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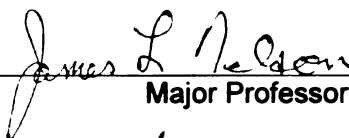
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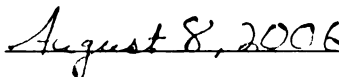
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**CAN MEDICAL THEORIES BE OBJECTIVE?**

**By**

**Zahra Iqbal Meghani**

**A DISSERTATION**

**Submitted to  
Michigan State University  
In partial fulfillment of the requirements  
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## **ABSTRACT**

### **ARE MEDICAL THEORIES OBJECTIVE?**

**By**

**Zahra Iqbal Meghani**

Are medical theories objective? Christopher Boorse and Tristram Engelhardt, two well-regarded philosophers of medicine, offer radically different responses to that question. Boorse appears to be committed to the idea that medical theories can, in principle, provide objective accounts of the medical conditions at issue. Englehardt, in contrast, has argued that medical theories cannot avoid being laden with social and political values because they reflect human attempts to correlate a variety of phenomena for the social purposes of explanation, prediction, and control.

Although both philosophers' arguments are interesting, neither one's analysis is satisfactory. I believe Boorse's position is flawed because it is based on an uncritical commitment to the logical-positivist conception of objectivity. While Engelhardt does not make that mistake, I contend that the argument that he gives in support of his stance (that all theories of disease are normative) is weak. A key goal of this dissertation is to remedy the shortcomings of Boorse's and Engelhardt's analyses. To that end, I analyze and critique the concept of objectivity that is based on the logical-positivist epistemology. I construct an alternative account of objectivity that is informed by actual human knowledge-building practices. Amongst other things, my notion of objectivity includes strategies that could be used to identify the social, ethical, and political values shaping medical theories about particular conditions.

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## DEDICATION

With many thanks to A.

In appreciation of my parents, brothers, and my grandfather.

## ACKNOWLEDGMENTS

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## CHAPTER ONE

### *1.1 A Brief Sketch of The Problem Motivating The Dissertation*

Are medical theories, say about ulcers or the pre-menstrual syndrome, objective? In other words, do understandings of the phenomena that are categorized as diseases, syndromes, or disorders incorporate the social, ethical, or political values and concerns of medical theorists and their societies? Or are they determined solely by the relevant facts about nature, which are independent of any person's or community's social, ethical, or political values?

Christopher Boorse and Tristram Engelhardt, two well-regarded philosophers of medicine, offer radically different responses to that question. Boorse appears to be committed to the idea that medical theories can, in principle, provide objective accounts of the medical conditions at issue. Engelhardt, in contrast, has argued that medical theories cannot avoid being laden with social and political values because they reflect human attempts to correlate a variety of phenomena for the social purposes of explanation, prediction, and control.

Although both philosophers' arguments are interesting, neither one's analysis is satisfactory. I believe Boorse's position is flawed because it is based on an uncritical commitment to the logical-positivist conception of objectivity. While Engelhardt does not make that mistake, I contend that the argument that he gives in support of his stance (that all theories of disease are normative) is weak. A key goal of this dissertation is to remedy the shortcomings of Boorse's and Engelhardt's analyses. To that end, I analyze and critique the concept of objectivity that is based on the logical-positivist epistemology. I construct an alternative account of objectivity that is

informed by actual human knowledge-building practices. Amongst other things, my notion of objectivity includes strategies that could be used to identify the social, ethical, and political values shaping medical theories about particular conditions. Thus, I radically re-configure the ground conditions for future discussions in philosophy of medicine about the objectivity of medical theories.

Besides critiquing the answer that philosophers of medicine might give to the question about the objectivity of medical theories, I also assess the response that medical theorists<sup>1</sup> might give to that question, "can medical theories be objective?". In all likelihood, their response to that question will be a resounding "Yes." Their answer must be understood in light of their continued commitment to the logical-positivist conception of objectivity. Logical-positivists, such as Carl Hempel, claimed that objectivity of science and theories was assured by requiring inter-subjective confirmation of evidence cited in support of particular hypotheses. That belief appears to be premised on the assumption that the process of intersubjective verification (or loosely speaking, peer review) of the evidence (given in support of a particular hypothesis) filters out claims tainted with the idiosyncratic beliefs and values of theorists. That occurs, presumably, because, first, not all of the peer reviewers would subscribe to those particular values, and thus, they would reject claims loaded with values. And second, claims that are laden with social, ethical, and political values cannot be empirically verified; thus, the peer reviewers would reject such claims as well. The logical positivist notion of objectivity held sway in the sciences and in medicine during the 20th century and appears to reign even today.

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<sup>1</sup>By the term "medical theorists" I mean all persons involved in constructing and testing medical theories.



In recent years, philosophers of science, such as Helen Longino and Lynn Hankinson Nelson, have formulated particularly compelling critiques of the logical-positivist notion of objectivity. Using a number of scientific theories as case studies, Longino has argued that the logical-positivist conception of objectivity cannot guarantee value-free theories because it does not control for social, ethical, and political values that may be embedded in background assumptions mediating the relationship between evidence and hypotheses, and thus, could be influencing the shape of the scientific theories.<sup>2</sup> Nelson, in tandem, has made the case that the logical-positivists failed to recognize that scientists are part of the larger social order, which both permits and shapes their work, and thus, science cannot be regarded as autonomous from the values of that community.<sup>3</sup>

Unfortunately, philosophers of medicine, such as Boorse and Engelhardt, and medical theorists do not appear to have been attentive to these developments in philosophy of science and epistemology, and thus, their response to the question "Are medical theories objective?" are uninformed. The aim of this dissertation is to address that oversight in the following two ways:

1. By constructing an argument that demonstrates that Longino's and Nelson's critique of the logical-positivist conception of objectivity has relevance for medicine and philosophy of medicine.
2. By correcting the flaws in Longino's and Nelson's critique of the logical-positivist conception of objectivity and using their analysis to construct an

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<sup>2</sup>Longino 1993 and 2001.

<sup>3</sup>Nelson 1990.

alternative notion of objectivity that will be of use to philosophers of medicine and medical theorists as they assess particular medical theories.

My engagement with the issue of the objectivity of medical theories reflects my interest in bioethics, philosophy of science, and epistemology. I bring recent developments in epistemology and philosophy of science (regarding the nature of knowledge building activities) to bear on the answer that philosophers of medicine and medical theorists may give to the question about the objectivity of medical theories. I do so to demonstrate that epistemology and philosophy of science can offer medicine and philosophy of medicine crucial analyses and critiques about their commitments and practices. That would enable them to be disciplines that are much more self-reflective than they are currently.

In the remainder of this chapter, I outline the logical-positivist conception of objectivity. In the second chapter, I give a detailed account of Helen Longino's and Lynn Hankinson Nelson's analysis of the logical-positivist notion of objectivity, followed by a critique of their work. Then, I formulate a notion of objectivity that is a viable alternative to the logical positivist one. In chapter three, I use my conception of objectivity to bring to light some of the values that motivate, are embedded in, and espoused by two recent medical theories about monthly menstruation. In chapter four, I argue that Christopher Boorse's and Tristram Engelhardt's ability to identify the social and political values embedded in the two menstruation theories is limited. Boorse is compromised because of his (unacknowledged) commitment to the logical positivist notion of objectivity. While Engelhardt does not make that mistake, his position is weak because, among other things, he does not provide any strategies for

identifying social and political values shaping medical theories (this is a shortcoming of Boorse as well). I argue that by adopting my conception of objectivity their assessment of medical theories, including the two menstruation-as-pathology theories, would be informed by actual human epistemic practices *and* their analyses would be more sensitive to the values shaping medical theories than they are capable of being as they stand now. In the fifth and final chapter, I identify and respond to two objections to my notion of objectivity.

As I have now mapped out the road I travel in the course of the dissertation, the remainder of this chapter is a prelude to that journey. I begin by outlining the logical-positivist conception of objectivity, then, I sketch the critique of this paradigm that has been formulated by various epistemologists and philosophers of science, with special attention to Helen Longino's and Lynn Hankinson Nelson's work. I focus on them because, first, they have developed substantive critiques of the logical-positivist conception of objectivity that are directly relevant to my project. And second, I use their analyses of gender biased scientific theories to guide me as I critique the two sexist theories that medicalize menstruation (I use those two theories to demonstrate the efficacy of one of the strategies I develop for identifying social and political values shaping medical theories). Thus, their work has an edge for my purposes that is not there in the works of other critics of the logical positivist epistemology.

After that, I argue that by taking to heart my notion of objectivity, philosophers of medicine may be able to formulate a fairly robust conception of the epistemic responsibilities of medical theorists. That notion of epistemic responsibility would call on medical theorists to create theoretical accounts of women's and men's physiology that are informed by actual human epistemic practices as opposed to ones that are

uninformed about them (by virtue of being based on the logical-positivist notion of objectivity). I close the chapter by acknowledging that although philosophers of medicine may come to see the wisdom of rejecting the logical-positivist conception of objectivity, they may still have reservations about adopting my notion of objectivity.

## *1.2 A Road Map to The Rest of The Dissertation*

As a philosophical movement, the development of the logical-positivist empiricist epistemology was a reaction to the heavily metaphysical, non-empirical theorizing of the 19th century. During the 20th century, the logical-positivist epistemology became deeply embedded in the natural and social sciences as well as in medicine. It remains entrenched within these disciplines even today. Although, in some of the social sciences, such as sociology, a number of theorists have rejected that epistemic paradigm,<sup>4</sup> that is not the case in medicine. One would be hard pressed to find a medical textbook whose editors are aware that they owe their methodology, especially their notion of objectivity, to logical-positivism.

The success of that epistemology in establishing itself in the sciences and in medicine is attributable to the political and social events of the 20th century. The founders and proponents of logical-positivism were physicists, mathematicians and philosophers of science, such as Hempel, Carnap, Reichenbach, and Feigl. After fleeing Europe to escape persecution from Nazis, they came to occupy prominent and influential positions at some of the more prestigious higher education institutions in the United States and on the editorial boards of leading academic journals. Thus

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<sup>4</sup>Longino has identified sociologists Steven Shapin (1982), Barry Barnes, and David Bloor (1982) as theorists who have rejected the logical positivist epistemology (Longino, *Fate of Knowledge*, 13-22).

situated, they were able to give shape to academic inquiry in mathematics, physics, and philosophy. I also believe the logical-positivist epistemic paradigm was adopted by the social sciences and medicine because the empiricist approach advocated by it was already very much a part of these disciplines. The success of logical-positivism may also, in part, be attributable to the fact that it was introduced to a society that, at the beginning of the 20th century, was already committed to empiricism because it was the approach that was responsible for major technological advances.

The founders and proponents of logical-positivism were interested in empirically verifiable knowledge about the world, which they believed could only be provided by testable claims.<sup>5</sup> In the interest of systematically acquiring and accumulating such knowledge, they marked the sciences as a domain exclusively constituted of verifiable claims. Hempel writes, “Science ... aims ... at a comprehensive body of sound empirical knowledge, represented by a body of well-confirmed (read: inter-subjectively verified) system of empirical statements...”<sup>6</sup> Non-empirically verifiable claims, be they metaphysical or value claims, were to have no place in theories.

The interest of the logical-positivists in empirically verifiable claims had much to do with their theory of meaning. According to logical-positivists, there were two sorts of meaningful sentences. A meaningful sentence was one that was true or false by virtue of its form (i.e., tautologies and contradictions). Or a meaningful sentence was one that was either itself empirically verifiable or empirically verifiable by means of “deductive (i.e., syntactical) relations which the sentence [in question] ... [bore] to

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<sup>5</sup> Hempel, “Valuation and Objectivity in Science” in *The Philosophy of Carl G. Hempel*, 2001, 376.

<sup>6</sup> Hempel, *Philosophy of Natural Science*, 40.

some finite set of protocol sentences, whose truth values are themselves determinable by direct observation.”<sup>7</sup>

Rudolf Carnap has argued that all metaphysical claims were meaningless. He categorized metaphysical claims as pseudo-statements. Pseudo-statements fell into two categories; one, statements that contained words that were held to be meaningful but were not. And two, statements whose “constituent words are meaningful, yet they are put together in a counter-syntactical way, so that they do not yield a meaningful statement.”<sup>8</sup>

Sentences employing metaphysical terms such as “the Absolute” qualified as pseudo-statements of the first sort because they failed to meet the criteria for meaningfulness by containing a word that was not meaningful. For the logical-positivists, a word, say “b,”<sup>9</sup> used in an elementary sentence, say “S(b),” qualified as meaningful if it met the following four criteria:

1. “The *empirical criteria* for b are known.
2. It has been stipulated from what protocol sentences “S(b)” is *deducible*.
3. The *truth-conditions* for “S(b)” are fixed.
4. The method of *verification* of “S(b) is known.”<sup>10</sup>

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<sup>7</sup> Joseph Hanna, "The Logic of 20<sup>th</sup>-Century Empiricism." *The Heritage of Logical Positivism* ed. Nicholas Rescher. 1985. 37.

<sup>8</sup> Carnap, Rudolf. 1996. "Elimination of Metaphysics." *Logical Empiricism at Its Peak : Schlick, Carnap, and Neurath (Science and Philosophy in the Twentieth Century: Basic Works of Logical Empiricism)* ed Sahotra Sarkar, 11.

<sup>9</sup> Although Carnap used ‘a’ instead of ‘b’ to refer to some word, in the interest of clarity I made the substitution.

<sup>10</sup> Carnap, "Elimination of Metaphysics," 14-15.

The other sort of pseudo-statements were meaningless because even though they were constituted of meaningful terms, the conjunction of these terms resulted in claims that were not empirically verifiable.<sup>11</sup> As an example of such a claim, Carnap gave the following sentence from Heidegger's *Was Ist Metaphysik?*<sup>12</sup>: "Does the Nothing exist only because the Not, i.e., the Negation, exists?"<sup>13</sup> The problem with such questions was that there was no way of empirically verifying any imaginable answer to them.

On the basis of this theory of meaning, Carnap concluded that metaphysical claims as well as value claims were meaningless:

Logical analysis, then, pronounces the verdict of meaninglessness on any alleged knowledge that pretends to reach above or behind experience. This verdict hits, in the first place, any speculative metaphysics, any alleged knowledge by pure thinking or by pure intuition that pretends to be able to do the kind of metaphysics which, starting from experience, wants to acquire knowledge about that which transcends experience by means of special inference (e.g. the neo-vitalist thesis of the directive presence of an "entelechy" in organic processes, which supposedly cannot be understood in terms of physics; the question concerning the 'essence of causality,' transcending the ascertainment of certain regularities of succession; the talk about the 'thing in itself'). Further, the same judgment must be passed on all philosophies of norms, or philosophy of value, on any ethics or esthetics as a normative discipline. For the objective validity of a value or norm is (even on the view of

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<sup>11</sup> Carnap, "Elimination of Metaphysics," 18.

<sup>12</sup> Carnap does not provide the page number for this quote from the 1929 edition of *Was Ist Metaphysik?*

<sup>13</sup> It is worth noting that while Carnap uses the quoted bit from Heidegger to argue that the claims made by idealist philosophers were non-verifiable, the quote that he uses is a question and not a claim. Of course that does not discount the point that Carnap was making. I owe this point to Professor J. Nelson.

the philosophers of value) not empirically verifiable nor deducible from empirical statements; hence it cannot be asserted (in a meaningful statement) at all. In other words: Either empirical criteria are indicated for the use of 'good' and 'beautiful' and the rest of the predicates that are employed in the normative sciences, or they are not. In the first case, a statement containing such a predicate turns into a factual judgment, but not a value judgment; the second case, it becomes a pseudo-statement. It is altogether impossible to make a (meaningful) statement that expresses a value judgment.<sup>14</sup>

Although metaphysical and value claims did not qualify as descriptive statements about states of affairs (because they were not verifiable), they functioned as *"expressions of the general attitude of a person toward life."*<sup>15</sup>

Given that the logical-positivists believed value claims were meaningless claims that were only expressive of a person's general attitude toward life, it is unlikely that they would tolerate the presence of social and political values in theories. In fact, Carnap made it clear that value claims had no place in science when he declared: "We (i.e., the logical positivists) take the view, expressed already by Hume, that besides logico-mathematical tautologies (analytic sentences), science contains only the empirical sentences of the factual sciences (cf.n.1)."<sup>16</sup> Scientific theories that included value claims would be a morass of meaningful and meaningless sentences, marring any attempt to know the world.<sup>17</sup> Their assumption that scientific theories

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<sup>14</sup> Carnap, "Elimination of Metaphysics," 26-7.

<sup>15</sup> Carnap, R. "Elimination of Metaphysics," 28.

<sup>16</sup> Carnap, R. "The Task of The Logic of Science." *Unified Science* ed Brian McGuinness.

<sup>17</sup> Although the logical positivists objected to the presence of value laden claims in the theory proper, they were not opposed to values determining which scientific projects were pursued and how scientific knowledge was used (more on this later).



could be and should be value-neutral is problematic for reasons that will be discussed below.

Another related problematic feature of the logical-positivist position of interest to my project is the epistemic assumption that observation claims were not laden with theory or values. Joseph Hanna has argued that the logical-positivists believed the aim of science, unlike that of other disciplines, was the effective resolution of all professional disagreements.<sup>18</sup> The assumption underlying this conception of science was that (with enough work) effective decision procedures could be constructed to resolve any disputes between researchers, including discrepancies in observation statements.<sup>19</sup> That assumption was based on the unjustified supposition that observation claims were not laden with theory or values (more on this below).

The third problematic aspect of the logical-positivist epistemology relevant to the purposes of this dissertation is its commitment to the idea (embodied in C.G. Hempel's *Studies in The Logic of Confirmation*) that just by looking at hypotheses and evidence claims, any theorist "could determine in a rule governed way the observation reports that would confirm the hypothesis (in question)."<sup>20</sup> According to Helen Longino, the assumption here is that the relationship between hypotheses and evidence claims is relatively straightforward, devoid of any complicating background assumptions, including background assumptions laden with contextual values. (She uses the term "contextual values" to signify "social, ethical, and political values."<sup>21</sup>)

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<sup>18</sup> Hanna, Joseph. "The Logic of 20th-Century Empiricism," in *The Heritage of Logical Positivism* by Nicholas Rescher, 48.

<sup>19</sup> Disputes that were not amenable to this approach were, presumably, metaphysical issues and not the purview of scientific inquiry.

<sup>20</sup> Longino, Helen. *Science as Social Knowledge (SSK)*, p. 25.

<sup>21</sup> She contrasts those values with constitutive values. Constitutive values are epistemic values of theories, such as simplicity, explanatory scope, predictive success, etc.

Longino contends that this assumption is not justified; below I lay out her critique of this epistemic assumption.

In the last four decades, the various features of the logical-positivist epistemology delineated above have come under heavy fire. Thomas Kuhn<sup>22</sup> and Norwood Russell Hanson<sup>23</sup> have argued that one's observations confirming or disconfirming one's theory are colored by one's theoretical commitments. On the basis of their work, Hanna has argued that the logical-positivists were not justified in assuming that observation statements were not value-laden. In fact, he critiques Carnap's views on the grounds that he fails to engage with the underlying problem of the value-ladenness of observation statements.<sup>24</sup>

Presumably building on that critique, Helen Longino<sup>25</sup> contends that the logical-positivists were not justified in assuming that the process of developing and verifying hypotheses could be rendered value-free by implementing protocols that barred the subjective value preferences of individual practitioners from intruding on scientific inquiry. She argues that norms could influence theorizing in the form of value laden background assumptions mediating the hypotheses-evidence relationship (the background assumptions would be encoded with contextual values). The larger goal of her critique is to call into question the logical-positivist assumption that all scientific theories could be denuded of social, ethical, and political values.

A number of feminist theorists have argued that the notion of the ideal epistemic agent underlying logical-positivism is deeply flawed. Lynn Hankinson Nelson has developed the idea that community membership in the larger social order

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<sup>22</sup> Kuhn, Thomas. *The Structure of Scientific Revolutions*.

<sup>23</sup> Hanson, N.R. *Patterns of Discovery*

<sup>24</sup> Carnap, "Empiricism, Semantics, and Ontology."

<sup>25</sup> Longino, SSK, p. 20-25 and 38-52.

is a necessary pre-condition for knowing, and therefore, the larger social order, and not just scientific communities, qualify as epistemic communities. Thus, she challenges the logical-positivist assumption that scientists can detach themselves from the concerns of the larger social order in their work and that science can be autonomous from social, ethical, and political values.

While Longino's and Nelson's critique is directed at scientific theorizing that is based on the logical-positivist conception of objectivity, it has direct relevance for philosophy of medicine and medicine. Medical theorists, like the logical-positivists, appear to believe that there can be value-neutral theories, i.e., theories that do not contain or are not shaped by social, ethical, or political values.<sup>26</sup> They seem to hold that the practice of intersubjective verification of hypotheses (or more loosely speaking, peer review) controls for those claims that contain contextual values. For instance, R.V. Short has theorized that monthly menstruation is a pathological evolutionary aberration that requires treatment in the form of menstrual suppressant contraceptives (I discuss his work at length in chapter three). He, presumably, considers his work to be objective because the network of hypotheses constituting his theory has been intersubjectively verified.

I believe if philosophers of medicine took to heart the notion of objectivity I formulate (using Longino's and Nelson's analysis of logical-positivism), their ability to 'see' the values shaping medical theories would be enhanced (I discuss this issue at length in chapter four). Moreover, by adopting my notion of objectivity, they would be able to formulate a fairly robust account of the epistemic responsibilities of medical theorists. I contend objectivity is a way of engaging with the world that requires,

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<sup>26</sup>In chapter two, I discuss at length the issue of values influencing or being encoded in theories.

amongst other things, recognizing that all theories are laden with contextual values and that all knowledge building enterprises are communal activities. It also means responding appropriately to criticism.<sup>27</sup> Using my notion of objectivity, philosophers of medicine will be able to argue that medical theorists who recognize that their work is a value-laden activity will be more self-aware and self-critical than those, like Short, who labor under the illusion that their work is objective in the traditional (logical-positivist) sense. That may result in medical theories that generate representations of human physiology that are better than the ones that have been constructed so far. For instance, it may result in theories about women's reproductive physiology that are better than those that have been formulated by implicitly holding up men's reproductive physiology as the paradigm case.

In speaking of a better representation of human reproductive physiology, I do not mean to commit myself to the idea that it is possible for humans to attain an epistemic perspective about women's and men's physiology that is unmediated by any political, ethical, or social values. But I do believe there are better and worse representations of human physiology. There are at least three criteria that could allow one to judge between competing representations. They are as follows: first, *how well a particular representation maps on to the world relative to other empirically adequate theoretical models*.<sup>28</sup> For instance, if a particular representation of women's reproductive physiology asserted that women in this society menstruate on a bi-monthly cycle; such a representation would have to be judged worse than one that claimed that women menstruate roughly on a monthly cycle. The second criterion that

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<sup>27</sup>In chapter two, I provide a detailed account of my notion of objectivity.

<sup>28</sup>The notion of theories as models that map on to the world is borrowed from Ronald Giere and Helen Longino. See chapter five for a discussion of the theory of theories as model (or maps) that reflect the particular interests and concerns of those constructed them.

could help discriminate between better and worse representation of human physiology has to do with *the purpose that the theorizing is supposed to serve in a particular social context*. If theories of women's reproductive physiology are meant to foster their well-being, then it is dubious that a theoretical model of women's reproductive physiology that is mediated by patriarchal assumptions serves that purpose well (the cases I discuss in chapter three illustrate this point).<sup>29</sup> The third criterion that could help distinguish between better and worse accounts of human physiology has to do with *the purpose that a particular theory actually serves (as opposed to the purpose that those who created it claim it serves)*. For instance, in the case of a theory that relies on sexist assumptions to medicalize women's periodicity (but whose creators claim to be motivated by a concern for women's well being), it could be argued that it serves patriarchal purposes by construing women as inherently defective relative to men, and thus, it should be rejected in favor of a theory that is not complicit in the oppression of women.

In the interest of identifying the social, ethical, and political values shaping particular medical theories, I contend theorists ought to invite and respond appropriately to criticism of their work from members of marginalized groups. I espouse that strategy because various feminist epistemologists and philosophers of science (including some of those discussed above<sup>30</sup>) have cogently argued that by virtue of their peripheral social loci, disenfranchised groups are better able to identify

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<sup>29</sup> Elaine Showalter, for instance, has argued that the medicalization of species typical aspects of women's biology has allowed patriarchal societies to categorize women who have rebelled against oppressive practices as pathological, neatly side stepping any moral responsibility toward these victims of social injustice. (Showalter, Elaine. *The Female Malady: Women, Madness, and the English Culture, 1830-1980*. Penguin, 1987).

<sup>30</sup> Longino, SSK and "Essential Tensions- Phase Two: Feminist, Philosophical, and Social Studies of Science," and Sandra Harding, *Is Science Multicultural? Postcolonialisms, Feminisms, and Epistemologies*.

oppressive values shaping theorizing than members of the scientific community, many of whom occupy a position of privilege in various societies.<sup>31</sup>

But some philosophers of medicine may have reservations about that strategy. They could object that, first; my proposal appears to be based on the unjustified assumption that all members of the marginalized group will have critical insight about theory under scrutiny. Second, my proposal is impractical because different disenfranchised groups may offer competing analyses of the theory under consideration but I have not offered a method that medical theorists could use to adjudicate between them.<sup>32</sup> Thus, medical theorists would not know which one of the competing analyses they should use to revise their theories or define future research agendas. In the fifth and final chapter of my dissertation, I discuss these objections and respond to them.

Having mapped out my project, in the next chapter, I discuss Helen Longino's and Lynn Hankinson Nelson's analysis of the logical-positivist notion of objectivity. On the basis of their work, I also formulate an alternative to the logical-positivist notion of objectivity.

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<sup>31</sup> This is not to say all members of marginalized groups by virtue of their membership in non-dominant communities will necessarily be critical of the dominant paradigm. Various standpoint theorists have argued that only those who are critical of the account of reality provided by the oppressive dominant paradigm justifying its practices are the ones whose insights should be privileged over that of those who have uncritically accepted the dominant paradigm, even though both parties might be members of marginalized communities.

<sup>32</sup> This objection has been leveled against standpoint theory. I discuss it at length in chapter five.

## CHAPTER TWO

### 2.0 *Introduction*

In this chapter, I formulate an alternative to the logical-positivist notion of objectivity.<sup>33</sup> My conception of objectivity is informed by Helen Longino's and Lynn Hankinson Nelson's analysis of logical positivism. I begin by delineating the logical-positivist conception of objectivity. Then, in turn, I describe and assess Longino's and Nelson's analyses. I close the chapter by articulating a new notion of objectivity and discussing its significance for the question motivating the dissertation.

### 2.1 *The Logical-Positivist Conception of Objectivity as Embodied in Hempel's Work*

Carl Hempel articulated a vision of the sciences as an activity which “aim[ed] ... at a comprehensive body of sound empirical knowledge, represented by a body of well-confirmed systems of empirical statements...”<sup>34</sup> It is because the logical positivists believed that social, ethical, and political values were not empirically verifiable that they held that such (contextual) values ought not be part of scientific theories in the sense of being embedded in the concepts, principles, hypotheses claims, or observation claims of theories. But they did not oppose values influencing scientific inquiry in other respects. Hempel, for instance, did not object if the decision to pursue a particular field, project, problem, or method of investigation rather than some other one was based on social, ethical, and political concerns. Moreover, he was not opposed

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<sup>33</sup>In chapter four, I use my conception of objectivity to critique Boorse and Engelhardt.

<sup>34</sup>Hempel, *Philosophy of Natural Science* (PNS), 40.

to the decision to make use of scientific knowledge in certain ways as opposed to others being made on normative grounds.<sup>35</sup> Hempel also did not appear to be averse to the idea that individual practitioners' subjective values and beliefs might play a role in the context of theory discovery.<sup>36</sup>

In the interest of preventing contextual values of individual practitioners from being encoded in the theories themselves (i.e., in the concepts, principles, evidence claims, or hypothesis claims of theories), Hempel sought to develop a protocol that could guarantee objective theories.<sup>37</sup> He attempted to formulate the hypotheses-evidence relationship in purely formal terms such that just by looking at hypotheses and observation claims, without reference to the subject matter, one would be able to tell whether or not the evidence in question confirmed or disconfirmed the hypotheses under consideration.<sup>38</sup> Hempel seemed to assume that hypotheses and evidence that satisfied that constraint would be free of "idiosyncratic beliefs and attitudes on the part of the scientific investigators,"<sup>39</sup> and thus, the objectivity of the theory in question would be assured.<sup>40</sup> Elaborating on that notion of objectivity, Hempel said, "science aims at *knowledge that is objective* in the sense of being intersubjectively verifiable, independently of individual opinion or preference, on the basis of data obtainable by suitable experiments or observations."<sup>41</sup>

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<sup>35</sup>Hempel, "Valuation and Objectivity in Science," (VOS) 372-3.

<sup>36</sup>As an example of the role that values could play in 'the context of theory discovery', Hempel notes that Kepler's theory of planetary motions "... was inspired by his interest in a mystical doctrine about numbers and a passion to demonstrate the music of spheres" (*Philosophy of Natural Science*, (PNS) 16). Hempel also recognized and considered it appropriate that epistemic norms, such as theoretical simplicity, predictive prowess, accuracy, and explanatory scope, were used by the scientific community to assess competing theories.

<sup>37</sup>Hempel, "Science and Human Values," (SHV) 46.

<sup>38</sup>Hempel, "Studies in the Logic of Confirmation," (SLC) 10.

<sup>39</sup>Hempel, VOS, 374.

<sup>40</sup>Hempel, PNS, 16, and VOS.

<sup>41</sup>Hempel, 1965, 141. Emphasis in original. Hempel argues that even Carnap can be read as being committed to this conception of objectivity. He gives Carnap this reading in the interest of defending



## 2.2 *Helen Longino's Critique of the Logical Positivist Conception of Objectivity*

Longino's critique begins by noting that Hempel's aim (in "Studies in the Logic of Confirmation" in articulating the concept of confirmation and that of disconfirmation) was to provide a formal characterization of the hypotheses-evidence relationship without reference to the subject matter of the hypotheses or evidence:

For Hempel one ought to be able to tell simply by looking at the logical form of a hypothesis sentence and an evidence sentence whether the confirmation relation holds between them, just as one can tell simply by inspecting the logical forms of premise sentences and conclusion sentences whether the implication relation holds between them. The search, then, is for formal syntactic criteria of confirmation analogous to the formal criteria for the validity of deductive arguments.<sup>42</sup>

Longino gives two arguments against Hempel's attempt to conceptualize the hypotheses-evidence relationship in purely formal terms. I call these arguments the No Shared Predicate Argument and the Background Assumption Argument.

The No Shared Predicate Argument takes Hempel's critique of the inductivists and uses it against him. Hempel argues that the inductivists' claim that hypotheses are formulated by being inductively inferred from observations was not justified because observations statements and hypotheses sentences do not necessarily contain common

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the logical-positivist conception of scientific objectivity against the charge of epistemic atomism and relativism. According to Hempel, Carnap justified his theory of rational credibility of empirical hypotheses by appealing to intersubjective agreement about the matter (Hempel, VOS, 380-1).

<sup>42</sup>Longino, *Science as Social Knowledge (SSK)*, 23.

terms.<sup>43</sup> He used the atomic theory of matter to instantiate his argument. Evidence claims for the atomic theory of matter contain claims about “cloud chambers, lines observed in spectrographic analysis, et cetera,”<sup>44</sup> while hypotheses sentences refer to atoms, neutrinos, quarks. Therefore, the inductivists were not warranted in making the claim that hypotheses are derived by inductive inference from observations claims.

Longino uses Hempel’s argument against the inductivist to discredit his attempt to articulate the evidence-hypotheses relationship in purely formal terms with the end of ensuring the objectivity of theories. She contends that Hempel’s assumption that the hypotheses-evidence relationship could be articulated in formal terms, without reference to their subject matter of the hypotheses or evidence claims, is unwarranted. As not all hypotheses sentences and evidence sentences share common terms, the hypotheses-evidence relationship cannot be formalized:

The implication of this state of affairs (i.e., Hempel’s analysis of the inductivist position) are, however, equally devastating for Hempel’s analysis of confirmation, if intended as a description of evidential relation. The analysis he provides is of a formal, syntactic relation between sentences. This relation holds only between sentences containing the same predicates: the development of a hypothesis for a class contains only those predicates occurring in the hypothesis, and as an observation report only confirms a hypothesis if it entails its development for the class of objects mentioned in the observation report, it too must contain at least one of the predicates occurring in the hypothesis. That

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<sup>43</sup>Longino, *SSK*, 24.

<sup>44</sup>Longino, *SSK*, 24.

is, the confirmation relation as Hempel conceives it makes the same impossible demand upon science as does the inductivist conception.<sup>45</sup>

Longino's Background Assumption argument builds on the No Shared Predicate argument. It faults Hempel's conception of the hypotheses-evidence relationship on the grounds that it assumes there is a necessary and unique relation between hypotheses and evidence, unmediated by background assumptions. Longino argues that that is not the case as the relationship between hypotheses and evidence may be mediated by background assumptions. Moreover depending on one's background assumptions, one could read differently the same state of affairs, i.e., evidence. (Longino uses the term "background assumptions" variously to mean interests, point of view, or theory.) To make her case about the relevance of background assumptions in shaping one's reading of the evidence, she gives a variety of non-scientific and scientific examples. As an example of the non-scientific sort, she gives the case of two men who enter a friend's house and see a gray hat on a banister. Depending on their background assumptions (in this case, their belief about who owns a gray colored hat and who is inclined to leave it on the banister), the presence of the hat qua evidence means different things to them.<sup>46</sup> Longino also gives another example. She claims that one's reading of the presence of red spots on a child's stomach is colored by one's background assumption. A person's whose background assumption is that stomach rashes are a symptom of gastric disorder will not read the red spots on a child's stomach in the same way as a person who subscribes to the background assumption that they are indicative of measles.

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<sup>45</sup>Longino, *SSK*, 24-5.

<sup>46</sup>Longino, *SSK*, 42-3.

Longino also faults Hempel's conception of the hypotheses-evidence relationship on the grounds that it assumes that the relationship between hypotheses and evidence is not mediated by background assumptions that are laden with contextual (i.e., social, ethical, and political values -be they of individual practitioners or those of a particular group). Depending on the value laden background assumptions mediating between hypotheses and evidence, one could read same state of affairs (i.e., evidence) very differently.<sup>47</sup> For the purposes of this chapter, I briefly lay out her analysis of a recent research project on the biological basis of gender role differences.

Behavioral endocrinologists Anke Ehrhardt and Heino Meyer-Bahlburg (1981) have theorized that difference in gender role behavior are significantly determined by fetuses' exposure to sex hormones.<sup>48</sup> This theory is anchored in their research on girls with Congenital Adrenocortical Hyperplasia (CAH).<sup>49</sup> Ehrhardt and Meyer-Bahlburg hypothesized that greater than normal exposure to androgen as fetuses causes these females to exhibit "tomboyish" behavior. As evidence they cited the correlation between girls with CAH and "tomboyish" behavior. On the basis of their research on CAH girls *and* drawing on cases in other mammalian species, for examples, rats, where behavior *is* hormonally determined,<sup>50</sup> Ehrhardt and Meyer-Bahlburg theorize that prenatal exposure to sex hormones "importantly influences" gender role behavior in the human species.<sup>51</sup>

Besides critiquing Ehrhardt and Meyer-Bahlburg for using loaded language (for example, "tomboyish") in their study and not taking into account that their evidence

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<sup>47</sup>Longino, *SSK*, 46-8.

<sup>48</sup>Longino, *SSK*, 119.

<sup>49</sup>CAH is a condition marked by higher than statistically normal production of androgen during female fetal development (Longino, *SSK*, 114).

<sup>50</sup>There is good evidence that rodents exposed as fetuses to higher than normal levels of androgen exhibit more aggressive behavior than they would otherwise.

<sup>51</sup>Longino, *SSK*, 119.

(i.e., the reports about the “tomboyish” behavior of CAH girls by the girls themselves and their parents and teachers) might be problematically influenced by the observers’ expectations,<sup>52</sup> Longino contends that the relationship between their hypothesis (about CAH girls) and the evidence that they cite in support of it is mediated by background assumptions of which they appear to be unaware. The background assumption in question is the model of the brain that assumes that there is an uni-directional, causal relationship between behavior *and* brain structure and chemistry. Longino argues that if Erhardt and Meyer-Bahlburg had instead worked with an alternative model of the brain that recognized both agent intentionality *and* interaction between agents and their environment (along with brain structure and physiology) as determinants of human behavior, it is unlikely that they would draw as strong a causal connection between prenatal exposure to sex hormones *and* gender role behavior as they have done.

Longino argues that not only is the relationship between hypotheses and evidence mediated by background assumptions, some of those assumptions may be laden with contextual values. For instance, the two models rely on different “metaphysical assumptions about causality and human action,” which, presumably, have different attendant social, ethical, and political commitments and entailments.<sup>53</sup> The model that draws a strong uni-directional relationship between brain structure and physiology is committed to a form of biological determinism. Longino does not believe that such a model recognizes the possibility of agency and intentionality, thus, she argues, it limits our sense of ourselves as autonomous entities. Alternatively, the

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<sup>52</sup>Longino notes that there are numerous other problems with this research project including the fact that these theorizers are taking a very small population (which is non-representative of the larger human population) along with data about another less complex species than humans, in this case, rats, to generalize about a fairly complex phenomenon like gender role differences in humans.

<sup>53</sup>Longino, *SSK*, 161.

model that allows for agency, interaction between agents and their environment, and biology as determinates of behaviors confirms our sense of ourselves as capable of acting autonomously and intentionality. Longino also argues that given that equally compelling evidence could be found in support of either theory of the brain, and assuming that both theories have the same epistemic merits, and supposing that one would not want to decide between them in a flippant manner (say, by flipping a coin), the decision to support one or the other would have to be made on the basis of the contextual values encoded in them.<sup>54</sup>

Thus, Longino challenges the logical positivist assumption that if the evidence in support of a particular hypothesis was intersubjectively verifiable, the non-normativity (i.e., objectivity) of their inquiry was assured. Her analysis demonstrates that the logical positivists failed to take into account the possibility that contextual values, embedded in the background assumptions mediating the hypotheses-evidence relationship, could be influencing theorizing:

Background assumptions are the means by which contextual values and ideology are incorporated into scientific inquiry. *While not all such assumptions encode social values*, their necessity to evidential reasoning means that the basic components of methodologies - logic and observation- are not sufficient to exclude values from proper inquiry (my italics).<sup>55</sup>

(It should be noted that Longino has not argued that all background assumptions contain contextual values; she is conceding that there are background assumptions that

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<sup>54</sup>Longino, SSK, 161

<sup>55</sup>Longino, SSK, 216.

do not encode social or political values (see italicized phrase in above quote). I discuss that concession below.)

Believing that she has done serious damage to the logical-positivist of objectivity, Longino offers an alternative account of scientific objectivity. She argues that the type of scrutiny that allows science to lay claim to being objective is not the kind that purports to identify and cleanse theories of any particular scientist's subjective preferences. Rather what allows science to lay claim to being objective is the inter-subjective scrutiny of theories by the larger epistemic community. That scrutiny exposes the contextual values encoded in theories in the form of value-laden background assumptions, concepts, and claims. It also brings to light non-value laden background assumptions. Longino's account of scientific objectivity does not require that all contextual values that are identified as encoded in theories be eliminated. Rather she calls for the elimination of oppressive values shaping theorizing and research such that science becomes an emancipatory practice. Longino argues that to attempt to eliminate all contextual values from theories may "severely truncat[e] the explanatory ambitions of science" because values importantly motivate and give shape to human inquiry.<sup>56</sup> The choice of the model of the brain in the case of the study of the biological basis of gender role differences, for instance, is indicative of the potential contextual values have for expanding and molding theorizing and research projects.

Scientific objectivity for Longino entails that there are publicly recognized forums for the critique of evidence, hypotheses, methodology and inference. She also

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<sup>56</sup>The case of the two competing theories of the brain that are mediated by metaphysically charged background assumptions illustrate this point. Longino, *SSK*, 223.

claims that objectivity requires that the scientific community not only tolerates dissent but responds appropriately to criticism (more on this in chapter five).<sup>57</sup>

In the interest of identifying contextual values encoded in theories, Longino advocates that scientists ought to engage in a critical dialogue about their work with marginalized groups, such as feminists. I believe that proposal is premised on the idea that by virtue of their social marginality, disenfranchised groups are more likely to 'see' the dominant social, political, and ethical values encoded in theories than those who occupy positions of privilege. Members of the dominant group are less likely to 'see' the dominant values embedded in theories because by virtue of their position of social privilege they have less reason (than those at the margins of society) to question why society is set up the way it is and to question the dominant norms, practices, and customs.<sup>58</sup>

While Longino endorses critical dialogue between the disenfranchised and scientists, she makes it clear that only those marginalized communities that would be willing to assess theories on the basis of their epistemic merits (as well as their contextual values) should be at the table. In particular, she appears to be interested in including feminists in this dialogue because of their commitment to an emancipatory agenda.<sup>59</sup> She opposes the presence of "crackpot" marginalized groups, say 'crystalogists' or creationists at the table.<sup>60</sup> The failure of such groups to meet the core

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<sup>57</sup>Longino, SPK, 273 and SSK, 76-81.

<sup>58</sup> The assumption is that members of the scientific community occupy a position of privilege in Western societies vis-à-vis members of marginalized groups. This is not to say that there is no overlap between the two.

<sup>59</sup>I attribute that position to her because Longino identifies feminists as a group that she would want at the table and she identifies feminism as being at core about "expanding human potential" (Longino, SSK, 190) and committed to opposing oppression.

<sup>60</sup>Longino (SPK, 118) in *Feminist Epistemologies* edited by Linda Alcoff and Elizabeth Potter. Routledge. 1993.



standards of scientific inquiry warrants excluding them from the “knowledge-constructive dialogue” (more on this in chapter five).<sup>61</sup>

Thus, in arguing for this conception of objectivity, Longino is, in effect, arguing for an inter-subjective conception of objectivity that goes beyond identifying and eliminating the subjective preferences of particular scientists. It means identifying all contextual values embedded in theories- whether they are the social, ethical, and political values of individual practitioners or that of a certain group- and ideally, developing those theories that are consistent with the community's social, ethical, and political values.

We cannot restrict ourselves simply to the elimination of bias but must expand our scope to include the detection of limiting interpretative frameworks<sup>62</sup> and the finding or construction of more appropriate frameworks... Instead of remaining passive with respect to the data and what the data suggest, we can, therefore, acknowledge our ability to affect the course of knowledge and fashion or favor research programs that are consistent with the values and commitments we express in the rest of our lives.<sup>63</sup>

Longino's conception of scientific objectivity is at odds with that of the logical-positivists; the latter would consider any encroachment of social or political values on theories as compromising the objectivity of science.<sup>64</sup>

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<sup>61</sup> However, she acknowledges that this may not be a foolproof standard (Longino, “Subject, Power, and Knowledge,” (SPK, 118) in *Feminist Epistemologies*).

<sup>62</sup> I read Longino to mean “theories” when she uses the phrase “interpretative frameworks.”

<sup>63</sup> Longino, *SSK*, 191.

<sup>64</sup> However, (as discussed earlier in section 2.1.1.) this is not to say that logical-positivists did not recognize that the decision to pursue a particular project rather than some other one and the decision to

### 2.3 *Limitations and Strengths of Longino's Analysis*

Longino's critique is fairly compelling but it is not a categorical refutation of the logical positivist ideal of objectivity. As I noted in the previous section, she argues that *some* of the background assumptions that mediate the hypotheses-evidence relationship of scientific theories may be value laden. That leaves open the possibility that not all background assumptions are value-laden. She writes that while "background assumptions are the means by which contextual values and ideology are incorporated into scientific inquiry ... *not all such assumptions encode social values* (my italics)."<sup>65</sup> In acknowledging that not all background assumptions are laden with social or political values, Longino allows for the possibility that there could be some theories that are free of such values. Such theories would be ones that contain background assumptions that are not laden with social, ethical and political norms (assuming, of course, that contextual values are not present in those theories in other ways that the logical positivists would consider unacceptable).

Longino's concession is significant. It may embolden those of the logical positivist persuasion to argue that their ideal of value free scientific theorizing ought not be abandoned. Hempel, for instance, might claim that all contextual values encoded in background assumptions should be identified *and* eliminated. Better epistemic protocols than the current ones should be developed to prevent theories from being corrupted by any contextual values, including culturally pervasive contextual values that might be encoded in background assumptions. In fact, he might even appropriate her strategy of engaging with marginalized groups to identify culturally dominant values that might be ingrained in background assumptions. His aim would

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make use of scientific knowledge in certain ways as opposed to other ways did not entail value judgments. (Hempel, SHV, 46).

<sup>65</sup>Longino, SSK, 215-6.

be to identify such assumptions *and* to replace them with background assumptions that are not tainted by values, with the end of constructing theories that were free of contextual values. Longino's argument that theories should not be denuded of all contextual values because that might severely limit the explanatory goal of science would hold no water for logical positivists.

There is good evidence that Hempel would be averse to allowing social, political and ethical values to be encoded in theories. He has argued that confirmation of hypotheses by evidence has nothing to do with value claims. Using the case of the confirmation of the existence of a certain kind of radiation belt surrounding Earth, he contends that confirmation of this hypothesis is independent of the ethical, political, and social values of particular scientists. Rather it is solely a factual matter.<sup>66</sup>

To shore up his argument for purging theories of contextual values, Hempel would probably critique Longino's analysis of the Erhardt study of the biological basis of sex role differences. Hempel could claim that although it could be argued that evidence could be marshaled in favor of either one of the two models of the brain, the one that does a better job of explaining the phenomena of gender role differences should be preferred, without reference to the sort of ethical, political, and social values that Longino claims that the two models embody and foster. Thus, for purely epistemic reasons he would probably prefer the model that takes into account that humans are a more complex species in terms of their intellectual abilities and social relations than rats. Hempel would claim he was appealing to the epistemic merits (such as explanatory scope and fruitfulness) of the two competing background assumptions-models to decide between them, rather than any social, political, or

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<sup>66</sup>Hempel, SHV, 91.

ethical values. He would decry any attempt to argue that the decisions to espouse one of two competing theories should be made on the basis of the contextual values coded in those theories. Such an argument would be entirely consistent with his logical-positivist stance.

In response to his argument, Longino would probably argue that it is based on the unwarranted supposition that epistemic values can always serve as the sole arbitrators of background assumptions. There are at least two kinds of cases that challenge that supposition.

**Case One:** Suppose you have two competing background assumptions with the same kind and degree of epistemic merits (i.e., explanatory success, predictive success, etc).<sup>67</sup> In that case, their epistemic merits cannot be the grounds on which one could decide which one should be privileged over the other. Under such circumstances, assuming that one may have limited research resources (and thus, one could not keep seeking further experimental basis to decide between the two background assumptions), and assuming that one might not want to arbitrarily pick one background assumption over the other, one may want to decide between them on substantive grounds, such as the contextual values encoded in them. For instance, one would probably want to pick the background assumption that serves the purpose that motivated one to theorize (say, the desire to alleviate human suffering) better than the background assumption that is incompatible with that goal. In doing so, one would have, in effect, decided between theories on the basis of the values contained in them. What this case demonstrates is that logical-positivists like Hempel would not be justified in assuming that the decision to choose between competing background

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<sup>67</sup>As I noted earlier, Longino uses the term “background assumptions” variously to mean interests, point of view, or theory. I use it here to denote theories that serves as background assumptions that mediate between the hypotheses and evidence claims of other theories.

assumptions could always be made *solely* on the basis of epistemic values; in some cases, one may very well have to use ethical, political, or social values to decide between competing background assumptions.<sup>68</sup>

**Case Two:** Suppose you have two competing background assumptions such that they have varying degrees of epistemic merit. Background Assumption A scores slightly higher than Background Assumption B as far as simplicity is concerned but scores slightly lower than the former as far as explanatory scope is concerned<sup>69</sup> but both A and B have the same degree of success as far as other epistemic merits are concerned. So, although Background Assumptions A and B have varying degrees of epistemic success, unless one has substantive reasons for ranking simplicity over explanatory scope or vice versa, for all intents and purposes, one finds oneself in the same sort of quandary as one faced in Case One. What this case demonstrates (as did Case One) is that logical- positivist like Hempel would not be justified in assuming that epistemic values can always serve as the final arbitrators between theories; in some cases, one may very well have to appeal to ethical, political, or social values that are embedded in those theories or flip a coin. In response, Hempel might argue that using Background Assumption A and B, in turn, two distinct version of the theory should be developed, and then, depending on the epistemic merits of the competing versions of the theory, say T-A and T-B, the scientific community should decide between the two competing background assumptions. But that proposal is problematic because, first, the two competing theories may have differing epistemic virtues, with neither one a clear-cut winner, and thus, one would find oneself in the situation the quandary described in Case A. And second, given that research monies are limited, it

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<sup>68</sup>This could be done provisionally until future research reveals that one set of background assumptions has a clear epistemic advantage. I owe this point to Professor J. Nelson.

<sup>69</sup>Assuming there is a way of quantifying these epistemic virtues.

is not likely that institutions will be willing to devote equal amounts of resources, including labor hours, to develop two competing theories. Therefore, for pragmatic reasons, one might have to decide between competing background assumptions on non-epistemic, normative grounds.

Having described the strengths and weaknesses of Longino's position, now I turn to Nelson's criticism of that epistemology. I do so because her work is not limited in the way Longino's is and it provides fresh insight into logical positivism. Nelson offers what has the potential to be a categorical refutation of the idea that there can be theories that are free of any contextual values.

#### *2.4 Setting-Up Lynn Hankinson Nelson's Critique of the Logical-Positivist Conception of Objectivity*

Nelson contends that the logical-positivist notion of objectivity must be rejected because it is based on the unwarranted assumption that a line can be drawn between science, metaphysics, and values.<sup>70</sup> Her analysis is rooted in Quine's epistemology. In the interest of contextualizing Nelson's critique of logical positivism, below, I outline Quine's position (I also discuss Quine's work because I draw on it to critique Nelson.) After that, I delineate and critique Nelson's work.

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<sup>70</sup>Nelson, *Who Knows: From Quine to a Feminist Epistemology* (WK).

## 2.5 Quine on Ordinary Language Metaphysics, Science, and Values

Unlike the logical positivists, Quine holds that metaphysics and science are part of the same fabric.<sup>71</sup> That is so for two interdependent reasons. First, our ordinary language is committed to a macro-physical object metaphysics that allows us to make sense of our experiences, and second, our ordinary language macro-physical object metaphysics makes possible scientific theorizing (though the latter, in turn, shapes the former too). Elaborating on that relationship, Quine holds that scientific claims make sense only insofar as they are read against the backdrop of the macro-physical object metaphysics of ordinary language because "the positing of physical objects must be seen not as an *ex post facto* systematization of data, but as a move prior to which no appreciable data would be available to systematize."<sup>72</sup> Although ordinary language macro-physical object metaphysics serves as a precondition and backdrop for scientific theorizing, both the macro-physical object metaphysics and science serve the same master. Their task is to make sense of experience. As servants to experience, both are subject to revision if they are inconsistent with experience. The following passage clearly demonstrates Quine's empiricist convictions:

The totality of our so-called knowledge or beliefs, from the most casual matters of geography and history to the profoundest laws of atomic physics or even of pure mathematics and logic, is a man-made fabric which impinges on experience only on the edges. Or, to change the figure, total science is like a field of force whose boundary conditions are experience. *A conflict with*

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<sup>71</sup>Quine, "The Two Dogmas of Empiricism" (TDE), 79.

<sup>72</sup>Quine, "Posits and Reality," 158. Quine also claims that "[c]ommon sense bodies, finally, are *conceptually* fundamental: it is by reference to them that the very notions of reality and evidence are acquired, and that the concepts which have to do with physical particles or even with sense data tend to be framed and phrased" ("Posits and Reality," 159).

*experience at the periphery occasions readjustments in the interior of the field*  
(my italics).<sup>73</sup>

Developing his analysis of the similarity between non-scientific theorizing (read: common sense theorizing<sup>74</sup>) that embodies and is built on our ordinary language macro- physical object metaphysics *and* scientific theorizing, Quine contends that, epistemically speaking, the macro-physical objects posited by common sense theorizing as well as Homer's mythic divine figures are on par with conceptual scientific constructs posited by, say, the atomic theory:

As an empiricist, I continue to think of the conceptual scheme of science as a tool, ultimately for predicting future experiences in the light of past experience. *Physical objects are conceptually imported into the situation as convenient intermediaries- not by definition in terms of experience, but simply as irreducible posits comparable, epistemologically, to the gods of Homer... Positing does not stop with macroscopic physical objects. Objects at the atomic level are posited to make laws of macroscopic objects, and ultimately the laws of experience, simpler and more manageable...(my italics).*<sup>75</sup>

The thrust of Quine's argument is that the logical positivists were not justified in assuming that there was a categorical distinction between common sense and scientific

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<sup>73</sup>Quine, TDE, 78.

<sup>74</sup>The term 'common sense' theories is meant to denote the ordinary language macro-physical object metaphysics. Lynn Hankinson Nelson uses it in that sense.

<sup>75</sup>Quine, TDE, 79.



theorizing. Both are part of the same fabric, constructed by humans to make sense of the world.

Although Quine rejected the distinction that the logical positivist sought to draw between common sense posits and scientific theoretical posits, he believed that there was a difference in kind between scientific claims and value claims. Observation claims on which scientific theories rest could be verified by testing their correspondence with actual state of affairs, while value claims could not be subject to such a verification process. The best one could do with them was to test if they were coherent with one's other value claims:

The empirical foothold of scientific theory is in the predictable observable event; that of moral code is in the observable moral act. But whereas we can test a prediction against the independent course of observable nature, we can judge the morality of an act only by our moral standards themselves. *Science, thanks to its links with observation, retains some title to a correspondence theory of truth; but a coherence theory is evidently the lot of ethics.* Scientific theories on all sorts of useful and useless topics are sustained by empirical controls, partial and devious though they may be. It is bitter irony that so vital a matter as the difference between good and evil should have no comparable claim to objectivity (my italics).<sup>76</sup>

Although it would seem that it is quite clear that Quine, like the logical positivists, wanted to draw a line between value claims and scientific claims, some have argued

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<sup>76</sup>Quine, "On the Nature of Moral Values," 63.

that that is not his position. Richard Rudner, for instance, claims that Quine's arguments in "On Carnap's View on Ontology," and "The Two Dogmas of Empiricism" suggests that the conceptual distinction Carnap wants to draw between value judgments (which he deems to be questions external to science) and theoretical questions (which he considers to be internal questions for science and which are addressed using observation claims) is one that Quine would reject.<sup>77</sup> Rudner, however, does not attempt to substantiate that claim about Quine's supposed stance by engaging with the aforementioned essays.

Owen Flanagan has tried to make the argument that Rudner should have but does not make for his "reading" of Quine. Flanagan has argued that Quine's rejection of the two dogmas of empiricism (i.e., the analytic-synthetic distinction and radical epistemological reductionism) entails a commitment to holism. He believes that commitment would lead Quine to reject any attempt to draw a distinction between scientific claims and value claims.

But Flanagan is wrong. Although Quine is a holist, he is not committed to the radical holism that Flanagan attributes to him. Quine supports a mitigated form of holism that "allows, nay, insists, that observation sentences have their own, individual empirical (stimulus) meanings."<sup>78</sup> That much is clear from Quine's insistence that even the most well-trenched common sense or scientific theory should be rejected if it is not borne out by experience. A radical holist, presumably, would be interested in preserving the coherence of the entire system of knowledge, even if it meant giving a short shrift to observation claims that were inconsistent with one's larger network of theories (which were consistent and coherent with each other). Thus, it could be

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<sup>77</sup>Rudner, 1953, 5.

<sup>78</sup>Roger F. Gibson, "Flanagan on Quinean Ethics," 537.

argued that Rudner's "reading" of Quine is not justified either. Lynn Hankinson Nelson has not aligned herself with Rudner or Flanagan. Rather she reads Quine as committed to the position that there is a difference in kind between observations claims (on which scientific theories rests) and value claims. Nelson argues that he is wrong.

## 2.6 *Nelson's Critique of the Logical-Positivist Conception of Objectivity*

Nelson makes no bones about the fact that she finds Quine's critique of logical-positivism compelling. However, she contends that not only is there no boundary between metaphysics<sup>79</sup> and scientific theories, there isn't one between metaphysics, science, and values. Here she parts ways with Quine, who held that there was a fundamental difference between observation claims (on which scientific theories rest) and value claims; the former are subject to a correspondence theory of truth (i.e., whether or not the observation claims match up with the world), whilst the latter can only be assessed by a coherence theory of truth (whether value claims are coherent with one another). For an empiricist like Quine, claims that could only be subject to a coherence test were sadly unreliable compared to claims that could be subject to empirical verification. Summing up Quine's position, Nelson writes, "For now it is clear that one of the directions we must take involves re-appraising what Bill Wisdom call Descartes' covenant: his promise, as Quine characterizes and advocates it, that science will 'stand ... aloof' from values. (Quine, *Roots of Reference*, p.49)."<sup>80</sup> She also notes, "... Quine and the other empiricists have been concerned to exclude values,

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<sup>79</sup>Nelson and Quine appear to use the terms "common sense theorizing" and "metaphysical theorizing" interchangeably. Both terms are used in contrast to the term "scientific theorizing."

<sup>80</sup>Nelson, *WK*, 248.

including political interests, from the content of scientific theories and from their accounts of the way we go about constructing theories- at least good theories.”<sup>81</sup>

Nelson gives two arguments in support of her claim that science is value-laden. Her first argument relies on the idea that all kinds of human actions or creations that have social or political consequences have ethical import. As scientific theories are things that humans create and insofar as they have political and social significance, they cannot be considered value neutral. To instantiate her point, Nelson uses the theories of sociobiologist David Barash and Daniel Janzen. They have argued that given that the rape of female mallards by male ones is a fitness maximizing strategy used by male mallards to secure their reproductive rights *and* given that variety of other species also exhibit that behavior, humans ought to rethink how they view rape of human females by human males:<sup>82</sup>

Rape in humans is by no means as simple [as rape among mallard ducks], influenced as it is by an extremely complex overlay of cultural attitudes. Nevertheless, mallard rape and bluebird adultery may have a degree of relevance to human behavior. *Perhaps human rapists, in their own criminally misguided way, are doing the best they can to maximize their fitness. If so, they are not that different from sexually excluded bachelor mallards* (italicized by Nelson).<sup>83</sup>

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<sup>81</sup>Nelson, *WK*, 260.

<sup>82</sup>This is a patently bad argument because it assumes that what science deems natural is morally justified. I owe this point to Professor J. Nelson.

<sup>83</sup>Barash, David. 1979. *The Whisperings Within*. New York: Harper & Row., p.55. (In Nelson's *WK* on page 159).

Nelson argues that although such theories purport to be purely descriptive, they are laden with metaphysical assumptions about the nature of men and women.<sup>84</sup>

Moreover, she argues, as such theories construe heterosexual rape as an evolutionary fitness maximizing strategy; they appear to provide a veneer of legitimacy to violence by men against women.<sup>85</sup> Thus, Nelson contends, such theories cannot be regarded as devoid of any ethical import:

The most sophisticated geneticist or zoologist can, under the guise of scientific insulation from political context and the apolitical nature of scientific theorizing, engage in and propound the most unsophisticated social and political theorizing- and, in so doing, legitimize violence against women. (Sociobiologist David) Barash's irresponsibility will, I hope, appall readers. But the more far-reaching issue with which we need to contend is that his explanation of rape can be offered as scientific and "value-free" because we grant scientists cognitive authority that we do not scrutinize and insist that scientific theorizing is value-free. Thus Barash feels free to reject any responsibility for the implications of his claims. He is, on his account, simply telling us how things are.<sup>86</sup>

The second reason that Nelson gives for categorizing science as not being autonomous of values is tied to her critique of the logical-positivist conception of the epistemic agent. The logical positivists held that it was possible for epistemic agents to be

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<sup>84</sup>Nelson, *WK*, 160.

<sup>85</sup>Nelson, *WK*, 158 and chapter four. It is worth noting that those who might be inclined to excuse violence against women by men on the grounds that it is natural are guilty of a fallacy. Just because something is natural does not mean that it is good or legitimate.

<sup>86</sup>Nelson, *WK*, 158.

objective cognitive authorities (i.e., they believed humans qua knowers could detach themselves from all social, ethical, and political commitments and assume a value neutral stance). Nelson's refutation of this conception of the epistemic agent has two parts. First, she challenges the metaphysical conception of the person underlying this notion of the knower. Second, she argues that communities, not individuals, qualify as primary epistemic agents. Given that communities have values, interests, and beliefs that motivate and shape their theorizing, as epistemic agents they cannot be considered objective. Below, I outline her two-part argument.

Nelson claims that the logical-positivist conception of the epistemic agent as an objective cognitive authority is rooted in the metaphysical conception of personhood underlying the moral and political philosophical writings of Immanuel Kant, John Locke and Thomas Hobbes. Nelson notes that Alison Jaggar has argued that this modern notion of the self is one that assumes that persons are prior to their society. According to this conception, persons may have beliefs, values, interests and desires that are independent of and uninfluenced by those that permeate their society:

The assumption ... is that human individuals are ontologically prior to society; in other words, human individuals are the basic constituents out of which social groups are composed. Logically, if not empirically, human individuals could exist outside of a social context; their essential characteristics, their needs and interests, their capacities and desires, are given independently of their social context and are not created or even fundamentally altered by that context.<sup>87</sup>

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<sup>87</sup> Jaggar, 1983, 28-9.

Drawing on Quine's work on the social nature of experience, language and knowledge, Nelson argues that the notion of a self that is logically prior to her social context is not a viable one because the public conceptual framework (embodied in ordinary language) of any society shapes individuals' epistemic possibilities. Nelson writes that "'social preconditions' are necessary for anyone to know, and even 'basic ... sense experiences' are coherent only because they are shaped by public language and a conceptual scheme, a body of theories embodied in language."<sup>88</sup> Given that community membership is a necessary precondition for language development, acquisition and formulation of beliefs, values and interests, and given that even sensory perceptions are socially mediated,<sup>89</sup> "*theories and questions are necessarily the product of social activity, and the experiences of individuals are made possible and shaped by these*" (my italics).<sup>90</sup> Nelson also points out that because theories are socially constructed, it can be argued that what constitutes evidence is also socially determined,<sup>91</sup> thus further undermining the possibility of epistemic agents who function in utter isolation from their social context:

[E]vidence is (determined by) a public standard. It is deeply dependent on public theorizing and, more specifically, on 'common sense' theorizing.

Individuals do not determine what constitutes evidence for a claim, but by the standards a community accepts concomitantly with constructing, adopting, and

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<sup>88</sup> Nelson, *WK*, 259.

<sup>89</sup> Nelson, *WK*, 285.

<sup>90</sup> Nelson, *WK*, 277.

<sup>91</sup> This is a deeply problematic claim that seriously undermines Nelson's position although she is not aware of that fact; I discuss it in the next section.

refining theories. These constrain what it is possible for an individual to believe as well as the theorizing we engage in together.<sup>92</sup>

Thus, individuals cannot be regarded as logically prior to their communities, and therefore, they cannot be the sort of objective epistemic agents that the logical-positivists want them to be. What also falls out of that critique is that communities, not individuals, are primary epistemic agents.<sup>93</sup> In other words, knowledge is made possible by and created by communities of knowers.

Elaborating on the notion of communities as epistemic agents, Nelson claims that by virtue of their shared knowledge, practices and standards, scientific communities qualify as paradigmatic cases of epistemic communities:

... [E]pistemological communities can be identified in terms of shared knowledge, standards, and practices. Science communities serve as obvious examples of epistemological communities, with bodies of theory, accepted procedures, questions, and projects defining such communities; and membership being a function of education in and allegiance to community-specific knowledge, standards, and practices...<sup>94</sup>

Though Nelson identifies scientific communities as paradigmatic epistemological communities, she believes they are not the only communities engaged in knowledge

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<sup>92</sup> Nelson, *WK*, 277.

<sup>93</sup> At first glance, Nelson's claim that communities have epistemic agency appears to be problematic. But if one recognizes that communities make decisions about what qualifies as knowledge and what does not and who can be and who can't be a bearer of knowledge (for instance, historically and even today, women in various societies have been denied recognition as capable of having scientific knowledge), the attribution of agency to communities seems appropriate.

<sup>94</sup> Nelson, *EC*, 148.



production. Given that the theorizing that goes on within scientific communities is not independent of the beliefs, values, and notions floating about within the larger non-scientific communities, non-scientific communities must also be recognized as epistemic communities:

But scientific communities are not the only epistemological communities, nor have they a lock on generating knowledge. In terms of their very existence and authority, and the knowledge and standards they generate, scientific communities are interdependent with the larger communities within which they function. More to the point, there are, in fact, many communities that develop and share knowledge and standards, including our larger world community and its multiple and evolving sub-communities.<sup>95</sup>

Nelson describes epistemic communities, specifically non-scientific epistemic communities, as dynamic entities with porous, shifting boundaries.<sup>96</sup> However, they are not reducible to mere aggregates of individual knowers. Individuals cannot be said to be knowers except as members of these communities because these communities offer their members ways of organizing and grasping the world. And it is because these communities offer us, their members, “standards and knowledge that enable us to organize our experiences into coherent accounts, underwrite the specific contributions that we make as individuals, and determine what we and our communities will recognize as knowledge” that we have to concede that they are

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<sup>95</sup> Nelson, EC, 148.

<sup>96</sup> Nelson, EC, 149.

epistemic communities and primary knowers.<sup>97</sup> (They are primary knowers in the sense that it is only as members of these communities that humans function as knowers.)

Given that communities have values, beliefs, interests *and* given that scientific and commonsense theories are the product of these communities that reflect their concerns, the larger implication of Nelson's argument is that scientific theorizing cannot be regarded as aloof from values: "Science is not autonomous from commonsense dealings with the world, or from social and political interests, and values. The beliefs, practices, and experiences of scientists as members of our larger community both permit and shape the theorizing done in these special communities."<sup>98</sup>

## 2.7 *Strengths and Limitations of Nelson's Analysis*

Nelson's analysis is compelling but it could be challenged on the grounds that although in some scientific disciplines, say sociobiology, theorizing may be value laden, that is not necessarily the case for other sciences such as physics.<sup>99</sup> To respond to that argument, I draw on what Nelson has to say to the charge that only certain scientific disciplines are gender biased. I make this move because the issue of gender bias in the sciences is essentially an issue of ethical, social, and political values permeating scientific theorizing.

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<sup>97</sup> Nelson, EC, 150. This also means that for one to yodel, one must be part of a community of yodellers. otherwise it is just a sound and not a yodel. Even though particular persons yodel, yodelling is a socially constructed practice. I owe this point to Professor J. Nelson.

<sup>98</sup> Nelson, *WK*, 315.

<sup>99</sup> This is also a problem for Longino.

Nelson gives two arguments in response to the claim by Sandra Harding and others that physics may be a science that is not corrupted by gender bias.<sup>100</sup> Nelson's weaker response to this claim is that although we may not *yet* be able to see the gender bias that may be at work in physics, it would be unwise for us to conclude that it is categorically free of sexism.<sup>101</sup>

Nelson's stronger response to Harding is that even if physics is free of gender bias that has no larger significance for science as a whole. She also argues that given Quine's analysis of the interconnection and dependence between scientific and common sense theorizing, neither physics nor any other highly abstract discipline can be considered untainted by gender bias:

A fundamental part of Quine's argument against the analytic/synthetic distinction is that the *meanings* of mathematical and logical sentences, and the point holds no less for those of theoretical physics, are *related to* and *shaped* by the other sentences to which these bear a relationship- and ultimately, to and

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<sup>100</sup>Harding makes that claim while responding to those persons who attempt to minimize the significance of feminist analyses that show science to be a value-laden enterprise. Such persons argue that those feminist critiques are only applicable to "the less rigorous' or 'less mature' biological and social sciences " and not science as a discipline (Harding, Sandra. 1986. *The Science Question in Feminism*. (SQF). Cornell UP. 1986. 43 )." They contend that those critiques do not establish that scientific inquiry cannot be value neutral. To do that feminists would have to show that the theories of physics are value laden because it is the paradigm scientific discipline.

Harding's response to that charge is that physics ought not be considered *the* discipline that embodies science because it is fundamentally different from the various other disciplines, such as the biological and social sciences, that also fall under the rubric of science. Harding is interested in dethroning physics as the paradigm science because she believes that would mean that the argument that feminist analyses of science as a value-laden activity only apply to the less developed, less rigorous scientific disciplines will fall apart. In other words, those who do not take feminist critiques of science seriously would have to change their attitude.

In making that argument, Harding concedes that physics, albeit not the paradigm science, is value neutral. She writes, "[W]e should understand physics not as the model for all scientific inquiry, but as atypical of inquiry just insofar as its ontological and methodological assumptions can in fact secure value free results of research" (Harding, SQF, 48).

It is that concession on Harding's part that physics is value neutral that Nelson picks up on and criticizes.

<sup>101</sup> Nelson, WK, 250.

by all other things we say about the world... Given this view of empirical content, no sentence or body of theory has the kind of purity or independence from other theories that Harding (43-44, 1986) suggest we grant to physics. There is no separating physics, or pure mathematics, or logic, or *any other theory* from the rest of our going theories. The sentences of each are not 'pure' of empirical content or without connections- a sharing of supports and a sharing of meaning –with other theories.<sup>102</sup>

Although Nelson categorically rejects the possibility of value-free science, it is not entirely clear what she means by the claim "science is not aloof from values." If she means that the personal values of the individual scientific practitioner may influence her/his choice of field as well as her/his choice of problems and method of investigation, not all logical-positivists would disagree with her. Hempel, for instance, would have no trouble conceding to her point.<sup>103</sup> Or if she means that social, ethical, and political values determine the direction of research as well as "encourage the advocacy of ill-founded theories," Hempel, again, would be in agreement with her that science is value laden in that sense as well.<sup>104</sup> Alternatively, if she were to claim that epistemic norms, such as theoretical simplicity, truth, predictive prowess, accuracy and explanatory scope, are used by the scientific community to assess competing theories, Hempel would be willing to concede to that point too.<sup>105</sup> Given that Hempel recognizes that science could be value laden in all of these ways, it could be assumed that he might also be amenable to acknowledging Nelson's claim that scientific

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<sup>102</sup> Nelson, *WK*, 250-2.

<sup>103</sup> Hempel, *VOS*, 372.

<sup>104</sup> Hempel, *VOS*, 372-3 and *SHV*, 46.

<sup>105</sup> Hempel, *VOS*, 372-3.

theorizing cannot be considered value-neutral because it has political and social consequences. Alternatively, one could suppose that when Nelson claims that science is a value laden activity, she means that scientists make value judgments when they, for instance, decide whether a particular level of risk associated with testing a certain hypothesis is acceptable or not.<sup>106</sup> Richard Rudner has argued Carnap would reject the claim that scientists as scientists make value judgments on the grounds that those sorts of questions are external to science. Rudner writes, "Carnap can still deny the essential involvement of the making of value judgments in the procedures of science by insisting that concerns with *external* questions, admittedly necessary and admittedly axiological, is nevertheless in some sense a pre-scientific concern."<sup>107</sup> Whether Hempel would echo his fellow logical positivists' rejection of Rudner's claim is not obvious.

But even if Hempel were to concede that science is value laden in the sense Rudner means, Nelson would probably argue that Rudner does not capture what she means when she asserts that science is not aloof from values.<sup>108</sup> Nelson might elaborate on her position as follows: Given that science is the product of the larger epistemic communities (and not just the scientific epistemic community) *and* that communities have values, beliefs, interests, which motivate and shape their knowledge building projects, scientific theorizing should not be considered as different from common-sense theorizing in the sense that one is regarded as being objective (i.e., autonomous of values) whilst the other is not. Her argument effectively highlights the

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<sup>106</sup>Rudner, "The Scientist "Qua" Scientist Makes Value Judgments," 3.

<sup>107</sup>Rudner, 5.

<sup>108</sup>This is not to say that she wouldn't recognize that scientific practice was value laden in the sense Rudner meant.

deep connection between scientific theorizing and the larger non-scientific epistemic community that the logical positivists overlooked.

Though Nelson argues that scientific theorizing is not aloof from values, she does not mean to suggest all theories, regardless of the contextual values they endorse, are on par. She contends that our decision to commit to a certain theory, as opposed to another, should be based on *evidence* for or against the value laden claims endorsed by the theories in question:

Feminist scholarship, including science criticism, indicates that ‘culturally determined beliefs’, including political views and views about sex/gender, can be and should be subjected to *empirical controls*.<sup>109</sup> Feminist science critics have been able to evaluate the assumptions about sex/gender and politics involved in scientific theories and to do so on the basis of evidence. There is evidence that women’s activities are central to the dynamics of human social groups, and that androcentricism has distorted cross-cultural studies, animal sociology, and evolutionary theory. There is *evidence* that indicates that male dominance is neither natural nor universal, that research into sex differences is wrongheaded, and that current divisions in power by sex/gender are not based on, or justifiable on the basis of biology. Moreover, the *evidence* that calls the various theories into question that incorporated these things has at its most basic source common-sense experience and theorizing, including political experience and theorizing. So, common-sense theory is itself not static or

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<sup>109</sup>By "empirical control," I take Nelson to mean verification of evidence.

unable to be subjected to *empirical control*. It is currently changing in dramatic ways within sub-communities in our larger community (my italics).<sup>110</sup>

The above passage indicates that Nelson holds that epistemic communities should evaluate theories about gender on the basis of "empirical controls." Her use of that phrase suggests that she is arguing that theories of gender should be subject to verification or testing. That verification would occur by using evidence (i.e., observation claims about the world) that would conclusively demonstrate whether theories arguing for men's natural superiority and dominance over women are justified or not (see passage quoted above).<sup>111</sup> In arguing that such theories should be tested using evidence, specifically, observation claims about men's and women's behavior, Nelson must believe that the evidence in question is neutral with respect to the competing theories at issue. Otherwise she would not suggest using it to judge between competing theories (see passage quoted above).

Nelson's claim that evidence (i.e., observation claims based on basic sense experience (see passage quoted above)) can be used to decide conclusively between the competing value laden theories is problematic. Her proposal is predicated on the assumption that evidence (i.e., observation claims based on basic sense experience) can serve as the neutral arbitrator between competing paradigms. But I believe she is not justified in asserting that because she has *also* made the following three claims:

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<sup>110</sup>Nelson, *WK*, 248-9.

<sup>111</sup>Here Nelson parts ways with Longino, Quine, and other empiricists (Nelson, *WK*, 248). She claims that they hold that value claims cannot be evaluated.

- i. Basic sense experiences are "coherent only because they are shaped by public language and a conceptual scheme, a body of (common-sense ) theories<sup>112</sup> embodied in language (in general and not just particular languages)."<sup>113</sup>
- ii. Public theorizing shapes and is shaped by scientific theories.<sup>114</sup>
- iii. Public theorizing determines what counts as evidence.<sup>115</sup>

If Nelson holds that i, ii, and iii, then she cannot *also* make the claim that "evidence" can be used as a neutral arbitrator to decide between competing value laden scientific theories without contradicting herself. She can't have her cake and eat it too.

Aside from the problem of self-refutation, there are other aspects of Nelson's stance that merit scrutiny. Nelson claims that basic sense evidence is made coherent by common sense theories, which shape and are shaped by scientific theories. She writes, "'social preconditions' are necessary for anyone to know, and even 'basic ... sense experiences' are coherent only because they are shaped by public language and a conceptual scheme, a body of theories embodied in language."<sup>116</sup> Her use of "coherent" in this context suggests she believes that it is only by virtue of membership in a linguistic community that basic sense experiences are experienced or processed by humans in a consistent way. I take that to mean that it is only because we are members of epistemic communities that we consistently experience the same basic sense

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<sup>112</sup> Nelson uses the term 'common sense' theories is meant to denote the ordinary language macro-physical object metaphysics.

<sup>113</sup> Nelson, *WK*, 259. Nelson notes that Quine shares the empiricist commitment to objectivity, understood as entailing the exclusion of political interests and values from scientific theories and the construction of theories (Nelson, *WK*, 260). Nelson writes, "... Quine and the other empiricists have been concerned to exclude values, including political interests, from the content of scientific theories and from their accounts of the way we go about constructing theories- at least good theories" (Nelson, *WK*, 260) Nelson also differentiates her position from Quine's (Nelson, *WK*, 265).

<sup>114</sup> Nelson, *WK*, 277.

<sup>115</sup> Nelson, *WK*, 277.

<sup>116</sup> Nelson, *WK*, 259.



stimulus, say a surface heated to a 100 degrees farenheit, in the same way each time we come into contact with it. On Nelson's account, all other things being equal, one's basic sense experiences of that heated surface would vary each time one came into contact with it, rendering that experience incoherent and chaotic, if one were not a member of a linguistic community.

In taking that stance, Nelson commits herself to a form of social constructivism that is even more radical than the one proposed by R. N. Hanson. He contends that even though two people have the same sense experience of a phenomena, their observation claims reflect their theoretical commitments. While in terms of basic sense experience (specifically, color and size), both Johannes Kepler and Tycho Brahe saw the same phenomena- the sun and various other celestial bodies- their observation claims about the phenomena were theory laden. The former's claim reflected his commitment to a heliocentric conception of the universe, while the latter's observation statement echoed his commitment to a geocentric account of the universe.<sup>117</sup>

Like Hanson, Nelson is a social constructivist. However, she subscribes to a radical version of the position. For her, even basic sense experience is theoretically mediated. On Nelson's account, Kepler and Brahe would not even see the same thing. It is unlikely that that form of social constructivism is a viable position. The behavior of newborns, for instance, indicates that basic sensory experiences are coherent to them even though they lack any sort of linguistic abilities.<sup>118</sup> That newborns *consistently* react to sensations they experience as uncomfortable, say an overheated environment, by crying indicates that those basic sense experiences are coherent to

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<sup>117</sup>Hanson, *Patterns of Discovery*, 7.

<sup>118</sup>For instance, Jacob E. Steiner, Dieter Glaser, Maria E. Hawilo and Kent C. Berridge's research shows that that human newborns and newborns of other primates consistently react to sweet and bitter taste stimuli ("Comparative expression of hedonic impact: affective reactions to taste by human infants and other primates" in *Neuroscience & Biobehavioral Reviews*. Vol. 25, no.1. 2001, 53-74.)

them (i.e., they consistently experience them in the same way) even though they are not members of any linguistic community, and thus, are not privy to any theoretical framework.

There is some evidence that Nelson might see the merit of my critique of her work. Her discussion of fetal and postnatal neurobiology suggests that much: "Our current understandings of neurobiological development, for example, indicate that interpersonal experience is a necessary causal factor in the fetal and postnatal neurobiological development that permits language acquisition, conceptualization and *many* perceptual experiences (my italics)." <sup>119</sup> I believe that her use of "many" indicates that she does not believe all perceptual experience is culturally mediated. But it is a problem that that claim is at odds with her categorical assertion that basic sense experience is made coherent by membership in epistemic communities.

In response to my argument against her radical social constructivist position, Nelson might argue that my theoretical commitment (to the idea that basic sense experience is not theoretically mediated) has colored my reading of newborns' behavior. I believe that response highlights yet another problem with the form of social constructivism that Nelson appears to be espousing. Proponents of radical social constructivism can discount all attempts to refute their position by reading them as mere reflection of the theoretical commitments of their opponents, and thus, not consider them a challenge to their position. I have serious reservations whether a theory, in this case, radical social constructivism, that does not admit of conditions

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<sup>119</sup>Nelson, *WK*, 285.

under which it could be refuted or falsified, and thus, does much to encourage critical dialogue. That kind of dialogue is crucial to science if it is not to be dogmatic.<sup>120</sup>

In her own defense, Nelson could try to argue that by positing evidence as empirical control for theories, she is only following in the footsteps of Quine. But Nelson can't evoke that line of defense because by "empirical controls" Quine means sense experience, i.e., the impinging of the world on human sensory organs, which he considers to be independent of and unmediated by one's theoretical commitments.<sup>121</sup> Thus, a fundamental difference between Nelson and Quine is that while the latter is committed to realism with respect to basic sense experiences, the former is a social constructivist all the way through. Given these problems with Nelson's account of the presence of values in science, her position should not be accepted uncritically.

Having assessed both Nelson's and Longino's critique of the logical-positivist conception of objectivity, now, I articulate a notion of objectivity that is based on my analysis of their work. I believe that philosophers of medicine and medical theorists ought to adopt it because it is informed by actual human epistemic practices (unlike the logical positivist one) and it is more capable of identifying the values shaping theories than the one that logical positivists subscribed to (I argue the latter point in chapters three and four).

## 2.8 *An Alternative Notion of Objectivity*

I contend objectivity is a stance or a particular way of engaging with the world. Its adoption by epistemic agents means, amongst other things, recognizing that

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<sup>120</sup>One of the reasons creationism, for instance, cannot lay claim to being a scientific theory is that it does not admit of conditions under which it could be falsified.

<sup>121</sup>Quine, TDE, 80. See earlier discussion of Quine's position.

all theories are value-laden. Theories are created by scientific communities as well as the larger non-scientific community within which the scientific communities are embedded. The theories formulated by these epistemic communities reflect and are expressive of their values and interests.<sup>122</sup>

Although this argument may appear to be the same as Nelson's, it is not. The following three claims capture the fundamentals of my position, whilst highlighting the similarities and differences between it and Nelson's stance:

- i. Unlike Nelson, I believe basic sense experience is independent of common sense and scientific theorizing. This is a crucial point because it allows me to argue that basic sense experience can be used to serve as neutral arbitrator between competing theories, whilst sidestepping the problem of self-refutation that Nelson runs into.
- ii. I, like Nelson, hold that there is a deep connection and interdependence between common sense theorizing and scientific theorizing because communities, rather than individuals, are primary knowers of theories. It is by virtue of one's membership in epistemic communities that one knows the world. (This claim is consistent with (i) because that is an assertion about basic sense experience, while this claim is about non-scientific and scientific theories.)
- iii. Along with Nelson, I believe that given the deep connection between common sense and scientific theories, no scientific theory can be considered value free. All theories reflect the values, interests, and beliefs of the larger non-scientific epistemic community within which the scientific community that produced that theory is lodged.

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<sup>122</sup>That is not to say that theories that are expressive of oppressive values should not be discarded in favor of others that are not encoded with such values.

Thus, it must be acknowledged that cultural norms *necessarily* play a role in shaping scientific theories.

Although I hold that particular scientific and medical theories are the product of specific forms of life (i.e., cultures), I acknowledge that virtually no culture is homogeneous, with only one coherent set of beliefs, values, and interests. Therefore, scientific and medical theories must be understood as representing the complex interplay between the various values, interests, and beliefs of a particular society. I also want to qualify my position by arguing that although a particular society may have produced a certain theory, it does not mean that no other culture could have constructed it. For instance, it could be argued that it is unlikely that the current Hmong culture could have produced the atomic theory but (given that cultures change over time), it could at some point in the future become the kind of society that might have created that theory. Cultures are not static but change as the material circumstances of life change. Also, given that very few cultures are categorically isolated from others, I contend the interplay between various forms of life also accounts for changes in them.

Even as I argue that scientific theories reflect the values, interests, and beliefs of the societies that generated them, I do not mean to deny that the use a particular society makes of a certain theory may imbue it with a different sort of ethical or political significance than the one that it "had" by virtue of being the product of a society that was committed to a certain form of life. For instance, a scientific theory developed by a society, specifically, for the purposes of developing warfare technologies, if used by another society for therapeutic purposes would in that cultural context have a different value than what it had in the society that originally conceived

of it. It would also mean that those two societies would conceptualize and interact differently with the world.

The notion of objectivity that I endorse places a premium on encouraging critical dialogue as means for testing the merits of the theoretical model under consideration. Thus, as part of that conception of objectivity I advocate the use of the six strategies that Longino proposes for encouraging such dialogue. They are as follows:

1. There are and should be publicly recognized forums (such as meetings, conferences, journals, etc.) for critiquing hypotheses, evidence, methodology, and inference
2. The social and epistemic standards by reference to which theories, hypotheses and evidence claims are critiqued should be and are public.
3. The scientific community should and usually does tolerate dissent and responds appropriately to criticism.
4. Scientists should know the history of their discipline (as that would give them critical insight into it).
5. The various communities engaged in critical dialogue should and, arguably, do afford each other equal respect on the basis of their arguments rather than because of a certain group's social or political power.<sup>123</sup>
6. The scientific community should engage in dialogue with disenfranchised communities in the interest of identifying culturally pervasive assumptions and values ingrained in theories that might be invisible to the members of the scientific

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<sup>123</sup>Longino, SPK, 273 and SSK, 76-81.

community by virtue of their dominant position but visible to those at the margins of society.

Besides the above mentioned six strategies, I also advocate that the institutional and cultural context of medical theories should be analyzed *through the eyes of the historically oppressed populations*.<sup>124</sup> That strategy is different from the one that Longino has proposed. She claims that scientists be aware of the history of the field they are working in in the interest of having a critical perspective on it. While that approach can be fruitful (and thus, I endorse it too), I believe my approach has the potential to generate a different kind of critique than the one that scientists versed in the history of their field but who unquestioningly accept the dominant oppressive value system might be likely to produce.<sup>125</sup> In the next chapter, I demonstrate the efficacy of that strategy by applying it two theories that medicalize menstruation as a pathological condition.

I have argued that objectivity is a way of engaging with the world that requires that epistemic agents acknowledge, amongst other things, that cultural norms necessarily play a role in shaping theories. However, I do not believe commitment to that notion of objectivity precludes the possibility of theorists recognizing that particular theories may also have values embedded in them in a number of other ways. For instance, values may be present in particular theories in the following five ways:<sup>126</sup>

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<sup>124</sup>In chapter five, I discuss at some length the question why epistemic privileged should be afforded to the marginalized.

<sup>125</sup>Although Longino does not espouse my strategy, I believe that she would find it appealing because it is consistent with her overarching goal of science as a practice that is self conscious and critical.

<sup>126</sup>This is not to say that these are the only ways in which values may be present in particular theories.

1. Scientific theories may contain value laden background assumptions<sup>127</sup> that mediate the relationship between hypotheses and evidence cannot be considered value neutral. (That, as Longino acknowledges, does not mean that all theories have values in this respect but only those who have these sorts of background assumptions.)
2. It must also be conceded that insofar as particular theories contain concepts that are value laden, those theories may not lay claim to being value free.
3. The shape of scientific theories (for example, which hypothesis is developed and which one is not) could also be determined by contextual values that are not embedded in the theories proper (in chapter three, I use some case studies to demonstrate this point).
4. A scientific theory cannot lay claim to being value neutral if its development or testing required that scientists make decisions about the acceptable level of risks posed by particular research projects or products.<sup>128</sup>
5. Given that technologies are the means by which humans organize or interact with their material and social world, scientific theories qualify as technologies. Insofar as technologies have a social impact, they cannot be considered devoid of any political or ethical significance. Thus, to the degree any scientific theory qua technology is used by humans to organize or interact with the world in a manner that has a social impact, it cannot be said to be ethically or political neutral.

My notion of objectivity is informed by human epistemic practices. Given the social, value laden nature of knowledge building activities, all theories have contextual values. All theories are value laden in the sense that they are expressions of the values and concerns of the communities that formulated them (and they *may* have value laden

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<sup>127</sup>See earlier footnote on Longino's use of the term "background assumption."

<sup>128</sup>See earlier discussion about Rudner's argument about scientists qua scientists making value decisions.



concepts and background assumptions). Thus, I contend it is problematic to speak of "objective" as a predicate applicable to theories or to science as a discipline. My position is at odds with Hempel's. He claimed that knowledge (read: claims about the world, including theories) could be objective<sup>129</sup> (see section 2.1 of this chapter for account of Hempel's notion of objectivity).

As Nelson has argued that all theories have contextual values, she, I believe, might be receptive to my position that "objective" is not a predicate applicable to theories (Nelson does not articulate a notion of objectivity nor does she offer any strategies for identifying the values shaping theories). Given that Longino has argued objectivity is a method of inquiry that entails critical dialogue about scientific theories,<sup>130</sup> I believe she too might be receptive to my notion of objectivity even though there are significant differences between our positions.

In the next chapter, I elaborate on and argue for my notion of objectivity, focusing on the strategy of assessing medical theories *as well as* their institutional and cultural context through the eyes of historically oppressed populations. I make the case that approach could render visible values shaping theories that might otherwise remain invisible.

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<sup>129</sup>Hempel, 1965, 141. Emphasis in original. As noted in section 2.1, Hempel argues that even Carnap can be read as being committed to this conception of objectivity (Hempel, VOS, 380-1).

<sup>130</sup>See above account of the critical practices endorsed by Longino as part of her notion of objectivity. Also, see SSK, 76.

## CHAPTER THREE

### 3.0 Introduction

In this chapter, I use my notion of objectivity to analyze two recent theories medicalizing monthly menstruation.<sup>131</sup> I view these theories through the eyes of marginalized communities. I argue that the contextual values embedded in the theories *as well as* their cultural and institutional values play a significant role in shaping the two theories. I begin by describing and analyzing R.V. Short and *Williams Obstetrics*' menstruation-as-pathology theory. Then, I detail and analyze Elsimar Coutinho and Sheldon Segal's menstruation-as-pathology theory.

### 3.1 Williams Obstetrics' *Menstruation-as-Pathological-Evolutionary-Aberration Theory*

In North America, *Williams Obstetrics* has the status of an epistemic authority on women's reproductive physiology. Since the 1980s, in every edition of this obstetrics textbook, monthly menstruation has been portrayed as a diseased evolutionary abnormality. In the 1993 edition, for instance, the editors, Gary Cunningham, Paul MacDonald, Norman Grant, Kenneth Leveno and Larry Gilstrap claim that because monthly menstruation is not the evolutionary norm for non-human female primates *and* because some women (percentage unknown) experience the socially debilitating PMS,<sup>132</sup> women's periodicity is an evolutionary aberration that is

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<sup>131</sup> A version of this chapter was published as "Of Sex, Nationalities and Populations: The Construction of Menstruation as a Patho-Physiology," *Menstruation: A Cultural History*. Andrew Shail and Gillian Howe eds. (Basingstoke: Palgrave- MacMillan 2005).

<sup>132</sup> *Williams Obstetrics*, 1993, 97.

pathological.<sup>133</sup> They characterize it as an endocrinopathy (i.e., an inherent hormonal disorder):

With all good intentions, we have reassured our daughters that cyclical menstruation is normal; and from this perspective, it has been assumed by many women that repetitive menstruation is equated with femininity. If this prevalent premise that recurrent ovulation is the biological evolutionary norm were incorrect, it would be easier to accept the likelihood that the recurrent secretion and withdrawal of progesterone is an endocrinopathy that is pivotal in the development of luteal phase disabilities (i.e., PMS).<sup>134</sup>

The implication of this argument is that menstrual suppressant contraceptives ought to be used as 'treatment' for all females of childbearing age, barring those contemplating pregnancy.<sup>135</sup>

### 3.2 *Short's Menstruation-as-Pathological-Evolutionary-Aberration Theory*

The editors of *Williams Obstetrics* owe this theory to R.V. Short, a British veterinarian. Short's 1976 "Evolution of Human Reproduction" argued that in evolutionary terms monthly menstruation was a recent phenomenon attributable to the agricultural revolution. The increased availability of food heralded an increase in human female fertility, followed by the development of various means of

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<sup>133</sup>These ideas, in one form or another, are voiced in the 1989, 1997 and 2001 editions of this textbook as well.

<sup>134</sup>Williams Obstetrics, 1993, 98.

<sup>135</sup>The "logic" of their argument is discussed later.

contraception. That development, in conjunction with better diet, resulted in human females experiencing a nine-fold increase in monthly menstrual cycles (previously their menstrual cycles were not monthly). Short contended that the increase in the number of monthly menstrual cycles was an evolutionary aberration because females of other primate species did not experience it and neither did women in primitive human societies, such as the !Kung hunter-gatherers of the Kalahari Desert:

There can be no doubt that this ninefold increase in the time spent having menstrual cycles poses a number of new problems for us; it is something of which we have had no prior evolutionary experience, and hence we are not genetically adapted to cope with this situation... If an endless succession of menstrual cycles is a relatively new experience for us, we must immediately ask whether they are in any way harmful.<sup>136</sup>

He charged that this deviance from the evolutionary norm for primates must be considered pathological because monthly menstruation in human females is correlated with the incidence of certain forms of cancer,<sup>137</sup> premenstrual syndrome (PMS) that impairs women's ability to function well socially, and blood loss, which seriously compromises malnourished women in developing nations.<sup>138</sup> On the basis of this analysis, Short concluded:

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<sup>136</sup>Short 1976, 18-9.

<sup>137</sup>The jury is still out on this one.

<sup>138</sup>Short 1976. I discuss the issue of anemia later in this chapter.

Since the female is the limiting reproductive resource,<sup>139</sup> it seems sensible that we should emulate nature and concentrate mainly on female-oriented birth control technology. We should also try to recapture what civilization has destroyed, the ability to keep the ovaries and the female reproductive tract in a state of quiescence when reproduction is not desired. Women may be psychologically ill-adapted to spend the greater part of their reproductive lives having an endless succession of menstrual cycles.<sup>140</sup>

He believed that his work “highlighted the importance of developing a non-steroidal contraceptive that would allow a woman to return to the reproductive state that was the norm for our primitive ancestors- amenorrhea.”<sup>141</sup>

Of the various reasons that Short gave for his position, his PMS argument<sup>142</sup> is the least speculative. Short’s claims about PMS were derived from the work of a British psychologist and general practitioner Katharina Dalton. As Dalton's research is central to Short's argument, I briefly discuss her work.

Dalton, researching in 1960, noted that out of a group of 217 11-17 year old girls, the performance level of 54 girls fell by a letter grade in academics and housekeeping tasks during their pre-menstrual phase, and rose to its former level in the week following menstruation. Dalton attributed the change in academic performance to water retention during the pre-menstruum. She concluded that, “[a]bout one girl in every six in any examination entry will be in her pre-menstruum and thus at her lowest

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<sup>139</sup>Short argued since females invested greater energy than males during reproduction (females gestate, males don't), and "human societies tend to be polygynous, woman is the limiting resource (in reproduction)" (Short, 1976, 5).

<sup>140</sup>Short 1976, 21.

<sup>141</sup>Short, 1976, 20.

<sup>142</sup>Short, 1976, 19.

intellectual ebb. *While zealots campaign assiduously for equality of the sexes Nature refuses to grant equality even in one sex.*"<sup>143</sup>

Dalton's 1961 "Menstruation and Crime" argued that there was a significant correlation between the pre-menstruum and criminal behavior. Her claim was based on a six-month long study of a group of 156 regularly menstruating women prisoners. Dalton claimed that out of the group of 94 prisoners reported for bad behavior and who menstruated regularly, misbehavior in 54% coincided with the pre-menstruum or menstruation.<sup>144</sup> Citing her earlier work, she noted that there was a marked similarity between "the effect of menstruation on naughty schoolgirls, newly convicted women, and disorderly prisoners."<sup>145</sup> She claimed that though the offenses of the schoolgirls and criminal women were different, they shared certain traits. She speculated that menstrual hormonal changes probably made these females "less amenable to discipline"<sup>146</sup> and thus inclined to misbehavior or criminal activity. But she also acknowledged the possibility that the symptoms of PMS, including "lethargy, slower reaction time and mental dullness," may have made these females' anti-social activities more easily detectable than otherwise, accounting for more reports of miscreant behavior.<sup>147</sup>

The problems with Dalton's research are numerous, ranging from a lack of control groups to the use of non-representative samples to unjustified conclusions to unwarranted universalizations.<sup>148</sup> Therefore, Short was not justified in invoking her

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<sup>143</sup>Dalton, 1960a, 328.

<sup>144</sup>"Misbehavior" is not evidence of "criminal behavior" but Dalton appears to think so.

<sup>145</sup>Dalton, 1961, 1753.

<sup>146</sup>Dalton, 1961, 1753.

<sup>147</sup>Dalton, 1961, 1753.

<sup>148</sup>Given that Dalton is herself a woman, her rejection of feminism is interesting. I address the issue of women who subscribe to a patriarchal world view in chapter five. Also, see Emily Martin, 7, for a developed critique of Dalton's research.

research to derive his larger conclusion that monthly menstruation was a diseased evolutionary aberration, necessitating use of menstrual suppressant contraceptives by fertile women not considering pregnancy. Furthermore, insofar as the editors of *Williams Obstetrics* rely on Short's work (and albeit indirectly, Dalton's research) to categorize menstruation as an endocrinopathy, their analysis lacks credibility too.

Aside from these general methodological concerns, Short's attempt to argue that women's reproductive physiology ought to conform to the evolutionary norm for non-human primates is problematic because he makes no such demand of men's reproductive physiology. His treatment of the issue of the incidence of morphological abnormal spermatozoa in human male semen is illustrative of this double standard. He notes that in contrast to the semen of males of other primate species (with the exception of gorillas), the sperm of human males has a greater incidence of abnormality. Whilst Short acknowledges it as being responsible for the high incidence of embryonic mortality in humans, he makes no effort to argue that that aspect of human male reproductive physiology must be considered an evolutionary aberration that is pathological:

Normal human semen is notorious for showing an extremely high proportion of morphologically abnormal spermatozoa, often in excess of 40%, whereas the other primates (except the gorilla) have remarkably uniform spermatozoa (Seuanez, Martin & Short, unpublished observations). This suggests that a high proportion of human spermatozoa may be genetically defective, and would produce an abnormal embryo if they were capable of fertilizing the egg. There

is circumstantial evidence to suggest that we do suffer an extremely high incidence of embryonic mortality.<sup>149</sup>

It is also interesting to consider whether his work would have been enshrined in a textbook on human physiology or anatomy if he had constructed a theory construing a feature of human physiology that was common to men and women as a diseased evolutionary aberration. For instance, if he had theorized that bi-pedalism is an evolutionary aberration that is pathological because, first, it is not common in other primates, and second, it causes some percentage of the population to experience various physical ailments, such as fallen arches and back pain, impairing their ability to function well in society, it is unlikely his theory would have been enshrined in any textbook on human physiology. But the analogous argument about women's reproductive physiology has received uptake in *Williams Obstetrics*.

In general, although Short and the editors of *Williams Obstetrics* use arguments mired in sexist bias and shoddy research to categorize women as intrinsically flawed entities,<sup>150</sup> they consistently identify themselves as genuinely concerned about women's well-being. For instance, Short worries about women enduring "heavy, painful, and socially inconvenient" menstruation.<sup>151</sup> He also expresses concern about the health of women in developing country being

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<sup>149</sup>Short 1976, 12. On Short's behalf, it could be argued that even though men's sperm may have a high incidence of abnormality, it ought not be considered pathological because it does not impair men. In contrast, menstruation should be considered pathological because it harms women. I owe this point to Professor J. Nelson.

<sup>150</sup>It is also worth noting that by arguing for the use of menstrual suppressant contraceptives as 'treatment' for monthly menstruation, Short and the editors of *Williams Obstetrics* are able to align themselves with those who support reproductive rights. Thus, they could construe any critique of their position as motivated by anti-choice and anti-birth control sentiments that compromise women's well being.

<sup>151</sup>Short, 1976, 18.



compromised by menstruation if they have malaria or hookworm disease.<sup>152</sup> The editors of *Williams Obstetrics* express similar sentiments. They claim that in this era of scientific development

we as obstetricians ... look forward to the time when women can choose infertility without obliging the endocrinopathy of recurrent progesterone secretion in massive amounts, which also necessitates the associated distress of menstruation that this choice now brings. We submit that each woman should be able to select when to be (or not to be) pregnant as a positive demonstration of her own choice without being obliged to make such a choice from a variety of inadequate options.<sup>153</sup>

Short et al.'s endorsement of the use of menstrual suppressant contraceptives (a form of hormonal therapy) as *the* medical technological intervention of choice for women with PMS and anemia is puzzling. During the 1960s and the 70s in Britain, when Short formulated his theory, the standard treatment protocol for anemia was iron tablets, and that for PMS was tranquilizers, diuretics, *or* hormonal therapies.<sup>154</sup> Similarly, currently, in North America, anemia is usually treated using iron tablets, while PMS is treated using psycho-pharmaceuticals (such as anti-depressants and anti-anxiety drugs), diuretics, *or* hormonal therapies.<sup>155</sup> However, Short and the editors of *Williams Obstetrics* contend the use of menstrual suppressant contraceptives is *the*

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<sup>152</sup>Short, 1976, 18-9. He also articulates other worries in similar vein that appear to show his concern for women's well being.

<sup>153</sup>*Williams Obstetrics*, 1993, 15-6.

<sup>154</sup>Appleby 1964, "Premenstrual Symptoms" 1973, Tonks 1975.

<sup>155</sup>Dickerson, L.M. "Premenstrual Syndrom," *American Family Physician* 15 April 2003; 67(8): 1743-52.

solution to the health risk to women posed by monthly menstruation, without providing arguments that demonstrate that all the standard treatments for PMS and anemia have little or no efficacy or, at any rate, are inferior to menstrual suppressant contraceptives. What is also puzzling is that neither Short nor the editors of *Williams Obstetrics* provide any explanation for their decision to advocate the wholesale use of menstrual suppressant contraceptives for all women (barring those contemplating pregnancy) rather than their selective use by women who suffer from PMS and anemia.<sup>156</sup>

As Short's and the editors of *Williams Obstetrics*' menstruation-as-pathology theories do not provide a satisfactory answer to those questions, I examine the cultural context of their theories from the perspective of the socially marginalized in the hope that it might provide some insight about their stance. I do not advocate examining the social framework of theories *solely* from the perspective of the marginalized group that the theory is about (in the case of the menstruation theory, that group is women). I believe that because various forms of oppression may be interconnected, scrutinizing the cultural context of theories from multiple marginalized positions may reveal things about the values shaping those theories that might not be visible if the inquiry is limited in terms of the marginalized perspectives it draws on. So, in analyzing the social framework of Short's theory, I evoke the perspective of those who are marginalized- nationally and globally- in terms of their sex, race, class, and nationality, rather than just their sex (I offer a fuller account of this strategy in chapter five; I bracket this issue for now in the interest of maintaining the flow of my analysis of the menstruation theory).

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<sup>156</sup>Whether menstrual suppressant contraceptives are the appropriate treatment for anemia is discussed later.

### 3.3 *Scrutinizing the Cultural Context of Short Et Al. 's Theorizing Using Marginalized Perspectives*

Short's endorsement of menstrual suppressant contraceptives, rather than iron tablets and mood stabilizers, as 'treatment' for problems like anemia and PMS correlated with monthly menstruation takes on an interesting hue when it is considered in light of his cultural context. Four years prior to the menstruation paper, in 1972, Short, deeply influenced by Robert Malthus' hypothesis about the relationship between population growth and food supply work, had written "Reproduction and Human Society." He had argued that the rapid growth of population in Asia and Africa posed a most serious threat to the survival of the human species. Short maintained that the current accumulation of wealth by Western nations at the cost of nations in Asia and Africa, coupled with the rapid growth of population in Asia and Africa,<sup>157</sup> had set the stage for global conflict with potentially disastrous results:

Many economists have predicted that the rich nations of the world are destined to grow richer, at the expense of the poor nations who are doomed to become poorer. It seems doubtful if mankind could survive the ensuing racial and national tensions if there was to be an increasing polarization between the haves and the have-nots.<sup>158</sup>

Short appeared to believe that this global crisis could be averted if the West developed population control strategies, specifically contraceptives, for the female populations of

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<sup>157</sup>In these poorer nations, children were "looked upon by parents as the best insurance against old age, and by politicians as a way of ensuring that their country's voice would be heard in the council of world" (Short, 1972, 124).

<sup>158</sup>Short, 1972, 124.

Asia and Africa. In 1972, Short was not alone in expressing this populationist anxiety, colored by racism, about the growth rate of population in Asia and Africa; in fact, he was vocalizing sentiments that had been echoing across Britain for more than a decade then. (I use the term 'populationist' to refer to the school of thought that holds that high population growth is the most critical threat to economic development and democracy).

Beginning in the 1950s, an acute labor shortage in Britain had occasioned an in-flow of semi-skilled workers from former British colonies in South Asia and Africa that had gained independence in the wake of WWII. Most of these immigrant labor populations were concentrated in the industrial areas experiencing labor shortage. David Eversley and Kollmann Wolfgang argue that the high fertility amongst these localized commonwealth immigrant populations,<sup>159</sup> in contrast to the low and declining fertility amongst earlier white immigrant groups, coupled with stark differences in social practices (such as dress, religious practices, festivals, shop hours) and the resolution of the labor shortage, accounted for the public outcry amongst the white working class citizens opposing the presence of colored new immigrants:

Where jobs and housing were already in short supply, the employed 'colored' population was accused of taking jobs which 'should have been given' to the indigenous populations, and keen resentment was felt, for example, against immigrants with large families who thereby earned more points on council house waiting lists and were accused of 'jumping the queue.'<sup>160</sup>

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<sup>159</sup>Eversley and Wolfgang, 1982, 48.

<sup>160</sup>Eversley and Wolfgang, 1982, 55-6.

Although, the ‘jumping the queue’ claims were not justified and the newer immigrants were more economically vulnerable in periods of high employment than the white citizenry,<sup>161</sup> in response to these nationalist tirades, the British government implemented the first in a series of four immigration acts aimed at closing-off Britain to South Asian and African immigrants. With the passage of the 1962 Commonwealth Immigration Act, the British government began collecting data on the flow of people from its former colonies into Britain *and* limiting entry to those commonwealth citizens who held work vouchers, their dependents and the dependents of those already settled in the UK.<sup>162</sup> Until the passage of the 1962 Act, citizens of the British commonwealth nations had not been regarded as aliens, and therefore, they had not been subject to immigration control by the British government. In 1968, pandering to continued public opposition to colored immigrants, the Commonwealth Immigration Act was extended to include East-African Asians (the erstwhile business strata of these nations had recently begun immigrating to Britain in increasing numbers). To further placate the white working class populations, two other immigration control acts were passed in 1969 and 1971 by the British government.

Short’s 1972 paper on the global population crisis cannot, therefore, be read as having no local significance. In fact, his 1976 “Evolution of Human Reproduction” prescribing more or less universal use of menstrual suppressant contraceptives could be read as an attempt to solve the ‘problem’ of high rates of reproduction amongst colored populations within Britain and globally.<sup>163</sup> When viewed against a larger

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<sup>161</sup>Eversley and Wolfgang, 1982, 56.

<sup>162</sup>Eversley and Wolfgang, 1982, 439.

<sup>163</sup>However, it must be acknowledged that he was also advocating menstrual suppressant contraceptive use by white women as well. In response, it can be argued that his motivating factors are a complex mix. I have already pointed out that he appeared to be genuinely committed to women's well-being even while espousing sexist arguments.

cultural backdrop of racist, populationist paranoia, the espousal of menstrual suppressant contraceptives certainly appears to be politically motivated.<sup>164</sup>

The analysis of the cultural context of Short's work from the perspective of those at the margins of society reveals that his theory was shaped by sexism as well as a host of other concerns. Specifically, his endorsement of menstrual suppressant contraceptives as "treatment" for menstruation was motivated by a complex of nationalist, racist, populationist concerns. I have also argued that those values were a reflection of the concerns of the dominant population in Britain in that era. Thus, Short's theory must not be understood as merely reflecting the idiosyncratic beliefs of its creator but mirroring the concerns of the communities with whom he identified.

I have analyzed the cultural context of Short's theory through the eyes of the disenfranchised. That approach casts a different kind of light on Short's work (specifically, his reliance on shoddy research, fallacious arguments, and espousal of menstrual suppressant contraceptives as the treatment of choice for PMS and anemia) than the light provided by the standard method for assessing theories (based on the logical-positivist notion of objectivity). The standard approach for assessing theories might be critical of his fallacious arguments and reliance on shoddy research but it would assume that his theory was value-free because the evidence in support of the hypotheses constituting his theory could be intersubjectively verified. Thus, the standard method for evaluating theories would do little or nothing to make Short or other medical theorists aware of the host of values shaping particular medical theories. In contrast, my approach, based on my notion of objectivity, can do much to make

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<sup>164</sup>Whether Short's theorizing or the flood of racist, nationalist, populationist sentiments had any impact on birthrates in Britain amongst the colored immigrant, working class population is beyond the purview of this project. But it is an important question that needs to be investigated.

medical theorists aware of their work as a value-laden enterprise, and thus, more aware and critical of their work than they might be otherwise.

Now, to return to the *Williams Obstetrics*' menstruation-as-pathology theory. Interestingly enough, the populationist concerns that appear to have motivated Short's theorizing (especially his endorsement of menstrual suppressant contraceptives, as opposed to iron tablets or better nutrition or mood stabilizers, as 'treatment' for the problems of anemia and PMS correlated with monthly menstruation) are also present in *Williams Obstetrics*. Much like Short, the editors of this obstetrics textbook construe "over-population" to be "the greatest hazard to the health and environmental and economic future of humankind."<sup>165</sup> Whilst they do not identify Asia and Africa by name as regions with high population growth, they note with alarm that "[t]he population doubling time in some countries today is believed to be less than 20 years, perhaps as low as 12 or 15. The population of the world has already passed the 5 billion mark!"<sup>166</sup>

Disturbing as the endorsement of universal use of menstrual suppressant contraceptives by Short and the editors of *Williams Obstetrics* is, it is reassuring to note that their position seems not have received much uptake amongst the larger North American medical community. Not all gynecological and obstetrics textbooks discuss the similarity between women's reproductive physiology and that of non-human primates, and those that do, such as *Kistner's Gynecology & Women's Health*, do not go on to posit the reproductive biology of non-human primates as the norm that human

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<sup>165</sup>*Williams Obstetrics*, 1993, 11. Presumably, economic exploitation, sexism, racism, industrial pollution, militarisation are not key threats to human well-being.

<sup>166</sup>*Williams Obstetrics*, 1993, 11.

females ought to conform to.<sup>167</sup> However, it is troubling to think that in at least some North American medical schools, future generations of gynecologists and obstetricians are cutting their teeth on *Williams Obstetrics*. One can only hope that they understand the model of menstruation as an endocrinopathy as a theoretical construct and not as the truth about women.

Although the populationist anxiety expressed in the *Williams Obstetrics* menstruation-as-pathology theory may appear to be an idiosyncrasy of the editors of that textbook that is not shared by the American medical community, I believe it reflects certain cultural concerns. (I discuss them below in section 3.6 while analyzing another theory that medicalizes women's periodicity.)

### 3.4 Coutinho and Segal's Menstruation-as-Pathology Theory

Distinct from the attempt in *Williams Obstetrics* to characterize monthly menstruation as a pathological evolutionary development, necessitating the uniform use of menstrual suppressant contraceptives, in the last few years another movement construing women's periodicity as a health risk has emerged. Its proponents are Elsimar Coutinho, a Brazilian gynecologist, and Sheldon J. Segal, an American biochemist and embryologist. In *Is Menstruation Obsolete* (1999), they claim that unless contemplating pregnancy all females of childbearing age ought to use menstrual suppressant contraceptives because "[r]ecurrent menstruation is unnecessary and can be harmful to the health of women. It is a needless loss of blood."<sup>168</sup>

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<sup>167</sup>*Kistner's Gynecology & Women's Health*, edited by Kenneth J. Ryan, Ross S. Berkowitz, Robert L. Barbieri, Robert L. Barbieri M.D. and Andrea Dunaif, Mosby, Inc., 1999, 25.

<sup>168</sup>Coutinho and Segal, *IMO?*, 159.



Foremost and strongest amongst their many reasons for ‘treating’ monthly menstruation with menstrual suppressant contraceptives are the arguments that it is responsible for PMS and it both causes and compounds iron-deficiency anemia. Below, I evaluate these two arguments.<sup>169</sup>

### 3.5 *Coutinho and Segal’s Iron-Deficiency Anemia and PMS Arguments*

Coutinho and Segal claim that iron deficiency anemia is a serious problem for developing nations.<sup>170</sup> They contend that the female population in these parts of the world is particularly compromised by anemia because of menstrual blood-loss:

For countless malnourished women around the world already suffering from chronic anemia, the blood loss associated with normal menstruation causes depletion in iron stores that can worsen the condition. For these women, anemia must be recognized as a potentially serious consequence of repetitive menstruation.<sup>171</sup>

As a solution to the problem of iron-deficiency anemia in these parts of the world, they suggest the use of menstrual suppressant contraceptives by all females of childbearing age:

The problem of anemia in less affluent countries of eastern and southern Europe and in many developing countries remains much worse (than that in the

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<sup>169</sup>I focus on these two arguments because they are their strongest arguments.

<sup>170</sup>Coutinho and Segal, *IMO?*, 93, 160-1.

<sup>171</sup>Coutinho and Segal, *IMO?*, 92.

richer, more developed nations). Considering that the monthly loss of blood is absolutely needless and that the treatment of anemia significantly increases the cost of the already overburdened health services of these countries, the suppression of menstruation would be an important public health measure, as well as a health benefit to individual women.<sup>172</sup>

There are numerous problems with this proposal. First, the predominate cause of iron-deficiency anemia amongst the poor (men, women, and children) of the global South is inadequate sanitation facilities, resulting in high incidence of hookworm disease.<sup>173</sup> Assuming that Coutinho and Segal would not be opposed to treating the root cause of iron-deficiency anemia by providing the compromised populations with adequate sanitation facilities, iron tablets and treatment for hookworm disease (the latter solution, according to UNICEF, cost only a few U.S. dollars per years per person),<sup>174</sup> it is unclear how suppressing monthly menstruation is any part of a sound solution

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<sup>172</sup>Coutinho and Segal, *IMO?*, 160-1.

<sup>173</sup>According to the U.S. Department of Health and Human Services, "Hookworm eggs are passed in human feces onto the ground where they develop into infective larvae. When the soil is cool, the worms crawl to the nearest moist area and extend their bodies into the air. They remain there – waving their bodies to and fro – until they come into contact with the skin, usually on a bare foot, or until they are driven back down by the heat. Hookworm is widespread in those tropical and subtropical countries in which people defecate on the ground and soil moisture is most favorable" ("Parasitic Roundworm Diseases," National Institute of Allergy and Infectious Diseases, National Institutes of Health, U.S. Department of Health and Human Services. Source: [www.niaid.nih.gov/factsheets/roundworm.htm](http://www.niaid.nih.gov/factsheets/roundworm.htm) (April 22, 2004)). The hookworm larvae once inside their human host cause trouble for them when the "... worms attach their mouths to the lining of the small intestine and suck the person's blood... Once established within the intestine, the adult worms can cause abdominal pain, decreased appetite, diarrhea, and weight loss. Most importantly, the worms suck between 0.03-0.2 ml of blood per day. When a worm moves from one area of the intestine to another, it detaches its mouth from the intestinal lining, leaving an irritated area that may continue to bleed for some time. This results in even further blood loss. A single adult worm can live for up to 14 years in a patient's intestine. Over time, the patient's blood loss may be very significant. Anemia is the most serious complication of hookworm disease, progressing over months or years. " ("Hookworm Disease," Health A to Z. Source: [http://www.healthatoz.com/healthatoz/Atoz/ency/hookworm\\_disease.jsp](http://www.healthatoz.com/healthatoz/Atoz/ency/hookworm_disease.jsp). Downloaded: 18 May 2006). I owe this point to Dr. I. Khan.

<sup>174</sup>Source: [www.unicef.org/sowc98/panel20.htm](http://www.unicef.org/sowc98/panel20.htm) (7th April 2004)

(and quite clear how awesome a potential market the global South can be for menstrual suppressant contraceptives -more on this later).

Coutinho and Segal also argue that menstrual suppression should be adopted by women in wealthy, industrial nations, such as the U.S., because, according to a *Journal of the American Medical Association* article, about 7.5 million young females in the U.S. are iron-deficient.<sup>175</sup> They take this as evidence of a direct causal connection between monthly menstruation and iron-deficiency. However, their attempt to marshal this study in support for their position is problematic because the authors of the research project in question came to a conclusion that was inconsistent with Coutinho and Segal's thesis. In "Prevalence of iron deficiency in the United States," Anne Looker et al. argue that "iron deficiency and iron deficiency anemia are still relatively common in the United States among women of childbearing age, especially those who are black or Mexican American, poor, and have 12 or fewer years of education or 4 or more children."<sup>176</sup> Looker's conclusion (that the incidence of iron-deficiency and iron-deficiency anemia appears to vary with the race, number of children and education and poverty level of their female research subjects, rather than just their sex and fertile status) is at odds with Coutinho and Segal's simplistic analysis.

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<sup>175</sup>By means of an extensive search of the JAMA archives on the subject of anemia, I was able to identify the article in question as Coutinho and Segal failed to cite the authors, the title and the date of publication of the article. This is true for a number of studies that Coutinho and Segal mention but do not adequately reference. For instance, Coutinho and Segal cite US National Sleep Foundation research project on PMS related sleep disturbances but fail to adequately identify the study (Coutinho and Segal, *IMO?*, 68). They also claim that the Center for Safety Studies in the US has determined that due to cognitive impairment of women experiencing PMS "the forty-eight hours preceding menstruation are the most dangerous because this is the time when most accidents occur" but provide no additional information about this organization (Coutinho and Segal, *IMO?*, 74). I was not able to locate these and a number of other studies that they mention even after a careful and fairly elaborate web search.

<sup>176</sup>Looker, A.C., Dallman, P.R., Carroll, M.D., Gunter, E.W., Johnson, C.L. Prevalence of iron deficiency in the United States. *JAMA*. Vol. 277, Iss. 12, Mar 26, 1997.

The other argument that Coutinho and Segal use in support of their thesis focuses on the phenomenon of PMS. Citing the work of Katharina Dalton<sup>177</sup> and several others who purport to document women's impaired state during the pre-menstruum,<sup>178</sup> Coutinho and Segal attempt to argue that suppression of ovulation by means of menstrual suppressant contraceptives is the "most logical rationale for resolving the discomfort of PMS at its source."<sup>179</sup> Citing a study conducted on only 20 women,<sup>180</sup> who showed improvement in their symptoms (which included sadness, bloating, food craving, irritability, etc) after being put on a regimen of ovulation suppressants, Coutinho and Segal try to argue that because menstrual suppressant contraceptives alleviate PMS symptoms in some women who suffer from the condition, all females of childbearing age ought to use menstrual suppressant birth-control. This argument does not hold much water because it makes no sense to 'treat' someone for a condition she does not have.

Coutinho and Segal also attempt to appeal to feminists by citing the work of Emily Martin and Sophie Laws on PMS. In *The Woman in the Body*, Emily Martin challenges the predominant negative reading of PMS in Western cultures. She argues that such a reading is a function of the patriarchal, capitalist industrialist complex. During the pre-menstruum, any expression of anger or resistance by women against their oppressive reality is attributed by the establishment to an alleged physiological flaw in these women, rather than treated as a warranted critique of the large social

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<sup>177</sup>Coutinho and Segal, 1999, 73-80. See above for an analysis of Dalton's methodology.

<sup>178</sup>Here again, Coutinho and Segal do not adequately cite their reference(Coutinho and Segal, 1999, 68 and 74). These failures and the blind faith they expect from their readers reflect their assurance in the justification of their own epistemic authority, not an approach compatible with the conception of women as rational agents, interested in and capable of making informed health decisions.

<sup>179</sup>Coutinho and Segal, 1999, 72.

<sup>180</sup>Schmidt, Peter J., Nieman, Lynette K., Danaceau, Merry A., Adams, Linda F., Rubinow, David, R. "Differential Behavioral Effects of Gonadal Steroids in Women with and in those without Premenstrual Symptoms." *New England Journal of Medicine*, Jan 22, 1998.

order. Martin also worries about the use of this sort of logic to justify violence against women by their male partners in the domestic context. In *Issues of Blood*, Sophie Laws argues that men's negative attitude towards the pre-menstruum and menstruation as well as their commitment to sexist gender stereotypes about women has played a significant role in shaping women's experience of the physiological and psychological changes preceding menstruation.

Although Coutinho and Segal claim to engage with Martin's and Laws' analysis, I do not believe that they actually do so because, on the heels of a brief synopsis of Martin's and Laws' work, they write, "Clearly, PMS can be a serious problem for the patient and for those around her, but it should not be accepted as inevitable."<sup>181</sup> To claim that PMS is the problem, whilst (supposedly) acknowledging the merit of Martin and Laws' analyses is to utterly miss the point of their work. It could also be argued that Coutinho and Segal misrepresent Laws' work by juxtaposition's her analysis about men's negative attitude towards the pre-menstruum and menstruation shaping women's experience of the premenstruum with claims by researchers (whom Coutinho and Segal do not identify) who contend that women with PMS are abusive and neglectful of their children, without making it clear that Laws herself makes no such claim about women with PMS.<sup>182</sup>

Coutinho and Segal's failure to treat seriously feminist analyses of the patriarchal construction of PMS can be understood in light of their comment about women who do not view menstruation as a wholly negative experience. They seem to consider most such analyses as the emotional outpouring of women who suffer from PMS and experience relief with menstruation. They write, "Articles describing

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<sup>181</sup>Coutinho and Segal, 1999, 71.

<sup>182</sup>Coutinho and Segal, 1999, 71.

menstrual bleeding as being good for women's health have appeared in magazines and newspapers, frequently written by women with PMS, who experience relief from these symptoms with the arrival of their menstrual flow."<sup>183</sup>

Coutinho and Segal's willingness to espouse patently weak arguments seems to speak of a commitment to the idea that the female body is intrinsically flawed. In fact, this notion appears to motivate them to argue that delaying menarche in young girls (by 'treating' them with menstrual suppressant contraceptives) might be a solution to the problem of incest and rape:

In Brazil, almost 30,000 pregnancies in girls age 10 and 11 were reported in 1995. Family members and friends impregnated most of these girls. A girl's biological father was often the sexual assailant. The direct solution to this problem, which should be applied forcefully and without compromise is to control the offenders or to remove them from the home ... A late menarche is no guarantee of protection, but it delays her pubertal change, the young girl may be less likely to be targeted and victimized by sexual harassment and abuse.<sup>184</sup>

To suggest delaying adolescence, including menstruation, as part of the solution to the problem of rape and incest reveals a deep commitment to the idea that the sexually mature female body is responsible in some way for the violence that is visited upon it by rapists and sexual abusers.

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<sup>183</sup>Coutinho and Segal, 141.

<sup>184</sup>Coutinho and Segal, 1999, 162.

In general, Coutinho and Segal rely on methodologically suspect research and misread studies and feminist scholarship to make shoddy arguments characterizing women as intrinsically flawed. But, like Short and the editors of *Williams Obstetrics*, they identify themselves as motivated by and committed to women's well-being and autonomy. Coutinho and Segal write,

*Is Menstruation Obsolete?* argues for menstrual suppression based on *improving women's medical health*. The discussion does not involve wresting control of women's bodies in order to place it in the hands of doctors, nor exhortations to conceal evidence of menstruation, nor attempts to suppress or erase women's femininity. Instead, it presents a fresh view of menstruation, ultimately concluding that regular menstruation over the course of a woman's reproductive years is simply not necessary from a medical perspective. It is then up to the reader to decide whether these arguments outweigh her personal feelings about menstruation (my italics).<sup>185</sup>

Coutinho and Segal's decision to advocate menstrual suppressant contraceptives as *the* treatment for PMS and anemia even though, in North America, the standard treatment for anemia is iron tablets and for PMS it is psycho-pharmaceuticals (such as anti-depressants and anti-anxiety drugs), diuretics, *or* hormonal therapies is puzzling.<sup>186</sup> I believe examining the institutional and cultural context of their theory through the perspective of the disenfranchised, nationally and internationally, might prove fruitful

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<sup>185</sup>Coutinho and Segal, *Is Menstruation Obsolete?*, x.

<sup>186</sup>Dickerson, L.M. "Premenstrual Syndrome," *American Family Physician* 15 April 2003; 67(8): 1743-52.

in answering that question as it did in the case of Short's theory.<sup>187</sup> So, mirroring my analysis of Short's theory, I analyze the social and institutional framework of Coutinho and Segal's theory from multiple marginalized perspectives. (As mentioned earlier, I bracket discussing this strategy until chapter five in the interest of not disrupting the flow of the case analysis.)

### 3.6 *Examining the Cultural and Institutional Context of Coutinho and Segal's Theory Using Marginalized Perspectives*

Sheldon Segal is part of the Population Council, a transnational non-profit organization with a populationist agenda, and Elsimar Coutinho has also worked for the Population Council but his association with it has been much more limited.<sup>188</sup>

As a senior key figure at the Population Council, Segal has been influential in guiding the development of various forms of contraceptive devices, including intra-uterine devices and sub-dermal hormonal contraceptive devices, such as Norplant and Jadelle. He has also written a number of articles on the problem of over population. His book *Under the Banyan Tree: A Population Scientist's Odyssey* (2003) reflects his deep engagement with the issue. Segal, by virtue of his prominent institutional position, has also been instrumental in shaping global family planning programs by

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<sup>187</sup> As discussed earlier in section 3.2 of this chapter, given that various forms of oppression may be interconnected, as part of my strategy for identifying values shaping medical theories, I advocate examining those theories from multiple marginalized perspectives, including the oppressed group or groups that are directly impacted by those theories.

<sup>188</sup> Coutinho was instrumental in the development and testing of Depo-Provera (Coutinho and Segal, 1999, 8-9).



serving in an advisory capacity to the World Health Organization, the United Nations Population Fund, the World Bank, the European Parliament, and the U.S. Congress.<sup>189</sup>

Established in 1952, the New York based Population Council is the brainchild of John D. Rockefeller.<sup>190</sup> Its mission is to improve the material condition of humanity as well as strive towards a humane, equitable, and sustainable balance between people and resources<sup>191</sup> by developing and encouraging the use of contraceptives in poverty stricken areas of the world with high population growth.

The Council's web-site characterizes Rockefeller's interest in the issue of population growth as purely humanitarian, arising out of his travels through poverty stricken, densely populated regions of South and East Asia.<sup>192</sup> Thomas Shapiro indicates that a 1950 Rockefeller Foundation report with a Malthusian analysis of the relationship between population, human welfare, social change and the threat of political upheaval<sup>193</sup> significantly shaped Rockefeller's views on population growth amongst the global poor. The report recommended that human fertility ought to be reduced "so that growth can be kept at the least dangerous possible" (quoted in Shapiro, 64).<sup>194</sup> In the wake of that report, in 1952, along with like-minded industrialists and researchers, Rockefeller organized a conference where the need for an organization to address the 'population problem' was articulated.<sup>195</sup> At its inception, the trustees of the Population Council included Frederick Osborn

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<sup>189</sup>Source: <http://www.popcouncil.org/staff/bios/ssegal.html> (April 24, 2004). According to Eugenics Watch's "Activities Database: Directors: Last Name Beginning M-Z," Segal served as the director of the American Eugenics Society between 1969-80 and 1987-92 (Source: <http://www.africa2000.com/ENDX/aedir-m.htm> (June 26, 2004)).

<sup>190</sup>Member of the American Eugenics Society since 1930 (Source: <http://www.africa2000.com/ENDX/> (July 1, 2004)).

<sup>191</sup>Source: [www.popcouncil.org/about/about.html](http://www.popcouncil.org/about/about.html) (April 18, 2004).

<sup>192</sup>Source: [www.popcouncil.org/about/about.html](http://www.popcouncil.org/about/about.html) (April 18, 2004).

<sup>193</sup>Shapiro, 1985, 64.

<sup>194</sup>Shapiro, 1985, 64.

<sup>195</sup>Shapiro, 1985, 65-6.

(Advisory Council Member, American Eugenics Society, 1928-81),<sup>196</sup> Frank Notestein (Director, American Eugenics Society, 1950-56)<sup>197</sup> and representatives of some of the nation's wealthiest industrialist families, including Rockefeller, Mellon, and Ford.

The Council's mission statement suggested that high fertility rates were the crucial cause of widespread poverty in Africa and Asia, endangering the survival of the species and occasioning global economic and political instability. It failed to appropriately acknowledge that the poverty pervasive in the 1940s and 50s in Asia and Africa was the product of complicated interaction between local and global multiple systems of oppressions, including the systematic exploitation of the labor force and natural resources of these regions by colonizers. Working in conjunction, these factors gave few the opportunity to escape poverty. Even today, the Council remains committed to its original analysis, labeling high population rates as the critical threat to the well being of humanity.<sup>198</sup> It fails to acknowledge that the present cause of poverty and political instability in developing nations is attributable to a number of factors, including corrupt political regimes, interaction between local and global multiple systems of oppression, systematic exploitation of natural resources and labor by transnational corporations, unjust trade agreements benefiting the global powers and transnational corporations, mis-guided developmental policies and massive military budgets.

The shortcomings of the Council's agenda have in no way limited its ability to marshal the support of various non-governmental and governmental agencies across

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<sup>196</sup>Eugenics Watch. "Activities Database: Officers." Source: [www.africa2000.com/ENDX/aeoff.htm](http://www.africa2000.com/ENDX/aeoff.htm) (June 26, 2004).

<sup>197</sup>Eugenics Watch. "Activities Database: Directors: Last Name Beginning M-Z." Source: [www.africa2000.com/ENDX/aedata.htm](http://www.africa2000.com/ENDX/aedata.htm) (June 26, 2004).

<sup>198</sup>Source: [www.popcouncil.org/about/about.html](http://www.popcouncil.org/about/about.html) (April 18, 2004).

the globe, including, at present, the U.S. government, the World Bank,<sup>199</sup> the United Nations, the World Health Organizations and various governmental entities. In the interest of providing an account of how the Population Council came to have global influence, I focus on the history of its relationship with the U.S.

Frank Notestein, as Laura Landolt points out, was instrumental in marshaling the support of the U.S. government for the Council's populationist agenda. After traveling through China in 1949 (on a trip funded by the Rockefeller Foundation), Notestein, "influenced by China's fall to communists, ... (had) suggested that US efforts to promote liberal political and economic institutions might be threatened by political instability and communist expansion amongst developing nations" whose disaffected rural citizenry was mired in poverty.<sup>200</sup> Prior to and in the wake of WWII, the U.S. government was deeply committed to the Domino Theory about the spread of communism.<sup>201</sup> Therefore, Notestein's analysis suggesting that curtailing population was the solution to preventing the spread of communism would have garnered the U.S. administration's support for the Council.

Bonnie Mass notes that the spread of communism in resource rich nations in Central America, Asia and Africa was seen as a serious threat by the premiere U.S. industrialists. The nationalization of mining and various other industries in these regions of the world would seriously impact the ability of U.S. industries to obtain raw material. According to Mass, The President's International Development Advisory

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<sup>199</sup>The staffers of Population Council routinely serve in advisory and consultant capacity to the World Bank and other organizations (Population Council, Annual Report 2001, 51). Therefore, it could be argued that it is because of the influence of the Council that World Bank has introduced lowered population growth as a lending term on its loans to developing nations.

<sup>200</sup> Landolt, 2003, 13-4.

<sup>201</sup> Roosevelt, Franklin D. "Address by President Franklin D. Roosevelt on July 4, 1941," source: <http://gurukul.american.edu/heintze/Roosevelt1.htm> (June 27, 2004) and Truman, Harry, "President Harry S. Truman's Address before a Joint Session of Congress, March 12, 1947," source: <http://www.yale.edu/lawweb/avalon/trudoc.htm> (June 27, 2004).

Board, chaired by Nelson Rockefeller,<sup>202</sup> estimated that nearly three-quarters of the raw materials used in the United States originated in Asia, Africa and Latin America.<sup>203</sup> Given that the Council's agenda of curbing population growth could help ensure U.S. industries access to raw material by curtailing poverty and stemming the spread of communism in Asia, Africa and Central America, it was in the interest of the U.S. administration to establish ties with the organization.

Over the years, the collaboration between the Council and the U.S. government has flourished, translating into significant budgetary support for the organization. In 2000, \$41.12 million or 58% of the Council's budget was covered by monies from the U.S.<sup>204</sup> In 2001, \$37.3 million or 53% of its operating budget was covered by the government.<sup>205</sup> In 2002, \$37.84 million or 51% of its operating budget was provided by the U.S.<sup>206</sup>

Currently, the vast majority of the council's budget goes into the development of new contraceptive devices and into research about successful family planning programs for the poor. Although the threat of communism no longer substantiates a government rationale for global population control, the U.S. continues to support the Council's work because it remains committed to the idea that curbing population growth amongst the poor is key to global economic development and fostering democracy. In the recent past, as pharmaceutical companies have expressed reluctance to engage in contraceptive development, citing the risk of product liability lawsuits

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<sup>202</sup>Nelson Rockefeller was the chairman of the Advisory Board (1950-51). Sources: [www.infoplease.com/ce6/people/A0842152.html](http://www.infoplease.com/ce6/people/A0842152.html) & [www.encyclopedia.com/html/R/RockefeIN1.asp](http://www.encyclopedia.com/html/R/RockefeIN1.asp) (29 April 2004).

<sup>203</sup> Mass, 1976, 35.

<sup>204</sup>Source: [www.popcouncil.org/about/ar00/financials.html](http://www.popcouncil.org/about/ar00/financials.html) (April 18, 2004).

<sup>205</sup>Source: [www.popcouncil.org/pdfs/ar2k1.pdf](http://www.popcouncil.org/pdfs/ar2k1.pdf) (April 18, 2004).

<sup>206</sup>Source: [www.popcouncil.org/about/ar02/financials.html](http://www.popcouncil.org/about/ar02/financials.html) (April 18, 2004). Although the Population Council may appear to be a relatively small organization that should not be considered the indicator of the scope of its influence. Rather its interaction with various entities, such as the UN, the World Bank, USAID, etc, are a better indicator of its authority.

and social controversy, the U.S. government has taken the onus of supporting- albeit, indirectly- contraceptive development. Rather than provide monies to pharmaceutical companies, it funds the Council, a non-profit organization. The development of the sub-dermal contraceptive implant, Norplant, can be considered a ‘success’ story (of sorts) of this approach.

The Population Council invested \$23.5 million on researching and \$16 million on introducing Norplant in developing nations. As the groundwork for Norplant development and marketing was already in place, Leiras Oy, a Finnish company, purchased the license to manufacture Norplant from the Council and spent an estimated \$23 million in developing the manufacturing protocol. Also, thanks to the Council’s efforts, Norplant was picked up by Wyeth-Ayerst, a pharmaceutical company, which introduced it in the American private sector at a cost of \$50 million. As the pharmaceutical giant went on to garner significant profits from a product developed mostly through public funding, the claim of pharmaceutical companies that it is never profitable for them to enter the contraceptive market does not appear to be justified. For instance, sales of Norplant just in the United States during its first year on the market totaled \$141 million with 800 devices being inserted in women per day. By 1993, about 1 million women in the U.S. were using Norplant.<sup>207</sup> Although in 1999, a class action lawsuit was initiated by U.S. users (because the company had downplayed the side effects of the product), Wyeth-Ayerst’s parent company, American Home Products Corporation, settled the case to its advantage.<sup>208</sup> It made a

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<sup>207</sup> *Contraceptive Research, Introduction, and Use: Lessons From Norplant* (1998) by the Institute of Medicine (IOM), 108-10.

<sup>208</sup> It is worth keeping in mind whether any sort of legal redress was and is available to women who access Norplant and other contraceptives through family planning clinics for the poor in Third World countries but were and are not provided with complete information about these products. For that possibility significantly impacts the profit margins of pharmaceutical companies.

payment of about only \$50 million to approximately 36,000 women. Litigation analysts have argued that this settlement did not pose much of a burden to the company given its net profit of \$2.5 billion in 1998 (no small amount of which was attributable to the global sales of Norplant).<sup>209</sup> All in all, a number of pharmaceutical companies found it in their interest to participate in Norplant manufacture and sales thanks to the relationship between the Council and the U.S.

The Council's relationship with the U.S. has also translated into a symbiotic bond between the Council and the U.S. Agency for International Development (USAID). Using funds provided by various governments, non-governmental organizations and private individuals, USAID decides which contraceptive devices to purchase to supply to family planning clinics for the poor in developing nations.<sup>210</sup> It also funds and directs the training of the staff for these clinics.<sup>211</sup> The substantial financial support that USAID provides for the Council's contraceptive development efforts and family planning program research<sup>212</sup> translates into products that USAID uses to meet its own foundational goals. The development of Norplant and Depo-Provera as well as their use in family planning clinics for the poor globally, for instance, served the purposes of both organizations.

Given the Population Council's history of influence with the U.S. government (especially, USAID) and transnational financial institutions, if the Council throws its full weight behind Coutinho and Segal's argument for the use of contraceptives that

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<sup>209</sup>“Norplant company agrees to settle suits: Offer to women could top \$50 million” by Charles Ornstein in The Dallas Morning News, 08/26/99. Source: [www.legalactionforwomen.org/norplantsettlement.html](http://www.legalactionforwomen.org/norplantsettlement.html) (April 18, 2004).

<sup>210</sup>Hartmann, 1987, 256.

<sup>211</sup> See “USAID Policy Paper: Population Assistance,” Bureau for Program and Policy Coordination, USAID, September 1982, 6. Source: [www.usaid.gov/our\\_work/global\\_health/pop/populat.pdf](http://www.usaid.gov/our_work/global_health/pop/populat.pdf) (April 25, 2004).

<sup>212</sup>“Evaluation of the Population Council's Programmatic Cooperative Agreement with USAID,” Poptech Report No. 98-132-071 August 1998, vii. Source: [www.poptechproject.com/pdf/pcp71sum.pdf](http://www.poptechproject.com/pdf/pcp71sum.pdf) (April 25, 2004)

suppress menstruation, it is not inconceivable that in the name of promoting women's health these organizations could influence the governments of developing nations to 'encourage' the wide scale use of such contraceptives by their female populations by means of family planning programs for the poor. If there is such a push, the financial benefits that would accrue for pharmaceutical companies and for the Council, which owns the patent and licenses for various contraceptive devices that serve as menstrual suppressants, are worth taking into account.<sup>213</sup>

Whether the use of menstrual suppressant contraceptives universally translates into better health for women is unclear. In *No More Periods?*, as part of her critique of Coutinho and Segal's work, Susan Rako carefully documents the relationship between the use of hormonal contraceptive that suppress menstruation (with or without producing withdrawal bleeding) and the increased vulnerability to the human papilloma virus (HPV) infection and cervical cancer, along with the other risks associated with hormonal birth-control devices.<sup>214</sup>

It could, however, be argued that the decision of women who, under no pressure from poverty, make informed choices about using hormonal contraceptives must be respected. But are women in developing nations who access family planning clinics for the poor provided with information that would allow them to make informed choices, under conditions where their decisions are not dictated by poverty? In this regard, it is instructive to look at USAID-World Bank-Population Council supported family planning clinics for the poor in Bangladesh during the 1980s. Betsy

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<sup>213</sup> Of course, the Council would argue that as a non-profit organization it is not motivated by the possibility of financial gain but by its mission "to improve the well-being and reproductive health of current and future generations and to help achieve a humane, equitable, and sustainable balance between people and resources." Source: <http://www.popcouncil.org/about/about.html> (June 27, 2004).

<sup>214</sup> Rako, 2003, 100-39.

Hartmann documents appalling abuses and problems with these programs.<sup>215</sup> Working in conjunction with the local government, these family planning clinics used food and other material incentives to ‘convince’ a desperately poor population to use birth control, both reversible and permanent. The female clientèle were routinely not provided with adequate information that would allow them to make informed decisions about contraceptive use. Nor were these women provided with the medical follow-up and support required for the safe use of various forms of contraceptives.<sup>216</sup> Of course, this is not to characterize the women of developing nations as helpless victims, lacking any agency to speak out against the violation of their human rights. Local grassroots feminist organizations have successfully organized movements against abuses perpetrated by these programs.<sup>217</sup> But it would be unwise to underestimate the scope of influence of organizations, such as the Population Council, the World Bank and USAID, especially when they identify their policies, decisions and projects as motivated by concern for women’s health.

In recent years, all three of these entities have been quite vocal in their opposition to coercive practices in family planning clinics for the poor, which would seem to suggest a level of genuine concern for the plight of women in poverty-stricken nations. But whether that commitment has translated into practice is questionable. A 1995 publication of the INFO Project (a USAID funded project) advises family planning programs for the poor in developing nations to be less than forthright in discussing possible side effects. According to the report, “Because bleeding changes

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<sup>215</sup>Hartmann, 1987, 208-226.

<sup>216</sup>For an account of the human rights abuses perpetrated in various nations under the auspice of family planning for the poor, see Hartmann (1987) and Mass (1976).

<sup>217</sup>Hartmann identifies various other grassroots feminist organizations in Third World countries that have spoken out against human rights abuses by family planning programs (Hartmann, 1987, 171-2, 286-96).



and weight gain are so common, during counseling all women who choose injectables should be told of these likely changes. Program managers need to decide what other side effects to mention based in part on the side effects most often reported by clients. These decisions should be made with the goal of helping clients to make a fully informed choice and to use the method effectively and confidently.”<sup>218</sup> That the report recommends that program managers decide which side-effects to mention based on whether or not it is perceivable to the user allows for the possibility that program managers may consider themselves at liberty to not mention the risk of cancer, stroke, and other illnesses that may not be readily apparent to users as caused by the use of certain contraceptive devices.

I have “read” the cultural and institutional context of Coutinho and Segal’s through the eyes of the disenfranchised. That approach differently illuminates Segal and Coutinho’s work (specifically, their misreading of research, reliance on dubious studies, use of shoddy arguments to claim women are intrinsically defective (as opposed to men), categorization of menstruation as a disease, and espousal of menstrual suppressant contraceptives as the treatment of choice for PMS and anemia) than the standard method of assessing theories (based on the logical positivist notion of objectivity). Although the standard approach for evaluating theories might be critical of Segal and Coutinho’s fallacious arguments and expose their shoddy methodology for what it is, it would assume that their work was value-free because the evidence in support of their hypotheses could be intersubjectively verified. Thus, the standard method of assessing theories might not be of much help to Coutinho, Segal,

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<sup>218</sup> “New Era for Injectables with Counseling Guide and Fact Sheet,” *Population Reports*, XXIII/2, August 1995, chapter 4. Source: [www.infoforhealth.org/pr/k5/k5chap4.shtml#top](http://www.infoforhealth.org/pr/k5/k5chap4.shtml#top) (April 20, 2004).

or other medical theorists in making them aware of the host of contextual values shaping particular medical theories.

In contrast to the standard method for evaluating medical theories, my approach for assessing theories has much more to offer to medical theorists. In particular, it would allow them to 'see' the values shaping their theories, and thus, they would be more self-aware and self-critical than Short, the editors of *Williams Obstetrics*, Coutinho and Segal appear to be. Adoption of my approach may result in medical theories that generate representations of human physiology that are better than the ones that have been constructed so far. For instance, it may result in theories about women's reproductive physiology that are better than those that have been formulated by theorists who are unaware of, and thus, uncritical of the contextual values shaping their work.

### 3.7 Conclusion

In this chapter, I analyzed Short et al.'s theories as well as the cultural and institutional context of their work *through the eyes of historically oppressed populations*.<sup>219</sup> That approach is part of my notion of objectivity. Using that strategy meant drawing on the work of theorists that are themselves members of disenfranchised groups *or* theorists who may not be part of various marginalized groups but who have adopted the lens of the marginalized in their theorizing.<sup>220</sup> Both groups are critical of the account of the world that the dominant oppressive order has

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<sup>219</sup>In chapter five, I describe this strategy in some detail and consider objections to it.

<sup>220</sup>Some of those theorists are Emily Martin, Betsy Hartmann, Helen Longino, Bonnie Mass, Sophia Laws and Laura Landolt (I have cited their work). The other theorists whose work has influenced my analysis but whom I have not cited are Ruth Hubbard, Sandra Harding, Jackie Zita, Abby Wilkerson, Lynde Birke, bell hooks, Marilyn Frye, Patricia Hill Collins, and Donna Haraway.

created. The exercise of re-reading the menstruation-as-pathology theories as well as the cultural and institutional context of those theories through the eyes of the marginalized reveals that *the theories in question were shaped by and reflect the dominant cultural account of the women's bodies, population growth, scarcity of resources, poverty, and disease*. I have argued that the medicalization of menstruation in Short et al.'s theories is the product of a complicated interplay between multiple systems of oppression. Patriarchy was not the sole factor at work in those theories rather it was acting in concert with capitalist, racist, nationalist, and populationist concerns.

In espousing that strategy (as discussed earlier in chapter two, section 2.8), I differ from Longino. While she argues that members of marginalized communities should be invited to critique theories, she does not specifically recommend that they be asked (by the scientific community) to scrutinize the cultural and institutional context of theories. Instead, she claims that scientists be aware of the history of the field they are working in the interest of having a critical perspective on it. While that method might be fruitful (and thus, I endorse it too), I believe my approach has the potential to generate a different kind of critique than the one produced by scientists versed in the history of their field but who unquestioningly accept the dominant value system and social reality. Given that my strategy can illuminate the values that shape theories but which are not located in its concepts or background assumptions, and given Longino's interest in making visible all contextual values shaping theories, I believe she would endorse my strategy (of viewing the cultural and institutional context of theories through the eyes of the disenfranchised).

In the next chapters, I consider how Christopher Boorse and Tristram Engelhardt might assess the two menstruation-as-pathologies theories. I focus on Boorse and Engelhardt because their conceptions of disease are diametrically opposed and represent standard and highly reputed theories in philosophy of medicine circles. I make the case that their ability to "see" the host of values shaping those theories would be enhanced if they adopted my conception of objectivity, including my strategy of viewing theories as well as their institutional and cultural context through the eyes of the marginalized.

## CHAPTER FOUR

### 4.0 *Introduction*

This chapter, in a sense, is the culmination of my project.<sup>221</sup> In chapter one, I claimed that philosopher of medicine Christopher Boorse's and Tristram Engelhardt's answer to the question "medical theories objective" was unsatisfactory. In this chapter, I provide a well developed argument in support of my assessment of their position. My argument draws on the notion of objectivity I constructed in chapters two and three. It also relies on the two menstruation-as-pathology cases that I analyzed in the previous chapter.

With the goals of demonstrating the shortcomings of Boorse and Englehardt's position *and* making the case that they ought to adopt my notion of objectivity, I begin this chapter by laying out Boorse's definition of disease and his account of theories of diseases. Then, I consider how he might respond to Short et al.'s theorizing. After this, I use my conception of objectivity (including the strategy that I use in chapter three to identify values shaping medical theories) to highlight the shortcomings of Boorse's account of theories of diseases as well as his (possible) assessment of the menstruation as pathology theories. Then, I analyze Engelhardt's position using the same three step approach. I close the chapter by discussing the significance of my analysis for the question motivating my dissertation project.

### 4.1. *Boorse's Definition of Disease and His Account of Theories of Diseases*

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<sup>221</sup>I say "in a sense" because in the next chapter I consider objections to my notion of objectivity.

Boorse defines disease as involving interference in the function typically performed by the systems and subsystems within the organism belonging to a particular species, compromising its ability to reproduce and survive. This account of disease conceives of biological entities as constituted of a number of complex, interconnected systems, most of which "work together harmoniously in such a way to achieve a hierarchy of goals."<sup>222</sup> Just as the goal of cells is metabolism, elimination, and mitosis, and that of the heart is blood circulation, the goal of the organism as a whole is survival and reproduction.<sup>223</sup>

Boorse holds that his definition of disease allows him to sidestep two problems that arise when disease is defined as deviance from statistically normal functioning. First, if disease is conceptualized as deviation from statistically normal functioning within a species, benign anomalies (say, in terms of eye color) have to be considered diseased. And second, defining disease as deviation from statistically normal functioning means that, more or less, universal phenomena, such as tooth decay could not be classified as disease.<sup>224</sup>

Boorse contends that it is a strength of his definition of disease that it is sufficiently universal and abstract to be non-normative.<sup>225</sup> Distinguishing between strong and weak normative definitions of disease, he characterizes the former as the

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<sup>222</sup>Boorse, DDI, 551.

<sup>223</sup>Boorse, DDI, 551-2.

<sup>224</sup>Boorse, DDI, 546 and 551.

<sup>225</sup>In the 1975 version of this paper (published in *Philosophy and Public Affairs* 5 (Fall)), he gives a normative account of illness. However, in the 1983 version of the same paper (published in the anthology, *Concepts of Health and Disease*), in an addendum, he recants this distinction, eschewing it as a misguided concession to normativism. He writes, "Among many revisions I would make in this paper today, two are especially important. First, the view that illness is disease laden with values (i)-(iii) now seems a mistaken concession to normativism. Illness is better analyzed simply as a systemically incapacitating disease, hence no more normative than disease itself. Features (i)-(iii) are common social evaluations of illness, underlying the sick role, but they are probably no part of the meaning of 'illness'." (Boorse, DDI, 560) Given that Boorse rejects the distinction that he had drawn between disease and illness, I treat the two notions as being equivalent for him.

view that disease constructs are mere expressions of social disapprobation.<sup>226</sup> Weak normativism, in contrast, classifies various clusters of phenomena as disease if they interfere in species typical functioning of the organism *and* are the subject of social approbation.<sup>227</sup>

Boorse rejects both normative definitions of disease. He argues that the concept of disease that has a strong normative component is untenable because it treats all and any socially disvalued phenomena, ranging from ugliness to adolescent pregnancy, as diseased. Boorse also rejects the definition of disease that has a weak normative component. He contends it cannot appropriately deal with conditions, usually classified as disease, say, an infection, that may be desirable under some circumstances, say, following an inoculation. To round off his argument against the two normative definitions of disease, Boorse contends that if what is classifiable as disease is indeed a matter of social opprobrium, then disease and health become a matter of public consensus. Given that human consensus about disease and health do not determine claims about disease and health in non-human animals and plants,<sup>228</sup> allowing them to do so in the case of humans is inappropriate because humans are fundamentally the same sort of entities as non-human animals and plants, regardless of their complex social reality.

Given that Boorse is committed to a non-normative concept of disease, I believe he would be opposed to theories of particular diseases that had contextual values. In other words, his interest in formulating a universally valid, non-normative concept of disease suggests that *he would want theories of diseases to be value free as*

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<sup>226</sup>Boorse, DDI, 547.

<sup>227</sup>Boorse, DDI, 547-8.

<sup>228</sup>Boorse, DDI, 548.

*well*. It seems likely that he would reject theories of particular diseases that are laden with political, social, and ethical values as bad theories.

Having outlined Boorse's definition of disease and extrapolated from it his stance on the issue of theories of diseases being encoded with contextual values, now, I consider how he might assess Short et al.'s menstruation-as-pathology theories.

#### *4.2. Boorse's Possible Response to Short's, Williams Obstetrics', and Segal-Coutinho's Menstruation-as-Pathology Theories*

Short contends that menstruation must be regarded as a pathological evolutionary aberration because (a) human females are primates and females of other primate species do not menstruate, and (b) human females who menstruate may experience PMS and anemia. As Boorse defines disease (as interference in the function typically performed by the systems and subsystems within the organism belonging to a particular species, compromising its ability to reproduce and survive), I believe he would reject Short's claim (a) for categorizing menstruation as a disease on the grounds that the decision to classify a particular phenomenon as a disease must be decided without reference to the question whether that phenomenon manifests itself in other species.

However, Boorse might consider Short's claim (b) as necessary and sufficient reason to categorize women's periodicity as a disease, provided Short could make the



argument that PMS<sup>229</sup> and anemia compromise women in terms of their ability to reproduce, and possibly, survive. (To that end, Short would have to marshal research that was of a better quality than Dalton's.)

Boorse might also accept the argument made by the editors of *Williams Obstetrics* characterizing monthly menstruation as a pathological condition. They contend women's periodicity is an endocrinopathy that is responsible for PMS -a universally socially debilitating condition. If they were to claim that it impairs women's ability to reproduce or survive, Boorse would recognize monthly menstruation as a disease.

Boorse might also concede to Coutinho and Segal's argument for considering women's periodicity a disease. They claim monthly menstruation should be considered a pathological condition because it causes PMS and anemia in a significant number of women. Assuming that anemia impacts one's ability to reproduce and survive, and supposing that Coutinho and Segal could make a strong argument for the claim that PMS also impacts women's survival and reproductive capabilities, Boorse would grant their argument for medicalizing menstruation.

In the next section, I use my notion of objectivity to illuminate various shortcomings of Boorse's position.

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<sup>229</sup>He would probably argue that PMS impairs women's ability to function well in personal and professional contexts, compromising their capacity to reproduce, and eke out a good existence for themselves.

#### 4.3 *The Significance of My Notion of Objectivity for Boorse's Account of Theories Of Diseases And His (Possible) Assessment Of Short Et Al.'s Theories*

In chapter two, I made the case that objectivity should be understood as a way that knowers could choose to engage with the world. It required that epistemic agents concede among other things, that all theories are value laden insofar as they are expression of the interests and concerns of the epistemic communities that constructed them and that they should invite and respond appropriately to criticism about theories and research projects.<sup>230</sup>

If Boorse were to adopt my notion of objectivity, he would have to acknowledge that all medical theories are normative. He would have to rescind his commitment to the idea that medical theories can and should be objective (i.e., value-neutral), otherwise he would be making the same mistake as the logical-positivists. The belief that theories could be and should be denuded of value claims is premised on the idea that knowers could utterly detach themselves from their social context as they theorize. I have argued that conception of the knower is unrealistic because humans can only engage in knowledge building activities if certain social pre-conditions exist. Community membership is a necessary precondition for language development, acquisition, and formulation of beliefs, values, and interests, thus, individuals can know only as members of epistemic communities. Given that communities have values, beliefs, and interests, *and* individuals know only as members of communities, no theory - be it scientific or non-scientific- can lay claim to being absolutely value-free, in the sense of having being constructed by a knower who is detached from all

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<sup>230</sup>This is an abbreviated account of my conception of objectivity that is relevant to my present purpose.

contextual values.<sup>231</sup> By dint of that reasoning, Boorse would not be justified in positing value-free theories of disease as a realizable ideal nor would he be warranted in assuming that 'good' medical theories are value-free.

I would also urge Boorse to identify the contextual values shaping and endorsed by particular theories of disease using the strategies I have identified. To that end, he would find it useful to identify the value-laden background assumptions mediating the hypotheses-evidence relationship of particular theories of disease.<sup>232</sup> In the case of the menstruation-as-pathology theories, Boorse would have to begin his assessment of Short's<sup>233</sup> and Coutinho and Segal's theories by flushing out the background assumptions underlying their work. Short, for instance, *hypothesized* that monthly menstruation is an evolutionary aberration that is pathological that ought to be treated by means of "a non-steroidal contraceptive that would allow a woman to return to the reproductive state that was the norm for our primitive ancestors-amenorrhoea" (Short, 1976, 20). As *evidence* in support of that hypothesis, he made the following claims:

1. Monthly menstruation is the product of civilization (i.e., the agricultural revolution).
2. Females of other primate species do not menstruate on a monthly basis.
3. Females of primitive hunter-gatherer tribes such as the ! Kung of the Kalahari Desert do not experience monthly menstruation.

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<sup>231</sup> Nelson, *WK*, 315.

<sup>232</sup> See section on Longino in chapter two for a discussion of this strategy.

<sup>233</sup> As the argument in Williams Obstetrics is a truncated version of Short's I do not treat it separately from Short's.

4. Monthly menstruation in human females is correlated with the incidence of certain forms of cancer, premenstrual syndrome (PMS) that impairs women's ability to function well socially, and blood loss, which seriously compromises malnourished women in developing nations (Short 1976).

The key *background assumptions* mediating between Short's hypothesis and evidence are, first, PMS is a purely biological phenomenon caused by the chemical changes preceding menstruation.<sup>234</sup> Second, human females ought to conform to the reproductive norm for other female primates; not doing so causes anemia and PMS. Third, the cancers correlated with monthly menstruation are not attributable to any environmental factors. Fourth, women in primitive societies (like the females of other primate species do not menstruate monthly) do not experience the same incidence of cancers correlated with monthly menstruation. And fifth, the theory of evolution holds. Although Short does not acknowledge these background assumptions, they make his argument possible; there is nothing about the evidence claims in of themselves that necessitate the conclusion that women's periodicity is an evolutionary aberration that is pathological, necessarily requiring treatment with menstruation suppressant contraceptives.

Boorse could take Short to task over a number of these background assumptions. He could ask him to provide data about the incidence of various forms of cancer in women in primitive societies and females of other primate species. Boorse could also argue that Short's background assumption that human females ought to

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<sup>234</sup>That point is debatable because there is some evidence that at least in part women's experience of the premenstruum phase of their menstrual cycle is determined by cultural factors (Ross, L.E. "A biopsychosocial approach to premenstrual dysphoric disorder." *Psychiatric Clinics of North America*. Sep 2003; 26(3) and Johnson, Thomas M. "Premenstrual syndrome as a Western culture-specific disorder." *Culture, Medicine and Psychiatry*. Vol 11(3) Sep 1987. )

conform to the reproductive physiological norm for other female primates reflects a sexist bias. While Short acknowledges that relative to the incidence of abnormal spermatozoa in the semen of males of other primate species, the semen of human male contains a large percentage of morphological abnormal spermatozoa, he makes no effort to argue that this aspect of human male reproductive physiology should be categorized as an evolutionary aberration that is pathological. This sexist bias in favor of men must not be dismissed as Short's idiosyncratic belief because it resonates with and echoes the larger cultural belief that women are more animal-like than men are.<sup>235</sup>

Using my notion of objectivity (specifically, the analysis of the hypotheses-evidence relationship entailed by it), Boorse could also criticize Coutinho and Segal's theory on the grounds that it implies that menstruation is an evolutionary development that compromises women. But that argument wouldn't go far because Coutinho and Segal's thesis focuses primarily on PMS and anemia to justify the claim that women's periodicity should be medicalized. Boorse could call to the carpet Coutinho and Segal for the poor quality of their research methodology. But if Coutinho and Segal were to marshal better research and improve their methodology, Boorse might have to assent that monthly menstruation is a pathology. However, before conceding to their argument, I believe he ought to view their theory as well as its cultural and institutional context from the eyes of the marginalized. In chapter three, using that strategy, I argued that Coutinho and Segal's theory must be understood as the *product* of the contextual values embedded in their theories (i.e., sexist bias) *as well as* their

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<sup>235</sup>This should not be read as an argument for creating theories that medicalize species typical aspects of men's reproductive physiology. Rather my point is that Short et al.'s theories are value laden. I also want to draw attention to the value laden nature of all theories, including medical theories. That is not to say that all (value laden) medical theories are on par. Rather they should be assessed in terms of their epistemic merits as well as the values that shape them and the purpose they serve.

cultural and institutional values, specifically, racist, populationist, nationalist concerns. While I do not believe that a medical theory should be rejected just because it is encoded with contextual values, I do hold that theories that are shaped by sexist values, such as Short et al.'s theories, should not be viewed as value neutral and endorsed uncritically. What Boorse would have to say about this issue is unclear given that (I have argued) he seems to believe medical theories should be value neutral.

Next I discuss Engelhardt's account of concept of disease as well his conception of theories of particular diseases and his possible response to the menstruation as pathology theories as it presents an interesting contrast to Boorse.

#### 4.4 *Engelhardt's Conception of Disease and His Account of Theories of Diseases*

Engelhardt uses the notion of functioning proper to humans as a criterion for distinguishing between health and disease.<sup>236</sup> He also contends that theories of diseases identify clusters of phenomena correlated with impairment in functioning "held to be proper (however that is to be determined) to humans, or a state of affairs including pain that does not contribute to a proper human function, or a state which is characterized by deformity or disfigurement."<sup>237</sup> On that account, specifically the claim "impairment in functioning," it would appear Engelhardt subscribes to a concept of disease that is similar to the one Boorse is committed to. But it is because Engelhardt leaves open how impairment in function is to be determined, it seems that he might believe that what counts as disease might have a cultural component. That

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<sup>236</sup>Engelhardt, IE, 261.

<sup>237</sup>Engelhardt, IE, 261.

reading of Engelhardt's definition of disease is reinforced by his claim that theories of diseases<sup>238</sup> are social attempts to correlate a variety of phenomena "for the purposes of explanation, prediction, and control."<sup>239</sup> That suggests Engelhardt might not find it troubling if definitions of disease were normative concepts. Boorse, as I argued earlier, would find that idea unacceptable.

Understanding theories of diseases in the way Engelhardt does has two advantages. First, all diseases do not have to be treated as real entities with distinct etiologies. According to Engelhardt, treating diseases as a real entities with distinct etiologies is problematic because diseases, such as cancer and asthma, have a relational nature. They do not fit into the subject-predicate mold that is presupposed by the conceptions of disease as real entities with distinct etiologies. Diseases like cancer and asthma are the manifestation of a complicated relationship between various factors that range from the physiological to the genetic to the psychological to the sociological: "Diseases such as asthma, cancer, coronary artery disease, etc., are as much psychological as pathophysiological in that the likelihood of such illness is closely bound to experienced stress and the availability of support for the person stressed. They are thus sociological as well."<sup>240</sup>

According to Engelhardt, the other advantage of his notion of disease is that it allows theorists, with different interests, the opportunity to develop a variety of causal analyses, ranging from genetic to metabolic to psychological to social, of the

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<sup>238</sup> Although Engelhardt speaks of "notions of disease," I believe he means "theories of various diseases" rather than "concepts of disease." I am led to this conclusion because an account of the cluster of phenomena termed a disease usually requires a fairly elaborate model (which has predictive prowess); it is arguable whether concepts or notions, given their rather limited explanatory scope, are up to the task. This is, of course, a loose and incomplete description of the difference between theories and concepts.

<sup>239</sup> Engelhardt, CHD, 39.

<sup>240</sup> Engelhardt, CHD, 37.

phenomenon in question, without having to commitment themselves to understanding it in a one-dimensional, reductivist way.<sup>241</sup>

Engelhardt contends that theories of diseases are not value-free; rather, they reflect particular cultural norms. He makes this point by tracing the history of the pathologization of masturbation during the 19<sup>th</sup> century in various Western nations. The masturbation-as-disease theory gained currency because of pre-existing cultural values and beliefs about sexual activity. Those background assumptions (to use Helen Longino's terminology) gave shape to what 19<sup>th</sup> century medical practitioners and lay persons took to be evidence for the hypothesis that masturbation was a disease. In support of his account of theories of diseases, Engelhardt also points to medical theories of drapetomania and dysaesthesia aethiops as problematic, overt examples of value-laden theories of disease. During the 18<sup>th</sup> century in the slave-owning states, slaves who tried to run away were pathologized by the medical establishment as afflicted with drapetomania and those who refused to abide by the will of the slave owners and overseers were categorized as suffering from dysaesthesia aethiops.

Elaborating on his claim that theories of particular disease are value-laden, Engelhardt notes that a variety of different sorts of values may play a role in disease constructs, ranging from ethical values to aesthetic values (ideas about the ideal human form) to epistemic norms (the simplicity, the explanatory prowess and other epistemic virtues of competing notions of disease).<sup>242</sup> Though he argues that particular theories of disease express socially disvalued phenomena, he does not believe that all socially disvalued behaviors should be pathologized. His desire to refrain from

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<sup>241</sup>Engelhardt, CHD, 38. It could be argued that Boorse's concept of disease also allows for the construction of various causal accounts of particular diseases, including genetic, physiological, metabolic, etc.

<sup>242</sup>Engelhardt, "Ideology and Etiology," (IE), 260.



arguing for the medicalization of all socially disvalued behaviors (such as ignorance, greed, political violence) can be read as an attempt to allow for autonomy, and thus, moral responsibility.<sup>243</sup>

Moreover his account of the dominant population's attempts to pathologize attempts made by slaves to resist injustice<sup>244</sup> strongly suggests that, in general, he would not support theories of diseases that denied members of a particular group (who share a certain arbitrary biological characteristic) the opportunity to engage in self-determination so as to benefit members of another group. Engelhardt holds that although theories of disease are value-laden, as a society committed to the principle of autonomy, medicine should (and usually does) foster individual's autonomy rather than undermine it.<sup>245</sup> Thus, I believe he regards the medical theories of draptomania and dysaesthesia aethiopis as "bad" theories of disease.

While Engelhardt places a premium on autonomy that does not mean he is espousing libertarianism.<sup>246</sup> He argues that formulating a bioethics that recognizes a diversity of moral perspectives means acknowledging that, first, not all moral communities subscribe to the same notion of the divine, and second, reason does not lead all of them to the same moral conclusions. It is because such a bioethics cannot justify itself by appealing to either a transcendental reason or divine authority that it must rely for its legitimacy on the consent of the various parties involved in the formulation of such an ethic and who agree to abide by it. Therefore, for such a bioethics, autonomy (broadly speaking, the ability to engage in self-determination,

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<sup>243</sup>Engelhardt, CHD, 40-1 and *The Foundations of Bioethics*, 82.

<sup>244</sup>Engelhardt, CHD, 42-3.

<sup>245</sup>Engelhardt, CHD, 42.

<sup>246</sup>In the preface of the second edition of *The Foundations of Bioethics* (1996), Engelhardt is at pains to make it clear that his endorsement of autonomy as a primary value in the first edition of *FB* (1986) ought not be read as a commitment to libertarianism per se. He contends that although a number of readers of his 1986 edition of *FB* made this move, they were not warranted in doing so.

including the ability to give consent), by default, becomes primary. In other words, Engelhardt values autonomy above all other values because of a need to find a common ground for justifying a bioethics that could bridge the gap between a variety of moral perspectives. However, realizing that the use of the term "autonomy" seems to encourage readers to misread him as a libertarian, he substitutes "the principle of permission" for "the principle of autonomy" in his later works.<sup>247</sup>

Engelhardt conceptualizes health as a "regulative ideal" under girding theories of disease. The ideal in question is one that represents freedom from psychological and physiological forces, such as kleptomania and congestive heart failure, respectively.<sup>248</sup>

There are problems with Engelhardt's definitions of disease and health. Insofar as he appeals to a notion of functioning proper to humans as a criterion for distinguishing between health and disease states<sup>249</sup> (and if by that he is appealing to some sort of statistically normal functioning), he is vulnerable to the charge that his notion of disease would require the medicalization of phenomenon, such as eye color, that are a benign statistical deviances. Furthermore, virtually universal phenomena, like tooth decay, could not be categorized as diseases. Although, Englehardt does not appear to have addressed these charges, a response could be formulated on his behalf. He could respond that his conception of disease as impairment in functioning proper to humans is akin to Boorse's notion of disease as interference in the function typically performed by the systems and subsystems within the organism belonging to a

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<sup>247</sup>Engelhardt, FB, 1996, x-xi.

<sup>248</sup>Engelhardt, CHD, 42.

<sup>249</sup>Engelhardt, IE, 261.

particular species, compromising its ability to reproduce and survive.<sup>250</sup> But this move might be problematic because by endorsing Boorse's definition of disease (with his commitment to non-normativism), Engelhardt would have to rescind his claim that theories of diseases are normative constructs. I have argued that is not a tenable position because all theories are encoded with contextual values.

There is another serious problem with Engelhardt's position. His claim that particular theories of disease are value-laden is based primarily on an analysis of a few theories of disease (such as the medical theory of drapomania) and Joseph Margolis' claim that "medicine in general must subserve, however conservatively, the determinant ideology and ulterior goals of given societies, [and that] the actual conceptions of disease cannot but reflect the state of technology, and the social expectations, the division of labor, and the environmental conditions of those populations' (Margolis, 252)."<sup>251</sup> Analysis of a few theories of disease and a descriptive claim about the 'nature' of disease do not make for a compelling argument (more on this later). Thus, Engelhardt needs a substantive argument in support of his position that theories of disease are normative constructs.

Bracketing these concerns for now, below I consider how he might assess Short et al.'s theories.

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<sup>250</sup>Engelhardt might attempt to evoke Boorse's notion of disease because Engelhardt appeals to a notion of functioning proper to humans as a criterion for distinguishing between health and disease states (Engelhardt, CHD, 42) and that notion is very like Boorse's notion of disease as interference in species-typical functioning of the organism.

<sup>251</sup>Engelhardt, IE, 257. It is worth noting that Engelhardt does not mean to commit himself to an unmitigated social constructivist position (see above account of his concept of disease (Engelhardt, IE, 261 and 267)).

#### 4.5 Engelhardt's Possible Response to Short's, *Williams Obstetrics*, and Segal-Coutinho's Menstruation-as-Pathology Theories

Short claims menstruation must be regarded as a pathological evolutionary aberration because (a) human females are primates and females of other primate species do not menstruate, and (b) human females who menstruate may experience PMS and anemia. Claim (a) would be of no interest to Engelhardt because, as discussed above, he appeals to a notion of functioning proper to humans as a criterion for distinguishing between health and disease states.<sup>252</sup> However, he would find claim (b) quite compelling, provided Short could argue that PMS<sup>253</sup> and anemia are psychological and physiological phenomena that compromise women's autonomy. Similarly, if the editors of *Williams Obstetrics* and Coutinho and Segal were to make an argument characterizing women's periodicity as a pathological condition that compromises women's agency, Engelhardt would be compelled to concede their point.<sup>254</sup>

But perhaps this analysis is a bit quick. Engelhardt's careful, context-sensitive assessment of the medical theories of drapetomania and dysaesthesia aethiopis suggests he would be interested in examining the social concerns and context underlying and motivating Short et al.'s theories. However, he does not outline a method that could be used to examine theories of disease for the contextual values that might be influencing them.

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<sup>252</sup>Engelhardt, IE, 261.

<sup>253</sup>PMS is generally understood as a condition that has both a psychological component (for example, moodiness, irritability, etc) and a physiological dimension (e.g., bloating, cramps). But Short et al. understand it as a purely physiological phenomenon, attributing the psychological component to hormonal changes characterizing the pre-menstruum period.

<sup>254</sup>Whether they could garner sufficient evidence for their claims about the relationship between menstruation and anemia remains to be seen.

Putting aside that issue for now, let us assume that he is able to identify the sexist and populationist concerns as the values shaping Short et al.'s work. Given that he rejected the medical theories of the diseases of drapetomania and dysaesthesia aethiopis as bad theories of disease on the grounds that they were efforts on the part of the dominant social group to compromise the autonomy of another population, it might be supposed he would object to Short et al.'s theories because of their sexist commitments, compromising women's well being. However, it is unclear whether he would regard the populationist concerns motivating Short et al. as grounds for rejecting their theories, unless an argument could be made those concerns are an attempt on the part of the dominant group to compromise the autonomy of another people.

In the next section, I use my notion of objectivity to bring to light some of the shortcomings of Engelhardt's notion of disease as well as his (possible) assessment of the menstruation-as-pathology theories.

#### 4.6 *The Significance of My Notion of Objectivity for Engelhardt's Account of Theories Of Diseases And His (Possible) Assessment Of Short Et Al.'s Theories*

Engelhardt believes that all medical theories of disease are value-laden. While he is correct, (as I noted earlier) the argument that he gives in support of his claim is weak. He relies on a few historical cases of theories of disease and Joseph Margolis's observation about medicine. Engelhardt is not justified in making a universal claim about the nature of medical theories of disease on the basis of a handful of historical

cases (of theories of disease) and Margolis' claim that medicine serves the ideology and goals of particular societies as well as the social relations between various groups of that community and is constrained by the state of technological development of that society.<sup>255</sup> He needs a different kind of argument to motivate his universal claim about the nature of theories of disease.

I believe the argument that I constructed in chapter two about the communal, value-laden nature of all knowledge has the wherewithal to serve as justification for the claim that all medical theories of disease (as well as disorders and syndromes) are normative. No theory can be considered free of contextual values because (as I argued in chapter two on the basis of Nelson's work on, first, the deep connection between common sense theorizing and scientific theorizing, and second, the relational and communally constituted nature of the human self) theories reflect the interests, beliefs, and values of the communities that constructed them.<sup>256</sup> By arguing that that holds for medical theories about disease, Engelhardt would have provided a substantive epistemological argument to ground his claim that all theories of disease are value-laden.

The other reason my notion of objectivity should also be appealing to Engelhardt is that it offers strategies that he could use to identify the values that might be embedded in particular theories. For instance, he could interrogate the menstruation-as-pathology theories for value-laden concepts and background assumptions. As I have demonstrated earlier in this chapter (in the section on Boorse), how that could be done and how fruitful it could be, I will not repeat that here. I would

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<sup>255</sup>Engelhardt, IE, 257.

<sup>256</sup>Particular theories are attributed to specific individuals but given that those individuals know only as members of epistemic (scientific and non-scientific) communities that they are part of, in a deep sense, theories are the creations of those communities.

also urge him to examine the theory as well as its cultural and institutional context through the eyes of historically marginalized populations as I do in chapter three. Given the way he analyzes the medical theories pathologizing the desire of slaves to be free, I believe he might be willing to adopt that strategy. Moreover given his rejection of the medical theories of the diseases of drapetomania and dysaesthesia aethiopis. I believe he would regard theories of disease that foster or are complicit in compromising the autonomy of particular populations to benefit other ones as bad theories of disease.

#### 4.7 *Conclusion*

This chapter, as I mentioned earlier, in a sense, is the culmination of my dissertation.<sup>257</sup> I was motivated to engage with the question "are medical theories objective?" because I found Boorse's and Englehardt's response to it unsatisfactory. Drawing on my notion of objectivity, including the strategy of analyzing theories and their institutional and cultural context through the eyes of historically marginalized groups, I made the case that Boorse's and Englehardt's stance on the issue whether medical theories can be value neutral is flawed. I argued that they ought to adopt my notion of objectivity because it would allow them to construct an answer to the question of the objectivity of medical theories that was informed by actual human knowledge building practices (as opposed to an answer that was based on

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<sup>257</sup> I say "in a sense" because in the next and final chapter, I do more work on my notion of objectivity by addressing some objections to it.

misconceptions about those activities or was uninformed about those practices). That answer would be that medical theories can't be free of contextual values.

I made the case that espousing my conception of objectivity would mean that Boorse would have to give up the idea that medical theories could be non-normative. In contrast, for Engelhardt adopting my notion of objectivity would not have such a severe consequence. However, he would have to amend the argument that he gives in support of his position. As I explained above, it does not have the ability to serve as justification for his claim that all theories of disease are value laden. I have argued that he ought to use my argument about the social, value laden nature of all knowledge building enterprises to motivate that claim. Moreover, I have made the case that both Engelhardt and Boorse ought to use my strategies for bringing to light values at work in medical theories as without them they might not see those values.

The account of objectivity that I have formulated does not categorical prohibit the medicalization of species-typical aspect of men's or women's physiology.<sup>258</sup> But what it does do is, first, it requires that medical theorists recognize their work to be a value-laden activity, and second, it provides strategies (discussed in chapters two and three) that could help them recognize the contextual values shaping particular medical theories, and thus, be more critical and reflective about the theories that they devise.

In the next chapter, I respond to two serious objections to the notion of objectivity that I have constructed.

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<sup>258</sup> However, I do think the two menstruation-as-pathology theories discussed in chapter three are bad theories. They should be rejected on the grounds of the analysis that I have provided of them using my notion of objectivity.



## CHAPTER FIVE

### 5.0 *Introduction*

In this final chapter, I describe and respond to two objections to my notion of objectivity, specifically, the strategy that medical theories be critiqued from perspective of the marginalized communities. Then, I conclude the dissertation.

### 5.1 *Critiques of Medical Theories from the Perspective of the Socially Marginalized*

My conception of objectivity requires that medical theorists invite and respond appropriately to criticism of their work. That entails, amongst other things, that they engage with and respond to critiques of medical theories that have been constructed from the perspective of disenfranchised groups. Longino also posits this requirement as part of her notion of objectivity. She justifies it on the grounds that because background assumptions and values motivating and embedded in theories tend to be invisible to the members of the community that generates them,<sup>259</sup> in the interest of critically questioning them and bringing them to light, the scientific community ought to engage in a dialogue with those with alternative viewpoints.<sup>260</sup> Longino identifies feminists of different orientations and members of various other marginalized groups as those with an alternative perspective who ought to be part of that critical inquiry. Their involvement in critiquing scientific theories has the potential to change radically the nature of scientific inquiry:

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<sup>259</sup>In fact, acceptance of those assumptions may even be a precondition for membership in those communities.

<sup>260</sup>Longino, SPK 111-2.

The feminist intervention I imagine will be local, that is, specific to a particular area of research; they may not be exclusive, that is different feminist perspectives may be represented in theorizing; and they will be in some way continuous with existing scientific work. The accretion of such interventions, of science done by feminists as feminists, and by members of other disenfranchised groups, has the potential, nevertheless, ultimately to transform the character of scientific discourse.<sup>261</sup>

While Longino contends that the socially disenfranchised are more likely than those who are part of the dominant group to see background assumptions and values shaping theories, she does not provide a detailed account of why that is the case. I believe Carlo Filice's work is somewhat illuminating in that respect. He has argued that by the time one reaches adulthood in any society that is not utterly closed, one is exposed to alternative values *and* that all of us have moments of doubt about the account of social reality that is presented to us from childhood.<sup>262</sup> He contends one has a moral duty to explore those doubts.<sup>263</sup> While Filice's intent is to make the case that all of us (both those who are members of the dominant group and those who are socially marginalized) have a duty to be reflective about the account of the world that society presents to us, his argument could be used to explain why members of disenfranchised communities are *better positioned* than those who are privileged in a certain society to recognize the dominant cultural values as oppressive. Presumably, members of

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<sup>261</sup> Longino, SSK, 193-4.

<sup>262</sup> Carlo Filice. 1990. "On the Obligation to Keep Informed about Distant Atrocities" in *Human Rights Quarterly*, Vol.12, 397-414.

<sup>263</sup> However, he is silent about why that duty exists (more on this later).

marginalized groups by virtue of their social marginality *have more reasons* to question the dominant, oppressive paradigm than those who hold positions of power and privilege in that social order. Thus, in conjunction with others who are similarly situated and dissatisfied with the status quo, they can create an alternative vision of reality (that is, imagine the world otherwise than it is). In that reality, they are not oppressed. Whether all members of disenfranchised groups are able to or chose to pursue that option is an issue I discuss later (in the Critical Perspective Objection section).

Although I endorse Longino's strategy, for the sake of clarification, I want to add two qualifiers to my proposal.<sup>264</sup> The kind of critique about particular medical theories that I am interested in fostering has to, first, recognize that medicine is about providing accounts that are open to corroborations or disconfirmation empirically. For instance, the critique that I construct (in chapter three) of R.V. Short's theory' gendered use of the theory of evolution<sup>265</sup> is consistent with this criterion but a critique that takes issue with his use of the theory of evolution on the grounds that it renders redundant the notion of a supernatural creator is inconsistent with that criterion. Second, the critiques of medical theories constructed from the perspective of disenfranchised communities ought to assess medical theories on the basis of the values embedded in them *as well as* their epistemic merits (such as explanatory scope, predictive success, interenal coherence and consistency, compatibility with credible theoretical paradigms of other disciplines, etc).

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<sup>264</sup>I believe that Longino would approve of these criteria as they are consistent with her position even though she does not articulate them as such.

<sup>265</sup>Short uses it to argue that women ought to conform to the reproductive norm for other female primates but makes no such parallel claim about men;s reproductive physiology (see chapter 3, section 3.2).

I espouse those two criteria because the success of medicine can be attributed to them in no small degree. While the critical dialogue I envision occurring between medical theorists and marginalized communities is meant to be revolutionary in certain regards, it is intended to be respectful of those aspects of medicine that have a proven track record.

There are at least two objections that could be raised against my proposal. I term them the "Critical Perspective Objection" and the "Epistemic Privilege Objection." Below, I describe and respond to them.

## 5.2 *The Critical Perspective Objection*

It could be objected that my strategy appears to assume that all members of marginalized groups will have a critical perspective on the background assumptions and values shaping the theory under scrutiny. The argument could be made that that supposition is not warranted because some members of marginalized communities may subscribe to any one of the following three problematic positions:

Position A: Persons who subscribe to this view regard the epistemic success of a particular theory to be evidence that it is the one and only true unbiased account of the phenomenon at issue, untainted by any human concerns, interests, or contextual values. For instance, on the basis of the empirical adequacy of Short's menstruation-as-pathology theory, some women may believe that it is *the* one true value-free account of human female reproductive physiology.

Position B: Those who are committed to this position uncritically accept the oppressive dominant cultural values as the "right" values even though, *unlike Group A*,

they are not committed to the idea that the epistemic success of a certain theory is evidence of it being the one true, value neutral account of the phenomenon under consideration. For instance, some women may uncritically accept the sexist values motivating and embodied in Short's menstruation theory as good values, even though they recognize that the epistemic merits of that theory does not mean that it is the one true value neutral account of menstruation.

Position C: Those who hold this position may believe that the dominant contextual values shaping particular medical theories contribute to epistemic success. For instance, they may believe that sexist values are at least in some part responsible for the predictive and explanatory success of that theory relative to that of other theories.<sup>266</sup>

### 5.3 *My Response to the Critical Perspective Objection*

I do not consider this objection to be a threat to my notion of objectivity; specifically, the proposal that members of marginalized groups be invited to critique medical theories. Rather, I read it as an opportunity for me to clarify my position. I believe positions A, B, and C are problematic and have the potential to undermine the kind of critical dialogue about medical theories that my notion of objectivity aims to foster. Below, I explain my reservations about them.

My concern about those who subscribe to position A has nothing to do with their peripheral social position and everything to do with their concept of theory. The notion of theory they subscribe to is problematic for the following two reasons:

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<sup>266</sup>I owe this point to Professor J. Nelson.

1. The assumption that a particular epistemically successful theory is the one, true account of the phenomenon at issue, unmediated by the social, political, and ethical values of those that constructed them is not warranted. I argued in chapter two that theories are representations (or models) of the world that necessarily reflect the interests of the communities that constructed them. Thus, a theory's epistemic success, including empirical adequacy, cannot be considered evidence that it is the one and only true unbiased account of the phenomenon under consideration.

2. Assuming that theories reflect the interests of the communities that created them *and* given that theories are underdetermined by data (i.e., the same set of data can provide support for different theories) *and* given that competing theories may have varying degrees of epistemic success,<sup>267</sup> no one theory, regardless of its epistemic merits, can be considered the one true value-neutral account of the world.

It could be objected that this is a set-up for a free fall into relativism. But I do not believe that the form of theoretical pluralism that I have argued for has to lead to that conclusion. Given that theories are motivated by and reflect different interests and concerns, different, and even incompatible, empirically adequate theories can be regarded as responses to different cognitive needs and interests of various communities:

To say that there may be irreconcilable but coherent and empirically adequate systems for accounting for some portion of the world is not to endorse

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<sup>267</sup>For example, suppose there are two theories, X and Y. Theory X might be simpler than theory Y, while the latter coheres better with the credible theoretical paradigms of other disciplines than theory X.

relativism but to acknowledge that cognitive needs can vary and that this variation generates cognitive diversity.<sup>268</sup>

Aside from having reservations about those who subscribe to position A,<sup>269</sup> I also have serious concerns about the kind of contribution that those who subscribe to position B would be capable of making in terms of the kind of critique they could offer of particular medical theories. Nancy Hartsock and a host of other standpoint theorists have argued that members of socially marginalized groups are *better situated* than those who are dominant within a particular oppressive social framework to see it for what it is. But that vision "must be struggled for and represents an achievement which requires both science (and education) to see beneath the surface of the social relations in which all are forced to participate..."<sup>270</sup> Members of disenfranchised communities who have unquestioningly and unreflectively accepted the version of social structures and relations that was presented to them, such as women who believe patriarchal values are good, cannot be relied on to provide the kind of critical contribution that the interaction between the medical theorists and marginalized groups is meant to generate. It is intended to make theorists aware of the contextual values shaping their work and to encourage them to question those norms.

I also find position C troubling because of its endorsement of oppressive values and the assumption that those values are responsible for a certain medical theory's epistemic success. Given that medicine is an activity is about fostering human

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<sup>268</sup>Longino, SPK, 113.

<sup>269</sup>It must be acknowledged that Position A is not a position that *only* members of marginalized communities may hold. Those that are part of the dominant group(s) may hold it too.

<sup>270</sup>Hartsock, Nancy. 2004. "The Feminist Standpoint: Developing the Grounds for a Specifically Feminist Historical Materialism" in *The Feminist Standpoint Theory Reader: Intellectual and Political Controversies* edited by Sandra Harding. Routledge. 37.

well-being, rather than the well-being of particular groups at the cost of other groups, position C is ethically untenable.

Although I have serious concerns about the ability of those members of marginalized communities who are committed to positions A, B, or C to make the kind of contribution to the critical dialogue between the disenfranchised and medical theorists about medical theories that I envision, I do not believe that they ought not be part of this dialogue. My reason for not excluding them is that they could provide critical stress to the critiques of medical theories developed by the disenfranchised who do not unquestioningly accept the dominant value system and who are theoretical pluralists. That kind of stress could result in better critiques of medical theories than if those critiques were not subject to any kind of scrutiny at all.<sup>271</sup>

Those who take issue with my notion of objectivity (specifically, my strategy of critical dialogue between medical theorists and members of marginalized groups) could also object to my position on the grounds that I have not accounted for the fact that only *some* from those groups struggle to "see beneath the surface of the (oppressive) social relations in which all are forced to participate."<sup>272</sup> Given their social marginality, it would seem that all members of that community are equally well-positioned to "see" through oppression.

I believe that while it may appear that all members of disenfranchised groups are equally well-situated to question the dominant paradigm, a number of factors determine which of them develop a critical consciousness. The following is a list of some of those factors:

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<sup>271</sup> I owe this point to Professor J. Nelson.

<sup>272</sup> Hartsock, Nancy. 2004. "The Feminist Standpoint: Developing the Grounds for a Specifically Feminist Historical Materialism" in *The Feminist Standpoint Theory Reader: Intellectual and Political Controversies* edited by Sandra Harding. Routledge. 37.



1. The existence of multiple forms of oppression *and* the nature of interaction between them in a particular society may play a role in determining whether the disenfranchised recognize as unjust their treatment and engage in collaborative resistance and emancipation projects. Depending on where particular members of a marginalized group are located within the intersecting webs of oppression, their ability to establish alliances with other disenfranchised persons may be compromised or enhanced. For instance, in a patriarchal, capitalist society, women who have class privilege may not view themselves as belonging to a marginalized group vis-à-vis their sex because of the privileges afforded to them by class.<sup>273</sup> Or even if they do recognize themselves as oppressed by patriarchy, they may decide not to form alliances with women who are working class in order to maintain their class privileges.

2. The political freedom (in a particular nation) that various members of disenfranchised communities have to collaborate in critiquing the dominant oppressive paradigm and constructing an alternate vision of life may also determine whether or not members of marginalized groups are able to develop a critical consciousness. In societies afflicted by multiple forms of oppression, some of the oppressed may be better-positioned socially and financially than others to take political action.<sup>274</sup>

This is by no means an exhaustive list. However, it identifies some of the crucial factors that determine why some, but not other, members of marginalized groups recognize dominant oppressive cultural values for what they are.<sup>275</sup> Thus, it

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<sup>273</sup>This is a grossly simplified example. In real life, things are much more complicated given that multiple forms of oppression may be interconnected.

<sup>274</sup>The political freedom afforded to the marginalized may also determine their exposure to alternative world views. That, in turn, determines whether they are able to "see" (and to what degree) oppressive values for what they are.

<sup>275</sup>An extended treatment of this issue lies beyond the scope of this project.

explains why not all members of disenfranchised communities are equally qualified to be part of the kind of critical dialogue with medical theorists that I espouse.

I have argued that the Critical Perspective Objection is not a defeater argument against my position. Now, I consider another objection to my proposal.

#### 5.4 *The Epistemic Privilege Objection*

It could be argued that my strategy is impractical because it offers no way of adjudicating between different, possibly incompatible, analyses members of disenfranchised groups may provide of the values motivating and embedded in particular theories. Consider the following scenario: After reading the analysis of the two menstruation-as-pathology theories that I provide in chapter three, a Marxist (a member of a marginalized group in a capitalist society such as this one) could argue that the two theories are *fundamentally* motivated and shaped by the capitalist concerns of rich industrialized nations that see the growing populations in non-industrialized, natural resource rich, developing nations as a threat to their ability to secure raw materials for their industries. Alternatively, upon reviewing my analysis of the two menstruation theories, a feminist might argue that the theories are *essentially* rooted in and defined by a patriarchal world view.<sup>276</sup>

Which one of the two competing analyses ought medical theorists accept and use to guide their work? After all, depending on the one they decide to follow, they would have to make different moves in terms of revising their hypotheses and research agendas. If they accept the Marxist analysis, they would have to re-consider their

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<sup>276</sup>This should not be read as the feminist analysis of the two cases; it is merely a hypothetical posit for heuristic purposes.

commitment to the idea that overpopulation in the global South is the cause of poverty and a threat to global stability. Acknowledging that capitalism run amok is a key cause of global poverty would mean they might have to significantly change their research project, if their primary motivation is to end poverty. Alternatively, accepting the feminist analysis, among other things, would mean revising some of the foundational hypotheses of their menstruation theories because they are gender biased.

Given that both analyses have merit and given that there are serious consequences depending on which of the two competing analyses they accept, should medical theorists decide between them by tossing a coin? No, that would be wrong. Such weighty decisions ought not be made in a flippant manner and it is demeaning to the two analyses at issue. Thus, it could be argued, unless my proposal for a critical dialogue between medical theorists and disenfranchised communities can offer an appropriate protocol for resolving such dilemmas, it is not a tenable strategy.<sup>277</sup>

#### 5.6 *Response to the Epistemic Privilege Objection*

This is a serious problem for me as well as for Longino (she espouses this strategy too). Before I offer my response, I consider what she has to say about it as it might be illuminating. Longino does not appear to have addressed this problem head on (in *Science as Social Knowledge* and other key works). But I believe she does consider it a serious problem if a theory can't provide a guide for deciding between competing claims. In fact, she rejects standpoint epistemology and psychodynamically

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<sup>277</sup> Presumably medical theorists would want to decide between competing analyses because, given that they do not have unlimited resources, they would have to make decisions about which projects to pursue and which one's to revise.

based theories<sup>278</sup> because they do not offer a method for deciding amongst the competing analyses generated by those who are differently positioned and oriented:

The standpoint and psychodynamically based theories recommend certain new positions and orientations as superior to others but fail to explain how we are to decide or to justify decisions between what seem to be conflicting claims about the character of some set of natural processes. On what grounds can one social location or affective orientation be judged superior to another?<sup>279</sup>

Interestingly enough, Longino fails to acknowledge that her proposed strategy is vulnerable to the same problem. But I believe it may be possible to craft a response on her behalf using the notion of theories that she subscribes to and by drawing on her commitment to theoretical pluralism. Longino is committed to a model theoretic theory of theories (as opposed to the theory of theories as sets of propositions).<sup>280</sup> According to her, theories are representational devices for modelling aspects of the world. The adequacy of a theory is determined by two factors. First, its success in mapping some subset of the relations or structures posited in the model onto some portion of the experienced world.<sup>281</sup> Second, its efficiency in modelling those relations or functions that are of interest to those that constructed it.<sup>282</sup> As particular ethical and political values and interests motivate and are embedded in specific theories, each theory is expressive of a different form of engagement with the world *and* indicative of

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<sup>278</sup>Longino discusses Evelyn Fox Keller psychodynamically based theory to make her point.

<sup>279</sup>Longino, SPK, 109.

<sup>280</sup>Longino, SPK, 114-5.

<sup>281</sup>Longino, SPK, 115.

<sup>282</sup>Longino, SPK, 115.

"the partiality of each model in revealing some aspect of natural phenomena ..." <sup>283</sup> It is because Longino believes that a multiplicity of theories is a necessary consequence of theories reflecting and being shaped by different human interests, *and* it is because she holds that they reveal different aspects of the phenomenon at issue that she endorses fostering the development of multiple theories, even those that are incompatible. The multiple accounts and dialogue between their formulators "make possible the refinement, correction, rejection, and sharing of models." <sup>284</sup>

Given Longino's stance on theoretical pluralism, I believe she might not be unduly troubled by the objection that she has not identified a strategy for deciding between competing analyses that disenfranchised groups might provide of the background assumptions and values at work in a particular medical theory. She would probably argue that because each of their analysis offers different, partial insight into the theory under scrutiny, they should all be allowed to flourish.

Although I find appealing the idea of encouraging the development of multiple, possibly incompatible, analyses of theories, that approach might be regarded as impractical by medical theorists, who, constrained by time and resources, would want a definitive answer about which of the two (or more) competing analyses they should use to determine which hypotheses should be revised and which ones should be developed. If medical theorists, such as Short et al, are expected to take seriously the analyses of marginalized communities about the background assumptions and values shaping their work, they must be afforded a method for deciding between competing

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<sup>283</sup> Longino, SPK, 116.

<sup>284</sup> Longino, SPK, 117. I am puzzled why Longino does not offer this as a solution to the problem of epistemic privilege that she believes undermines standpoint theory and psychodynamically based theories. I assume it is a case of an oversight on her part. Or it could be the case that she thinks that standpoint theory and psychodynamically based theories don't have the resources within them to offer this response (I owe this point to Professor Schwartzman).

analyses of their work by members of disenfranchised groups. Otherwise, they might consider the critical dialogue with members of marginalized communities to be a futile activity that cannot be translated into practice.

I believe that in some cases the solution might be to "encourage" those who have provided competing analyses of the medical theory under scrutiny to collaborate with one another. Each of them would be asked to attempt to resolve an aspect of the theory (or the circumstances of its development) that lies beyond the scope of the tool that they have used to construct their analysis. For example, fostering collaboration between the Marxist and feminist would mean asking the Marxist to account for the fact that R.V. Short holds that women's reproductive physiology ought to conform to the evolutionary norm for non-human primates, whilst he makes no such demand of men's reproductive physiology.<sup>285</sup> He/ she would have to do so using only class as the axis of analysis. It would also entail asking the feminist to use only gender as a tool of analysis to account for Frank Notestein's success in securing the support of the US government for the Population Council's agenda at a time when the US was concerned about the spread of communism in poor nations that were rich in natural resources and had high birth rates (the worry was if communism spread in those nations, US industries would not be able to procure the raw materials that they needed from those nations- see chapter three).

The purpose of this exercise is to make the Marxist and the feminist realize that neither gender by itself nor class by itself, as a tool of analysis, can provide a satisfactory account of all aspects of the two menstruation theories. Ideally, as a result,

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<sup>285</sup>For instance, Short acknowledges that in contrast to the semen of males of other primate species (with the exception of gorillas), the sperm of human males has a greater incidence of abnormality) resulting in high incidence of embryonic mortality in humans than in other primates, he makes no effort to argue that that aspect of human male reproductive physiology must be considered an evolutionary aberration that is a pathological condition.

they would come to the conclusion (as I do in chapter three) that the two menstruation theories are shaped by multiple forms of oppression, including sex and class, interacting in complex ways with one another. That realization might lead them to collaborate to create an analysis of the menstruation theories that covers more ground, and thus, has more epistemic merit than their individual accounts. On the basis of that joint endeavour, they could make suggestions to medical theorists about how they should revise and re-conceptualize their theories and research program. In the best case scenario, they would make at least the following three recommendations to Short et al:

1. They ought to re-evaluate their commitment to the idea that poverty is caused solely or even primarily by high birth rates in developing nations. Instead, they should recognize that capitalism run amok, colonialism, poorly planned structural adjustment programs, unjust trade agreements, civil war, and corruption are responsible in no small part for those miseries.
2. They should rethink their advocacy of menstrual suppressants as the cure for anemia in developing nations. There is good evidence that hookworm disease and malnutrition are primary causal factors for anemia amongst poor children, women, and men in developing nations (see discussion in chapter three).
3. They ought to reconsider their arguments construing menstruation as an evolutionary aberration that is pathological because it is under girded by gender biased assumption about women.

This approach of "encouraging" collaboration between different members of marginalized communities is a strategy that medical theorists could use in *some* cases to resolve the problem which one of two (or more) competing analyses of the values

shaping their work they should give epistemic privilege. Ideally, the disenfranchised would be willing to participate in such collaborative exercises because of a desire to oppose and eradicate oppression not just for the group that they are part of but also other populations that are subjugated.

However, I have to admit that there is no guarantee that it will work in every situation. The possibility exist that in some cases, a particular disenfranchised group using a certain tool of analysis, say sex, will be able to account for the phenomenon at issue just as well as another marginalized group that employs another tool of analysis, say class. That would mean medical theorists would have no substantial way of deciding which analysis to give privilege.

In light of that limitation of my approach, medical theorists, such as Short et al, and philosophers of medicine, such as Boorse and Engelhardt, might balk at the prospect of using it. But I do not believe my strategy's limited scope justifies its wholesale rejection. Rather, given that it can be fruitful in at least some instances and given that there isn't a viable alternative for identifying values encoded in medical theories, I believe medical theorists should adopt it.

Having adressed two objections to my notion of objectivity, now I close the dissertation.

## 5.6 *Conclusion*

In the interest of rendering transparent my motivation in undertaking this particular project, I delineate the actual trajectory of my inquiry in the form of a narrative. The dissertation chapters are not organized in that sequence because the



actual path of my research, including the process of formulating my notion of objectivity, was contingent, circuitous, and complicated.<sup>286</sup>

I began thinking about the issue of the objectivity of medical theories while taking a graduate level seminar in medical anthropology. I was quite impressed by Emily Martin's careful and insightful analysis of sexist medical theorizing about women's reproductive physiology.<sup>287</sup> Reading it also made me curious about the characterization of species typical aspects of women's reproductive physiology in obstetrics textbooks. Whilst perusing a few textbooks in the library, I came across *Williams Obstetrics*. I found that over a period of two decades and in various editions of this standard US textbook, the editors of this work had consistently categorized women's periodicity as a pathological condition that necessarily required medical intervention. The arguments that they gave in support of their position were fallacious and blatantly sexist. As someone opposed to sexism, and oppression in general, I found that very troubling.

Delving deeper into their theory, I realized that they had appropriated a good bit of it from R.V. Short's work on menstruation. I found Short's theory to be infected with the same sort of sexism, only it was much worse. He believed that women's reproductive physiology ought to conform to that of non-human female primates while making no such analogous demand of men. Short too appeared to be blind to the values shaping his work. Interestingly enough, he also identified himself as concerned about women's well-being. Moreover, I believe he too would consider his theory to be an objective account of women's reproductive physiology.

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<sup>286</sup> Although I have argued in the dissertation that all theories have contextual values, I believe a similar argument could be made for concepts too. Though making that case lies beyond the scope of this project, by describing the concerns and concrete circumstances that shaped my notion of objectivity, I provide an instance in support of the claim that concepts have values too.

<sup>287</sup> Martin, Emily. 1992. *The woman in the body : a cultural analysis of reproduction*. Beacon Press.

As I struggled to make sense of all of that, I perused Short's other publications. I saw a connection emerge between his fears about population growth, his categorization of menstruation as a disease, and his endorsement of *menstrual suppressant contraceptives* as the treatment of choice for that disease. His theory appeared to have been influenced by a host of values. Some of them, such as sexism, were embedded in the hypotheses of his theory, whilst others, such as racism, populationist, and nationalist concerns, were external to it (i.e., they were not located in the theory proper but appeared to have influenced his decision to advocate menstrual suppressant contraceptives as *the* preferred treatment for menstruation). Curious whether the populationist, racist, and nationalist worries infecting Short's work were an idiosyncratic bias or a culturally pervasive phenomenon, I researched the issue of population growth in the UK during the 1960s and 1970s. I realized that the values shaping his work mirrored the social and political concerns of the dominant population of Britain during that era.

Whilst developing that analysis, I was asked to contribute to an anthology on menstruation. The editor wanted me to expand my paper on Short and *Williams Obstetrics* to include a discussion of the then newly published *Is Menstruation Obsolete?* I found that Elsimar Coutinho and Sheldon Segal's theory medicalizing menstruation seemed to have been motivated and shaped by the same cluster of values that appeared to have influenced Short. There were also other striking parallels between them. Coutinho and Segal too identified themselves as concerned about women's well-being, all the while espousing fallacious and patently sexist arguments. They too, I believe, regarded their account of menstruation to be objective.

As a bioethicist, was curious to see how Christopher Boorse and Tristram Engelhardt would respond to Short et al.'s work. I realized that while Boorse holds that theories of disease ought to be objective (i.e., non-normative), his conception of disease is such that he would accept Short et al.'s theories categorizing women's periodicity as a disease, provided they could make the argument that it compromised women's ability to reproduce and survive.<sup>288</sup> In contrast to Boorse, Engelhardt acknowledges that all theories of disease are value laden constructs. But on a certain reading of his notion of disease, it would seem that he would, too, support Short et al.'s classification of menstruation as a pathological condition.<sup>289</sup>

While trying to make sense of Boorse's and Engelhardt's (possible) responses to Short et al.'s theories, I realized that Boorse's stance (that theories of disease ought to be value free and that good theories of disease are value free) was predicated on a fairly strong commitment to the logical positivist conception of objectivity.<sup>290</sup> (He, of course, does not acknowledge that.) Logical positivists, such as Hempel, believed theories could be objective (i.e., value neutral). They claimed knowledge could be objective if it had been "intersubjectively verifi[ed], independently of individual opinion or preference, on the basis of data obtainable by suitable experiments or observations."<sup>291</sup> The assumption underlying that notion of objectivity is that the presence of values within a theory exerts a distorting influence, marring its ability to account for what is out there. The logical positivists held that peer review guaranteed objective knowledge because (they assumed that) statements that were laden with values would not pass the test of intersubjective verification.

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<sup>288</sup> See chapter four.

<sup>289</sup> See chapter four.

<sup>290</sup> Whether Engelhardt is committed to logical positivism to the same degree is arguable (see chapter four).

<sup>291</sup> Hempel, 1965, 141. See also Hempel, VOS, 374 and 380-1.

Reflecting on the logical positivist notion of objectivity, I realized that Short, the editors of *Williams Obstetrics*, Coutinho, and Segal also appeared to subscribe to that notion (although they do not identify themselves as such). They too believed that it was possible for theories to be value free. They apparently held that theories that passed the test of peer review (or more precisely, intersubjective verification) qualified as value neutral. As their menstruation-as-pathology theories had passed that test (i.e., the various hypotheses constituting their theories had been intersubjectively verified), they believed they were value free.

The logical positivist epistemology, including the notion of objectivity that it endorsed, has come under heavy fire in the last few decades. In the interest of developing a critique of that conception of objectivity, I turned to the works of critics of logical positivism. I was particularly impressed by Helen Longino and Lynn Hankinson Nelson analyses. Their critiques addressed the issue of gender bias in the sciences *and* were based on close textual analysis.<sup>292</sup> However, while I found much that was admirable about their work, I also realized that their analyses were flawed in certain crucial respects.<sup>293</sup>

Longino made a compelling argument against the logical positivist conception of objectivity. She argued that because the background assumptions mediating the relationship between hypotheses and evidence could be value-laden, the logical positivists were not justified in assuming that the intersubjective verification of hypotheses guaranteed value free theories. The problem with Longino's argument was that it did not categorically refute the possibility of value free theories. It allowed for

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<sup>292</sup>Nelson's engagement with logical positivism is indirect in the sense that she develops some of Quine's work, who has formulated a number of devastating critiques of logical positivism.

<sup>293</sup>See chapter two.

the possibility that there could be theories that are value free. Such theories would have concepts, claims, or background assumptions that were not value laden.

Analyzing the limitations of Longino's critique, I realized she needed a different sort of argument to categorically refute the logical positivist notion of objectivity. To that end, drawing on Lynn Hankinson Nelson's work on, first, the deep connection and interdependence of scientific theories and ordinary language, and second, the communal nature of knowledge building practices, I argued that any and all theories are value laden. That argument is a more effective refutation of the assumption underlying the logical positivist notion of objectivity- that theories can be value free- than the one that Longino formulates. Using it, I categorically undermined any effort on Short et al.'s part to argue that their menstruation-as-pathology theories were value-free. I also constructed an alternative conception of objectivity.

I hold objectivity is a stance whose adoption entails, amongst other things, that all knowledge construction activities are social, value laden. Assuming that stance also entails placing a premium on critical dialogue about medical theories and research. My notion of objectivity differs from the one that Longino constructs. She contends that objectivity is a "characteristic of a community's (critical) practice of science."<sup>294</sup> That practice requires that, first, there are recognized public venues for criticism, second, there are shared epistemic and social standards for assessing scientific claims, third, the scientific community responds appropriately to critiques that have merit, and fourth, intellectual authority is shared equally and not afforded only to those with social and political power.<sup>295</sup>

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<sup>294</sup>Longino, SPK, 74.

<sup>295</sup>Longino, SPK, 76-9.

I find Longino's account of objectivity as a critical practice problematic. I believe objectivity ought to be conceptualized as a practice based on certain principles. The principles at issue are those that define the nature of medical inquiry. Some of the key principles are as follows:

- i. Scientific and medical inquiry aims at constructing empirically verifiable accounts of phenomena.
- ii. All theories are value laden.
- iii. One can only know as a member of an epistemic community.<sup>296</sup>

Conceptualizing objectivity purely as a critical practice, without acknowledgment of these principles, appears to invite wrong headed theories and critiques. For instance, someone like Short might think that it is possible for him to construct a value neutral theory, or some might think that creationism qualifies as a scientific theoretical alternative to the theory of evolution. It is in the interest of preventing such misunderstandings that I argue for conceptualizing objectivity as a

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<sup>296</sup>Longino would find this principle unacceptable because she rejects the idea (borrowed from Lynn Hankinson Nelson) underlying it- that epistemic communities are the primary knowers (see *Fate of Knowledge* 146). Her reason for rejecting is the same one that Heidi Grasswick articulates. Grasswick claims that Nelson's attribution of primary epistemic agency to communities deprives individuals of any cognitive or doxastic autonomy, which bars the possibility of any change or growth in knowledge.

I believe Grasswick and Longino's critique of Nelson misses the mark because it does not take into account the interaction between multiple epistemic communities that is at the heart of Nelson's theory. Nelson's claim that epistemic communities are primary knowers does not preclude the possibility of change or growth in knowledge. As individuals are members of multiple epistemic communities simultaneously, and given that these communities go in and out of existence, individual members who are part of those communities carry knowledge between communities. The knowledge brought by a new community member could cause that community to re-evaluate its conception of the world, resulting in a change in its knowledge. This interaction between communities, I believe, accounts for change and growth in knowledge. Also, Grasswick and Longino's claim that individuals have no doxastic or cognitive authority is misguided because there is nothing in Nelson's account that bars individuals from exercising epistemic agency. An individual's decision to join or leave an epistemic community could be an exercise of his/ her doxastic or cognitive agency.

stance, whose adoption entails acknowledging the kind of activity scientific and medical inquiry is *as well as* a commitment to critical dialogue about theories.<sup>297</sup>

Given that I define being objective as a particular stance that knowers may adopt, my response to the question motivating this project - "are medical theories objective?" - is that theories cannot be considered free of contextual values. Theories reflect the values and interests of the communities that constructed them, thus, no medical theory can be value free. While I hold that no medical theory is value free, I do not mean to suggest that all value-laden medical theories should be accepted uncritically. Moreover my notion of objectivity does not entail a categorical prohibition against the construction of medical theories pathologizing aspects of physiology unique to women, men, or humans. But what it does require is that medical theorists should recognize their work as a value-laden activity and that they should be more critical and reflective about their work than they are currently. To that end, the strategies that I delineate for identifying values shaping theories should be more useful than the ones endorsed by the logical positivists.<sup>298</sup>

While I have answered the question that prompted this project, there are other questions that this dissertation has raised that I have not been able to address. Below, I sketch the three that interest me the most.

Earlier in this chapter, I noted that not all members of marginalized communities can be expected to have critical perspective into the dominant social system. The question that I did not address is as follows: What could be done, both

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<sup>297</sup> Although my position may seem to be similar to Longino's, it is different from hers in a crucial respect. While I (drawing on Nelson's work) make a rigorous epistemological argument for the claim that all theories have values, Longino does not make such an argument.

<sup>298</sup> Of course, I do not hold that any and all contextual values should or can be eliminated. Theories with oppressive values should be revised or discarded in favor of those theories with emancipatory values with appropriate epistemic merits. Moreover, given the nature of human epistemic enterprises, I contend all theories have contextual values.

nationally and internationally, to create conditions that would allow all members of marginalized groups develop critical consciousness?

Another question that remains unanswered is as follows: Given that the work of feminist philosophers of science and epistemologists (including the work I do here) has much to offer to philosophy of medicine as well as medical theorists, what can be done to introduce them to it and to engage with it?

The third and final question that I believe deserves attention is based on Carlo Filice's work. He contends that all of us have a duty to question the account of the world that society presents to us. However, Filice does not explain why that obligation exists.<sup>299</sup> I believe that is a question worth thinking about.

I close with these questions because they are of particular interest to me as they have to do with making theory relevant to practice and so I view them as potential future projects.

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<sup>299</sup>Earlier, I discussed why the disenfranchised might be more likely to question the dominant account of reality than those who are socially privileged but I did not take up the question why all of us have a duty to do so.



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