.

In spite of his reputation and evidence, U.S. scientists at the meeting were dismissive of Montagnier's hypothesis, with the AIDS director at the CDC saying, "Dr. Montagnier is out on a limb." [146] Once again, social uptake and engagement was lacking in spite of the fact that Montagnier had the highest status, other than Gallo, in the world of AIDS research, and at least plausible scientific evidence to back his view. Montagnier's frustrated attempts to keep the scientific discourse open to cofactors continued throughout the next decade and a half.

Another interesting example of the important (but also insufficient) role that scientific status plays in enabling peer-review criticism is in the person of Peter Duesberg. Prior to the beginnings of the AIDS epidemic, Peter Duesberg was a prominent researcher on retroviruses and their relation to cancer. He had both mapped the genetic structure of retroviruses and was also the codiscoverer of the first "oncogene"; a special type of gene with the potential to cause cancer. At the time he splashed onto the AIDS research scene, he had authored or coauthored more than 200 professional publications. Between 1975 and 1979, he was cited in published articles more than a thousand times. Gallo, in comparison, had received over 800 citations. Thus, both were eminent scientists in their fields with considerable (and earned) social authority.

---

[146] Epstein, *Impure Science : Aids, Activism, and the Politics of Knowledge.*, 129.

Like Shyh-Ching Lo earlier and Montagnier later, Peter Duesberg was critical of

Gallo's monocausal viral hypothesis. In contrast with Lo, in March 1987 Duesberg was

easily able to publish in *Cancer Research* an article that, while primarily discussing

retroviruses in general, ended with a criticism of the monocausal viral hypothesis in

AIDS research. Duesberg initially pointed to the very clear gaps in the evidence for

Gallo's hypothesis, beginning with the large disparity between the number of people with

HIV and the number of people with AIDS (about .3% in the U.S. at that time). [147]

 Second, he pointed to the inconsistency between Gallo's claim that the retrovirus

would not cause an illness for a long period and *then* become potent and his explanation

that HIV causes AIDS by directly killing T-cells. If the latter were the case, one would

expect that the illness would gradually worsen, not be absent and then appear in full

force. Similarly, Gallo's 1986 study had found the virus in only one of every 10-100,000

T-cells of infected persons, and this doesn't account for the proposed damage since the

body is always producing more T-cells. Finally, Duesberg challenged the absence of

animal models or other possible explanations of the pathogenesis of the disease, since

chimps and monkeys injected with the virus had failed to develop AIDS.

While not providing a counterhypothesis (at that time) about the etiology of

AIDS, Duesberg controversially suggested that HIV was just another opportunistic

infection with mononucleosis-like symptoms that plagued those with AIDS or at risk of

AIDS. The presence of HIV antibodies, rather than the prevailing scientific belief that

this provided the first indication of a future AIDS sufferer, was actually evidence that a

person's body was successfully fighting off HIV. In this way, his views were much more

[147]Ibid., 107.

extreme than those of Lo and Montagnier, who were more modestly suggesting that HIV was necessary but not a sufficient cause of AIDS.

Unlike Lo, Duesberg's first criticisms were easily published in a prominent peer-reviewed journal. All of Duesberg's criticisms in the article were logical and conventional. While it is clear that Lo's work was not amenable to the "social uptake of criticism", perhaps Duesberg's was? In fact, in spite of his scientific status and the initial publication of his views, Duesberg's critical comments were also not engaged by the scientific community, at least not at first. In the last chapter, I showed that Longino advocates recognizing scientific criticism on the same level as novel scientific hypotheses in the scientific community. This norm was ultimately achieved in the case of Duesberg's criticisms, but only through a process that Longino herself would likely have condemned.

The process to uptake

Within the scientific establishment, which by 1987 had almost universally accepted Gallo's HIV causal hypothesis, Duesberg's critical article, in spite of the eminence of its writer and the respectable journal in which it was published, was widely ignored. On the other hand, periodicals from outside the scientific establishment picked up on the dissenting opinion, initially gay publications like the *New York Native* (renowned for unorthodox and speculative theories and intended for a gay audience) and *Gay Community News*, but later in the more mainstream *Atlantic Monthly*, *Bio/Technology*, *Spin*, as well as a British documentary series, *Dispatches*. [148] As word began to spread (largely voiced by Duesberg himself) that his criticisms were ignored because of the ulterior motives of the scientific community (both professional and

[148] Ibid., 111-13.

financial), the popular press began to pick up the story, like *The New York Post* in January 1988 and the *New York Times* and the *Los Angeles Times* shortly thereafter.

Some of these publications were supportive of Duesberg while some were more critical, but the important point is that while the scientific press ignored his criticisms, the lay press engaged him. One traditional response to this state of affairs is that only scientists can adequately distinguish worthwhile criticisms from "hack" criticisms, so the fact that Duesberg held both scientific credentials and (at least initially) voiced a conservative argument that the prevailing hypothesis was lacking in evidence makes his dismissal by scientists all the more puzzling. In addition, the similar dismissal of Lo and Montagnier's criticisms imply that the rejection of criticisms could not have been just a vendetta (legitimate or not) against Duesberg. Once popular pressure arose to a pitch that compelled the scientific establishment to respond, their espoused reasons for not engaging him were even more enlightening.

Gallo was asked to respond to Duesberg's views in a 1988 interview with *Spin* magazine. When asked whether one should keep an open mind about the causality of AIDS, Gallo replied, "No. I don't think anybody needs to keep an open mind on that. It is silly, OK?" When asked to point out any flaws in Duesberg's logic, Gallo answered, "No. He is a good fellow. It's a useless interchange. Really totally useless. He's an organic chemist. . ." When pushed further, he ultimately replied, "Call 5,000 scientists and ask." [149]

Several interesting points are made in this interchange. First, Gallo implies that the case was closed on the causality of AIDS, when according to even relatively lax

---

[149] Celia and Liversidge Farber, "Aids: Words from the Front," *Spin* 1988., 57, 67 as cited in Epstein, *Impure Science : Aids, Activism, and the Politics of Knowledge*., 115-116.

scientific standards this was not true. Second, he did not deny Duesberg's logic or make

any attempt to refute his view, either empirically or conceptually. Rather than refuting

the *content* of Duesberg's challenge, Gallo challenges his *status* as an expert on AIDS, a

dismissive argument we shall see Gallo resorting to often in arguments from the

experience of AIDS patients as well. This was especially ironic because a few years

before, Gallo had introduced Duesberg at a university talk by attributing to him "a rare

critical sense which often makes us look twice, then a third time, at a conclusion may of

us believed to be foregone."   [150]

These same words, originally used as a compliment, seem to have remained true

for Gallo, but had become an insult. The challenge to Duesberg's AIDS credentials was

also interesting due to the fact that as a new disease, no one was technically an AIDS

expert originally. Everyone who came onto the scene with authority came from other

related fields, and the relevance of Duesberg's expertise on retroviruses and cancer for

the case of AIDS was as strong a link as Gallo's.   Finally, Gallo appealed to the number

of scientists who believe that HIV causes AIDS, which we have already seen must be

scrutinized in terms of the circular appeals in peer-review journals.

While it would be unfair to use Gallo's words in one interview to characterize the

attitude of the entire scientific establishment, it appears that they are quite representative.

The editor of *Bio/Technology*, Bialy, wrote a letter in March 1988 to the editor of the

prestigious journal *Science*, which up to that point had been notable in its publication

silence on the issue since Duesberg's initial publication of his criticism. Bialy writes:

> I am very tired of hearing AIDS establishment scientists tell me they are "too busy saving
> lives" to sit down and refute Peter's arguments (although each one assures me they could
> "do it in a minute if they had to" . . .I urge you to use your offices to get Fauci or Gallo or

---

[150] Epstein, *Impure Science : Aids, Activism, and the Politics of Knowledge.*, 116.

Levy or Hazeltine, or Essex to prepare a rebuttal of Peter's arguments that is as carefully argued and referenced as his paper in *Cancer Research*[151].

The blessing and curse of exile

Unlike Lo and Montagnier, Duesberg refused to be ignored. When he could not achieve uptake within the scientific community, he began to disseminate his views more widely. As he continued to be marginalized by the scientific community, he star began to rise outside of it. While Duesberg's early views were carefully argued and referenced, his increasing appeals to the gay community and lay press for advocacy not only gave his criticisms an audience, but also began to change the timbre of his views as well. Duesberg continued to get more virulent and extreme, culminating in the infamous offer to inject himself with HIV to demonstrate its harmlessness. While he had begun in his initial article merely questioning the adequacy of the evidence for the HIV hypothesis, in time he went on to argue with just as little evidence for alternatives such as long term drug use as the cause of AIDS and that AZT was "AIDS by prescription." [152] Epstein tries to make sense of this apparent hypocrisy.

> Perhaps comments such as these were intended only to goad his critics and were not supposed to be taken seriously. Or perhaps by this point [1992] Duesberg was so embittered by the behavior of his scientific colleagues—who, he believed, had blackballed him, tried to silence him, and succeeded in cutting his funding—that he was willing to employ any rhetorical device at his disposal to cast doubt on the worth of their accomplishments.[153]

Further, the early alliance that formed between Duesberg and John Lauritsen, a survey researcher and frequent reporter for the *New York Native*, helped push Duesberg away from his critical roots towards a more reckless speculative mood. Lauritsen, who was a strong advocate of correlating AIDS to the gay lifestyle, against AZT as a

---

[151] Ibid., 123.
[152] Ibid., 149.
[153] ibid

treatment, and who made the first lay press reference to Duesberg's views, strongly

encouraged Duesberg (whose scientific credibility he needed) to advocate

counterhypotheses to his liking. Epstein relates an interesting interview by Lauritsen in

1987 where Duesberg starts out agnostic about the causation of AIDS ("Well, that's a

difficult one. . .I really wonder what it could be") and by the end of the discussion shifts

to an endorsement of a judgmental version of the immune overload hypothesis.

[Lauritsen:] Looking at that profile. . .I think it would be surprising if such people did not
become seriously sick from their lifestyle.

[Duesberg:] I would be surprised, too. The number of contacts, the number of things they inject.
You wonder how they could do it.[154]

The overlapping but divergent interests between AIDS scientists and different lay

groups is interesting to note here. Not only were the gay and popular press interested in

good copy and spectacular headlines, but the gay community in general had a vested

interest in supporting heretical views in the scientific establishment, although one would

think that they would be resistant to heretical views blaming AIDS on the gay lifestyle,

like Lauritsen's. But desperation breeds strange bedfellows, and by 1988, there was

increasing frustration at the slow rate of progress in explaining or treating AIDS, as the

numbers of infections continued to climb and the scientific word on the outlook for those

infected continued to darken. As a result, the gay population used its considerable social

clout and activist impetus to support the exploration of radical theories, even those that

were detrimental to the gay cause in other ways.

[154] John Lauritsen, "Saying No to Hiv: An Interview with Prof. Peter Duesberg, Who Says, "I Would Not Worry About Being Antibody Positive,"" *New York Native* 1987., 21 as cited in Epstein, *Impure Science : Aids, Activism, and the Politics of Knowledge*., 111.

Lay involvement, while giving voice to scientific dissenters, also served to further dissuade the scientific community from engaging dissent. The National Institute of Health's Anthony Fauci argued in a 1988 *Science* article that "Many AIDS researchers refuse to comment publicly because they fear it will legitimize Duesberg."[155] This was a fear, founded in many cases, that the lay press and lay readership tend to see any dissent as good dissent. On the other hand, it does not appear that in Duesberg's situation there was a good alternative. Somewhat ironically, a member of the President's Commission on the HIV Epidemic told Duesberg, "I would hope that you would press your theory within the scientific circles and not carry this uncertainty to the public. . . Don't confuse the public—don't confuse the poor people who are suffering with this disease."[156] Since it was precisely Duesberg's frustration at the lack of engagement by the scientific community that led Duesberg to engage the public in the first place, this charge is at least disingenuous.

It was only after this harrowing period with the scientific community failing to recognize Duesberg's criticisms at every turn and the lay communities strongly advocating for him did the atmosphere start to shift. Not so much because Duesberg gained credibility within the scientific community, but rather that the scientific community continued to lose credibility to the public as their lack of engagement with Duesberg went longer and became more public. In April of 1988, AmFAR sponsored a forum and invited a range of panelists representing different views about the AIDS epidemic, including Duesberg, Anthony Fauci, and a Berkeley anthropologist named Warren Winkelstein.

[155] Epstein, *Impure Science : Aids, Activism, and the Politics of Knowledge*., 119.
[156] Ibid., 119-120.

Winkelstein presented studies through which he demonstrated (finally) how the causation relationship between HIV and AIDS could be established up to the standards of scientific criteria. This point is interesting because it was precisely the lack of this evidence in Gallo's initial studies to which Duesberg pointed in his initial paper, and one wonders whether the issue would have ever been further addressed if not for Duesberg's vigilant, if often irresponsible, opposition. One also wonders whether if Duesberg's initial point had been initially acknowledged, whether he would have found it necessary to continue to make his counterposition a cause, rather than a question.

Another interesting aspect of this forum was that it attracted much more attention from scientific publications than from the mass media. Epstein suggests that this is because the popular attention given to Duesberg was contingent upon the suspicious lack of scientific engagement with him. Once the scientific community engaged him, his often-unquestioned popular support began to shift. [157]

In addition to this forum, *Science* ran a "Policy Forum" in its July 29, 1988 issue in which it ran a statement from both sides of the "debate," along with each side's response to the statement of the other. In it, Gallo and his like-minded colleagues argued for the crucial distinction and explanatory independence between etiology and pathogenesis (the former of which is HIV, the latter of which what Duesberg charged them with not explaining). They also provided more epidemiological evidence of HIV being present in a statistically significant percentage of those people who developed AIDS, seeing this as conclusive evidence for the hypothesis. Duesberg provided counterarguments to these views, and remained unconvinced into the future (becoming more dogmatic and recalcitrant as time went on), but the crucial point is that the critical

[157] Ibid., 122.

debate was finally engaged *within* the scientific venues and justifications were finally

provided for many of the assumptions and inferences that before had gone unnoted.

The debate issue in *Science* and the AmFAR forum were both perfect examples of

Longino's social venues for criticism, where

> critical activities should receive equal or nearly equal weight to "original research" in
> career advancement. Effective criticism that advances understanding should be as
> valuable as original research that opens up new domains for understanding; pedestrian,
> routine criticism should be valued comparably to pedestrian and routine "original
> research."[158]

Longino does admit that "[peer-review's] confidentiality and privacy make it the vehicle

for the entrenchment of established views."   [159] While I would be inclined to argue that it

is the social entrenchment that is reinforced by journals which is responsible for their

conservative nature, the more important point here is that critical venues were finally

achieved within the scientific community, but we have also uncovered the conditions

behind the enactment of this norm.

These two venues were not achieved until a year and four months after

Duesberg's initial criticism was published. Not only was this an incredible delay in the

context of a scientific discourse that was only seven years old, but it also would arguably

never had happened had the chain of pressure not come from the popular press, back to

popular scientific journals, and up the ranks to the highest echelons of science. The

impetus to engage criticisms and to create venues for this engagement were not only *not*

norms originating within the scientific community, but they required compulsion from

those who, according to many scientists, do not share scientific community standards nor

do they share intellectual authority. Further, in spite of the opening of the debate in 1988,

---

[158] Longino, *Science as Social Knowledge : Values and Objectivity in Scientific Inquiry.*, 76-7.
[159] Ibid., 76.

the scientific community swiftly thereafter considered the case closed and returned to its normal stance.

Lessons to be learned. . . lay involvement or scientific engagement?

While the orthodox position of the scientific community remained entrenched and in most quarters adamantly unenthusiastic about engaging dissent, one lone voice seemed to have learned a lesson from the Duesberg situation. John Maddox, the editor of *Nature*, in the September 1991 issue made reference to two studies that suggested autoimmune mechanisms were present in the development of AIDS and how this information could be evidence for Duesberg's position. By 1991 Duesberg was largely considered a scientific pariah and his views were immediately dismissed.

As a result, a reporter for *Science* later interviewed Maddox and asked him why he would endorse as heterodox a figure as Duesberg in his article. Maddox responded, "I'm not for a minute saying that Duesberg is right in all points. But I feel sorry that *Nature* has not done more to give his view prominence. It would have hastened the process by which the scientific community is coming around to the view that the pathogenesis of AIDS is more complicated than the baby-talk stories [monocausal] we were all given a few years ago. [160] The hastening of this "process" to which Maddox refers would seem to be a simple request for continued critical debate on hypotheses with still inconclusive evidence. While much can be argued about the "shared standards" of the scientific community, perpetuating the possibility of criticism and debate (as I have argued) seems to be a noncontroversial one. The norms for maintaining this possibility theoretically arose in the last chapters, but here it arises in a practical vein.

---

[160] Joseph Palca, "Duesberg Vindicated? Not Yet," *Science* 254, no. 18 October 1991 (1991)., 376 as cited in Epstein, *Impure Science : Aids, Activism, and the Politics of Knowledge*.

Several important lessons can be gleaned from the history of scientific acceptance of and instances of dissent from early hypotheses about AIDS. First, while Duesberg's and Montagnier's scientific status at least got their dissent voiced in scientific venues (as opposed to the case of Shyh-Ching Lo), even their clout was not enough to get these criticisms engaged by a scientific community that no longer recognized the assumptions behind their accepted hypothesis. The social mechanisms of entrenchment in peer-reviewed journals, the fact that Gallo's view enforced the scientific background beliefs about monocausality and microbial explanations, and the at least equal clout that Gallo had vis-à-vis Duesberg were sufficient in this case to keep the content of these criticisms from being addressed. In this example, Longino's call for the social uptake of criticism was not practiced by the scientific community until it was initiated and enabled by the nonscientific press and lay activists.

Granting that the uptake of relevant criticism is a constitutive value for science, two different implications can be read from the social barriers to scientific uptake in the early years of AIDS research. Either the lesson is that scientific norms should advocate the recognition of the lay press and activists in scientific debates (for epistemic and not only political reasons) to increase the social uptake of criticism, or else a scientific norm should enforce an engagement with heterodox views from within scientific venues. I will argue that the latter is a better social norm. Although the power of the lay people in the debate *did* enable marginalized scientific voices to be heard, it also led to many important difficulties.

While the popular press served as a good watchdog for the extremes of scientific malpractice in the case of AIDS, it also served to encourage and lend weight to views,

like Duesberg's later ones, that lacked evidence as well. As an evidence-based practice, the ideals of the scientific community regarding the etiology and pathogenesis of AIDS were best served by those attuned to evidence that pertains to these questions, and these were usually specialists from within the scientific community. In addition, the dynamic that led Duesberg to give up on scientific venues and to engage the popular press was also the dynamic that led him to turn from a skeptic into an iconoclast. By being ignored and dismissed, his moderate empirical argument against a hypothesis turned into a war against the scientific establishment.

As a result, I would argue that the "bias" toward heterodox scientific views, practiced by the gay community and the popular press, each for their own reasons, should be adopted within the scientific community for epistemic reasons. While in the AIDS situation it may turn out that Montagnier, Lo, and Duesberg were all ultimately wrong, the history of science provides many examples where those scientists that challenged the orthodox position were responsible for the most significant innovations. [161] The price paid for ignoring renegade scientists is much greater than the possible extraneous attention given to criticisms that ultimately turn out to be unsuccessful. Further, since the institutions of science often serve to make orthodox positions seem more credible than they are, it seems logical to assume that heterodox positions may initially appear less credible than they actually are. This seems the only likely explanation for the dismissal of the criticisms of Duesberg and Montagnier, who arguably couldn't have had much more intellectual authority in their fields when they made their criticisms, but were nonetheless ignored.

---

[161] We saw many examples of this in Chapter 1.

While Longino advocates critical venues and equalizing intellectual authority to encourage the uptake of criticisms, she does not respond to this particular imbalance in her norms. This is why, in the case of AIDS research it was necessary for espousers of heterodox views, like Duesberg, to balance out their authority with factors outside the scientific community. The problem with relying too much on lay press to provide the impetus for this "bias" is that this requirement should only be initial and provisional. As Maddox argued, the requirement to engage more heterodox scientists is not to agree with their views uncritically, but to encourage the process of complicating and challenging scientific hypotheses that all too easily become entrenched due to the dynamics we have seen. [162]

To counteract this imbalance a norm is necessary to weight heterodox views, at least initially, in order for them to be entertained in an environment institutionally inclined to exclude them. Using these marginalized criticisms as a "starting point", as standpoint theorists advocate, would merely force the scientific community to evaluate its orthodox hypotheses from the outside, turning implicit background assumptions into explicit beliefs that can then be further justified or rejected in the face of criticism.

As examples of this, forums like the AmFAR dialogue and the debate issue of *Science* are crucial. These critical venues, as Longino argued, should not be rare occurrences resulting from popular demand, but rather institutionally required scientific activities that should be performed on a regular basis. On the other hand, these are critical venues *among scientists*, and rely heavily on the peer-review process, made

---

[162] It is important to note that neither epistemic blindness nor social entrenchment are the result of *intentional* malpractice by scientists, either due to active bias or active politicking. Rather, I am arguing that these are natural and commonly unwitting results of the general sociology of scientific communities that must be addressed, and not blamed.

explicit, to engage criticisms that meet (or at least responsibly engage) scientific community standards.

But here we must be careful not to be too broad. As we shall see in the next chapter, the balance between the advantages and the perils of including the lay community in scientific discussions depends significantly on the types of questions asked, and the types of implicit assumptions that lie behind them. The point is to counteract epistemic blindness and social entrenchment in scientific research, so in each case understanding the particular dynamics of is necessary. Significantly in the debate about the etiology of AIDS, while the lay press and the gay activists were involved, they were largely involved insofar as they backed one or another *scientific* figure that was socially marginalized and was challenging scientific assumptions (regarding specific etiology, statistical relevance, etc.). This was partly for the epistemic status that came with scientific credentials in the lay world, but also because there was recognition by many that expertise on these types of scientific matters should be granted to scientists. The question was only, which scientists?

As we turn to the scientific debates around the treatment of AIDS, the dynamics, and thus the resulting norms, will significantly shift. In this discourse, the questions and the background assumptions behind treatments directly involved the experiences, the activities, and ultimately the expertise of the people outside the scientific community who would be involved in treatment research, the most prominent model of this being clinical trials. In this situation, we shall find that the dynamics of social entrenchment and epistemic blindness are enacted not between different scientists, but rather between the scientific community and the AIDS-affected communities. Thus, while the former

discussion is more closely akin to the social dynamics of the natural sciences in general,

this set of issues is more akin to those found in the social sciences.

<u>Chapter 5:</u> <u>AIDS Case Study:</u> <u>Part II</u>

AIDS treatment research

Introduction

Unlike the case of AIDS etiology research I examined in the last chapter, the orthodox views regarding AIDS treatment research, specifically in the methods and models of clinical trials, were largely shared by the entire scientific community involved with AIDS research. The heterodox views that challenged with their implicit and entrenched background assumptions were not those brought out by heterodox scientists, but rather views espoused by AIDS activists. This is an important shift because it points to a crucial disanalogy with the case as discussed in the last chapter. I indicated the problem of relying too heavily on lay criticisms in the case of AIDS etiology research, since these criticisms were ultimately backing heterodox *scientific* views which, once heeded, were best criticized from within the scientific community.

While in the scientific discourse regarding the etiology of AIDS, social entrenchment and epistemic blindness were manifested *between* different members of the scientific community, here the input that brings scientific assumptions to light and criticizes them is not from scientists, but from AIDS activists. Of course, AIDS activism played a prevalent role in both discourses, but as I argued in the last chapter, the main effect of extra-scientific engagements in AIDS etiology was to draw popular attention (and ultimately scientific attention) to the viewpoints of scientists that were being marginalized. In this case, AIDS activists drew attention to the content of AIDS *patients'* perspectives that were being ignored.

As I will show, in the case of AIDS treatment the lay criticisms were bringing contributions to the scientific questions from sources that had no counterpart within the scientific community. The collective assumptions about the necessity of placebo trials, restricted access, and the predominant inclination toward academic questions were not only held by the upper echelons of AIDS researchers, like the monocausal viral model, but were certain standards and norms that were shared by the scientific AIDS community itself. Moreover, these standards were often shared by the broader scientific community as well. The experiences, interests, and background beliefs of those afflicted by AIDS, on the other hand, were conditioned by their position as AIDS patients, homosexuals, drug users, etc. It was as a direct result of these experiences, as opposed to experiences within the scientific community, that scientific standards were challenged, and alternative possibilities emerged.

Since it is more controversial to argue that nonscientists have relevant contributions to science than to argue, as in the last chapter, for the legitimate voice of already credentialed scientists, I will spend the first part of this chapter illustrating the various ways that AIDS activists challenged epistemic blindness and were able to enable better scientific procedures (by scientific standards). I will then analyze this case in terms of how AIDS activists were able to overcome the social entrenchment of scientific standards while other AIDS affected demographics were not. In the next chapter, I will examine the implications of this case for current scientific values and practices.

Brief history of AIDS treatment research

In the United States, the complex interaction between drug distribution and scientific analysis historically began with the Pure Food and Drug Act in 1906 and the

Food, Drug, and Cosmetic Act in 1938. These two Acts empowered the FDA to regulate

drug manufacturing by requiring evidence from "adequate tests" before drugs could be

made available for popular consumption. After the outrage over Thalidomide's effects in

1962, Congress passed an amendment shifting the emphasis of drug regulation even more

heavily on scientific proof of efficacy. These different laws, interacting with the FDA's

engagement with biostatistics and the National Institute of Health, resulted in a

methodological orthodoxy for clinical trials.

The FDA required evidence from three distinct phases of clinical trials before

endorsing a drug for distribution. Phase I used a small set of people to determine the

drug's toxicity and an adequate dosage for drug absorption. Phase II was larger and

analyzed the drug's efficacy, and finally Phase III was the largest trial which compared

the efficacy of the drug with other treatments for the same condition. [163] Trials are

composed of a random selection of "pure" subjects who are then anonymously assigned

to either the "treatment" or the "control" group of the trial. Only after passing all three

phases could a drug be considered "adequately tested" and legally be distributed.

This was the model that was sponsored by the National Institutes of Allergy and

Infectious Diseases (NIAID) to be used in the forty-one AIDS Clinical Trial Units

(ACTUs) implemented around the U.S. By January 1990, there had been a total of 99

ACTG trials, the majority of which (85.8% of test subjects) involved testing antiretroviral

drugs, with the next highest category testing opportunistic infections (17% of test

subjects). [164]

---

[163] Epstein, *Impure Science : Aids, Activism, and the Politics of Knowledge.*, 185-6.
[164] Carol Levine, Nancy Neveloff Dubler and Robert J. Levine, "Building a New Consensus: Ethical Principles and Policies for Clinical Research on Hiv/Aids," *IRB: Ethics and Human Research* 13, no. 1/2 (Jan - Apr 1991)., 4.

Corresponding to the use of this clinical model, an ethical model has also developed. There has been an explosion of publicity in the latter half of the 20th century exposing abuses and manipulations of test subjects in clinical trials like the Tuskegee Syphilis study and secret trials withholding penicillin from servicemen to study alternative therapies. As a result, in 1974 Congress created the National Commission for the Protection of Human Subjects to determine ethical guidelines for research. The NIH also required the ethical approval of institutional review boards before research could receive federal funds.

The intentions of both sets of Congressional acts were aimed at ensuring more objective and ethical biomedical research, but the objectivity and ethicality of both of these models would be brought into question within the larger discourse of AIDS treatment in the early years. This was largely achieved through the work of AIDS activists to bring to light the implicit assumptions shared by the scientific community which were presupposed by these clinical trials and ethical models. These activists were able to challenge the social entrenchment of many of these background beliefs by gaining epistemic leverage for their own models and frameworks.

Epistemic blindness and AIDS activist contributions

Background belief 1: Paternalism

The first and most obvious assumption behind both the scientific and ethical standards of clinical trials was a (benevolent) paternalistic relationship between the scientific community and research subjects. In the medical context, paternalism is the principle that the medical community, due to its greater knowledge, has the right and the duty to determine the best interests of patients, and then to act in those best interests

whether or not the patients recognize and consent to these actions. While paternalism

rarely is manifested in its purest form, aspects of the principle are prevalent throughout

medical research and treatment.

AIDS activists challenged this paternalism by arguing in some cases that patients

should have the right to assume *greater* risks than were deemed ethical by the scientific

community, such as instances where they demanded the distribution of drugs that had

only passed Phase I or Phase II clinical trials. In other cases they argued that the

scientific community was ignoring and/or failing to seek out methodological alternatives

that offered *lesser* risks to their populations, such as alternatives to the risky waiting

game of taking a placebo instead of trying an alternative experimental treatment.

Rather than exhibiting an apparent inconsistency (which should be the policy,

greater risk or lesser risk for AIDS patients), the point most activists were making was

that risk assessments and patient "protections" were being decided entirely by

governmental and medical authorities, while the right to determine "acceptable risk"

should reside in the people assuming the risks.

> What scientists interpret as a naïve and impracticable public expectation of a zero-risk
> environment can thus be seen instead as an expression of zero trust in institutions which
> claim to be able to manage large-scale risks throughout society. These social dimensions
> of risk and trust are the general context within which specific issues are played out.[165]

Taking inspiration and advice from their feminist contemporaries, two distinct

types of arguments were hidden in this activist push for patient autonomy. The first was

a stakeholder-style argument that since it was in the end the patients' lives on the line, it

was their political right to decide what was to be done with it. If an AIDS patient is

---

[165]Alan Irwin and Brian Wynne, "Conclusions," in *Misunderstanding Science? The Public Reconstruction of Science and Technology*, ed. Alan Irwin and Brian Wynne (New York: Cambridge University Press, 1996)., 218.

informed and chooses to participate in a high-risk clinical trial or take an experimental

drug, the government should not refuse them this right, even in the name of the patients'

own best interests. This type of argument had been made many times before, and

although powerful and useful, it was not the most novel, or effective, type of argument

used by the AIDS activists to challenge the research establishment.

While having a life-or-death stake in the outcome of AIDS research was behind

the *interest* that AIDS activists had in the research, they also argued that this interest was

grounded in a specific *expertise*. As a result of their first-hand experiences with AIDS, as

well as with gay communities, the medical establishment from the patient's perspective,

and being subjects of clinical trials, AIDS activists argued that they had accrued

knowledge relevant to ascertaining the best possible treatment, and they should be

attributed credibility as a result. In other words, this argument is more than the

stakeholder claim that patients have a right to do what they subjectively "want" with their

lives, but that patients in many cases have relevant knowledge to determine a more

generally "better" response to AIDS treatment questions in their communities.

While some aspects of treatment research were strictly biomedical, most

biomedical claims presupposed background beliefs about the crucial questions to ask, the

optimal methodologies to use, criteria for acceptable answers, and interpretation and

application of the results (Kuhn's paradigms, again). AIDS activists would demonstrate

that they had important expertise that did not occlude scientific expertise, but had much

to inform it, on most of these issues. Behind this argument was a basic questioning of the

paternalistic belief that presumes that human subjects of scientific research are analogous

to the subjects of scientific research of the natural world, where research is only *done* by the scientists and is only *about* the domain they study.

Background belief 2: Restricted access is necessary for motivated enrollment

The essential purpose of a clinical trial for researchers is often the obvious aim of advancing justified knowledge about a given treatment. On the other hand, for many people who suffer from illnesses, clinical trials are first and foremost a means of access to drugs otherwise unavailable either for economic reasons, geographical reasons, or because scientific and governmental institutions have yet to deem the treatments adequately tested. [166] As AIDS Activist John James noted, "Scientists who run clinical trials are interested in maintaining scientific standards, in doing studies correctly so that they get solid, trustworthy results. People with life-threatening illnesses, on the other hand, are interested in using whatever knowledge is available to make the best treatment decisions they can. [167]

While the dual role of subject and patient held by people involved in clinical trials was not invisible to anyone, the different frameworks and intentions involved on both sides led to interesting conflicts. Researchers often saw the ethical dilemma as choosing the best interests of society over the best interests of an individual patient. They argued that while unrestricted access to experimental treatments may be best in the short run for a given AIDS sufferer desperately in need of any possible help, it would ultimately impede science's ability to get enrollment in clinical trials (why would people enroll if not compelled by their need for the drug?) Successful clinical trials, in turn, would produce the greatest good for the greatest number of people affected by AIDS.

[166] Epstein, *Impure Science : Aids, Activism, and the Politics of Knowledge*., 197-8.
[167] Ibid., 267.

   AIDS activists morally contested the use of restricted access to coerce people into studies, but they much more efficiently argued that the the assumed link between restricted access and populated clinical trials was unfounded. Martin Delaney, Executive Director of Project Inform, argued "[i]f patients had other means of obtaining treatment, force-fitting them into clinical studies would be unnecessary. Volunteers that remained would be more likely to act as pure research subjects, entering studies not solely out of a desperate effort to save their lives."   [168] Activists pointed out that there were other motivations for people to participate in clinical trials, such as altruism, financial compensation, and access to free, high quality medical care. Participants in clinical trials often received more careful monitoring and responsive care than those outside of them. And contrary to the assumptions of the medical establishment, activists' claims about their own communities turned out to be correct.

   It is important to note that the content of the scientific assumptions here were about the psychology of AIDS patients. These beliefs would be justified based on knowledge of what would and would not be an adequate motivation for someone afflicted with HIV. This required a certain background and experience, but once we see the specific knowledge needed, it is also clear that scientists were not necessarily the most qualified people to have this particular type of knowledge. It appears commonsensical that the background and experience necessary to have an understanding of the psychological motivations of those with HIV would be found among those who had HIV themselves or those who had direct experience with those who had HIV. Scientific background about the etiology of AIDS, which treatment researchers often had, does not qualify someone to have knowledge about the motivations of someone exposed to that

[168] Ibid., 228.

disease. Thus, it is clear that AIDS patients, especially the vocal activists, had direct

experience regarding these *scientific methodological* decisions in ways that scientists

themselves did not.

As a result of these contributions, new "parallel track" programs, also known as

expanded access, developed. For decades, there had been a "compassionate use" policy

in place where access to unapproved drugs was provided to terminal patients whose risks

taking experimental drugs were therefore negligible. But in 1989, the scientific

establishment decided to recognize the merit of activists' arguments and instated a

"parallel track" program that would provide access to drugs in the midst of clinical

testing to those who "were unwilling or unable to participate in the normal clinical

trial." [169]

AIDS activists were also instrumental to the FDA adoption of "Treatment

Investigational New Drug Applications (INDs) by which drugs could become available to

patients prior to marketing approval to persons who might benefit, provided that the

disease is serious/life-threatening, there are no satisfactory alternatives, the drug is also

being tested by a clinical trial, and the sponsor is actively pursuing marketing approval.

This significantly accelerated the process by which drugs became available to patients in

dire need, and also increased the amount of information available about the effects of

these drugs. [170]

Background belief 3: Either placebo trials or "messy" science

The placebo model of clinical trials, initially employed to study the effects of

AZT, also carried with it important scientific assumptions.

[169] Ibid., 236.
[170] Levine, "Building a New Consensus: Ethical Principles and Policies for Clinical Research on Hiv/Aids.", 5.

> The RCT [randomized control trial] is the most reliable method for developing information about the effects of therapies, preventions (e.g., vaccines), and diagnostic tests. It is more reliable primarily because random allocation mitigates the effects of bias in the assignment of subjects to one or another of its "arms," or regimens. . .Furthermore, the RCT is reliable because unknown variables (attributes of patients) that might influence the outcome of therapy tend to be distributed evenly among the two or more "arms." This further reduces bias in the results.[171]

AIDS treatment activists also actively criticized the assumptions behind this general argument. Ethically, clinical trials can only be employed in situations with "clinical equipoise", where there is a legitimate dispute as to which treatment or non-treatment is superior. In addition to arguing directly on a moral plane against placebo clinical trials, most activists also made appeals to extant scientific standards for their criticisms.

One line of argumentation against placebo-controlled trials by AIDS treatment activists was against their practical efficacy. Doubts were raised about the possibility of the subjects and investigators actually being ignorant (as protocol requires) of which arm of the trial a patient is in, since the side effects of AZT were difficult to ignore. There was also the practical problem of the noncompliance of many desperate patients who were anxious that they were swallowing sugar pills and sought creative ways of getting other medications on the sly. Noncompliance had long plagued the reliability of clinical trials, but attempts to resolve it framed the issue as disciplining unruly patients, rather than understanding patients as making rational choices from their own perspective and attempting to align the interests of patients and researchers.

While much of the biomedical establishment adhered to the rigorous, and time-consuming, process of placebo-controlled trials as a necessary evil, by the mid 1980's many researchers from other areas, including community physicians and patient

---

[171] Ibid. 5-6.

advocacy groups, challenged the assumption that placebo trials were always necessary and advocated alternative research protocols. Many AIDS activists, like New York cancer researcher and co-chair of AmFAR Mathilde Krim, argued that placebo controls were not the only way to perform a controlled study. By questioning the unquestioned scientific assumption that the choices were either rigorous placebo-controlled trials or problematic anecdotal evidence, Krim argued that data from treatment groups could be compared to "historical controls", or the medical records of those AIDS patients who had been followed in the past, or the medical records of those enrolled in the active trial *before* they started taking AZT. Although not unproblematic procedures, these alternatives worked well in many situations and had never even been attempted in AIDS research up to that point.

Another aspect of placebo-trials challenged by AIDS activists was that the clinical advance must be measured by contrasting improvement in the treatment group with the deterioration of people in the control group. Since deterioration moves at the pace at which the disease affects the body, this advancement in the case of AIDS was almost always slow. Activists challenged this assumption by arguing for a methodology used with other life-threatening diseases that had never been considered in AIDS research. This was the use of a "surrogate marker", in which a particular positive effect of a drug in the body is considered to correlate to potential effects on the disease in question. Although this methodology was fundamentally more uncertain than the traditional mode since it relied on the supposition of how the drug would ultimately affect the disease (i.e. via the marker chosen), this was often seen as an acceptable trade-off for the accelerated speed by which results could be achieved.

Several alternatives methodologies to standard control placebo trials were also developed during this time. Activists and researchers on each U.S. coast worked to develop their own alternative research models that avoided placebo use. In San Francisco, a group of community-based physicians collaborated with researchers at San Francisco General Hospital and UC-San Francisco to form the "San Francisco Community Consortium." These primary care physicians whose practices largely ministered to AIDS patients would integrate drug distribution, patient monitoring, and data collection with their primary care work with their patients. This alternative protocol had several advantages. Monitoring and follow-up was easier since long-term relationships between the doctors and their patients had already formed. If patients chose to stop participating in the study, the statistical outcomes could still be observed. In addition, doctors tended to have more face-to-face experience with their patients and the disease, which enabled a better fit between patients and drugs, better outreach, and better compliance.

The model developed in New York City by the New York City Community Research Initiative was an even more collaborative approach. In a model that has since been utilized by public health, environmental sciences, and many social sciences, a group of community physicians and their patients developed a model that involved the subjects of research in the conceptualization, development, application, and interpretation of AIDS research.

> From the start, people with AIDS or HIV infection participated in decision making about what trials CRI [Community Research Initiative] should conduct and even how they should be organized, '[setting] policies on placebo use, and [insisting] that trials under

[CRI] sponsorship be effectively open to women and minorities, not only to gay men.'"[172]

These alternative approaches both challenged the scientific researchers' assumptions that rigorous drug trials could only be achieved through their accepted protocols, and simultaneously challenged the social entrenchment of the credibility of traditional researchers over that of cooperating community doctors and activist AIDS patients. [173] In addition, both of these models were able to avoid the required hospitalization and sophisticated laboratory work required by the clinical model. They focused more on testing therapies for opportunistic infections (a more urgent need for current patients than retroviral therapies), and they also were able to recruit and retain test subjects more easily. [174]

In 1989, after activists on both coasts had become frustrated by the red tape slowing the testing of a drug that showed promise in preventing pneumocystic pneumonia, they launched their own tests with three different treatment arms and no placebo. Based on the results of these community-based research results, the FDA approved the drug, which was the first time it had given its approval based solely on community-based research.

Once again, the methodological belief that rigorous research could (in almost every case) only be achieved through comparison with placebos in traditional ways

---

[172] Epstein, *Impure Science : Aids, Activism, and the Politics of Knowledge*., 217. The New York City Community Research Intiative is an early example of Community-based Participatory Research, which I will discuss at length in Chapter 6.

[173] As noted above, this alternative approach was not considered appropriate for all types of scientific research. Epstein comments that "Community-based research was not suited for high-tech trials requiring sophisticated lab tests that the average primary-care physician did not have the equipment to perform."Ibid., 217. I would also add that it isn't appropriate in those cases where the experiences and knowledge of lay communities is not directly at issue.

[174] Levine, "Building a New Consensus: Ethical Principles and Policies for Clinical Research on Hiv/Aids.", 4.

presupposed both (1) that there were no good alternatives to standard placebo trials and also (2) there were no good reasons to look for alternatives since they could only lead to "worse" science. AIDS activists challenged these beliefs by bringing to light scientific values that were sacrificed in many cases due to unquestioned loyalty to placebo trials, such as the efficiency of research, the reliability of outcomes (requiring compliance among participants), and the minimizing of risk to participants. Their common interest in these virtues led AIDS activists to more vigorously pursue alternatives like surrogate markers and historical controls, many of which could be found in other scientific fields such as cancer research. In addition, AIDS activists challenged the assumption that all science was best practiced within the walls of academic institutions and showed how collaborative scientific methods in lay communities could lead to reliable, efficient, and less risky outcomes. [175]

Background belief 4: "pure" subjects are better than "complex" subjects

While AIDS activists sometimes challenged placebo trials as fundamentally immoral practices, they more often held a more nuanced position. As Jim Eigo put it at a 1989 symposium, "If every arm of every trial asked a question of real importance to people with acquired immune suppression, enough of those people would find every arm of every trial a viable treatment option and, therefore, if they knew about the trial, could be accrued for that trial." [176] In other words, activists did not have a problem with placebo trials *per se*, but rather with what they perceived as *unnecessary* placebo trials in light of the real-life scientific questions at issue and existing alternatives. Traditional biomedical

---

[175] SEE "An approach to the validation of markers for use in AIDS clinical trials" Donna et al. Mildvan, "An Approach to the Validation of Markers for Use in Aids Clinical Trials," *Clinical Infectious Diseases* 24 (1997).
[176] Epstein, *Impure Science : Aids, Activism, and the Politics of Knowledge.*, 251.

research questions were often seen as too academic and too antiseptic to address the day-to-day realities of patient care and patient needs, and thus there was very little motivation, and later much anger, at the design of the resulting trials.

Under the umbrella of criticizing "academic" scientific research was another background belief that came under fire by activists. Entrance into clinical trials at the outset of AIDS research usually required that the patient (in whatever arm of the trial they would belong) be "pure." A pure subject was one who was not currently and at no prior time had taken treatments, whether the ones to be studied in the trial or alternative treatments. The assumption went that only pure subjects could produce clean data, untainted by the unaccounted influences of other drugs. A San Francisco activist named Terry Sutton argued,

> The idea of clean data terrifies me, because it punishes people for trying to treat early. My roommate. . . has made the decision not to treat early because of the pure subject rule. What he says is 'I want to be a pure subject so that I can get access to the best protocol once it starts to move.' You only get to be a pure subject once."[177]

One example of this dilemma was where a patient was asked to choose between taking AZT and enrolling in a trial for a drug to prevent his eye infection from blinding him. This patient and his doctor actively pursued his interests and ultimately became involved in a new trial that tested the interaction of these two therapies, illustrating that important information could be gained without requiring the patient to sacrifice one treatment for another. [178] Further, biostatisticians supported activists' interests by arguing that, if properly randomized, there was no statistical barrier to studying "unclean" subjects, and moreover there was no statistical barrier to participation in multiple trials.

---

[177] Tim Kingston, "Justice Gone Blind: Cmv Patients Fight for Their Sight," *Coming Up!* February 1989 (1989)., 4 as cited in Epstein, *Impure Science : Aids, Activism, and the Politics of Knowledge*., 254).
[178] Epstein, *Impure Science : Aids, Activism, and the Politics of Knowledge*., 254.

While in this case, an alternative set of experts in biostatistics were involved to back up activists' points, an analogy with the other situations still holds. AIDS researchers prioritized clean data, but there were no explicit scientific standards that justified this criterion over others.

> Sometimes rigid entry criteria are defended because the investigators desire to study homogeneous groups, but this reasoning is usually difficult to justify. It is important to study patients with abnormal baseline values, because such patients will receive treatments shown to be effective, and we need to know in advance whether or not they can tolerate them"[179]

Although the expertise of the statisticians was used to make these points, the initial assumption that clean data and pure subjects are scientifically optimal was first problematized by AIDS activists, whose first-hand experiences made them more likely to notice the disconnect between the norms of these studies and the practical problems and questions of those with AIDS.

Summary

As these examples show, the direct benefits of scientific engagements with activists' knowledge were that they made recruitment easier and reduced the incidence of noncompliance. It also opened up alternative research models and generated new ideas and new criticisms that were not perceived within the community of AIDS researchers. While not initially recognized as rigorous practices *by* traditional researchers and government health officials, these practices gained increasing acceptance when their achievement of scientific goals became evident. The rest of the scientific establishment slowly began to recognize that these various engagements with "outsiders" were ultimately fruitful. As a result, the scientific community began to be more open to the insights of AIDS activists, who started to be invited onto local community advisory

[179]ibid

boards and IRB boards, hired as intermediaries to enable informed consent by educating

potential clinical trial subjects, and whose publications were often engaged by

establishment researchers.

This discussion of the specific insights of AIDS activists and their positive effects

on epistemic blindness show that the activists *were* in fact competent on many subjects

pertinent to scientific research. Their criticisms brought to light hidden background

beliefs that guided traditional treatment research and their suggestions often bore

empirical fruit.

Social entrenchment and AIDS treatment research

One could infer from these examples that although the criticisms of the

background assumptions in AIDS clinical research were delayed, they *were* ultimately

heeded. Moreover, AIDS activists themselves eventually became incorporated in

significant ways as part of the research process. Maybe, as Nelson argued, good science

is time-indexed, and as time goes on, "methodological principles. . .should be adopted,

revised, or abandoned on the basis of their evidential warrant." [180] As in the case of

Nelson's example of feminist critiques of sociobiology and Wylie's account of feminist

critiques of archaeology, the important criticisms were eventually brought to light and

heeded, just not in as timely a manner as would have been the case if these critiques had

come from conventional sources.

Also, by incorporating AIDS activists into the research process, the diversity of

the scientific community was increased in ways that echo Longino's suggestions. So

while the example of AIDS treatment research illustrates epistemic blindness, perhaps it

can be resolved through the extant empiricist norms and practices of science, and social

[180] Nelson, "Empiricism without Dogmas.", 96.

entrenchment by professional standing and professional specializations is not as much of

an issue as I made it seem earlier in my discussion.

This seems to be the case, at least, until we embark on a more precise examination

of the dynamics by which AIDS activists' criticisms came to be recognized as credible

knowers. This examination will show that the factors that enabled and impeded AIDS

activists from being considered credible by the AIDS establishment were based on

considerations that were often extrinsic to the empirical value of their contributions.

Epstein, among many other sociologists, points to the fact that it was not merely

the relevant value of their input that enabled AIDS activists to be recognized by the

scientific community. AIDS activists were predominately white, middle-class, and

educated gay men. As a result of this constituency, the AIDS movement possessed many

social properties including political clout, social status, and access to money and press

venues that aided them in their engagement with the scientific establishment.

> Within these communities are many people who are professionals, artists, and
> intellectuals of one sort or another—not to mention many doctors, scientists, educators,
> nurses, and other health professionals. On one hand, this has provided the AIDS
> movement with an unusual capacity to develop its own "organic intellectuals" and contest
> the mainstream experts on their own ground."[181]

In addition, the gay activist community was already organized and mobilized and had a

wealth of experience with activism dealing with various institutions and establishments

before AIDS hit the scene.

As a direct result of the cultural power and educational background of many

AIDS activists, another useful property was acquired by AIDS activists. Due to their

educational status and relative social privilege, many activists were able to learn and

---

[181] Epstein, *Impure Science : Aids, Activism, and the Politics of Knowledge.*, 12.

utilize the language of scientists with scientists. While experience in organizing and the

social position to get the word out to affected communities were vital for accruing a

political power base, in order to frame their critiques of scientific institutions effectively

activists learned that acquiring a capacity with scientific discourse was also necessary.

While battles with the FDA were often fought on political fronts, AIDS activists

also engaged the National Institute of Health (NIH) and the National Institute of Allergy

and Infectious Disease (NIAID). In order to critique the internal workings of these

scientific agencies, AIDS activists needed to acquire markers of a type of credibility that

would be recognized by them. As a result, many AIDS activists began to attend scientific

conferences, study research protocols, and learn from various willing medical

professionals in order to achieve a working knowledge of the relevant medical

vocabulary and culture.

> Once they could converse comfortably about Kaplan-Meier curves and cytokine
> regulation and resistance-conferring mutations, activists increasingly discovered that
> researchers felt compelled, by their own norms of discourse and behavior, to consider
> activist arguments on their merits.[182]

Amazingly, achieving a familiarity with scientific conventions (even without credentials)

was often sufficient to overcome the scientific resistance to granting credibility to the

other nonconventional properties of their person. A San Francisco activist, Brenda Lein,

recalls her experience after boning up on the scientific lingo. "I mean, I walk in with. . .

seven earrings in one ear and a Mohawk and my ratty old jacket on, and people are like,

'Oh, great, one of these street activists who don't know anything. . ."   [183] But once she

demonstrated her background in the scientific aspects of the issue, she found that

researchers were reluctantly willing to engage her seriously.

---

[182] Ibid., 232.
[183] ibid

It was on the grounds of their facility with scientific theories that the NIH's Anthony Fauci advocated heeding activists' arguments and incorporating them into the scientific process. "When it comes to clinical trials, some of them are better informed than many scientists can imagine." [184] While many researchers still balked at the possibility of recognizing the credibility of AIDS activists within scientific decisions about clinical trials, after a yearlong campaign Fauci agreed to give a regular seat on every committee of the NIH's AIDS Clinical Trials Group (ACTG) to a representative from a Community Constituency Group (CCG). Finally the scientific community had institutionally included relevant community voices into the process of developing scientific methodologies and procedures for AIDS clinical trials.

The process by which AIDS activists achieved credibility, and thus recognition of their knowledge regarding effective scientific clinical trials, can be read in two different ways. On one reading, it appears that AIDS activists were able to gain scientific recognition as lay scientists; they acquired sufficient comfort with scientific standards and language to become experts in the specialized science of AIDS research, and were recognized by scientists as such. By doing so, AIDS activists became scientists (just without official credentials) and thereby improved scientific research because they increased the diversity of the scientific community (ala Longino).

This was often the interpretation that the scientists gave to the situation. Robert Gallo made the following comment about Martin Delaney, one of the AIDS activists most successful in acquiring scientific respect and the founder of Project Inform. "[He

[184] Epstein, *Impure Science : Aids, Activism, and the Politics of Knowledge.*, 286.

is] one of the most impressive persons I've ever met in my life, bar none, in any field. . .

I'm not the only one around here who's said we could use him in the labs." [185]

Associating facility with the language and practice of science with the rational

authority to contribute to AIDS treatment research is highly problematic for two reasons.

First, acquiring this facility is not practically possible for many people. While white gay

AIDS activists were able to ultimately make inroads into the scientific discourse, Epstein

points out that

> It's no surprise that gays were hotly debating the details of causation theories while intravenous drug users—often the poorest of the poor—sat on the sidelines. . .Even people with hemophilia, a diverse group that had the benefit of a preexisting lobby, did not mobilize forcefully in response to the emergence of the epidemic. In this early period, Haitans were the only other group to challenge medical claims. . .[a]nd most of this opposition came not from the grassroots but from politicians in Haiti and Haitian doctors living in the United States.[186]

The ability of gay activists to challenge medical claims and treatments tended to focus on

improvements related to the gay community and the sexual transmission of AIDS. This

mode of transmission, although representing the vast majority of transmissions in the gay

communities, was not the major cause of transmission for minority communities in the

US.

In August 1987, the keynote address at the National Conference on AIDS in

minority populations reported that 40% of black and Hispanic men with AIDS were

intravenous drug users or had sex partners who were. Among women with AIDS, the

percentages were even higher (47.8% for white women, 69.9% for blacks, and 82.5

percent for Hispanics).   The situation has not vastly changed in the 20 years since.

According to the NIAID fact sheet on HIV infection in minority populations published in

[185] Ibid., 338.
[186] Ibid., 65-6.

April 2005, injection drug use remains a major factor in the spread of HIV in minority communities, while heterosexual transmission is increasingly a factor. [187]

The second, and more important reason why I want to disassociate scientific properties with rational authority is because these are not the properties that were derived from the source of the AIDS activists' ability to criticize scientific background assumptions. This mismatch is demonstrated by the fact that while assumptions about gay men were questioned and the general "patient" viewpoint, voiced by gay activists, were recognized as important and relevant, other patient experiences which were capable of developing into important criticisms of other assumptions and collective biases in AIDS research remained excluded from scientific discourse at that time.

A front-page exposé in the *Los Angeles Times* in 1989 brought to light a striking example of the imbalanced incorporation of AIDS affected populations in AIDS studies. Drawing from NIAID documents, the article showed that "while blacks and Latinos accounted for 42 percent of adult U.S. AIDS patients, they made up only 20 percent of the research subjects in the ACTG trials. Only 11 percent of the ACTG subjects were injection drug users, though this population accounted for 28 percent of AIDS cases." [188]

A later study brought out a similar pattern regarding women, who only comprised 6.7 percent of ACTG trial participants in 1991 while they were ostensibly 9.8 percent of the population. This disparity also did not take into account the problem, brought up in the last chapter, that many symptoms that strike only women were automatically excluded from the definition of AIDS, and thus the percentage of actual women with AIDS could have been much higher.

[187] http://www.niaid.nih.gov/factsheets/Minor.htm
[188] Epstein, *Impure Science : Aids, Activism, and the Politics of Knowledge.*.

The April 2005 NIAID report shows that the amount of AIDS infections in minority populations has only increased. Today, African Americans make up 50% of all AIDS cases reported in the US, and Hispanics represent 15% of all AIDS cases. The CDC reports that as of 2005, African Americans and Hispanics represent 64% of people living with AIDS and 83% of those are women. African-American children represent almost 71% of all pediatric AIDS cases. [189]

In 1997, the *Journal of Internal Medicine* published a study that compared the participation rates of women, persons of color, and injection drug users by taking a cross-sectional survey of 260 patients with HIV disease at an ambulatory practice of a municipal teaching hospital. The findings were striking.

> Overall, 22.3% of patients had participated in a clinical trial. Women, patients of color, and drug users were significantly less likely to have ever participated in an AIDS clinical trial ($p < .05$). Multiple logistic regression confirmed being a person of color (odds ratio [OR] 2.14; 95% confidence interval [CI] 1.12-4.08) and injection drug use (OR 2.09; 95% CI 1.08-4.04) as significant predictors of nonparticipation in AIDS clinical trials ($p < .05$). Patients of color and women reported less knowledge of clinical trials, and were less likely to have been told about clinical trials for which they were eligible ($p < .05$). Patients of color were half as likely as whites to cite ineligibility as their reason for not participating (10.4% vs 22.4%), and more likely to hold unfavorable opinions of clinical research (50.7% vs. 40.5%). Reasons for nonparticipation did not differ by gender.[190]

What impact do these numbers have on the subsequent development of AIDS clinical trials and treatments? Not surprisingly, while scientific assumptions about the behavior and needs of gay communities were challenged, the assumptions about women and minority populations have taken much longer to be criticized and changed. More specifically, the influences on risk behavior of many high risk groups have been inadequately understood. For example:

---

[189] U.S. Department of Health & Human Services: The Office of Minority Health: Data /Statistics
[190] Valerie E MD Stone, MPH, Maya Y Mauch, Kathleen Steger RN, MPH, Stephen F Janas MA, Donald E Craven MD, "Race, Gender, Drug Use, and Participation in Aids Clinical Trials: Lessons from a Municipal Hospital Cohort," *Journal of General Internal Medicine* 12, no. 3 (March 1997).

1) Early HIV prevention strategies focused on avoidance of high-risk partners and protective actions when engaging with them. These strategies do not include education about safer-sex practices within relationships, so it is not surprising that condom use remains low among women and men in established relationships, particularly among African American and Latino couples. [191]

2) Because little attention has been paid to gender roles within different contexts, the assumption of individual responsibility for sexual behavior often ineffectively addresses women's issues, especially when requests for condom use by women are often seen as demonstrations of lack of trust in their partners, could put them at risk for violence, or could lead to loss of income (in the case of sex workers)

3) Because studies of youth have been delayed, assumptions were made that they engaged in unsafe sex due to lack of information or lack of skills. In later direct engagements with youth, studies discovered that these assumptions were largely untrue.

> the reasons why [young] people have sex include to gain status and maturity, to prove your love for your partner, to feel loved by your partner, and because they did not want to feel left out of what their friends were experiencing. Youth told us that the reasons why people would not have safer sex included to avoid having to talk about sex, to not have to plan sex, to avoid losing or being rejected by a partner, to display trust for your partner, and because they did not want to be seen as dirty or a "slut" for having condoms.[192]

Thus, the contributions of AIDS activists to the development of AIDS clinical trials and interventions is not best understood as establishing AIDS activists as some type of lay AIDS scientists, although this was sometimes the case. Rather, by being able to express their criticisms within the background of the relevant scientific framework, AIDS activists were sneaking their *own* expertise in through the back door. This expertise was not about AIDS in general, or even about AIDS-affected communities in general, but arose from their experiences in US gay communities strongly affected by the AIDS epidemic. Each of the criticisms launched by AIDS activists arose because they had interests and experiences as a sub-community of AIDS *patients*. It was membership in an organized and empowered community that reflected on these specific experiences that

---

[191] Mary Sormati, Leslie Pereira, Nabila El-Bassel, Susan Witte, Louisa Gilbert, "The Role of Community Consultants in Designing an Hiv Prevention Intervention," *AIDS Education and Prevention* 13, no. 4 (2001)., 313.
[192] ibid

enabled the alternative viewpoints and possibilities, examined earlier in this chapter, to emerge.

This account leads to a strangely inverted picture. The properties that enabled gay activists' critiques to be recognized by the scientific community (overcome social entrenchment) were not those that made those critiques possible and relevant. Rather, their scientific credibility was based more on those qualities that enabled them to be socially credible: namely, being white, male, educated, and able to talk like scientists. This is the only way to account for the fact that the scientific community did not simultaneously seek out the input of others with relevant backgrounds with AIDS and HIV, like women, youth, and minorities, and these groups remained underrepresented and underconsulted in the research, and remain so to this day. [193]

But here we must be careful. I do not mean to imply that activists' engagement with the biomedical side of the issues did not add complexity and even valuable content to their criticisms. Rather, I only want to emphasize that the original critical push and initial notice of implicit scientific assumptions (the main content of their contributions) were made possible independently of this engagement. The biomedical engagement primarily served the purpose of overcoming the social entrenchment of certain views by giving criticisms access to the scientific credibility they deserved. In other words, there

[193] A number of important steps have already been taken to increase the participation in AIDS Clinical Trials of underrepresented groups, such as persons of color, women, and injection drug users, after early studies left unanswered questions about the effectiveness of AZT, due to lack of adequate representation of these subpopulations.10,14,15 These steps have included the funding of outreach programs in certain sites for minority, female, and pediatric patients; the requirement that ACTUs develop community advisory boards; the establishment of a collaborative relationship with the National Institute on Drug Abuse to reach and increase the number of injection drug users participating in AIDS Clinical Trials; and the establishment of a NIAID-funded community-based clinical trials program, the Community Programs for Clinical Research on AIDS, whose objective is to recruit previously underrepresented HIV-infected patients into clinical trials.6,8 And, in a step that affects all clinical trials, not only HIV-related trials, the FDA reversed its long-standing policy excluding women with "childbearing potential" from early phases of clinical trials.Stone, "Race, Gender, Drug Use, and Participation in Aids Clinical Trials: Lessons from a Municipal Hospital Cohort."

was an interesting disconnect between the grounds on which AIDS activists earned their rational authority (their first-hand experiences as and with AIDS patients) and the grounds on which they earned their credibility from the scientific establishment (their ability to acquire scientific properties).

So if the properties of scientists are not the correct grounds on which to locate the credibility of activist criticisms, what properties *do* reflect the grounds on which AIDS activists earned their rational authority? In order to answer this question, the acquisition of rational authority must be distinguished from the acquisition of scientific authority (credentials), on the one hand, and it must be distinguished from arguments for lay authority based on a stakeholder argument, on the other. What is it specifically (and sociologically) about scientific training that licenses us to attribute to scientists cognitive authority? What more than stakeholder reasons do we have to attribute cognitive authority to non-scientists?

My thesis, which I will give a thoroughgoing argument for in the next chapter, is that the process through which lay communities should acquire recognition is very similar to the process through which scientists gain their authority as well, and it is only because the source of scientific expertise has broadened beyond the scope of its authority that the compatibility and similarity of the expertise of certain lay communities is overlooked. Once this is acknowledged, the rationale for recognizing the input of certain communities outside of science on certain scientific questions becomes apparent. And only an impetus for this type of recognition, as a social norm, is capable of avoiding the problems of epistemic blindness and social entrenchment.

Chapter 6: New social norm

Recognize, Engage, and Develop Specialist expertise

Introduction

At the end of the last chapter, I made several interrelated proposals. First, I proposed that AIDS activists were able to have a positive influence on AIDS treatment research because they had acquired a type of rational authority whose source was scientifically independent but was directly relevant to scientific research about HIV and AIDS. Second, I claimed that the scientific recognition of this type of rational authority is crucial because it can be necessary to make scientific self-criticism and innovation possible, and is simultaneously hindered due to the social patterns of credibility as they exist within the sciences and throughout the larger society. These social patterns tend to track credible knowledge in precisely the opposite direction as that from which the rational authority to challenge epistemic blindness arise (social entrenchment). This, I concluded, is why a social norm that emphasizes seeking out and recognizing sources of this authority outside scientific communities, especially in marginalized communities, becomes necessary both to combat epistemic blindness and to uproot the social entrenchment that pervades scientific communities.

In this chapter, I will take the existence of epistemic blindness and social entrenchment as established, although as my case study illustrated, they appear in different ways in different sciences.   Here, I will outline the theoretical underpinnings of a social norm that is capable of addressing these hindrances to scientific self-correction and innovation. Specifically, in this chapter I will advocate incorporating three moments into scientific education and procedures:

1) Recognition that communities outside of science are often exposed to social and environmental experiences necessary for the acquisition of **expertise** necessary for vital criticisms and innovations in research programs (specifically criticisms and innovations of beliefs subject to epistemic blindness)

2) Recognition that the circumstances that constitute appropriate conditions to cultivate this type of expertise are often the same circumstances that hinder this expertise from being achieved, recognized, and taken up by scientific communities, or recognition of social entrenchment

3) Integrate into scientific research practices the *responsibility* to engage and facilitate the development of *potentially* relevant expertise (in terms of indicator properties) in order to make possible the challenge of invisible assumptions and uncriticized models that can inform every stage of the research process.

I have yet to systematically elaborate this particular solution to the problems of social entrenchment and epistemic blindness. In this chapter I will do by focusing on each moment separately. First, I will argue that the rational authority traditionally identified with scientists can be better specified and attributed when talk of "knowledge" is replaced with a more robust notion of "expertise." While I have been using these terms interchangeably throughout the dissertation, I will utilize the resources provided by sociologists of science to provide a more specific and robust notion of expertise and show how it is usefully and accurately applied to specific populations outside of the scientific community.

Next, I will show how there are many social hindrances to recognizing nonscientific expertise. I will utilize the work of Miranda Fricker and Karen Jones to articulate the dynamics that facilitate social entrenchment, and likewise make it difficult to institutionally recognize nonscientific communities. Finally, I will demonstrate a model of my norm as it is manifested in Community-based participatory research models. These models both acknowledge potential expert communities outside of scientific

communities, devote time, attention, and resources to developing them, and integrate them in various ways into the process of research.

Moment I: Recognize non-scientific experts

By arguing that scientific communities should recognizing the potential expertise of relevant nonscientific communities, I am making the controversial claim that the assumed expertise of scientists is not legitimately correlated to the general credentialed authority of members of scientific communities. At the same time, I do not want to deny the existence of legitimate expertise, nor the existence of properties by which experts can be recognized. The challenge is to formulate a notion and grounding of expertise that is compatible with the "good science" that has been produced by interactions between scientists and external communities (such as AIDS activists and feminists), while is incompatible with an account that dissolves the epistemic idea of expertise altogether into a political notion of stakeholding.

In order to make this argument, I will distinguish experts from credentialed scientists on the one hand, and from stakeholders on the other. I will then provide some criteria by which to identify experts that are independent of credentials and political stakes. These properties will then play a key role in the ultimate social norm that I will advocate.

There are a couple of reasons for the terminological choice of arguing that nonscientific communities have "expertise" as opposed to "knowledge." First, "expertise" is a notion that incorporates the social processes by which types of competencies are gained, and also has less of a tendency to be overgeneralized. While people often speak of knowledge per se, "expertise" is used both technically and

colloquially to apply to both scientific and nonscientific mastery in specific domains, both theoretical and practical. As such, the notion of expertise is useful to challenge the social entrenchment of authority within the scientific community in general, but remains compatible with epistemic conditions valued within the sciences.

The other important value of expertise as an epistemic concept is that expertise is not immediately associated with Western practices. Many criticisms of "Western Science" argue that certain "nonscientific" communities, in certain areas that are often socially and economically marginalized, have knowledge, skills, and interests that are significant and must be heeded by scientific research programs. Instead of talking about "Western knowledge" as a monolithic entity and opposing it to other homogenizing notions like "Eastern knowledge" or "Traditional Ecological Knowledge", the term "expertise" immediately lends itself to both plurality and specificity in a way that "knowledge" doesn't. A community can acquire expertise in farming or fishing in a local ecosystem, expertise in Tibetan religious texts, or expertise in meditative practices.

I will show how this shift towards discussing expertise is capable of providing an epistemic account that neither identifies expertise with "how scientists think" (at the problematic exclusion of everyone who is not a scientist) nor conflates expertise with personal opinion (thus dissolving epistemic pursuits altogether). Since I want to incorporate the process by which scientists engage "outsiders" in order to do better science, this analysis of expertise that is not coextensive with scientific credentials is quite a useful resource in order to expound a picture where those worthy of rational authority are not solely identified with those attributed authority in the scientific community.

To analyze the concept of "expertise" requires that my discussion come full circle. In Chapter 1, I argued that the tradition of Science Studies, spearheaded by H.M. Collins and Robert Evans, among many others, played a key role in the extreme impasses in discussions of those with loyalties to the rationality of the scientific endeavor and those who took the social analyses of the sciences to undermine its authority altogether. Consequently, I turned to the feminist empiricists to help understand these sociological insights in a way that was more consistent with traditional empiricist and rationalist scruples of the scientific tradition.

At this stage in my argument, I want to utilize the contributions of Science Studies again, but this time in its more contemporary guise. In their 2002 Discussion Paper, "The Third Wave of Science Studies: Studies of Expertise and Experience", H.M. Collins and Robert Evans advocate a normative conception of specialist expertise defined in terms of extensive experience and specialization, not formal training or certification. They also crucially admit that some scientists *are* legitimately understood to be experts under this definition.

One important nuance of Collin's and Evan's notion of specialist expertise is that the potential expertise found in nonscientific communities is not only about the natural world outside of the laboratory, like the ecology of local areas or the manifestations of diseases in their population, but also often about social dynamics relevant to the design and implementation of research projects on the ground. For example, nonscientific populations often have expertise about the dynamics between science and certain portions of society, between political authorities and those under them, between genders, between

races, etc. [194] As we saw in the case study of AIDS treatment research, AIDS activists had

a much more nuanced and effective understanding of the motivations and behaviors of

AIDS patients vis-à-vis the scientific community, healthcare providers, and treatment

programs than the AIDS researchers.

Distinguishing Expertise from Credentials

In their discussion paper, Collins and Evans argue for a novel distinction

"separating specialist experts, whether certified or not, from non-specialists, whether

certified or not." [195] . They draw a distinction between "core-group" scientists, a subset of

the scientific community who are deeply socialized in the experimentation or theorization

in a particular area of phenomena, and the generally certified scientific community.

Collins and Evans argue that core-groups of scientists should hold a special position

among scientists as "experts" on a particular subject, while the assumption that

credentialed scientists in general hold an expertise about fields of science distant from

their own is based on "mythologies of science." [196]

Collins and Evans argue for this position by pointing out that many scientific

endeavors *intrinsically* include hypotheses and theories about how information and

---

[194] This point yet again reinforces the feminist standpoint theorists key insight that the standpoint of marginalized social positions are forced to develop an intricate awareness of the social dynamics to which they are subject. See for example: Dorothy E. Smith, "Women's Perspective as a Radical Critique of Sociology," in *The Feminist Standpoint Theory Reader : Intellectual and Political Controversies*, ed. Sandra G. Harding (New York: Routledge, 2004)., Harding, *The Science Question in Feminism*, Nancy Hartsock, "The Feminist Standpoint: Developing the Ground for a Specifically Feminist Historical Materialism," in *Discovering Reality : Feminist Perspectives on Epistemology, Metaphysics, Methodology, and Philosophy of Science* ed. Sandra G. Harding and Merrill B. Hintikka, *Synthese Library* (Dordrecht, Holland ; Boston*;* Hingham, MA: D. Reidel ;Sold and distributed in the USA and Canada by Kluwer Boston, 1983).

[195] H.M Collins, Robert Evans, "The Third Wave of Science Studies: Studies of Expertise and Experience," *Social Studies of Science* 32, no. 2 (2002)., 251.

[196] Many (including Wynn) take issue with Collins/Evans presumption that the core-group is in some way nonnegotiable itself. I do not believe that this is implied by their argument, but I take the important (and relatively noncontroversial point) to be that the core-group is legitimated by a set of experiences and socialized training with a localized set of phenomena.

actions will play out in public domains. Expertise about public domains, and expertise about how scientific communities best interact with particular public populations, becomes integrated into scientific questions, methods, and results. Knowledge about public domains, in turn, brings the domain of scientific inquiry beyond the bounds of scientific training, and into the bounds of specialized experiences of specific nonscientific populations.

> Once it is recognised that the laboratory is important because it allows scientists a great deal of control, it becomes clear that moving to a real life setting, such as a farm, introduces new complexities that reduce this control and the certainty that it provides (Latour 1983). This is not to say that the science is no longer relevant, but that it can no longer be assumed to be sufficient. Instead, laboratory-based expertise needs to be complimented by the expertise of those with experience in the settings in which it is to be applied (Grin et al. 1997; Rip et al. 1995)[197]

We saw a clear example of this situation in the last chapter, where the models of clinical trials presupposed several assumptions about the motivations and dynamics of AIDS-affected populations. In another example, Collins and Evans appeal to Brian Wynne's paradigmatic case, published in a series of papers in the late 80's and early 90's examining the interactions between Cumbrian sheep farmers and environmental scientists after the Chernobyl disaster. [198]

In his initial article, "Sheepfarming after Chernobyl: A study in communicating scientific information," Wynne examines the specific case of the impact of the 1986 Chernobyl nuclear accident upon a community of sheep farmers living in the Cumbrian region of northern England. This area both underwent some of the heaviest radiation fallout in Western Europe as a result of the Chernobyl disaster, and is also located close

---

[197] Robert Evans, "The Sociology of Expertise: The Distribution of Social Fluency," *Sociology Compass* 2, no. 1 (2008)., 284.
[198] This situation closely parallels my case study of AIDS research in the last chapters.

to another nuclear complex that has be criticized for its radioactive discharge, including a nuclear reactor fire in 1957.

Wynne explores the tensions between the scientific expertise and subsequent policies regarding the Cumbrian sheep farmers on the one side and the experiences and interests of the Cumbrian sheep farmers on the other. Due to their extensive experiences with the topographical landscape, the physiological states of their sheep, and previous encounters with nuclear fallout and scientific responses from Sellafield, Wynn argues that the sheep farmers were able to accrue specialized abilities to discriminate and assess many relevant issues when the Chernobyl disaster occurred.

While the policies and regulations that were implemented in the region made perfect sense according to the radiation scientists, the sheep farmers found these policies both overgeneral and often practically inapplicable. For example, scientists ignored or were unaware of local variations in fallout effects between upland and lowland areas as well as variations in farming procedures that had direct impact on the relative contamination of the sheep. These were circumstances of which the sheep farmers were only too aware. As a result of this mismatch in expertise, regulations resulted in either noncompliance or compliance to the severe and avoidable detriment to the farmers' livelihood. By collaborating with the Cumbrian sheep farmers as core-group experts on farm management in the region (even thought they lacked scientific credentials), these problems could have been avoided.

By emphasizing that experts are determined by a type of specialized training, Collins and Evans are able to argue for a new epistemic boundary scientists in a specialized field and scientists in general. . .

> *The wider scientific community no longer plays any special part in the [expert] decision-making process. . .the wider scientific community should be seen as indistinguishable from the citizenry as a whole; the idea that scientists have special authority purely in virtue of their scientific qualifications and training has often been misleading and damaging. Scientists, as scientists, have nothing special to offer toward technical decision-making in the public domain where the specialisms are not there own.* [199]

As well as challenge the existing boundary between specialist scientists and specialist nonscientists.

> [i]t is not more certification that qualifies them for membership of the core. . .the difference between the core-set and others is informal. This informality—the fact that membership of the most esoteric groups is based on experience—gives us licence (sic) to dissolve the boundary between the certified experts and experience-based experts.[200]

Like the core-set of scientists, certain groups of people, due to their specific interactions with certain environments, have accrued a specialist expertise that has a legitimate voice throughout research about that subject. The case of the Cumbrian sheep farmers does not demonstrate that the sheep farmers were "lay scientists" any more than AIDS activists were. Rather "there were not one but two sets of specialists, each with something to contribute. The sheep farmers were not 'lay' anything—they were not people who were not experts—they were experts who were not certified as such." [201] Louise Portmann has coined the term, "civil scientists" to refer to people of this status.

Distinguishing experts from stakeholders

It is important also to examine the distinction between arguing that certain non-scientist contributions should be heeded in science because they are experts and the argument that they should be consulted as stakeholders. Here the case of the Cumbrian sheep farmers is again helpful. Since the Cumbrian sheep farmers were both potentially

---

[199] ibid 249-50
[200] Collins, "The Third Wave of Science Studies: Studies of Expertise and Experience.", 260.
[201] Ibid, 261

experts on radiation effects *and* stakeholders in the situation, it is easy to see why these two roles are often conflated.

Collins and Evans demonstrate that there are important differences between the argument that the Cumbrian sheep farmers had a special expertise in the situation with their sheep and the argument that scientists should consult them as political stakeholders in their policies. The frequent idea of stakeholding, as used to argue for the input of lay voices in the sciences is important, but not equivalent, to the arguments that I am providing here.

As stakeholders, the sheep farmers were directly affected by the scientific conclusions about acceptable radiation, as this had direct bearing on the policies and regulations that impinged on the welfare and marketability of their sheep, which in turn affected their livelihoods. In spite of this role, Collins and Evans argue that it is not only qua stakeholders that the farmers should have received recognition from the scientific community. They demonstrate this with a thought experiment.

> Imagine that just prior to the Chernobyl explosion a group of London financiers had got together to buy the Cumbrian farms as their private weekend resort, employing the farmers as managers so as to preserve the existing ecology: the financiers, not the farmers, would then be the owners of the sheep, yet all the expertise would remain with the farmers.[202]

This thought experiment exhibits one problem with not clearly distinguishing stakeholding from expertise, namely that stakes can be economically transitive while expertise is not. While shifting the ownership of the sheep changes the population of stakeholders, it does not thereby change the population of potential experts about the Cumbrian fells. Although the sale of the farms would shift the stakes to the financiers, we would not want to say that the scientists should have then consulted the financiers to

[202] Ibid.

learn about the effects of radiation on sheep, although perhaps they should consult them for other things at other stages of the research process. [203]

This example illustrates one important difference between stakeholders and experts. Stakeholders have political authority, which is legitimated or disqualified based on the claim that research has influence and/or impact upon them. In a just society, people should have a say in how they want their lives to be impacted by research and arguably to participate in the discussion of which questions are to be asked as well. This type of argument is relevant to those who stress the important role of the general public in arenas where science and policy converge. [204]

Experts, on the other hand, have rational authority, which is justified or not based on the justification of the claims that they make in a particular domain. This "rational" authority (as opposed to political authority) would justify the involvement of experts in the empirical and methodological process of research in that domain. The properties that correspond to this expertise would be those associated with the process that enables the acquisition of this expertise.

Thus, the sheep farmers are also misunderstood to be merely stakeholders, who should have been heeded solely due to the potential affects of scientific policies on their lives and livelihood. While Collins and Evans argue that public citizenry in general may have a stake in much scientific research (resource allocation, national defense, general environmental research, for instance), they do not have a specialist expertise merely by

---

[203] This distinction also leads to interesting issues in the language of knowledge ownership that is often used in interactions between the U.S. government and Native American tribes. While useful to protect the intellectual and other property of the tribes currently, I wonder if they could be given more recognition as experts (due to expertise handed down) even in those areas where they no longer have any recognized economic rights.
[204] Beck 1992 [1986]; Funtowicz and Ravetz (1992), Alvin Weinberg (1972, 1985) Jasanoff , Wynne 2003

being affected by research. Collins and Evans conclude that the case of the Cumbrian sheep farmers were non-certified experts about the Cumbrian fells, not "lay scientists" or merely stakeholders.

Social conditions for expertise

So how do we identify non-certified experts? This is the key question in this discussion because the traditional way of locating experts is to look for those with credentials in a domain. If those with expertise are to be distinguished from those with credentials and from the general public, there must be an epistemic account of how experts can be identified in another way. This identification must successfully track *both* how specialized scientists acquire their expertise *and* how nonscientific experts acquire it as well.

Collins and Evans provide some preliminary clues to the answer. In their original "Third Wave" article, Collins and Evans argue that <u>experience</u> in a particular domain is a necessary condition for specialist expertise, whether within the scientific community or out in the world.   In his most recent article "The Sociology of Expertise: the Distribution of Social Fluency", Evans argues that qualifications are the least reliable mode of identifying experts [205], track record is somewhat reliable [206], and "experience" is the most reliable mode of identifying experts, and "the more extensive and recent the better." [207]

This aspect of Collins and Evans' arguments is quite vague. Many of their critics have pointed out that although their categorizations of expertise are quite specific, they do not provide satisfactory accounts of how it is acquired. This is an especially crucial

---

[205] They cite situations of nonscientist experts (like the Cumbrian sheep farmer and AIDS activists) who have no formal qualifications. This reflects their critique of "credentialism" in the first section.
[206] They referr to the problem that this would exclude recognizing expertise in newer fields.
[207] H.M. and Evans Collins, R., *Rethinking Expertise* (Chicago: University of Chicago Press, 2007)., 293.

flaw since most of their examples of specialist experts in the sciences are gravitational

wave scientists and scientists who study radioactive isotopes. If Collins and Evans want

to advocate distinguishing expertise from credentials, it is surprising that they provide

most examples from those who have both. It is frustrating because the most

revolutionary aspect of their account, their argument for expertise in nonscientific

communities, bears very little articulation. I will attempt to provide some articulation of

this notion of nonscientific expertise, since it plays a crucial role in the social norm that I

am advocating.

Collins and Evans do recognize that "recent and extensive" experience cannot be

sufficient to constitute expertise. In several of their works, they repeat the example that

lying in a bed every night does not make one an "expert" on sleeping. They also add to

their account that expertise requires the ability to "do" something with knowledge, to

enable one to make, debate, correct, or make sensible decisions about the consequences

of X. [208] They follow in the tradition of Michael Polyani, LudwigWittgenstein and Peter

Winch in emphasizing that expertise requires not only formal knowledge, but ". .

.becoming a scientist means being socialized (i.e. trained) in the formal aspects of a

scientific discipline while, at the same time, acquiring the tacit social and cultural

knowledge needed to apply and use these facts in new contexts." [209]

So far, the examination of expertise in the work of Collins and Evans has given us

two criteria for identifying an expert: localized experiences in a particular domain and

socialization in an interactive community that conceptualizes and engages with those

---

[208] Ibid.
[209] Evans, "The Sociology of Expertise: The Distribution of Social Fluency."

experiences. [210] One immediate inclination would be to look for assistance in this discussion from social epistemologists and philosophers of science. Perhaps they have a more specific account of how to identify expertise. Unfortunately, the focus of the prominent thinkers in these literatures is quite different. Alvin Goldman presents five criteria for assessing epistemic practices.

1) The *reliability* of a practice: measured by the ratio of truths to total number of beliefs fostered by the practice
2) The *power* of a practice: measured by its ability to help cognizers find true answers to the questions that interest them
3) The *fecundity* of a practice: measured by its ability to lead to large numbers of true beliefs for many practitioners
4) The *speed* of a practice: measured by how quickly it leads to true answers
5) The *efficiency* of a practice: measured by how well it limits the cost of getting true answers[211]

Ostensibly, experts would then be those whose practices measured up to these standards.

One major problem with these criteria for our purposes, pointed out by Paul Thagard in his article, "Collaborative Knowledge," is that "[m]any scientists would blanche at describing their findings as "truths", since the truth of scientific claims only gets sorted out in the long run, as experiences and theories accumulate." [212] I don't want to engage any in-depth theories of truth here, but if our challenge is that of *identifying* experts, defining experts as those with truth-conducive theories is highly problematic.

---

[210] The requirements of continuous localized experience and socialization within a community for the acquisition of expertise sheds some light on more reasons for the frequent conflation in literature between political stakeholders and "lay experts." The set of circumstances under which a stakeholder is affected by a research project or research results often overlap the set of circumstances necessary to socialize an expert. A stakeholder and a nonscientist expert may both be exposed to the same intersection of social and environmental locations, and be exposed to similar sets of experiences as a result of them. On the other hand, the socialization, the long-term concentrated interaction with other people to interact and solve problems faced in these social locations, while necessary to become an expert, are not necessary to be considered a stakeholder as the Cumbrian sheep farmer example demonstrates. It is therefore understandable why these two arguments are often mistaken for each other, and also must be kept distinct.
[211] Alvin Goldman, *Liaisons: Philosophy Meets the Cognitive and Social Sciences* (Boston, MA: MIT Press, 1992)., 195.
[212] Paul Thagard, "Collaborative Knowledge," *Nous* 31, no. 2 (1997)., 247.

Not only for the reason that Thagard articulates, but also because many of those who we often consider rightful experts are often wrong on many occasions, like meteorologists.

Thagard attempts to improve upon Goldman's standards in several ways. First, he reframes them in less veritistic terms. He shifts to talking of short-term "results" instead of end of inquiry "truths". Results may include both empirical results (observational findings) as well as theoretical results (development of theories that explain empirical results). This move is important, because as we have seen in this dissertation, many positive impacts in science have been shifts in questions, frameworks, emphasis, tools, and other "paradigmatic" elements that make sense of empirical findings, not merely empirical findings themselves.

Second, Thagard shifts from the metaphysical (or else highly philosophically nebulous) criteria for assessing truth to the more pragmatic criterion of "acceptability by scientific peers" for measuring results. He goes on to say that a minimal requirement for this would be publication in a reputable peer-reviewed journal. While this move does remove the philosophical implications of relying too heavily on a truth-standard to practically assess epistemic claims, limiting assessment to those within the scientific community leads directly to the problem that Collins and Evans warned against. For example, Thagard rephrases Goldman second standard as "The power of a practice is measured by its ability to help cognizers find results that answer the questions that interest them." [213]

As we saw in the AIDS treatment case study, the biomedical research that interested the scientific community often yielded answers to those questions, but upon implementation did not yield health results outside of the laboratory due to epistemic

[213] Ibid., 255.

blindness. Only when the questions of interest shifted towards those that included

sociological and psychological dynamics of AIDS sufferers did the results become

applicable. This was in spite of the fact that there was nothing within the framework of

the questions and the data that answered them that was out of place.   [214]

　　　While Thagard's approach to epistemic assessment solves several challenges he

associates with Goldman's account, he confines communal approval to the community of

scientists. As my project is to define expertise without immediately identifying it with

scientists, this approach is problematic. Thus we are thrown back on the same problem

that emerged from Goldman's criteria. If we want to identify which communities should

be consulted to evaluate epistemic claims, and further we have established that at least

sometimes these communities are not scientific communities, we need criteria by which

to identify these communities that do not make immediate appeal to scientific credentials.

　　　I suggest that the best way to answer this question is to better understand how we

assess expertise from the "outside." In other words, if we cannot immediately appeal to

shared beliefs or to credentials, how can we determine which people we should consider

to have rational authority on any given subject? As Linda Alcoff quiries, "On what basis

should we make an *epistemic* assessment of another's authority to impart knowledge?

What features of the other are relevant for such an epistemic assessment?"   [215]

---

[214] Thagard makes a final change to Goldman's standards by adding a fifth one. He famously introduces his term "explanatory coherence" and argues that epistemic practices can be measured by how well they contribute to the development of theoretical and experimental results that increase explanatory coherence. He sees explanatory coherence as increasing the breadth and unity of scientific explanations. This suffers from the same problem of having no traction with epistemic blindness shared by Alison Wylie's account of horizontal independence discussed in Chapter 2.

[215] Linda Martin Alcoff, "On Judging Epistemic Credibility: Is Social Identity Relevant?," in *Engendering Rationalities*, ed. Nancy Tuana (Albany, NY: SUNY Press, 2001)., 58. To note here is that Alcoff's question refers to the epistemic assessment of "persons," while Goldman and Thagard are referring to the epistemic assessment of "claims", and we can also ask after the epistemic assessment of "ways" of doing

Luckily, this is not a new question either to the sciences or to social epistemology. The presumption that epistemic assessments are made fundamentally on directly assessing the evidence for particular claims has come under attack from various quarters. In Chapter 1, we saw its limitations in the face of an array of underdetermination arguments. In Chapter 2, we saw even more arguments for this as Lynn Hankinson Nelson showed how scientific *communities*, rather than individual scientists, constitute the locus of epistemic assessment. Moreover, it has been pointed out that. . .

> The popular conception of science says that science is a collection of observable facts that anybody can verify for himself. We have seen this is not true in the case of expert knowledge, like that needed in diagnosing a disease. Moreover, it is not true in the physical sciences. In the first place, for instance, a layman cannot possibly get hold of the equipment for testing a statement of fact in astronomy or in chemistry. Even supposing that he could somehow get the use of an observatory or a chemical laboratory, he would not know how to use the instruments he found there and might very well damage them beyond repair before he had ever made a single observation; and if he should succeed in carrying out an observation to check up on a statement of science and found a result that contradicted it, he could rightly assume that he had made a mistake, as students do in a laboratory when they are learning to use its equipment."[216]

Not only is the evidential nature of epistemic assessment between nonscientists and scientists challenged, but John Hardwig in his 1985 article, "Epistemic Dependence," argues that scientists must appeal to factors other than assessment of the content of scientific claims and must turn to evaluating *each other*.

> . . .we can see how dependence on other experts pervades any complex field of research when we recognize that most footnotes that cite references are appeals to authority. And when these footnotes are used to establish premises for the study, they involve the author in layman-expert relationships even within his (sic) own pursuit of knowledge. Moreover, the horror that sweeps through the scientific community when a fraudulent researcher is uncovered is instructive, for what is at stake is not only public confidence. Rather, each researcher is forced to acknowledge the extent to which his (sic) own work rests on the work of others—work which he has not and could not (if only for reasons of time and expense) verify for himself. [217]

research. My ultimate goal is to assess "ways" of doing research by how well they include those "persons" who are best equipped to make reliable "claims."

[216] Michael Polanyi and Harry Prosch, *Meaning* (Chicago: University of Chicago Press, 1977)., 184-5.
[217] John Hardwig, "Epistemic Dependence," *The Journal of Philosophy* 82, no. 7 (1985)., 348.

Hardwig additionally provides several examples of the trend in scientific research to consist of several authors, often on several continents, over many years. [218] In this research model, the project spans several subspecialties, and each subspecialty is not in a position to assess the results of the other subspecialties. In addition, the results of certain stages of research, performed by certain researchers, are the basis of the research performed by others. In this model of research, performing corroborating studies is rare, and each subspecialty in the project must trust the results of their colleagues from another specialty. Thagard also recognizes the increasingly collaborative nature of scientific research programs in his article, "Collaborative Knowledge." [219] These points further bolster Collins and Evans argument above that the expertise found in the scientific community is a specialist expertise, not a generalist one.

If we take Hardwig's point seriously and recognize that even within scientific research, specialist scientists relate to each other's subspecialties as laypeople, we can connect our discussion to the rather substantial social epistemology literature on evaluating expert testimony, since both rely on a situation where nonexperts must assess the credibility of experts. Karen Jones argues that

> To testify for p contrasts with arguing that p insofar as it is the testifier herself who vouches for the truth of p. In arguing that p, one presents reasons for public consideration, reasons that can be evaluated and accepted or rejected. Testimony replaces argument when the audience can't assess the cogency of the reasons given or when it can't have the right sort of access to those reasons (e.g., when the witness alone saw the event). Testimony is accepted (if it is) on account of the audience's accepting that the testifier is in a position to vouch for the truth of p. Hence the connection between testimony and trust: we must trust the testifier because she herself is warranting the truth of p (unlike the arguer, who can let the arguments provide warrant for the truth of p.) [220]

---

[218] His specific favorite is the situation of an article on the lifespan of charmed particles in particle physics with 99 authors, each representing different specializations in the field, with the research taking place over a span of consecutive careers.

[219] Thagard, "Collaborative Knowledge."

[220] Karen Jones, "The Politics of Credibility," in *A Mind of One's Own: Feminist Essays on Reason & Objectivity*, ed. Louise M. Antony and Charlotte Witt (Boulder, CO: Westview Press, 2002)., 156.

By understanding specialist expertise as we have in this chapter, it becomes clear that

evaluating expert testimony is not merely legal problem, but a problem that faces

scientists amongst themselves (expert in A assessing the testimony of expert in B) and

between themselves and possible experts outside of their credentialed ranks.

Hardwig's account is not intended to undermine the rationality of scientific

endeavors with a faith-based notion of trust. Rather, he argues that ". . .one can have

good reasons for believing a proposition if one has good reasons to believe that *others*

have good reasons to believe it and that, consequently, there are good reasons for

believing *p* that do not constitute evidence for the truth of *p*." [221] These good reasons are

rather evidence for assessing the *experts* with regard to *p*. Later, Hardwig calls this

orientation "rational deference" to an expert. Mirroring Hardwig's claim, Goldman

states, "although a novice N may lack (all or some of) an expert's reasons R for believing

conclusion p, N *might* have reasons R* for believing *that* the expert has good reasons for

believing p. . ." [222]

So what evidence ought we seek in order to ascertain who is likely to be an

expert? Some of our intuitions about credentials are useful here. Credentials seem to be

a proper indicator of experts because they indicate that a person with credentials has

undergone a particular type of socialization and extensive training in a type of inquiry.

Hardwig picks up on this element when he says that the basis for this rational deference,

in place of evidential appeal, is the belief that an expert "has conducted the inquiry

[221] Hardwig, "Epistemic Dependence.", 336.
[222] Alvin Goldman, "Experts: Which Ones Should You Trust?," *Philosophy and Phenomenological Research* 63, no. 1 (2001)., 96.

necessary for believing that *p*." [223] We found above that the generalist scientific training is itself insufficient to acquire expertise in this type of inquiry, and not even necessary to acquire it in others (like the Cumbrian fells or AIDS communities).

Here I believe we can find an important third criterion for epistemically assessing experts. In addition to Collins and Evans' localized experience and socialization, experts must have conducted some sort of critical investigative process regarding the local phenomena, although with the important codicil that this type of communal inquiry need not be a speculative or institutionally scientific one, but may be a completely pragmatic or instrumental inquiry to solve problems in the particular contexts. But as we gleaned from the discussion of Collins and Evans, the inquiry must be informed by localized experiences in a particular domain and socialization in an interactive community that engages with those experiences. While this does not yield a precise account of the conditions for expertise, it does provide necessary conditions for becoming one, and it gives us a way of both identifying potential experts and knowing how potential experts (people exposed to experiences in a particular domain) can become actual experts (socializing into a community of those with similar experiences and engaging with them in an inquisitive process regarding the phenomena). While success and failure cannot be absolutely measured, "results" that are acceptable to a majority of that specialist community seems a probable measure.

Summary

In order for scientific communities to recognize the existence of potential experts on issues relevant to scientific research, they must first acknowledge the amount of cross-field collaboration that is already being used to answer intrascientific research questions.

[223] Hardwig, "Epistemic Dependence.", 337.

As Thagard points out, cross-disciplinary collaborations are prevalent in several

disciplines like cognitive science, psychology, artificial intelligence, linguistics,

philosophy, physics, and neuroscience. [224] Intradisciplinary collaborations are even more

widespread, and different background theories and collateral information is brought to

bear on different fields. In order to research these complex subjects, experts in one field

often consult experts in another field, and these are claims that cannot themselves be

assessed by the first set of experts, ie. A philosopher could not assess the accuracy of the

claims made by a neuroscientist. At the same time, these multiple-expert collaborations

are seen to be fruitful and enable new questions, frameworks, methods, and answers to

emerge from the research process.

Likewise in the case of AIDS research, if the virologists and epidemiologists had

from the outset recognized that they lacked an expertise about sociological and

psychological factors in populations of AIDS sufferers rather than making assumptions

about them, they may have much more quickly and much more accurately incorporated a

more complex picture of how treatment research could be developed. [225] The problem of

relying too heavily on politics and the contingencies of wealth and education to

determine who was ultimately represented could have been remedied by recognizing that

potential experts were being conditioned by a set of common experiences (suffering from

AIDS or loving those suffering from AIDS) and seeking out those people for

collaboration.

---

[224] Thagard, "Collaborative Knowledge."

[225] Leonard Krimerman, in his article "Participatory Action Research: Should Social Inquiry be Conducted Democratically" calls this the commonsense idea of "insider advantage" in which "most social situations— families, neighborhoods, organizations, and so forth—are quite complex, and those who inhabit them for long periods of time usually know more about them than those fresh to them. . ."Leonard Krimerman, "Participatory Action Research: Should Social Inquiry Be Conducted Democratically? ," *Philosophy of the Social Sciences* 31 (2001)., 70.

Moment 2: Recognize the social hindrances to recognizing non-scientist experts

Now that we have some criteria by which to identify experts, both credentialed and noncredentialed, we face a further problem. The prevalent identification of expertise with credentials has tended to limit the examination of the social process of becoming an expert, so in the last section I attempted to address the epistemic hindrance to recognizing nonscientist experts, i.e., we don't know how to find them. But beyond the epistemic hindrances (our ignorance) to recognizing non-scientist experts, there are also substantive *social* hindrances to this recognition as well.

In her article "Rational Authority and Social Power: towards a truly social epistemology," Miranda Fricker examines how rational authority becomes socially recognized. Taking inspiration (and many of the terms) from Edward Craig's *Knowledge and the State of Nature*, Fricker analyses the social and empirical factors that are used to recognize the testimony of informants, but I believe that the traits that she indicates are likewise used to identify authoritative experts more generally. Fricker argues that good informants are distinguished by three features. First, they must be competent, meaning that they believe *p* when *p* is the case, and not believe that *p* if *not-p* is the case. [226] Our discussion of the last section enables us to replace these overly abstract criteria with some practical tools by which to identify the conditions to enable the acquisition of competence.

Second, they must be trustworthy in that the informant is accessible, speaks the same language, is willing to part with the information, and has no reputation for

---

[226] It is important to note that this definition of competence is an ideal. No competent person is never mistaken, and my account is not committed to this being the case.

deception. The important challenges to determining or else enabling these factors will be dealt with in the final section.

Finally, a good informant must have positive indicator properties that indicate to the inquirer that he or she is likely to be right, i.e. both competent and trustworthy. [227] While Fricker does not specify this, her account also entails that certain people exhibit negative indicator properties that indicate to others that they are *un*likely to be competent and trustworthy. While the first two requirements depend upon the epistemic qualities of the informant, indicator properties depend upon how the informant is *perceived* by the inquirer. Fricker reflects this contrast by labeling those who are competent and trustworthy as having "rational authority" while someone who possesses indicator properties is "credible." [228], or has the appearance to others of being competent and trustworthy.

In the real-life contexts where these epistemic evaluations take place, indicator properties are often linked with institutional affiliations, such as attending certain schools or achieving certain degrees. Historically, they have also been linked to social identities such as race, gender, caste, and class. For example, Steven Shapin's famously analyses the historical example of gentlemanly veracity in 17 century England. Shapin argues that in that time, "being a gentleman" was used as an indicator property for rational authority in general, while being nongentle and/or female was a negative indicator. The justification given for this connection was that competence and trustworthiness were increased with the economic and social independence that result from social advantage.

---

[227] Miranda Fricker, "Rational Authority and Social Power: Towards a Truly Social Epistemology," *Proceedings of the Aristotelian Society* 98 (1998)., 162-3.
[228] Ibid., 166-7.

Social advantage meant that gentlemen had little to gain and much to lose from deception (challenging the status quo).

This association of indicator-properties with social identities to determine whom to trust is also the case in scientific communities.

> Gravitational wave scientists report that they use the following criteria to judge whether an experiment by another scientist needs to be taken seriously: faith in experimental capabilities and honesty, based on a previous working partnership; personality and intelligence of experimenters; reputation of running a huge lab; whether or not the scientist worked in industry or academia; previous history of failures; "inside information"; style and presentation of results; psychological approach to experiment; size and prestige of university of origin; integration into various scientific networks; nationality.[229]

Fricker uses Shapin's account to bring credence to her hypothesis (shared by many others) that

> [t]here is likely (at least in societies recognizably like ours) to be some social pressure on the norm of credibility to imitate the structures of social power. Where that imitation brings about a mismatch between rational authority and credibility—so that the powerful tend to be given *mere* credibility and/or the powerless tend to be wrongly denied credibility—we should acknowledge that there is a phenomenon of *epistemic injustice*.."

Epistemic injustice, as Fricker defines it, can be the result of the phenomenon of "credibility-overspill" in which indicator properties are seen to convey more credibility than is due as a result of unjustified correlations between general credibility of certain social identities and the rational authority to make competent and trustworthy claims about certain topics. More importantly, this phenomenon can manifest where credibility is withheld from people with certain social identities who *do* have the rational authority to make these types of claims. In her article "The Politics of Credibility", Karen Jones points out that that "testifiers who belong to 'suspect' social groups and who are bearers

---

[229]Collins, *Rethinking Expertise*., 50 footnote 10.

of strange tales can thus suffer a double disadvantage. They risk being doubly

deauthorized as knowers on account of who they are and what they claim to know." [230]

This is what we have found in the case of those excluded from representation in AIDS

studies in the last chapters. Indicator properties introduce socio-political dynamics such

as background assumptions not just into the core of scientific theories, but also into the

core of scientific relationships as well.

Applying these analyses to the previous chapters, Gallo's dismissal of Duesberg

on the grounds that he was not an "AIDS expert", and the more general dismissal of Lo's

claims, can be understood as epistemic injustices. The rational authority of these

scientists to criticize prevailing hypotheses was undermined because they found

themselves on the underside of the powerful people and powerful beliefs of the scientific

community, and their credibility was correspondingly weakened as a result. This

credibility was denied *before* a direct engagement and assessment of the claims that these

scientists were making. [231]

As one could imagine, the challenges to acquiring credibility faced by AIDS

activists, who were often not scientifically trained, regularly had piercings, tattoos, and

unconventional haircuts, and used language and argumentative styles far removed from

those used in scientific circles (all negative indicator properties in the scientific

community), were considerably higher. While Gallo had dismissed Duesberg on the

grounds that he did not have adequate scientific credentials to criticize AIDS research, he

was not surprisingly even more scathing towards activist input. "I don't care if you call

[230] Jones, "The Politics of Credibility.", 158.
[231] If credibility is withheld as a *result* of consistently unfounded or ujustified claims, rather than a major *cause* of their being seen as unfounded or unjustified, this would no longer be considered an epistemic injustice.

[the AIDS activist group] ACT UP, ACT OUT or ACT DOWN, you definitely don't have a scientific understanding of things." [232] This dynamic was even starker in the case of AIDS sufferers who were poor, uneducated, or hailed from developing countries. To reiterate Karen Jones' point, these potential contributors were marginalized both because of their marginalized status in society and because of the "nonconventionality" of their views.

Moment 3: *Responsibility* to facilitate development of potential experts into actual experts and collaborate

By combining the last two insights, we find the scientific community in a challenging position. Relevant experts on complex scientific research questions, especially in the health, environmental, and social sciences, are often located outside of the credentialed scientific community. Simultaneously, people with this expertise, or with the localized experiences that provide the potential to develop this expertise are often also economically and socially vulnerable, either in relation to scientific institutions, governmental/legal institutions, or Western institutions. The current state of affairs, where political empowerment and social organization is often a necessary condition for being heeded by scientific institutions is both ethically problematic and epistemically problematic in terms of the scientific ideals articulated in this dissertation.

Combining these two insights, we also have useful resources with which to address this challenge. Fricker's notion of indicator properties in the last section was used to express the aspects of social identity which are often problematically used to attribute credibility, such as scientific credentials, ethnic identity, gender identity, and formal education. These are also the qualities that tend to track those with the most

[232]Epstein, *Impure Science : Aids, Activism, and the Politics of Knowledge*., 116

power in Western society.   We have found that while use of these indicator-properties

has led to frequent epistemic injustices in the history of the sciences, we have also found,

with Hardwig and other social epistemologists, that the use of indicator properties is a

necessary part of epistemic interactions in both the scientific and the more general social

world.

I believe that the most productive response to the problematic use of indicator

properties is to utilize our more robust notion of the social conditions for expertise to

articulate the indicator properties that *ought* to be used to identify potential experts. For

example, scientists should institutionalize the recognition that experts in how clinical

trials will be understood by particular communities is best located by looking for those

indicator properties associated with those communities instead of those that associate

experts automatically with scientists. Further, epistemic credibility can further track the

powerful *within* lay communities, and the impetus to seek out experts should be sensitive

to this dynamic as well.

Indicator properties can also usefully be combined with the distinction between

potential and *actual* experts I articulated above. While those with potential expertise will

be those with the indicator properties associated with localized experiences, the further

ability to communally organize, interact, and reflect upon these experiences may or may

not be pre-existing within nonscientific communities. Due to the challenges,

complexities, and economic, political, and social exigencies of those living their own

lives out in the world,   [233] organization, communication, and reflective articulation of local

experiences is not always possible. This should not be perceived as a reason to re-

marginalize the members of these communities, but rather since the resources, power,

[233] as opposed to those specially trained and paid to be inquirers

and interest is often aggregated on the scientific side, the responsibility to aid in developing actual experts out of potential experts should also lie on the scientific side (when needed.)

This sensitivity and responsibility is even more important due to the historical fact that lay communities have not been treated well by scientific researchers. Collins and Evans' account is useful to articulate this point. Referring to the example of the expertise held by the Cumbrian sheep farmers, Collins and Evans argue that

> through experience, the [Cumbrian] farmers had developed discrimination in respect of the pronouncements of (in particular) Sellafield authorities: they found the authorities more questionable than they otherwise would, and more questionable than they would seem to an outsider with less experience of this particular social and geographical location. [234]

The farmers' expertise regarding the local Sellafield nuclear plant and its scientific mouthpieces was the result of repeated encounters over time in which the farmers had suspected the plant of increasing the risk of cancer in the area without adequate response, as well as a major nuclear incident in 1957 after which large amounts of milk had to be thrown away but adequate monitoring studies were either never performed or never released to the local residents.

As a result of these Sellafield experiences, the sheep farmers had become very critical of the accuracy and honesty of information coming from scientific and governmental authorities. Thus, when they began to encounter similar types of communications and information from MAFF (Ministry of Agriculture, Fisheries and Food) scientists after Chernobyl, they were already inclined to be skeptical and distrustful of it. Likewise in the case of AIDS medical research, the resistance to research and noncompliance of many communities (especially economically impoverished ones) can

[234] Collins, "The Third Wave of Science Studies: Studies of Expertise and Experience.", 259.

be traced to consistent experiences that have led to a particular sociological

understanding of the dynamics between scientific researchers and their particular

communities.

While Collins and Evans attribute the failure of communication between the

MAFF scientists and the Cumbrian sheep farmers to the farmers' lack of expertise, I

would argue that it should be understood as a *result* of their expertise, i.e. their

sociological understandings of the power and information dynamics between scientists

and people-on-the-ground. They had learned, though experience, to be wary of and

resistant to the promises and recommendations coming from bureaucratic scientific

authorities, and to assume that their knowledge of the concrete realities of the Cumbrian

land and sheep would be ignored. It turned out these beliefs were well-warranted, as the

research from MAFF turned out to be largely problematic in precisely these ways. Not

coincidentally, a similar resistance to and suspicion of scientific research occurred in the

case of early AIDS research, increasing with the level of marginalization of the

populations involved.

This point importantly links up with the insights of many feminist theorists, who

argue that experiences arising from certain social locations can lead to an ability to

understand and maneuver among different social roles. In the case of standpoint theories,

this skill is acquired at least partly as a result of experiences in less powerful social

positions, as these social skills are not as visible or practically vital to those in more

powerful social locations. [235]

---

[235] See Chela Sandoval, "U.S. Third World Feminism: The Theory and Method of Oppositional Consciousness in the Postmodern World," *Genders* 10 (1991)., and the various writings of standpoint theorists including Dorothy Smith, Nancy Hartsock, Patricia Hill Collins, and Sandra Harding for examples.

Collins and Evans recommend that due to the asymmetrical power relationship between scientists and the nonscientific populations, the scientists were the ones who needed to develop the social expertise to interact with the farmers. I am arguing for a similar point and urged for institutionalizing this responsibility into a social norm for scientists, but not for the reasons that Collins and Evans provide. The problem with their argument can be found in their definition of "interactional expertise" which includes two elements, an understanding of the social dynamics and discourses of both the scientific and nonscientific communities involved, *and* the ability to bring these communities into dialogue with each other. In an asymmetrical power relationship (as is almost always the case between scientific expertise and the claims of those outside the scientific community), an understanding (or the concrete conditions for an understanding) of these dynamics and discourses usually resides in those outside of the scientific community (as the standpoint epistemologists argue).

But there are two major hindrances to incorporating this expertise into scientific research. First, those in less powerful positions often face economic and social barriers to developing general articulations of this expertise. In addition, even those who have articulated this expertise are often *more resistant* to dialogue precisely because their experiences have taught them that it as at least ineffectual and at most a process that can lead to the complete displacement and exploitation of their expertise and interests. As historically marginalized populations have not had the power to have their voices heard in a symmetrical dialogue, cases abound where communities find it more effective to use what power they have to hinder the exploitative interactions that would develop

otherwise. This resistance can be seen in the case of the sheep farmers, but also in

similar responses to scientific study by other socially vulnerable peoples.   [236]

On the other hand, a better interactional expertise on the side of the scientists

would lead to the recognition that the Cumbrian sheep farmers have a type of expertise

necessary to provide an accurate and effective understanding of the harms of the

Chernobyl radiation and the appropriate responses to it in the area. From the more

powerful side of the dynamic, a recognition of this expertise *does* lead to a commitment

to dialogue, along with the corresponding norm to respect the integrity of the

interlocutors expertise and interests. Thus, Collins and Evans' theses that "Only one set

of experts need have interactional competence" and "only the party with the interactional

expertise can take responsibility for combining the expertises" miss the point. The

significant insight about interactional expertise is that it has a strong power-indexed

component, and as a result, it manifests quite differently when obtained by those with

more power as opposed to those with less.

The normative point that results is that the scientific community, once it

acknowledges that relevant expertise is often held by nonscientific communities on

particular subjects, should be committed to developing a methodological sensitivity to the

power dynamics and cultural intricacies of the situation. This sensitivity is necessary in

order to successfully engage any relevant nonscientific expertise into research projects,

because those outside of the scientific community that potentially hold this expertise are

also usually less likely to be recognized, and have also been historically resistant to being

involved as a result of their experiences. And the ability to incorporate relevant expertise

[236]Office of Biological and Environmental Research U.S. Department of Energy, "Navajo Nation Irb,"
Protecting Human Subjects 8 (Spring 2003).

to a given question or problem can easily be understood to increase the validity,

reliability, and objectivity of scientific research.

This is a crucial norm because it shows that the shared communal standards in

science, while enabling fruitful engagements in many ways, are often geared away from

granting cognitive authority to expertise that is "indirectly scientific." By this term, I

mean knowledge that is pertinent to scientific questions and epistemic goals, but may not

be derived from scientific training or experiences. We have seen that local experience

and communal interaction and articulation of those experiences is essential for acquiring

expertise, but it need not be scientific communal interactions. Acknowledging that in

many types of science, specifically biomedical, environmental, and social sciences, both

scientific and certain lay experiences and interests are directly involved would require an

important shift in science's self-conception, but it is one that is already being recognized

in areas such as public health, nursing, and was seen directly in AIDS research.

Conclusion

Summary

As a result of the insights of underdetermination, set out in the 20th century most famously by Thomas Kuhn, but also by many others,[237] the traditional scientific standards that were supposed to guarantee unbiased science, such as the accurate measurement of data and licensed logical connections between that data and the hypotheses and theories derived from them, were found insufficient without a further engagement with the background frameworks of metaphysics and methodology that make these possible. But while the means of criticizing measurement and logic are relatively clear, the means and even possibility of criticizing the metaphysical and methodological beliefs behind them are much more problematic.

Arguments regarding the rational criticism of the background commitments in science fall into three general camps. There are those who deny the possibility of such criticism at all, and are charged with introducing irrationalism into the inner sanctum of science. Some argue that metaphysical beliefs in science are continuous with physical beliefs (scientific ones) and are therefore empirically accessible and criticizable as well. Finally, there are some who believe that metaphysical commitments are criticizable, but not directly by appeals to evidence.

While all three of these camps are grappling with the implications of underdetermination arguments, they fail to address a key insight of feminist criticisms of androcentrism and sexism in science (and also the points I found in Kuhn's work in Chapter 1). This is because they do not directly engage how the social conditions of both

---

[237] W.V.O. Quine, Feyerabend, Duhem, Hanson, and Mary Hesse, to name a few,

science and society are relevant to the possibility of self-criticism and innovation in science.   The social conditions necessary for feminist theorists to bring assumptions to light and assess them are also conditions for the criticizability of these beliefs. Feminists were and are able to criticize the androcentric assumptions in science not only because they found empirical, conceptual, and even political flaws in the method and content of science, but because finally feminists, to a greater or lesser degree, achieved the epistemological or cognitive authority in society and in scientific communities to have those criticisms heard and heeded. Before those voices were both politically and epistemically recognized, not only were their criticisms of entrenched views often not heard, but the mere existence and role of the androcentric frameworks themselves went unnoticed.

As a result, the question debated above by the three camps of whether the content of scientific metaphysics infused with social values can be *rationally* criticized (by appeal to what kind of arguments, conceptual, empirical, political?), requires the further question of the *social* possibility of criticizing scientific metaphysics (who is socially allowed to make appeals? How do social conditions of science and society affect the awareness and criticism of metaphysical presuppositions in the first place?) This is the question that this dissertation has examined, and has hopefully provided the outlines of an answer.

This problem isn't new. Kathryn Pyne Addelson, in her 1983 article "The Man of Professional Wisdom", focuses the discussion about the criticism of metaphysical commitments in exactly this direction. Addelson made three general recommendations about how to incorporate the significance of social dynamics in science and to thereby correct the attribution of cognitive authority to its proper domain. I will utilize these

three recommendations to bring together the insights of the dissertation up to this point

and to present the lessons and solutions that I propose.   First, Addelson argues that "*we*

should acknowledge metaphysical commitments as part of the content of scientific

understanding." [238]   As we saw in Chapters 2 and 3, the feminist empiricists all

incorporated this insight in different ways in their philosophies, and simultaneously

provided a more in-depth understanding of the different types of background

beliefs/linking principles/theories of nature that inform scientific practices.

And as we saw in Chapters 4 and 5, this is not merely a theoretical insight. The

sets of acceptable questions and answers in AIDS causation research informed by specific

etiology were significantly different from those that entertained multiple causation as a

possible explanatory framework.   [239]   Likewise, the various background beliefs about the

most effective and most scientific models of clinical trials informed what possible

research programs were developed.

Addelson's second recommendation is to "*open [metaphysical commitments] to*

scrutiny and criticism by specialist and non-specialist alike." This suggestion appears to

be a natural reaction to the argument in the last chapter that the cognitive authority of

scientists in many fields both presupposes expertise derived from outside the scope of

that scientific specialty, as well as applies to domains outside of it. As we saw in

Chapters 4 and 5, this dynamic was also manifested in the early years of AIDS research.

---

[238] Kathryn Pyne Addelson, "The Man of Professional Wisdom," in *Discovering Reality : Feminist Perspectives on Epistemology, Metaphysics, Methodology, and Philosophy of Science*, ed. Sandra G. Harding and Merrill B. Hintikka, *Synthese Library* (Dordrecht, Holland ; Boston Hingham, MA: D. Reidel ;
Sold and distributed in the USA and Canada by Kluwer Boston, 1983).

[239] AIDS update: Multiple causation is now coming back to the forefront of discussions of AIDS causation. It has been found in various cases that other sexually transmitted infections facilitate the likelihood of contracting HIV. In addition, there are mysterious discrepancies between risk behavior and likelihood of contracting HIV in different ethnic populations.

While medical and scientific specialists held cognitive authority over the domain of AIDS etiology, the domain required to inform the beliefs, questions, and frameworks that guided AIDS treatment was significantly broader than the understanding of virology and epidemiology.

Much of the argument in Chapter 5 was intended to establish that populations without scientific credentials (like AIDS activists) had access to sociological and psychological insights due to their experiences that was crucial to achieving effective medical treatment. Conversely, there was nothing special about the knowledge of the AIDS scientific community that would authorize them to assess this sociological and psychological domain. As a result, scientific assumptions about treatment often became entrenched in ways that ultimately impeded the goals of the scientific interventions. Only when criticisms and alternatives to these "metaphysical" (or at least meta-empirical) assumptions were recognized and incorporated did the research program change for the better.

In the language recommended by this dissertation, the specialist expertise of certain "core-group" scientists (those with a background and training in virology, epidemiology, pharmacology, etc) and with significant peer-reviewed articles and credentials) was attributed legitimately to them within the domain of AIDS etiology and therapeutic drug development. Expertise was illegitimately attributed to them within the domain of developing effective clinical trials in the gay communities. [240] Thus, the

---

[240] More precisely, the exclusive cognitive authority of AIDS scientists over the development of clinical trials was the problem. There are of course certain elements of scientific research (biostatistics, an understanding of the medical phenomena being studied, etc.) that *are* in their legitimate domain. But an adequate grasp of these factors is in many cases insufficient to attain a successful clinical trial. Without an understanding of the sociocultural characteristics of the population(s) being studied, clinical trials can still fail to be rigorous or effective, as was shown in the case of early AIDS clinical trials.

criticisms by those who were not initially considered credible experts (AIDS activists), due to their particular background and experiences, were able to achieve expertise and therefore their input was likely to improve the rigor and effectiveness of clinical trials.

The importance of engaging the misalignment between the scope of scientific training and the scope of its credibility relates to Addelson's third and final suggestion. "*We should institutionalize this sort of criticism and make it an explicit part of 'scientific method*.'" [241] We left her last suggestion with "this sort of criticism" quite vague. What types of criticism should we be attempting to institutionalize? Should we understand "this sort of criticism" as a democratic epistemological process whereby the line between "non-specialist" and "specialist" criticism is dissolved? Or should we redefine criticism in terms of different expertises in the domain of the background beliefs of research?

This is a vital question left open by Addelson's account, as well as the decisive question of my dissertation. I have provided arguments that lead to a direct answer to this question that differs markedly from the answers provided by the other feminist thinkers I have discussed.

Nelson incorporates Addelson's first two suggestions into recommendations of the democratic sort. As we saw in Chapter 2, Nelson opens scientific criticism to "scientist and non-scientist alike" insofar as she blurs the conventional line between scientists and non-scientists. Nelson's view of epistemology can be interpreted as an epistemic democracy where one need only be self-conscious and self-critical about the empirical adequacy of one's cognitive commitments to be a citizen with an epistemic vote.

[241] Addelson, "The Man of Professional Wisdom."ibid

This view is not unique to her. Many sociologists of science advocate a similar view.

> To the extent that the public meanings and the imposition of the problematic versions of these by powerful scientific bodies is the issue, then the proper participants are in principle every democratic citizen and not specific sub-populations qualified by dint of specialist experience-based knowledge.[242]

This is consistent with Addelson's suggestion that we should subject these metaphysical commitments to "specialist and non-specialist alike", specifically because for Nelson the distinction between the two is one of degree, not of kind. The "institutionalization", then, is no more than the community-wide awareness of what science really amounts to, and the consequent "self-reflection" of the community to consciously evaluate and subject its theories, metaphysics, *and* values to empirical controls.

While I argued in Chapter 2 that this recommendation is neither adequate to account for the social reality that draws the line between scientists and non-scientists, nor the social changes necessary for non-scientists to be recognized by scientists, I do believe that her broader understanding of scientific practice has its place. While it seems neither accurate nor helpful to deny the specialized training and recognition that separates scientists from non-scientists, Nelson's notion of the activities of a "scientific community" are more helpfully seen as characterizing rational thought in general, and at least a necessary condition for experts of any sort, which as we have seen is not the unique province of scientists. When I attempted to provide an account of expertise in the last chapter, Nelson's characterization of these processes and activities re-entered the picture, not as replacing the definition of a scientific community, but rather as helping to

---

[242] Wynne, 2003, p. 411. See also Jasanoff (2003), Gerold and Liberatore (2001), Council for Science and Technology (2005), etc. This argument is akin to the stakeholder arguments set forth below.

define the practices that are necessary (but not sufficient) conditions for the acquisition of expertise.

Helen Longino advocated four social norms that were required to guarantee the possibility of transformative criticisms in science, or criticisms that were able to affect not only the evidence and conclusions of research, but also the background beliefs that make these possible. These social norms were:

1) Recognized communal avenues for criticism

2) Shared public standards of criticism within the scientific community

3) Community responsiveness to criticisms (uptake)

4) (Tempered) equality of intellectual authority.

These norms were to be enacted *within* the scientific community and were to guide scientists in their interactions with each other. The test to which I have been subjecting these norms is how they can address the persistent dynamics of social entrenchment and epistemic blindness in scientific communities. By confining diversity to perspectives within scientific communities, Longino suffers from the same challenges as Kuhn did. In other words, the background assumptions shared by scientific communities may be challenged by a more diverse set of scientists, but this is not *because* they are scientists, but rather because they were able to bring background assumptions and experiences from other social locations to bear on scientific questions.

In order to be heard, either as fellow scientists or as nonscientists with a voice, these groups have historically required political leverage gained by activism and political clout. But in many other situations, relevant voices are unable (or are as yet unable) to gain this clout but nevertheless have relevant expertise from which to criticize scientific

assumptions. This situation should be epistemically unacceptable, and Longino's norms, if confined to scientific communities, are unable to remedy it. As a result, I advocate a further social norm requiring science to institutionalize "leverage" for relevant social positions currently without cognitive authority to have their views developed and/or recognized.

A social norm of this sort is an important step beyond Longino's social norm advocating "equality of intellectual authority" because it explicitly requires an interaction with the different types of expertise relevant to scientific projects, many existing outside of the scientific community. This requires at least a duality of authorities, with the novel one understood as a provisional cognitive expertise necessary to counteract the traditional, and still occurring, imbalances of cognitive authority at work in the sciences today. The novelty of this approach is that it applies scientific standards to the engagements between scientific communities and nonscientific communities vis-à-vis scientific research questions without advocating absorption of nonscientific communities *into* scientific communities, or vice versa.

This argument appears to lead us straight into a standpoint epistemological framework, where we are commanded to start out thought from the everyday lives of people in oppressed groups in order to identify otherwise obscured features of dominant institutions, their cultures, and their practices. But I would argue that the social norm articulated in this dissertation can avoid the traditional ambiguities and challenges that can easily plague such an account as well as provide effective arguments that go well beyond that of standpoint theories. Since Harding's espousal of standpoint theory, many feminist standpoint theorists, and third world theorists (among others) have brought out

the inherent social complexity of oppression. Many social identities "intersect" in individuals, and even groups, and there is rarely, if ever, a singular quality of "oppression" or "marginalization" that can be identified in isolation from other relations that mark one's location in social groups. As a result, one major ambiguity with feminist standpoint theories surrounds the question of whose standpoints should be attributed cognitive authority ("privileged").

As we saw in Chapter 2, Alison Wylie points out a couple of other important challenges that they face. To be a viable position, Feminist Standpoint theory cannot imply or assume 1) an **essentialist** definition of social categories or collectivities in terms of which epistemically relevant standpoints are characterized or 2) an **automatic epistemic privilege** where those who occupy particular standpoints (usually subdominant, oppressed, marginal standpoints) automatically know more, or know better, by virtue of their social, political location. [243]

My examination of social arrangements does not lead to the inference that there are no relevant factors by which to determine who should be consulted, when, and why. On the contrary, it suggests that people in specific social locations, due to their contingent historical exclusion from realms of cognitive authority and their specific experiences of certain natural and/or social phenomena, are in a unique position to notice and contrast their corresponding metaphysical and empirical frameworks, questions, and problems with those that may have persisted without scrutiny and criticism up until now.

In my account of nonscientific expertise, social locations are not identified with identities (like woman or specific races, which invites biological questions), but rather are correlated to sets of experiential conditions which, when combined with an expertise

[243] Wylie, "Why Standpoint Matters."

socially and critically acquired with others in those conditions, mirror the expertise acquired socially and critically from the experiential conditions within the scientific community. While experience in marginalized social locations is *one* factor that aids in determining who should be heeded and consulted, it is neither sufficient nor automatic.

It is not sufficient in that the experiences from a social location must then be collectively reflected upon and analyzed. The case must be made that these experiences are relevant to the scientific questions at issue, and also that the conceptual categories make sense, refer to experienced realities, and are useful to address the problems at hand. Both sides (the scientific and the non-scientific) must make the case (to others with similar experiences) that they have the relevant expertise to address whatever question is at issue. [244]

Likewise, the cognitive authority of experts is not automatic. While scientific credentials do not automatically confer expertise, neither do "marginalized credentials." Just as staring at a bubble chamber or a night sky through a telescope for a significant period of time does not automatically (in an "aha" moment) enable a scientist to make credible claims about the phenomena perceived, persistent experience in a particular social location (like being a woman, a farmer, or a AIDS patient) does not automatically lead to expertise about the qualities of those experiences. Like Nelson says, a certain type of engagement with our perceptual experiences is necessary, and the skills that result from this type of engagement are what provide expertise. Additionally, the fact that social locations are never private means that there is always the possibility that someone

---

[244] The broader problem of what questions should be asked by scientists, I gather (but will not argue) is a more directly political question of politics, funding, and social interest.

who also has access to the conditions of that social location can provide criticism to any particular claim/conclusion.

I contend that my expertise-leveraging norm is capable of addressing the challenges of epistemic blindness and social entrenchment. I brought out how meta-empirical assumptions about the world serve to shape the acceptable questions, answers, and challenges to empirical scientific programs. Without searching for potential experts and committing to the cultivation and recognition of experts within marginalized social locations (sometimes within and sometimes outside of scientific communities), there is no institutional safeguard against these assumptions remaining unrecognized, unjustified, and immune to criticism.

One of the shared standards of the scientific community is to be responsive to relevant criticisms to both their own claims and the background assumptions that inform them. Another common scientific standard has also been that only those with scientific credentials have the expertise to voice relevant criticisms. My analysis of AIDS treatment activism and expertise acquisition has shown that relevant criticisms often arise from people who lack these credentials, and thus have shown that the second standard comes into direct conflict with the first.

As responsiveness to criticism is more critical to the cognitive and practical aims of science, it is clear that the second standard is the one that needs to change. Cognitive authority should be based upon expertise relevant to a given question. Scientific credentials are not therefore useless, but they should be understood in terms of the sociological conditions for the acquisition of a specific expertise on certain cognitive questions, not broadly as conferring expertise full stop (Fricker's credibility-overspill).

Especially in the medical, environmental, and social sciences, scientific questions are often an intricate combination of theoretical, practical, ethical, and socio-psychological questions, and in these cases it is important to ascertain which kinds of expertise and interests are relevant. Relevant expertise in these situations will often rest in unconventional places.

The normative point that results is that the scientific community, once it acknowledges that relevant expertise is often held by nonscientific communities on particular subjects, should be committed to developing an awareness of and methodological sensitivity to the power dynamics and cultural intricacies of the situation (Collins and Evans' interactional expertise). This awareness is necessary in order to successfully engage communities with nonscientific expertise because those outside of the scientific community that have expertise are also usually less inclined to be recognized, and further resistant to being involved as a result of *their* interactional expertise. And the ability to incorporate relevant expertise to a given question or problem can easily be understood to increase the validity, reliability, and objectivity of scientific research.

Conclusion and future directions

The overlapping character of power dynamics is a reality of social systems, and as such, overlapping issues of marginalization and the corresponding issues of recognition will always exist. For this reason, I have refrained from articulating a specific norm to govern all cases, but rather have utilized the analyses of background assumptions, cognitive authority, and expertise to provide a guide to determining where to locate relevant marginalized populations outside the scientific community. This question of

epistemic relevance will always be contentious, but hopefully my arguments in this dissertation have established that relevance a) cannot be automatically conferred, either upon a scientific community or a particular lay population, and b) *can* be initially conferred, by attributing it to those with specialized expertise within a scientific community and, when applicable, seeking out those with conditioned experiences with different facets of a given phenomenon outside of the scientific community. It can then be checked based upon well-articulated goals of particular research problem (be they explanatory breadth, prevention results, efficacy, etc.)

In articulating a social norm for scientific practice, it is also easy to be drawn into the Scylla and Charibdys of presenting a norm that is either too vague or too specific. If too vague, it is easy to challenge the use and applicability of such a norm outside of the hallowed halls of abstract philosophy of science. If too concrete, the generalizability of the norm begins to be undermined. I have already gestured toward one such tension within such a norm by drawing attention to the different implications of the power dynamics *within* AIDS research communities and a corresponding norm to heed marginalized scientists in the case of AIDS etiology (Ch. 4), as opposed to the power dynamics *between* AIDS researchers and particular lay communities and the challenges that attend to such a dynamic (Ch.5).

In this final section, I will indicate the practical instantiations of this norm in these two general types of situations. It will be a goal of my future work to better articulate how scientific regulations and policies can recognize and support the development of practices that are capable of manifesting this social norm, and thus increasing the self-corrective and innovative capacities of science.

In the case of power imbalances within the scientific community, I made suggestions as to the types of institutionalized structures which leverage cognitive authority among scientists in the ways suggested by the general social norm that I advocate in Chapter 4. Institutional structures that regularly facilitate engagement with heterodox views within the scientific community, such as the AmFAR forum and "debate issues" of both popular and scientific periodicals, are prime examples of the application of this norm.

These types of engagements serve many purposes vital to the scientific endeavor. First, they prevent hasty or assumed closure of scientific debates before it is communally recognized that sufficient evidence has been provided to stop asking certain questions. Second, they can prevent the phenomenon where certain scientists, feeling unjustly ignored or underrecognized for their views, abandon the scientific channels of justification altogether and make appeals to those outside of the scientific community who usually do not have the background or training to be critical of those views (the Duesberg phenomenon). Third, these forums can improve the public understanding of science by going public with not only the results, but also the process of arising at scientific consensus. Finally, and most importantly for my purposes, these types of open debates can serve to prevent both the social entrenchment and epistemic blindness toward metaphysical, methodological, and/or practical assumptions that often occur in scientific research.

The implementation of this social norm becomes much more complicated when the marginalized expertise resides not in heterodox scientists, but rather in people who are not scientists at all. As we saw in Chapter 5, the standard approach to scientific

research that engages nonscientific populations, such as medical research, has traditionally been based on the "gold standard" of randomized clinical trials, stressing the individual rather than social or environmental factors of explanation, and separating researchers as the agents of research from the individuals or communities that serve as the objects of research.

The problems of epistemic blindness and social entrenchment emerge in the context of this model of research because there are no scientific channels through which the methodological assumptions (such as that randomized trials are the "gold standard"), metaphysical assumptions (such as that human behavior should be analyzed in terms of individual psychology and motivating factors), and further social and cultural assumptions can be challenged. Especially in the human sciences where explanations and interventions are so closely intertwined, these assumptions are further entrenched due to the conceptual separation between the efficacy of a study and the efficiency of its conclusions.

The real-life consequences of these entrenched assumptions and methodological models emerged, as we have seen, in critiques of AIDS treatment research in the early 80's, and they have been continuously challenged by many social scientists and public health practitioners in general on both epistemic and ethical grounds. It is by now widely recognized that the results of human research have often been unbalanced, reflecting and benefiting certain populations and ignoring and neglecting others. Researchers and practitioners alike have articulated means to remedy these problems. Generally, the proposed solutions have included calls for: increased attention to the complex issues that compromise the health of people living in marginalized communities; more integration of

research and practice; greater community involvement and control; increased sensitivity

to and competence in working with diverse cultures; expanded use of both qualitative and

quantitative research methods; and more focus on health and quality of life, including the

social, economic, and political dimensions of health and well-being. [245]   In other words, as

we saw in the case of AIDS, they have argued for more collaborative methods that

incorporate nonscientific communities as agents into the practice of research.

   While the idea of community collaboration was first introduced into discussions

of research ethics as early as the mid-70's, it has only been with the catalyzing effects of

the AIDS epidemic in the 1980's and 90's that research which collaborates with

communities has emerged and begun to be recognized by governmental and scientific

institutions. [246] As a result of these catalysts, many of which have been articulated

throughout this dissertation, institutions like the NIH, CDC, and FDA have responded by

funding collaborative research that utilize community-based participation. These

organizations include the W.K. Kellogg Foundation's Community-Based Public Health

Initiative, the Pew Charitable Trusts' Community-Campus Partnerships for Health, the

CDC's Urban Centers for Applied Research in Public Health Initiative, and the World

Health Organization's Healthy Cities Initiative.

   Community-based Participatory Research is an approach that incorporates

members of nonscientific communities as agents in the process of research, and its many

instantiations manifest my social norm in different ways and in different contexts.

---

[245] Barbara A. Israel, Amy J. Schulz, Edith A. Parker, and Adam B. Becker, "Review of Community-Based Research: Assessing Partnership Approaches to Improve Public Health," *Annual Review of Public Health* 19 (1998).

[246] Gary B Melton, Robert Levine, Gerald P. Koocher, Robert Rosenthal, William C. Thompson, "Community Consultation in Socially Sensitive Research: Lessons from Clinical Trials of Treatments for Aids," *American Psychologist* 43, no. 7 (July1988).

According to the Agency for Healthcare Research and Quality's 2003 National

Healthcare quality Report   [247], community-based participatory research (or CBPR) is

defined as

> a collaborative research approach that is designed to ensure and establish structures for participation by the communities affected by the issue being studied, representatives of organizations, and researchers in all aspects of the research process to improve health and well-being through taking action, including social change.[248]

CBPR exemplifies my social norm in various ways. First, and most obviously, the

"fundamental characteristic of community-based research. . .is the emphasis on the

participation and influence of nonacademic researchers in the process of creating

knowledge." [249] This recognition of the relevance of nonscientific voices in the scientific

process is the key component of my social norm. CBPR further exemplifies my social

norm as a

> . . .co-learning and empowering process that facilitates the reciprocal transfer of knowledge, skills, capacity, and power. For example, researchers learn from the knowledge and "local theories" of community members, and community members acquire further skills in how to conduct research.[250]

Recognizing research in certain domains as a co-learning process with information and

expertise coming from both the scientific community and particular lay communities

further exhibits the social norm that I wish to advocate. This conceptualization also

recognizes that the crucial advances required for research are not just increases in data,

---

[247] Agency for Healthcare Research and Quality, "National Healthcare Quality Report," (2003).I am utilizing the definition provided by this report for three reasons. First, since it was published in 2003, it is a recent formulation that has taken into account the experiences and experiments in collaborative research that occurred previously. Second, this report set out to provide a systematic review and synthesis of the extant scientific literature regarding CBPR, and thus does not represent any individual theories or viewpoint, but rather a common consensus. Finally, since my main interest is the legitimation of collaborative research to the larger scientific research and funding institutions, that this report is set out by a governmental scientific agency makes its analysis makes its voice more likely to be accepted.
[248] Ibid p.2
[249] Israel, "Review of Community-Based Research: Assessing Partnership Approaches to Improve Public Health."
[250] Ibid.

but also the development of skills, capacities, and cognitive leverage (increasing the

"power" of community populations) in certain domains.

> Furthermore, recognizing that socially and economically marginalized communities often
> have not had the power to name or define their own experience, researchers involved
> with community-based research acknowledge the inequalities between themselves and
> community participants, and the ways that inequalities among community members may
> shape their participation and influence in collective research and action. Attempts to
> address these inequalities involve explicit attention to the knowledge of community
> members, and an emphasis on sharing information, decision-making power, resources,
> and support among members of the partnership.[251]

By recognizing that social inequalities (both between marginalized communities and

scientific communities, *and* among marginalized communities themselves) influence how

expertise and decision-making power are attributed, CBPR incorporates empowerment of

non-scientific communities as a key component of research. In this dissertation, I have

suggested connections between the political process of empowerment and the epistemic

process of becoming an expert about one's experiences and circumstances. In the future,

I want to examine in detail the ways in which different CBPR programs develop the skills

and conceptual capacities of community members, and use these examples to better

understand the process of developing expertise as both a theoretical and practical

capacity.

In addition to utilizing CBPR as an illustration and application of my social norm,

I believe that my arguments can be utilized to improve the efficacy and support of CBPR

approaches as well. While CBPR methods are increasingly being funded and attempted

throughout the world, these methods continue to face persistent challenges from scientific

and governmental institutions. In their article, "Review of Community-based Research:

Assessing Partnership Approaches to Improve Public Health", Israel, Schulz, Parker, and

---

[251] ibid

Becker categorize and list many of the internal challenges that they have encountered

during the process of engaging in and assessing the literature of CBPR. One important

problem is that

> Community-based research is continually challenged by questions raised regarding its
> validity, reliability, and objectivity for both basic research and evaluation research. The
> predominance of the scientific method in public health makes it difficult to convince
> academic colleagues, potential partners, and funders of the value and quality of
> collaborative research.[252]

By connecting participation of outside experts to the possibility of scientific self-

criticism, the arguments in this dissertation can illustrate how CBPR, by manifesting a

constitutive social norm of science, can be understood to increase the validity and

objectivity of research.

My emphasis on the expertise of lay communities distinct from their political

rights is vital, for I believe that its neglect is one major reason why the epistemic value of

collaborative research is brought into question. Extant justifications of CBPR are based

upon a mix of political/democratic values (stakeholders "affected by research", social

change, decisionmaking power, mutual ownership) and epistemic values (expertise). As

we saw earlier, arguments for stakeholding confer to the public communities affected by

research, or the public at large, a legitimate voice in the scientific process, just as the

political stake of people affected by a democratic governmental structure grants them a

legitimate voice in the political process. This "stakeholding" rationale for collaborative

research can be found in the definition of CBPR quoted above in its political references

to political notions such as "ownership", "representation", and "participation" allocated

to those "affected by research."

---

[252] Israel, "Review of Community-Based Research: Assessing Partnership Approaches to Improve Public Health."

While there are straightforward arguments for the input of stakeholders in certain aspects of research like risk assessment, as we saw in the last chapter, the concept of stakeholding and democratic science in general is too broad and vague in most cases to legitimate the norms of a more thoroughgoing incorporation into scientific research, such as those that guide CBPR. The complete dissolution of the boundary between the epistemological rights of scientists and the political rights of the public also results in the problem of weighing the opinions of the vast number of potential (and legitimate) stakeholders relevant to scientific decision-making. In addition, stakeholding arguments are unable to respond or account for the idea that there is something about scientific training above and beyond de facto attributions of authority that legitimate scientific analyses of situations, as well as the many predictions and successes that they have and continue to yield.

As a result of some combination of these issues, those who begin with a notion of stakeholding inevitably start appealing to a type of expertise to be found in the public. Those who choose not to jettison all talk of expertise justify stakeholder input because they have some sort of "lay expertise", "insider expertise", "local expertise", or other similar notions, which are either contrasted with scientific expertise or used to replace it.[253] While I believe that these terms indicate a fruitful epistemic avenue, in most extant discussions of CBPR this type of expertise is not clearly defined or distinguished from the political justifications of stakeholders, as is evident in the combination of both found in the definition of CBPR cited above. Here we ironically run into the same type of problem that we encountered in discussions of Feminist Standpoint Theory of epistemically determining who needs to be recognized and privileged and why.

[253] Krimerman, "Participatory Action Research: Should Social Inquiry Be Conducted Democratically? .";

As a result of the discussion of expertise above, we are in a better position to distinguish relevant stakeholders from relevant experts. We know that scientists can initiate the search for relevant potential experts by locating the indicator properties that are associated with local experiences with the social, geographical, and/or environmental phenomena that they are examining. We know that in addition to local experiences, communal socialization and critical interaction with local experiences are necessary to develop from potential experts into actual experts. Moreover, the recognition of scientific responsibility to help *develop* potential expert communities into a community of experts can provide an important rationale for CBPR's commitment to empowerment.

This dissertation has shown that a commitment to recognizing and empowering critical positions in marginalized locations is not only politically fruitful to achieve a community more able to advocate for and address its own problems, but also an epistemic priority because it can yield a community of experts with unique and necessary contributions to scientific questions, and ultimately can increase the accuracy and the scope of knowledge about ourselves and the world.

Bibliography

Addelson, Kathryn Pyne. "The Man of Professional Wisdom." In *Discovering Reality : Feminist Perspectives on Epistemology, Metaphysics, Methodology, and Philosophy of Science*, edited by Sandra G. Harding and Merrill B. Hintikka, 165-86. Dordrecht, Holland ; Boston

Hingham, MA: D. Reidel ;

Sold and distributed in the USA and Canada by Kluwer Boston, 1983.

Alcoff, Linda Martin. "On Judging Epistemic Credibility: Is Social Identity Relevant?" In *Engendering Rationalities*, edited by Nancy Tuana, 53-80. Albany, NY: SUNY Press, 2001.

Assessment, Office of Technology. "The Cdc's Case Definition of Aids: Implications of Proposed Revisions." In *The Definition of AIDS: Epidemiological, Clinical, and Policy Implications*. Washington D.C., 1992.

Barber, Bernard. "Resistance by Scientists to Scientific Discovery." *Science* 134, no. 3479 (1961): 596-602.

Barnes, Barry. *Interests and the Growth of Knowledge*. London: Routledge & Kegan Paul, 1977.

———. *Scientific Knowledge and Sociological Theory*. London: Routledge & Regan Paul, 1974.

Collins, H.M, Robert Evans. "The Third Wave of Science Studies: Studies of Expertise and Experience." *Social Studies of Science* 32, no. 2 (2002): 235-96.

Collins, H.M. and Evans, R. *Rethinking Expertise*. Chicago: University of Chicago Press, 2007.

Collins, Harry M. "An Empirical Relativist Programme in the Sociology of Scientific Knowledge." In *Science Observed: Perspectives on the Social Study of Science*, edited by K Knorr-Cetina and M. J. Mulkay, 85-114. London; Beverly Hills: Sage Publications, 1983.

Duhem, Pierre. "[Selections from] the Aim and Structure of Physical Theory." In *Can Theories Be Refuted?*, edited by Sandra Harding. Dordrecht: Reidel, 1976.

Epstein, Steven. *Impure Science : Aids, Activism, and the Politics of Knowledge*. Berkeley: University of California Press, 1996.

Evans, Robert. "The Sociology of Expertise: The Distribution of Social Fluency." *Sociology Compass* 2, no. 1 (2008): 281-98.

Farber, Celia and Liversidge. "Aids: Words from the Front." *Spin* 1988.

Fricker, Miranda. "Rational Authority and Social Power: Towards a Truly Social Epistemology." *Proceedings of the Aristotelian Society* 98 (1998): 159-77.

Glymour, Clark. "Relevant Evidence " *Journal of Philosophy* 72 (1975): 403-20.

Goldman, Alvin. "Experts: Which Ones Should You Trust?" *Philosophy and Phenomenological Research* 63, no. 1 (2001).

———. *Liaisons: Philosophy Meets the Cognitive and Social Sciences*. Boston, MA: MIT Press, 1992.

Hacking, Ian. *The Social Construction of What?* Cambridge, Mass.: Harvard University Press, 1999.

Hanson, Norwood Russell. "An Anatomy of Discovery." *The Journal of Philosophy* 64, no. 11 (1967): 321-52.

Haraway, Donna. "Situated Knowledges: The Science Question in Feminism and the Privilege of Partial Perspective." *Feminist Studies* 14, no. 3 (Autumn1988): 575-99.

Harding, Sandra. "Rethinking Standpoint Epistemology: What Is "Strong Objectivity"?" In *The Feminist Standpoint Theory Reader : Intellectual and Political Controversies*, edited by Sandra G. Harding, 127-40. New York: Routledge, 2004.

Harding, Sandra G. *The Science Question in Feminism*. Ithaca: Cornell University Press, 1986.

Hardwig, John. "Epistemic Dependence." *The Journal of Philosophy* 82, no. 7 (1985): 335-49.

Hartsock, Nancy. "The Feminist Standpoint: Developing the Ground for a Specifically Feminist Historical Materialism." In *Discovering Reality : Feminist Perspectives on Epistemology, Metaphysics, Methodology, and Philosophy of Science*

edited by Sandra G. Harding and Merrill B. Hintikka, 183-210. Dordrecht, Holland ; Boston

Hingham, MA: D. Reidel ;

Sold and distributed in the USA and Canada by Kluwer Boston, 1983.

Irwin, Alan, and Brian Wynne. "Conclusions." In *Misunderstanding Science? The Public Reconstruction of Science and Technology*, edited by Alan Irwin and Brian Wynne. New York: Cambridge University Press, 1996.

Israel, Barbara A., Amy J. Schulz, Edith A. Parker, and Adam B. Becker. "Review of Community-Based Research: Assessing Partnership Approaches to Improve Public Health." *Annual Review of Public Health* 19 (1998): 173-202.

Jones, Karen. "The Politics of Credibility." In *A Mind of One's Own: Feminist Essays on Reason & Objectivity*, edited by Louise M. Antony and Charlotte Witt, 154-76. Boulder, CO: Westview Press, 2002.

King, M.D. "Reason, Tradition, and the Progressiveness of Science." In *Paradigms and Revolutions*, edited by Gary Gutting, 97-116. ????: ??????, 1971.

Kingston, Tim. "Justice Gone Blind: Cmv Patients Fight for Their Sight." *Coming Up!* February 1989 (1989).

Kitcher, Philip. "A Plea for Science Studies." In *A House Built on Sand: Exposing Postmodernist Myths About Science*, edited by Noretta Koertge, 32-56. New York: Oxford University Press, 1998.

Knorr-Cetina, Karin. "The Constructivist Programme in the Sociology of Science: Retreats or Advances?" *Social Studies of Science* 12 (1982): 320-24.

Krimerman, Leonard. "Participatory Action Research: Should Social Inquiry Be Conducted Democratically? ." *Philosophy of the Social Sciences* 31 (2001): 60-82.

Kuhn, Thomas S. *The Essential Tension : Selected Studies in Scientific Tradition and Change*. Chicago: University of Chicago Press, 1977.

———. *The Structure of Scientific Revolutions*. 3rd ed. Chicago, IL: University of Chicago Press, 1996.

Lauritsen, John. "Saying No to Hiv: An Interview with Prof. Peter Duesberg, Who Says, "I Would Not Worry About Being Antibody Positive,"." *New York Native* 1987, 17-25.

Levine, Carol, Nancy Neveloff Dubler and Robert J. Levine. "Building a New Consensus: Ethical Principles and Policies for Clinical Research on Hiv/Aids." *IRB: Ethics and Human Research* 13, no. 1/2 (Jan - Apr 1991): 1-17.

Longino, Helen. "In Search of Feminist Epistemology." *Monist* 77, no. 4 (1994): 472-85.

Longino, Helen E. *The Fate of Knowledge*. Princeton, N.J.: Princeton University Press, 2002.

———. *Science as Social Knowledge : Values and Objectivity in Scientific Inquiry*. Princeton, N.J.: Princeton University Press, 1990.

———. "Subjects, Power, and Knowledge: Description and Prescription in Feminist Philosophies of Science." In *Feminist Epistemologies: Thinking Gender*, edited by Linda Alcoff and Elizabeth Potter, 101-19. New York: Routledge, 1993.

Masterman, Margaret. "The Nature of a Paradigm." In *Criticism and the Growth of Knowledge* edited by Imre Lakatos and Alan Musgrave. Cambridge; New York: Cambridge University Press, 1974.

Melton, Gary B, Robert Levine, Gerald P. Koocher, Robert Rosenthal, William C. Thompson. "Community Consultation in Socially Sensitive Research: Lessons from Clinical Trials of Treatments for Aids." *American Psychologist* 43, no. 7 (July1988): 573-81.

Mildvan, Donna et al. "An Approach to the Validation of Markers for Use in Aids Clinical Trials." *Clinical Infectious Diseases* 24 (1997): 764-74.

Nelson, Lynn Hankinson. "Empiricism without Dogmas." In *Feminism, Science, and the Philosophy of Science*
edited by Lynn Hankinson Nelson and Jack Nelson, 95-119. Dordrecht ; Boston: Kluwer Academic Publishers, 1996.

———. *Who Knows : From Quine to a Feminist Empiricism*. Philadelphia, Pa.: Temple University Press, 1990.

Palca, Joseph. "Duesberg Vindicated? Not Yet." *Science* 254, no. 18 October 1991 (1991).

Polanyi, Michael. *Personal Knowledge*. Chicago: University of Chicago Press, 1958.

Polanyi, Michael, and Harry Prosch. *Meaning*. Chicago: University of Chicago Press, 1977.

Potter, Elizabeth. *Gender and Boyle's Law of Gases*, *Race, Gender, and Science*. Bloomington: Indiana University Press, 2001.

Quality, Agency for Healthcare Research and. "National Healthcare Quality Report." 2003.

Risjord, Mark W. *Woodcutters and Witchcraft : Rationality and Interpretive Change in the Social Sciences*, *Suny Series in the Philosophy of the Social Sciences*. Albany: State University of New York Press, 2000.

Sandoval, Chela. "U.S. Third World Feminism: The Theory and Method of Oppositional Consciousness in the Postmodern World." *Genders* 10 (1991): 1-24.

Smith, Dorothy E. "Women's Perspective as a Radical Critique of Sociology." In *The Feminist Standpoint Theory Reader : Intellectual and Political Controversies*, edited by Sandra G. Harding, 21-33. New York: Routledge, 2004.

Sormati, Mary, Leslie Pereira, Nabila El-Bassel, Susan Witte, Louisa Gilbert. "The Role of Community Consultants in Designing an Hiv Prevention Intervention." *AIDS Education and Prevention* 13, no. 4 (2001): 311-28.

Stone, Valerie E MD, MPH, Maya Y Mauch, Kathleen Steger RN, MPH, Stephen F Janas MA, Donald E Craven MD. "Race, Gender, Drug Use, and Participation in Aids Clinical Trials: Lessons from a Municipal Hospital Cohort." *Journal of General Internal Medicine* 12, no. 3 (March 1997): 150-57.

Thagard, Paul. "Collaborative Knowledge." *Nous* 31, no. 2 (1997): 242-61.

Treichler, Paula A. "Aids, Hiv, and the Cultural Construction of Reality." In *The Time of Aids: Social Analysis, Theory, and Method*, edited by Gilbert Herdt and Shirley Lindenbaum, 65-98. Newbury Park, CA: Sage 1992.

U.S. Department of Energy, Office of Biological and Environmental Research. "Navajo Nation Irb." *Protecting Human Subjects* 8 (Spring 2003).

Wylie, Alison. "The Engendering of Archaeology: Refiguring Feminist Science Studies." *Osiris* 12, no. Women, Gender, and Science: New Directions (1997): 80-99.

———. "Gender Theory and the Archaeological Record: Why Is There No Archaeology of Gender?" In *Engendering Archaeology: Women and Prehistory*, edited by Joan M. Gero and Margaret W. Conkey, 31-56. Oxford: Blackwell, 1991.

———. *Thinking from Things : Essays in the Philosophy of Archaeology*. Berkeley: University of California Press, 2002.

———. "Why Standpoint Matters." In *The Feminist Standpoint Theory Reader : Intellectual and Political Controversies*, edited by Sandra G. Harding, 339-52. New York: Routledge, 2004.